Jeffrey Friedman

INTRODUCTION: WHAT CAN SOCIAL SCIENCE DO?

The publication of conference proceedings is a first for *Critical Review.* But as you will see, the conference in question was an unusual one.

First of all, rather than the usual reading, or summarizing, of academic papers, the format of this conference was entirely conversational.

Before the conference began, the participants did receive papers that were intended to spark discussion; many of these are now available online, and can be read at the web address shown for these papers before each session. One paper, by David A. Bell, is published below.

At the beginning of each session there was a further provocation: remarks from one of the participants designed to address the broad question at issue for the following three hours. After these preliminaries, however, there was a genuine dialogue across disciplines, methodologies, and ideologies that never degenerated into either the acrimonious "discourse" typical of politics, or the distanciated conversation characteristic of scholarly conferences dedicated to presenting normal-science research in various subdisciplines.

It is, naturally, our hope that the dialogical and provocative nature of the conference is a fitting example of the type of interchange that *Critical Review* has always tried to encourage.

An even greater departure from the norm was the topic of the conference, which did not take for granted, as is typically and unob-

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jectionably the case, that the usual objects, subjects, conclusions, procedures, and organization of the social sciences (including History) are sound.

Instead, the premise of the conference sprang from one of the approaches championed by some of its participants: a culturalist approach, which treats the object in question—in this case, social science—as a phenomenon with roots in the thoughts of human beings who, being fallible, might have unintentionally created something less than ideal.

The particular variant of culturalism regarding the social sciences that inspired the conference has been expressed by its organizer, Liah Greenfeld, in the form of five theses, which also animate the Institute for the Advancement of the Social Sciences at Boston University (www.bu.edu/uni/iass)—an institute that was launched at the conference:

1. Even though the human sciences were meant to study humanity, they focus on social structures, which, far from being distinctively human, are essential in the lives of all animals, hence are best addressed by biology. What separates humanity from the rest of the animal kingdom is not society or social structures, but the transmission of social order via symbolic means, i.e., culture, rather than by genes. Symbolic or cultural processes take place primarily in people's minds. Instead of being unconscious subdisciplines of biology, the social sciences should be sciences of culture and the mind.
2. Because physics was seen as the science par excellence, the social sciences imitated the physical model, trying to understand human reality by using quantification and mathematical modeling—methodologies obviously inadequate for understanding the values, meanings, and motivations that determine and explain the actions of conscious entities.
3. The institutionalization of disciplines and subdisciplines in the human sciences was almost invariably linked to ideological and political—as opposed to purely scientific—concerns.
4. Quantification and mathematical modeling, applied to the study of social "structures," tended to treat them as if they evolved mechanically. Mental reality, in turn—i.e., the meaning that people assign to their actions—was postulated to be a mere reflection of these structural processes, and therefore was grossly under-researched.
5. The problems of this paradigm are seen in the failure of social-scientific predictions, and the inability of social science to provide useful solutions to social problems. It is also reflected in the fact that, while sophomores in physics have left Newton far behind, the writ-

> ings of Weber, Durkheim, even Plato and Aristotle, often seem more adequate than many contemporary social-scientific studies for understanding the world in which we live.

Standing behind these five theses are, arguably, three assumptions: (1) ideas play an important and overlooked causal role in human behavior; (2) valid hypotheses about the role of specific ideas in human behavior can, as in natural science, be tested by successful prediction; and (3) the non-scientific (e.g., political) aims of social scientists are responsible for their failure to produce sound predictions. Correspondingly, a good deal of the subtext of the conference consists of the participants' struggle over three questions: whether social science should pay more attention to the causal force of ideas; whether natural science is a good model for social science; and whether the problems of social science can be solved by a depoliticized dedication to impartiality.

Whatever one's answers, these seem to be good questions to be asking, especially in these pages.

Asking these questions—in the form of both the conference and the publication of its proceedings—would have been impossible without the support of the Earhart Foundation of Ann Arbor, Michigan. I am sure that my gratitude for this support is shared sincerely by all of the conference participants.

The Hellenism and Modernity Project of the IASS

Since human societies do not lend themselves to laboratory experiments, the historical-comparative method represents for us the main approximation to experimentation. To understand modern culture, we must study not only this culture itself but place it in comparison with different cultures. The first such comparison that the IASS is undertaking is with the culture of Ancient Greece.

The Hellenism and Modernity program was created in September 2003 with the support of the Greek Ministry of Culture. Its purpose is to explore the influence of Hellenism on modern consciousness. One of the main areas of emphasis is the impact of ancient Greek political concepts on modern political culture. Other areas include the modern effects of ancient Greek ideas of the self, reason, the individual, social ethics, religion, polytheism, logic and scientific method, and art.

The Hellenism and Modernity Program in the academic year 2003–2004 centered around the seminar on Ancient Greek culture. This seminar included several guest lectures by classicists, such as David Roochnik on Book XI of the *Odyssey* and Homeric conceptions of death; Stephen Scully's discussion of the Homeric *Hymn to Demeter* and ritual similarities of weddings and funerals; discussions of Plato's Socratic dialogues and Aristotle's *De Anima;* and students' independent work on related subjects, in particular the interconnection between monotheistic traditions and Aristotelian logic. The best paper received the prize of a ticket to Athens, provided by the generosity of Olympic Airlines.

The Institute for the Advancement of the Social Sciences is currently seeking funding to make possible the development and continuation of the program.

HELLENISM AND MODERNITY PROGRAM
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CONFERENCE ON THE STATE OF THE SOCIAL SCIENCES

Boston University, December 6–7, 2002

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Hernando Forero
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Nikolas Prevelakis
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Peter Wood, Department of Anthropology, Boston University
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OPENING REMARKS

GREENFELD: This conference has been in preparation for a very long time and now we have a snowstorm that makes things more difficult than they should have been. Nevertheless, we'll start and hope that everyone will get here safely.

Last year I wrote to all of you and in no uncertain terms presented my position on the state of the social sciences. Since then, more than a year passed and while other things seem to me much less certain than

they did before, the state of the social sciences doesn't appear to have improved.

When I was 17, I moved from Russia to Israel and enrolled in the preparatory course at the Hebrew University. Most of the time there was devoted to languages, but I still had plenty of it to explore the university environment, and in this free time I wandered into a course that was given in English for foreign students, titled "Sociology." I was attracted by the word itself, because in Russia, which was then still Soviet, the word sociology appeared only in the phrase "bourgeois sociology," which had an unambivalently derogatory meaning, and referred to some sort of absurd and clearly unscientific doctrine that nobody would think of teaching in a university.

I understood very little English at that time, but what I understood in this course on sociology was enough to sweep me off my feet. What I understood was the idea that it was possible to gain objective knowledge—similar to that existing, say, in chemistry or mathematics—objective knowledge of human actions, to understand and explain them with the certainty and accuracy of science, and this idea struck me as a most wonderful revelation. I fell in love with it, and never fell out.

When my preparatory course was over, I added a major in sociology to my already chosen major in the history of art. From then on, I am sorry to confess, it was all downhill.

There was no objective knowledge of human action to be gained from studying sociology. Texts that deepened one's understanding, many of them written by those present in this room, were few and far between, and what one mostly got was greater confusion.

I had a couple of very good teachers, to whom I am grateful to this day, but especially Joseph Ben-David, who became my graduate advisor and mentor, was also, like me, deeply frustrated with the state of the discipline. Without excessive optimism, he advised me to look for enlightenment in economics and other social sciences. I did, and found them all wanting.

Professor Ben-David was, together with Robert Merton, a founder and a foremost practitioner of the sociology of science, and he got me interested in the closely related disciplines of the history and philosophy of science—of physics and biology in particular. Reading in the history and philosophy of these disciplines made it clear to me that social scientists, by and large, did not think the way natural scientists do, and that, viewed as a collective activity, the social sciences, in their development, have followed a pattern strikingly different from that of the

natural sciences. In other words, their trajectory was not that of the social institution of science, which means that, from the structural point of view—as collective, patterned activities—they cannot be considered sciences.

The course of least resistance, even for those frustrated with the state of affairs, would be simply to contribute to the small body of useful work by satisfying one's desire for objective knowledge and the deeper understanding of one or another little piece of human reality. But in the past several years, I have been persuaded that such a personal solution is no longer enough.

It is no longer enough, first of all, because the accumulation of objective knowledge of human action is made more and more urgent by the advances of physics and biology. The desire for understanding our distinctive human experience has always been with us. And this experience has always been central. But in the past, our attention was claimed by physical and biological necessities and we, collectively and individually, had many fewer years to focus on and be affected by the distinctively human experience.

First, we had to survive, for longer periods. Next, we had to survive in better health and comfort. Physics and biology have assured this. For the great majority of people in the past, most suffering was caused by natural causes—death, disease, starvation, heat and cold. Today, in modern societies, we are largely protected from these. Many of us have not experienced the loss of loved ones until well into our adult years. Death in childbirth is a rarity. Few parents lose children to infant mortality. Most of us are healthy until fairly advanced age. The great majority has never gone hungry, or without comfortable shelter in inclement weather.

But these same advances in physics and biology also dramatically increased both our exposure to man-made suffering and our ability to inflict it. In this scientifically advanced age, we do not suffer less. We only suffer from different causes. Modern suffering is mostly a result of human action, intended and unintended.

To an unprecedented degree, we have subjected natural forces to our desires, but we have not the faintest idea what to do with ourselves. It is the population of affluent and physically healthy modern societies who chronically suffer from depression. It is our teenage children who kill themselves in record numbers, and often take, in addition, the lives of their peers. It is needless to point out that our modern jets are flown into our modern skyscrapers, destroying as many lives as in the good

old days would be destroyed by a major volcanic eruption. Thus today, more than ever, we need a science of humanity capable of furnishing us with objective knowledge and taking us, at last, to the springs of human action.

Personal solutions are not enough, secondly, because as teachers, we must do better by our students. Bright, interested, and, most importantly, innocent young people come to us to understand the world in which they live. Their minds are still open, and they trust us; we all too often respond to their trust by clogging these innocently open minds with ideologically inspired misinformation presented as data, and useless techniques for its manipulation presented as methods of analysis.

Despite ourselves, perhaps, we encourage some of our students to become scholars. As I watch the fledgling careers of several generations of my students, I am less and less ready to send them looking for objective knowledge in the unfriendly academic environment that is clearly committed to entirely different pursuits.

Finally, the time seems to be ripe for reform, and for this reason too, purely personal escape from frustration with the state of the social sciences is no longer enough. There are signs that dissatisfaction with the condition of the social sciences is very widespread. In every one of the social-science disciplines, there is a sense that they, collectively speaking, have been doing something wrong. The Nobel Prize in economics this year went to Daniel Kahnman, a psychologist, and Vernon Smith, a behavioral economist, who, though reaching diametrically opposed conclusions, arrived at them with the help of the methods of experimental psychology, thus bringing to the foreground of the discipline of economics a previously marginalized and most uncharacteristic approach. A former president of the American Sociological Association, thus a bona-fide mainstream sociologist, indeed employed by one of the central, mainstream departments, published an essay in the current volume of the *Annual Review of Sociology* arguing that sociologists should abandon physics as their model of how sociology should be done, and instead, focus on biology, specifically the theory of evolution. Adopting the methods of physics, he claims, essentially, was a mistake, and it is to this that the sorry state of sociology can be attributed. Similar pronouncements are made in political science, anthropology, and history. Something is wrong. Something must be done to correct it, so we find ourselves here today, in the avant garde of a burgeoning movement.

Before I declare the workshop open, I want to express my gratitude to two groups of people. The first one is you all, our guests and partici-

pants; I deeply appreciate your interest and your willingness to contribute of your time and knowledge. We here keep meticulous records, and who knows? If we are successful in this workshop, you may have a place in the history of a new discipline. In a hundred years or so, when they take stock of its past achievements, you may be gratefully remembered for being present at its birth or conception. I want to thank, in particular, those of you who have no vested interest in the outcome of this undertaking—such as some of us, social scientists in the midst or very beginnings of their careers, might, and I think should have. I mean the natural and exact scientists among you on the one hand, and the humanists, whom one would hardly expect to hanker after scientific objectivity, on the other. I also mean those most distinguished social scientists present, who cannot hope that a beneficial change in their disciplines would advance their professional lives, simply because they have already reached the very apex of professional fame and success. I am personally indebted to you all for your intellectual generosity.

The other group I wish to thank is the organizing committee. These young and very bright scholars, all of whom I have been privileged to have as students in the course of the almost nine years since I joined the University Professors Program at Boston University, and who range from recently titled Ph.D.s, to graduate students, to seniors in college, have been a constant inspiration and help for me, and it is their quest for real knowledge about society, specifically, that is the direct reason for this workshop. In particular I want to thank the chair of the organizing committee, my graduate student and long-time assistant, a scholar of unusual promise, soon to become—I predict, as social scientists are wont to do—a foremost authority in Latin American and Iberian studies, Jon Eastwood. You are all very well familiar with his name. This workshop, as you undoubtedly know, simply would not be possible without him.

Just to remind you, this is a workshop, not a conference where papers are presented. Some food for thought, in the form of papers, was distributed in advance, as you know. But obviously, every participant is free to bring to the table any other materials relevant to the discussion. Each session is going to be introduced by one of several participants, mostly guests, whose responsibility it is to spark the discussion. The discussion will be moderated by members of the organizing committee, in the sense that they will take down the names of speakers and keep things in order. I greatly hope that our discussions will result in something new, viable, and possibly of great benefit for society, a new and true science

of humanity, in fact. I declare the workshop open. Let's start the revolution.

SESSION I. THE RECORD OF THE SOCIAL SCIENCES

INTRODUCTORY REMARKS: Bruce Mazlish
MODERATOR: Chandler Rosenberger
PARTICIPANTS: Ali Banuazizi
Carlos Casanova
Jeffrey Friedman
Nathan Glazer
Liah Greenfeld
Jason Kaufman
David Landes
Bruce Mazlish
George Prevelakis
Peter Wood

Papers distributed to the participants for this session:

Bell, David. 2004. "Class, Consciousness, and the Fall of the Bourgeois Revolution." *Critical Review* 16 (2–3): 323–50.

Karsh, Efraim. 2002. "Revisionists, Arabists, and Pure Charlatans." www.bu.edu/uni/iass

Landes, David. 1991. "Introduction: On Technology and Growth." In *Favorites of Fortune: Technology, Growth, and Economic Development Since the Industrial Revolution*, ed. Patrice Higonnet, David Landes, and Henry Rosovsky. Cambridge, Mass.: Harvard University Press.

Liber, George. 2002. "New Soviet Nationality Studies: An Incomplete Social Construction of the Past." www.bu.edu/uni/iass

Mazlish, Bruce. 1998. "Some Achievements to Date." In idem, *The Uncertain Sciences.* New Haven: Yale University Press.

Rosenberger, Chandler. 2002. "September 11: The 'Global Century' Deferred." www.bu.edu/uni/iass

MAZLISH: Let me try to suggest some of the leading themes that we might want to take up.

One, very clearly, is the danger of politicization—in other words, ideology instead of social science. This difficulty has many ramifications, as we all know, because entwined with it is the question of the role of values. One man's ideology is another man's set of revealed truths and values and so forth, so we need to at least be aware of the

difficulties of this question. Certainly none of us, obviously, want to offer ideology instead of social science, but who knows what we're offering, as seen from the outside?

Second, the ideological aspect shadows the question of how social science is to help us deal with critical issues, such as terrorism; international conflict; sustainable development, if you like that term; culture itself; nationalism, of course. . . . And this raises all kinds of questions of its own. What if we came out with real, objective knowledge as to how to end racism, conflict, etc., etc.? You'd need another science that tells you how you implement it, or how you combine those two. So we've got a whole nest of problems there.

Third, this theme returns us to the question of the nature of social science, which is a subset of something larger called the human sciences. Is social psychology part of the social sciences, is it part of the human sciences, or what is it?—and the effort to meet positivist criteria, even if unspoken, tends to dominate much of the research that is undertaken. Yes, positivism is a nineteenth-century notion, and you'd think maybe it's outdated, but just look at neoclassical economics for a moment, and one can see that it is still very much trying to take a kind of positivist approach to its materials.

A fourth question or theme is the role of theory and the role of evidence. In the preliminary papers there is a general sense that theory has been distorting the empirical data, the empirical reality. My answer to that, by the way, if I can give a personal aside, is that we need more sophisticated theories—not that we should do away with theory, but that we need more sophisticated theories, and a better understanding of how we use them. I do not think we want to retreat to mere empiricism. I would simply say that that, in my view, is a dead end.

Fifth, present-day social science, by and large, has emerged out of the experience of modernity. It arose in response to the French Revolution and the Industrial Revolution. Indeed, the term "social science," as you know, was first used around 1789. And I think we face a challenge now, which is posed by globalization—a buzzword, a topic very much in the air, and so forth, but it points to something, which is that we are in a transitional period in which it appears that the nation-state is being challenged both from below and from above. I don't believe the nation-state is going to disappear, but certainly we have to look at it in a new and different way, and not take it as necessarily the jumping-off place for our effort at social science.

There are other challenges to the basis of existing social science in

modernity. One of them is, of course, post-modernity, which I was a little surprised to note was not even mentioned in any of the preliminary papers. Now, this may be legitimate because "postmodernity" in most of its expressions simply discards the possibility of science, but still, it's had its effect elsewhere, and it does have the virtue, I think, of emphasizing the general notion of representation. In other words, how do you move from supposed empirical data to getting a grasp on it? That's a good question to ask.

Now, having said this, the existing social sciences, modelled on modernity, have indeed borne fruit and, I would argue, scientific fruit. But of course then the question is, what do you mean by "science"? And as Liah [Greenfeld] suggested, you do not want to take physics as your model. I think we have to ask ourselves how far even biology goes as a model and, if that isn't the answer, where do we find our definition of science, so that we can say, "this is not objective knowledge in the physical-science sense, or even the biological-, but it is, in the social-science sense"? That's a whole other argument.

ROSENBERGER: Professor Casanova?

CASANOVA: I read the papers that were distributed and I am going to focus on that of Bruce Mazlish. I have some questions regarding several of the issues he raises. He says, for example, that history needs criteria of relevance to select facts and documents, and that history is scientific because it relies on footnotes and archives, and moreover that the footnote is like an instrument that reinforces the objectivity of history. Additionally, he says that political science is probably not a science because it has a great deal to do with action, political action and institutions, and it's very changeable. And he says that anthropology tries to meet the "Other," and that this discipline is therefore very close to literature.

The first thing I want to say is that I think that every social science relies on history, because every phenomenon that is studied by economics or sociology is a historical phenomenon. Therefore the problems of history are shared by all of the disciplines, because they have to use history to study their respective subject matters. That means that they have to rely on *fides*, on faith—in witnesses, in archives, in footnotes, and so forth. Moreover, I think that almost all of the social sciences, especially economics, for example, have to do with action. For this reason they all share the problems of political science, in that a science that has to do with action cannot be objective.

This does not mean that sciences of action do not produce true knowledge—I am not advocating a Nietzschean approach to these

questions. Yes, the knowledge generated by the social sciences must be true, but it cannot be objective, and I think that social sciences have to recognize that, because all of them have to do with action—all of them. That's not a problem.

I know that Professor Horowitz [paper for Session II] is worried about this because he writes that "sociology cannot be committed to particular actions in a community." That's true, but here comes my second point. We can use the Greek classics as a model, in some sense. We can draw a distinction, for example, between the discourse of Demosthenes and the discourse of Aristotle. The discourse of Demosthenes [*Phillipic* 3 speech] was not so "scientific" as the *Politics* of Aristotle, because the discourse of Demosthenes was committed to a particular aim, which was to guard the freedom of the city-states against the Macedonian threat. Aristotle, in contrast, wrote theoretical works—the *Politics*, for example, or the *Nicomachean Ethics*—and he knew that it had to do with action, but that didn't mean that he was committed to some particular party in any of the Greek cities. The same is true for us. We cannot allow social science to serve particular political aims, but this should not lead us to treat social science as if it were not a science of action.

Moreover, we have to turn to the Greeks because they had many problems similar to ours today. For example, against the claim in the paper by Professor Mazlish, where it is argued that Aristotle had no knowledge of economics except as a tool for the household, I would argue that there was significant economic knowledge in classical Greece. In Athens, for example, there was a big problem with economics because Eubulus wanted to focus politics on economics, preventing Athens from making war against the Macedonians, for example, and Aristotle understood this perfectly and discussed it in the *Politics*, Book 1.

Among the Greeks, who knew many of the problems we have now, there was no wish to do social "science," and Aristotle didn't say that the knowledge of society was a science, an *episteme*; rather, it was philosophy; it was *phronesis*; it was other things. I don't know why we should have an inferiority complex in the face of the natural sciences. In what sense could history be a science if it relies on trust in witnesses? Not in the same sense as the natural sciences. That is obvious. But, for the Greeks, that wasn't a problem. This only means that there are many fields of knowledge and there are many ways of knowing things, and not all of them have to be *episteme*, no?

MAZLISH: Well, the difference between science and scientific method is key, because we're not going to be able to produce social science like physics or even molecular biology or anything. But we can think in terms of scientific method. That's what makes something scientific, but it has to be appropriate to the phenomena you're dealing with. So, that would be the first point.

Next, when thinking of the achievements of the social, the human sciences, or what you will, one of them clearly is that we now know about evolution, and that within evolution, there are emergent properties. That was not particularly well known to Aristotle or anyone else in the past. I don't want to fight the battle of the Ancients and the Moderns again, but clearly, while Aristotle was far more brilliant than any of us, he didn't know that the universe has existed for about 12 to 15 billion years. He didn't know that the human species has got about 2.5 million years behind it, etc. Those are facts. Experts will certainly differ about them, you'll dig up a new fossil and you'll date it a little differently, but those are facts and they should be overshadowing our actions. That is, our actions should be taken in terms of that knowledge, and I think one of the key problems is not that we don't have knowledge of a certain lack of objectivity—even in the natural sciences, the assumption of certainty has been shaken, but in the social sciences you're quite right—we are subjects, not just objects. We study ourselves as objects that take action, and that's where the difficulties arise, but that doesn't make social science unscientific.

G. PREVELAKIS: Liah Greenfeld's introductory remarks are very useful because she pointed to the real problem that we are discussing—the problem of useful knowledge, real knowledge, in order to help our societies. This, of course, is not a new problem; it is a problem that has existed since ancient Greece, and the methodological difficulties of this problem are the same today as they were in ancient times. How do you cope with practice? How do you cope with implementation? How do you cope with the relationship between theory and empirical data? These are not new problems. So, what is new in our discussion?

What is new in our discussion is *modernity.* Has modernity helped us in promoting useful knowledge? And very related to this question, I think, is the term "science," because modernity has introduced this idea that we have to be scientific. That is, that we have to imitate the model of the natural sciences. So, maybe one of the first things that we might ask ourselves is, do we really want to be prisoners of this term *science*?

What are the connotations of "science"? The first, I would say, is

specialization, that little by little we start cutting this search for knowledge into different pieces and separating ourselves from them. The second connotation is the model of the physical sciences. They were so successful, so why shouldn't we do something similar?

These are the issues we are going to be discussing and I don't want to talk for too long so, for the time being, let me just give you some impressions of the state of my own discipline, which is Geography.

Geography is a discipline that is very close to this kind of problem because, as you know, it is divided into physical geography and human geography, which means that we are constantly dealing with the question of what the relationship between the humanities and science is. We have been faced with this problem in a very acute form since the Second World War. The solution that was adopted was very radical in a certain way—in the sense of the radical limitations of the physical sciences: the quantitative revolution in geography. Now, half a century afterwards, we can try to see what has happened with this quantitative revolution. It was an extreme form of the imitation of the physical-science model. Well, of course, this is a subjective point of view, but my point of view is that it has been a disaster. It has been a disaster especially in the country that followed this model most, which was the United States of America. Look at the discipline of Geography, look at the "landscape" of Geography in the United States of America. You cannot look at it, because, I think, you cannot perceive Geography as a field of study—it is nowhere to be found. It is different in France only because in France we have been more traditionalistic by inertia—I am not saying that there is some special merit to that—but as a result, we haven't managed to respond to the challenge of making Geography innovative, but we have managed at least to preserve something of Geography as a field of study.

ROSENBERGER: Professor Glazer?

GLAZER: Some of Liah [Greenfeld]'s introductory remarks, Bruce Mazlish's remarks, and others, led me to reflect on the work of Robert Merton, who Liah referred to as a sociologist of science, at least in his earlier career; and of my efforts to deal with the sociology of science a very long time ago when Merton, along with other leaders of sociology, Samuel Stouffer and others, was involved in producing large volumes of studies on the American soldier in World War II. These were basically a form of public-opinion research, and four volumes were published, which I reviewed at the time—this must have been at some time in the '50s—and they made me ponder this question of science

because great claims were made for these volumes. Merton did have the model of the physical sciences. As I recall, in classes with him he would try to indicate where sociology stands in its attempt to become a science. And he would insist that it isn't even up to Copernicus yet; and it certainly isn't, or wasn't—and that was 40 years ago—but I don't think it's gotten any farther since.

Like Bruce Mazlish, who has gone further with it, the difference between a science and scientific method was something I illustrated in reviewing those volumes by referring to a baseball player who is now forgotten: Stan Musial—a very fine batter, of whom the newspapers said, "He is a scientific batter." His science consisted of the fact that he had a file catalogue of every pitcher he faced, and of each one's characteristics. My point was that you can apply scientific method to almost anything, you can organize it, you can try to be more than anecdotal. He wasn't taking one pitcher, he was taking all pitchers he had faced. You can try to make a thorough record and reflect on it, and that's why I thought the reference in Bruce's paper to the footnote, minor as it appears, is very important.

But now the question is, having applied scientific method to various subject matters, where do we get? Certainly the ambition to get beyond that to something more systematic, general, predictive if possible, and so on, is admirable; and I recall that Merton did not let *The American Soldier* just lie there with his claim that it was making sociology more scientific (which I must say I disputed in my article but that's another matter): he then commissioned, and was involved in commissioning, followup volumes of critical articles, and he had a general name for the series: *Studies in the Continuity of Science*, because if it is science, you want to be able to correct it.

Let's go further. There's a volume in that series in which he attacked my article; and there was a volume on *The Authoritarian Personality*, which some of us may recall—a huge volume, by Adorno and others, and I wonder if American social scientists selected these topics as being possible building blocks towards a science. By the way, there was also a volume on *The Lonely Crowd*, in which I participated, which was getting pretty far from science—I mean, compared to *The American Soldier* with its public-opinion research; and compared with *The Authoritarian Personality*, with its psychological scales and so on. Well, my conclusion then was that in the social sciences, what we have is legitimate studies of subjects but that we are always aspiring to something more—and I have been pondering a question that I haven't thought about in a long

time and only because of this conference and these papers: What is the something more we've gotten to?

And here I may be on the other side of Mr. Prevelakis's point. One of the things we have gotten to is the ability to attach numbers. Now, sometimes, if you think of, for example, public opinion, well, censuses, that's a subject: how many of us are there? And we have been able to do better at it since 1800 than we did before, and we keep on doing better and learning more and more problems with it. Or: what do people think? Where do they stand? I think that over the history of public-opinion research we do see progress in our ability to attach numbers to those things—and in David Landes's paper, attaching numbers to wealth, and the progress of wealth, and its distribution—so, we have done, I think ("we"—I mean the social sciences) have done pretty well on this, and the question is whether we can get beyond that much. My orientation has been less on whether we are approaching a scientific model, and more on simply studying problems. Prevelakis spoke about problems; and, when we deal with problems, I don't know how far theory gets us, or if the theories in circulation get us anywhere. In economics maybe, as we all say, but not in other areas.

I'm always impressed when we have some interesting, difficult problem and journalists try to get social scientists to say something more about it! I think our experiences are always that this "something more" seems to be very thin as compared to concrete knowledge of the problem itself. Well, I've said enough about the question of how far we can get beyond attaching numbers and giving ourselves a little more security as to the empirical situation.

ROSENBERGER: Professor Prevelakis?

G. PREVELAKIS: Yes, let me explain what I mean. . . . I tried not to speak too much but it will be necessary to return to my point, to avoid misinterpretation. Of course I cannot disagree with the use and with the utility of statistics, numbers—all those things are extremely useful, and we have made progress because of them. The problem is whether this kind of mathematical methodology is used as an instrument, or if it becomes an alibi for not asking other questions.

I don't want to generalize because my other colleagues obviously can speak more for their own disciplines. I'm speaking about Geography. The problem of Geography is that the quantitative revolution led to an abdication of truly theoretical research, because suddenly we thought that through mathematics we would solve all of the problems. You cannot solve the problems, the deep theoretical problems, with mathemat-

ics. All you can do is use mathematics as an instrument; it can add a little, and this of course has led to a severe crisis because at a certain moment we realized that the wonderful formula that would express everything in all the aspects of geographical space was not possible. When we found out that it was not possible, of course, we were left with a lot of progress in the sense of instruments, and this is very real progress, but at the same time we found that we had lost a large part of our heritage, and that the real problems, the big problems of the discipline, had not been addressed.

So I don't disagree on the use and the necessity of the scientific method, and so forth, but what I think is a problem is the kind of mystification of those instruments which leads, first of all, to an abdication, and then also to a certain kind of, I would say, self-censure, or censorship, in the sense of a reluctance or refusal to use other capacities or possibilities.

I tell my students, okay, you have to master the scientific method, but also don't forget that there is intuition, that there is introspection, that you have to use all the capacities of your self, scientific and nonscientific, and the final criterion of success is, for me, the relationship with reality—and in that sense, of course, it is true that very often we are not able to respond to the demand for answers, and we see all the time on television university and academic professors who cannot offer substantial answers to questions. There are exceptions, of course; it's not always like that, and I think good scientific work leads to answers and leads to intuitions that can be useful for society. The problem is that in the last century we substituted not only useless but also disorienting elements for important ones, because of this kind of inferiority complex towards the physical sciences, and what I think is necessary is to free ourselves, in various directions, but not to destroy the progress we have gained with this kind of imitation of the physical sciences. This is a good thing. We have to keep it, but we must open up to other dimensions, in the sense of a tradition that goes back to ancient Greece, and also in the sense of opening to other disciplines, to other sciences, unifying the field of what was called, in the past, humanities.

ROSENBERGER: Professor Kaufman?

KAUFMAN: I'm here from the sociology department at Harvard and, since I crossed the river this morning I'm going to cross the Rubicon—yes, I want to propose the idea that it's not truth that we should be concerned with. Journals in the social sciences are filled with true observations—true but uninteresting. I think what we want to search

for is charismatic truth, and I use "charisma" here in the Weberian sense: truth that transcends the mere criteria of empirical validity, and speaks to wider conditions, speaks to the human condition, speaks to the deep questions in the social sciences.

Now, how do you identify charismatic truth? How do you train people to do that? That's probably an unanswerable question, but I do think the worst thing we could do is try to emulate the factory model of truth production that occurs in the natural sciences. I think it's appropriate in the natural sciences, but in the social sciences what we need are visionary thinkers who ask the right questions, not armies of people providing correct but irrelevant, uninteresting answers.

ROSENBERGER: I think there has been a slight blurring here between imitating the natural sciences and the scientific method. There is no reason to think that it is not possible to develop a scientific method that is not necessarily as based on quantification, as the natural sciences are. Why shouldn't we distinguish between scientific method—the simple ability to falsify data, to develop interesting questions, perhaps even to reach "charismatic truths," on the one hand—and the ability to attach numbers on the other? These are, I think, two different projects, and one of the interesting questions would then be why have we blurred that distinction.

Why have the social sciences become so obsessed with the positivistic, and what does this have to do with their position in the universities vis-a-vis the natural sciences? There is something very interesting going on when, as I forget who pointed it out, the journalists will call up social scientists to get at some sort of deeper truth on a pressing social issue, and the social scientists tend to not have something to say that seems to matter to the broader public. Professor Landes, I see you nodding your head there, is there something from economics that you could speak to on this?

D. LANDES: Economics is perhaps the field that has moved farthest in the direction of mathematicization of data and analysis, and indeed it would be most difficult for a bright economist of large thought and vision to make it in the profession if he couldn't show that he was a master of mathematics. That's really changed things considerably in a field like economic history, which once dealt with both sides of that designation; that is, they did economics, and history, and so for example I spent the last part of my career in the economics department—well, I had reasons for leaving history, but I remain a historian. Still, I was able to live with economists, and talk with economists, and so on. I don't

know if they'd let me in the door now, and this has posed a real problem for the position of economic history as a discipline. I see it in many ways even more in Europe than the United States, because the Europeans have really been ready to exclude economic history from economics, and to isolate and diminish that part of the discipline.

There are changes taking place here of a most serious character and they have to do, I feel, not only with what you need to know, what you need in order to know, but also with what you need in order to seem smart, to be the kind of person who people respect because they don't entirely understand you. Yes, that's what's going on.

ROSENBERGER: Professor Friedman?

FRIEDMAN: Is Professor Landes talking about more positivism in economics, as you [Prof. Rosenberger] put it, or less? The trend in political science and, for all I know, other fields too, is toward *less* quantification—more mathematicization but less quantification. Mathematicization means coming up with theories about the way things would be given certain assumptions. Quantification is, if anything, the opposite of that. It's actually going out and investigating whether things are a certain way, and if so, how much.

I think it would be a mistake for us to attribute the problems in the social sciences to quantification. Before about 1964, when rational choice theory began making its incursions outside of economics, it's true that there was a provincial, quantificationist positivism—which just goes to show that any method or approach to method can become a fetish, and can start excluding people who use different methods. It used to be true, and still is to some extent, that people who tell narratives, people who use case studies with an N of 1, or even people who do comparative case studies with an N of 2 or 3, are looked down on by quantificationists, who want an N of 60 or 600. But even though people can be provincial in that way, all of those people are at least in the same ballpark. They're all trying to amass empirical evidence to support or disprove empirical claims.

What the mathematicians are doing is assuming things about reality that they don't even attempt to prove. They require the higher mathematics, rather than simple counting-type mathematics or even advanced statistics, in order to spin out logical implications of a few premises that they posit as being applicable to all human beings. I think the problem here is one that quantifying, positivistic social sciences are actually immune to.

The problem with the mathematicization of social science is the

assumption that what science means is the search for universal laws. The reason that they imitate physics is not that they look at all the rocket ships that the physical sciences have enabled us to build and say, "Gee, we social scientists should be able to do that too," I think, so much as they are simply misunderstanding what science means. They see that in the physical sciences there is the assumption—not the proof, as the problem of induction shows—that the universe is uniform in all times and places. On the basis of that assumed uniformity, natural scientists make predictions, and are able to not only future predict but retrodict the way things *must* be or *must* have been because of those uniformities. So social scientists have tended to look for the bases of universal social laws that will allow them to predict human behavior, both future and past.

Then what they end up doing is spinning out the logical implications of these "laws" rather than seeing whether they actually apply in the real world. When it comes to any confrontation between the assumptions behind the so-called laws and the real world, they tend to close their eyes to the contradictions and—in my field, political science—say, "Granted, it isn't instrumentally rational for people to vote in an electorate of more than a few people, because the likelihood is that none of their votes will make any difference, but when we see millions and millions of people all around the world voting all the time, it *must* nevertheless be some manifestation of their instrumental rationality"—because instrumental rationality is the assumption behind rational-choice modeling. They assume this rather than actually investigating what is going through the minds of these people who are voting. They assume some form of instrumental rationality must be at work; and when it really is not at work, they end up defining instrumental rationality more and more broadly so that its universal laws have less and less content, in order to preserve their universal applicability.

So that's a problem, but I think it will probably pass as just another fad; though a larger lesson might be that the reason for the occurrence of fads like this, and of pendulum swings to fads of the opposite type—like postmodernism, which says that there can be no such thing as science at all—is the idea that if social science is to be scientific, then it must be looking for universal laws of behavior.

ROSENBERGER: Professor Wood?

WOOD: The emphasis that we've put on the longing of the social sciences to be scientific and thus the turn in the conversation towards quantification or mathematicization, and the like, prompts me to want to remind you that one of the disciplines under consideration is an-

thropology, and probably among the social sciences represented here there is none that has gone so far in the direction of repudiating any claim to be a science. Of course there are exceptions—small specializations within the field that don't do this—but quantification, mathematicization, empiricism itself, the piling up of facts, the search for scientific laws, are in bad odor in anthropology. There are a couple of other anthropologists here, and I hope to hear from them on this matter, but whether it is called postmodernism or something else, it has taken over the field of anthropology almost completely—with the exception of those pockets of anthropology like the study of evolution, and archaeology, where there is a kind of wall that blocks them off from what most anthropologists talk about.

I don't want to exaggerate this case, but anthropology provides or, in its foundations, once provided maybe the most encompassing claims of the social sciences. It was going to take in the whole world, all of time, and examine the basis of common humanity, which would, in principle, include economics, politics, psychology, and so on. But now it has dissolved almost entirely into very privatized thematic studies of particular peoples and particular places and eschews the search for universal laws. I don't know if that means that we just decide to move ahead with the social sciences, forgetting anthropology, relegating it to some small corner of the humanities, or whether there is something fundamental here to be challenged; but I just thought I would get it into the discussion.

ROSENBERGER: One point that I would like to raise, too, to bring some of these themes together, is the question of what people might cite as a particular achievement of any discipline in the past century, and what that achievement shows us about the ways the disciplines can function. What are the great insights of the past century? And how did they emerge from the university academic setting and how did, how were those achievements structured scientifically? [*Long silence.*]

ROSENBERGER: I hope somebody is going to tell me that their discipline has achieved something in the past century. Professor Prevelakis?

G. PREVELAKIS: Well, in this session we are speaking about the record of the social sciences, so you are right to bring us back to our theme. Why are we asking ourselves this question? I think it is because we feel that we are in a transition period, and we want to ask ourselves the question, "What have we done in the past?" before looking towards the future.

So the record is the record of a certain period—a certain period that

starts when? I would say it starts with modernity, or the nineteenth century, which gives this status, this new status, to the social sciences—and we have the feeling, an intuition, that maybe this spirit is at its end. So we ask ourselves what the social sciences have done.

Well, I would say that the major achievement of the social sciences is not scientific. They have instead been a major instrument of ideology, and ideology is not useless. They have helped in keeping our societies together and in structuring the nation-state, and so I think the ambition was not really scientific—it was rather political ambition. If we take—again I'm speaking from my experience—if you look at the historiography of the Balkans, it is pure ideology. It very often reflects extraordinary imagination. I'm Greek so of course I know the Greek case very well. The father of Greek historiography was Paparidopolous. Paparidopolous completely—I will not say "deformed" the history—he created a historical myth. It was a feat of imagination. One cannot avoid admiring the man, but was it scientific in the sense that we are discussing? However, this has been a useful achievement in the sense that he created a myth that kept the Greek society together and has been very useful for Greek nation-building, state-building, etc. So my answer to the question would be, there has been an achievement, although it has not been a scientific achievement in the sense that we are speaking now. It has been a political or an ideological achievement.

ROSENBERGER: Professor Mazlish?

MAZLISH: If I may comment on this last comment. One, the fact that Professor Prevelakis can see this other work as myth is itself a scientific step if you like. . . .

G. PREVELAKIS: Maybe because we are in a transition period. I don't know if I would have 20 years ago. . . .

MAZLISH: I think it may be helpful to remind ourselves of the historical background of the emergence of something called social science. History itself, as my friend here knows, is a Greek achievement—what we call scientific history. Herodotus certainly used myth, but between Herodotus and Thucydides you try to substitute history, inquiry, for myth, and that's a giant step. Then there's another revival of history in the seventeenth century—really the eighteenth century, but it's the seventeenth-century scientific revolution, with regard to natural phenomena, that serves as the model and inspiration, because look at the wonders of that achievement—that's really significant. So, by the eighteenth century, the question is, can you apply this same approach to social phenomena?

That's a worthy aspiration. Whether it's realistic is obviously something that we need to talk about, but the model that was available was of a seventeenth/eighteenth century science, which did tend to be positivistic. Natural science has changed incredibly, of course, since then, and the social sciences have tended to lag badly—that's the real lag, in many ways. But I think it still is a noble inspiration that we should hold. That is, we recognize that there is something called society, and that's a new recognition, it doesn't really come until about the seventeenth century—that it is not created by God, it is not something given. It is a creation of man—that is a major, major jump. If so, then we have to understand how we affect this creation.

As I understand it, you're quite right, ideology is a constant monster, lurking over all of us, but the whole point of the effort of science is to identify it, isolate it, to minimize it. We're not going to make it go away, but I think it is useful to remind ourselves what the scientific enterprise is, and what its roots are.

ROSENBERGER: Professor Glazer?

GLAZER: I wanted to suggest as a followup a kind of case study for the discussion, or commentary, on the achievements of the social sciences. I did mention quantification, that is, adding numbers—which is, I know, different, and Jeffrey Friedman's point is very important, it is very different from mathematicization—but I think that one big change, almost in our lifetimes, beginning let's say in the '20s, has been the elimination or the radical reduction of racist interpretations in almost all of the social sciences. In sociology, in history, the assumption of the significance of race (although the term used sometimes was "tribe" or "people")—the assumption that some genetic inheritance will explain things—has diminished radically. Now I think we can consider that an achievement, an achievement based on scientific advance; we don't study skull shapes as much as we used to, we don't assume they mean something.

A number of interesting questions come up. One, is it a real achievement? Second, is it merely an ideological achievement?—there was Hitler, so we're not going to talk about these things anymore? On the other hand, with the new genetics and sociobiology, are we back to racial interpretations again? But in view of how significant race was in the social science of the late nineteenth and early twentieth centuries, it would be an interesting thing to consider how we have fared in that respect. I wonder if anyone would like to make a comment on that.

ROSENBERGER: Peter?

WOOD: Well I'm skeptical about that claim in several ways. One is that I don't think that the racial theories have really disappeared. They've just taken on new forms. Even leaving aside the neo-racialists of the far Right, the anti-immigration groups that are keeping alive racism as it was formulated 60 or 70 years ago, there is the racialism of the Left, which keeps alive the categories of race for its own purposes, but culturalizes them. And even when we're not culturalizing the category of race and keeping it alive in that form, the very idea of culture has taken on a kind of homology to race. Much of what we do in anthropology these days just puts race in the place of culture, or vice versa, but if you can explain people by their particularity, by focusing hermeneutically or otherwise on specifically what some small social group is doing, you really haven't gone very far from the idea of race as it was being explicated from Lord Kames in the eighteenth century on to what Chamberlain and the other proto-Nazis were doing.

The idea is that essentially destiny lies within a group. Whether you define that group in quasi- or pseudo-biological terms, or in cultural terms, doesn't seem to me to make a profound difference. So, I don't think that we've made that advance. We've simply replaced one pernicious theory with another, changed the label and kept the substance.

ROSENBERGER: Professor Banuazizi?

BANUAZIZI: Let me address two of the pathologies of the science of Psychology.

Currently it ignores the historical dimension of behavior, almost entirely. That is, if one looks at contemporary psychological theory, the one presumption in it is that whatever laws or commonalities of behavior are discovered through psychological research are applicable to all times. The kind of research, the kind of theorizing that one sees in Norbert Elias, or in Vico and Herder, that recognizes the historicity of the human mind and mentality, has almost entirely disappeared from the discipline of psychology. I don't know of anyone, any contemporary psychologist who writes from that perspective anymore.

The other pathology is—surprisingly, perhaps—the disappearance, contrary to the example of anthropology, of treating the notion of culture in a serious way. Serious in the sense that if you look at the principal discipline or subdiscipline within Psychology that deals with human social behavior, considerations of culture are almost entirely secondary or accidental. It is a fact that, according to some recent studies, something like 85 to 90 percent of social-psychological research published in the United States today has a subject base that is entirely limited to col-

lege students, and specifically college sophomores—this is not an exxageration. [*Laughter.*] Sophomores, right. So, we are, for all intents and purposes, dealing with a discipline whose subject base is a very narrow slice of humanity, yet it has the ambition of universalizing its findings.

This has all kinds of consequences, substantive consequences. I will mention one or two.

Think about conceptions of the self, which is a very interesting, a very active topic in the field today. Those conceptions, when based on college students and college sophomores, obviously take on a very different tone than, say, if you were focused on Chinese farmers and their conceptions of the self. Take questions of attitudes and attitude change. Quite clearly, college is *about* attitude change, so one might expect that the dynamics and the laws and the theorizing that take place around this very central concern in social psychology—which is the study of attitudes, values, norms, and so on—would have a very special development, appearance, and character in the context of the college. But in fact, the studies of attitudes are almost entirely like everything else in social psychology: based on that terribly narrow and time-bound subject base.

So those are two small . . . well, I think, actually quite large, consequences of the adoption of this universalistic, positivistic model—the ahistorical model—in a discipline that is probably one of the most advanced in the social sciences in terms of the sophistication of its methodology.

D. LANDES: I'll try to be quick because I know that the plan is for a break at the half-hour, but I want to follow up; forgive me if I jump back to what Professor Wood was saying. The most important word in what he said was *pernicious.* He said of all these "findings" about characteristics attributed to a group joined by relationships, or race, if you will—it could be a smaller group within a larger group—in any case that these so-called findings are *unfortunate*. I wasn't sure whether you [Prof. Wood] were saying that it was unfortunate because it was untrue, or simply unfortunate because we don't want to think of the world as divided in some way by inheritance, by race, or by culture—and I had something of the feeling that you were negative on culture too.

But as a historian I have the feeling that culture counts, it really matters, and it does differentiate people among groups, and it can, in fact, be judged by certain results, and my question is, does it matter to have group differences that are in fact shown by results? Now I want to take

an example that you may disagree with, but I have the strange impression from what I've seen on TV screens and in other media, that there are some races that do better in certain sports than others. I find that the proportion, say, of blacks in American football is extremely high, and I also have the feeling that it's justified. They really do run faster, jump farther, and generally make the difference, and all of the coaches in the business know it. Now they don't worry about whether or not this is good for society, they want the best players they can get, and they look for ways to make them eligible for admission and hence for athletic participation, and I think, on the whole, that however that may make some people feel bad, it is more normal that you have criteria, physical criteria, that some groups will do better than others.

Now, culture, of course, is something else again. No one wants to think that there is any difference in intellectual or mental ability among different groups. We just feel that that is pernicious, if I may copy the term, but if someone were to put forward the thesis that all groups are equal intellectually and that there are no differences, that if you took some measure they would all come out to the same figure, would that be a racist argument? I mean, is the world necessarily equal in this respect? We know it would be nice if it were, but must it be? Would you expect it to be? I don't know, I put those questions forward by way of causing trouble.

MAZLISH: You've achieved your purpose. [*Laughter.*]

ROSENBERGER: I think Professor Greenfeld wanted to make some comments on the possible relevance of the study of culture.

GREENFELD: No, I want to conclude the session on the record of the social sciences not on such a belligerent note (which is wonderful and we're going to fight later), but by indicating the astonishing agreement here that, even though the social sciences have perhaps made some debatable ideological contributions, there is no "record" at all in any of them since the day of their institutionalization as social sciences within the universities, around the end of the last century. No contribution to our knowledge of the subject matter in which they were supposed to contribute! I gather this from the fact that the practicing representatives here assembled of all those disciplines have nothing good to say about any of their disciplines in this respect in the twentieth century. They made some contributions before they were ever defined as social sciences, and certainly before they existed as academic disciplines—in Ancient Greece, that's good; some in the seventeenth century. Since the seventeenth century, ap-

parently, we have not at all progressed in our knowledge of humanity. So we end our opening session, on the record of the social sciences, on this very sad note.

SESSION II. THE ACTUAL PREOCCUPATIONS OF THE SOCIAL SCIENCES

INTRODUCTORY REMARKS: Jeffrey Friedman
MODERATOR: Jonathan Eastwood
PARTICIPANTS: Carlos Casanova
Jeffrey Friedman
Nathan Glazer
Liah Greenfeld
Natan Press
Nathalie Richard
Dennis Smith

Papers distributed to the participants for this session:

Friedman, Jeffrey. 1996. "Introduction: Economic Approaches to Politics." *Critical Review* 9 (1-2): 1–24.

Greenfeld, Liah. 2002. "How Economics Became a Science: The Surprising Career of a Model Discipline." In Amanda Anderson and Joseph Valente, eds., *Disciplinarity at the* fin de siecle. Princeton: Princeton University Press.

Horowitz, Irving Louis. 1995. "Decomposition or Reconstruction of the Social Sciences?" *Schweierische Zeitschrift für Soziologie* 21 (1).

Lipset, Seymour Martin. 1994. "The State of American Sociology." *Sociological Forum* 9 (2): 199–220.

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Prevelakis, George. 2002. "Jean Gottmann's Relevance in Today's World." www.bu.edu/uni/iass

Seligman, Adam. 2002. "The Self in the Social Sciences." www.bu.edu/uni/iass

Wood, Peter. "What is Anthropology?" www.bu.edu/uni/iass

FRIEDMAN: I'm not at all sure I know what is actually going on in the social sciences. In the one that I'm most familiar with, political science, I find myself wondering a lot: What in the world *is* going on? So I leave it open to people after I speak to detail what is going on in the various social sciences, if they have the taste for it.

My general impression, though, is that at least two things are going on. The first is one or another form of scientism: the insistence, or the

implicit assumption, that there must be universal laws, and that the goal of the social sciences is to find them.

That, I suppose, leads generally in two directions, one of them being a-priorism—as in rational–choice theory in economics and in political science; another being historicism, although this is an older and, I take it, not very contemporarily popular view: looking for a-priori laws of history.

The other main thing that seems to be going on, whether as a result of dissatisfaction with scientism—for instance, recognition that laws of history have not been discovered—or as a result of something else, which I will discuss—the other thing that's going on is politics.

I want to focus primarily on one sentence in Seymour Martin Lipset's article. It occurs on the bottom of page 209: "Academia as a whole is, of course, considerably to the left of other occupations and professions, a fact that is related not only to the emphasis of leading scholars and intellectuals on innovation and creativity, but also to selective recruitment." Two hypotheses about the left-wing politicization of the social sciences are offered in this sentence: the scholarly emphasis on innovation; and selective recruitment into scholarly pursuits. I want to take them in turn and criticize those hypotheses, though I know that there is a good deal to be said for both of them. I'd like to provide a bigger picture that would lead to other reasons for the politicization of the social sciences.

It seems to me that innovativeness certainly wins academic prestige, but only when it is innovation well within left-wing normative and empirical assumptions. To take some examples old and new: Weber, Schumpeter, Hayek, Lippmann, and evolutionary psychologists, it seems to me, are far more innovative than most of what goes on and what gets the plaudits and the endowed chairs in the social sciences today, and yet they are virtually unknown or are routinely dismissed (or, in the case of Weber, merely gestured at respectfully). If you go along with me on that, then it can't be innovativeness alone that is being rewarded; so the reason that left-wing politicization proceeds isn't because it is more innovative. Moreover, if that were the cause, it would raise the question of why left-wing thought would be inherently more innovative. I think the way to answer that question is just to say that it is innovative left-wing thought—whatever is a new twist on leftist worldviews—that becomes popular in scholarly circles. But that doesn't answer the question of why the leftist politicization is in place to begin with, which sets the

boundaries that determine what sorts of innovations count as worthwhile.

As far as selective recruitment, from the context of the article it is unclear if Lipset means selective recruitment of leftists into sociology in particular, or social science in general, or the academy in general. There is however a passage Lipset takes from Hayek on page 210, where Hayek says that there is selective recruitment of the smartest students to become left-wing professors in general—but this begs the question of why. Why should the smartest people become scholars, and why should the smartest scholars become leftists?

Of course one answer is that left-wing views are obviously right, and smart people are intelligent enough to recognize that. That is a possible answer—it has been suggested to me a few times in my life—but I am going to table it for now, and if anyone would like to defend it please feel free. Instead I'm going to consider "societal" explanations, in order to criticize them and come up with an alternative explanation.

One societal explanation for the left-wing politicization of the social sciences would be that there is something about modernity or industrial society that leads to leftist thought. That might have been plausible at one time, but it seems to me that if there is one thing about modernity, if you leave out what I want to leave in and get to later—that is, if you leave out the *ideas* that are peculiar to modernity—leaving those aside, and leaving science aside, the one thing that is most peculiarly modern would have to be the economy: prosperity, the immense increase in wealth; and yet we don't find intellectuals, for the most part, gravitating toward economics, and when we do, there is that astonishing table in the Lipset article, on page 214, that shows that the political views of economists are almost diametrically opposed to those of the other social scientists, other than psychologists. For some reason economists and psychologists are overwhelmingly non-leftist, according to Lipset's way of parceling out who is a leftist and who isn't; whereas sociologists, historians, political scientists, anthropologists overwhelmingly are leftist. So it doesn't seem logical to me, at least at first cut, that there is something about modern society, at least in the sense of the economy, that would account for the politicization of the social sciences, or at least their left-wing politicization.

What about post-modernity? Is there something about post-industrial society, such as the fact that we now have so many knowledge workers, so many manipulators of symbols? Is left-wing thought somehow more inherently attractive to them? It *is* more attractive to them,

as an empirical matter, but the key question is, is it inherently attractive to them, given the nature of things; or is it just that they happen to be attracted to it, contingently?—which is what I will argue for in about three minutes.

But just to consider for a moment the possibility that there is something inherently congruent between a post-industrial world in which one deals with words and manipulates symbols, and left-wing thought, let me propose to you three different ways of viewing society.

The premise of all three is that a societal explanation for the left-wing views of society held by symbol manipulators would have to lie in the complexity of those views, which only someone with complex thought processes could master. So I am now going to give you three different views of society and ask you to judge how complex they are, and then compare how likely they are to attract post-industrial knowledge workers, including members of the social sciences.

The first view I will call "intentionalism," for lack of a better term. This view is that bad things happen to people in society because other people do bad things to them, and that good things happen as a result of good people, who have a social conscience, undertaking social activism.

It seems to me that that is a very simple and straightforward view, and yet it is probably the view that commands the allegiance of most of the politicized faculty members and students that Lipset and Horowitz and the other papers referring to social-science politicization are talking about.

Okay, here is a second view, much more complicated: good or bad things happen because those things serve people's interests. So it's not that bad things happen because bad people maliciously do bad things to good people and activists have to come to the rescue. It's that bad things happen because they serve people's interests, whether or not there is any deliberate effort to make them serve their interests. This is functionalism.

Obviously Marxism is an example, but before I get to Marxism I want to refer to Durkheim in *The Division of Labor*, where he makes the claim that the correlation between societies that have a complex division of labor and societies that have a complex regulatory state must indicate a causal connection between the two, mediated by social need. There is some way in which complex societies that have a complex division of labor *need* a complicated regulatory state, and *therefore* the modern regulatory state (somehow) emerges in order to meet that

need. This is a functionalist theory of society. The state serves the function of regulating the economy because the economy needs to be regulated. Another example would be the medieval view that the division of society into estates serves social needs. In both the Durkheimian and the medieval view, everybody's needs are being met by the existing institutions.

Now, the Marxist view is not that different, it's just that Marx says that instead of everybody's needs being met by existing institutions, only the needs of the ruling class are met. But there is the functionalist element that somehow the needs of the ruling class get turned into laws and institutions, and if you actually look for people who do it, consciously—people who say: "I am going to go about making the state the handmaiden or the executive committee of the ruling class"—it is very difficult to find people actually meeting in smoke-filled rooms trying to do that. There is in Marx a functionalist assumption that whatever needs to be done for the preservation of capitalism will be done, until it can no longer be done and then the system will be overthrown; but there is a notorious difficulty in identifying who it is, exactly, who figures out how to match structure to function, and then implements the institutions that make the match.

My last example of functionalist thinking is on the other side of the political spectrum generally speaking, and that is the economist's assumption that individual behavior, or the political scientist's assumption that individual voter behavior, in markets or in democracies, produces results that serve individual self-interest. Again there is a functionalist explanation for the actions of people in markets and in democracies, in that their actions automatically serve the interests they seek.

Now all of these variants of the second view of society, it seems to me, although being less simplistic than the first view, are still extremely simplistic. They are almost magical, because there is no answer to the question, or no good answer to the question, of how it is that individuals, let alone classes, let alone entire societies, know what their interests are and know how to implement them—let alone any evidence that, having made these recognitions of what their interests are and how to implement them, people who are imbued with that understanding then go about creating those institutions for that reason. There is a marvelous passage in Durkheim, in *The Division of Labor*, when he pauses to consider this question, and he gives us a picture of democratic politics as a matter of the members of a society getting together, and the words he uses, translated into English, are "solemnly deliberating" about what

their collective interests are. Then "society" imposes the proper regulations, and that accounts for the thick lawbooks of regulations that Durkheim points to as the corollary of the complexity of advanced economies, in order to reach the conclusion that there is a functional relationship between the two.

In Marx we have the same kind of idea: proletarians, just by virtue of working in the factory, and perhaps looking around and seeing other people working in the factory, will somehow intuit Marxist theory without reading Marx.

Now Foucault, as a member of the New Left, like many others felt a need to dispense with the Marxist theory of history because it had been used to justify Leninism, and therefore Stalinism, and therefore tyranny; so he gives us Marxism minus the philosophy of history, and instead what you get is the implication that disciplinary institutions, like the prison, the hospital, the school, which Foucault asserts serve the interests of the bourgeoisie, come into being without anybody creating them, or recognizing the need for them. They just happen: a new *mentalité* emerges, new institutions emerge along with the *mentalité*, and they serve bourgeois interests. But nobody seems to have recognized this at the time these institutions were being created, or at least there is no evidence that anyone with such a recognition is responsible for having created these institutions.

All of these functionalist views of society—Durkheim's, Marx's, Foucault's—are magical, because they all assume that people act without having to have ideas in their heads about what their interests are. That is how these functionalist theories of society make good things or bad things happen—just "because" they serve people's interests. There's a completely unexplained correlation between a need—whether it is the need of an individual, or of a class, or of a society as a whole—and the requisite, functional actions or laws or institutions. And correlation is not causation unless one believes, in effect, in magic.

The third view that I am going to propose is that good or bad things happen because of *theories* that people have in their heads about good or bad things: theories about what would be good ends, normative theories; and theories about good means—empirical theories about good instruments for achieving those ends. These theories that people have in their heads are ideas. They come from somewhere—not necessarily from a direct, unmediated perception of their interests, in fact almost certainly not from there—and therefore these ideas don't necessarily

magically track the idea-holders' interests, even when they are ideas about what is in their interests.

These ideas, if they don't come from unmediated contact with the reality of what one's interest is, or what a society's interests are—where would they come from?

My suggestion is that they are path-dependent, meaning that they are contingent on history; meaning, in concrete terms, that my ideas about what are good ends and about what are good means and institutions for achieving those ends are contingent on what I have been *taught* are good ends and good means, by my parents and friends and formal educators, and what I've *read* are good means and good ends, and what I have picked up from movies and television and the newspapers are good means and good ends. And what determines the ideas of my teachers and the editors of the newspapers that I read and the directors of the movies that I watch, what determines the ideas that they teach me are, well, the movies that they watched, and the newspapers that they read, and the education that they received, and what their parents told them. So there is an endless path leading back into history: Who taught your teacher? Who taught your teacher's teacher? What movies did—oh, his name escapes me—"JFK"?

SOMEONE: Oliver Stone?

FRIEDMAN: Yes, what movies did Oliver Stone see when he was a kid? What movies did whoever directed those movies see as a kid, and what books did they read?

Now the first theory of society I called intentionalism; the second theory I called functionalist; and the third view, which I have just endorsed, is culturalist. Or, with some apologies, one could call it Weberian. Apologies, because I think Weber gave too much credit to the Marxist view that people's self-interest is one variable, one important variable that should always be taken into account. I would like to maintain that people's self-interest is always something to be mediated by their ideas about their self-interest. Not that there aren't other elements that enter in, like their psychology, and this is where evolutionary psychology or any other form of psychology may come into play; but it is not as if self-interest is the simple thing that Marx thought it was and that, I think, Weber gave him too much credit in allowing for.

That said, the cultural explanation focuses, as Weber did, on people's ideas and on the history of them and, therefore, the contingency of them.

Now this theory is the most complicated of the three. So you would

think that if there were something about post-industrial society that explains the views of society that people in post-industrial society have, then knowledge workers who like to deal with complicated symbolic manipulations would be drawn to this third, culturalist view—and yet very few of them are. You see very little examination in the mass media, let alone in the social sciences, of the cultural sources of the ideas of the actors being investigated. You see very little examination of our *own* ideas, the ideas of us social scientists, and of where they came from; very little recognition that we, as social scientists, even *have* ideas—theoretical perspectives that we were taught by somebody—and that they may therefore be questionable: things we believe, like the existence of patriarchy, aren't just obvious, concrete realities.

It doesn't seem to occur to us as social scientists that our perceptions are based on theories, that not everyone in every time and place would have those particular theories, and that it is an interesting question, perhaps, where we got our theories, and whether they have anything more to say for them than that they are just what "everybody around here" believes, in this time and place, in Richard Rorty's famous words endorsing theoretical complacency.

So that's the first thing I would like to say about the third theory of society—the culturalist one: that it is relatively complicated, yet generally overlooked by symbol manipulators. It seems to me that its lack of adherents in the social sciences is strong evidence against a "postmodernity" explanation for why we have the politicization of the social sciences that we do.

The second thing I want to say about it is that this cultural approach is potentially *subversive* of left-wing or any other kind of politicization of the academy, because of the fact that by problematizing theories of society, by making them something more than unmediated reflections of obvious realities like class self-interest, or the interests of patriarchy, it calls into question where we got the idea that those interests are realities that are unmediated by theories—and it makes us question where we got those ideas. It directs us to not only a path-dependent, historical, idea-focused history of the phenomena that we are studying, but it makes one of the phenomena that we should be studying our own ideas, including the idea that ideas reflect things like unmediated class interests—unmediated by ideas—to which, as academics and social scientists, we can apply the same path-dependent, idea-oriented methodology.

The penultimate thing I want to say about a cultural approach is that

it offers an explanation for the leftist politicization of social science that isn't magical, like functionalist explanations of that trend would be, and that doesn't beg the question, like the self-recruitment explanation of that trend. The culturalist explanation would be: it is not that smart kids choose to go into the social sciences and then for some mysterious reason become leftists, or that for some mysterious reason they start out as leftists and then tend disproportionately to go into the social sciences. Instead, for path-dependent historical reasons, people in our culture get exposed to a wide variety of left-wing ideas and very little in the way of sophisticated alternatives, and the more intellectually engaged they are, the more sophisticated are the left-wing ideas that they are exposed to, so it is no wonder that they would want to go off and study them. If we were in the Middle Ages, smart kids would have instead been exposed to varieties of Christian theology, and the more literate they were, the more intelligent they were, the more they would have been attracted to the professional study of Christian theology. It doesn't seem to be that difficult to explain, as long as we don't try to do a reductionism on the ideas so that we stop paying attention to their historical origin in our culture.

Now to proceed in a culturalist way, we would have to have a story of the history of our culture that would explain how it came to be that leftist ideas got taught to us, whereas Christian ideas got taught to St. Thomas, and I am not going to provide that story here. But providing such a story, a history, is something that I am saying we should be interested in doing.

The last thing that I want to say about a culturalist approach is that it is consistent with our own experience as scholars. This is the thing that amazes me the most: in the few discussions of leftist politicization of the academy, whether they are critical discussions or celebratory discussions, almost never do they take seriously what we see going on in our classrooms all the time, and in the classrooms of our colleagues. We assign books to students, they read the books, they get new ideas from them. They talk about them with us in class, we see their minds change, we test them on how well they have understood the new ideas, and to the extent at least that the tests are essays, and that we have any genuine discussions at all with our students, as opposed to lectures, we can see and hear that the ideas are often not just being spit back at us to get a good grade but are being integrated into the way the students see the world. And weren't we all, once upon a time, students like that? Yet the impact of teaching—or really of learning—on people's beliefs and be-

havior is invisible when, as in functionalist approaches, people's beliefs and behavior are attributed to the "social functions" they serve—or, to be more up-to-date than Durkheim, to "social forces."

It may be an injustice to call the alternative a culturalist, let alone a Weberian, approach. That may put too fancy a name on it. But we've all been influenced by what we've read and by the teachers who taught us, haven't we? It seems to me that that is what we should focus on if we want to understand what is going on in the social sciences. Who has taught us? Who has taught our colleagues? And what have our colleagues read?

GLAZER: On the political views of social scienctists in colleges and universities, we owe a lot to Lipset's work on that and opinion surveys, but I would like to raise two points which I think may complicate the matter. I don't know if they do or they don't.

One is that there was a big change in the outlook of social scientists, I would say, probably in the 1950s and '60s, in terms of whether they are conservative or liberal or left. I am reminded of an article by C. Wright Mills on sociology in the earlier part of the century, which he describes as very conservative based on the fact that most of the sociologists teaching then were the children of Protestant ministers. They were interested in social work and in the Social Gospel, and the transition from being interested in social work to calling yourself a sociologist was taking place at the time. I am mostly reminded of that, and again from Lipset, that if you looked at elite colleges, 1960 was a key moment in which, for the first time, the majority of Harvard students said they were voting for a Democrat rather than a Republican. So I think one has to think of that change, and how it relates to the overall question of the political outlook of social scientists.

Now, I also have a very narrow point, and it is too narrow I know, but it is on selective recruitment, and it again comes back to Lipset: the change in the social composition of academic social scientists, and the very large increase in the number of Jews among them, in America. Now I realize that we are dealing with an overall left-wing point of view among social scientists, and a Jewish cause of that can't explain the same phenomenon in Europe, but at least in America that Jewish infusion of the '50s and '60s as a result of a social change in the universities and a change in the Jewish population too is very critical, because Jews are predominantly Left, and there is a history, there's a reason for that.

One further reason for the leftist views of social scientists, and again I am thinking mostly of sociology but maybe it might apply to anthro-

pology too, is, once again, this history of race and racism as an ideology, and its decline because of the impact of Hitler and World War II, in which the social sciences became, so to speak, bearers of the flag of antiracism. It was their job, so to speak, to say "no, it isn't race" and so on. And I think it had a big impact on how social scientists thought. I haven't spelled that out fully, but I think that social change played a role also.

I accept the fact that yes there are liberal-left views dominant among academic social scientists today, but how important it is to go into a history of that change, or how far back it goes, I am not sure.

EASTWOOD: Mr. Press?

PRESS: I think, in response to Professor Friedman, by way of the comments made by Professor Glazer, that an important question that needs to be asked about the preoccupations of social scientists isn't just what the social sciences are preoccupied with but why they are preoccupied with what they are preoccupied with. I think it is suggested in the title of this particular discussion that social scientists are occupied with something before they are occupied with social science. An exchange held in the previous discussion revolved around the issue of race, and that has just been picked up again by Professor Glazer. What Glazer had said is that there was a biological sort of reasoning based on the belief in distinguishing different races and that in the end, or after a while, this changed and it was decided that there was no legitimate biological reason to distinguish between races.

I think it can be very easily supported that those people who argued that it was possible to relate the social behavior of racial groups to the alleged biological composition of their races, and that this biological composition was reflected in the groups' legitimate social positions, believed that those groups' positions were legitimately based on race before they did their biology; that is, they believed that certain races were not as capable as other races in performing certain functions, and so they tried to use biology to support their beliefs. Then Professor Wood responded to Professor Glazer, claiming that though this idea of biology as a reason for distinguishing between races has disappeared, people still distinguish between races under different names. They might call it "culture." But still they define these cultures on the basis of a biological category of race—"Black culture," for instance.

They do this, I think, because they believe that this is a very important topic. The situation of people of various races, especially in this country, preoccupies people even beyond the social sciences. Even now,

there is a very important debate, at least about to be held in the Justice Department, over affirmative action, so of course this preoccupies much of our thought even beyond the social sciences. What we see, however, in this change from the belief in a biological sort of definition of race to a cultural definition of race, I think, is that there is purpose behind both of them. There was a decision to prove that there was a difference in race. Some people wanted to believe that, so they tried to prove it. And now people want to believe that there isn't a difference, or they want to believe that "racial conditions" occur for other reasons. So I would propose that these are instances of people being ideological. Instead of trying to approach issues in a scientific way, they are simply trying to apply scientific terms to ideology.

This I think also can be seen in the formation of the institutions that we now call social sciences. Around the time, from the end of the nineteenth to the early twentieth century, that the professional associations were formed, I believe that many of the people who became the presidents of these new-born institutions were all originally, or not all, but mostly originally, students at theological seminaries or schools and had originally been members of the first social science association, the social science association from which all those other institutions were born. I can't remember what it was called, but it was an institution specifically directed at social work—they were trying to make the world better. . . .

GREENFELD: You're talking about the Association for Social Science.

PRESS: The founders of the American Sociological Society, the American Economic Association, the American Historical Association, were very, very intent on fixing the world. That suggests that they realized that something was wrong with it. So the very impetus to perform social science is ideological; social scientists have their ideological beliefs before they are preoccupied with social science.

EASTWOOD: I'd like to step back just a bit from the discussion. One of the things that we are trying to do today, obviously, is talk about the state of the social sciences. We have a problem to overcome. This problem is also, I think, the greatest strength of this conference, and it is that we have representatives of a variety of disciplines with us today. Not only do we have sociologists, anthropologists, political scientists, economists, and psychologists, but we have theologians and other representatives of the humanities. We have at least one natural scientist with us now and we are going to have more later; we also have legal scholars, philosophers, and representatives of other traditions. I think it might be

helpful for all of us if, before we get to an analysis of the actual preoccupations of social scientists today, if we had some presentation of what those preoccupations are. So I am hoping that some people might step forward, for their disciplines, and talk first about what the central problems of your disciplines are today. What are, for instance, the central problems of sociology, according to the mainstream tradition in sociology practiced today? Political science? Economics? If we can establish the current preoccupations of sociology, anthropology, economics, and so forth, we might talk about what those central problems ought to be, and whether we are on track, so to speak.

RICHARD: I am not going to speak about any discipline in particular but I am going to give you my reflections about what I read in the conference papers about preoccupations. I think they are full of preoccupations. I saw, of course, a preoccupation with the limit, or the line, or the border between politics and science coming through all of the papers that I think we are going to speak about this afternoon. I also saw two other things: one is a preoccupation with the position of economics—that is, a preoccupation with the hegemonic position of economics within the social sciences. I am not sure that this is the case everywhere, but it seems very much the case in the United States, at least so far as I can see from the papers.

The other preoccupation seems to me to be the question of the unity—or actually the existence—of the disciplines. It seems to me that, if we couldn't find any record of the social sciences, it is because, at least in the papers we have here, there are conflicting records on the same facts. So, it is like this: if you look at economic growth from the point of view of the economists, or if you look at it from the point of view of the historian of economics, you don't get the same picture, so you don't get the same record.

Many of the papers speak about the selection or the misinterpretation or mis-selection of data in anthropology, in history. We have here the example of the revisionist controversy in Middle-Eastern studies, but we could also speak about the Africanist version of history, for example, so this points to the problem of rules—agreed procedures for looking at data—before even interpreting or doing any quantitative, or any other kind of, interpretation of data.

I think that this also goes back to the question of objectivity or subjectivity. I am not sure that this distinction is very good, because I am not sure we will find any instances of objectivity. Perhaps it would be better to speak of intersubjectivity, or of agreed norms of objectivity.

So I think all of these are questions going through these papers, which are very basic questions—and I'm not sure there are answers, but these are preoccupations.

CASANOVA: Yes, I wish to say two things. The first is that, in my last comment, of course I didn't mean that we ought not to use technical tools. I only meant to say that we can seek inspiration in the Greeks, but not that we need to go back to the Greeks in the sense of simply repeating what they wrote.

The second is the following: In reading the papers of Professor Greenfeld I noted her claim that culture is a central concept in the social sciences and that it looks as if it were the *only* central concept and that every social-scientific discipline, apparently, has to do with culture. I agree. There is much to discuss here. However, I believe that there's a problem lurking behind "culture."

While, perhaps due to the influence of the social sciences, we are less Eurocentric than we once were, we are in danger of sliding into relativism. We say that there are many kinds of culture and that every people, in fact, has its own kind of culture, but is there any way to judge these cultures? Eric Voegelin arrived at a very similar conclusion, though in different terms, but he was an Idealist, and that led to one great problem which becomes clear in his discussion with Leo Strauss. Strauss saw the problem and pointed out that from Voegelin's point of view there was no way to distinguish good societies from bad ones, because Voegelin's perspective required the assumption of the standards of each society and allowed no position from which those standards could be judged. I think that if we assume the concept of culture as central we have to be very careful, because we could fall into historicism, and, in this case, we would be unable to answer the question of why the Germans who worked with Hitler could be judged at Nuremberg.

The answer, of course, is that there is something beyond culture that they should have recognized, and they failed to do so.

FRIEDMAN: I don't understand why using culture as a lens through which to understand why people did what they did has anything to do with the entirely separate question of judging whether what they did was good or not. If I want to understand what Nazis did, then it seems to me to be perfectly reasonable to look at Nazi culture, read the books that they read, view films of the rallies they attended, listen to what their preachers told them and their parents told them, and so on and so forth. That doesn't mean that I am suspending judgment except temporarily, just like you suspend disbelief when you read a work of fic-

tion—in order to understand it. But then when I assume the role of normative theorist, I can say what they did was evil.

CASANOVA: Okay, this is a very big topic. Perhaps Alasdair MacIntyre could help us find an answer because, while he acknowledges that every culture is formed historically and in a tradition, he nevertheless recognizes that tradition has to do with some thing or reality which is beyond the tradition itself. For example, we could say from this point of view that architects in Ancient Greece had an art—a *techne*—and that the *techne* is obviously a tradition, but beyond that tradition there is a reality—the need for shelter—and every tradition of architects has to do with that necessity, and for that reason I can compare several different traditions. Therefore, in order to understand any given culture, I have to "go inside" that culture, but in doing so I have to understand that the members or participants in that culture are trying to solve some real problems, and that those real problems are themselves beyond culture.

FRIEDMAN: But why not just say that they are trying to solve what they *see* as a real problem?

CASANOVA: Of course.

FRIEDMAN: Why prejudge whether they're right about it *being* a real problem?

CASANOVA: No, I'm not suggesting that we prejudge their perceptions of their problems. Let's consider the case of language. Language has something to do with reality and, in fact, with intelligible things in reality, no? And, of course, every language is a different tradition, but some imperfect translation between languages is always possible—an imperfect translation, but a translation nonetheless. This is due to the fact that every language has something to do with reality. I think that culture in some sense is the same as language but related to action, and people are searching for or oriented towards some good, and some good not in the Utilitarian sense, but in Aristotle's sense, which is too big a topic to discuss at present.

In short, there are many different traditions, but all of them have something to do with reality. If I am wrong and tradition and culture don't have something to do with reality then Nuremberg was a great injustice, and I don't think that it was a great injustice. I think that it was just.

WOOD: I found this to be, overall, a very interesting session, and I am very much taken with Professor Friedman's way of framing it. I do want to pick at one little piece of it, and that is the path depen-

dency of the ideas that have become dominant in the politicization of the universities.

I think it is true to some extent that they are path dependent, but I am most struck in anthropology by the historical discontinuities, which is a different kind of path dependency, I suppose. And I think it's true in at least some of the other social sciences that they proceed by repudiation of their pasts.

The central problems in anthropology have changed radically several times. In the nineteenth century, since this was a field that arose out of the antislavery movement, there was the question of whether humanity is biologically one species or many, and that question got wrapped into questions about the origins of our main institutions, all of which just got wiped from the slate in the early decades of the twentieth century. Nobody was interested in those questions any more at all. They were considered either settled or irrelevant. We instead got questions like, What is culture? How do you account for the stability of societies? Is there an important difference between primitive and advanced societies, other than the obvious ones of scale and technology? What is the place of kinship in human societies?

By the 1970s, in turn, those questions are completely wiped from the slate. Nobody writes about them, nobody thinks about them, or they become highly marginalized. Anthropology today has entirely new preoccupations. What is the nature and origin of inequality? How do marginalized peoples cope in the increasingly globalized world? How can anthropology advance the interests of the people that it studies?—a professedly political project. All of these, I think, feed into what Mr. Press was referring to.

Professor Friedman used the more generalized term "politicization," and pointed to the dominance of the academic Left. But we can surely be more specific about that. The refrain of "race, class, and gender" is basically what that politicization is about. Power, inequality, self and identity, all those larger concepts are focused into race, class, and gender, and, although perhaps that discussion is beginning to exhaust itself a little bit, it remains the central content of the social-science education, at least in anthropology, sociology, and I think political science and psychology. I am willing to be corrected on this, but I have a pretty strong impression that those are the topics that set the agenda before we get around to doing anything supposedly scientific.

I guess we've also been invited to the question of what *should* the agenda be, and I'll put in my two cents on that. I think we ought to be

striving towards once again figuring out what the universal conditions of being human and having a worthwhile society are; we ought to be attempting to understand how to read our past in a manner that makes productive sense to our students and ourselves; we have to bring history back into the social sciences in a profound way; and, particularly for anthropology, we need to strive to create some kind of synthesis of human evolution with our new understanding of cultural diversity—a project that has long been before us and has, essentially, gotten nowhere, but seems to me to be pretty central. But that is a big portmanteau of responses to many different comments here, all of which I found very stimulating.

PRESS: I would like to add to what Professor Wood said. This crisis of identity in the social sciences, at least in American social sciences, is not a new phenomenon. From the very creation of the social sciences as institutions, whether or not social science should be a matter of social reform, or whether social science should be a matter of scientific study of society, has been a debate. When the American Social Science Association, or Sociological Society, was created, they came up with a constitution with six points, half of which focused on their dedication to reforming society and the other half on their dedication to remaining completely objective and scientific. Clearly these two don't go together, but both were within the definition of the aims of the professional society. I think since then, at least in America, this debate has been in itself a preoccupation of the social sciences, and I suppose that perhaps that, in itself, may hint at the reason for the lack of a record within the social sciences.

SESSION III. THE POLITICAL LEANINGS OF THE SOCIAL SCIENCES

INTRODUCTORY REMARKS: Richard Landes
Peter Wood

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WOOD: The session is on the political leanings of the social sciences. We have been leaning politically for a while, so I intend to make my remarks somewhat abbreviated.

It seems to me that one of the questions that was put before us by the last session was whether we want to draw a distinction between the political leanings of social scientists and the political leanings of the social sciences. I am inclined to give that distinction a fair amount of weight. In any case, we have already talked a good bit about the political leanings of social scientists, so let me see if I can wrap this up and then move on to the other question.

I would be inclined to say that we would not be very concerned about the political leanings of mathematicians, chemists, cell biologists, and the like. We'd assume that they would get on with the business of their mathematics, cell biology, whatever, regardless of whether they were socialists, capitalists, anarchists, no matter. In fact, the political views of people in the natural sciences are quite diverse and don't seem to interfere very much with how they go about their work.

Having said that, I am aware that there is a fairly substantial body of criticism that has developed in the last 10 or 15 years that attempts precisely to claim that the natural sciences are politicized, that the work is infused everywhere with agendas derived from things like sexism, classism, that kind of thing, as well as a much more theorized critique that takes something of the form of a chain of syllogisms: all knowledge is social; science is a form of knowledge; therefore science must have a social basis; all things that have a social basis are subject to manipulation by powerful people; manipulations by the powerful are the sine qua non of politics; and, therefore, science by its nature is a political enterprise and can be treated like all other political enterprises.

Having drawn attention to that chain of reasoning, I intend, basically,

to ignore it other than to say that if you think it holds validity, you have to be prepared to deal with such things as Professor Alan Sokal's famous hoax in *Social Text* in 1996, where he submitted a bunch of gobbledygook claiming to show that physical reality is socially constructed. The editors of *Social Text* bought into it, published it, praised it—and then he revealed in *Lingua Franca* that it was all nonsense.

The hard political claims of the social construction of knowledge in the sciences tend not to impress the sciences very much. They go on about their work. It really is just another reflex of the pathologies in the social sciences that that discussion even takes place.

That having been said, I think where it leaves us is with the realization that somehow it seems that the political leanings of individual social scientists present a more troubling prospect. That kind of politicization has a more perturbing effect on the conduct of social science than it does in the natural sciences. Why should that be the case?

I can think of a couple of reasons. I think one of them simply is the quantity of the social scientists' leanings in one particular political direction. We've recently had David Horowitz's surveys showing 85, 90, 100 percent of the members of various social-science departments are registered Democrats or voted Democratic in the last five or six elections. It turns out that this isn't a phenomenon of slight margins. It is the case that overwhelmingly the social sciences in the United States are dominated by people of one particular political outlook. Yes, they have many differences among each other, but compared to the population of the United States at large they are definitely ghettoized; they are a particular segment of the American public. That seems more than coincidental, and we might want to take account of it as something that does mean that the identities of the social scientists themselves are part of the leaning that needs to be explained.

Another way of going about this is to ask whether the political leanings in the social sciences are partly the result of the way those sciences are organized.

Now, I can see both sides of this question. One can take the view that no, the social sciences as such are pretty well insulated from political pressure, and that therefore the politicization must be the result of something other than the organization of knowledge in the field. It must instead be that the social scientists betray their calling, not the way the calling exists.

The other way of approaching the question is to say that the social sciences, as I alluded to earlier, have an aspect to them that is strikingly

different from the natural sciences. This doesn't have to do with objectivity; it has to do with the manner in which one gets ahead in one's career. In most of the natural sciences that is done by contributing knowledge to existing research programs, adding by increments, sometimes large ones, to knowledge bases that are fairly public and well established. This is much less the case in the social sciences, where some of the best ways to get ahead lie in challenging one's teachers, overturning paradigms (or thinking that is what you are doing), joining up with hostile camps within a field: both the repudiation of the past and the repudiation of rival groups turn out to be key aspects of the social dynamics of doing social science. That being the case, it would appear that there is something political about the way the social sciences proceed, and that this leaning is not something that we can expect to do away with, short of a kind of social reformation, perhaps a moral reformation, of what the social sciences do.

I have three or four other thematic areas that seem to me to be important to this topic of leanings in the social sciences, and they have to do with something like this: if we lean politically, can we do that without compromising our basic reasons for existing as social sciences? I take those reasons to be providing answers to fundamental questions about the nature of human organization, human identity, human enterprise. If we lean politically and put those sorts of queries in danger, it would seem to draw inevitably towards three fairly disastrous conclusions. One is the tendency to lie. The second is the tendency to engage in self-deception; that is, you're lying but you may not realize it. The third is utopianism.

In the batch of papers we're looking at, we have a paper about lying. This is Karsh's paper [Session I] on what happened when he exposed flat-out historical inaccuracies in a body of scholarly work, and, well, you've read the paper, you've found out that he somewhat amused and sometimes irritated people, but he didn't change any views very much. We're fortunate to have present David Stoll from Middlebury College. Professor Stoll is noted for having exposed the inaccuracies in *I, Rigoberta Menchu*, the autobiography of a Guatemalan peasant who appears to have falsified various key episodes in her life history. That book, nonetheless, remains a profoundly important one in the work of many American humanists and social scientists, who don't seem to be all that perturbed about the fact that it is based, at least in part, on lies. My own paper is in part a rendition of hoaxes and lies that have been instrumental in the history of anthropology, going way back to the nine-

teenth century, when people were inventing Indian tribes, coming right up to the present, when we are still inventing tribes like the Tasaday in the Philipines in the 1970s.

Anthropology is roiled every decade or so by the exposure of some fabrication that goes right to the heart of what we do. Lying is part of what social scientists do. I take that to be a shameful condition, not one that we should try to excuse, but one that we had better reckon with if we expect to continue to get public support and toleration for our work.

As for self-deception. I think I could nail this most easily with a short quotation from Horowitz's paper [Session II], referring to our tendency to shift from empowerment of those who are presumably out of power, to advocacy of what those out of power advocate. I tend to think we do that without recognizing it. It may be that some go ahead and do it shamelessly all on their own, but essentially the social-reform program of much of the social sciences comes from identification with the people we think we are going to help, which puts up a powerful incentive for us to delude ourselves into tolerating the suppression of this fact, the overemphasis of that fact, the helpful interpretation that might advance the cause of people that we think deserve a break. It is just the common stuff of, at least anthropology, and I am pretty sure, a good many other social sciences.

Again, about the only way you can defend it is to deny that it exists. I encounter those discussions pretty often with my own graduate students, who believe that they can go off and change the world and be utterly immune from the temptation to suppress the truth or advance an argument in spite of the facts.

The third area where I think our political leaning exposes us to a pretty long-standing danger in all the social sciences is utopianism. Again, as Horowitz writes, the gap between the sociological and political is not the problem—the mindless fusion of the two is the problem. Utopianism is the idea that we can erase human nature, or that there is no such thing as human nature, that it is all culture, it is all therefore malleable, that we can change the world to suit some sort of ideal. As soon as we put ourselves in the position of those who think that we can outwit all of humanity and propose a better regime—or perhaps get us to the best regime, skipping over the better-but-not-best regime in the meantime—it seems to me that we are putting social science in the service of something that ultimately is destructive, not only to the

social sciences but also to the people that we suppose that we are going to help.

I said there was a possible fourth danger: the danger to academic freedom. It's true that social sciences are not wholly in the universities, but what goes on in the universities tends to define the social sciences, and what goes on there is wholly dependent on the doctrine of academic freedom: that we can freely question and teach and pursue ideas where ever they might lead us. But if we are engaged in lying, if we are engaged in self-deception, if we are engaged in a wrongful sort of utopianism, I think we put that academic freedom in jeopardy.

Academic freedom, of course, exists at the sufferance of the societies that make our institutions possible, and those societies in general do not have a strong interest in supporting self-serving lies. The sorts of things that academic freedom is predicated on are truth-seeking, commitment to fairness, and, although I can't think this is exactly the right word, something like wholesomeness. Social science aimed at something that might be intellectually valid but socially putrid is probably not something that is going to advance academic freedom either.

As I promised to be brief, I will conclude. It seems to me that the danger of having political leanings in the social sciences is that if you lean too far you fall over, and we're doing that.

PRESS: Before we open up the discussion we are going to hear remarks from Richard Landes.

R. LANDES: What I'd like to address primarily is an issue on which I, as an historian, am luckier on one level than many other social scientists, who have to deal with the present. The people I deal with are dead, and therefore, my political allegiance to them, or to opposing them, is somewhat lessened. But it does seem to me, first of all, that the social sciences are, in some senses, inherently . . . I think Nate Glazer said something about Jews being essentially leftist, and it seems to me that the social sciences are as well—to some extent because of the fact that they are trying to figure out socially creative ways of, if not ushering in modernization, at least making it comfortable and reasonable, not destructively chaotic.

So I think that to some extent for all sorts of reasons that have to do with the academy as a place where dissent can take place, a place where criticism can take place, that there is a sort of natural tendency for academics to move in this direction. As a medievalist, it is easier for me to understand that the entire modern project is a leftist project. Given the very fact that academia is one of the products of modernity and that it,

in fact, goes back to the Middle Ages, when some of these leftist projects were starting, like the urban communes in the eleventh and twelfth centuries, it is not a surprise that academics would, in fact, be leftist. Meritocracy is a very subversive notion in aristocracies, and so on.

What I have come to think a lot about recently—and I just got the label for it from a cognitive psychologist—is "cognitive egotism." Cognitive egotism is essentially projecting your own mindset onto other people. It can often substitute for empathy, and a liberal disease is that liberals, and I consider myself one, would like to think that people are good: if you treat them nicely, they'll be nice. It's not always going to work, but it is going to work a lot of the time, which is why so many liberals come up with what I would call therapeutic policies, in which, rather than treating somebody as set in their ways, if you can find the right way of interacting with them maybe you can get them away from violence and so on, to negotiation and the like.

The projection of one's own mentality onto others also works for authoritarians. We were talking at lunch about how the *The Protocols of the Elders of Zion*—an interesting authoritarian text—views modernity as a pernicious ideological and social and political movement that is going to destroy the world. Essentially it is a classic case of cognitive egotism, in that it is written by people who believe that the only way that there can be social order is with an authoritarian elite cramming it down the throats of the rest of the population. They believe, essentially, in what Eli Sagan has called—in what I consider one of the most seminal meditations on democracy, *The Honey and the Hemlock*—the paranoid imperative, which is: Rule or be ruled. Either you dominate others or they'll dominate you. This was a sort of standard axiom for most political thought in international relations up until modernity. What you have, in the *Protocols*, is essentially people who are certain that everybody plays by the paranoid imperative. Therefore if you have people walking around invoking democratic principles, it's not because they believe in them but, in fact, because they're using them as a trap in order to get around to being able to rule.

So you have cognitive egotism on the part of people who come from an authoritarian point of view, and you have cognitive egotism on the part of people who come from a liberal point of view, and I think that a great deal of what we generally view as the failings of the kind of scholarship that Peter Wood was describing, in fact is a form of utopian cognitive egotism in which we wishfully project onto every-

body else the kind of mentality that to some extent we wish we had, and by doing this we affirm hopefully that we have this attitude.

This brings me to an issue that I think comes out strongly in the papers on economics [Session II], which is that economics has gone in the direction of scientism out of an act of radical cognitive egotism: that is, ignoring the fact that the project of producing the kind of people that Adam Smith is describing had, as of 1776, at least according to my reading of history, taken well over 700 years to produce: the kind of individual, choice-making, rational agent who is committed to the use of money as a major way of manipulating his environment. This is a very long social and cultural process and I shudder for the Chinese, if they're going to try to use economic "science" to figure out what to do with their culture, because in fact the process of moving to money is, among other things, a process of moving away from honor-shame culture, and there are numerous transformations of mentality that have to go on in the process. Recently I was talking with some economists, and I asked, what's the literature in economics on the factor of shame-honor culture in choice? These economists really had nothing to offer but one study of Japanese culture and then sort of jokingly the comment was made that, you know, they make the same choices that we do but they do so for reasons of honor and shame rather than guilt and integrity. I beg to differ. One of the classic descriptions of an honor culture is two people driving cars toward each other on a narrow one-way street, and one of them needs to back up. Well, in a rational culture the guy who took the wrong turn and is going down the wrong way backs up, but in an honor culture, not all honor cultures, but, in a toxic honor culture, you can't back down because you can't admit that you're wrong and, as a result, the two cars collide. The man who has driven down the street the right way can't back up because he's right, and the man who has driven down the wrong way can't back up because he's wrong. This seems like self-destructive behavior from our point of view, and one of the problems with cognitive egotism is that we almost wish away this behavior by completely ignoring it.

Here we get to what I think Peter Wood was describing as either lying or self-deception. I think that one of the motivations behind such deception is cognitive egotism, and I see the same issue brought up in Chandler Rosenberger's text, on why we weren't thinking clearly before 9/11, or at least why a lot of our policy makers weren't thinking clearly before 9/11, because essentially we weren't paying attention to a whole series of psychological issues. Chandler [Rosenberger], you

raised the issue of *ressentiment* and you cited Liah Greenfeld, but I think that Nietzsche is the original source of this argument.

I definitely think *ressentiment* is something that we have to pay attention to. I think that it is probably important to understand that *ressentiment* is very often the response of an elite segment of a shame-honor culture who have been shamed by the advent of modernity—they have not really lost all that much because of modernity, they haven't been enslaved by it, but they have lost one of their crucial rights, which is the right to dominate. Not recognizing this is a kind of cognitive failure to recognize the Other, which brings me to my favorite criticism made by a friend of mine of postmodernists, which is that they can't walk and chew gum at the same time. They can't on the one hand empathize with other cultures, and listen to other voices, and then say: "This voice really doesn't sound very good to me." We've discussed the issue of moral value judgments. One of the things that strikes me about the "effort" to do scientific social science is the pretense of not making value judgments. It seems to me that not only is that a pretense, it's a foolish pretense. It's a kind of self-delusion with all sorts of very serious consequences because the less you know what you're doing, the worse the consequences are for what you're doing, and what we have, as a result, is a sort of pretense of not judging, all the while embedded in a set of judgments about things like academic freedom and political agendas to help the people who have been dealt a nasty hand by life.

One of my questions to Peter Wood: at one point you said it's indefensible for social scientists, anthropologists, to come to the defense of what they feel are marginalized or oppressed cultures—am I reasonably paraphrasing you?

WOOD: I don't think so.

R. LANDES: Well, you used the word "indefensible," and I thought it was in reference to advocacy for various cultures.

WOOD: Well, I'm speaking without a text, so I am not exactly sure what I said. You might have me. What I find most indefensible is the knowing propagation of falsehoods, but on the utopian side of it is this willingness to suborn the truth in order to advance the agenda of those whom you would like to help. There's nothing wrong with trying to help people, it is just that lying in the process of doing that is indefensible. . . .

R. LANDES: Okay, good. Okay, I'm with you on that. One of the points I wanted to make is that it doesn't seem to me that it is indefensible for people to do this, but it does seem to me, and I think this is a

legitimate postmodern demand, that we acknowledge what we're doing, rather than hide behind the pretense of objectivity or scientific truth. This gets back—I am sorry that Adam Seligman isn't here [stormed out of conference after Session II and denied rights to publish his comments—Ed.]—but it does seem to me that it gets back to the issue of agency.

It gets back to the issue of owning agency. I think the whole notion of scientific sociology or scientific social science in some sense is not owning agency. And this leads me to my last reflection, which is one of the points I think Jeffrey Friedman was making earlier, this issue about how we perceive our self-interest, and do all of our manipulations of symbolic systems contribute to our becoming aware of our interests? I think that Adam Seligman raised an important point in his volleys before departure about unintended consequences.

My favorite part of Weber is unintended consequences. I'm stunned by reading critiques of Weber's *Protestant Ethic and the Spirit of Capitalism* that don't get the idea that unintended consequences are what he is arguing about. And that's where we get to an issue that I try to bring out with my students, which is that people don't always behave in their self-interest. People are capable of immensely self-destructive behavior. And part of what it seems to me that the efforts to understand society and to communicate about society are, is precisely to get at how we can become wise enough not to be self-destructive. Cognitive egotism can be a barrier to doing that.

FRIEDMAN: Two points: politics versus scholarship, and the inevitability of leftist scholarship.

Both Professors Wood and Landes gave versions of the argument that there is something about social science, or else something about modernity, that makes it inevitable that social science will be leftist. But just look. There are some non-leftist social scientists. So that seems to me to be empirical falsification of the claim.

Professor Wood's argument, as I understand it, is that social science rewards revolutionary new ideas that repudiate past ideas. But that entails being on the Left only if non-leftist social science is conceived of as being a defense of the status quo. Yet right now, leftist social science itself *is* the status quo, but we don't see many revolutionaries trying to overturn it; so it doesn't seem as if the dynamic at work is a dynamic of overturning, as much as a dynamic of extending the list of recognized victims of oppression, from certain men to all men of all races, to women, to animals, etc.—extending the dimensions in which their op-

pression is recognized, and coming up with new methodological wrinkles on the process by which it happens, such as dropping historical agents from history, as in Foucault. My question is whether this normatively admirable and inherently modern, Western project is also inherently part of social science. Is the process of intellectual inquiry, with its inherent slaughtering of sacred cows, inherently leftist? That would mean that there are no leftist sacred cows to be slaughtered.

I agree that modernity is a leftist project. But does that mean that we moderns inevitably have to accept all leftist ideas?

R. LANDES: Only if we're stupid. . . .

FRIEDMAN: So as social scientists we can pick and choose among those ideas, and normatively we don't even have to accept modernity—after all, we could reject it.

R. LANDES: Good luck.

FRIEDMAN: Well, I want to defend that as a possibility to keep open in our minds, in order to keep our minds open as social scientists. I consider myself to be a Rawlsian who wants to change the world normatively but, on the question of politics versus scholarship, I am just going to take my cue from Weber again: why else would we do social science if not because we want to change the world, which requires understanding it first? Well, even though I don't take advantage of it myself, let me just keep open the option that we might want to do social science to understand the world because we think the status quo is the best we can do and we want to glory in it. And if you instead, like me, think that there are problems with the status quo, then you might want to understand the world for reformist or even revolutionary reasons—but even that doesn't get us to leftism unless we assume that all reform of the status quo, or all revolution against the status quo, has to be leftist reform or revolution. That's just not true.

It seems to me that the reason that we want to do something other than intellectual activism, or political activism, in our scholarship is that it occurs to us that what at first we assume are the answers to the problems of the world may not be the correct answers, so the intuitively obvious answers, which in our day are leftist ones, may not actually solve the problems they are intended to solve, and they may spawn new problems, or they may aggravate the original problems.

R. LANDES: Unintended consequences.

FRIEDMAN: Yes! So we investigate that possibility, and this is why economics is good. It has gotten into a corrupt state, I have no doubt about that, but it's good for at least trying to notice unintended consequences.

It seems to me that there isn't an inherent conflict between politics and scholarship. Rather, it is a matter of trying to just keep an open mind. That's all Popperianism, for instance, is, in the final analysis. Keep an open mind, as much as possible, because the only reason that you're doing scholarship instead of politics is that you want to find out whether the accepted answers, the accepted political answers to social problems, are correct. So you do research, and you've got to keep an open mind if it's not to be a pointless exercise in confirming the need for what you already wanted to do.

What I'm trying to contribute is the concept that what's really at stake is not leftist politicization, or politicization in general, but rather politicization that loses sight of the whole point of bringing politics into scholarship in the first place, which is that scholarship be an open-minded process of finding out what the best kind of politics is. This is why it seems absurd to me to try to hide from ourselves or from our students the political directions in which we think our research leads, because why else would we be doing the research if not to discover those directions?

MAZLISH: Liah, you may want to rescind my invitation to this workshop after what I am going to say. I am rather troubled by the tone as well as the content of some of the remarks that have been made, and I think we need to look very closely at the terms we are using. To say that modernity is a leftist project—materially I understand what [Prof. R. Landes] you're saying, and then for you [Prof. Friedman] to echo that—but I'm sorry, I don't think Immanuel Kant was a leftist. I mean, I think this is an anachronism. We are imposing very contemporary positions.

Clearly we need to make distinctions between leftist, liberal, etc., and to disengage that from a fixed view of modernity. Modernity has had many, many forms, and I certainly wouldn't describe it as leftist in a simple way. I know you didn't mean that. So that's the first source of my trouble.

Second is, we're good social scientists, we need ways to examine what it is we're advancing as a theory, and be aware that empirical data can trip up the best of theories. I don't have the answer to this but let me suggest the following, also to be said about the position of the social sciences and their politicization. As I understand it, most of academia is not like BU, or MIT, or Harvard, or what you will. Ninety-five percent of academia does not publish. That's a hard one to believe but somebody correct me if I am wrong on this. If any of you have had the

experience of going through this country and giving lectures at small Baptist colleges and suchlike, I think you'll have a very, very different view of what is being given to students, at least. Okay, you can put that aside and say, well, unlike them, we're the pace-setters, and I agree, there's an academic elite, and we do tend to think that the whole world is like us. But I don't think it's that simple.

Next, and this is partly subjective but I think there's evidence for it. We talk about the social sciences. My knowledge of economics is that it is by no means left-wing. It is quite the contrary. It is dominated by neoclassical economists who to a large extent are ideologues for capitalism. These are some of my best friends by the way. But nevertheless that's the state of affairs. I'm an historian. There are some other historians here. I have to say to you that I am not aware that most of my historical colleagues are leftists, certainly not extreme leftists. There is a fringe group of Marxists and so forth, but we have to be careful about regions, you know, we're talking from a geographical position, and that's part of that confusion of talking about leftists, liberals, Democrats, Republicans. I think we've got to be very, very careful, in that regard.

I was very struck by the point of cognitive egotism. I found that very, very interesting, but Richard [Landes], I was struck by the way in which you, basically, left out a group. You talked about leftists and liberals. Is there any reason why conservatives might not also have cognitive egotism?

R. LANDES: I did, I talked about how authoritarians project their attitude onto. . . .

MAZLISH: Ah, but they're not conservatives, are they? You're saying?

Anyway, you're seeing the point I'm trying to make, and I would like to make one more point if I may. Peter Wood's work—as he knows, I admire it greatly, but when he talks about hoaxes and myths and lies, and suchlike, these are not restricted to the social sciences. After all, many of these have taken place in the natural sciences, but they are corrected, and I see no reason why, in fact, they are not also correctable and corrected by the social sciences—in fact, by someone such as Peter Wood.

WOOD: I am not sure which order to take these in. Let me start with Bruce [Mazlish]'s comments. Certainly lies, self-deceptions, that sort of thing, are not the exclusive domain of the social sciences, and they do occur with great frequency in the natural sciences too, but as you say they are self-correcting there. I will offer just as an argumentative hypothesis that the social sciences are not as good at correcting their slips

in this fashion. All the works of Margaret Mead are still in print. Mead is a writer who invented some things out of whole cloth, whose work is riddled with inaccuracies that have been proven, over and over again, by subsequent researchers. You can go to the BU bookstore and buy a shelf full of Margaret Mead books. I don't think that's true of the works of Lysenko. Bad science disappears pretty quickly once it's exposed. Bad anthropology, and I think, some other bad forms of social science hang around. They get their own advocates. They get people who can add epicycles and explain away the discrepancies and will defend them ideologically for a long, long time.

R. LANDES: Efraim Karsh's piece [Session I] starts out with the idea that what happened in American history would not happen, and did not happen, in Middle Eastern Studies.

WOOD: But it does happen in Middle Eastern Studies.

R. LANDES: But the point is the correction that happened in American history would not happen and did not happen in Middle Eastern Studies.

WOOD: In any case, certainly there is a burden that falls on all well-meaning people to try to set the record straight, and we should do that, but that seems to come back to Professor Friedman's question, or assertion, that I seem to be, at least in one of my remarks, siding with the idea that there is something different about the social sciences that . . . let me see if I can get this straight. You [Prof. Friedman] seem to be interpreting my remarks as arguing that the dynamics of the social sciences favor leftist politicization. But that wasn't really my meaning. I was arguing, at least at that stage, that the dynamics of the social sciences, career advancement by repudiation and bickering, favors politicization per se—left, right, indifferent. Of course, if the ocean you're swimming in is a leftist ocean, then those repudiations are going to be fights on the Left. Are there—I know anthropology better than any other social science, and within anthropology, in the last two generations, the intellectual revolutions haven't meant the replacement of a status-quo orthodoxy or a right-wing orthodoxy but a Left one, exchanging one leftist theory with another, and another and another. So the Left is perfectly capable of turning on itself and having those battles, but I don't think that's particularly interesting. That's just what's going on because of path-dependency.

Rather what's interesting is that these disciplines seem to be constituted in such a way that gives positive social rewards, not to building on cumulative knowledge, but to shredding what your teachers have done.

That may be less true in economics than in the other fields, but I'll let that one go. I guess that's it. Other points will come up.

ROSENBERGER: Well, I am just trying to follow up on a point that Peter Wood made, and also that Professor Mazlish made, which is that the failures and mistakes that the social sciences have made don't get corrected the way they do in the natural sciences. I think one of the points that we should be discussing here, then, is what is it about the organization of our disciplines that makes it so unlikely that things are corrected by empirical evidence? This is not merely a matter, I think, of politicization—or at least it is frequently a matter of micro-politicization, in that it is not a question of Left or Right, but Chicago versus Berkeley or Harvard versus Yale. There are schools of thought that establish themselves as progenitors of particular techniques, and those schools are very unwilling to give up their identity as a school of thought, even in the face of empirical evidence. In that sense, I think, the social sciences tend to resemble the ideologies, either Left or Right, more than they do the natural sciences. And so if there is some technique that we can find to organize our disciplines around their questions, and methodologies to address those questions, then I think that is a possible path out of politicization, Left or Right.

GLAZER: I also wanted to make some comments on this issue of politicization. One of them is that there are certain areas in the social sciences that are going to be inevitably politicized and can't escape it. One is Middle Eastern studies. In all departments, there is the Arab-Israeli struggle, and it's tied up with imperialism, you just can't get away from it, but people try to be objective and do their best. It's similar in South Asian studies, what with Pakistan and India and so on. So there are certain areas that are just heavily politicized, which is not to say that people on either side are not following, to some extent, scholarly norms, footnoting and trying to get the quotations correctly and so on, but they can't escape making their case.

But I was thinking there's a larger sense in which we think of the social sciences as being politicized, or a larger set of vectors, and that is because of their basis in the notion of self-interest and acting in self-interest, which is so decisive in trying to understand things. Now, here you go back to Adam Smith and the writers of the Federalist Papers and Marx and so on, and I am impressed by the fact that this basic self-interest paradigm—which of course can be countered by Weber: there are symbolic interests, religious interests, and shame, and honor, people can destroy themselves and so on—but the self-interest paradigm is so

strong and is so obvious that often we inevitably see a kind of politicization in which arguments are made which we properly see as being made not on the basis of common interest or an overall interest but some partial, immediate group interest or individual interest. There is a kind of denatured Marxism that appears almost everywhere, in which even people who think of themselves as Marxists are not being particularly Marxist anymore, simply because they're talking about economic interests, and economic interests as possibly molding political argument, presentation of self, and so on. So, for example, I've been amused by the fact that certain writers on urbanisim think of themselves as Marxists, and possibly are, because they see that urban properties are so developed that the people with the most money get the best locations. Well, yes, of course the people with the most money get the best locations. Scholars who notice this can pride themselves on being Marxists just by virtue of noticing this obvious fact. I think it's that paradigm that, in effect, makes it easier to see everything as politicized.

One other comment: I am thinking of a major theme in the sociology of ethnic groups, which is related to a conversation I heard at lunch, and that is the automatic assumption of assimilation: everyone will get assimilated. That is a very dominant theme in sociology, such that, for example—and here you can attribute this to politicization—but if you write about American Muslims you note that they are like everybody else, they get jobs, and they want their children to advance in school, and they want to buy houses, and their mosques are becoming like churches, and they have Sunday services, Sunday schools. . . . The automatic assumption of a sociological perspective—and I don't mean in any absolute sense, I mean the American sociological perspective on ethnic groups—leads you to this position. Somebody can argue that's politicized, it isn't truly seeing the degree to which Muslims are different, that possibly they're not going to become assimilated, that they may become a thorn in the side of the American polity, and so on.

I am pondering these two orientations from the point of view of social-science politicization, and I am thinking: well, politics are affecting both of them, but the first position, of automatic assimilation, let's say the position that Herbert Gans takes about American Jews—while he is a man of the Left, is that a Left position or not? Well, it's the sociological perspective. And it may be totally wrong; those of us who seem to take that perspective automatically find it very difficult to deal with the rise of orthodoxy among American Jews and a variety of other topics.

The same is true for secularization—it's the automatic assumption. But is that political? It's based on the experience of modernization, and maybe it's based on commitment to the idea that it would be better if we got rid of religion, or maybe it's based on the idea that it would be better if all Americans were alike, but I think we may be overdoing the politicization aspect, as against perspectives that have made themselves at home in the disciplines on the basis of fairly effective explanatory power, or something of that sort.

PRESS: I see Professor Landes is eager to speak. Professor Prevelakis had his hand up before. Professor Prevelakis first.

G. PREVELAKIS: Okay, well, why are we so interested in this relationship between politics and the social sciences?

I think that the answer is that social science has become a way of legitimizing political decisions.

It is something that we have seen in various fields in a very explicit way. Let me mention two from my field. One was the German school of geopolitics, between the two world wars, which legitimized certain political aims through geographical concepts. The other has been in urban planning in the '50s and the '60s, where political issues have been advanced through a technical and a technocratic discourse. So I think the problem is that science, and especially social science, is a very strong instrument for politics, and in that sense, of course, we're not speaking about science, we're speaking about politics. So I think that there is a major and inherent element of politicization in social science that we cannot avoid completely.

If we are not followers of those ideologies that use science for political purposes—and we have seen some philosophers and some intellectuals who have been involved in politics, but using the prestige of scientific or philosophical background—if we are not interested in doing that, is there hope? I think there is hope because in those cases, the force, the legitimizing force of social science is based on the illusion of scientificity, and this has to be maintained—and this creates a space for real, scientific endeavor, which can give the possibility to certain people to work in an objective way, to promote knowledge and also, I would say, to promote innovation in the system, because the system cannot stay the same. It has to change in order to adapt to evolution. So if we are interested more in science than in politics, I don't think we can completely change the situation, but rather can explore the possibilities of those spaces and to try to extend them and to expand them.

R. LANDES: Okay, I'd like to address a couple of previous points. One,

Bruce [Mazlish], when you were taking me to task for the use of the term *Left*, I agree it's always in scare quotes, but I disagree that somebody like Kant is on the Right and that I am using an anachronistic idea, because as a medievalist I can tell you that Kant would have been burned by the Inquisition for being too progressive. When I use "Left" and I talk about modernity, I mean basically the idea of equality before the law, and all of its implications, which include things like meritocracy and so on, which I think are the cultural motors of modernity. I do think that the Left-Right dichotomy is actually much more of a stumbling block than it is a clarifying concept, and we're probably better off moving away from it because one of the things that we misunderstand gets back to the cognitive egotism problem of, for instance, dealing with Arab culture: we talk about Arab "moderates," but the most moderate figures in the Arab world are over to the right of fascism, in most cases, on our Left-Right scale. There is a fundamental difference between "right-wing" in a democratic society and "right-wing" in an authoritarian society, so I think we systematically misread Right and Left. . . . If you look at what Ariel Sharon says and does and you look at what other people say and do, you know, there is a huge difference between "right-wing" in Israel and "right-wing" in the Arab world.

As for the point Nate Glazer made about both the secularization and the assimilation models as having great explanatory power, well, so did Ptolemaic astronomy. It had enormous explanatory power, so great that for over a thousand years people really felt that it was the best way to handle the data. So the real issue, it seems to me, one of the issues that Kuhn brings up, is the psychological issue of who pays attention to anomalous details. Who's got an eye for what's a signficant anomalous detail, because there are millions of anomalous details; which of them are significant, and who's got the capacity to approach that anomalous detail in a way that integrates it successfully in a larger understanding?

And here I think we come up against all sorts of resistances, but in particular what strikes me as the political or the ideological dimension of the problem with anomalous detail is that there can be a commitment to see the world in a certain way, say in the tacit alliance between what we might call leftist or progressive thinking on the one hand and economic thinking on the other, producing the widespread piety that terrorism is the product of poverty. All of the data indicate that that is not the case, that really poor people are not terrorists, and that, in fact,

the terrorists who struck on 9/11 were hardly what one could call poor, but the idea that terrorism is caused by poverty is particularly appealing to us for a series of ideological/political reasons, one of which is we think we might be able to do something about it. We don't know what to do about *ressentiment*, but we do know what to do about poverty, or we think we do; and second, again, the cognitive egotism that makes us think: "Well, what would drive *me* to be a terrorist would be being reduced to extreme poverty." So in this sense, it seems to me that there is a kind of political, or ideological, or psychological commitment to seeing the world in certain ways that makes for bad social analysis—I don't want to use the word *science*.

My final comment is that even though I am of the generation that does not believe in objectivity, what I do believe in is honesty, and I think it is far more interesting to ask the question, "Is somebody an honest scholar?" I don't mean that just on the most obvious level, but also honesty coupled with integrity: "Is somebody an honest and therefore a fair scholar?" Rather than, "Is somebody objective?"—because just like you can lie with statistics, you can lie with facts.

CZERKOWICZ: I'm a student here, and perhaps it is the role of the student to say something optimistic and hopeful; or something like that. Social science as a whole science cannot be only "political" or "economic," because it is concerned with things that have happened and how they play out in the present. It is intrinsically linked to history. Maybe we don't believe in objectivity, but we certainly acknowledge something that took place; we can read documents and we can examine all the facts that there are, and we can say: "Yes, this happened." But it seems that the problem is that we don't look at the world that way. We ask instead, "Is this good, or is this bad?" and then form a conclusion before we even start looking at facts.

I have an example. If you study social movement research, it never starts with "Did this happen?" or the specifics of how things happened. It starts off with the assumption that social movements are bad, or social movements are good, and they happened because of bad economic, racial, etc., conditions or they happened because of pluralistic democracy, and this is a good (or bad) thing. If we're to develop a science, a real science, out of the social sciences, we would have to look at it the other way: facts before grand theory and value-laden judgment. Knowledge does not come from assumption. Look at history, look at facts.

GLENN: We keep passing the microphone back and forth here. Just a quick comment, perhaps in support of what Nathan Glazer was saying earlier. Just an observation. Certainly it appears that academic social scientists, who overwhelmingly vote on what we conventionally call the Left, also overwhelmingly don't go to church, or synagogue, or mosque, or whatever. The research on the different academic fields finds that scientists are about as likely to be religious believers as the general American public, but that there is a declining range of religiousness going down to anthropologists at the absolute bottom. Now, it seems to me just as likely that that is an explanation for the prevailing fads and fashions and perspectives that operate within the social sciences, as that any particular political commitments are responsible, and it would be interesting to hear more about why there is that connection, that low level of religious commitment on the part of academic social scientists.

Just a practical observation. I've been for the last ten or twelve years the faculty advisor for the graduate Christian fellowship at Boston University. We've had many graduate students out of the sciences, out of the law school, out of the school for the arts, out of education. I don't think we've ever yet had a graduate student out of sociology, anthropology, or the other social sciences.

LINDHOLM: Well, anthropology is my religion [laughter]. And also, I like Margaret Mead. Anthropology isn't a field where you can totally disprove something. Margaret Mead's work was flawed but on the other hand she has wonderful data, wonderful material, wonderful ideas to argue with.

WOOD: Too bad she made it up.

LINDHOLM: She made some things up; she didn't make it all up. But, let's not go there right now. I'm not even going to get into a discussion of Arab moderates, which would take us away from the point.

I believe Professor Rosenberger implied that the issue is not so much politics as it is group dynamics. Schools are in opposition to each other, people make their reputations through connections with one another and seek status through those connections. I consider myself a Weberian as well. However, the problem I have with Weber is that he is a cognitivist. For him, people are meaning-seeking, interest-seeking figures. They're looking, within different cultural frameworks, to maximize their benefits. Those benefits can be honor; they can be accumulation of goods; it depends on the sort of meaning system that you are operating in. I think that orientation works extremely well about 99 percent

of the time, but not if you are looking at social movements and things like that. Weber himself said that people are motivated in large measure by things that he really couldn't theorize about as a sociologist, which are tradition, habit, repetition; and they are also motivated by charisma, by emotional connections, which he also had no adequate way of talking about.

I think that that's one of the directions that social science can go; to investigate how people are socialized to become parts of a system, so that the beliefs that they have are inculcated deep into their hearts and souls, so social systems transform them not through changes in ideas, but through changes in, how shall I put it, in the very sense of reality, which is something that is embodied, transcendental, and capable of leading people to sacrifice their very lives. These are aspects of the human experience that I think we can consider when we're thinking about reforming social science.

CASANOVA: Earlier today, Professor Greenfeld justified in some way the social sciences, noting that natural science has grown a lot, and natural science doesn't make people better. We have bombs, but we don't know what the bombs are for. I think that social science has to do with action, as I said before, but I have the impression that many social scientists think that that action is not rational (since, they believe, action is motivated by values, and values are not rational), and that is why I think many social scientists think that everything in politics is ideology.

Not everything in politics is ideology. I don't like labels such as "Left" or "leftist," because I don't know what they mean. They don't mean anything, in fact. They meant something during the French Revolution but now what do they mean? I, myself, have been accused of being leftist and of being rightist.

I think that the social sciences don't recognize that they are about action, and because of that some social scientists are utopian. They want to be objective, but they cannot because they are involved in action themselves, and I want to use an example to illustrate what I mean. Hans Kelsen, the great Austrian juridical thinker between the two wars, thought that action was always ideological, and because of that you couldn't say that a legal judgment was just or unjust. If you say that a solution contemplated in law is unjust, you are a leftist. If you say it is just, you are a rightist, no? Because, according to Kelsen, action was necessarily irrational. I don't agree, and I think this is a core problem of the social sciences. If we want to produce truly useful knowledge, as

Professor Greenfeld suggested, then we need to leave behind the idea that action's sources lie beyond reason.

SESSION IV. THE INFLUENCE OF THE SOCIAL SCIENCES

INTRODUCTORY REMARKS: Chandler Rosenberger
MODERATOR: Nikolas Prevelakis
PARTICIPANTS: Fred Brownson
Lauren Costello
Hernando Forero
Jeffrey Friedman
Nathan Glazer
Geoffrey Hill
Jonathan Imber
Katrina Leach
Bruce Mazlish
George Prevelakis
Chandler Rosenberger
Dusko Sekulic
John Stone
Eric Williams
Peter Wood
Carl Woog

Papers distributed to the participants for this session:

Greenfeld, Liah. 2005. "The Trouble with Social Science." *Critical Review* 17(1) (forthcoming).
Greenfeld, Liah. 2002. "An Invitation to a Dialogue: A Comment on Neuroscience and Culture." www.bu.edu/uni/iass.

ROSENBERGER: I guess in a sense I want to get away from a discussion of the overt politicization of the social sciences when discussing their influence, because I think that a lot of the problems with the social sciences that we should be addressing are not necessarily problems that come immediately from its politicization, but rather from its methodology.

When we look at the difficult—or rather, to steal a word from Peter Wood, pernicious—influence of the social sciences on our society today, one of the biggest problems is the unintended consequences of

the methodologies that we've chosen. I think this is particularly true when you look at the way that the social sciences have decided to imitate the natural sciences.

Imitating natural science disguises the failure in the social sciences to actually use the scientific method. One of the biggest problems, I think, in that imitation, has been the choice of material to study, on the ground that it is easily quantified. The collection of material in quantities gives it a kind of scientific authority. In the papers that we read for the last session, we see that that's especially true in economics and in the discussion of the economic behavior: the material that can be mathematicized easily is studied—prices, unemployment rates—while other topics, or other areas of investigation, such as values and ambitions, are for the most part ignored. I also thought Professor Banuazizi's comment about psychology and the choice of the materials used in the studies in experimental psychology was interesting, in that it's rather easy to collect materials from surveys that one does among American sophomores; and it is convenient, in using that methodology, to universalize their attitudes and assumptions.

So we have, in a sense, a political result from a mere choice of methodology. That is, we come to believe that most of the world thinks the way American sophomores do, because it is easier to collect data on behavior from American sophomores than it is from people in the field, necessarily. This is, obviously, an enormous failure with enormous consequences. In the papers that we read, these failures were discussed largely in the field of economics, and in particular, the rise of the economists within the social sciences, such that the economic approach to the social sciences has become the reigning historical paradigm, and as Liah Greenfeld put it, man is seen as *Homo economicus*, in relentless pursuit of wealth and consumption. It would appear that if we combined the Amerocentric vision of psychology, using the data that it's easy to collect from American universities, and the economic approach, what we have is a new universal as the subject of our study: the college sophomore who wants a DVD player.

This, I think, is a serious problem. It leads to the belief that the rest of the world is composed of, essentially, greedy college sophomores. And it doesn't give us a chance to consider what other ambitions people might have, what other values they might hold, and what other goals they might have.

For one thing, I think the emphasis on economics, and on an essentially Amerocentric psychology, has been based in the hope that moder-

ation will emerge out of modernization. That in our international relations, the more we push countries to modernize their economies and the greater emphasis we place on economic growth in other societies, the more moderate we can expect them to become—and I am not sure that's true. It certainly leads us, in International Relations, to ignore the degree to which such economic ambitions have origins such as Liah has described in her most recent book: origins in national pride that are outside of the closed circle of mere economic aims. Other countries may be eager to pursue this modernization project for reasons that have absolutely nothing to do with the mere economic ambitions of their individual citizens. It also ignores the degree to which wounded pride, or *ressentiment*, can play a role when that project goes awry. And finally it leads us to a sense of self-deception about the project, about what modernity actually looks like.

I would dispute the claim that was made frequently in earlier sessions, that modernity is in some way a project. It's not. It is a project to us to the degree that we try to inspire other nations around the world to imitate our ways. In that sense, globalization is also, I believe, a project; only to the degree that we try to create imitations of ourselves around the world, this self-deception leaves us completely incapable of seeing, in International Relations, what actual attitudes towards us around the world are, and who it is that actually means to do us harm. I find it very disturbing. I would just like to reiterate, for those of you who may not have read my paper, that before September 11, our national security intellectuals spent most of their time essentially looking for enemies among the religious and the poor of the world, rather than the secular, the middle class, and the particularly aggrieved.

That's where the influence of the social sciences, as they're now practiced, has been particularly pernicious in my own discipline. But we might want to consider what the influence of this particular model of human affairs has been among the broader public. I think it has had the unfortunate result of producing a deep misunderstanding of our own lives, as free citizens in a free country. This is particularly true of the American case, where journalists have increasingly substituted the word *citizen* with the word *consumer*. I believe that this has contributed to a loss of a sense of moral authority in the ordinary citizenry of the country, when questions that were previously questions of moral judgment are increasingly questions answerable only by a technocratic social-science elite.

There are many questions that social scientists want to address for

ideological or political reasons and may want to even be explicitly political about—in saying that they're pursuing these questions because they would like to abolish the idea of race in American society, or they would like to better the conditions of the poor in society. But for social scientists to acquire not only a certain amount of political authority, but moral authority as the ultimate arbiters of what questions the society ought to be addressing, is, I think, quite dangerous to a democracy.

I also wanted to pick up on a point that Peter Wood made when he was describing the tendency among social scientists to identify with the people whom they are meant to help. I think there's a flip side to that, in which social science as it is now practiced, and for reasons that we may want to discuss, has produced a profound sense of alienation and even self-loathing in the American public, which is now convinced that the majority of aims and ambitions of their fellow citizens are a kind of greedy pursuit of material gain. That prevalent and deeply cynical approach to American life is evident, I think, frequently in reflection on what September 11 really meant: in some way, because the United States has pursued such an imperialistic end, a promotion of capitalism around the world, the country in some sense deserved what it got. And it has also provoked, I think, profound self-doubt in the American public about what the nation potentially could stand for or ought to stand for.

Those are a few opening thoughts on the influence of the social sciences. Obviously there are many beneficial influences that I am sure we'll want to discuss, but I want to open with those as a comment on some of the problems that we may want to address, both by changing the methodology and possibly also changing our subjects of discussion.

MAZLISH: Chandler's remarks have been very interesting and helpful. . . . Having complemented Chandler, I want to now build on what he said. One, I do agree with him on his description of *Homo economicus*. But I think that, within the social sciences, there are now very different voices as well. For example, the people in Middle East Studies with whom I talk are very aware of all the other factors, and indeed are less interested in the economic than they are in the cultural and the social and the rest of that. Some of them, I'm afraid, we have to find in Cultural Studies, or in other areas that we may not think of as part of the social sciences. But I do think we need a balanced view of what's going on.

My next comment would be on poverty, because I think that it is true that poverty is not where the terrorists are coming from. They're

very upper class or middle class, they're educated, they've often been in America and so forth. But that doesn't mean that poverty isn't part of the problem. In other words, it's a question, I believe, of the level at which we want to look at this.

Poverty doesn't create the leaders; you're absolutely right on that. I think in terms of the background it is clear that within a number of the Middle Eastern countries they have been trying to grapple with modernity, a large topic, and many of them tried to do it in terms of nationalism—Nasser in Egypt would be a case—and by and large this has failed. There are corrupt, authoritarian regimes—pick your favorite country, and they have done nothing for the people. And yet these people are exposed to the forces of globalization. They see, in the media, what another life could be, and there they are, still in poverty. So I think we ought not to dismiss poverty. Poverty is something we want to do away with, for many reasons, but it also is the sea in which some of these terrorists can swim more readily than they might otherwise. So I simply suggest we think about the role of poverty in terms of levels.

My last comment is about globalization, which is a subject dear to both Chandler's and my hearts, and we're going to have to have much discussion about it. If you listen to the economists, globalization is simply economics, the market, and the rest of it. No question, that is a large part of it. But if you look at globalization as a whole phenomenon, the cultural form of it is as important, and perhaps will have even longer and larger results. Now cultural globalization is tied to economic globalization; I would never say that it is detached from the multinational corporations and the rest of that, but culture has a life of its own, and that's what we need to understand as well as the economics.

FRIEDMAN: I just wanted to ask Chandler whether there's any specific methodology that he condemns, and if he has any magic bullets, and before you answer, I want to give my opinion. I am suspicious of the idea—not suspicious inherently—but when I look around at all the pathologies in the social sciences, I don't see anything that they have in common methodologically except closed-mindedness.

Even mathematics has its place. Certainly quantification has its place. There are many issues that do lend themselves to quantification. There are other issues that lend themselves to interviews or documentary research. It depends on the research question.

Take Professor Banuazizi's point about psychology, and the work on sophomores. I think the point could probably have been put as "Stanford sophomores." My understanding is that his reference is to the prac-

tice of Stanford psychologists—and Stanford is the leading psychology department—of subjecting their sophomores to experiments, not so much surveys about their attitudes, or their values, but experiments, to see how they'll react to various things; and they're very clever, these psychologists. I don't think there's anything inherently wrong with that procedure, as long as those undertaking it, or their critics, are open-minded enough to recognize that there may be variations across cultures and across times, such that these results may not be revealing a universal human nature. But we are at least getting data about a significant slice—or, some would argue, a significant slice—of human nature. So I don't see a pathological problem there, of the sort that we do see so many of around us.

My suggestion is that the problem that I have been putting under the rubric of "politicization" is also a problem shared by the other big target that people have had today, which is rational-choice theory/*Homo economicus* as universal human nature. In the case of political excesses and hoaxes—the Emory historian who made up the data about American gun ownership, and the various anthropological hoaxes that Professor Wood's paper is about, and the Sokal hoax—they all happen to be on the Left, but the reason for that is that virtually everyone in the academic world is on the Left, so the bad apples will be also be there. But I think the reason that these mistakes aren't corrected, and also that the increasing implausibilities of rational-choice theory, as it gets further and further removed from reality, aren't called up short, is not a matter of methodology, because I don't see a common methodology; it's a matter of attitude.

The attitude is that the purpose of scholarship is to confirm something that we already know. Scholars without a question mark. Scholars who, in the case of the leftist political activists, replace a question mark with an exclamation point. And who, in the case of the rational-choice theorists, replace a question mark with an elipsis. More and more iterations of the same old thing.

We need open mindedness and we need the intellectual responsibility to accept unpalatable conclusions. You have to ask a question and then, when the answers come in, you have to be willing to change your mind. This is a unique danger in social science, I think, because while closed-mindedness is part of human nature—and I certainly don't think it's possible to simply and straightforwardly find "the facts" without the benefit, or the curse, of a theoretical or even ideological framework that identifies some things as facts worth noticing—in social sci-

ence, unlike natural science, different closed-minded scientists can't check each other's theory-laden interpretations of the facts through experimentation. So maybe I'm being naive, but I see the Stanford psychologists, at least methodologically, doing something positive when they ask a question about which it's possible that data may come back that might change their mind about something. Similarly with the use of regression analysis in other social sciences—although there's certainly a danger that either of these methodologies will tend to be overemphasized, so we only investigate things like Stanford sophomores because they can be subjected to controlled experiments, or survey responses because they can be quantified.

But generally speaking, even at Stanford and even with regression analysis, social-science experimentation isn't possible—certainly interpretation-free experimentation isn't possible, even in those cases—and that places a greater burden on us than on natural scientists to try as best we can to overcome our closed minds. And I don't know of a magic methodological bullet that would accomplish that.

ROSENBERGER: I agree with you about methodology. I take your point about methodology, but if I were to propose a methodology for the social sciences it would be a Weberian style of methodology within sociology, and even within supposedly far-flung fields like International Relations. I think a Weberian sense of international relations might have spared us a little bit of the trouble we now find ourselves in.

I also wanted to address quickly one point that Professor Mazlish made. It is very interesting that the questions that we want to ask are now at least being raised in departments of Cultural Studies; clearly there is something going on there that resembles, or that at least uses, the same materials as a Weberian would look for. Then the question is, how do we convince them, or how do we work with them, to come up with some kind of method that would be able to prove or disprove things that, at least, as far as I can tell in Cultural Studies, aren't very rigorously assessed, aren't rigorously falsified?

G. PREVELAKIS: Yes, I would also like to comment on this question of economics vis-à-vis culture, because I think that if we look at the history of ideas during the twentieth century we will see that there is a kind of competition between econonism and culturalism. So the fact that culturalism is coming back is not a satisfactory solution. I think that the basic issue is whether those concepts are, if they are separated, able to understand the reality. In a certain sense the exaggeration of economism leads to the exaggeration of culturalism.

The problem for me is that we are trying to understand the motivations of the individual. If we are on the economists' side we are trying to see how the individual makes decisions according to how he perceives his interests. If we are on the culturalists' side then we are trying to understand how people decide according to some kind of embedded characteristics that have been transmitted by culture. I believe that we can understand the world better if we see different levels of motivation because, in fact, if we return to Aristotle, man is a social being, and it is not only in order to be able to satisfy our interests, our individual interests, that we have to be in a framework, not a society of anarchy.

If our life is threatened, if we are in a world, let's say, like the Middle Ages or the early Middle Ages, there are the interests of the collectivity, which are finally the guarantee of the individual interests, but for which we can sacrifice our individual interests. So there is, I think, a kind of combination of economics and a cultural approach which develops on different levels and this is, I think, the kind of complexity that we should try to understand, rather than the polarization between economism and culturalism, which leads to two contradictory visions of reality which we cannot synthesize.

I think that this applies to the debate on globalization, because on the one hand we have the idea of globalization: finally the world will be unified economically and culturally, through the power of economics; and on the other hand we have all those ideas of tribalization, of nationalism, etc. And with these two completely contradictory approaches, we cannot understand how they combine, and I think that they combine because, in fact, those factors are not contradictory; they are complementary.

SEKULIC: I think that we have a tendency to accuse ourselves of some kind of methodological narrowness, because in the social sciences we are comparing ourselves to the experimental techniques of the natural sciences. But maybe we should be a little bit positive regarding this. For example, this excellent case of experiments with students. How useful they are or not depends on the basic assumptions from which we are starting. Classic books such as George Homans's *The Human Group and the Elementary Forms of Social Behavior* argue that if there are some universal laws of sub-institutional behavior, then experiments on human beings, regardless of institutions and cultural differences, will produce effects such that we can prove or disprove general assumptions about human behavior. We cannot compare different cultures and institutions

when we are dealing with students, but we can prove or disprove universal laws, if there are any. But are there any?

I think that we also tend to idealize the natural sciences, which sometimes is and sometimes is not a tendency shared by natural scientists themselves. What are universal laws? All of the so-called universal laws are always valid only under certain conditions—not, strictly speaking, universally. The difference is that the conditions under which laws in the natural sciences are valid are much broader than the laws that we think that we are discovering in the social sciences, but the presence of certain conditions is required in both cases for the law to be applicable. So I am not very satisfied with the picture that on the one side are universal laws of natural science, and on the other side, nothing. We actually have laws with different spans of validity, different conditions under which these laws apply.

The third point I want to briefly make is that we have much better explanations for what we are doing than politicization or ideologization. Example, the very interesting paper by Professor Rosenberger, saying that the American security establishment didn't predict what happened because they were probably under the political influence of the idea that terrorists must be poor. The only thing that I don't agree with is that I would not call this politicization; I would simply call it bad social science. We have good social science which is available and which was not used. Of course you can call it "politicization," but just because good social science was not used, not everything is lost: we still have good social science.

For instance, there is the status-inconsistency theory of revolutions, and there is simple comparative analysis of the history of revolutions. The communist revolutions and nationalist revolutions were led not by the poor, but by middle-class people experiencing status inconsistency. This theory was not used before September 11, because some people in the social sciences, because of their political blinders, do not use good social science; but good social science is available and can be used.

LEACH: Yes, thank you. My name is Katrina Leach. I'm currently a student of Professor Greenfeld's. I think it is really important in trying to decide on a methodology to realize that all of the social sciences are related. It seems as if there's been an assumed division between economics and culture, but economics is cultural. It was created by us. Economics doesn't exist in nature. I think it is critical to try and figure out some sort of common methodology. Methodology is basically the overall process that you use to discover things, and it can differ in the differ-

ent fields by specializing, by perhaps using ethnographic studies and other techniques, but overall, a continuity exists. The scientific ideal of conjecture and refutation, where you make up a theory and then defend it, seems logical in the social sciences, but it seems that right now that doesn't really happen.

FORERO: I am also a student. The way I perceive a lot of the social sciences and specifically economics, which is my area, is that rather than influencing policy or even having unintended policy consequences, economic theory doesn't really have much influence at all in many cases. In economics, for instance, policy makers actually go against the advice offered by economic theories. If this is widely true, what is happening is that the social sciences are distancing themselves from the people who actually make the decisions and from the policy-makers, which might be because they are not addressing the right questions, or are not giving the right answers.

WOOD: I was going to take hold of this subject from a slightly different direction. If the question is the influence of the social sciences, at least in American society we can give a kind of ethnographic answer to that, and it does have to do with unintended social consequences, probably. I would distinguish three of them.

One is that, at least in America—and I think this may be true more broadly—Americans have acquired a huge vocabulary of social-scientific terms by which they now mediate their experience: *role model, alienation, diversity, culture*; those are all part of the common vocabulary not of social scientists but of ordinary people, and that's a change, that's not how people a hundred years ago talked about themselves. It's not a random change, it's a change brought about by social science.

Second, connected with the change in vocabulary is a change in expertise. We now generally, as a people, seek economic, psychological, sociological explanations, and the experts who can master those kinds of explanations. In so doing, I think we've lost some things. There's been a flattening of our capacity for reflection in other areas, a loss of other native vocabularies for describing, thinking about, reflecting on human experience.

And along with that, third, is a drastic shift not just in terminology but in ideas. To take one, our ideal of an educated person is now certainly not somebody who has read great books, knows the world of art, has travelled widely—the sorts of things that might have been the desiderata of an educated person three or four generations ago. The educated person now is generally someone, if he's not in the sciences,

who has a highly skilled ability to manipulate symbols—precisely the sorts of symbolic terms that I listed. Around 1900, roughly 5 percent of American high-school graduates went on to college or the equivalent. Now it's over 60 percent.

How did all this happen? Well, we began an experiment in mass higher education in this country, which created the means by which social science propagated itself beyond the realm of social science into cultural phenomena. I am not stating this view from a wholly nonpartisan perspective. It seems to me that the losses entailed in social sciences are on the whole probably greater than the gains that have been made. But it is a hard question. Clearly social science's influence has been culture-transforming and socially transforming, in ways that weren't exactly what was intended when the Durkheims and Webers and other discipline-creating figures were in the process of formulating their major ideas, but that's how it's turned out: we've created a society that thinks of itself through the terms of social science, and it brings losses as well as gains.

IMBER: I was going to remark that I appreciate Peter Wood's comments very much because they reminded me—I think it was many decades ago that Robert Merton described the popularization of social science. I think he called it "obliteration by incorporation," and that is certainly a phenomenon that has accelerated, largely because there is not a semiskilled journalist in the United States who does not have the capacity, when it is necessary, to repeat one or another social-science generalization, no matter how hackneyed or old it may be, and we simply take that for granted; which is why one is always looking for someone who has the capacity to see into the phenomenon that we're trying to understand, to sort it out from all the rest.

I am just going to address briefly the comment about how one defines what is meant by the influence of the social sciences. The thing that I would say, first, is that social science has a particular characteristic, if I understand the disciplines within it: to be engaged in activities and thinking that, at their best, are ahead of their time. That is, it is only by virtue of our thinking about a phenomenon ahead of time that, at some point afterwards, we begin to see what the phenomenon's effects are, what its influence may be—and that's a different kind of problem. It seems to me that that's a road without end.

The nature of social change and the nature of social order are two issues that are under constant scrutiny by a variety of people with different motivations and different interests, and it's incumbent upon

everyone to try to sort out what those are and to describe them and name them as best they can. I was interested in another kind of influence, and this may be best described as a question, such as "How did Marx get into social science?"—how *did* that happen? Did we just wake up one day and Marxism was there? Or was it Lewis Coser who decided in the *Masters of Sociological Thought* that Marx deserved a chapter? How does that work?

It strikes me . . . one of the ways that I, when I teach about these things, what I try to do, is have the students open up the first page after the preface of the *Communist Manifesto*, the first line, which everyone should know by heart: "The history of all hitherto existent societies is the history of class struggles." And I say to my students that that is either the most profound sentence to have ever been penned, or the most paranoid. One then unpacks the capacity of—at least, the way I teach the history of sociological theory—the amazing capacity of a single discipline to bear so much internal contradiction.

And yet, I think, I'm still in Sociology, more or less, in that I have something to say, or at least have some capacity to give people insight about, what is going on in their society, or even in their minds (which would be, in an educational setting, the first order of business). That is influence of an entirely different kind. That is, the interpretation of a particular line of Marx opens one up to the possibilities of seeing the way the world is carried forward, such that clearly some people do think that the history of all hitherto existing societies is the history of class struggles. Their ideas have been influenced by that interpretation. It doesn't strike them that their ideas are anything more than common sense—but then, you or I didn't think of it, so that it is now common sense in some very important particularities, and that kind of common sense, it strikes me, is what social science has at its best built for us.

You're right, Peter [Wood], that it's devastating to think that one social-scientific interpretation or another replaces other ways in which people can articulate their experience of the world, and I would hope that it doesn't crowd them out, but then that's a responsibility of universities and a responsibility of teachers. It's also the responsibility of professional associations, and lately I haven't heard any professional association in the soial sciences even give a moment's thought to teaching, except occasionally saying, "Undergraduate colleges, if you're interested in being more active in the association, we'll let you have a few sessions on teaching at our annual meetings, as an afterthought to what we're really about"—which, in my professional association, the American So-

ciological Association, is: constantly being behind the times . . . which strikes me as exactly the opposite of what social science should be about, but I'll stop there.

BROWNSON: I make my living advising families of wealth on their investments in financial matters, so I'm a bit of a duck out of water with this group, but Chandler [Rosenberger], I'd like to pose to you a series of questions picking up on your despair about the failure of our government to identify the enemy before September 11.

The first question is, why would we expect them to get it right? Second, in the social sciences, has there been a project in place looking for the enemy, identifying the enemy? If not, why not? If there was, and the enemy had been identified, in a meaningful way, why was there a failure to powerfully communicate that message—or is there simply no one listening? If the answer had been developed and powerfully communicated, all of which, it seems to me, gets to the issue of the influence of the social sciences, then—were they relevant in this case? Did they have an answer? And did they have enough influence to communicate the answer?

GLAZER: In Chandler's case, one of the problems is whether the field he's talking about, International Relations, is much of a social science. It's true that people try to order it and give it a theory and so on, but mostly it seems to consist simply of people who are well informed about foreign affairs. I don't see much else, so if they couldn't predict this—I don't know if it is a failure of social science or not.

And we've heard from Peter Wood about the influence on discourse, on language, of social-science terms. So then we'd have to ask—I don't know if he wants to apply the word "pernicious" to that influence—but when people use these terms, what do they mean and what do they think? I helped launch many years ago the terms "inner-directed" and "other-directed," and for a long time it was part of the language, and occasionally David Riesman or I would protest that they had it wrong or didn't quite understand what we were saying; but there's one aspect of the influence of social science. And Jonathan Imber had another angle.

There's one aspect I thought might be addressed which hasn't come up yet—maybe it has—but that is the influence on very specific policies; I'm thinking of, for example, the Council of Economic Advisors, which was created at a time when people were very high on economics. Very distinguished economists have headed and served on it, and I suppose there must be a history of the role of the Council of Eco-

nomic Advisors and the degree to which its advice has been taken. I recall that there was a time in the '70s, Dan Bell would remember it more exactly, when he was one pushing for a council of social advisers. You have to be very optimistic about the social sciences to say, well, just as the economists will tell you what the effect of this or that economic policy would be, the social advisers will tell you this will weaken the family or this will increase crime. . . . It would be interesting to look into why we never did have a council of social advisers.

But there is another point I wanted to make. There are certain areas of policy where possibly social science *has* had an influence. One of them might be our incarceration policy. As you know, we have become one of the great countries of incarceration in the world, 2 million prisoners, and 20 years ago we had a quarter of that or so. There are two social scientists, James Q. Wilson and John DiIulio, who have been in the newspapers recently, who played a role, at least, in putting incarceration at the center of the treatment of crime. They called it "incapacitation," and that replaced another social-science idea, or theory: that we could change people in prisons and improve them. The new view was, we couldn't change or improve them, all we could do was keep them there, and that would reduce crime. Now, obviously, other factors led to the increase in the number of prisons. It was good for the prison industry, certain communities like to have prisons, it gave employment to backward areas of the states, etc. And a lot of people just thought it was a good way to reduce crime.

But it would be interesting to look at a number of such areas in which there was clear influence made by a social-science argument, although the number of such areas is rather small. I think this is one of them, and, I am thinking of another, but there I think social science played a relatively small role: the general turn in almost all modern societies against high-rise buildings for low-income groups. I was in Washington working for the housing agency in the '60s when there was some research being done showing that conditions in high-rise buildings for low-income families were bad. Whether that was owing to high-rise buildings or to other aspects of their lives was hard to tell—this is a case where the social science was not as sharp and clear. (Not that it was that sharp and clear, either, in the case of incarceration.) In the 1970s we blew up one project in St. Louis (after taking the people out, of course!), and now we've blown up a lot more, and we know we shouldn't build high-rise buildings for low-income families anymore. The issue of how to trace out the social-science influence on these

policies would be difficult, but I throw these out as concrete examples where social science seems to have had, or may have had, an influence on policy.

HILL: If I may just say a few words as a complete outsider. I am a teacher of the humanities and I have no expert brief in the field of the social sciences.

With that in mind, on the basis of what I've heard this afternoon, it might be not unreasonably concluded by a well-wishing auditor that the chief aim of social scientists is to outdo each other in the creation of definitive aphorisms: "Cognitive egotism," "obliteration through incorporation." And one might also conclude in entire good will that the chief anxiety among social scientists is a self-created anxiety; it's an anxiety about the credibility gap that they themselves create between aphoristic mantras and hard evidence of fact. "Cognitive egotism" is a striking term. But it is not a new thing. I think the old philosophers used to call it "solipsism," and I think Jane Austen, in her novels, analyzed very well the ways in which sentimentality is a form of cognitive egotism. I think that I would conclude from this, in a rather contentious way—and I say it with great diffidence because I am not an American citizen, I am a guest in this country—but I've also heard several times this afternoon the term "9/11" used as a kind of automatic mantra, and I would say that there is some danger of the 9/11 mantra itself being a form of cognitive egotism, a kind of solipsistic self-projection. It could be a form of false empathy in which that which is desired is a self-created image of the innate grandeur of the American mind and spirit.

ROSENBERGER: I'd like to try to address Fred Brownson's questions and then also maybe include in that address a couple of points that Nathan Glazer had made.

Why would we expect our social scientists—our international-relations experts, our historians, our political scientists—to correctly identify who it was that was most likely to attack our country? Because, just simply because we hope that their primary occupation is with the empirical world, and the only person who had attacked our country for 10 years before September 11 was this man, Osama bin Laden. So, you know, there's some hope that an attention to reality would have been their primary focus, and I think that there was not that attention to reality, when there could have been. I don't think there was, even in the academy. I was going to say that the Defense Department documents that I looked at are especially egregious on this matter, but I don't re-

member seeing Osama bin Laden or his ilk discussed seriously in social-science journals, either.

Is International Relations a social science? Well, probably not. But there are plenty of other social sciences that we might have expected to address these questions.

MAZLISH: In the face of Professor Hill's comments, I need to gather my courage. I'm given some courage by the fact that for quite a long time I have tried to work between what are called "the two cultures," as sloppy a term as that may be, but in the effort to reconcile the humanities and science so-called, I of course became very aware that there is a third culture, social science. It has somewhat different aims from the humanities, but I don't see enmity among these three.

We're trying to see what are the lines of commonality, if you like. I do take your point about September 11 by the way, I would agree with that. But I would still like to take up Dr. Brownson's question as Chandler did a bit, and I would say first that there was a good deal of knowledge available about terrorism: there were numerous episodes in the past decade, not striking this country, but after all we are capable of inference and extrapolation—but that wasn't the job of the social scientists. It really was the job of of the policy people, of those who were in military intelligence and the CIA and so forth. They normally, I am afraid, like all the rest of us, lack vision. You get caught up in "cognitive dissonance." Not "cognitive egoism," although that is a very good term, too!

For example, the Israelis knew, before '73, that the Egyptians were massing their armies, were moving, etc., etc. They had perfect intelligence. They didn't act on it, for a very simple reason: they were convinced that the Egyptians were cowardly, not good fighters, you name it. They would never dare attack Israel. That's called "cognitive dissonance." In other words, you block out whatever it is that doesn't fit into your preconceptions. By the way, I think the reason September 11th seems to be so, you know, critical for the Americans is that this country has had a sense of invulnerability for so long that we thought we were not like other people. This strong notion of "we are unique" etc., etc., is deep in the American psyche, character, history, whatever you wish. So we simply were not prepared to read the signs.

I'll instance one other piece of evidence, if you like. This goes back to the Iranian revolution, in which the preconception, held by all social scientists, more or less, was that all revolutions go Left. That had been the pattern, the French Revolution right on through, and so the as-

sumption was that if the Shah were to be toppled, it would be by forces on the Left—the Mujahaddin. Now "Mujahaddin" has taken on different meanings, but the idea back then was that it was Left. And the notion that revolution might come from another part of society, in this case the religious, the clergy, was simply unacceptable.

There was one scholar who absolutely nailed it. This was a convert to Islam named Hamed Alga, who—before 1978, when Khomeini was still in Paris, and hadn't gone to Iran—wrote an article saying the overthrow of the Shah is going to come, sooner or later—he didn't predict the date—from Qum, which is the center where the clergy were being trained; and it will be led by a man named Ayatollah Khomeini. Now, I have to tell you I took that information to various friends of mine on the various intelligence committees, including the National Security Advisor, who happened to be a friend of mine, and they absolutely dismissed it; they said, "Bruce, you're really dumb if you can believe this." That's an example of cognitive dissonance.

CZERKOWICZ: In this discussion and throughout the day we've heard the phrase *the aim of the social sciences* several times, and I don't know whether it's assumed that we know what the aim is, or what the aim should be. I'm a political-science major because I want to make the world better, but there is something wrong in that motivation as an aim of the social sciences. Perhaps I'll use the word *should*, as I'm a student and I feel I'm allowed to be optimistic: so perhaps the aim "should" be a better understanding of the world around us, and in that respect there really isn't a difference between the aim of the social sciences and the aim of the humanities.

COSTELLO: I'm also a student, and I'm just wrestling a little bit with the idea that the politicization of the social sciences is a big problem, because I don't think that it should matter what your motivations are in trying to understand how humanity is, or why people do things. Because if it is truly a social science then the methodology should either support or refute what your hypothesis is, and therefore it shouldn't matter why you're undertaking the study, or what your hypothesis is, if it's supported empirically. And I think that that is why there are serious methodological issues within the social sciences, especially concerning experimentation, because I don't think that you can really isolate a single cause concerning human action, especially if you take a Weberian approach, where we trace ideas through generations before us and therefore you can't really limit why someone does something to a single reason causing a single action. Similarly, I think that you can manip-

ulate statistics to support whatever idea you present, and therefore I don't think that valid conclusions are drawn, and that is why social sciences are creating all of this supposedly empirical evidence to support their hypotheses.

WOOG: I'll touch on some points my fellow students have made that I hope will reach the overarching purpose of our conference. First, we have a process that scientists have used for centuries to study natural phenomena: conjecture and refutation. This works for social science as well as biological sciences and physical sciences. From the words of individuals, past and present, we can come to understand an entire society. We can glean ideas from the fossils of history, such as literature. To understand why we were attacked on September 11, we must talk to Osama bin Laden, or hear what he has to say on Al Jazeera, or maybe we can go to Guantanamo Bay and interview the detainees there to find out their views. Until we have done that, all arguments that we make are simply conjectures.

Conjectures that are untested are just assumptions, and they can have serious consequences. When social scientists espouse them without having tested them, they can influence public opinion dangerously. I disagree with Professor Mazlish. It's the responsibility of social scientists to come up with tested conjectures that can guide intelligent policy making. Right now, if policy makers listen to social scientists they're playing with fire, because of the generally poor state of affairs in social science.

FRIEDMAN: At the risk of beating a dead horse—regarding whether the failures of policy makers are traceable to the failures of social scientists. I can understand, having read some social psychology, the sort of cognitive barriers that would have prevented bureaucrats from alerting themselves, or allowing themselves to be alerted, by the evidence of actual threats because they, like all human beings, have a worldview, a paradigm, and they proceed within it until reality slaps them in the face. I don't want to draw a sharp distinction between policy makers and social scientists because policy makers should indeed be applied social scientists, and I don't see why social science shouldn't be in a condition where it actually offers useful knowledge to policy makers. But we live in a world in which there is a distinction between the two, although people cross back and forth between the two realms. And the failures in both realms strike me as cognitive, not motivational. It's not as if politicized social scientists are confronting opposition all the time and are dogmatically saying "No, no, no, I simply close my ears to any alterna-

tive perspective." On the contrary: they never hear an alternative perspective, and so they are only prepared to see the world one way, and all cognitively dissonant evidence is invisible to them.

That's a description, it's not a diagnosis. The dead horse that I'm going to beat is that Kramer's recent exposé of the Middle Eastern Studies Association, on the one hand, and the fact that there were scholars like Bernard Lewis who have been warning about Islamism and Steven Schwartz who have been warning about Wahhabi Islam, on the other, shows that it is possible to overcome cognitive barriers, or at least to have a different set of blinders than one that makes you expect that the only kind of opposition to the United States is going to come from the poor, because supposedly the poor have an intuitive grasp that "we," the United States, or the West, are responsible for them being poor—as we've heard for more than a decade: we in the West face military danger from the poor, from the Third World. Yes, it came from the Third World, but not from the poor; but the scholars were fixated on that idea, and I think it would be indulging in cognitive egotism of our own to be blind to the role that politics played in that form of cognitive egotism on the part of the scholars. Am I wrong? Not having read your paper, Chandler.

ROSENBERGER: I just wanted to say that I didn't mention Martin Kramer's book in the paper, but you're not far off what I was describing, I think. What I was describing is a more global problem—not that Middle East terrorism was covered up in some way or ignored by Middle East experts, but rather that people whose responsibility it is to consider the broader interests of the United States just weren't paying attention to this, for exactly the reasons that we've talked about and that you described, that they were obsessed with poverty as a cause of terrorism, rather than *ressentiment* or some other psychological phenomenon.

On the question of whether policy makers should be social scientists. I don't think they should be. I think policy makers should be elected politicians. I am not ready for the rule of the sociology department quite yet. But I agree that there is a connection, and I want to flesh out this connection, between the academics and at least the staffers in institutions like the NSA and the Department of Defense.

These people are frequently our former students. They are frequently Ph.D.s from the top institutions. There is a very close connection between their ideas and the academy. It may not be the responsibility of a scholar to go out and figure out which apartment Osama bin Laden is in, but it might be, it certainly is the responsibility of a scholar to de-

scribe the world to people who then go out and make decisions about how the United States should act in the world in a way that allows them to pursue rational policy, that identifies genuine threats instead of the ghosts of dead social scientists.

WILLIAMS: I just wanted to contribute to this conference by adding a bit of objective knowledge. I believe we've been using the term "cognitive dissonance," which is a term coming from psychology, incorrectly. We've been using it as meaning when a person, a social scientist, ignores some part of reality that is different from what they want to advance as the truth, and because this outside thing is dissonant with what they are asserting, they ignore it. That's not cognitive dissonance. That's just ignoring reality. That's just a lack of integrity. Cognitive dissonance is when, within one person, two opposing wills clash, creating psychological friction.

MAZLISH: Wow.

WOOD: I have been saying so many sour things today that I thought I would try to end the day on a cheery note. I am sorry that Professor Glazer left because he inspired a frenzy of good feeling on my part. The question is, on the influence of the social sciences, have there been some positive influences, and he mentioned some aspects of criminology—and I say "hear, hear!" Community policing, all sorts of good criminology ideas have come out of the social sciences. The social sciences may have done a poor job in preparing our policy establishment for anticipating the terrorist attack of September last, but after it occurred our social scientists, including my colleague in the anthropology department, Tom Barfield, did a splendid job in advising the American military just how to approach the factions in Afganistan, so that the Taliban would collapse. They took his advice; it was a marvelous success. Social sciences can do things like that. Barfield had spent a good bit of time in country in Afghanistan and he knew those factions, and he brought his knowledge to bear precisely on a practical question.

I did want to respond a little bit to Geoffrey Hill. This shadow of the humanities seems to me to have been an unacknowledged presence here today, and it is an opportunity to say something about how, if the social sciences, any of them, aspire to a degree of greater objectivity, we can aspire to being good social scientists without giving up our desire to ameliorate bad social conditions or without giving up our ideal of leading good lives filled with rich ideas from many sources. We do that by, to use a bit of jargon, "role specialization." By being an anthropologist, I don't give up my capacity to be a humanist or to be a natural sci-

entist, if I want to devote myself to that task. It seems to me that one good way out of this situation is not to pretend that we can be only one thing, but to realize that when we are being social scientists we are, to that extent, not being something else.

When Oscar Wilde gives us his portrait of what it's like to be a prisoner, in the "Ballad of Reading Gaol," he's giving us a sense of the misery of incarceration, of guilt and crime and punishment, that's beyond anything a criminologist could give. It's different—but the criminologist still has something important to tell us. I would say that we ought to be willing to do a certain amount of that horrible Bill Clinton term, "compartmentalization." Understanding parts of the world through acts of the imagination is a crucial component of our experience, one that I would certainly not wish to sacrifice in the pursuit of purifying the social sciences, but I think I can do both as long as I do them separately.

STONE: I've been grappling with some simple way of testing our achievements in the social sciences in a fairly personalized way, and because I come from an economics training originally and then moved over to sociology, I clearly am unbiased in looking at this particular case study. . . . I'm thinking about Robert K. Merton, the distinguished sociologist, whose name has been cited many times today in a number of regards. Well, one of Merton's great achievements was in the sociology of science, working with Harriet Zuckerman and others.

Showing who actually gets Nobel prizes in the sciences, his son, Merton Jr., is a very distinguished mathematical economist of risk analysis, another kind of social science that we've also been discussing today. What are the rewards of these two different approaches to social sciences? Well, for Merton Sr., obviously his achievement, or one of his great achievements, is that his son actually got a Nobel prize. For Merton Jr., he joined Long-Term Capital Management and nearly destabilized the world economy. So I think one can take this and draw one's own conclusions about different strategies in the social sciences.

SESSION V. THE NATURE OF SCIENCE

INTRODUCTORY REMARKS: Gerald Holton
Nikolas Prevelakis
MODERATOR: Nikolas Prevelakis
PARTICIPANTS: Carlos Casanova
Jeffrey Friedman

Liah Greenfelsd
Gerald Holton
Natan Press
George Prevelakis
Joseph E. Steinmetz
John Stone
Peter Wood

N. PREVELAKIS: With us today is Gerald Holton, the Mallinkrodt Research Professor of Physics and of History of Science at Harvard University, who is both a physicist and an historian of science, and the author of many books, among them *The Thematic Origins of Scientific Thought*. I'm giving the floor to Professor Holton; I'm going to make some remarks after that; and then we'll start the discussion.

HOLTON: Ladies and gentlemen, my task is essentially impossible: in 15 or 20 minutes to tell you something about the nature of the physical sciences. My remarks will of course be very abbreviated, and I know that others here could not only add to them, but could give this talk very much better. We have Robert Cohen, we have Glashow coming and other physicists coming, so they would tell you, I think, their side of the story in a much better way.

Now there are inevitably of course huge differences of opinion among people who are working in the physical sciences. There is a huge spectrum of interests and accomplishments. At one end of the spectrum is work, for example, on precision measurements to test theories; John King of MIT is an example. He measured the charge of the electron and the proton, which in theory should cancel equally in magnitude opposite in direction, and he found it to be true to one part in ten to the twenty-third. Now there are not many sciences, or any other activities of human beings, which try to reach that level of accuracy, and he did it not by social construction by the way, if I may just interject. At the other end there are the people who engage in vast cosmological speculations.

And parallel to that spectrum there is one that displays a healthy variety of answers to the question how to proceed, to make good science. The context of discovery, private science, rather than the context of justification, public science, in the famous distinction of Reichenbach. At one end of that spectrum there is the opinion of Percy Bridgman, his famous reply to the question of how to proceed to do research: he said, "The scientific method is doing one's damnedest, no holds barred."

And at the other end is the textbook definition of the supposed hypothetico-deductive method, which is said to proceed in six steps. First, propose a provisional hypothesis by induction from experience; then refine this hypothesis by mathematical or other means; then draw logical conclusions and assemble predictions; then check against experience, or experiments; and then, to pacify Karl Popper, try to falsify all the work that you've done so far; and if that falsification fails, then you have achieved what you can proudly call a "universally valid result," which may or may not be accepted in public science by the organized skepticism of the profession itself.

Now that is a textbook recipe and it is, of course, generally regarded as a caricature of actual practice, but it does have a kernel of truth. It hints at a worldview more or less shared by physical scientists, though in large part not consciously. A list of components of such a scientific *Weltbilt*, if I may use the word that's favored by the gentleman who is behind me [large portrait of Einstein] and who makes me cower, might include the following components: a high place for objectivity; preferably quantitative rather than qualitative results; extra-personalized, universalized results when available, instead of ego-centered or unique, not generalizable results (this is, incidentally, why Ernst Mayr is so angry at physicists, because he likes the unique rather than the universalized); abstract, divorced from the direct sensual world of experience, I would say de-eroticized, de-anthropomorphized. Next, rationality rather than moralistic thinking. Then, problem-oriented as against mystery-oriented or purpose-oriented. Next, truth-oriented. Then, Enlightenment-based, therefore opposing the sacralization of any subject, and I'll come back to that at the end. Next, a tendency to accommodate contrary views only if proven, but to be open to debate and new experiences—what has been called the democracy of intellect rather than the aristocracy of intellect. Next, the view that there is a hierarchy among fields of knowledge, with the more fundamental ones serving as the source of explanation for the rest. (This is very often in the work of Stephen Weinberg, and it is contrary to, let us say, Stanley Fish's equivalence of the laws of physics with the rules of baseball, which he once wrote up in the *New York Times*.) Next, avowedly secular, anti-metaphysical, disenchanted, and I'll come to that at the end also. Evolutionary, rather than preferring discontinuous or revolutionary changes. Cosmopolitan and globalist in reference and in hopes for acceptance. This is a brief sketch of the elements of a typical physical scientist's world-

picture, unrealized in the intellect but deep down in the motivating emotions.

Now as sketched, this worldview is, of course, often under attack, chiefly from non-scientists, from ancient times on through the Romantic rebellion of the nineteenth and the twentieth centuries and still ongoing. Now most scientists pay no attention whatever to these attacks, but both they and their opponents fail to see two more essential components of the scientific world-picture that greatly modify the severe, apparently totalitarian or over-intellectualized, account so far.

The first of these two active components is, as I have found especially in studying the nascent period in the work of outstanding scientists such as Kepler, Einstein, Planck, Bohr, Heisenberg, that they rely heavily on implicit references, on preconceptions, on presuppositions at the very beginning. Einstein himself recognized and commented on this repeatedly. He said, if the researcher went about his work without any preconceived opinion, how should he be able at all to select out the facts from the immense abundance of more complex experience, and to select just those which are simple enough to permit lawful connections to become evident? By way of example, he discussed the dilemma that in formulating the laws of mechanics, one has to follow either the natural tendency, as he called it, of mechanics to assume material points—which necessarily leads to the presupposition of atomism—or else to erecting a mechanism of continuous media based on another fiction, for instance, that "the density and the velocity of matter depend continuously upon the coordinates of time."

These fictions—and they are not unrelated to what Frank Kermode, in another context, called "The Necessary Fictions" that are found in the heart of literary works—these have, of course, considerable practical value. For example, they guide the development of mathematical tools—in Einstein's last example, partial differential equations—but they are much more than that. Einstein referred to them as categories, or schemes of thought (and I'm quoting), "the selection of which is in principle entirely open to us, and whose qualification can only be judged by the degree to which its use contributes to make the totality of the content of consciousness intelligible."

An example of such a category is the distinction between sense impressions and mere ideas. Einstein warned that we do not conceive of these categories as unalterable, conditioned by the nature of understanding (and in this way he was completely opposite to Immanuel Kant), but as, in the logical sense, free conventions. They appeared to be

a priori only in so far as "thinking without the positing of categories and of concepts in general is as impossible as breathing in a vacuum." As I've tried to show in a number of studies of scientific work, we can recognize the existence—and, at certain stages of scientific thinking, even the necessity—of postulating and using precisely such unverifiable, unfalsifiable, and yet not-quite-arbitrary conceptions; a class to which I have referred to as "*themata*."

Different scientists are attracted to different *themata* and allow themselves to be led to them to different degrees. And some can do without them entirely, such as Fermi. This explains, in part, why some results are accepted by others, either quickly or slowly, and why there are oppositions, such as between Schroedinger and Heisenberg, when actually they are talking about the same thing.

Among the *themata* that guided Einstein in theory construction are clearly these: the primacy of formal explanations as against materialistic ones; unity or unification and, on a cosmological scale, logical parsimony and necessity, symmetry, simplicity (even Newton asked for this in one of his principles of reasoning); causality, completeness, continuum; and of course constancy and invariance. It is *themata* such as these that explain in a specific case why a person like Einstein would unshakably continue his work in a given direction even when testing against experience was difficult or unavailable, and sometimes even opposite to the results of experiments.

In 1906, when Max Planck was faced with a roomful of people to whom he was trying to sell relativity (he invented the term *relativity theory*; at that time, Einstein talked only about "relativity principles"), the audience was largely against him and pushed him to the wall and said: where are the experimental evidences? And finally Planck had to give in and say, "I believe in it because it is to me *mehr sympathetisch*"—it is more sympathetic to me. It took 11 years, really, for people in large enough numbers to rally around relativity.

His reliance on *themata* explains equally why Einstein refused to accept theories that were well supported by correlations with phenomena, but which were based on thematic presuppositions opposite to his own—and that was the case, of course, in the quantum mechanics of Niels Bohr's school, with its discontinuities, with its probabilities, with its lack of complete explanation of phenomena, which were hidden behind that uncertainty principle.

Now in addition to the use of unprovable and unverifiable *themata* by certain scientists, there is, as I said, a second missing component of

the world-picture as given before, at least for some of them, and that component is—and I hesitate in this room to say it—that component is a secret feeling of "the moral authority of nature," to adopt a phrase used recently in the Tanner Lectures by Lorraine Daston. This feeling shows itself in the motivating sense of awe before the beauty and simplicity of nature's laws—laws that can explain in a few lines, or equations, an essentially infinite number of phenomena; and there are so few laws! In physics there are fewer than 20, and they apply from eternity to eternity, and from one end of this universe to the other—14 billion light years away. The hydrogen atom, from the beginning, behaved itself the same way as it does in our lab now, as we can see by looking at very distant phenomena. This is, I think, why Ludwig Boltzmann, at the beginning of his *Treatise on Maxwell's Theory of Electricity and Light*—as he called it, with its four simple, symmetrical, and all-powerful equations of Maxwell—why Boltzmann put the lines from Goethe's *Faust*: "Was it a God who designed this hieroglyph? Into one whole now all things blend." Maybe this feeling of awe, which the sociologist Ben-David called "the charismatic element of science," demands from its serious admirers the most fruitful labor, and also accounts for the fact that eventually Boltzmann, apparently distressed about his own work, committed suicide—as did, or as attempted by, several other physicists who came to feel powerless in the work they thought was demanded from themselves.

As usual, Einstein put the feeling of awe before nature frequently in quasi-religious terms, saying, for example, that "the only true religious people are the scientists, and all the others use their religion to fight against each other." He might be right. In 1918 he said, "The state of feeling which makes one capable of such achievements" as, for example, the work of Planck, "is akin to that of the religious worshipper. His daily strivings arise from no deliberate decision or program, but out of immediate necessity." He might have been speaking in the same way of the poets, who a century earlier had railed against scientists.

In conclusion, let me suggest that the more complete picture of the scientific worldview, which I have sketched so briefly here, is one that allows *themata*, necessary fictions, and the power of the sense of wonder. When these are added to that more severe list I started with, then the human element in the doing of science is brought into view more clearly, and perhaps helps us to understand why mere human beings have been able to reveal so much over time about that magnificent continuing puzzle, the world of nature, which ever stands before us.

N. PREVELAKIS: Thank you very much, Professor Holton. Before starting the discussion I am going to just add one or two remarks to help us in our task of discussing the social sciences. It would be, maybe, interesting to discuss science as a social institution. We talked yesterday of scientific progress: whether there has been any progress in the social sciences, and if not, why? We have touched upon the scientific method, the method of conjectures and refutations. It would be good if we could discuss today the grounding of this method and how it is supported by science as a social institution.

The scientific method of conjectures and refutations would probably not exist if it were not supported by the goal of science as a social institution—the goal of understanding empirical reality and finding regularities in nature. It is this institutional goal that demands and that makes necessary the method; if the goal were different, if the goal were ideological, or if it were to provide social happiness, for instance, we could expect a different method, but then we might not have the accumulation of knowledge that characterizes science. We can see this, for instance, in other spheres of social activity. Literature, for instance, does not have the same social goal, and therefore one would have much more trouble arguing that in literature, one can find progress similar to what we have found in the natural sciences so far.

This premise leads me to ask three questions. First, if what characterizes science is this goal of the social institution, is there something like a necessary methodology that would appear in the natural sciences and also in the social sciences—if they were to be truly scientific? We touched yesterday upon the question of whether physics should be the model for the social sciences. It would be interesting to hear from natural scientists today to what extent quantification or mathematicization are absolutely necessary components of scientific activity. It is a pity—I don't think we have any biologists here today. But—yes, we do! That's good. Yes, neural science.

STEINMETZ: I'm not really a biologist.

N. PREVELAKIS: In any case it would be interesting to discuss areas of biology, for instance evolutionary biology, that have very limited quantification and that don't have universal laws, to see whether we can isolate methodological components that are necessary for any scientific activity, or not; and how does this influence our discussion of the social sciences?

Another question which I'm afraid to even mention, since it has been developed largely by Professor Holton, is the process of the for-

mulation of theories in science, and the nature of imagination, and the importance of imagination. There's this widespread idea that a scientific theory is a theory that derives somehow from automatic induction from the data, from the facts, and that it requires very little imagination—and that the more imagination we have in the formulation of theory, the more subjective the knowledge. In fact, one could argue that the formulation of a theory is as imaginative a process as the writing of a novel, for instance, or the composing of a piece of music. Is it in the formulation of theories that science is distinguished from other activities, or is it just in the testing of evidence; and how important is imagination in the formulation of scientific theories?

The third question, which was also raised yesterday, is the question of the external influences on science. How impermeable is the scientific activity to social influences, to ideologies, etc.; how impermeable have the natural sciences been to that? We raised this question yesterday regarding the social sciences. It would be interesting to see how this works in the natural sciences and, if we want to be more precise, we could see the role of external influences or ideologies or images of order—*themata*—in the formulation of theories, in the imagination of theorists, but also in the choice or the direction of scientific investigations. For instance, does a technological need of a society at a given time influence science, influence the direction of scientific research, and to what extent? To what extent are the choices and the directions of sciences dependent on the inner logic and the inner development of science; or are they directed by other social concerns? What is the influence of the demand for social positions in science? Does the specialization and the development of new fields in science derive from a social demand, or does it obey the inner logic of scientific development? A related question is the role of external social influences in periods of what Kuhn called "scientific revolutions." When there is a crisis of a paradigm, social influences are, it seems, much more important. How important is that for the development of science?

Of course we should not lose sight of our main subject, which is how we can use these conclusions and experience from the natural sciences in our discussion of the social sciences, which, as we concluded yesterday, do not seem to have the same accumulation of knowledge as the natural sciences. We know that they have made very little progress as compared to the natural sciences, although, of course, there is social demand; and we have discussed their influences in society and the de-

mand of society for some kind of knowledge and some kind of advice from social scientists.

GREENFELD: I shall permit myself to be authoritarian and take the floor, even though the moderator has not allowed me to do so, just to connect what Nikolas so very well expressed to what Professor Holton was saying.

First of all, what Professor Holton described as the motivating emotions of physical scientists we would call, in our discourse, the "normative structure of science," and, as Nikolas pointed out, it is directly related to the social goal of the institution of science, which is the goal of understanding empirical reality. It is from that that the so-called scientific method, that normative structure, derives. In other words, the normative structure of science—is the scientific method itself.

In addition, I wanted to ask the question that Nikolas asked, perhaps just in different words from the several ways in which Nikolas asked it, for it is a very, very imporant question. Given that the "human element," in Professor Holton's words, is so central in physical science—the unprovable and unverifiable *themata*, the sense of wonder, imagination, which Einstein emphasizes above exact knowledge—given that this element is so central in the physical sciences, what prevents social sciences from advancing at the same pace?

CASANOVA: I have some questions about Professor Holton's talk.

The first is, he says that physics has developed along evolutionary and not revolutionary lines. If that's in answer to Thomas Kuhn, I agree. I don't agree with Kuhn, but when we see what Heisenberg, Schroedinger, and Einstein did, I cannot help thinking that it was a revolution in some sense. For example, Heisenberg came to the United States and had a discussion with a pragmatist philosopher, as related in *Physics and Beyond: Encounters and Conversations*, and he said: look, the development of physics is not like engineering, because physics is an exact science. We haven't developed the Newtonian axioms, we have founded *new* axioms, and those axioms are not a simple development of Newtonian axioms; but Newtonian physics was right, Newtonian physics was true. It's not untrue now, Heisenberg said; its axioms will be true forever if we accept the exactness of his instruments—didn't he? And we have to suppose Newtonian physics to make our own experiments. And he said that then there was a revolution.

Paul Forman, the sociologist of science, investigated the development of physics in Weimar Germany and I believe he showed that public opinion influenced the acceptance of the new axioms but didn't, of

course, determine their discovery, because the scientists were looking for truth, not for being well-regarded by the public. That's the first thing—if physics is an exact science, it must be revolutionary, because it discovers new axioms; it doesn't just develop the old axioms, like engineering does. But though the development of science may be revolutionary, Forman showed that it wasn't so in Kuhn's sense.

The second thing is that I think that the very use of mathematics makes physics *not* the first science. I know many physicists in my university in Venezuela who think that physics is the first science, but I tell them: look, it cannot be, because physics uses the results of mathematics as a tool, but it uses those results even while the mathematicians don't know in which sense mathematics is true; and the investigation about the truth of mathematics doesn't belong to mathematicians, but to philosophers.

I agree that in physics you need preconceptions, but, for example, for the measurement of temperature, which is not at first glance a quantitative notion but a qualitative notion, you use a substance that expands in response to heat. Using mercury to allow us to measure temperature quantitatively—that's a convention, and that convention, applied systematically, allows us to make a science of nature. But before the convention, of course, you have to know what mercury is, what temperature is, and so forth. For this reason, physics cannot be the first science.

And the third thing, connected with that, is the following: you [Prof. Holton] say that the laws of physics are applied everywhere. I don't agree, because there are phenomena, biological phenomena and human phenomena, that physics does not explain. I know what you mean, of course, but the problem is that some people push the limits and try to apply physics to everything—that is, they are reductionists—and I think that a good case against reductionism can be seen in Roger Penrose's *The Emperor's New Mind*, where he tries to demonstrate that reductionism in the philosophy of mind is possible—but in the end the reader is convinced that it is impossible.

There are many different realities and there is room for many kinds of research, research tailored to the character of the reality being studied.

HOLTON: Thank you for your remarks. I'll be very brief.

I agree with you on the matter of revolutions in the following sense, that elaborate theories about revolutions such as that of Kuhn have been much examined and I, for one, associate myself with those such as Steve Weinberg, who say there was only one great revolution in physics,

the Galilean revolution. That is to say, the beginning of the seventeenth century saw a scientific breakthrough. That one certainly was a novelty, which shook not only physics but all society. You remember John Donne's poem of 1611, in which he says that all our old ideas are now overthrown, all the old philosophies gone, all "atomies" are now dispersed—that indeed was a cultural revolution as well as a scientific one, and it had—I'll come back to this in a moment—but it had some importance also in another aspect of what you have been talking about.

When it comes to other so-called revolutions, such as that of Einstein behind us here, he constantly was beleaguered by the charge that he had caused a revolution, and he always, without exception, opposed this idea with respect to his relativity theory. He said, in a letter which he wrote to one of his friends at the time when he was writing his manuscript, "I am working on a modification of the ideas of space and time." He saw himself as a continuist. Over his bed in Princeton was a picture of Newton. He was a Newtonian even in thinking about causality as being essential. So to him the idea of revolution was one that was quite foreign, and there is but one instance when he used the word *revolution* in one of his letters about his work—not about relativity, but about the idea of the photon, the quantization of light; he writes to a friend, "This you will find a revolutionary work," and what he means is that it is without basis in theory. It was put forward by him without having any substantial backing. That's what he meant by "revolutionary."

When you come to Heisenberg, there is, I would say, certainly a breakthrough, indeed two of them: one is that Heisenberg adopts the ideas of discontinuity and probability as fundamental; and the second is that he gives up the idea of the visualizability of physical processes at the atomic level. But if you look at where these ideas come from, they have a long history. Discontinuity already was discussed in the 1911 Solvay Congress. Poincaré wrote a letter in 1912, shortly before he died, in which he says that these people have been talking about quanta, which implies discontinuity, which Planck himself so hated that he was the last to accept quantum theory, I believe. And then Poincaré adds, "We now have to live with discontinuity"—this is 1912, and of course, young Heisenberg knows how to live with discontinuity. He adopts that, and so there are these traces ahead of these breakthroughs. The breakthroughs are not to be demeaned, but the fact is that it takes a genius, a person like Heisenberg—who writes to Pauli, when Pauli is worried about the size of this breakthrough—he says:

well, the ends make sacred the means. To him, the end is the important thing, whatever the means; whatever he has to throw out is not that important, so this is a trait, a human trait in him, which allows him to profit from something that's already been ongoing, hence a breakthrough.

Bohr is another example, but I won't develop this. We do agree as long as we keep away from the generalized theory of revolutions taking place all the time, and in between there being puzzle-solving, so that between one revolution and the next there is no progress likely in the sciences. That aspect, I think we both agree, is not correct.

Lastly, let me say something about the application of laws of physics. Of course they don't apply necessarily outside physics, although, strangely enough, they've found their way into biology, the laws of conservation of energy are there; but I was talking only about the limits within which physics itself works, and that is an astonishing thing which sometimes gets challenged. People like Eddington thought perhaps the law of gravitation is changing in time, and that is something that may yet have to be examined, but on the whole, the current laws of physics are more or less those that work, and there will be others because the whole purpose of science is to do better science tomorrow. And so we are going to have wonderful new things coming that show that the old is not quite sufficient. That is not deniable, obviously.

My last remark, and I don't think I'll, at this point, respond to the interesting ideas that you [Nikolas Prevelakis] raised, is about Galileo himself. Galileo, you see, acted entirely outside the social institutions of science. Science was alive long before there were social institutions for it. The scientific societies were founded afterwards, after his observations. In a sense, they, along with journals, with professorships, with all the things that make for social influence and institutions, all imposed themselves on science, owing to its apparent success at the time and the patronage that it achieved. But science could have gone on without it. The Royal Society, at the beginning, was a bunch of amateurs who showed experiments to each other. It could have gone on like that and could still be called "science." It wouldn't be as good as it is today. But it is not necessary to couple the effects of science with our understanding the fact that the social institutions exist to further it. The joining of those two is fascinating and empowering and worth talking about, and yet the two are not necessarily linked. I'll turn this over to you.

STEINMETZ: First I want to thank Liah Greenfeld for inviting me

here. I am the token natural scientist, I think, here amongst you, and I listened to yesterday's discussion with great interest because I sit in a department that straddles the natural sciences and the social sciences. I chair the Department of Psychology at Indiana University, and within this department we have faculty that range from molecular biologists to social psychologists and clinical psychologists. But I think the difference in the definition of this particular department has been the dependence on empirical science by every single member, regardless of which area of psychology they actually represent.

Now for myself, being in this department sort of flavors the way you look at science, and I consider myself something of a hybrid scientist, if I can use that term. My training is as an experimental psychologist, and to prove that, I'm the current President of the Pavlovian Society, which is an old society dedicated to the promotion of Pavlovian conditioning, of all things. Why? Because I've used Pavlovian conditioning as the basic behavioral technique to study the brain. So as a neural scientist, a natural scientist, the natural-scientist part of me, I'm interested in brain function. My main technique is the recording of single-cell activity in various regions of the brain during Pavlovian conditioning, mainly. I just say that as a preface because it sort of sets up my view of what science is, and I hope to address a few of the things that Nikolas [Prevelakis] brought up, from the perspective of a practitioner of this hybrid sort of science.

Let me talk about neural science in particular, because I think it provides a good example of a modern natural science. First of all, neural science is a relatively young science. It's been around for about 30 years, even though we've been studying the brain for a few centuries now. But as a recognized, independent field, it was formed around 1970 or just prior. So, as sciences go, or as organized sciences go, it's fairly young. I would describe it as an empirical science, meaning that everybody who is in neural science, or nearly everybody, does experiments, as described by Professor Holton earlier, using some derivation of the scientific method. It's also considered by many—I heard the term yesterday—to be a factory science (I think that's the term that was used), in the sense that it's incremental. The subject matter requires that, I think, work is done at many levels. It's a chipping-away process to understand brain function.

So I think neural science differs from a lot of what I heard described yesterday of the subject matter of what people study. When you get to the level, for example, of one of seven subtypes of an MNDA receptor

that somebody spends 20 years studying, it may sound pretty small and irrelevant, but I think the success of this science has been based on the fact that there are many people willing to do this, and that incremental knowledge is added to a larger base of knowledge, which tells us something about the MNDA receptor. Now, what's an MNDA receptor? Well it seems to be the fundamental receptor for learning and memory. So for somebody who has a theory of learning and memory, this very small step is extremely important.

Neural science is an integrative science, an interdisciplinary science, and I think it represents a new type of science that's also present in other areas like genomics and proteomics, where we actually have a collection of scientists who work at various levels of analysis, from the most molecular to the most systems-oriented. An interesting result of this process is, I don't see the existence of what I'll call the sole scholar very much in this discipline, meaning somebody who works by himself with very little contact with other scientists who are in the discipline. I think those who are most successful in neural science are those who work with others. And the reason for this is that it's been almost impossible to keep up with the development of technology in this field. Being an expert in everything it takes to do this kind of integrative work is impossible. So as far as the sociology of science or the socialness of science goes, there's a whole wrinkle here in the last 30 years, and perhaps one of the things that stand out is how students are trained now, relative to the way they were trained when I was a student 30 years ago. They're trained as collaborative, interdisciplinary scientists, not as people who are off by themselves not talking with others.

Our discipline has a language of terms and practices. Believe me, "modernity" is a term that I've never heard in a neural-science meeting. And by the way, the discussion here has been at a level of scholarliness that I've never seen in a neural science meeting, either. I mean, neural scientists and natural scientists typically get to the heart of the problem or the experiment in about 30 seconds, and there's very little high-level discussion of concepts and things like that, which is a fault, I believe, in the neural sciences.

Just a few other things, just to give you another flavor of neural science as a natural science. The concept of objectivity that was raised yesterday I interpret as the idea that for some reason, natural scientists are more objective about what they study. I don't think this is absolutely true. I think what is objective are the measures that natural sci-

entists use, and that's why in this science replication is extremely important, and that the measure that one use be replicable by others; as a result, you have a tendency for theories not to hang around in the field for long, because they can be put to the test more readily because of the existence of these objective measures.

But the scientist is still subjective. I'm subjective when I have a viewpoint about what the cerebellum does in learning and memory, based on the background I come from, the school of thought regarding the function of the brain itself. Experiments that I design reflect that background, the prejudices I have about what the brain should be doing, and so the objectivity is only there because of the measures. The data are the data, as I've heard many of my colleagues say, and if they don't fit the hypothesis that you've come up with, you have to change it or drop it or do something else.

Just a statement about what influences the choice of a question one asks in this science. The federal government does have influence for sure, because we are very dependent on the National Institutes of Health and the National Science Foundation for funding for any of the work that's done, and they have priorities. In my field, for many years investigating schizophrenia and problems in learning and memory were initiatives that were funded by the government that heavily influenced what research was selected, because it's an expensive science, and you're very dependent on this funding. Another example is the National Institutes of Mental Health, which have a huge drive right now in an area they call "translational research." This is a reaction to the failure of clinical psychology in a lot of respects to solve clinical problems with research monies that they've given them over the years, so what the government has said is, you must link yourself with basic behavioral scientists or basic neural scientists or cognitive scientists in studying your problems. This is a mandate for a chunk of money that's been set aside specifically for this, but in a lot of respects this influences what questions might be answered.

People mentioned quantification yesterday and its rise to prominence in social science. Something similar is happening in natural sciences. I think the most exciting work that I see done right now is by people who are linking computational models of brain function or behavioral function with empirical data; together, these two have, I think, made predictions about where experiments should go in the future, so it's become a very, very important tool in the neural scientist's arsenal.

And finally, one comment about the human element in science: nat-

ural scientists are not devoid of having reasons for conducting research, reasons that revolve around bettering the human condition. Like social scientists who may go into this field or take up experiments because they want to better the human condition, I think there are natural scientists who do the same thing, only it's disease states that they're looking at, not social problems.

G. PREVELAKIS: I'd like to address the basic questions of this session—which I think are, on the one hand, the question of scientific institutions and scientific imagination and, on the other, nature and society in science—through the case of Jean Gottmannn, a great geographer, whose story is in my paper [for Session II]—but I would like to come back to it and to stress a few points.

Gottmannn is known for many things, but not for the most essential aspect of his work. The most essential part of his work is an act of imagination. In the late '40s and the beginning of the '50s, he develops a theoretical framework for geography that corresponds to the great idea that Professor Holton mentioned, in that it's not based on empirical data. Of course, Gottmann was somebody who knew the geographical literature very well, but he put all that aside and he made an act of imagination, and this act of imagination first of all started from basic hypothesis, which is a *thema*; I'll come back to that. Second, it led to a very elegant and an extremely simple framework, echoing Professor Holton's point about the moral or religious principle of simplicity.

What he said is so simple that I can present it to you in a minute or two. He asks, what is our basic problem in Geography? The basic problem in Geography is the partitioning of geographical space, the fact that geographical space is divided, subdivided, it's not continuous. He starts from a proposition that is unprovable. He says: Mankind always partitions space. You cannot prove it—and in fact it is contrary to what seems to be the general belief today that we are moving towards globalization and therefore towards the unification of space. And then he asks, How can I explain the partitioning of space, or rather how can we explain the change of partitioning of space, which is fundamental for Geography (but also for social sciences, I would say)?

He answers that there are two groups of forces at work. One is the movement factor—circulation—and this is something so obvious that nobody can argue against it. Circulation comes from nature, it comes from men's activities, it comes from the fact that everywhere there are differences of potential, natural or man-made. And then he says, well, if

this world of circulation were free, then it would be extremely destabilizing for mankind, so human societies have created defensive, self-defense mechanisms that impede circulation, which he calls "iconographies," allowing him to introduce all the cultural factors. The interplay of those two forces can interpret all geographical phenomena.

You see, it's extremely simple, and it comes from an idea, so I see there, if you like, an abolition of this division between physics and natural sciences on one hand and social sciences on the other.

But what happened? We have an act of imagination, and then nothing happened, because the scientific institutions were not interested in it. This is how I see, how I perceive, the influence of society. We have at any time, probably, a large number of acts of imagination, of the creation of science; and then we have a selection, a natural selection through our institutions, so our institutions are there in order to introduce the social element, and this can be a positive or a negative. Usually, I would say, in most cases, it must be a negative influence because it is a factor of censorship. Now the scientific institutions, in order to perform this function of selection, have to legitimize themselves, so they have to create their own ideology, and this ideology creates a false image of scientific discovery. This ideology says all those things that Professor Holton mentioned: that we have to start from empirical data and so on. All those are, I think, legitimizing concepts that create a false image, and of course they have their effect also in restricting the imagination of young people.

Some are able to overcome it. Some are iconoclastic enough to refuse to obey, but I think that most follow this principle and it becomes extremely pernicious when it is transferred from the natural sciences to the social sciences. This is what I think happened with the social sciences. They were so fascinated by the successes of the natural sciences that they tried to imitate, we tried to imitate, but we did not try to imitate Einstein; we believed the ideology and we followed the ideology, and I think that this is why we failed. So it's not, I think, a question of not receiving influences from the natural sciences, but rather that we have misinterpreted the information.

WOOD: I would like to propose a question to Professor Holton. It's a genuine question, not a sort of endorsement of a view that I'm going to hide in the question itself. Among the dozen or so elements of the typical world-picture of the typical scientist, you mentioned the hierarchy among areas of knowledge, and referenced Steven Weinberg. That seems to me to be one of the elements that goes to the heart of the

enterprise of this conference, which is, in part, trying to figure out the scientific status of the social sciences. The issue is whether there is genuine continuity or discontinuity—not the discontinuity of scientific revolutions, but the kinds of continuity that connect one kind of knowledge to another.

Now in recent years there have been some strong elaborations of the claim that all knowledge is at some fundamental level unified. I'm thinking of E. O. Wilson's "consilience," which puts physics at the base of this hierarchy of knowledge, but that's of course not the only one. I think Professor Prevelakis's mention of Gottmann points to another kind of consilience, an idea that there is some kind of complete system that would connect the sciences and geographic knowledge through this schema of circulation and iconography.

The references that Professor Steinmetz was making to interdisciplinary studies like genomics and proteomics point towards a kind of breakdown of consilience in a way. It suggests a sort of flowing together of knowledge that doesn't privilege some one field over all the others, and I come away, as a non-scientist, or a quasiscientist, or a social scientist, generally perplexed about all this. In the field of the history of science and philosophy of science, I look for some enlightenment as to whether the disciplines—which can obviously conduct their business without answering this question; one can get along in physics or biology or chemistry without having a grand theory of unification—but at some level, there seems to be an intuition that knowledge, at least in the sciences, has some deeper consistency, and whether that is so or not I think bears directly on the question of whether the social sciences should attempt to find their place in that hierarchy, or should instead just call the game off.

Of course in some of the social sciences there is a debate—Professor Coulter, I think, knows quite a bit about this—about whether there should be a kind of antifoundationalism that would just let areas of knowledge proceed along their own chosen paths; and these questions of deeper continuity become superfluous, so there are articulated views that are utterly the opposite of E. O. Wilson's attempts to create grand syntheses. These alternative views say that such a search for syntheses is a hopeless delusion. I stand baffled by this and I seek counsel.

HOLTON: Well thank you. This is very imaginative. Constructing hierarchies among the sciences, of course, is an old game. Auguste Comte famously put all the sciences into order. Luckily, the National Science

Foundation doesn't believe in it, and gives less money to mathematics than to physics, for example, contrary to what Comte might have advised.

But you know, I think it is a very bad game. It perhaps comes all the way from the schoolboys' experience with bullies who become the top of the hierarchy. In fact, it is true to some extent in the sciences, in the physical sciences, that there are ways of understanding phenomena in one field by digging lower to the next. For example, the way gases behave had been a puzzle for a long time. Bernouli made a good stab at it, not much admired at the time, but finally atomism became unchallengable, and that was not until about 1908. Even Ostwald wrote his second edition of *Allgemeinen Chemie* without, in chemistry, having atoms, because they were hypotheses; or as Ernst Mach says, "Have you ever seen one?" So it became necessary through experiments such as those of Perrin. And so then it was clear that there is a hierarchy, that is, from atomism you can build up a great deal of what the other sciences are doing.

Nowadays Steve Weinberg would say you've got to know about quarks in order to understand everything else that is science; to which P. W. Anderson answers, in a famous article, by saying No, we in condensed-matter physics don't need quarks, they'll never explain anything that we are doing. We are not satisfied with hierarchies at all. We would like to have our own layer of authority.

That leads me to think, and I think here we are on the same line, that the answer to this hierarchy game, and particularly to the fact that some, possibly social scientists, feel that they are not high enough in the hierarchy—it leads me to think that this can be cured by a revision of how the sciences cohere. They cohere by virtue, not of hierarchies, I believe, but by virtue of the complementarity principle of Niels Bohr, expanded—as he wanted—to more generalized things. That is to say, in physics the complementarity principle says that all the different layers of interpretation—for example, the electron is a wave, as against the electron is a particle—each is true in its own setting, in its own experimental lab, in its own theoretical way; but what is really true above all is the fact that all of those things, together, even though they may apparently be opposite, all of these things together form a kind of layer cake of reality, some of the layers being very different from others.

I always think of our old-style *Dobosh* torte in that sense, which has these wonderful layers of different flavors, and it is the whole cake, rather than any one of these layers which is the important thing. And I

would suggest, therefore, that this quasi-inferiority complex of some, feelings such as those that animate the chemists versus the physicists, and the physicists versus the mathematicians—that this is really something that goes back to the childhood play-yard, and that we should give it up and think in terms of Bohr's complementarity, science as a great cake that has its many layers.

Otherwise we get into terrible trouble, and one trouble that we got into because of the fight between Weinberg and Anderson on the subject of hierarchy was that it cost us the superconducting supercollider, because the community around Anderson, that is to say the solid-state community, was against the expenditure of the money that would have built the collider, and we very much miss the fact that we do not have that, because it would have done a very beautiful job of explaining not only what's in elementary particle physics, but far beyond.

GREENFELD: Science as a "*Dobosh* cake" is a very sweet metaphor, and this is exactly what I want to argue for, on several levels. First of all, I propose to Professor Steinmetz that we bake our own *Dobosh* cake of the—what to call it?—the layer-cake of reality that we call "the mind."

But I wanted, actually, to respond first to Professor Holton, and to clarify something very important for us here, which is the institution of science. Professor Holton and most of us are using the phrase *institution of science* with two completely different meanings, and we are also using the word *institution* in two completely different meanings, and both of them are absolutely legitimate, and true in their own place. Professor Holton is speaking about science basically in the private sense, and basically in the context of discovery: what happens in the thought-process of a scientist, a person who wants to understand reality. But we are also talking about science as an organized activity, as a collective activity of many, many people. Science as a private thought-process and science as a collective activity both exist as such, and they are both very important. And for either of them to succeed, they have to be necessarily combined.

Professor Holton is using "institution" in the very concrete sense of *a* social institution, such as the Royal Society or the National Science Foundation, or, I don't know, a university. But I, of course, and Nikolas [Prevelakis] were using the phrase *social institution* as a famous sociological abstraction, not at all connected to any particular organization.

Now, obviously science in the private sense has existed for a very, very long time, because since we exist as cultural organisms there have always been some of us who wanted to understand reality, and we can

call them scientists; and some of them were very, very smart and had fantastic ideas, and Galileo was one of them. But it was only after Galileo, though also in the seventeenth century—not in Italy, but in England—that another kind of scientific revolution occurred, and that was the revolution that consisted in the social institutionalization of private science, and the conversion of it into a collective, patterned activity. This institutionalization happened before the foundation of the Royal Society in 1662. The Royal Society, in fact, was the first flower of this institutionalization.

What is involved in institutionalization, speaking about this process as an abstraction? It is a natural process in the sense that it is not intended, it is never intended, and it is not intentional. People are necessarily involved, but they don't guide it, they just happen to be there.

So what happened in the institutionalization—in this case of science, but this happens in any institutionalization—is that what was once only private science, that is, the activity of various amateurs, various people who later combined into the Royal Society and first into the college that led to it—and who were considered rather crazy people (after all, why did they spend their time on doing whatever nobody was interested in?)—suddenly achieved social approbation, and the goal of these people, their desire to understand reality, was elevated to the position of a social value. Suddenly "society"—that is, people who were not at all interested in understanding reality—were, or was, looking at those crazy *virtuosi*, those crazy people, as people who were very respectable, who were doing something very important. And then, as a result of that, Prince Rupert and other members of the royal family contributed their prestige to the organization of the Royal Society.

But what institutionalization in itself, even before the Royal Society, did, was transform the activity of those private, few individuals—because of the added prestige of this private activity—into the foundation of a continuous, patterned social activity. And how is it patterned? It is patterned on the basis of the goal of understanding empirical reality, which is now elevated to the position of a social value. This social value is translated—again in a natural process, not as an intended process—is translated into a normative structure, into all those norms that you [Professor Holton] mentioned, those motivating emotions. And now lots of people are attracted to this activity simply because it is prestigious, since society appreciates it. And when they are attracted to it, they are behaving according to those norms, because this is what the activity means. As such they fulfill the social role of a scientist.

So there is a combination of the goal of the activity and its elevation to the position of social value that becomes translated into a normative structure, which then is implemented in a series of roles that constitute a social institution, not even necessarily organized in any specific "institutions." But what this combination ensures—in particular given the specific goal of science—is that, as in the economy, there is self-sustained growth.

So, while before, we could wait hundreds of years between one genius and another among those people interested in understanding empirical reality, and from time to time we would have a Galileo, from sometime in the middle of the seventeenth century in England forward, we have had a sustained growth of physical science. That is, we don't any longer rely only on geniuses. There are lots of people who are recruited into this social institution and who are merely socialized into it; that is, they acquire those motivating emotions, and they do their work—their incremental work. From time to time another genius arrives and takes us far beyond the point reached at a certain moment, but there is always continuous, incremental activity.

That was the real scientific revolution; instead of sporadic breakthroughs, it gave us this extraordinary sustained growth. This is what characterizes the institution of science. And since we know—this is to Professor George Prevelakis—since we know for a fact, for we reached this consensus in the beginning of our first session yesterday, that the social sciences, from the moment of their institutionalization as such—that is as social sciences in the universities—have not advanced an iota, that is, they still are exactly where they were in 1900, we can conclude—and this is to Professor Wood—that their scientific status is absolutely clear. They are *not* sciences. They may be something else, and they may be making fantastic contributions as something else, but they are not sciences. That is, they cannot satisfy our desire to understand reality.

They may satisfy numerous desires, such as to beautifully fill our prisons—as apparently they did, we now know—and to do other things; I don't remember exactly what other contributions they have made—it depends on your taste, on what you like, and, you know, some people may be very, very satisfied. But that particular desire—the desire for deeper understanding of empirical reality—they cannot fulfill.

We still rely, for deeper insight into social reality, on occasional geniuses. We have them. Some of our guests, in fact, are those geniuses. Those who were present yesterday certainly are among them. We know

this from their extraordinary achievements. But we don't have sustained, institutional growth.

FRIEDMAN: Well, all right, I am going to try to add a coda at the end. I think I understand why Liah has asked us . . . I'm beginning to understand why Liah has brought us together here. What I was going to say was prior to that flash of alleged understanding, but I'll say it anyway.

As I was listening to Professor Holton, at the end of his list of desiderata, he said, "of course, constancy and invariance," and I reacted with horror against that, naturally, because maybe—I'm not sure of this—but maybe by definition what social scientists study—not that Professor Holton was saying that social scientists should imitate natural scientists—but what social scientists study are the aspects of human behavior that aren't constant and invariant. We don't study the human heartbeat or human breathing, which are human actions, after all; we study the things that, at least in principle, if not always in practice, are subject to human volition, which is, by definition I guess, inconstant and variant.

That got me to thinking that maybe we are—not that there isn't a lot wrong with social science—but maybe we're holding it to the wrong standard when we say that no progress has been made and that there is a lack of accumulation of knowledge.

Just imagine this counterfactual. Imagine if there were no universal physical laws and that what physicists studied were inconstancies and variances, because at least nobody had yet discovered any constancies and invariances. And so physicists would only study particular historical events, as Weber urges social scientists to do in "Objectivity in the Social Sciences"—the physical equivalents of the French Revolution—so physicists would study the San Francisco earthquake, or the implosion of the World Trade Center, and that's the only type of thing that they would study because there'd nothing else to study, because there were no universals, just particulars.

If that were the case in physics, then physicists would probably be holding conferences on the lack of progress in the physical sciences—at least if, in this counterfactual world, there were social-scientific laws, and therefore there was a social science that was able to make progress identifying those laws, against which the physicists could compare themselves unfavorably.

What would also happen in that world, in the natural sciences, is that there would be a lot of wasted effort based on the fact that particular physicists would be interested in things that became uninteresting to

other physicists later on, or that just were ideological peculiarities of those individuals at the time, or that were interesting only if one accepted assumptions that were simply incorrect—but that the evidence was too ambiguous to weed out as incorrect. Maybe once we've designed better buildings, then the San Francisco earthquake would no longer interest us as physicists, so a lot of effort would have been expended on something that then became unimportant. And meanwhile a lot of trivial research would be going on just because someone happened to take an ideologically motivated interest in, I don't know, "the prison" (or the masonry in a particular prison—I forgot, I'm talking about physical science). Most of their research would be trivial, and from the perspective of other theoretical agendas, many of their assumptions would be worse than trivial. And yet, for all the wasted effort and lack of progress, there would be an accumulation of knowledge about the particular things that these physicists studied, even when the reason that they studied them was theoretically incorrect—just as I think there is, I don't want to go too far, but I do I think, as I've sat here and considered it more carefully over these two days, that there has been progress of that sort in the social sciences. We know much more about the French Revolution now than we did 100 years ago, and while a lot of dead ends have been pursued, and a lot of bad stuff has been written about the French Revolution in the last century, still, there has been an accumulation of knowledge about it that can be, and to some extent has been—look at David Bell's paper [for Session III]—reconstituted into something with (I think) a sound theoretical basis. And look at how much more we know about nationalism. . . !

Certainly the type of knowledge accumulation we've seen in the actual social sciences lacks the momentum experienced in the actual natural sciences, so maybe social-scientific "progress" isn't entitled to that label. But is this because of the institutionalization of natural science, allowing non-genius physicists to add to the accumulation of knowledge by doing Kuhn's "normal science"? Or is it because of the fact that there are . . . or there seem to be . . . universal natural laws, and experimental evidence that can, with a relative lack of ambiguity, falsify conjectures about those laws—so that for the most part, everyone in the natural sciences is on the same page at any given time?

I wonder, in fact, whether there might not have been as much wasted effort in the natural sciences as in the social sciences. Think of the natural science of a hundred years ago. Although it's historically foundational to the natural science of today—still, think of all the wasted ef-

fort that went on between then and now in pursuing research that ended up being falsified.

I don't know about that last point; but what I'm saying more generally is that if you're willing to look for the gold in the garbage-heap of social-science publications, there's plenty of it, and there's more and more of it as time passes. When I think about political science, for example, a lot of work was done in a burst of interest in the mid-twentieth century on public opinion by Philip Converse and his followers in the Michigan school, and among about 50 to 100 public-opinion specialists at any given time afterwards. This work is continuous to this day, and an immense amount has been learned from it that has revolutionary implications, at least normatively speaking. It's not well known because it's not interesting to most political scientists, so there's no institutionalization of political science that would generalize these findings—but that's because they don't relate to any universal laws, so the interest in these findings is not a universal one; it's a peculiarity of 50-100 people at any given time. And yet much more is known about public opinion now than before Philip Converse published the initial paper in 1964.

On the other hand there is also a lot of wasted effort when social scientists try to discover or insist a priori on the existence of universal laws that actually aren't there. I think from my contact with them that they are actually—ironically—motivated by distaste for the wasted effort and the lack of progress—in the natural-science sense—that they see all around them in the social sciences! And yet the direction that they go in is even worse than trivia and wasted effort. It would be interesting work if it were true, but it's untrue. Whereas most of what goes on in the social sciences, against which these universalists are reacting, is true but trivial.

Now here's the coda based on what Liah just said: maybe there could be an institution, in the sense of a particular institution that was just a very small corner of the larger world of social science, in which whatever it is that accounts for the triviality on the one hand, and the falseness on the other hand, of so much of what goes on in social science would be avoided. I take it that that is what Liah has in mind. But it seems to me—I'm guessing, although I haven't heard Liah's silver bullet yet, the . . . jack-in-the-box that we're all waiting for—I'm guessing that it would be a matter of the particular genius of the founder of the institution that would make its contribution a positive one, rather than anything structural that could be patterned after natural science, and

that could be a pattern for other institutions in the social sciences to follow.

In other words, I'm guessing that we'll never get away from the necessity for acts of individual genius that can't be institutionalized in the broader sense in the social sciences, simply because, unless somebody comes along and discovers a universal law of the social sciences, or a way to experimentally choose among the many theoretical models—*themata*, or Weberian "ideal types"—that can explain parts of the profusion of social-science data, we're going to be stuck with a lot of people doing a lot of busy work and a few geniuses making valuable contributions along the way.

GREENFELD: Just wait until 3 o'clock.

FRIEDMAN: Okay.

HOLTON: I think I had a nudge from behind me when you [Professor Friedman] said that you reacted in horror on the *themata* of constancy and invariance. I was only of course listing those *themata* that were very important in Einstein's own work. He was once asked by one of his correspondents, as I found in the archives, Why did you allow people to call it "relativity theory?"—because the whole point of that theory is not relativity, but constancy. That is to say, regardless of the motion of the observer, the laws of science stay constant. Yes, said Einstein in reply, I wish they had called it "invariance theory," because that's the point of relativity theory: the laws are invariant despite the fact that the phenomena look different for differently moving observers. So this just, I hope I didn't mislead you, but others of course have another set or fingerprint of *themata*: Max Abraham, for example, who profoundly loved the *thema* of the ether—that is, the fluid, all-space pervading essence—which was dismissed in a half sentence on the second page of Einstein's paper as being superfluous, throwing on the dustheap of history a hundred years worth of work and offending an awful lot of people who continued to work on it still.

So constancy or invariance is not a universal theme at all. Others did not accept it. As a matter of fact, when you look at the rise of science you realize that it sprang from the fascination with the unique, the wondrous, the kind of things that the archbishop might put into his so-called *Wunderkammer*: that is to say, things like the calf with two heads. This was the important thing to be worked on during the medieval period. It took some time to see beyond the unique to look for that which is common to many, and it is out of that that

modern science came. And so, if you read Kepler, for example, he still has in these many volumes a great variety of medieval ideas that don't work at all in modern science, and for his three great laws that we now teach our students, you have to find little places within the bulk of his work, which otherwise is full of astrology and talk about witchcraft and things of that sort, which is the pre-scientific area. I don't demean that because, while you said that we are wasting an awful lot of time, there is such a thing as entropy, and I regard it as necessary, almost, for great things to come out of a welter of things that do not qualify as great.

It is in the nature of the human mind, perhaps, that we spend a great deal of time unfruitfully in order for something fruitful to come out, and I think that is true even for the institution of science, and so let me now finish these remarks by talking just for a moment about institutions.

The test of a good scientific institution is whether it helps the scientists involved to get a hearing in the context of justification. Kepler was alone. There was no institution around him other than the "Mad Emperor," Rudolph II, who subsidized him. And after Brahe's death, he was essentially alone. Galileo didn't even send him a telescope. . . . The one person in the world who should have had a telescope wasn't given one by Galileo, who hoarded them for those who would do favors for him, and would help persuade others of his ideas. There was no institution around these people. They could not get verification. Kepler's laws were so unregarded that even Newton, while using them, still calls them, largely, "the Copernican hypothesis" instead of using the word *Kepler*. It is not until later that there are enough of your peers to whom you can present your work and who, in an institutional setting, can help you understand whether you are right or wrong; this is the basic purpose of institutions.

We now have over 2 million scientists in the U.S., we have 150,000 physicists in the U.S. They couldn't function without the *Physical Review*, and its many forms, which help them decide whether what they are submitting is correct, by peer review. That's the kind of verification in the context of justification that is needed by these institutions. Anything else is careerism, unfortunately, and we have to live with that as part of the entropy that institutions cause.

MAZLISH: I'm embarassed to say that I will try and be very brief. I will be because we've run out of time, but there's no way that this topic can be dealt with in two minutes.

I want to go back to Professor Holton's earlier remarks because I think it's there that we can move from the natural sciences to the social sciences. I think that the Galilean revolution is the basis of the effort to work, with regard to human phenomena, in a scientific fashion. We have to take that for granted, and on the subject of hierarchy. . . . In a way there is a hierarchy, in that every social scientist has to accept the fact that there is gravity. That goes without saying, and it is what Comte, I believe, had in mind. Comte, as you know, who was himself a mathematician, did not place mathematics in that hierarchy. It was a tool, and when he came to biology and then the new field of sociology, he declared that mathematics could not work with them properly, and you had to resort to comparative methods, so—with that difference, we need to get to the point of what is at the heart of the effort at social science, which is on a different level from the natural sciences.

If I can reverse the spatial image for a moment, you [Professor Steinmetz] spoke of the brain. Yes, of course, the brain. Without the brain there's not going to be mind. But you know the distinction between the two. You said you had never heard the word "modernity" in a conference, but that's what the social sciences are about, that's part of culture, it's part of the cultural evolution that has taken place, so we're moving into another level and the question is, how can we identify the reality with which we are trying to deal there? And there are many, many ways that we can.

There'll be much more time today and there are other speakers. I just want to make one last point: *consilience*, you introduced that word, E. O. Wilson has used it for his title. Darwin worked in terms of consilience. He didn't use the term, although he'd read Whewell, which is where it comes from. Whewell had specifically said that, with one or two exceptions, you could only have consilience within a given discipline. It would not go across disciplines, necessarily. So when E. O. Wilson takes this as the title for his book, he is simply misusing it. For Darwin, consilience means that a great many phenomena, such as embryology, histology, etc., etc., could be persuasively understood by his theory. For Wilson, everything, including the social sciences, could be understood by sociobiology. I disagree with him. You can not look at the atom and show evolution!

As we move to the social sciences we are not going to get the kind of rigor, verification, experimentation as in natural science. These are simply not possible with the phenomena we're trying to deal with. That

doesn't mean that we can't come to a reality, but I suspect that that reality will have to be approached in terms of consilience, which is a form of scientific method.

PRESS: I'm a student, and I am going to present my comments from that point of view, in that my comments are a reaction to classes that I have taken as an undergraduate, which I was forced to take as prerequisites to graduation, in biology and physics; and also to a discrepancy between these classes and comments made in classes on the nature of science, which is the discussion we're having, and comments made specifically by Professor Holton and Professor Steinmetz.

So I am going to present my reaction as a conjecture, which we don't have time, perhaps, to try to refute right now, but that is the vein in which I am presenting these comments, and I would like to start by maybe more than just appeasing Karl Popper; I'd like to take him by the hand and introduce him into our circle and say that science is a process of conjectures and refutations. That is, someone, a scientist, to use specifically the language that Albert Einstein loved so well, a scientist imagines a process, a causal connection, a situation, and then uses empirical data to prove that the figment of his imagination has no bearing on empirical reality. This, I conjecture, is the basis of scientific work, the process that defines the institution, and that allows, as Professor Greenfeld said, this institution to be a progressive one.

A scientist—and here I am responding to remarks made by Professor Steinmetz—a scientist, one who acts within this institution, may do a lot more than simply conjecture and refute, or may never conjecture at all, but may spend most or all of his time collecting empirical data. This collection of data, however, only contributes to the progress of science when it is applied to a conjecture or conjectures. That process refuses to allow one to prove anything, basically. All one can do is disprove; or, perhaps more positively, prove a conjecture false. Therefore, the progress of science is the accumulation of conjectures that are known for a fact to not be truthful empirically.

Those conjectures that are most useful when applied are simply that—useful, not truthful, such as Newtonian mechanics. There is some debate about the truth of this, since engineers use Newtonian mechanics to build bridges, etc. Prediction—and I say this in opposition to something that is harped on in every introductory biology and physics class that I have ever been in—prediction is not, I would say, a necessary aspect of science, and does not help prove a conjecture true. If one pre-

dicts that some phenomenon will occur, one must test this prediction. Therefore, prediction is conjecture. It is not the purpose of science to predict.

All these ideas express what I would think to be a definition, or a grounding for a definition, of the word *science*. When one asks, "What is science?" one could use these ideas to explain it. But these ideas do not help specifically define physics, biology, or social science. So the question is, what separates physics from biology from social science, assuming there can be such a thing as a social science? I suggest that it is a matter of the quality of empirical data used to test conjectures in the respective fields and also, therefore, the methods used to collect and interpret those data. I would also like to suggest, though this might be a bit more bold, that this conjecture I have just made may be what a scientific conjecture in the social sciences would resemble.

CASANOVA: Yes, I will be very brief. A. C. Crombie, in *Medieval and Early Modern Science*, shows very definitely that the notion of impetus was discovered in the fourteenth or fifteenth century by Jean Buridan, and that this discovery was essential to Galileo. Heliocentrism was proved as a mental experiment by Nicolas de Oresme, and that was in the fourteenth century, I think. Algebra was introduced to physics in the fourteenth century in the European universities. Therefore, I think that science was already institutionalized in the Middle Ages.

The revolution, I think, in the sixteenth and seventeenth centuries was that Scholasticism was exhausted, and some people, like Galileo and Copernicus—who, by the way, expressly quoted Aristarchus—these geniuses began to do science outside the universities. But even then they were in some way institutionalized because, for example, Galileo and Descartes wrote many letters to their collaborators in Europe. There was a kind of institution being born, and then the great revolution took place, which was that the institutions were no longer exclusively the universities for many years, but instead were complex networks of reseach in royal societies and academies, as well as independent scholars—in England science was institutionalized in another way, wouldn't you agree?

STONE: Yesterday Professor Wood mentioned the Sokal hoax and the limits of social constructivism. With three pictures of Einstein bearing down on me I am reminded of the lecture that Einstein gave at the Sorbonne in the early part of the century in which he said, I

believe—and Professor Holton perhaps can correct me on the details—"If the theory of relativity proves to be correct, the Germans will call me a German, and the French will call me a citizen of the world. However, if the theory of relativity proves to be incorrect, the French will call me a German, and the Germans will call me a Jew." Perhaps we should be careful before we throw out social constructionism totally.

SESSION VI. ARE THE SOCIAL SCIENCES IN NEED OF REFORM?

INTRODUCTORY REMARKS: Irving Louis Horowitz
MODERATOR: Natan Press
PARTICIPANTS: Ali Banuazizi
Carlos Casanova
Camille Czerkowicz
Jonathan Eastwood
Hernando Forero
Jeffrey Friedman
Irving Louis Horowitz
Charles Lindholm
Dusko Sekulic
Jonathan Underwood
Carl Woog

HOROWITZ: The purpose of my remarks is neither to describe nor to predict the demise, or the destruction, of social science. Quite the contrary, I am confident that social science will continue into the twenty-first century its major project of the twentieth century: it will remain a beacon for the honest evidentiary analysis of public policies and of social life.

Indeed, for over 40 years now, I have been involved, through Transaction, with publications that represent the best of social science. *Society* introduced Oscar Lewis's culture-of-poverty hypothesis. It gave expression to the innovative view of executive power as a unique domain in foreign policy, called the "two presidencies" thesis, of Aaron Wildavsky. We provided a forum for Daniel Patrick Moynihan's work on the crisis of black families, with which many of you are familiar. We introduced Morris Janowitz's work on the all-volunteer army, its limits, and its po-

tential in peace and war, after Vietnam. With studies of game playing and scenario design, we explored how the social sciences were involved with ideology building even more than with the empirical study of Third-World countries in battle with First-World values. We also offered a bracing halt to optimism about the social sciences, in the form of Peter Berger's painful "Disinvitation to Sociology," which offered his explanation of why, in effect, he felt he had to leave the field.

For over 40 years, Transaction has been producing good social science. It would be self-defeating to be mindlessly critical of that which we have helped build, and of which I am especially proud; even more so now that Jonathan Imber is in charge of our flagship, *Society.* Reform of social science is a constant. But social research must be built on the achievements of scholars past and present.

Without further preface, let me offer an admittedly simplified view from the trenches: a consideration of the conflict of subjectivity and ideology against objectivity and science, as it presently is being fought in the social sciences.

There is so vast a literature on the state of the social sciences that, rather than encompass all fields and all purposes, I will devote this limited time to less global but, arguably, more utilitarian matters. By this I mean that, rather than giving the usual bird's-eye view of the social sciences that begins with famous men, moves on to ideological disputations, and ends in metaphysical suppositions, I will outline a foot-soldier's view, a view from the publishing as well as the academic theater of battle.

Let us start with the behavior of social scientists. From approximately 1890 to 1920, five disciplines were established that continue to define graduate study in the social sciences: anthropology, sociology, political science, psychology, and economics. These fields all included high levels of generalization; theory construction; broadly based observation and ethnography; and, above all, a sense of historical and philosophical antecedents. Indeed, the social sciences took both history and philosophy into exciting new areas, using relatively modest tools of research to settle old scores. All of these areas are still represented by professional organizations that many mistakenly believe define contemporary social science.

This is a mistake because, from roughly 1920 to 1960, largely as a result of external pushes—such as advanced forms of warfare, totalitarian systems, mass state-sponsored murder, guerrilla insurgencies, economic upheaval and instability, and the rise of developing areas to political in-

dependence—the social sciences turned away from being strictly concerned with history and philosophy, or with antecedent conditions of change, toward a series of concerns about the future, often called "policy research." There was an ad-hoc shift from departments to programs, from schools to research agencies, and from large-theory construction to targeted positive outcomes. Sophisticated theory was augmented, and in some instances replaced, by solid methodology.

From 1960 to 2000, yet another great transformation took place: a series of professional changes that unalterably reduced the landscape of social research in the advanced nations, especially the United States and Europe. What began as a set of practical extensions of older social sciences—specialized projects and programs in policy research—blossomed into full-scale new disciplines. We had the emergence of communication studies, demography, urban studies, criminology, and penology, to cite the most obvious examples. Under the impact of computer technology, logic and method united to produce game theory, crisis intervention, risk analysis, chaos studies, scenario construction, evaluation research—all of which emerged as new ways to examine old issues.

The extent of the success or failure of each of these fields and methods of social research must clearly be left out of this brief summary. I will say that to the extent that such new techniques of analysis cast a bright light on the political process, they have been useful and innovative. To the degree that they reflect the needs and interests of those who simply purchased research for policy ends that extend far beyond empirical discourse, such new thinking has only further heightened the crisis of social science.

In macroscopic terms, these newer branches of social science have expanded their outreach and increased their numbers, and in part they have done so at the expense of older social-science disciplines. But beyond this base, these newer disciplines have reached entirely new constituencies. Take the American Sociological Association. What were once singular panels at its annual meetings on, for example, Criminology, Urban Affairs, Demography, Public Opinion, and Mass Movements have broken away and formed academic departments and professional organizations in their own right. A professional literature prepared by a younger cohort of scholars has emerged that does not so much engage in combat with older disciplines, as disregard them, leaving them to drown in their own theory, perhaps the unkindest cut of all.

These breakaway segments were often established without overt de-

sign or aggressive purpose, even as older disciplines heaped calumny and contempt on such efforts to organize new varieties of knowledge research. As a result, everything from social-welfare agencies to penal institutions found their concerns better expressed by the newer, more pragmatic organizations than by the older and more theoretical ones. Criminology, once a chapter in sociology texts, has now become a field that dwarfs in size and reach the fields that originally nourished it. Instead of a symbiotic relationship between older and newer forms of learning, what emerged was envy and animosity toward the upstarts on the part of the older disciplines. This was to little avail, since society usually gets what it wants from its intellectual classes. Today there are seven important criminology associations in the United States alone, with a combined membership of more than 100,000 people. This is more than eight times the size of the American Sociological Association.

At the purely organizational level, the new situation has created a Balkanization of data and information both within professional life and between professions. As a result, the need for generalists remains: the need for scholars who are concerned not so much with empirical research and observation, as with scanning the current plethora of research professions for mechanisms that permit integration and wide-ranging theorizing.

Even in the present climate, what has become apparent is that researchers within one profession can readily discourse with those of allied professions, sometimes professions that are remote in character and depth from each other. This has been made possible not by grand theoretical schemes, not by paradigms of a new science of man, but by methodological commonalities that are modest in design and instrumental in outlook. There is procedural integration in the use and management of tools, rather than theoretical integration in the higher regions of abstract analysis. It might well be that out of such new data compilations and formal designs, a new set of paradigms will emerge, and in turn, a new integration of the social sciences as a human science will emerge. But this is speculation. For now, the struggle is the message.

The struggle originates in a situation that permits researchers to engage in wide-ranging innovations without paying dues to any single social-science paradigm or theorem. In an imperfect world, this explosion in forms of doing social science offers great promise for larger breakthroughs later. In the immediate environment, it prevents dogmatic,

doctrinaire views from constricting the pursuit of knowledge. On balance, the new conditions of research and theorizing in social science are an immense step forward from earlier varieties in organizing the human experience.

The sounds of ideological clashing that one hears are more echoes of the past than engagement with social research as it is actually practiced in America and Europe today. In brief, neither the political process as such nor reactionary forces within each discipline "did in" older vistas. The old has given way to the new through the simple evolution and expansion of the social-science vision to reflect changing tendencies in advanced social systems and current trends in societal aims and ambitions. But some segments of the older disciplines have dug in their heels and see such trends as the bourgeois enemy entrenched in government offices. Some older disciplines have incorporated an anti-populist mood, an animosity for popular culture, a rejection of the values of material wealth and suburban life-styles, and engage in a continued assault on evolutionary processes to achieve social equity, preferring the apocalyptic and revolutionary. The collapse of communism only accentuated the isolation of older disciplines from market economics and social-welfare trends in the United States and elsewhere. Historic defeats in the world at large were denied, and segments of some older disciplines engage in subjective and fanatic commitments to the ends of the failed system.

Those older disciplines that have responded to changes in the structure of American society over the last century, such as economics and political science, have managed to hang onto and even increase their constituencies. They have also prevailed in the policy debates at all levels of government. Others, like psychology, have had internal fissures between the clinical and the experimental, but this sharpened their outreach (while admittedly also increasing their internal strains). At the other end, those disciplines that became manifestly ideological have found themselves at the margins of politics as well as science. Hobbes comments repeatedly in *Leviathan* that the end of regimes and rulers is a function of internal disintegration far more frequently than of external intervention. So it is with several of these older disciplines.

Professional careers in social science have cleaved, and with it, fissures have emerged that are as irreparable as the inherited conflicts between theology and physics. Can fusion follow fissure? Yes, of course. But such a new integration cannot simply be stitched together from old animosities and fundamental distinctions about the nature of social research as

such. In other words, the paradigmatic nature of the present situation remains to be assembled, not as a mechanical task by a brilliant personality, but as a common chore of many hard-working professional groups acting in concert for common ends defined as rational and scientific.

Even those who grudgingly acknowledge the accuracy of this view seem reluctant to accept its organizational consequences. Critics of contemporary social science speak of positivism, pragmatism, and empiricism as serious weaknesses. Such critics are quick to take refuge in the dark world of subjectivism, ideological irrationalism, and sheer prejudice. The net result is a morass of writings in which the notion of science is discredited by recourse to presumptively biographical weaknesses and blatantly ideological accounts, and in which events are seen not so much as having occurred as having been constructed. All efforts to use common methodological safeguards, and evidentiary appeals to a common body of data, are regarded as themselves indications of reactionary thought.

As a result, we end up with tendentious posturing that may seem chic and clever, but is in fact facile and cynical. Typical of this is a book by David Greenberg on *The Construction of Homosexuality* that concludes with the revelations that social research makes no assumptions that society has moral boundaries such as those that the prohibition of homosexual activity maintains, and that researchers would do well to avoid these gratuitous and frequently misleading assumptions. Another essay, by Ellen Berg, writing in *ASA Footnotes,* tells us that feminist theory is essentially a mechanism for moving sociology from the "Malestream" (into what, one can only imagine with trepidation).

The extraordinarily diverse world of television that now exists is reduced to the construction of reality. The old hatred for the new information technology takes on ideological colorations. Ira Glasser, executive director of the ACLU, tells us that television suddenly homogenized everything. Edward Herman and Noam Chomsky make the same point in their book *Manufacturing Consent,* in which we are informed that an underlying elite consensus largely structures all facets of the news, and also just how propagandistic our mass media are. Mark Fishman extends this premise in *Manufacturing the News.* Here we find out that bureaucracies prepackage the news, and that if only news were gathered in politically conscious ways, a different reality would emerge, one that might challenge the legitimacy of prevailing political structures. The actual diversity and multiplicity of news product is simply dismissed as a chimera, a collective cover for sameness. Needless to say,

this is not what ordinary Americans or Europeans see, nor is it what is actually displayed.

Yet such claims are hardly unique, or for that matter, even the most extreme. No part of American history or society has been left untouched by blatant appeals to a partisan theory of reality. If reality is subjective, then constraints upon imaginary scenarios are readily lifted. Stanley Karnow, usually a sober analyst of international strife, states in *In Our Image* that rule over the Philippines made America the great Pacific power that plunged it into World War II. This colonial relationship was thus the cause of the war—precisely the argument that Japanese militarists used to justify the attack on Pearl Harbor. Even poor George Washington has not escaped this Alice-in-Wonderland reversal of cause and effect. Thus Washington, in the past depicted as a reluctant leader who preferred private life to public affairs, is said to be in fact an eager, active, and astute politician. According to Paul Longmore in *The Invention of George Washington,* "from the very beginning of the Revolution . . . his fellow Americans substituted him for King George III."

One can multiply a hundredfold such partisan appeals to uproot traditional views of the human sciences. The amount of subjectivism enlisted in the service of political movements overtly aimed at destabilizing American society is virtually endless or, better, limited only by the imagination of the subjectivists. Perhaps the sum and substance of such sociology is best summed up by Michael R. Hill, who at least has the decency to state the honest intent of this barrage of subjectivism in the service of utopia. It is time to turn the tide, he says: ideology first, axiology second, epistemology third. The counter-revolution of postmodernism within at least some of the social sciences has become a code word for unabashed anti-Americanism as an ideology, and subjectivity as a critical literary expression.

Having said this, it is important to emphasize that flamboyant writings at the intellectual margins by no means exhaust the current state of affairs in social science. Such voices scarcely come to grips with the vital process of research and discovery. At the core of many disciplines, scholars still create fine analysis and fine writing accessible to larger publics, and levels of theoretical innovation that equal those of past generations. In some cases, social scientists today have exceeded earlier efforts from the pantheon of greats that we have come to admire. Many contemporary works extend the boundaries of social science by helping us appreciate relationships of factors that drive our society. In so doing, they also provide useful explanations and helpful predictions. Let

me illustrate this point by a brief mention of several such works that have appeared in the recent past.

We can start with Amartya Sen's *Rationality and Freedom*, which provides a brilliant analysis of the contrasting demands for economic efficiency and social equity, and of mechanisms available to bridge the gap. On a similar level is the new book by Peter F. Drucker, *A Functioning Society,* on competing values that drive business and commercial practice, or the gulf between public service and private avarice. Then there is Robert D. Putnam's well-known *Bowling Alone: The Collapse and Revival of American Community,* perhaps the most impressive effort in social psychology since David Riesman and Nathan Glazer's work on *The Lonely Crowd*. Whether one agrees with Putnam or not, his is essentially a starting point for the study of new varieties of privatization and alienation in advanced systems. Speaking of advanced systems, we have R. J. Rummel's work, *Death by Government*, which helps explain the organic and inverted relationship of democratic systems of government and low thresholds of violence and state controls. And the obverse of this is to be found in the brilliant work by Stephane Courtois and his associates, *The Black Book of Communism*, a work that within the limits of a database settles for all time the unitary and hideous nature of totalitarian regimes.

I do not intend this brief statement to be either a book review of decent books of this season, or a summary of available literatures. Fine books, essays, and reviews are legion. I wish only to share my sense that what we face in the first decade of the twenty-first century is an out-and-out struggle between a culture of social research and a counterculture of subjectivist ideologies with a deep animus for science and its findings. These subjectivist ideologies do not see themselves as constrained by rules of evidence or guidelines of experience. They claim to be rooted in higher principles of economic equity and social justice. They are the children of Schopenhauer, Nietzsche, and Bergson. For them, the force of Will overcomes the worth of evidence. Their commitment is to history as Will. For such scholars, social welfare is a utopian afterthought to the benevolent State. Such subjective metahistory drives metaphysical fanaticism.

Radicalism in periods of failed prophecy often turns rancid, subjective, and irrational. It did so in the 1890s and it did so in the 1990s. The failure of utopian dreams and ideological visions to materialize does not easily deter the true believers. Such people simply internalize remote goals, making them part of personality development rather than

social structure. At such moments, true believers can change their position, but more likely such people look at the world as a place that needs further modification in order to maintain the status of their failed beliefs about it. The gulf between normality and neurosis becomes transparent. But there is no assurance that "reality" triumphs. The search for new modes of theorizing (or rationalization) can be compelling, especially when the alternative is the surrender of enshrined beliefs and persuasions.

Specific forms of struggle are adapted to the times we live in, but larger considerations remain remarkably stable over time. What needs to be better appreciated is that high-quality social science is being widely and steadily produced and is available. But what also needs to be understood is that the victory does not go assuredly to the advocates of rationality without a struggle. The same ferocity, the same determination, the same courage that inspires modern prophets of the subjective Will must inspire those who uphold the principles of objective fact. In human affairs, victory does not go to better science by default. Irrationalists do not yield to truth as readily as to power; and alas, sometimes to neither. Those who labor under democratic guidelines need science, but they cannot ignore the potency of force.

The history of twentieth-century barbarisms that emerged in the belly of European civilization should persuade us that scientific knowledge is no guarantor of rational behavior. The role of passion in the forward movement of social science having been acknowledged, the edge, the advantage, does reside with the rational, as long as that edge is exercised on the field of intellectual battle within a democratic framework—what I have elected to call the trenches of social science. Karl Popper's *Open Society* is not an option, but a necessity, for social science to continue its service to society. What is required is less reform than preservation of the democratic order. There are dangers in prophetic modes, but also in narrow policy mandates that reduce social science to methodology, and human beings to experiments.

FRIEDMAN: Professor Horowitz . . . what happened? Did a bunch of bad people take over the traditional social sciences?

HOROWITZ: First of all, I would never use words like "bad people." The struggle is not between good people and evil people. Indeed, I have no evidence to indicate that those who share my views are more pleasant and decent to be with than those who espouse subjectivist beliefs or a commitment to a good society that differs from my own. One must presume that all social scientists aim for the good; they all want

the best. Every lunatic is a good person at the level of ambition and goals. I would not describe Hitler or Stalin as a bad person; it weakens the sense of resolve in opposing tyranny to speak in such broad terms. We are not talking about good people, bad people. We are talking about people whose prophecies have failed despite their efforts to change the world—sometimes by Draconian methods. Their totalitarian successors, cloaked in the garb of social science, are often trying to figure out a way to win by rear-guard intellectual action what was lost on the battlefields.

FRIEDMAN: But do they see that the prophecies have failed? Do they recognize that?

HOROWITZ: It is virtually impossible to answer whether such individuals see failed prophecy. They behave differently now, with a fatalistic sense of pessimism rather than optimism about the future of socialism or the worth of dialectical materialism. The pro-Soviets among them know that they lost the battle of historicism, lost the battle of a world that was going to be made more perfect by a communist state and a new world order.

I presume, like most people, that they want to win in the long run, whatever winning may mean in such circumstances. You have one choice or another. The social scientist as prophet can say, "Hey, I screwed up; I made a mistake; the world does not operate on principles of dialectical evolution or revolutionary evolution." Or one can say, "Despite all appearances, I am still right. We face a temporary setback. The times are different; mistakes were made in the rush to the future. We will get it right next time, perhaps in China, Vietnam, or Cuba." The problem is that such visions are based on political ideology, not social research. An irreducible cul-de-sac faces those operating with such premises within the sciences. That is what my remarks are intended to address.

All prophecy, religious or political, has a possibility of success. People are always predicting the end of the world, the beginning of the new millennium. They are always presuming, and then when events fail to confirm expectations, they have a choice. They can say, "The world is wrong, and it is the time frame that is off base"; or "I made a mistake, but one easily corrected by new predictions." The latter course of action, as Charles Sanders Peirce pointed out in the formative period of pragmatism, is very hard to predict with any exactitude, and hence, science is a practice for the few who can look at reality straight and no

chaser, however tragic that may be for the consequences of human nature.

The issue here is which of those types of thinking can be squared with the requirements and constraints of social science. What we are experiencing in the social sciences is to be expected. Most scholars go about the business of doing good work; others do not. Those disciplines that prefer prophecy to prediction pay the price. The price is to be ignored. Happily, very few people are going to use a gun in America to shoot opponents because they are practicing ideology. Even more joyful, even fewer people will pay any attention to those who preach doomsday scenarios. Still, the lesson of Jonestown remains as a warning to those who do have blind faith in fanatics and those convinced of their special divine powers. That is the delicate condition in American professional life.

Let me use as a crude example—since you brought up bad people and good people—the one presidential candidate who was running in the year 2000 whom the American Sociological Association saw fit to invite to its annual meeting was Ralph Nader. The fact that the campaign was being fought out in a totally different arena meant nothing to them. The members of this association wanted Ralph Nader. Likewise, the American Anthropological Association passes resolutions against intervention in Iraq, or in favor of Washington D.C. as a new state. That these are not compelling agenda items, certainly within the social sciences as such, means very little to ideologists of lost causes. It feels good.

It is wrong to think that ideologists are bad people. They are good people. But they disregard the codes of science: evidence, experience, and information as the basis for decision-making. The issue is the ground for establishing right and wrong, as opposed to pre-determining good and evil.

FRIEDMAN: I am questioning the assumption you seem to be making that they recognize that they're wrong, and nonetheless advocate these positions despite that.

HOROWITZ: Again, you misrepresent what I am saying. These individuals do not consciously pursue error, nor are they willingly trying to disturb equilibrium. They have made prophecies that are wrong, and they have to live in a world in which they decide either that the prophetic nature of their remarks didn't come to pass or that something is still wrong with the world and still right with their theories. You have to surrender either external reality or theory construction. Alas, some

social scientists much prefer to hold on to theory construction rather than the external environment. It is, after all, a more pleasant mythic place in which to reside.

FRIEDMAN: Or they could just see the world in a different way. They could now still be making mistakes in their theory construction, their interpretation of the world, their interpretation of what the fall of communism showed.

HOROWITZ: Sir, what do you want me to do, change reality by arguing the case for extreme relativism? Soviet communism collapsed. It failed in Europe. It failed worldwide. There are people who still want to retain or return to Marxism-Leninism. They are going to retain such beliefs no matter what happens or where it happens. If, tomorrow, Cuba exiles Fidel Castro, there will still be Castroites. That is the nature of human beings and their social movements. But should social scientists embrace the folklore of political dogmatism? That is why I speak of the rational and the irrational, rather than the good and the evil. It is held deep within older traditions of social science that the problem is not terror, violence or murder or genocide, but how one constructs the meaning of the world. But that is exactly the line of subjectivism that leads to apologetics and reaffirms the goals of tyranny.

Now, of course there are mistakes in evaluation. Could it be that world history will move in another direction in the future? By all means, that is quite possible. But the prophetic mode in which most ideology operates, and this is what Karl Mannheim brilliantly explained, is that such faith in predictive modes lacking evidentiary bases does not dissolve automatically with a change in the real world. Reality is a check, but not on everyone. On some people it becomes a license to go further deeper into the abyss. Dogmatic evaluations rest on a wider metaphysical canvas: never mind that the system collapsed. What did we do wrong that allowed it to collapse? How can we reconstruct the nightmare? Poor theory construction has a price, and it is rationality itself.

There will always be dogmatists amongst us; but we are talking about the character of social science. We are talking about what makes science possible, not why political movements continue under adverse conditions. One way to avoid being trapped into in that kind of line of querying is to ask, "What are the social sciences doing, and how do they express the world out there?" The alternative is to try to build new movements to replace old movements that have failed. Yes, there will be those new political movements, and yes, there will be people who react

that way to social realities they do not like. But my concern is the culture of social science, not the ethics of political extremists. We are not talking here about formations of new parties. We are talking about how social science adjusts its sights beyond the partisan demands of political life. How does one conduct research in economics? How are experiments performed in psychology? How does long-wave statistical method help us define the political process in political science? That is the point of my position. It is not an attempt to redefine the nature of the good citizen, but to help define what makes a good social scientist.

BANUAZIZI: Looking at a somewhat less political dimension of the social sciences compared to the thrust of your analysis, let's take the example of the thesis that with increasing modernization and so on and so forth, that there will be a decline of religion. Now, do you feel that social sciences erred in that? And would that error, would that failure be explicable in ideological terms? Do you think some major readjustment in our thinking about the role of religion in politics is needed, so this calls for a corrective from your point of view?

HOROWITZ: Yes I do. Absolutely.

BANUAZIZI: But this is not an ideological problem, right?

HOROWITZ: At some level social science must take account of ideology, just as it has to take account of religion. But that does not mean falling prey to a specific doctrine or credo.

Obviously your question is important and quite appropriate. Let me rephrase it slightly. For many years, social scientists did studies of the Middle East emphasizing only the developmental process, and they presumed that secularization would result there. Daniel Lerner's work in the late 1950s on modernization, for example, presumed a process of secularization and democratization. As an empirical matter, he was well intentioned, but arguably wrong. The Middle East did not move in a linear path, as anticipated by international relations experts. It would have been nice if it happened neatly and democratically, but it did not evolve that way. The burden of social science is to admit, "Daniel Lerner was wrong on that item. His prediction did not come to pass," and explain what happened.

What happened instead was a polarization: modernization and traditionalism were polarized in certain areas, and in certain nations traditionalism won because of factors such as the failure of proper distribution of wealth, the failure to incorporate the peasant classes or rural classes into the overall economy, the rise of new cultural forms of con-

trol that alienated ordinary people and made possible religious fundamentalism.

Social science is not dedicated to saying that modernization is going to dissolve religious traditions that have long existed. We are not engaged in a nineteenth-century battle about belief. The world of Andrew White and the war of science versus theology were essentially resolved by *noblesse oblige*, a separation of realms without declaring a victor. Social science does have to take account of elements of fanaticism, racism, and spiritualism in a proper fashion. We also have to examine what factors are involved in the retention of traditional faith in a universe of presumed modernization. What trends and tendencies were overlooked or not understood? In short, and to be blunt, why were serious analytical and predictive mistakes made?

Every science has to ask the question, What were the mistakes made, and why? It is not only the social sciences; the physical sciences as well must confront the source of error in judgment. In this particular instance we need to question the long-run tendencies and trends. If you are talking about Turkey, then we say, what possibilities are there for more secularization, or none, or a reversal? If you are talking about Iran, a great deal of modernization theory had to be revised and even discarded after the fall of the Shah in recent years. For example, the idea of the inevitable improvement of the lot of women under the pressure of modernization was dismantled by reality. Change did not follow the blueprints of development theory in Iran, among other Middle Eastern states. In Iran, after 1979, the *chador* went on. Things were different. Some women resisted; most did not. Most accepted the consequences of a theocratic state.

A social scientist may not like what happens, but a social scientist analyzes what happened. This is not a matter of liking and disliking outcomes of revolutionary movements. It is a matter of observing real time and space consequences. It is a matter of looking and observing, and looking honestly. That is hard to do without shedding ideological superimposition. But practicing good social science is virtually impossible if one wears the blinders of ideological commitments and utopian fantasies.

LINDHOLM: When you're talking about conflict in the social sciences, you talked about rationality and irrationality and the problem of excluding irrationality from the study of social sciences, and then you mentioned that this kind of conflict is part of human nature, so I wondered if you could elaborate on human nature. Is there a place in the

study of social science for the study of what human nature is? If we're going to look at irrationality, can we understand what the sources of irrationality are? Is there a science of the human being?

HOROWITZ: It is impossible to exclude irrationality either of the individual or collective sort from the study of social science. What we must exclude is irrationality as a methodological guideline for conducting our analysis. The entire field of psychiatry is predicated on a distinction between the rational and the irrational, the normative from the deviant. These are moveable measures to be sure; but we do not take the idea that the lunatics should run the mental institutions as an article of analytic faith.

Personal behavior often reflects larger social tendencies. But our analysis of these social tendencies must form the basis of our analysis of human nature. We do not read a text in Ancient Greek mathematics with the same sense of contemporary value as we read a text by Aristotle on the nature of the good. So we know that in the real world, certain phenomena, such as moral obligations, change more slowly than do the more rapidly evolving technological machinery for communication and transportation. Indeed, at some level, the science of ethics, if one can speak that way, is to measure the differences, the gaps between moral and technical evolution. Perhaps this is a way at getting into the subject of human nature in a dynamic rather than static manner.

I am inclined to the view that human nature, however defined, is a very slow-moving process. Its very durability, its conservative tendencies, allows us to look at ancient poetry, or medieval Scholastic scholarship, or Dutch paintings, with a sense of awe, wonderment, and interest. This is not the case with technological achievements that move far more rapidly. The question of human nature is not something that can be examined simply in terms of content, but requires a deep sense of context. Human nature is not a thing so much as a series of processes range of possibilities that only human beings can exhibit.

LINDHOLM: But it seemed that you did make reference to human nature, so. . . .

HOROWITZ: I do that almost in a pragmatic way. When I deal with the irrational, I am speaking of non-functional responses to the quotidian world, and not a state of pure mind. Self-destructive tendencies exist, in the psychoanalytical sense. People will commit mayhem; they will destroy themselves. I consider that to be irrational. There may not be a general theory of human nature, but there are specific theories of what you might call neurotic patterns of behavior that explain homi-

cide and suicide, mayhem and murder. This is not to say that human nature is changeless. Rather, what we observe are changing patterns of adjusting to surviving personal derangement at one end, and at the other end, surrendering life itself if larger public issues are involved.

Your concerns are important, and I wish I could have an answer as to what human nature is for all times. However, all one can really say in social-scientific terms is what kinds of modes of analysis allow the person to survive, grow, expand, and to live a fuller, longer life in the company of other persons. Now, those are values. Are they human-nature values? They certainly are commonly expressed beliefs. It is not the same as a fully blown theory of human nature. I confess to fall far short of such a goal. Human beings operate within different frames of discourse that allow us to survive in the real world once we leave this building. What is it that we have to do to prolong life? In that sense, suicide, in my scheme of things, is irrational. But even at the extremes one can find medical exceptions to preservation of life. Still, for social science, as for medical science, life itself is an ultimate value. Does one need a theory of human nature to justify the impulse to survival and transmission of culture from generation to generation? It might help, but I do not have much to add to classical normative theory in this matter.

SEKULIC: For the sake of the discussion, I will ask the question whether the proposition that you put forward, that history is somehow falsifying our theory, is true or not. If the fall of communism, just taken as an example, proves that those who created the theory of communism are wrong, then we can also argue at the same time that during the time when communism existed they were right, because we did not know that it would fall.

We can also make a more absurd argument, regarding the existence of the Roman Empire. Back then, those who would be formulating some kind of theory about the Roman Empire's durability would be right during the long duration of the Empire, and then they would be proved wrong when the Empire disappeared.

I would argue that history does not offer a judgment on whether a theory is right or wrong. It is simply wrong to assume that social science formulates theories that are proved or disproved by historical facts of such an ephemeral nature as communism, capitalism, the Roman Empire, and so on. Probably social science should formulate theories and understandings of reality on a different level. The conceptual apparatus and theoretical claims of the social sciences should not be prov-

able by the existence or the fall of communism. They should be created on a different conceptual level. Otherwise, it is not irrational for those keeping the idea of communism alive to do so, because, if it was true for 70 years, maybe it will come back and it will be true for the next 100 years, and so on. I think this is a fundamental question of social science.

HOROWITZ: I agree that it is a fundamental question, since at stake is the role of prediction in the shaping of actions and beliefs. There were those who predicted the demise of Soviet communism, and did so with a reasonable presentation of evidence. There were others, of similar intellectual persuasions, who felt that communism would triumph—one such pessimist was Whittaker Chambers. But the confusion here is that the rightness of a system is vouched by its simple existence. It does no such thing. Nietzsche understood as much in *The Use and Abuse of History*. He pointed out that decisions about moral actions were not predicated on victory or defeat in a battle or a war, but on the ethical guidelines with which men went into battle. Again, the social sciences are vessels of analysis, including the analysis of moral choices in a variety of empirical contexts. The social sciences neither justify the status quo nor do they justify resistance to existing social orders.

SEKULIC: It's what they are creating concepts about, and I do not think that we can create social-scientific concepts about communism.

HOROWITZ: Well you are a wise man and you offer a profound formulation and a very difficult one to rebut. What you say is, of course, correct. Does it mean if you were living in 1950 that the theory of communism was right?

I do not think that social science should be seen as historical or purely empirical and descriptive. Normative judgment is made in Stalinist Russia in 1950, as it is in Putin's Russia today. Discussions about regime success and failure take place all the time. Some are useful, most are foolish. We must look at actual events and tensions. Others may design scenarios for changing the system they disparage. In this regard, one might say that Leon Trotsky was right in his critique of communism, namely that a system based on a theory of pure nationalism or pure national communism was a contradiction in terms, that economic forces and political forces would overcome that kind of a system and that it was doomed to failure. But his recognition was belated, and he himself never lost faith in the goals of communism. Well, that too is a form of prophecy. Sometimes prophecies are right. Sometimes they are wrong. Sometimes they are not relevant to personal behavior.

When you look at the phenomenon of Hitlerism, for example, it did not crumble because of internal disruption or disintegration. It died at the hands of a more powerful set of international adversaries. Those who would argue that Hitlerism was doomed from the start rarely seem able to predict how the end comes about, or for that matter why it comes about, when it does come about. In point of historical fact, Nazism (happily I might add) was beaten, and severely so, by stronger military external forces.

Having said that, it is not an either/or situation; we pay attention to history or we do not; either we are rooted in the immanent, immediate present or we try to explain and predict the future. At some level we are creatures of the immediate, of the environment that we operate within, and of the social order that we're in, while at other levels we have, as social sciences, a right to critically evaluate those phenomena. But it seems to me somewhat sophistical to say well, you can say of communism now that it failed but if you were living 50 years ago you would not be able to say that. There were those who did say that, in fact, and those people were proved right. These critics of communism, often hated and despised, were correct in their evaluation. Those who are right are not always necessarily going to be loved, clearly.

The question you pose remains troubling, because you say, well, communism could come back. Of course it could come back! The Bulgarians have elected a communist regime, despite the fact of the experience they had. Yes, we are not dealing here with absolute determinations. Human life is a matter of will, and therefore the social sciences must evaluate the role of will in the formation of political processes and political systems. So there is no contradiction, it seems to me, between what I am saying and a return of communism—or for that matter a return of fascism. Political systems, even the worst of them, have a way of changing forms, rather than disappearing outright. Yes, it can happen; that is why at the end of the day the message is the struggle. Social scientists are not absolved from struggling for that which they want or do not want.

But that struggle cannot be seen as a necessary outcome of their concerns. The evaluations may be right, and the outcomes may be unhappy. We face that all the time. We cannot guarantee happy outcomes. Social science is not a business of happiness or of denying the prospect of evil empires raising their heads. It is a business that looks at the world and takes the consequences of losing as seriously as of winning, in terms of values. Human beings are more complex than the natural

order of things indeed; we are dedicated to a value system that social science itself cannot always warranty. What humans do is superimpose value theory onto the course and analysis of events. That is why we read novels and write poetry. There is something beyond the social study of human affairs. I am not urging that we dismiss those who are critical for one reason or another.

I am also not arguing the case for an end to value theory. I am not arguing against the idea of struggle for what is felt to be right simply on the grounds of an abstract historicism. I am saying, "Be prepared to lose, be prepared for the evaluations you come up with to be wrong." We struggle against outcomes we dislike, but they may happen anyway. The social-scientific fields that are triumphant, it seems to me, are suppler, more capable of making that distinction. And those that cannot accept the failure of prophecy are, it seems to me, holding a very bad end of the stick.

This is an important question. I wish I could give you a better answer.

EASTWOOD: I wanted to make reference to the session that we had this morning. Professor Holton introduced a discussion on the nature of science, and during the course of that discussion Professor Steinmetz, the neuroscientist who is with us, made a very interesting observation about the progress of his field. He told us—and please correct me, Professor Steinmetz, if I am wrong about this—that the field was really institutionalized in the early 1970s, and that it has made tremendous progress—that it has accumulated a tremendous amount of knowledge—over the course of 30 years. Now—again, if I understood him correctly—Professor Steinmetz linked the growth of that knowledge to the specific institutional structure of this very interdisciplinary form of inquiry that we call neuroscience, and especially to the fact that what has taken place in neuroscience, like other natural sciences, is the training of younger scholars to work on similar problems, to work on the hypotheses proposed by their teachers, and to attempt to subject them to attempts at refutation over time.

Among your many important points, you [Prof. Horowitz] reminded us of the fact that there is a great deal of very good work being done in the social sciences, and that there has been for some time now. I am wondering, however, if you agree with this graduate student's assessment that what we aren't seeing is an institutional structure that allows for the growth of social-scientific knowledge. What instead we're seeing are a number of individuals—very intelligent, imaginative theorists like

those that you mentioned—who work in relative isolation from some of their peers, and who aren't very often building upon each other's work.

And second, I am wondering if you, and if anyone else, feels that there might be a way that we could create an institutional structure in the social sciences that would allow us to try to match the success of some of the natural sciences in this regard.

HOROWITZ: Jon, in a boxing arena there are no students, only professional pugilists of unequal talents. Take Professor Steinmetz and his work linking psychiatry to physiological constructs of the human body. Of course you are right. The world of science, and social science, is one in which there are sometimes private individuals apart from the larger whole, who reinvent the way in which we see the world, what we emphasize. In Joe's case, many of the original inspirations were of a Pavlovian psychiatry that rested on a theory of the higher nervous system and the behavior of the nervous system, as distinguished from the more conventional theories of neurosis or psychosis that Freud had. So you have here Freudian and Pavlovian ways of looking at the same world.

There is nothing wrong with explanations rooted in physiology rather than medicine as such. That permits experiment in social science as well as in the medical sciences. It is not a call for destroying one or another vision. It is, rather, it seems to me, that what the social sciences have done, over the course of a hundred years, is allow us to filter into our brain new ways of thinking of what is important and what is not important.

One hundred years ago the appeal of survey research hardly existed. Now there is not a newspaper—such as *USA Today*, an ordinary paper read by ordinary people in every hotel in America—that does not have rich data, charts, and information on attitudes, beliefs, persuasions, outlooks, orientations. The filtering of public-opinion research itself changes the parameters, revises the way we look at the world.

There is nothing wrong with multiple theories to explain specific events. It's not that the old narratives are wrong or that the new emphasis on information is wrong. Rather, the problem was to infuse data into theory in the nineteenth century, and it is to infuse theory into data in the twenty-first century.

If there were something improper in making use of available information—which allows us to make decisions about people, about the world, about theory—that would be self-defeating. Social science

is a struggle, a debate conducted with the best information and ideas available at the time. It is not simply an event. Social research is more than a series of building blocks piled high onto each other leading to the stairway of heaven. Some social sciences are inclined to that evolutionary process. But the conduct of affairs in one social science after another argues against such a sheer piling on of data. Psychology, historically, is much more inclined to build on case studies, case-history analysis, than, say, sociology is. Anthropology is less involved in the study of small groups, and takes entire societies as its data. But the results are not uniform.

More to the point, the quality of thought, the refinement of analysis counts for more than the uniformity of the end results. Different fields yield different ways of looking at the world, but that is not the answer. The answer is the dynamic of the interaction itself. The real test of social science is the toleration of the new, and the ability to handle the new, and to be gracious and generous in estimates of the work done by others.

The danger in social science is to demand allegiance, to make it a function of the state, or a function of a party, or a function of anything external to itself. That is all I can add to the topic for the moment. Since you are a Latin Americanist let me note that I was involved in the attempt to disabuse the Department of Defense of the idea of doing work on civic action in Chile. I thought it was a mistake. It was wrong. It was dangerous. It was not thoughtful. It was survey research on public opinion, but it was self-defeating, risky, and the consequences of it were negative. Well, that is the way you present your findings. Their utility will be tested by the collectivity of scholars in a particular area over time. You do not require an ideology to do social science; indeed, fixation of belief is an impediment to honest research. What you need, in many ways, is for social science to understand ideologies.

I do not know how else to answer those kinds of questions. Sometimes you build, sometimes you critique past modes of thinking. Last night [in conversation] I gave you four or five people, who knew or know something about nationalism, but I forgot to mention Hans Kohn, and I forgot to mention Louis Snyder, who knows more about the subject than most. The present generation of students of nationalism and ethnicity build upon a legacy, but they do not simply mimic what came earlier. You may find that the nation-state is not a solid organizing premise for certain types of research. You may find that in Venezuela, for example, nationalism is not a significant explanatory

variable. The process of urbanization may prove more valuable, as may the serendipitous role of petroleum in Venezuelan history. Black gold turned out to be more valuable in the study of this nation than, say, real gold deposits. That is what the social sciences are about. It is most certainly not about carrying guns into battle and declaring that the superiority of arms is proof of the superiority of a given social system. That is a fascist way of viewing social research, not the way of scholars living and working in free societies.

CASANOVA: I don't agree with Eric Voegelin about everything but I think that he might useful for these themes. He points to something that I want to bring up here. He says that reason is something wider than Descartes thought. Descartes thought of reason as subject, *res cogitans* as opposed to *res extensa*. Before Descartes, in philosophy, what philosophers and political thinkers had to study was Being and, of course, they were a part of Being. They weren't a subject in front of an object. That is, the subject/object distinction was not clearly drawn and they recognized that they were themselves of the object of study.

HOROWITZ: From whom are you quoting? Excuse me.

CASANOVA: Eric Voegelin says that about Descartes. I don't remember in which work—probably in *Anamnesis*—he says that before Descartes we thought about Being, not about objects. And Being is wider than objects. Descartes himself lived in a context, and his science was part of a wider context. And in ethics, too, we were oriented to the good, and the good wasn't an object, as it is for the utilitarians; it was something wider than the utilitarian good.

I think that if we try to think of social phenomena as objective, we cannot understand phenomena like ideologies; they are inexplicable for us. And Voegelin—but before going on, let me say that I think that. . . .

HOROWITZ: You sound more like Martin Heidegger than Eric Voegelin. All right, Being is wider than objects. And? Now what do you want to do? What do you want to do with Being?

Social science, unlike metaphysics, is not in the business of Being. Social science is more into the study of Becoming than of Being, and we do not do that very well. Perhaps you are correct, maybe Martin Heidegger was right. Being is wider than Becoming. Such distinctions go back to the pre-Socratics (and with little improvement they persist). If I only knew what Being was, I would be happier. But it would hardly improve the quality of work one does in social science. The study of society is not an argument with philosophy, politics, or moral

history. It is a way of showing how the intellectual and theoretical scaffolding of a civilization may either explain or be explained by other forces: the economy, the technology, and the system of communication.

CASANOVA: But I wanted to say before, as a parenthesis: I agree with Averroes that, from the point of view of reason, faith is not the higher knowledge. And with Averroes I also agree that people need religion, need faith. Yesterday, Professor Friedman said that he could not understand why so many scholars were leftists. And I told him that, for example, MacIntyre tells us in *Marxism and Christianity* that he once was a Christian, a Catholic, and when he was 23 he was disenchanted and he looked for something else. Alasdair MacIntyre turned to Marxism as a substitute for Christianity; that's why he was a Marxist. And this phenomenon doesn't only lead to Marxism. For example, I think we can see something similar among the apologists of capitalism. For some, the liberal economy is a kind of religion. Markoff and Montecinos, in the paper distributed for this conference, point to some of these issues.

Look, in the course of history, human beings need something like a relationship between divine reality and individual will. Voegelin makes this clear in his treatment of Christianity, with its pairing of divine providence and the human will, in the first centuries; and in paganism, *tyche* and the will. I believe that we can see something similar in Kant: law and the will, the will as phenomenon and determined, but there is also law; and we can see this also in Marx: history and the will; and I think that for many apologists of the liberal economy, the market and egoism are that pair. In short, you've drawn our attention to the fact that the left-wing utopians act as prophets, that is, that their activity has a quasi-religious character; I am trying to point out that no less is true of other sorts of would-be prophets of neoliberal economic reforms.

I want to say one last thing, related to this, and with it I will conclude. In 1989, the liberal economists took power in Venezuela, in a manner very similar to how this occurred in other Latin American societies, according to Markoff and Montecinos. And Carlos Andrés Pérez appointed as his chief minister an economist. Pérez had a popularity rating of 80 percent on January 23, 1989. By February 27, he suffered the biggest riot in Venezuela in the past century. Why? Because the economists, including his chief minister, didn't see the political, economic, and social reality that they were facing; instead, they saw only the dictates of their economists' creed. Specifically, they didn't respect the government's commitments to the middle class, or of the poor to the rich, because industry had dollars with preferable exchange

rates; the government didn't respect that. Half of industry broke down. The government freed the interests of. . . .

HOROWITZ: There are levels as well as styles of discourse. I do not see how you can get from Venezuela and Carlos Andrés Pérez to The Great Chain of Being or from Arthur Lovejoy to Martin Heidegger. I do not hear any question emerging from such proclamations. You introduce matters that range far beyond the topic of the moment. Perhaps we should allow another person to redirect the questioning to the topic of moment.

WOOG: Professor, I hope you'll accept a question from an undergraduate. It goes back to the point that Professor Sekulic and you were debating a moment ago. I have a question that follows from his about communism. History says that the Soviet Union fell, and now we know that maybe it didn't work out so well after all. But Professor Holton this morning was discussing gravity very briefly, and he said that right now we think gravity does not change with time but there is some debate, so that maybe in a few years we'll know that there is a slight effect of gravity on time, and when that happens well know that we are wrong today. Today we can use gravity, I know I stay in my seat because of gravity, but small things may change.

I do not think we need to have Russia have a change in the government to realize that communism wasn't working. What we needed to examine was the individual, we needed to go over to Russia and say: How are people living in Russia? If our CIA, let's say, would have interviewed people on the streets who couldn't get bread—instead of talking to members of the KGB, asking them questions about how society is going—maybe we would have realized Russia was about to fall. And if we just do the empirical objective research there, then we may be able to realize what in fact is going on in other countries and here as well.

So, my point is that we are dealing with very dangerous issues. And in this context, what is the role of the editors of the journals? What is the role of the organizing members of the journals to say: "Okay, you're presenting this paper, you'd like to have it published . . .?" What should they be doing at these conferences each year, if not passing the political resolutions you mentioned? What should they be doing in their research to make sure that we are concentrating on the individual and are finding out exactly what is coming up, what is happening now?

HOROWITZ: Political systems do not necessarily fail because individu-

als are hungry. It is a well-known fact that revolutions are not necessarily made by the hungry, but by the upwardly mobile discontented.

It looks like the macro question has come up again. I do not think that the good society is necessarily always triumphant or that bad societies, if you want to call them that, are necessarily always vanquished. I think that there have been many societies that are less than noble, whether the Roman Empire or the Fascist empire, that are not especially to my liking, that were not overthrown by internal machinery but were defeated by large external forces. I think it is dangerous to presume a victory of the good society over the so-called authoritarian society. I would like to believe in perfect outcomes, but I do not think history demonstrates any such thing. As a result, truth is a value and obligation rather than the supposed good society as such.

Such considerations lead to the micro question: What is that evaluation? Evaluation is very simple: the exercise of the free press. The purpose of professional periodicals, professional journals, is to carry on a dialogue within a discipline and between disciplines. Transaction publishes something like 27 quarterlies and 20 annual reviews. Most of those journals are dialogues within each discipline. They look inward—whether in psychology, sociology, economics, or any other area; and they say, "Hey, look me over, look at the way I look at the world, look at the way my research program sees it, look at my paradigm, pick me. I see further, I'll make you a happier person in the process." At the same time, every journal looks outward; the process of a journal editor, or of a journal, is recruitment: you are recruiting people from within the discipline and people from other disciplines who you feel might be interested in your way of looking at the world because it will give you a deeper, clearer insight.

Does this change all the time? Of course it does. Will mistakes be made and are they sometimes rectifiable? Yes, they will be. But I think that we have to understand what we are. We are not kings; we are sweepers, we are janitors, the world of social science is janitorial. We are not sitting on top of the world mandating an outcome; we are sweeping up the leavings of others and trying to make sense of them so that we can have a respectable-looking world. When we see ourselves as janitorial, we are much better off, because it's more true about what we really are. We know better, maybe a little better, than others, but not much; we are sweepers, we clean up, we look at debris, we try to make it look nicer if we can.

But the idea, the Platonic idea that we are mandating world history

was a lovely idea that lasted from the French Revolution on through Hegel, and it must have been with Dilthey and Rickert in the social sciences maybe 80 years, in which the whole idea was to invent the world anew, to create a whole new paradigm, and come out with something spectacular, to make the world recognize prospects for teleology. But again, such disguised utopianism is not the way of the social sciences. Hegel, Rickert, Dilthey, Heidegger, for all their differences, shared an interest in metaphysical history, not in empirical social research. These people were not interested in the world as it is, so much as in a world that should be. We social scientists operate largely anonymously: the *Theses on Feuerbach* notwithstanding, we seek to understand the world, not necessarily to change it.

We need to recall that when we walk outside this room, we are not sages and prophets, or even professors of wisdom. We become very much anonymous persons in our work life. If you are not like that guy behind us [referring to picture of Einstein], you are not recognized. You teach, make a living, have students, and write papers read by the tiny network to which we belong, rather than the great mass. As long as you do that with some integrity, with some honesty, and recognize your own anonymity, and recognize the importance of janitorial service, you are going to have a long life. But if you start transforming what you do into the role of kings, into reshaping the world, a whole epiphany of the world in one fell swoop, we run the risk of playing God! You are also in for a long, sad life of unrequited expectations. That age, of German Romanticism, is gone, gone.

CZERKOWICZ: I have a very undergraduate question, also. Something that has been troubling me during this whole conference, something that I never really thought about until I was reading through some of the papers, was the idea of fact versus theory in the social sciences. Why is it somehow accepted that facts and theories do not match? I was hoping that you could perhaps explain this to me a little bit, but, if I could say my thought first, then you can tell me that I am wrong. . . . It would seem that, if we have a theory that can't be backed up with facts, we have a tremendous problem in explaining reality; and when the theory is terrifically far off the mark, because it doesn't adjust to reality—like communism, which can never be proved wrong—it existed in Russia and now it doesn't, and we don't really know anything more about it; or like rational-choice theory which, as acknowledged, doesn't fit many circumstances, but political scientists say, "Well, we're going to

stick with it, because it's all we've got right now"—it seems like there's a big problem there.

Why are faulty theories still regarded as reliable enough to be a sole dimension of analysis for social scientists? I'd like you to speak about the place of such theories in the social sciences and whether or not they pose a serious threat to social science as an actual science, or to the future of social science.

HOROWITZ: You are asking for a response to questions people have asked for thousands of years.

The role of theory is not only a matter of finding where propositions may fit in social science, but of defining general thoughts about massive amounts of data that actually serve explanatory purposes. There are facts on the ground, there are theories in the air, and when one explains the other, then you have a happy outcome. Sometimes theories actually explain facts; at other times facts change before our very eyes. Sometimes we make facts change.

I will just raise one fact. Cuba in 1958 was ruled by a relatively ruthless guy, a dictator—the Mafia was there, gambling was there, and everything was wide open. In 1959, in a boat called the Granma, something like 70 people got in the boat, most of them got killed, 26 of them survived, and they made a revolution. They changed the fact of what constitutes Cuba. It was not theory that proved the success of the revolution, any more than theory proved the failure of other revolutions. Human beings are social actors, and as such, make changes in the world in a variety of ways that social scientists try to understand. That is the sum and substance, for me at least, of the study of economic and political development. Did the revolutionaries have a theory? Some of them did feel the guidance of a general philosophy, some of them had dialectical materialism, some had nationalism, others had no theory, and some had theology. But they changed the world, they changed the facts.

Facts are not immutable. Whatever you walk away from this conference with, I hope it is not only that theory must adjust to facts; sometimes facts confirm theory, and at other times they disconfirm theory. It is a horrible fact that *Mein Kampf* was first a theory of the German future, written in the prison house by Hitler, and ten years later, it was a fact, with a system of government behind it. So you can't simply presume, in our world, a fact. You cannot, in other words, ask: Do you adjust fact to theory, or theory to fact? You do both. You use your God-given common sense. You look at the world, and you say, what theories

best explain the assemblage of facts; or, vice versa, what kind of facts require changing, no matter what kind of sacrifice is required by me as a person, in order to enlarge a theory?

Those are the dynamics of social life, and that is why social science is different than many forms of physical science. We do not have a pure atomic reality operating independent of human will, desire, and ambition. We have variety and movement, things going in all directions, some up, others down. That is why the analysis of a system is not the same as a moral validation of that system. You do not need to answer that normative question. You do not need perfect theory, and you do not have to be bound by every fact, in order to be a decent social scientist. It's the exchange of facts, of theories, of values, all of those mesh in your brain, and allow you to survive and grow and help others learn a little. That is what I mean by our performing the duties of an academic janitor.

UNDERWOOD: You mentioned earlier the role of professional journals. Looking at a lot of journal articles for the current courses I've been in as an undergraduate, as well as listening to others here, it seems that they've almost failed in the role of criticizing inaccurate theories. Is there a way, or do you think that there is a way, that we could reform this procedure or in some way—I don't mean to regulate it, but something to set up accountability. . . .

HOROWITZ: The best way to express disagreement or dismay is to cancel subscriptions or, for that matter, membership in organizations that do not reflect your apparent interests. That is how life is lived! Journals fail when enough individuals say, "This publication is not answering to my needs! This article is nuts! I won't subscribe." And the journal fails.

Again I hear this notion, "How do you make a journal more relevant?" Relax. A journal has nine articles, 128 pages, every issue, and is published every quarter. Some of the articles are dopey, some are smart, some are wise, some are foolish, some you agree with, others you disagree with; you can't say that we are going to change this programmatically, all in one fell swoop. What you can do is say, "Wait a minute. This thing is costing me $75 a year, am I getting my money's worth? If not, then I am out of here!"

There are lots of journals saying, "Hey, look me over, I am better, I have superior answers." It is called the marketplace of ideas. We do not do it top down. We do it one article and one criticism at a time. And I mean that. Sitting in my editorial office, it is one by one by one. There

is not a complaint, not an anxiety, not a concern, that goes unanswered. But it is one by one by one. Again, it is what we do, most of us who live in the basement. You have to learn to live in the basement; the people in the basement turn out to do very well. They may not make a lot of money. But they do very well intellectually.

People are simply not mandated to say, I do not like that journal, and the journal has failed us. Which journal? Which article? Which field? Which area? Which year? When we get down to the nitty gritty, we open the journal, look at the article, make a decision, and if needed, write a better one. The better is a critic of the no-good. You don't like a publication? Walk away from it. You don't like it a great deal? Tell your friends to walk away. That is all you can do, short of a public riot.

FORERO: I am an undergraduate, too. The problem is, people don't necessarily subscribe to a journal because they see that it gives a better understanding, in the form of facts; many times journals are successful because they say what people want to hear, and a lot of that has to do with theories, especially theories of the future—people love that. They say: "Ah, this is a theory and I love it, it's what I need to hear." Instead of actually providing a good understanding of reality, a journal might prove more successful in just lying, saying what people want to hear, based on present fears or future theories that do not really apply.

HOROWITZ: This is a wise observation. In the short run, you are on target. In the long run, however, I fear you are off base. If I edit a journal of phrenology, and Lombroso's theory of the physical or cranial representation of criminal intent is what drives this journal, it may enlist curiosity and immediate interest. But over time, when the absurdity of phrenology becomes plain to its users as a test of character or of predicting behavior, other research designs will enter the picture. They will either refine the original premises, or simply invalidate the original premises. In such conditions, journals or periodicals with a high quotient of eccentricity and a low level of reliable prediction will not survive, except as a curiosity of a particular moment in time.

The real risk here is not that journals cater to the popular will. I suspect that, as we did in the 40th anniversary issue of *Society*, they cater to the professional will, the professional organizational life. The only way to defeat smug insularity, if that were your intention, is to establish and promote journals that are outside of the profession. This is one lovely thing about the United States: there are lots of journals, lots of maga-

zines, many books, and a great deal of controversy. You say, "They print what people want to hear." Well, somebody has to *write* it. And in professional life, that is hard to do. Happily, it is hard to make ordinary people do what you want them to do.

Some journals do have points of view, biases, persuasions that you may not be in agreement with. Then go and form another journal. You don't like quantitative research? Then form a journal of qualitative research. You don't like the weight given to one method over another in the *American Sociological Review*, the amount of it in *Sociological Review*? Then read the *American Journal of Sociology*. You don't like that, either? Read *Social Forces*. You don't like that one, either? Try another, and another, and another. The situation is not exactly like living in a country where there is one journal due to problems of history or problems of sociology; this is America, it really is an open society—at least in terms of journals and serials. Take advantage of it.

What is the point of talking in these ways about journals? Are we talking in terms of conspiracy? Somehow an editor sits there and he conspires to get results that he or she wants? Very few journals can operate that way, and few want to operate that way. I would modify your cogent point by saying that people subscribe to journals for a variety of reasons; one might simply be that it is the product of an organization of which one is a member. Then the issue becomes organizational membership rather the price of subscription.

Changes do not always occur in public. They also take place in the private struggles of personal conscience. There are now dozens of journals of cultural studies, many of them catering to sentiment that I do not particularly appreciate. But they are out there in the public space, they are reaching a marketplace, people are responding to them, they seem to find an audience. Well, that is good for them. Then they are going to have to struggle with the likes of individuals who do not share in their organizing premises about the social construction of the world. In this struggle, if conducted with civility and rationality, the outcome could be the victory of one, the defeat of the other, or as is more likely, a synthesis that allows the researcher to move on with new projects and prospects.

The world of research is a dynamic interaction, as Herbert Blumer might have said, not a simple action. Every time people talk about having a better social science, they really mean a perspective on the human world that closely resembles what they believe to be the case. In such a struggle the victory is the process itself, not the triumph of a metaphys-

ical system over all others. A return to great-man history is not going to happen in a world defined by high speed communication and interaction. Not in this country, and not on our time, and not in this culture, thank God.

GREENFELD: Professor Horowitz, let's join forces and fight them together.

HOROWITZ: Not until we extract a common worldview, or if you prefer, get our paradigms in order! My fear is that the paradigms of yesterday may well turn out to be the dogmas of tomorrow.

SESSION VII. A NEW PARADIGM FOR THE SOCIAL SCIENCES?

INTRODUCTORY REMARKS: Liah Greenfeld
MODERATOR: Jonathan Eastwood
PARTICIPANTS: Ali Banuazizi
Carlos Casanova
Jeffrey Friedman
Geoffrey Hill
Natan Press
George Prevelakis
Michael O. Rabin
Nathalie Richard
Joseph E. Steinmetz
Peter Wood

EASTWOOD: I'd like to call to order the final session of the day, with the question: A new paradigm for the social sciences? Professor Liah Greenfeld will give the introductory remarks.

GREENFELD: I am sure I will take longer than other introductory speakers, for reasons you will appreciate. So you should prepare for an introduction that never reaches the end, because indeed this is a beginning.

As I promised Professor Horowitz, I am going to propose a new paradigm for the social sciences, and to appease him for doing that, I also promise to offer him a fantastic Russian dinner; so everything is taken care of.

So—my answer to the question, "A new paradigm for the social sciences?" is a resounding "Yes."

Yes, a new paradigm.

As I told you in the very beginning of this conference, I was attracted to the social sciences first because I fell in love with the promise of objective knowledge of humanity. This is what I wanted, and I didn't get it. What I want is precisely a science of humanity, pursued in the way in which the natural sciences in the seventeenth century defined their pursuit: a pursuit shaped in the first place by the desire for the reliable knowledge and a deeper understanding of empirical reality. In this case the empirical reality is the human reality.

During my years as a social scientist and a professor of social science, I gained many comrades, people who want exactly the same thing. Most of them are, at this point, my students. They want objective, reliable knowledge of human reality, and I want to provide it.

Therefore, I don't care very much whether other definitions of "science" are possible—they are certainly possible; whether the social sciences as they are have fulfilled many important needs for those who practice them and for society—probably they did, whether or not they can be proud of serious achievements on those other fronts.

Unfortunately, they did not deliver what they promised me, and they don't deliver what they promised my students. They are not to be looked to for reliable, objective knowledge of human reality and a deeper understanding of it. I, therefore, propose a complete revision.

It is clear to me, to begin with, that the science of humanity should focus on humanity. But what is humanity? What distinguishes it from other animal species and makes it a subject for an independent discipline, separate from biology, within whose expertise other animal species belong? It is certainly not social organization, it is certainly not society, which seems to be the premise on which the social sciences are now based, for social organization is a ubiquitous characteristic of animal life once we move beyond the lowest organisms.

What is distinctive about humanity, rather, is the fact that human social organization is not carried within human genes, is not maintained through biological reproduction—that is, *genetically*—and is not transmitted across generations by blood. Instead, it is carried, maintained, and transmitted by means of *symbolic* systems that we call, in their generality, "culture." First of all, language. The science of humanity should focus on what is distinctive about humanity, and what makes it a special reality—not what is common to it and other species, making it a part of something more general, which is taken care of by biology. The science of humanity should focus on culture.

Now, "culture" is a word very often used, there are even "cultural studies," but social scientists do not often put themselves to the trouble of defining it.

So what do we look at to study culture? What are these symbolic systems?

The answer is simple. At any given moment, all that is specifically human in society is culture.

Culture is the process of symbolic transmission. Examples of culture as such a transmission process thus would be religion; philosophy; literature; of course, above everything else, language as such; families; markets, money, taxation—all of the complex we call "the economy"; science, both as a practice and as scientific theory; universities; technology; buildings, paintings, cars. All these are subjects of changing traditions—that is, symbolic transmission—and we can study this dynamic aspect of culture through the economic tradition as well as through the literary tradition. Tradition is culture as process.

But we may study culture as a structure, too. What sociologists refer to as "social institutions," and what I talked about in the beginning session of the day, are in fact cultural structures. To call them "social" structures is to forget that the vast majority of social structures—that is, all those found among other animal species—are biological in nature. Examples of such cultural structures are a particular literary form, or religion, or family, or stratification, or, again, science—in this case, science as an organized activity.

And then, in addition, there are byproducts or fossils of culture: buildings, cars, paintings, printed scientific theories; that is, those theories that are made public—the context of justification—dollar bills, laws, coins, dictionaries, shirts, rugs, and other artifacts.

Humanity, in other words, is best seen as a way of life defined or shaped essentially by symbolic means. These symbolic means in their generality—culture—is the constitutive element or the organizing principle of humanity.

Humanity happens to be the way of life of a particular biological species. But it is not necessarily or logically related to the biological constitution of this species. Instead, it is an emergent phenomenon, in the sense in which biologists use this term—the sense in which life is an emergent phenomenon—and it is this that makes it a reality *sui generis*, and the subject for an independent discipline.

What I say applies even to the possibly singular capacities of the human brain. Which is to say that the human brain is a necessary but

not a sufficient condition for—and not a cause of—humanity, or culture.

Humanity is irreducible to the human species. Culture is irreducible to the capacities of the human brain. The study of culture is irreducible to cognitive neuroscience. But, of course, it is dependent on the brain, and cannot happen without it and its capacities. In fact, most of the life of culture, or the living culture, happens in the brain, by means of physicochemical mechanisms assuring the adaptation of the biological organism to the environment.

The brain provides the processing capacities for culture. It is, one may say, a culture processor.

The essential capacity of the brain in the culture process is imagination, which is the ability to supplement elements in received information: to complete the picture, to figure out the logical principle in a system, to jump to conclusions.

The life of culture, whether we are speaking of the economy, the family, literature, or science, is absolutely dependent on this by-definition creative capacity. And one can find it in every area of our human experience, working every single minute of our lives. You come here today, you came here yesterday, even though most of you had never been to a conference like this one. I could say this twice. Nevertheless, you all naturally jumped to conclusions about what kind of behavior was expected of you, and I must say, you performed wonderfully. Somehow, on the basis of very few givens, you were able to complete the picture. This is what we do, all of us, all the time. We are never taught how to behave in any given situation, unlike many animals that carry those rules in their genes. And yet most of us behave correctly.

The most striking example of imagination as constantly involved in the cultural process is the acquisition of language. Children, at the point when they acquire language, are actually taught very few words. Their vocabulary is very small. They are, as a rule, not taught rules of grammar. And yet, at a certain point, somewhere between ages 2 and 3, they suddenly acquire English, or French, or Russian. They are capable of constructing sentences they never heard, using words they have never used before. And most of them do it very, very nicely—some of them fantastically, in a very beautiful way.

The study of culture, as I say, is irreducible to cognitive neuroscience, but cognitive neuroscience can provide a conclusive test for our hypothesis. It can establish with certainty whether symbolic imagination in the brain takes place in a different location, or by means of different

mechanisms from, let's say, logical inference in rats, and therefore whether there is anything specifically human in the human brain's processing of culture.

Those of you who read my paper related to neuroscience will recall several other respects in which neurocience can help us, in my opinion.

While the brain is occupied with processing culture, it becomes acculturated. Symbolic systems such as language add a new world of stimuli to the stimuli of the natural environment.

Symbolic systems can be conceptualized as open programs, that is, stimuli that elicit all sorts of reactions—emotional, sensual, visual, audial, olfactory, and later on, intellectual. Each of these reactions in turn triggers others. Stimuli and nervous responses in the brain proliferate at an ever-increasing pace, and the activity of the brain dramatically increases. It is this dramatic leap in the activity and therefore the mass of the human brain under the impact of emerging language and culture, at an early stage in cultural evolution, that is referred to by evolutionary biologists and anthropologists as "the co-evolution of culture and the brain."

Culture—it seems—utilizes the brain, which before its emergence was underutilized. This leap of the human brain, stimulated by culture, takes the human species far away, cuts it off from the other animal species, makes men out of what previously were just great—and perhaps not so great—apes. It makes the human species a new, emergent reality, *humanity*, and takes it out of the province of biology.

When this leap is completed, at a very early stage in the evolution of culture, the previously underutilized capacities of the human brain are fully realized and co-evolution stops. From then on, culture alone evolves. That is why we are not any smarter today than our distant ancestors at the dawn of civilization. But in this long-since-completed process of co-evolution, we humans acquired what has been poetically referred to as a soul, the emerging phenomenon of the mind—which is the form culture takes in the human brain: an autonomous and self-generating consciousness, which is far, far more than what Marx calls a "conscious awareness" of reality, and which lives a creative life of its own.

Again, neuroscience can help us to discover whether there are special mechanisms in the brain supporting the mind. The mind supported by the brain, however, is already a culture, not a biological reality. It is a product of culture; it is a creation of symbolic systems. For symbolic systems are autonomous, self-sustaining, self-generating, and self-prolif-

erating systems. They do this—generate and proliferate themselves—by means of individual imaginations. But their building blocks, the material of which they are made, and ultimately the forms they take, are essentially culturally determined; that is, they are determined by the nature of symbolic systems themselves. Thus to understand culture, or the mind, *which is culture in the brain,* one has to analyze symbolic systems.

Symbols, and therefore symbolic systems, and therefore culture in its generality, are not biologically embodied. This is an implication of culture's emergent quality, of the symbols' freedom from what they represent. And this contradicts well-known intuitive views, which are so well exemplified by Noam Chomsky's view of the innate, genetic nature of language as embodied grammar.

Language is the central, the causally primary, the ubiquitous and ever present, and at the same time is the eminently explicit, accessible, and definitive of cultural systems. It is the very core of culture. It makes us human. And when I say that language makes us human, I do not mean that having the capacity for language means that we are human. No, I mean that language is something that exists beyond us, actually influences us as biological organisms, and makes us what we are. It is therefore the very core of humanity.

But it is extremely complex. Its rules, those implicit principles, however clearly guiding us in their acquisition and use, are unclear. For language does not exist as such. There is no "mentalese," in point of fact. It exists only in its infinite forms. So while it is extremely important, it is also tremendously difficult to understand how language operates, what the logic of its connections and development is, how its grammar evolves, how its vocabulary grows and changes. Whatever one says about it would be based on one's knowledge of a finite number of languages, usually very small, and of which, most often, one is perfectly comfortable with only one.

More than that, we are all native speakers of our native languages. Can a linguist claim greater understanding of his or her native tongue than a poet, just because he or she makes it an object of systematic and quantitative study? What if the linguist's methods or premises are wrong?

Fortunately, mathematics offers us an escape from this difficulty. Mathematics is arguably a kind of universal and formal language. Its logic is far less variable—one may even say, given the comparison, invariable—and far more explicit than that of language proper. The world is divided quite clearly into a few speakers and a vast majority of

non-speakers; and among the speakers, one actually can evaluate precisely the degree of proficiency. Mathematics, therefore, can offer us a model for the analysis of language, which is in several ways much simpler than the real thing, and thus a way into the study of language itself.

Now, in mathematics, the Chomskian "embodied grammar" argument does not work. It is made by nonmathematicians, that is, people who, if at all, speak this language but poorly; it is rejected by mathematicians, who claim that such a biologically deterministic, or at any rate reductionistic, approach can at best offer "a plausible though not a compelling picture of the prehistory, of the primordial centre of mathematics, but certainly cannot uncover or explain the mainsprings of fruitful mathematical activity." Sophisticated mathematical objects, they say, "are only definable in terms of the *tendrils* immediately giving rise to them. They are answers to specific questions posed in terms of other mathematical objects" [quoting Greenfeld 2002 paper for Session IV].

In other words, one can understand this symbolic system only in terms of the symbolic relationships within it. If we go back to language, this means that, for instance, we cannot reduce Shakespeare's linguistic creativity to innate grammar. The only way to understand it—even approximately, because there is of course a great intervening power of genius in this case—the only way to understand it is by understanding the state of language just before Shakespeare. This, as Professor Holton told us this morning, in fact is very similar to the case of Einstein in science. The only way we can understand Einstein is through the study of physics just before Einstein.

This brings us to a crucial point in regard to culture and its study: these symbolic relationships are essentially historical. The human science, says Marc Bloch, the great French historian, is the science of "men in time." Time, whose status in physical science and in the physical universe has been decisively undermined by the special theory of relativity, is, in culture, a fundamental property. And cultural time is strictly linear and absolute. It is also at any moment concrete, real, and unique (in the sense that it has tangible cultural properties, which are different from those of any other time). It is, in other words, an objective reality, a thing, a fact. We know that in physics, it is not a thing and not a fact. It is a matter of perception and does not exist outside of the perceiving mind. In contrast, culture, and language, exist or live in time. There are in them past, present, and future, which stand in a rigid and unchangeable relation to each other, because the present depends on the past and the future depends on the present.

This is the path dependency that Professor Friedman was talking about yesterday. Symbolic systems unravel or develop sequentially. There are organizing principles in them—which it is our prime duty to discover in symbolic-system analysis—that serve as boundary conditions for path-dependent development. But these principles constitute open programs, and which of their implications come to light depends—beyond the creative power and the imaginative bent of the involved individuals—only on the implications that came to light in the past. What we call "time" is our pervasive sense of the sequential connection of symbolic events; it is a central symbolic relationship; it enters centrally into the symbolic calculus.

So far the subject matter of this new science: It is humanity, which means culture, and we know of it that it is a symbolic, historical, mental process.

Now the methods. It is a rather common opinion that the distinctly human cultural reality, because of its symbolic nature, and because it takes place to a very large extent in individual minds, and is, therefore, by definition subjective, does not lend itself easily to empirical study—which was, to some extent at least, the reason why social sciences favored measurable, quantifiable and, so to speak, material phenomena. This, of course, is analogous to looking for a lost object under a streetlight, not because the object was lost there, but because the search there is easier on the eyes. In fact, however, we can get access to much cultural reality empirically, namely through our senses and direct experience, and surprisingly, this is an ability we lack to a far greater extent as regards physical and biological reality, which for the most part today we access epistemically—that is, through the logical process of thought, specifically characteristic of us as human beings.

Secondly, unlike in physics and biology, we do not necessarily impose on our subject frameworks that may be alien to it, nor do we conceive of it in terms of relationships that are natural to us but possibly alien to it—the subject matter itself—relationships such as causality or, as already mentioned, time. Those are themes, the *themata* that Professor Holton was talking about. They are empirically unprovable and unverifiable insofar as physics and biology are concerned. But in culture, we do not necessarily look at the subject matter through a lens whose very employment may modify what we see, because in the case of humanity, this lens is compatible with the reality studied, is taken from it, and applies to it perfectly. We can therefore gain access to cultural reality empirically: through introspection, direct observation—specifically, listen-

ing to living people—the study of documents and other cultural artifacts, and historical study. In all of these, language—which well may be our main focus of study—serves as our foremost tool, our microscope and telescope.

When from empirical study we proceed to analysis, language remains our most important *analytical* tool; it becomes our scalpel, the instrument of dissection and classification. The analytical usefulness of our empirical study depends on our ability to express or describe our experience with precision in words, while the quality, the accuracy of the analysis itself depends on the rigor of our verbal reasoning, that is, on its being logically organized, explicit, and unequivocal.

Human reality—the core of which is language—by definition transcends mathematics. Mathematics are not adequate for the description of human reality. It is very important to describe the reality the scholar is dealing with precisely in the language appropriate to it. In our case, mathematics is not adequate.

In the case of physics, nothing but mathematics can precisely express what is happening. I was just reading *The Brief History of Time*, by Steven Hawking, and I discovered that this book, written obviously by a very talented physicist, does not make sense. It does not make sense in English. For instance, Hawking says (I could quote, but, not to lose time, I will just remember), he says that no "normal object" can achieve the speed of light because, when it reaches 90 percent of the speed of light, its mass increases twice, and in the following 10 percent, it increases faster and faster, so that in the end, it becomes "infinite," and we need an "infinite" amount of energy to propel it. Well! In mathematics I am sure it is very easily and clearly expressed. But a person speaking English thinks: what normal object is he speaking about? A cat? You cannot propel a cat with the speed of light? Or a chair? And what about abnormal objects? After all, if there are normal objects, then there must be abnormal objects. How about them? Maybe we should not talk about normal objects but only about objects that *can* reach the speed of light. The language, at least the English language, is not proper for the expression of statements that must be made in physics. And what of the absolutely strange claim that the object's mass becomes infinite, so you need an infinite amount of energy to move it. What is "infinite"? In mathematics, it is very clear. "Infinity" means something very precise. But in English, it has a variety of meanings. What is infinite for me is not necessarily infinite for anyone else.

So ours is not an appropriate language for physics. In this case, per-

haps, because it is necessarily ambivalent, and we need language that lacks any ambivalence. In our case, mathematics is too precise and unambivalent to describe what we are dealing with. Nothing but language can capture and express the intricacies of relationships between human symbolic elements.

This does not mean of course that mathematics is irrelevant to us. As I mentioned earlier, it may be our surest way to the understanding of how symbolic systems work. In a sense, it represents an ideal type of a symbolic system, a symbolic system in a tightly controlled environment. Weberian "ideal types," in general, must be included among the analytical techniques of the science of humanity; as should be comparisons, historical and other—for instance, comparisons between direct introspective and indirect knowledge. Comparisons, perhaps, are our most powerful analytical tool.

Since much of the cultural process takes place in the individual mind, our fundamental approach, our way of thinking, must be Weberian *methodological individualism*. For this very reason, very little can be added to our honest understanding by statistics. I wouldn't say "nothing can be added," but "relatively little."

The connection between the mind and the brain suggests an important role in the science of humanity for neuroscience, again, whose experimental methods may permit us to test some of our hypotheses, such as the existence of physical signs of the social map that I mentioned in my paper for this session—the map of identity, the self, and will.

A few words on the nature of causality in culture in the "context of justification," or the meaning of *proof* in the science of humanity. We can approach this comparatively: The causes for which one looks in physics are in the nature of universal laws. This is what Professor Holton talked of this morning. The only demonstration of such causes is logical, mathematical. Empirical demonstration of these causes, of their existence, is impossible, because of the problem of induction. Therefore, physical theories, or causal explanations, are empirically unverifiable. They remain hypothetical forever, unless they are refuted and then discarded.

The causes for which one looks in biology are in the nature of highly probable explanations. Such causes can be demonstrated statistically. Statistical verification, however, is not empirical verification. The causal explanation or theory in biology is therefore never proven be-

yond a reasonable doubt—not certainties, but only likelihoods. Something may be highly likely, but it is never certain.

The causes one should look for in the science of humanity or culture are the actual reasons for individuals' thoughts or actions, in whatever it is we are trying to explain. The existence of these reasons in quite numerous cases can be empirically ascertained. Certain theories, therefore, can be definitively, because empirically, proven; they can actually be *true*, in the sense of being definitely not false. This means that in our study of humanity we can achieve the level of certainty undreamt of—because theoretically or logically impossible—in the established sciences of physics and biology. There is nothing at all that prevents us from making a true and progressing science of humanity.

We just have to want it. And we need a name for it.

BANUAZIZI: A short comment. I am entirely sympathetic to the direction of your thinking and your emphasis on this science of humanity, or the human sciences, as a distinctive field of inquiry. What I would like to bring to your attention—but perhaps you know it, I would be very surprised if you are not aware of it—is the work of another brilliant Russian, Vigotsky, who very much, I think, pursued some of the same ideas, but there are a couple of additions in his thinking that I think may actually complement yours very nicely.

In addition to placing the emphasis on culture and what he calls cultural-historical thinking—as many of you know, perhaps some don't, he lived in the first three decades of the twentieth century and I believe he died in the 1930s, a brilliant Russian philosopher, linguist, psychologist and so on—he brought in a couple of other notions that I think are interesting. One is the emphasis on human beings as tool users, so that in addition to the symbolic dimension of culture that you emphasized in you remarks, he also places a great deal of stress on the fact that thought, feelings, all human activities are, in his words, "mediated," that is, they are dependent on, the use of tools.

Now tools could mean physical tools, they could mean linguistic tools, they could mean analytical tools, and so on, but the use of tools, and the concept of tools, allows him to bring together what is essentially a psychological analysis of how the human mind operates and a sociohistorical analysis of how these tools emerge and function. So, for example, as we come to the age of computers, a new kind of tool, it opens up, as I think we would all agree, new possibilities for human imagination, for human thought, and possibly even for human emotions.

This is really not in any way modifying the thrust of your analysis—the emphasis on the *sui generis* quality of the human mind, and therefore of human science—but it adds this notion of humans as tool users, and of human consciousness, human thought, human everything as mediated, tying individual-level analysis to both social analysis and historical analysis—because the tools are obviously in the process of change, as civilizations, cultures, develop.

Just as a footnote to what you were saying.

GREENFELD: Well, it is a very apposite footnote. We have here a great expert on Lev Vigotsky—a Russian psychologist, Dr. Brofmann—and, yes, indeed, there is a great affinity.

I would not accept exactly that idea of tool using, first of all because we are not the only animals who use tools, and then because I don't accept the notion of "mediation." I don't accept this notion because it presupposes that our emotions and thoughts exist before culture. I think that they don't. Our senses exist, like the senses of a monkey or a mouse. But everything that makes us human is already created by culture. So it is not a matter of mediation. Culture in fact is autonomous, it is an emergent reality, it is something separate from our capacities—even though it cannot exist without them, even though it exists through us. It only exists through us, through our brains. Nevertheless, it has the ability of changing us, in fact transforming us into beings of a completely different nature.

BANUAZIZI: You obviously see him as a determinist.

GREENFELD: That's right.

BANUAZIZI: I don't read him that way, but if you do read him that way, I can see how you would come to that conclusion.

RABIN: I find myself surprised speaking up because, in contrast to the distinguished people here, I am just a mathematician and computer scientist. However I have a number of comments on this extremely illuminating and important presentation.

First of all, on the roots and bases of culture in this wide meaning that you [Professor Greenfield] assigned to it, in the human brain. I am not for reductionism, but I want to come back to Chomsky. Chomsky and some of his pupils are talking about a language instinct. In a society of birds or apes, it seems likely that their social behavior is completely within the genes. And I want to make a brief comment about that a little bit later. What Chomsky is advocating is that we have the language ability or, as Pinker called it, "the language instinct" wired into our brain, and consequently, we have the ability—explaining the wonderful

way and impressive manner in which children acquire language—the ability conferred on us by basic structures, which then get expressed in speaking English, Hebrew, Chinese, or what have you. Anybody who has observed their own children marvels at this enormous and quick progress that you have remarked upon.

That raises the following question: Might it be, and I will be inclined to assume so, that we also have various social instincts wired, so to speak, into our brain?

Well, that of course may be a result of natural selection, of people and generations living already within culture, and you actually alluded to that. And in the same way, people do have instincts about justice, about family, and other components of our culture. There are now tools—you are advocating a very high-level, analytical, introspective approach—but there are also tools, physical tools for mapping out the brain. So for example positron emission tomography can map out the brain, and it turns out that when we are listening to or telling a joke, a different part of our brain is excited and active than when we are, for example, viewing a picture of the Mona Lisa. So, this could be a possible topic of physical like study of the brain.

Now, to amplify on that point, there are people, let's say, in the criminal sector, who are characterized as completely lacking in moral sense—not because society deprived them or their father didn't like them or molested them, but because they lack a genetic moral sense and therefore commit those terrible crimes we read about from time to time. Now, for example, can the moral center in our brain possibly be identified and pinned down by objective physical methods?

Coming back to the question of our difference and our—maybe that's not politically correct—our superiority over the animal kingdom, there is an interesting question here: whether all, let's say, chimpanzee societies really behave very similarly and have exactly the same structure in fairly distant geographic locations, let's say in Africa? We know that human societies, even living in fairly close proximity, just a few hundred kilometers from each other, in some instances have different organizations. If it turns out that while humans speak in different languages, all chimpanzee societies—or lion societies—are organized in the same way, that would support the assumption that Liah is making about our cultural specificity, hence multiplicity.

Let me add that there is a very interesting variety of temporary logics, logics where time splits: namely you have a certain progression, and then from a certain point you have several possible futures, which I

think would be a useful concept for sociology in your sense as well, for cultural sociology or for the science of humanity.

A final remark: there is a very intimate connection between mathematics as we have it now and physics. Already computer science has given rise to different kinds of mathematics, without which you couldn't study the science of computers and, in fact, you could not have computer technology. So is it not possible that, if people don't just go and try to blindly shoehorn sociology and economics and so on into the narrow shoe of mathematics as it now exists, you might get different kinds of mathematics that are going to serve the science of humanity, in the same way that mathematics is so brilliantly serving physics and chemistry and the so-called exact sciences?

GREENFELD: Thank you very, very much. Every one of those comments is very helpful.

First I would like to address the comment on mathematics. Obviously I am not aware of the variety of logics in mathematics. But the existence of this variety does not at all change the comparison between mathematics, with its still very limited variety of logics, and the infinite variety and logical complexity of actual languages. So it would be extremely useful for the science of humanity to have a mathematician, truly proficient in all those logics, who would help us to analyze all of them, and would, therefore, give us several possibilities of approaching the actual language. Mathematics still remains the best venue we have to the study of symbolic systems such as actual languages.

I don't deny the possibility that we can, theoretically, have mathematics developed specifically for the science of humanity. I would say, though, that we could only have such mathematics when we understand tremendously more, about both humanity and mathematics, than we understand now. And perhaps it would be easier for us first to master language—not to understand how it works, but to master it as a tool for our analytical purposes—before we can develop a mathematics that could take its place.

Now about physical tests, neuroscientific tests, my opinion of the brain and all that? Given the hypothesis that I proposed, I am 100 percent for it. We have here a neuroscientist who was brought in by force to be the butt of my attacks and demands. To him and his colleagues I address the plea: Please, please, study these matters! Tell us, where is the mind? Where is the soul in the brain? Where is it located? Maybe there is a special location. I imagine from what I know that most of the work on the brain has been done not on human beings but on mice, and cer-

tainly not on human beings observing Mona Lisa—but there should be more.

My opinion of human beings who commit terrible crimes is that we assume an awful lot about them. We say they completely lack moral sense, but it's not that we put electrodes to their brain to see whether the moral sense is there or not. From what I know about mouse research, they can have special place-cells that reflect their imaginary map of their location. Now I imagine there should be something like this for human identity, which implies a social map, something like identity cells. Couldn't we test that? We should; this seems to be operational enough, right?

STEINMETZ: Probably one of the areas of greatest development right now in brain sciences is the use of brain imaging, and it's a technique that's changing monthly in its ability to do certain things, but what I am most impressed by is—in fact I am just putting the finishing touches on a paper that's entitled "Beyond Phrenology"—that in the very first few years of using this technique, what it did was validate everything that was known for a hundred years about the brain. For example, somebody has emotional problems and, guess what, if you stick their head in an MRI, an area lights up. So that's progress.

But what's really encouraging and exciting about this field are two developments that are on the horizon. One is the development of experimental paradigms that actually allow you to use this technique to study experimental questions. So when somebody defines what "identity," "self," and "will" are, they may be able to explore exactly where in the brain they are.

Beyond that is a development linking coding with imaging; this is a technique that the science is closer and closer to. This solves a very important problem in imaging that exists right now. Is imaging dependent on metabolism, so when you see something active in the brain it's equally likely that that area is inhibiting as it is exciting? That's the possibility they never tell you about in the imaging literature. But it's very important for determining what the brain is actually doing. So recording the electrical activity of neurons at the same time, once accomplished, will eventually answer the question if the structure is actually shutting down or if it's actually becoming excited.

So I think there are on the whole very doable experiments that can be performed in the next 10 years or so, but to do these you really have to have the definition of what the phenomenon is that we are really trying to look at. You can't say, "image the brain and show

me where the will and the soul are," because I don't know what the will and the soul are.

GREENFELD: Well, how about identity? How about the social map?

STEINMETZ: There's work, for example, that has already been done that shows that there are neurons in the temporal lobe that are excited when particular faces of people that you know, that are very individual to the people that you know, are actually showing stimuli. Interesting thing, monkeys show the same response in facial familiarity, so I don't know what identity, even, is at this point.

GREENFELD: Then, if I supply you with a definition of identity and of crises of identity, will you go back to Indiana and start testing that?

STEINMETZ: Sure, it sounds good!

GREENFELD: To continue the response to Professor Rabin. As to language acquisition, I don't believe in a language instinct. "Instinct" actually, at least as we use it in regard to animals, denotes information that is contained in the genetic material that actually tells the animal how to behave. For instance, a new mother rat will be told by this information from within her body how exactly to bring up her young. We don't have this kind of instinct. We have only the capacity to use language and to have moral values; our genes don't tell us which moral values to have, which language to use, under which conditions—so it is a very different thing. We are much less programmed than animals, and because we are programmed in such an open way, I wouldn't call even our programming an instinct.

I would say that we would be able to tie language acquisition, this extraordinary facility with which children acquire such tremendously complex knowledge as the knowledge of language, and the acquisition of social skills, which is also an acquisition of another very complex symbolic system that is not even as explicit as language—I would say that both these capacities are tied to our imaginative capacity. And here I would have to define, perhaps using teamwork, what precisely the imaginative process is, and how we jump to conclusions, so that a child has the vocabulary of 300 words, and the next thing you know, this child is composing poems, using both words he or she could never have heard and grammatical structures that he or she has certainly never had explained.

This extraordinary capacity that we have, to jump to conclusions, sometimes over huge distances, over huge intellectual distances—I wonder whether we could operationalize this concept of imagination

to such an extent that the neuroscientists would be able to provide us with hard data.

As to moral values, such as the idea of justice, the idea of truth, most cultures—cultures that I am familiar with, or anyone I am familiar with is familiar with—so most cultures have them, but they are open concepts in the sense that we all like justice, and we all like truth, but what we call "justice" and what we call "truth" are completely different things—sometimes diametrically opposed, contradictory. This, I would say, can be tied to the fundamental need for order, which is the functional basis of culture. The fact that we are *not* programmed genetically creates this need, and it is satisfied with culture. Culture then provides us with a sense of order, which mice and frogs and lions carry in their bodies. We have to take this sense from outside.

HILL: I think that I heard Professor Greenfeld remind us that language exists beyond us, as a dimension of knowing. If I heard her correctly, then I take this as a profound reminder of a necessary humility which we are in danger of losing. I would say that the evidence for the claim exists in the body of humane letters and can be demonstrated empirically and minutely. Language exists as a dimension capable of knowing itself and us. It is more than a simple mechanism to produce our concepts and paradigms, our formulas, our quasiscientific epigrams and axioms. The province of human nature is error and self-delusion, and language is the key to our comprehending that.

Let me just give briefly one or two illustrations. In the seventeenth century, *mediocrity* was a positive term. It meant keeping to the middle way, it meant keeping to the Aristotelian golden mean. *Enthusiasm* was a negative word; if you were an enthusiast, you were a dangerous religious fanatic. Since then those two words have entirely, completely reversed their significance. What does the history of that change tell us about the history of certain changes in human perception itself?

In the seventeenth century, the word *reduce* meant to lead back into the right way, to set right. It now means "to diminish." What is there in the history of human society and culture that has led a word, or has brought a word, which meant "to lead back into the right way," to mean "diminish"?

May I ask what Jewish members of this forum would think if they were to read—as I have read—the phrase "the menorah, the central symbol of Jewish religiosity"? I think their response should be that of deep offense. And, in fact, in the case I am referring to, there was a con-

siderable protest—and the bewildered writer of that phrase said that he meant absolutely no offense whatsoever. This is because "religiosity" for him—as for an increasing number of people—had lost its sense of "superficial appearance of religion," and had come to mean "the expression of true religion." That word is already, is still dangerously volatile. Since I came to the States 14 years ago, I have increasingly heard the word "religiosity" used as if it meant "true expression of religion" or "true religion." I merely cite this to show that what I am talking about is not, as it were, simply ancient history. We are talking about aspects of language, dimensions of language, that our formulas do not reach, and a dimension of language that, if not grasped, and understood, is still capable of inflicting enormous pain.

RICHARD: Just a question. I would like you [Professor Greenfield] to elaborate on your last point. I am not sure I understood what you said on causality in your new science of humanity.

GREENFELD: Marc Bloch, in the *The Historian's Craft*—a wonderful book, one of the greatest texts in the science of humanity—not that we have such a large choice, but this is one of the greatest—says that the idea of the relationship of cause and effect is an inherently human idea. This, in fact, is an essential element of the way we as human beings think. It is very possible—he doesn't explain where it comes from—but it is very possible that it comes from the historicity, that is the time-relatedness, the sequential order of our cultural experience. So it is very appropriate to understanding that order.

And indeed, in our cultural life, we constantly observe causes and effects. I take a sip of this tea because I want to take a sip of this tea. I have a reason; it causes my action. In other sciences, we do not know and we cannot know if there are causes and effects. But this is the way we necessarily think, and the same applies to time, for instance. Time is essential and fundamental and real in human life. It is not necessarily essential, fundamental, or real in anything else. Because it is so essential for us—this idea of cause and effect—we impose it on other realities.

This, however, as Professor Holton said, remains in physics a *thema*. It is not something that can be, under any circumstances, empirically proven. So what I was saying is that usually it is claimed that physics is very precise; biology is slightly less precise but nevertheless very precise because there are large numbers; and sciences studying humanity are completely imprecise because, well, everything is subjective and you cannot have causes and effects. Well, it turns out to be exactly the opposite. The causes for which one looks in physics are universal laws. There

is no way on earth we can demonstrate them empirically, because of the problem of induction. And only empirical demonstration is a proof.

It is said—I don't know if it is apocryphal or not—but there is a story that when Samuel Johnson was told about Bishop Berkeley's claims of solipsism, he kicked a stone and said, "I refute it thus." Of course, he should have kicked Bishop Berkeley, I mean, that would be a much better proof, but he wasn't there to be kicked so Johnson hurt his own toe, poor man, because this is a real proof, an experience, you know, you have it through your senses—physical senses.

The only way you can demonstrate your theories, your causes, in physics, is logically or mathematically, right? But this is nothing but a thought-process. This is no real demonstration. So, for that reason we have Popper: conjectures and refutations; the only thing that is possible in the way of proof is refutation. There is no way on earth you can prove a theory, because demonstration is not possible—that is, definitive demonstration.You can always have the sun, you know, decide not to rise or whatever. The causes for which one looks in biology, because of the great numbers they have to deal with, are highly probable causes. Whatever obtains in the 70 percent of the cases is a likely cause, which means that there are 30 percent of the cases where it does not apply. So again, such causes can be demonstrated only statistically, which is always just a probable demonstration. It is not that the probability equals 1. But when we are talking about the science of humanity, we can actually in many cases empirically demonstrate the causes, and therefore, prove the theory true. Not just refute it, but prove that it is true. And that is it; there is nothing more to say. So, we are in much better shape than all the other sciences, in fact, if only we want to pursue this path.

RICHARD: Well, let me just ask you another question then. You said you are drinking some tea because you want to, but what if I don't believe you? And say you are just doing it to convince me or . . . I am asking. . . .

GREENFELD: Well, you see, here I would have to go into a very specific discussion of historical verification, and there are various ways of doing it. It is not just that I am telling you. It is also, let's say, I am telling you, and you know about yourself that actually you also drink tea when you want to drink tea. You see, so then my claim acquires introspective verisimilitude for you, and perhaps you will believe it. But maybe you won't. Then you go around and ask a million people, well, why do you drink tea? Is it because you want to drink tea? Or is it ac-

tually because you hate it, but nevertheless some demon drives you to this drink? You know, that's possible too, but in any case, there are ways of testing it. There may be documents that you find, and in those documents you see one person after another claim: "I just drank tea, and this was only because I wanted to," you know? But in many cases the individual case may be so tremendously important, or there may be so very few people involved sometimes in very important events, that we *can* actually prove that all the evidence that is there demonstrates the existence of those causes.

CASANOVA: The first thing I want to say is that the best proofs I have found in philosophy for demonstrating that the mind cannot be reduced to the body are in Bergson and in some Aristotelian philosophers. In Bergson, *Matter and Memory*. And the second thing is that I am sympathetic with the presentation. But I have some worries. Because I think that it is too . . .

GREENFELD: I should tell you Carlos: Don't worry. Be happy. It does no good to worry.

CASANOVA: . . . too idealistic.

Consider some problems I have thought about: How is it possible, the communication (or translation) between languages, as I mentioned in my earlier comments?

Second, I think that beyond language, there is experience in our minds, and how could we explain that?

Connected with this, where does genius come from? From our animal/biological nature, or from language? If it comes from animality, I don't know how the argument can be sustained. And if it comes from language, or culture, it is a collective phenomenon, and this, to me, seems too Hegelian.

GREENFELD: You ask very hard questions, you know.

Genius. Genius is a very powerful imagination. An imagination that needs very few building blocks on which to build, and builds tremendous constructions. Einstein riding a train, seeing lightning, and jumping to conclusions. Well, of course, he also. . . .Okay, let's see how it happened. There were certain preconditions, but those were not enough for anyone to reach the same conclusions. He was trying in his autobiographical notes actually to analyze what happened, in the remarkable passage that he called: "What, in effect, is thinking?" And, in fact, he did need very few building blocks to reach his conclusions. Among these conditions is the fact that he was extremely sensitive to inconsistency. There was an inconsistency in the previous state of

physics, and from there, his logic was able to carry him a great distance. Then it took a very long time for him and other people to build the mathematical bridge that actually bridged the previous state of inconsistency and his conclusions. So maybe it was a mathematical bridge, not just riding a train and seeing lightning. But it is close to that.

The same—you can say that about any genius in any area. Is it animal or is it cultural? Of course, it depends on the constitution of the particular brain. Clearly, it does. But the brain itself does not cause a genius. The very same brain in a different cultural environment, in a different cultural situation will not realize this imaginative potential. So I suppose it is both. Geniuses exist only among human beings. Human beings exist only because of culture. But of course culture exists only on the basis of our brain. So it is all very tightly connected.

G. PREVELAKIS: We have very little time, so I will be very epigrammatic. I have three points.

The first concerns the idea of whether we can map things like identity, and I think that we can imagine different kinds of mapping. A map is a metaphor. I would say that maybe the metaphor of the hologram might be a better one to approach this question.

The second thing is: as a general comment, I am, again, the advocate of Geography. I think that in your approach, the historical element is obviously very strong, but let's not forget space, but of course this is part of the program, and it has to be done, to be introduced.

The third thing is the concept of "culture." As you use "culture," it becomes very general. This is not a bad thing, of course; I think that, in fact, in this way, you are saying that the categories we are using are not adapted—in economics, etc.—and that in the end, the symbolic structure is much more important than those categories—because of course those other categories send us to more deterministic causalities. But doesn't "culture" in this way become a kind of a black box? In any case, I think that there is a lot of work to go further than just the general category of culture.

GREENFELD: Of course, this is just the beginning. So, now I welcome you all to come and join the effort.

FRIEDMAN: I hope the Russian dinner tastes as good as this Russian winter is cold. . . .

I am not clear on a lot of things in what you [Prof. Greenfeld] said—and there are a lot of threads in what others, including Carlos, have said that could be pulled together.

First, I am not sure whether you would agree with me or not about

this, or this may not be what you are driving at: I don't understand, I don't see why it matters, whether we could put someone in a scanner at the moment when he is exercising his free will, and locate the spot in the brain—whether it is in the pineal gland, or somewhere else—where the will is, or where the soul is—I don't see why that matters, since we already know where it is within a radius of a few inches, so why would it matter to narrow it down to a few micromillimeters?

The larger point here is that it seems to me that you are proposing a compatibilism that I completely agree with, where the logical Kantian categories, generally speaking, are necessities for creatures like us, which means that we must think that they are true, in any meaningful sense of the word "think." People can "think" that they don't think that these categories are true, but they still actually use the categories, categories like cause and effect. But we know that from introspection, regardless of where those categories are physically located. That's my first thought/question.

My second one is related, but much more involved. I also lost you at the very end, on causality in the social sciences.

I agree that the problem of induction means that in natural sciences, there can be no certainty about laws, because the sun may decide, as it were, not to rise tomorrow; so there is the temporal problem of induction. There is also a spatial problem: even though laws seem to hold over time (thus far) here in the "Alpha Quadrant," as they say on "Star Trek," they may not hold in the "Gamma Quadrant," which we haven't yet visited or observed. So whether spatially or temporally, we can't make universal claims in the natural sciences–or rather, we can make universal claims, but we can't know for certain that they are true for all time and in all places, as Professor Sekulic said before.

Now I don't see why that's any different in principle in the social sciences, even though it's very different in practice, or quantitatively rather than qualitatively. In the same respect as in physical science, we can't know that universal social-science claims are true—either spatially/geographically, or temporally—because different people have different genes, and most importantly because cultures vary widely across time and space.

Given human cultural variability, though, the search for truth in the social sciences seems to carry *additional* burdens, which render certainty even harder to achieve than in the natural sciences—which doesn't mean that we don't believe that our conclusions, whatever they are, are "objectively true"; but it should mean, I think, that we recognize that

even more than in natural science, our conclusions *may* be wrong: fallibilism.

One additional problem in social science, that we can set aside if we don't insist that all knowledge must be of lawlike regularities, is that in natural sciences certain laws have—thus far—proven true, in the sense of not having been falsified. I am unaware of a single such case in the social sciences, because the default option of the cultural universe seems to be change, not regularity.

Still, only if we are trying to do exactly what so many allege has caused the problems in the social sciences to begin with—only if we are trying to mimic the natural sciences, in their search for universal laws—should cultural variability stop us from accumulating social-scientific knowledge, which has indeed happened, I claimed earlier, in public-opinion research and, now that you're prodding me to think about it, in evolutionary psychology, and in cultural anthropology—just think of what we know now about our hunter-gatherer forebears, and how important that is from an evolutionary perspective in understanding the workings of our own minds and emotions, compared to what was known about the world's "noble savages" just two or three hundred years ago.

Nonetheless, I would make a distinction—I don't know if you [Prof. Greenfeld] would agree with it or not—between knowing that *something* must be true and knowing—with certainty—that any one thing is true, which raises an additional practical obstacle to social-scientific knowledge, and surely a barrier to social-scientific certainty, even though in principle this is also a problem in natural science. Certainly, things are one way or another, or at least that's what creatures like us have to think; meaning that for creatures like us, that's true; meaning that that's true. Things are, necessarily, one way or another; truth exists; the truth is out there—to quote another television show.

But we don't necessarily know what the truth is in any given case, and we can't *ever* be *sure* of what it is. That's why Popper advises us to be fallabilistic in our posture toward our own theories. This is where I think there's a problem with social science that can't be set aside even if we abandon the search for universal laws. Yes, there is a truth about past, present, and even future, in social as in natural science—we are so constructed that we have literally no choice but to believe that. Either what was going through King James's mind when he did the things that precipitated the Glorious Revolution was x, or it was y, or z. But that metaphysical truth, of which we *can* be certain, doesn't help us identify

which theory—x, y, or z—is the truth about this particular event. The truth is out there, but the evidence doesn't always cooperate in making itself abundant to us on just the points we'd like to know about. I think I heard you say that you agree with this, but I'm not sure.

Even more importantly—this is really crucial, I think—in no science, natural or social, is the evidence decisive, in the form of indisputable "facts": discrete, interpretation-free, unmediated-by-people, unmediated-by-theory, culturally virginal evidentiary zingers that we can use to definitively prove or shoot down a conjecture. Even natural-science evidence is mediated by our senses, which can mislead us. That's why we need controlled experimentation. But in social science, we don't have it, because unlike the actions of molecules, standing behind the actions of people are minds governed by culture, just as you [Prof. Greenfeld] said.

But since the social scientist is analyzing those minds and cultures through the filter of his own mind and culture, what counts as good evidence for or against a conjecture is itself part of culture. We judge it by introspecting about how plausible one interpretation of the evidence is, versus another, by mentally putting ourselves in the shoes of the agent in question. We use *Verstehen*. But even though we can't but think that our *verstehende* conclusions are objectively true, the interposition of our own conceptual filter between us and what we are thinking about, the mediation of our ideas about the cultural phenomena we're studying by the cultural phenomena in our heads, means that there will be legitimate differences about the correct interpretation of a piece of evidence—say, whether a letter from Rousseau does or does not bear on interpreting a passage in *The Social Contract*; or whether a thousand voters' replies to an opinion survey asking them about the state of "the economy" does or does not explain their attitudes toward "the president of the United States."

Each of the theories brought to bear in such interpretive arguments is, in whole or in part, true or false, and so are the conjectural points they're intended to resolve. But that's a matter of metaphysical principle; it doesn't make conjecture and refutation any easier, let alone definitive—not only because of the apparent lack of regularities; but, partly because of that irregularity, because we can only very rarely perform controlled experiments in the social sciences—which, I suspect, are the practical source of progress in the natural sciences. Natural-scientific progress has been a matter of moving away from what's immediately, directly apparent to our senses, and theorizing about invisible causes,

not just metaphysically through the presupposition that there are such causes in general, but specifically by theorizing about what they are in a given case. And in order to falsify one or another of these theories, held by culturally embedded and creative scientists, the recourse has to be to experiments that control for possible variables other than the one the theory is proposing as the cause of the phenomenon.

"Experiments" in the social sciences, such as case comparisons, are usually open to many different interpretations because people are complicated, their culture is even more complicated, and when we try to sort out which possible cause of a human thought or action is the real cause, by using the inferential and ultimately introspective, rather than the physical, isolation of variables, our own minds and our own theories—those of the social scientists, which dictate which inferences are or aren't plausible—play a crucial role in the interpretation of what counts as evidence. Introspection is ambiguous. Not metaphysically "subjective," but practically so: it's hard enough to know what we're really doing—what the assumptions and implications of our own ideas are, let alone those of others—that I certainly don't think language can ever be unambiguous, let alone can thought be, let alone can the world of other thinking beings who we think about be.

Given the words on a piece of paper King James wrote, when we compare different answers to the question, "What was he thinking?"—what was his intention—there's the ambiguity of those words over time; there are the translation problems that occur—the misunderstandings, not just between "languages," but between the connotations of each word held by each separate individual, each in his own mental world; that's what makes them individuals. Each individual will understand words slightly differently from even contemporaries who speak "the same language." With that ambiguity, plus something that we've drastically underplayed here—the ambiguity that occurs when we're introspectively comparing different theories about the *un*intended consequences of people's actions—with those two types of ambiguity in play, introspection is going to be a lot less decisive than controlled experimentation would be. The ambiguities will not only lead to disagreements about whether a piece of evidence really falsifies a theory, but they'll also lead to disagreements about what counts as a theory, a conjecture, that's worth refuting in the first place. Consider the extra ambiguity when, as Adam Smith pointed out, the intentions of the people we're thinking about might contradict the overall effect of their actions, as is often true in economics and politics.

I was so keen to point out, early on, the politicized nature of contemporary social science not because I think politics and scholarship have to be separate in principle, and not because there's anything wrong, in the abstract, with the leftist politicization that happens to be going on in our particular time and place—the time and place of Western modernity—which I agree is a leftist project, to the extent that we're talking about intended instead of unintended results: the intention is to implement the continually refined implications of egalitarian, libertarian individualism. My point, though, is not that this project is itself objectionable, but that if one politically inspired perspective, whatever it is, sets the interpretive boundaries of everything that a scholar reads and everyone he knows—if, for example, the boundaries of what count as interesting conjectures, and what count as credible interpretations of the evidence that's supposed to sustain or refute them, are set by commonplace modern normative assumptions—then the human tendency to see only what we're prepared to see, which I do think "mediates" what we notice about the world, will be aggravated because we won't really—in our actions, not just as a matter of theory—we won't really be aware that we are, in fact, *using* conceptual filters when we notice things about both phenomena that we're trying to explain, and phenomena that are evidence for or against a certain explanation.

In other words, if everyone around us basically agrees with us, we'll tend not to be prepared to see that what seems like "common sense" to all of us is actually a theory or a set of theories, which are themselves cultural artifacts that we're using to interpret other cultural artifacts. "Exploitation" and "the economy" and "patriarchy" and other "social structures" and "social forces" can seem to be just obviously "there," unmediated by the very thing that probably makes the cultural world of such abstractions a necessity to begin with: the invisibility, or the opacity, of the vast majority of the world, which we can't get direct unmediated access to through our senses, particularly when the effects of our efforts to shape that world may run counter to our intentions. The fact that the cultural world is created by human action doesn't mean that it conforms to human design, so plumbing human intentions through *Verstehen* is only the first step.

Of course I agree that what is distinctive about us is that to some extent we rely on our minds' interpretation of sense-data, rather than on genetic programming, to navigate the world. But that means that the world doesn't interpret itself to us. As cultural beings, we have to supply the interpretation, and that's why we need to constantly try to identify

the substance and source of our interpretation and to test *them* as best we can. So when we reach a point where matters of theory or evidence seem to us to be direct and unmediated by our own culture, and to be clearly decidable through "common sense," we are in big trouble, whatever part of the political spectrum we are on. Culture, like natural science, is the transendence of the sensory—or at least it's the interpretation of the sensory—even the common-sensory. Any "commonsensical" interpretation contains implicit culturally derived assumptions, based on supposedly logical inference, claimed historical knowledge, claimed psychological insight, and certainly imagination. All of those claims, and the conclusions reached and leapt to within the view summarized by one's "common sense," even if one is a genius, may be wrong. And for those of us who are less than geniuses, even basic rules of inference seem to be hard to follow. . . . If we don't apply fallibilism to our own ideas, if we treat our evaluation of the evidence as if it comes straight from the world rather than from possibly objectionable theories we are imposing on the world, then we can be Popperians of the letter but not the spirit. The letter is conjecture and refutation, but the spirit is fallibilism.

Professor Horowitz predicted that certain forms of contemporary social-scientific nonsense will become extinct, because they are nonsensical in contradicting common sense about, say, the fall of communism in 1989. This prediction overlooks the fact that the very notion that it was really "communism"—not a bureaucratized, militarized travesty of communism—that fell in 1989 is itself not really common sense, but a theory that could be wrong, because it is based on a multitude of other theories—about Soviet history, about politics, about philosophy, about how to interpret Marx, about economics—that people with divergent beliefs might find highly dubious.

If I can be allowed to plug my own journal, the purpose of *Critical Review* is to subject just that sort of claim, about communism, to rigorous scrutiny from multiple theoretical directions. After 13 years doing that, I can only say that the world seems to me an even more complicated place than I thought it was when I thought the journal needed to be created; as an editor, and just as a reader, I've seen so many scholars make so many unnoticed assumptions and errors—maybe this is just a case of cognitive egotism!—errors in basic reasoning about evidence, and I've seen how uninformed they are, inevitably and without culpability, about so many of the "facts" that constitute potential evidence but that don't happen to be spotlighted by the theories with which

they're familiar. And worst of all, they—and I don't exempt myself—they find it so hard to gain objectivity about their own theoretical lenses by open-mindedly considering others.

Maybe this is what Professor Hill may have just been getting at in his remark about the human condition: the permanent state of ignorance and fallibility for cultural animals like human beings. If that's our condition, though, then the idea that we can attain social-scientific progress in anything but the sense in which some people in some corners of some disciplines will learn more about some things, even if most scholars think those things aren't worth knowing or are false, seems unrealistic.

Anyway, this very long speech could be a case of the narcissism of small differences, because Liah's points about the centrality of culture to human understanding, the path-dependency of ideas, the crucial role of introspection in evaluating evidence about human minds—I couldn't agree more. And the point about genius as the ability to "connect the dots"—it's a point of genius! I can't figure out if we just have a "quantitative" difference of opinion about how definitive the evidence can be in social science—whether Popper was right about the unity of natural and social science, or whether if he was wrong, it was just by a matter of degree. Or is it that there's a deeper philosophical question at issue: if we have *no alternative* but to interpret other minds through the lens of our own, as Hayek says in "The Facts of the Social Sciences"—does that mean that introspection can eventually clear up the ambiguities and sidestep the assumptions that are necessary in social science? Or does introspection itself introduce ambiguities that can't be cleared up by controlled experimentation, as in natural science?

In either case, since, in the social sciences, we can't do controlled experiments to sort good theories from bad, bad theories will, and do, have a life of their own based on the very institutionalization that Liah was discussing: people pick up bad ideas from their mentors and spend their careers churning out variations on those ideas, which they write down in books and articles and teach their students and protègés, ad infinitum, ad nauseam. That's the sad fact, I think, and we have to deal with it, if not make peace with it. If so, then it's not that we lack the institutionalization or the desire—or the institutionalization of the desire—to understand the world objectively. The problem is, how do we know when we've attained that understanding, as opposed to merely confirming what our conceptual apparatus—our culture—predisposes us to see?

In short, I see the problem as cognitive, not attitudinal, even though it's the cognitive problem that makes an open-minded attitude all the more necessary in the social sciences as a corrective to our conceptual blinders, because we don't have controlled experimentation to fall back on.

When you can't control the variables physically, the only way to test theories is to exercise self-control—by challenging oneself with diverse theoretical perspectives. But human beings aren't great at doing that.

GREENFELD: Let me try and respond to this very challenging set of comments. I counted in them four separate points—you can correct me if I missed something important.

The first point is that I am proposing a position practically identical to Kant's postulate of "a priori" concepts inherent in human reason, outside of which it cannot operate and which it inevitably imposes on everything around it. No. My position may be superficially similar to Kant's, but is in fact very different. The difference is not only that Kant must be interpreted either in the idealist vein, in which case the "a priori" categories are given to us—to all of us, and once and for all—by the creative intelligence behind the universe responsible for the existence of our souls; or in the materialist vein, in which case these same definite categories are in a Chomskyan manner wired into our brains and are a characteristic of the species as such. But my position is neither idealist nor materialist. Kant's postulate is very different from Marc Bloch's, and my claim that the ideas of time and, therefore, of cause and effect, are empirically and clearly perceptible everywhere in the life of cultural beings, everywhere in the symbolic reality, and, therefore, appear natural, ubiquitous, to human beings. These are not "a priori" categories either in the idealist or in the materialist sense: we experience them, because we live, as I said before, in time.

However, though most people tend to project what they know onto areas with which they are not familiar, imposing categories they derive from their limited experience on realities beyond the limits of this experience, such projection and imposition are not inevitable. In fact, my very discussion of the derivation of these categories from the specificity of cultural experience is a proof that there is no such inevitability; as is, of course, Einstein's theory of relativity. But questions about the universal applicability of the concept of time were raised long before Einstein settled it: one thinks, for instance, of St. Augustine, and those before St. Augustine, who asked whether there was time before God.

Your [Prof. Friedman] second point about causality in the social sciences is obviously related. Now, to begin with, in distinction to physics and biology, we know for a fact that we are not imposing the categories of cause and effect on human or cultural reality. Humanity exists in time, and there *are* causes and effects in time. Second, as I said, the very possibility of empirically proving a causal connection in either physics or biology does not exist, while it does exist in the science of humanity, which means that in some cases—clearly, not in all—one can actually demonstrate causality. In other words, the science of humanity does not have—by logical necessity—to limit itself to refutations (however often it must so limit itself), but can aspire to actual proof. This extraordinary possibility is related to the fact that where humanity is concerned there is no lawlike regularity: we don't look for—and were we to look, would never find—universal laws; the essential historicity of symbolic processes precludes this. Significant phenomena in cultural reality are unique, because they are time-related; and therein lies the strength, not the weakness, of the science of culture.

I am not speaking of proving a *law* true—physicists are interested in that, not I—but I say that I can prove a theory, a hypothesis, an explanation, and this is something no physicist can do. What I can prove, rather than simply fail to refute, while never universal, can nevertheless be very significant: for example, an explanation of economic growth, or of teenage suicide in modern society, or of international terrorism—not bad for a start. That's why I don't think that in the science of humanity we are bound by Popper's maxims and must be fatalistically fallabilistic. We work under a different set of constraints from physicists, for whom these maxims work.

Of course, I do not advocate arrogance: one has to assess the limitations of each case realistically—when we don't have enough evidence to prove a point, the point can't be proven, and that's that. But in some cases we do have enough evidence—and we always have to try and get every piece of the relevant data we can lay our hands on.

This brings me to your third point. The real source of progress in the natural sciences is not, as you suggest, the controlled experiment. The real source is threefold. First, there is the sociological factor, so to speak: the institutionalization of the values and norms of science, which happened in both physics and biology, but so far has not happened in the social sciences. Second, the essential theoretical factor: the emergence of a seminal cognitive framework or direction, allowing solutions to multitudes of new questions and inspiring multitudes of conjec-

tures—a theory fertile with ramifications. Such frameworks or directions were created by Newton and then Einstein in physics and by Darwin in biology, but nothing of this sort had until now existed in the social sciences.

Only in the third place is there the methodological factor: the elaboration of a method for the systematic testing of conjectures (the scientific method), as appropriate to the subject-matter. Now, the controlled experiment seems to be the appropriate method for biology; but in physics, controlled experiments are a thing of the past, they are clearly not appropriate for today's physics, and the only experiments one associates with Einstein are thought experiments. Insofar as culture is concerned, controlled experiments in a laboratory are not at all appropriate, and not only because they are very rarely possible. Nobody, though, prevents us from making thought experiments; only one needs indeed to think when one conducts them.

However, culture provides us with an excellent equivalent for controlled experiments in the abundant comparisons that it offers for our study and analysis. Comparisons, as I said, may very well be our most important analytical tool. Think, for instance, how much is added to the explanation of the emergence of the modern economy characterized by sustained growth by considering the Dutch case, which did not exhibit such growth.

But we shouldn't limit ourselves methodologically to comparisons; we have to make use of all the methods that are appropriate for the research and analysis of our subject. Introspection is one of these methods, construction of ideal types is another, linguistic analysis is yet another one.

But you [Prof. Friedman] are worried that introspection, in particular, could be misleading. Sure, it could. The case in point is Adam Smith, a person of very sharp intellect and great imagination, but limited in his research methods to introspection. The result: unwarranted generalization on the basis of limited, and as it happened, very unique experience. So one should not ever rely on introspection alone—one should use the methodological system of checks and balances.

These reasons for the inappropriateness and the lack of a need for controlled laboratory experiments—because we can check and balance introspection by other research methods—may somewhat alleviate your [Prof. Friedman's] concern, which I feel troubles you very much, that preferring and institutionalizing one theory must limit our collective creativity without offering any guarantees of progress in understanding.

That, as I understand it, is your fourth point. I would say it depends on the theory—and by theory I think both you and I mean a theoretical framework or direction, rather than any specific hypothesis. Privileging Darwin's and Einstein's theories not only did not stunt creativity in biology and physics, but in fact spurred it on where it was previously quite stunted, and this didn't happen because either of the theories was compared to their alternatives in controlled laboratory experiments—for there were no alternatives. No other theories were capable of similar creative ramifications or offered similar possibilities of further development. Whatever other theories were there had, in fact, exhausted their developmental possibilities.

But the social-science theories that have existed until today never had any developmental possibilities. They failed to define their subject-matter properly, and they mimicked methods that were elaborated for the study of pre-Einsteinian physics, rather than constructing ones appropriate for their own field—therefore, they remained completely self-contained, more like religious dogmas or party lines than scientific theories, and, in the course of a century-long institutionalized existence as academic disciplines, they did not develop. And—as you pointed out—they are politicized.

Yet, even such theoretically disoriented, methodologically muddled, and politicized social sciences cannot hermetically close our minds—as this conference, with its student participants, among others, and this discussion proves. And even during this somewhat darker age in the *pre-history* of the science of humanity, there were courageous, critically thinking people, such as the contributors to *Critical Review*, who subjected the claims of these politicized social sciences to rigorous scrutiny.

You did this [in *Critical Review*], as you say, "from multiple theoretical perspectives." Unfortunately, all these perspectives, for reasons I just mentioned, were barren. Nobody would think now of comparing the Darwinian evolutionary theory with that of Lamarck; Lamarck's theoretical perspective was barren and it is dead and buried. But new theories, developing the Darwinian theoretical perspective, are still subjected to rigorous scrutiny, first by their authors themselves, then by their peers. I am offering you a theoretical perspective that I believe to be capable of enormous creative ramifications; and there are methods appropriate for the study and analysis of our tremendous, fascinating subject—humanity. Our tools must be sharpened, of course—for that matter, our wits must be sharpened; there is no such thing, for the stu-

dent of culture, as being "uninformed inevitably and without culpability," as you [Prof. Friedman] put it. But I know you, so I know you'd prefer good hard work to being bored. . . .

WOOD: Two quick comments on the—it's a charismatic proposal and I'm not so efficient a symbolic processor as to have much of a critique, but two comments.

One has to do with something earlier in your chain of propositions. You [Prof. Greenfeld] emphasized the importance of that which separates humanity from the other animals, and that got you pretty quickly to culture. But, of course, culture is not the only thing that separates us from the other animals. Some of what you leave aside may be pretty important. Among other things, this: the human hand. The human hand has hundreds of different grips that no other animal has, and can do things that no other primate can do. The evolution of the human hand appears to be something that took place over a great deal of time, and one thing we think we know is that the localization of those immensely complicated controls over the coordination of human hands is located, if I understood a conversation with Professor Steinmetz correctly, pretty close in the brain to where language is.

Some evolutionary theorists postulate co-evolution between the hand and the brain, but the hand is not just an accidental thing, and that gets us to tool making and tool use. We are cultural creatures, but we are embodied, and some aspects of our culture may not be embodied in language, upon which you put such tremendous emphasis, but in these other capacities, the hand certainly not being the only one. Locomotion is another. But these things, which could build on the foundation you presented, seem kind of arbitrarily ruled out by the linguistic aspect of the development of your thesis. But that's the side comment.

The more important thing I wanted to do is answer the question with which you ended, the question about a name for this new science. And I am sure there will be other proposals, but none has come up, so I thought I would take a shot.

My paper for Session II began with the story of the Egyptian pharoah Sematicus, who, in an effort to find out what the original language was, isolated two newly born children and raised them up to find out what the first word they would speak would be. That word turned out to be *bekos*, meaning "bread." Sematicus was interested in the spontaneous emergence of culture, like you are; he put the priority on culture to history, just like you do, so I think there are two alternatives: you are proposing either "*bekos* studies" or the beginning of "Sematicology."

GREENFELD: I think "*bekos* studies." It has a ring to it; it is a catchy name, so when we raise our funds for the Institute, it will be the Institute for Bekos Studies. Thank you very much, Professor Wood.

As to the other point you made, yes, of course, you are absolutely right, there are several very important animal features that distinguish our species from other species: the hand is one of them; we talked about that, you and I, and I was very impressed; and the larynx, of course—where would we be without them?

But I wasn't talking about what distinguishes us as a biological species from other biological species. I was talking about what makes us distinctive as the subject for an independent discipline other than biology. If it were a matter of the hand, the larynx, or even the brain itself, then we should be dealt with by biology. But I believe that there is an emergent phenomenon that is a separate level of reality that no other science can explain: culture. Or—*bekos*!

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PRESS: I am supposed to sum up our experience. We have done a lot of things: we have had debates about the state of social science, whether conclusive or inconclusive; this is itself, I suppose, still up for debate. Is it possible for the study of humanity to be scientific in the same way that the study of physical reality, or biological reality, is scientific? In that case, the dialogues that are held amongst social scientists wouldn't have to be simply for social scientists, and they wouldn't have to be the janitorial work for society. They could, in fact, be productive and increase our body of knowledge.

I wanted to add something touching about the conference, which would cause everyone to leave in tears of joy, knowing they had participated in something truly sublime. Unfortunately, I am only a student, and I don't expect to move my teachers, so I can only say what this conference means to me.

As a student, the question that is foremost in my mind, and one we have not addressed much over the course of this conference is: why study culture? Why engage in scientific study at all? I wrote this before Professor Greenfeld made her last comments, so . . . I apologize; there was some reference to this question, but: Why engage in scientific study at all?

I started answering this question for myself as an English major, when I began to study Shakespeare. The power of his work, the passions of his characters, the complexity of his language, opened my

eyes to the beauty of imagination. It is this beauty, this attraction to thinking, to creating, that moves the writer and the philosopher, and it is the same power that moves the scientist.

In so many of the photographs—like those along the wall—of Albert Einstein, there is a characteristic spark in his eye. Often, such an expression is referred to as a childlike quality. But I don't think that this does Albert Einstein any justice. This spark is the spark of imagination, of the enjoyment of thought, of the belief that it is not the attainment of ultimate truth, but the pursuit of attainable truth and the practice of imagination, that make life truly enjoyable and worth living. As scientists of culture, we not only practice, but we study this most beautiful and powerful of human qualities. And in coming to understand this, we may have indeed participated in something truly sublime.