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SIDNEY W. MINTZ LECTURE FOR 2002

Anthropology, Sociology, and Other Dubious Disciplines[1]

by Immanuel Wallerstein

The social construction of the disciplines as intellectual arenas that was made in the 19th century has outlived its usefulness and is today a major obstacle to serious intellectual work. Although the institutional framework of the disciplines remains strong, there are cracks in the structures of knowledge that make them less solid than most participants imagine. If the social sciences are to perform the social task demanded of them—providing wise counsel on the problems of the present—it is time that we harvested the richness of each discipline for use in their reconstruction. Some possible foundation stones for a reconstructed arena that might be called the historical social sciences are here suggested.

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The so-called disciplines are actually three things simultaneously. They are, of course, intellectual categories—modes of asserting that there exists a defined field of study with some kind of boundaries, however disputed or fuzzy, and some agreed-upon modes of legitimate research. In this sense they are social constructs whose origins can be located in the dynamics of the historical system within which they took form and whose definitions (usually asserted or assumed to be eternal) may in fact change over time.

The disciplines are in addition institutional structures that since the late 19th century have taken on ever more elaborate forms. There are departments in universities with disciplinary names. Students pursue degrees in specific disciplines, and professors have disciplinary titles. There are scholarly journals with disciplinary names. There are library categories, publishers' lists, and bookstore shelvings with disciplinary names. There are prizes and lecture series with disciplinary names. There are national and international associations of scholars with disciplinary names. The disciplines as institutions seem to be everywhere.

Finally, the disciplines are cultures. The scholars who claim membership in a disciplinary grouping share for the most part certain experiences and exposures. They have often read the same "classic" books. They participate in well-known traditional debates that are often different from those of neighboring disciplines. The disciplines seem to favor certain styles of scholarship over others, and members are rewarded for using the appropriate style. And while the culture can and does change over time, at any given time there are modes of presentation that are more likely to be appreciated by those in one discipline than by those in another. For example, historians are taught to favor primary sources over secondary sources and therefore to admire archival work. Archival work is not really an important activity in many other social science disciplines. Indeed, the anthropologist who restricted fieldwork to culling what is in archives would be unlikely to find a very friendly reception within the disciplinary camp. I think of these attitudes as cultural prejudices that are difficult to justify intellectually but strongly rooted and that operate in the real world of interaction among scholars.

As I am making my arguments within the framework of a rubric entitled a "lecture in anthropology," I thought it would be appropriate to indulge myself in what I think of, perhaps wrongly, as one of the prejudices of anthropologists. Similarly to historians but in contrast to most other social scientists, anthropologists do not think it amiss to begin an analysis with anecdotal material, snippets of the microworlds in which we all live. And since this occasion is the Sidney W. Mintz Lecture, I shall begin with an anecdote about Sidney W. Mintz.

In the founding year of the Fernand Braudel Center, I invited Sid Mintz to come to Binghamton on February 2, 1977, and talk to a faculty seminar that met under our auspices. He agreed to come. However, I went farther. I gave him the title of his talk: "Was the Plantation Slave a Proletarian?" He graciously agreed to talk on that spe-

1. This paper was delivered, as the 2002 Sidney W. Mintz Lecture, to the Department of Anthropology of The Johns Hopkins University on November 13, 2002.

cific subject, and we later published the talk in *Review* (Mintz 1978).[2] What he did was to survey the successive labor processes on Caribbean plantations over several centuries and write a thoughtful, reflective article on the limitations of the traditional ways in which the terms "slave" and "proletarian" were defined "in isolation" from each other. His response to the actual question was nonetheless a cautious one.

There are two things to note about this. First of all, I was violating a rather strong academic norm. One may suggest to an invited speaker a thematic area, but it is not considered appropriate to dictate the specific title. Second, this is not a question one normally poses to anthropologists and even less one that anthropologists have often posed to themselves. Can you imagine Malinowski or Lucy Mair answering it? It was bad enough, they might have thought, that this bizarre Mintz type had actually believed that studying plantation slavery was a legitimate task for anthropologists, but to use the term "proletarian" was surely going too far. It was not a term that one normally found in the canonic texts. Economists (certain economists) might use it; historians too, and maybe sociologists. But anthropologists? It implied crossing the line between the West and the rest. And if this line seems now to have lost its salience somewhat in the anthropological community (but has it really?), that was not yet true in 1977.

My second anecdote is briefer. It concerns Hugh Gusterson, who teaches anthropology at MIT. In an interview in the *New York Times*, Gusterson responds to the question how he had come to study the folkways and mores of nuclear weapons scientists. He concludes his response by saying: "In 1984, it was unusual to be doing fieldwork in your own culture. If you did it at all, you studied down—ghetto residents, welfare mothers. Nowadays, there's a fast-growing field, the anthropology of science" (Dreifus 2002:21).

My third anecdote concerns a historian. In a review of a recent book by Richard D. E. Burton on violence in Parisian political life between 1789 and 1945, David A. Bell (2002:19) makes the following criticism: "But by posing as an anthropologist—the scientist who stands to the side taking notes as the natives slaughter each other—he also falls into a trap that has ensnared many others: failing to take seriously the reasons for which his subjects believed they were fighting and dying."

It is always revealing, if sometimes disconcerting, to know how your colleagues in neighboring departments view you. I shall not take sides in this internecine sniping amongst the social sciences, but clearly Bell is referring to the different tonalities of the cultures of the two communities, that of the anthropologists and that of the historians. I believe the issue of "taking notes as the natives slaughter each other" has recently been the focus of a rather passionate debate within the American Anthropological Association, one that has managed to seep through into the nonscholarly media.

All of my anecdotes concern disciplines as disciplines. What should they embrace as subject matter? How should we approach the subject matter? Do the lines matter, and, if so, for what and for whom? Let me start by making my basic position clear. I believe that the social construction of the disciplines as intellectual arenas that was made in the 19th century has outlived its usefulness and is today a major obstacle to serious intellectual work. Further, I believe that the institutional framework of the disciplines remains very strong, although there are important crevices in the overall structures of knowledge—crevices that are visible at the moment only to those who look for them—that render the solidity of these institutions far less certain than most participants imagine. And finally, I believe that there is richness in each of the disciplinary cultures that should be harvested, stripped of its chaff, and combined (or at least utilized) in a reconstruction of the social sciences. Let me deal with these three assertions successively.

The Intellectual Justification of the Disciplines

The first chapter of the report of an international commission that I chaired, the Gulbenkian Commission on the Restructuring of the Social Sciences (1996; see also Wallerstein 1995),[3] deals with "the historical construction of the social sciences, from the eighteenth century to 1945." In it we argued that the intellectual lines of the surviving disciplines (for one must think of disciplinary names as having survived a culling process that went on for over a century) hinged around three axes: the past (history) versus the present (economics, political science, and sociology), the West (the previous four) and the rest (anthropology and Oriental studies), and the structuring of the nomothetic Western present around the liberal distinction of the market (economics), the state (political science), and civil society (sociology).

Because this is 2002, you can all see the limitations of the presumed axes of distinction, and you are well aware that massive numbers of social scientists have begun in the past three decades to disregard these lines in the sand. Furthermore, you are aware that many persons have sought to modify the intellectual premises of the various disciplines in order to take these realities into account and to transform what might have been thought of as academic poaching into legitimate activities. But I can assure you that, when I was a graduate student (which is only 50 years ago), these 19th-century boundaries were not merely in place but very actively asserted and defended in all the disciplines.

What happened? Very simple: the world changed. The United States became a hegemonic power with global responsibilities. The Third World became a political

2. Mintz inserted an opening footnote: "I am grateful to Professor Wallerstein for the opportunity to air my views and, indeed, for the choice of topic to which he asked me to address myself."

3. The anthropologist member of the commission was Michel-Rolph Trouillot.

force. And there was a massive expansion worldwide of university education, with a consequent massive increase in the number of social scientists doing research and writing books. The first two changes meant that the separation into disciplines for the West and disciplines for the rest became untenable, and the third change led to a quest for originality via academic poaching. These days, as you also surely know, the names of the papers at annual meetings of social science associations are extremely similar, except for the addition of the heading "anthropology of" or "sociology of" or "history of" to the same substantive phrase.

Do these papers given at different disciplinary conferences read differently? Up to a point, they do, and this has to do with the cultures of the disciplines. But they read less differently than one might think, and certainly a social scientist coming from Mars might wonder whether the degree of difference was worth the fuss. Therefore I want to play with the following quixotic idea. Suppose that we merged all the existing social science disciplines into one gigantic new faculty called the "historical social sciences."[4] Now, when the fairy godmother had left the room and we found that this miracle had occurred, we would immediately feel that this structure was too big and bulky for our own good. Many of us (perhaps most of us) already find the existing departments too diffuse. A merger would compound the problem geometrically. But of course we know what would happen. People would create corners in which they felt comfortable, and sooner or later we would get new subdivisions, perhaps new departments. My guess is that, if this happened, the new departments would probably have quite different names from any we now know. This is what happened when zoology and botany merged into a single department of biology somewhere around 1945–55. We now have many, many subdepartments or specialties within biology, but none to my knowledge is called botany or zoology.

Let us speculate together on what the lines of intellectual division really are in world social science at the present time. I think that there exist three main groupings of scholars. There is clearly a large camp of persons who hold onto the classic nomothetic vision—who wish to construct the most general laws possible about social behavior via quasi-experimental designs, using data that are presumably replicable and on the whole as quantitative as possible. They dominate departments of economics (in the United States at least, but not only) and also, increasingly, departments of political science. They are strong in departments of sociology and geography. They can be found as well, albeit in smaller numbers, in departments of history and anthropology. These persons share a lot of fundamental premises and even methodological preferences. For example, methodological individualism is very popular in this camp. They talk to each other already, and they might be happier to do that full-time.

There is another camp that is in many ways heir to the idiographic tradition. It favors dissecting the particular and the different. This is not a question of scale; although many of these people greatly prefer to deal with small-scale phenomena, some of them are quite willing to venture into dissecting rather large-scale ones. The point is that their backs go up any time one suggests uniformities. As a result, they are not likely to seek out quantitative data. They don't all necessarily reject such data in every instance—it's a question of what you do with them—but they nonetheless use mostly so-called qualitative data. They favor close, almost textual analyses. They empathize with the objects of their study, and they denounce sympathizing with them because sympathy is an expression of power. Almost by definition, they talk to each other primarily about what they dislike about what people in the other camps do. But when they present their own work, they find a lot of resistance even in their own camp. They are a quarrelsome bunch. Still, surrounded by nomothetists, they might wish to escape into their own organizational universe. These people are primarily to be found in departments of anthropology and history and to an increasing extent in sociology. There are in addition some political scientists, some geographers, and even a few economists to add to the aggregation.

And then there are people who feel comfortable in neither of these camps. These are the people who do not deny that they wish to construct grand narratives in order to deal with what they think of as complex social phenomena. Quite the contrary; they are proud of it. In terms of data, they are catholic in their tastes, using quantitative or qualitative data as they find them available and plausible. In the construction of these grand narratives, however empirical they are in their practice and their preferences, they abut on larger philosophical questions, and some of them are quite willing to enter into dialogue with those who technically define themselves as philosophers. They also abut on large political questions, and some of them enter into dialogue with those political scientists who call themselves specialists in international relations. This group is found all over the place—in history, in sociology, in anthropology, in geography, in economics (especially of course economic history), and in political science—but always as a minority. They too talk to each other already, perhaps even more than members of the other groups—this being a reflex, perhaps, of their belief that they are a persecuted minority.

I would guess that social scientists, left to themselves in a remolded faculty of historical social sciences, might well create three such "disciplines" as intellectual constructs. And I suspect that such a configuration would be an enormous improvement over anything we have now or have had in the past. But will they be left to themselves?

4. I would not include psychology in the mix, for two good reasons. First, I think that the level of analysis is quite distinctive. Second, most psychologists would prefer to be called biological scientists rather than social scientists, and they would be right, in my judgment, given the kind of work that they are in fact doing.

The Institutional Framework of the Disciplines

Disciplines are organizations. They have their turfs, and they have no small number of members who will fight to the death to defend those turfs against quixotic ideas such as those I've just set forth or any others that seem to threaten the historic configurations in which they find themselves. No amount of purely intellectual argument is likely to sway the majority of the world's social scientists, since these people have "interests" to defend and probably the best way to do so is by preserving the status quo. They are quite ready to give verbal or even substantive support to multi-, inter-, or transdisciplinary projects/studies/even degrees. All these activities ultimately imply that the existing disciplines have specific and special sets of knowledge that can be pieced together to create a tapestry, if tapestry is what one wants. Ergo, they do not undermine the disciplines as organizations. Quite the contrary! They fortify them.

Let me identify what kinds of people defend the turf most. Of course, there is a certain element of ideological choice involved, but the issue is primarily generational. The young are sometimes audacious or at least inquisitive and perhaps impulsive. They have to be restrained from wandering off the reservation by the potential sanctions of their elders. The senior seniors are sometimes reflective, tired of the nonsense they and others have been spouting for oh, so many years, and it is hard to sanction them. But it is not hard to ignore them and ship them off into the never-never land of honorifics, where prestige substitutes for power. No, the villains are those 40–55 years of age who have become the full professors, chairs of departments, presidents of associations, members of national committees, the jury in awards activities. They have suffered the ignominies of being a junior professor (after the indignities of being a graduate student). They have worked hard to make their way up through the ranks. They have achieved, most often rightfully, reputations among their colleagues (not merely locally but nationally and internationally). Can you blame them for not wanting to cast all this aside by abolishing their formal positions and placing themselves in a new unstable boiling pot in which essentially they will have to fight their way forward again, and without the familiar tools that they have used so successfully? Of course not, and they won't do it. There may be a courageous fool or two, but not enough to make a difference. And remember, these are the people with the real power within the disciplinary organizations.

I have not the slightest expectation that the petty bourgeoisie will commit suicide, as Amilcar Cabral (otherwise the most astute of analysts) hoped would happen in movements of national liberation. They will fight such reforms in every possible way, and the young and the senior seniors will be no match for them. Nonetheless, the defenders of the status quo will probably lose, for they may meet their match.

First of all, the intellectual anomalies are mounting and becoming every day more visible, for one thing, to the general public. How many times have you read in the newspapers the complaint "What use are economists if they never predict correctly?" No matter that this may not be a reasonable complaint, it reflects a delegitimation of the work of social scientists. And in the end social science is dependent on being legitimated by the social system of which it is a part. Otherwise, there is no respect and no money, and recruitment will in consequence dry up. The fact is that, after 150 years of an amazing amount of work, world social science has much too little to show for itself and is unable to perform the social task that outsiders demand of it—providing wise counsel about solving what are considered to be the "problems" of the present.

This perceived failure will sooner or later become a source of major concern to those whose function it is to be the link between the university systems and other structures of knowledge and the larger social system and the money, power, and legitimacy that it confers on those systems and structures. These people are the administrators—the deans, the university presidents, and, in most countries, the ministers of education. Their job is not to preserve the organizational structure of the separate disciplines but to provide what is considered to be the optimal societal output in the production and reproduction of knowledge. It is a political job every bit as much as it is an intellectual one. We know that almost all such administrators are ex-scholars, most no longer able to devote themselves to serious new work or, often, to keep up with the work of others even in their immediate fields of specialization. Slowly, over the years, they have moved away from the chains that the disciplinary organizations have imposed on them.

Such administrators perceive the social sciences, on the whole, with unhappy eyes. The social sciences do not bring in a lot of money to the university, certainly not in comparison with the biological and physical sciences. The heyday of their legitimacy is over. Administrators are daily made aware of the degree to which disciplinary overlap exists. And yet, almost every week people come into their offices proposing new centers (almost always touted as interdisciplinary), whole new instructional programs, even new formal departments. And since many of these people are playing the game of outside offers, administrators far too often feel the need to yield and permit the creation of yet another epicycle of the social science astronomical map.

Meanwhile, these same administrators are beset by economic worries, and not short-term ones. Of course, the money they have at their disposal varies year by year with the rolling stock exchange, but the question is far larger than this. The world university system greatly expanded between 1945 and 1970. But this was a time when the world was economically flush. Some of us call this a Kondratieff A-phase. Ever since then we have been in a B-phase. The governments of the world have been less flush, but the universities have continued to expand. A larger percentage of high-school graduates throughout the world seeks to enter the universities every year. They do so because they think this will enhance their life chances, and quite often governments and entrepreneurs are happy to see them *not* enter the workforce yet, given the comparative surfeit of older workers. More students and less money equals chronic crisis. We have all been living through that, and I see no reason to expect this economic constraint to go away. True, we may have another A-phase, but we shall also see the further expansion of the world university system. For one thing, people are living longer and therefore working longer, and the authorities of the world-system will try even harder to keep young people out of the job market. Keeping them in the university system is a genuine social solution but an expensive one.

If I were a university administrator, I would look around to see if I could tighten the ship. One way would be to get professors to teach more to more students. This is what I call the "high-school-ization" of the universities, and it is going on apace. This of course makes some of the most prestigious professors seek to escape into permanent research institutions or even corporate research structures. From the point of view of the administrators, this is a prestige loss but a financial gain; they get rid of some of their most expensive professors. Another way would be to merge departments. Why not? They overlap. They don't teach enough. Students are confused by the present situation. A new department with a snazzy title might attract students and at the same time achieve economies of scale. I could even tout it as intellectual audacity. So when I say that the extremely strong organizational structure of the disciplines has cracks that are too little noticed, it is the potential intrusion of the administrators that I have primarily in mind.

Who knows? The administrators might do a wonderful job of reorganization. I have two fears, however. One is that they will be driven more by budgetary concerns than by purely intellectual ones. After all, administrators are paid not to determine the best way of defining the tasks of scholars but to find good people to be professors and therewith to create a socially useful product. The so-called best universities might be willing to sustain small, unpopular groupings that have some purely long-term intellectual justification, but there will never be many jobs for those who wish to teach Akkadian language and culture. And reconstructions that are driven by budgetary analyses will too often be following the fads of the moment or the poorly defined needs of the students' prospective employers.

The second concern that I have is that administrator-generated reconstructions will be done differently in each locality. Local situations are of course always very specific, and administrators do not have as strong as transnational organizational structure as scholars in the separate disciplines. The result could be, on a world scale, quite diffuse and militate against the emergence of the kinds of institutions that would facilitate the maintenance of world communities of scholars.

This all may well be unfair to administrators, since (as I am arguing) the scholar-teachers are not primed to do such a marvelous job themselves. The point is that we are heading into an era of chaos in the structures of the disciplines and, while I believe that order always emerges out of chaos (to echo Prigogine), the outcome is intrinsically uncertain (to take up another theme of Prigogine). We will not navigate this era well if we do not keep a sharp eye on what is actually happening.

Harvesting the Cultures of Social Science

Here I enter the most treacherous terrain. "Harvesting" is an agricultural metaphor, referring to various products of the soil that can be combined and transformed into useful products—food, clothing, and everything else we need in everyday life—and that may be better or less good products according to how well we perform the operations, within the constraints imposed by the conditions of the soil. Perhaps we ought rather to phrase the processes in terms of a different metaphor, that of the painter mixing his colors in order to produce a work of art on a canvas. I could then list for you my favorite colors and the combinations that I thought to be interesting or beautiful and then design my picture in the style I thought most meaningful to me, to you. The metaphor of the painter seems to give more autonomy to the subject—constrained, no doubt, but less constrained perhaps than the farmer by external realities over which he has no or little control. I do not want to get lost in metaphors but merely to indicate my uncertainty on this perennial issue of how much to emphasize agency or even how much agency is a real issue in analyzing the future of social science.

What I shall do therefore is to pick a series of cultural prejudices that I think work better than their alternatives and that I hope will serve as the foundation stones of the putatively reconstructed arena I am calling the historical social sciences. Let us start with the very name I am using for this new disciplinary construct. We cannot, I believe, talk about the real world in any way that is not based on a claim to science, by which I mean the assumption that the world is real and is potentially knowable (if only perhaps in part). Every word we use in speaking and writing involves a theory and a grand narrative. There is no way to escape this, however much we try or claim to do so. At the same time, there is no way to analyze or even describe the real world without being historical, by which I mean that the context of any given reality is constantly changing, evolving, and that statements of truth are no longer true the moment after they are uttered. The problem of social science (and probably of the natural sciences as well) is how to reconcile the search for structural continuities (laws or hypotheses, if you like) and constant historical change. In other words, the problem is to find modes of analysis or languages that can bridge this inherent contradiction of the process of knowing.

Stating the issue in this manner is a way of denying the usefulness of the *Methodenstreit*, of rejecting the claims of both nomothetists and idiographers, of saying that we are all condemned to be both simultaneously at all times and under all circumstances. Many, even most present-day social scientists will probably feel quite uncomfortable about this reality, for it violates the cultures within which they have been socialized. But we know that cultures can and do change—that they are malleable, if sometimes with difficulty. And I can hope that by 2052, at the 59th Sidney W. Mintz Lecture in Anthropology (although this last term may not survive the transformations), this *Aufhebung* will seem so natural that it will not even be thought necessary to advert to it.

In such a culture, what kind of work shall we be doing? Empirical work, largely, I hope, but of a certain kind. Let me start with what I think is social science's most pervasive failing: Much of what we do is an elaborate explanation of some dependent variable without any real empirical demonstration that the explicandum is in any sense real. It is all too easy to assume that a credible proposition is a reality. It is against this that Ranke insisted that history must concern itself only with *wie es eigentlich gewesen ist*. Paul Lazarsfeld (1949) long ago demonstrated that obvious facts are not so obvious once one actually tries to provide evidence for them, and the early ethnographers wrestled with imageries of strange, assertedly savage behavior that seemed to be quite different when seen close at hand. Ranke used his warning to argue that we must search for archival evidence. Lazarsfeld used his to argue the utility of public-opinion polling. The ethnographers used theirs to insist on participant observation. The solutions, it turned out, were many, all no doubt with their limitations. It is the recognition of the problem that is crucial.

Without a statement about a dependent variable that has been reasonably demonstrated empirically, there can be no analysis. This does not mean that the assertion is correct. There can never be a definitive fact of any kind. But between the definitive fact and the presumed but undemonstrated reality lies a wide range of possibilities, and it is in this murky middle ground—the world of what has probably really happened in the world—that the historical social sciences are called upon to work. Deductive models serve us ill. Common knowledge is at best a source of possibly correct perceptions and itself an object of study. This is why fieldwork (in the loosest, broadest sense of the term) is our eternal responsibility. Once we have something to explain, we need concepts, variables, and methods with which to explain it. And it is about concepts, variables, and methods that we have long been arguing with each other—arguing loudly and on the whole not very fruitfully.

We all use concepts, and we all have a bag of concepts in our mind, ones that we have learned in our continuing education from childhood on. Some of them are as mundane as "needs" and "interests," some as seemingly obvious as "society" and "culture," some as seemingly specific and "advanced" as "bourgeoisie" and "proletariat." They are all challenged by some people sometimes, but this doesn't stop others from invoking them. Therefore it is good to remember the admonition of Lucien Febvre (1962:481), writing about the concept of "civilization," that "it is never a waste of time to write the history of a word." This elementary truth, largely ignored, is what those devoted to deconstruction have tried to reinvent. We now have a whole Archiv für Begriffsgeschichte in Germany of which I would guess most social scientists are unaware.

Nor do most social scientists pay attention to the constraints of morphology. Listing multiple varieties of some phenomenon tends to be a sort of mindless empiricism. Morphologies are ways of creating preliminary order in the "blooming, buzzing confusion" of reality and in effect are hidden causal hypotheses. They tend to be worthless when there are too many categories; usually, three or four are the limit. This suggests that what social scientists need to do is to examine carefully and debate their philosophical, epistemological premises. They do not currently consider *Begriffsgeschichte* or the modes of constructing morphologies as a foundation stone of their own research or as a necessary part of graduate education. Here is where their scientism leads to distinctly unscientific outcomes, without even the awareness that this is happening.

As we move from concepts to variables, once again some simple truths are in order. To continue my metaphor, the prejudices of a minority need to be incorporated into the practice of all of us. First, virtually all statements should be made in the past tense. To make them in the present tense is to presume universality and eternal reality. The argument should not be made by grammatical sleight-of-hand. Anything that happened yesterday is in the past. Generalizations about what happened yesterday are about the past. This is perhaps a sensitive issue for some anthropologists (the famous "ethnographic present") and most mainstream economists and sociologists, but using the past tense serves as a constant reminder of the historicity of our analyses and the necessity for theoretical prudence.

I wish also to make a case for a culture of plurals. Most concepts are plural concepts—civilizations, cultures, economies, families, structures of knowledge, and so on. It is not that we cannot proffer a definition for a word and insist that what doesn't meet that definition should not be described by that term. But, as we know only too well, almost all conceptual terms are defined in multiple ways, and it is not very helpful to scholarly debate to assume away the debate by deduction from one's definition. Yet much of what we currently do is done in this way, and we are even rewarded for doing it and quite often penalized for not doing it. Failure to insist on a narrow definition is often pilloried as journalism, eclecticism, or deviation from the truth.

And along with the past tense and plurals comes the culture of multiple temporalities, multiple spatialities, and multiple TimeSpaces. The *Methodenstreit* that has governed most social science since the late 19th century has polarized our community into a battleground wherein we were all adjured to choose one side because the other side was false, or irrelevant, or worse. Not only was this forced conflict counterproductive but it led us to ignore the existence and importance of other temporalities and spatialities, including Braudel's *longue durée*, the necessary concept if reality is at once systemic and historical. What we need in our historical social science is to consider what our reality looks like within each of its possible temporalities and spatialities. And this is of course necessary whether we are analyzing a macrotopic such as the history of the modern world-system or a microtopic such as the introduction of some new element into the life stream of some remote village.

Whatever our object of investigation, we need a great deal more fluidity in our analyses as we move from one arena to another, from what we like to call the economy to what we like to call the polity to what we like to call the society or the culture. There is no *ceteris paribus*, for the other things are never equal. We may wish to ignore for a moment elements other than the immediate variables we are considering, since we may find it difficult to talk about everything at once. But we can never do this on the assumption that the surrounding variables do not impinge immediately on what we are studying. The whole lesson of the sciences of complexity is that if one changes the initial conditions ever so minutely the outcome may be radically different, whatever the truth of the equations we are using.

This then leads us to the question of methods and of methodologies. In my own education, I was taught that there was a radical distinction between what were called in the jargon of my teachers "small-m methods" and "big-m methods." Small-m methods were all those practical techniques that we use and that in the past were used to define disciplines: simulation, opinion polling, archival research, participant observation, and so on. The only attitude one can take to small-m methods is one of complete catholicity. They are simply methods of estimating, capturing reality. They are worth what they are worth in facing up to the ways in which the world makes it difficult for researchers to find out anything about the issues in which they are interested. Not only are none of these methods intrinsically better than others but no generally described research issue or site is necessarily and permanently linked with any one of them. We all need them all. They all have their virtues and their limitations. And graduate students would do well to become acquainted with the widest range of these small-m methods. Since I have been discussing them within the framework of cultural prejudices, I call for setting aside our prejudices. We will be the stronger for it.

But the real question is big-m methods. For example, should we trust only quantitative or only qualitative data? Here it is not a question of simply being eclectic but one of deciding what kind of data is valid. I have some simple rules that seem to me to cull our collective wisdom. It is clear that almost all our statements are quantitative, even when the statements use nothing more than "more" or "important" in their formulations. And it seems to me that it is always more interesting to be quantitatively more than less exact. It follows, then, that quantification is desirable whenever it is possible. But that "whenever" encompasses a big caveat. If one makes serious quantification an imperative and a priority, one risks ending up where the old joke sent us, looking for the lost watch under the lamppost because the light is better there.

But there is more to it than that. We have today a leading mathematician warning us that "the qualitative approach is not a mere stand-in for quantitative methods. It may lead to great theoretical advances, as in fluid dynamics. It has a significant advantage over quantitative methods, namely, stability" (Ekeland 1988:73). This goes against one of the main social-science arguments for quantification, that of reliability (or stability). And this has to do with what I would call premature quantification. We can only usefully quantify when we are fairly well advanced in the plausibility of our models and the strength of our data. Quantification comes in toward the end of a process, not at the beginning. Indeed, the beginning is preeminently the realm of ethnography and other nonquantitative modes of analysis. These techniques enable us, in a complex situation (and all social situations are complex situations), to tease out the issues and then explore the explanatory connections.

It is qualitative data, not quantitative data, that are simple. Simplicity, however, is not the goal of the scientific process but the starting point. Of course, one can start with simple statistical correlations as well. Complexification is the name of the game, and ever more complex is not necessarily ever more narrative. It may quite well be—and may even better be—more complicated equations, bringing in more and more variables in a controlled fashion.

It is only at this point of relative complexity that we can engage in real comparisons, ones that do not combine the investigated situation of the strange or complicated or exotic with the presumed truth of the situation we think we know well. Arnold Feldman, one of the early sociologists who studied what were in his time called "underdeveloped countries," used to say that, whenever he gave a talk on the patterns he discerned in his work, there was sure to be someone in the audience who would rise and say, "but not in Pago Pago." It may or may not have been true that what Feldman had recounted was not true in Pago Pago, but what is the relevance of this caution? The critic may have intended to deny the possibility that patterns exist or can ever exist. But then why study Pago Pago? Is it butterfly collecting? Or the critic may have intended to say that Feldman's formulae were too simple and needed further complexification if they were to be useful. Or perhaps the critic merely felt that the organizers should have invited him to lecture rather than Feldman. Criticism is a crucial tool in the historical social sciences, but mindless criticism is not.

And that brings me to narratives. Narratives are an admirably understandable and attractive way of communicating perceptions of reality. To be sure, even the harshest set of differential equations is a form of narrative, though not the most palatable form. Recently there has been much attacking of macronarratives. I suppose these critics think that what they produce is micronarratives and micro is better than macro. But of course the micro is a setting in which the macro manifests itself and one that can never be understood without reference to the macrosetting. There are in the end no narratives that are not macronarratives. The only question we have before us is whether the macronarrative we are putting forward is defensible.

The culture of the historical social sciences that I envisage will not be against theorizing or theories, but it will be cautious about premature closures. Indeed, the breadth of its data, of its methods, of its linkages to the rest of the world of knowledge will be its principal characteristic. Vigorous analysis in a climate of tolerant and skeptical debate will be what will aid it most. Of course, I am also assuming that in the next 50 years we shall be overcoming the relatively recent (only two centuries old) but deeply rooted divorce between philosophy and science—the so-called two cultures—and setting out on the path of constructing a singular epistemology for all knowledge. In this scenario, a reinvigorated social science, one that is both structural and historical, can provide the crucial link between the domains we classify as the natural sciences and those we classify as the humanities.

The adventure of the historical social sciences is still in its infancy. The possibilities of enabling us to make substantively rational choices in an intrinsically uncertain world lie before us and give us cause for hope amidst what are now the gloomy times of a historical transition from our world-system to the next one—a transition that is necessarily occurring in the structures of knowledge as well. Let us at least try seriously to mend our collective ways and to search for more useful paths. Let us make our disciplines less dubious.

Comments

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Wallerstein's assessment of the current state of the social sciences and his call for their reconstruction in terms of what he calls the historical social sciences would find widespread acceptance among many social scientists around the world. Here I would like to highlight how the assessment of the dubious nature of the social sciences and the prospects for reconstruction depend on one's position in the global division of labour in the social sciences.

As Wallerstein puts it, the problem of the social sciences is the reconciliation of the search for structural continuities with continuous historical change. This would require rejecting the claims of both the nomotheticists and the idiographers concerning the understanding of concepts, variables, and methods in the social sciences. But this is not the only reconciliation that is needed. There is the need to reconcile the cultural specificity of concepts with the self-understanding of the peoples being studied. There is something of a cultural divide in the social sciences that can be seen in the very concepts employed by the various disciplines. The nature of this cultural divide is as follows: Many concepts in the social sciences originate from a Greco-Roman, Latin Christian, and European tradition. This does not pose a problem, as Wallerstein recognizes, if these concepts become universal or plural. A great many concepts are, however, passed off as universal when in fact they derive their characteristics from a particular cultural tradition. This wreaks havoc on our understanding of social phenomena. For example, while "religion" is presented as a universal concept, the understanding of what makes up religion in phenomenological, historical, and sociological terms is often derived from Christianity, resulting in what Joachim Matthes (2000:98), referring to Islam, calls the " 'hidden' cultural *Christianisation*" of the Muslim world since it started to think of Islam as a "religion." This raises the interesting question of the extent to which "religions" such as Buddhism, Hinduism, and Islam have been intellectually reconstructed after the image of Christianity because of the concepts employed by the disciplines that study religions. Such concepts include "church," "sect," "denomination," "secularization," and "religion" itself. A case in point is a table presenting statistics on church membership in the United Kingdom in Anthony Giddens's *Sociology* (2001:549, table 17.3). However, the religions included are not just Christianity but Hinduism, Islam, Judaism, Sikhism, and others, giving the impression that the temple, mosque, and synagogue are all, sociologically speaking, "churches." The term "church" is generalized without the concept's being rendered universal or plural. What should be considered is the possibility of other concepts of religious organization that can be derived from these other belief systems. As long as the study of religion does not look at its objects as potential sources of concepts rather than just data, it will remain backward, playing down the objects' conceptualization of themselves and denying the culturally plural origins of ideas in the social sciences.

I agree with Wallerstein's case for a culture of plural concepts but would insist that the acculturation of concepts be made one of the major concerns of any effort to reconstruct the social sciences. In many parts of the world collectively termed "the Third World," "the South," or "developing societies," social science practitioners and institutions are highly dependent on their counterparts in the United States, Britain, France, and a few other nations for ideas, theories, and concepts, technologies of education, and aid and investment in education. This state of what we might call academic dependency is perpetuated by certain features of the global division of labour in the social sciences: the division between theoretical and empirical intellectual labour, the division between other-country studies and own-country studies, and the division between comparative and single-case studies. As long as academics in the

South continue to do predominantly empirical work that is largely confined to single cases in their own countries or localities and therefore lacks a comparative perspective, the prospects for theoretical or conceptual innovation are bleak. Even if the social sciences were to be reconfigured as historical social sciences and to extricate themselves from petty squabbles between the nomotheticists and idiographers, the global division of labour as I have described it would remain intact and culturally plural concepts and theories would remain the exception. Therefore, the reconstruction of the social sciences must simultaneously be the dismantling of the current global division of labour in the social sciences.

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17 IV 03

By and large, I support Wallerstein's agenda. A number of the exhortations are very general, and I would have to see "multiple temporalities and spatialities" and the "plurality of concepts" specified, but most other points—the importance of empirical backing, the value of using the past tense, the need for "small-m" methods employed in nonideological ways, the dangers of premature quantification, and the virtues of maximal quantification—are commendable. The question, however, is whether these points benefit from a merger of the social sciences. Surely we will all profit from tearing down the fences that hinder the inquisitive mind from foraging freely. Yet suppose that the "historical social sciences" had been instituted in my university. Surrounded by throngs of new colleagues, many of them interesting people working on interesting topics, I would consider the previous separation odd indeed. On getting to know my new colleagues better, however, I would find a good number whose inclinations to sweeping armchair theorizing, tenacious adherence to a specific master concept or method, and reductionist modeling of human nature and society's workings I considered unhealthy. And among the others I would find a considerable number who were simply not very interested in societies other than the Euro-American ones they studied. Therefore, in forming subgroups and specializations at some point I would still gravitate to those who shared my belief in the value of in-depth studies of exemplary cases, employed a systems perspective, and took a broad and comparative interest in the human condition. While those I would feel drawn to would include practitioners of all the former disciplines and certainly also many sociologists, the number of anthropologists would be large. Chances are that in the end I would find myself more or less where I was before.

Wallerstein suggests that there are more sensible ways of subdividing, for instance, along the three grand theoretical orientations or, rather, scientific temperaments that he outlines. Here he appears to be thinking of conditions in sociology, where the followers of, say, game theory, social network analysis, Luhmannian systems theory, or new institutional economics may pass entire careers without being forced to leave their favourite paradigms. Taking anthropological research inspired by Wallerstein's own work as an example, however, I believe that studies based on such diverse approaches as those of Bradley et al. (1990), Gewertz and Errington (1991), and (to cover the middle range) Robinson (1986) still belong together. There should be scholars who try to keep abreast of all this work and integrate it into their own perspectives. In anthropology, research experience on a variety of topics and/or societies, approached from a variety of positions on the nomothetic-idiographic continuum, continues to be a prerequisite for a successful career, and this has kept anthropologists of different theoretical persuasions able to talk to one another and use each other's results with a realistic sense of their possible worth and limitations.

Disciplines should be thematically, not methodologically or paradigmatically, defined; "world systems studies" would appear a more reasonable discipline to me than "idiographic studies." New insights and societal changes have already led to the establishment of a number of such "studies" (cultural, gender, gay, lesbian, etc.), demonstrating that the 19th-century framework of disciplines is not a straitjacket. Ideas also travel rather freely. I therefore wonder whether the classic disciplinary boundaries really are a "major obstacle." The potential for constructive exchange across disciplines is not, of course, exhausted. Ceasing to ignore each other's methods would be an important first step, as Wallerstein is right to emphasize. Certainly, too, the social sciences are often not well organized. The customary faculty partners of social/cultural anthropology in the United States, for example—physical anthropology, archaeology, and linguistics—are not necessarily closer to what most of the discipline's practitioners are currently doing than, for example, sociology or history. Rearranging the existing disciplines could therefore be fruitful.

To jump on the merger bandwagon, however, I would need a clearer view of the course that would be followed thereafter, given that—as Wallerstein points out and a glance at the corporate world confirms—downsizing is the only consequence that is dead-certain. After all, the classic disciplines are established brands, known by many and not as compromised as Wallerstein argues. Such disciplinary capital should not be wasted as the balkanization that Wallerstein anticipates would be sure to do. In any event, there is nothing to stop us from thinking, talking, and publishing across the borders. I take Wallerstein's lecture as valuable encouragement in this regard.

Two asides trouble me somewhat. First, Wallerstein presents Bell's insinuation that anthropologists tend to fail to take their informants' reasons seriously in a seemingly neutral way. For most anthropologists, however, taking people's reasons seriously is the whole point of doing ethnographic fieldwork, and they tend to detach themselves far less from the people they study than other social scientists. Second, the "passionate debate inside the American Anthropological Association" was not about whether "taking notes as the natives slaughter each other" is a good thing but about whether specific anthropologists actually performed specific unethical acts (which remains contested [see the report of the American Anthropological Association's El Dorado Task Force at http://www.aaanet.org/edtf/index.htm]).

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One of the many useful points Wallerstein makes is that the famous categories of the *Methodenstreit* invoke a false opposition. We are not condemned to choose between idiographic and nomothetic perspectives but can use both at once.

Since this diagnosis is sound, it is somewhat surprising that Wallerstein's ultimate vision of how the social sciences might be reorganized incorporates a restatement of the *Methodenstreit* categories. There will always be those pursuing idiographic and nomothetic agendas, he suggests, but let us make sure there is also a place for those who seek to overcome them through the writing of grand historical narratives. Indeed, I hope such a place exists, but it is not the only valuable intellectual project or style that spans the division of particular and universal.

Ethnography, for a start, need not be a pursuit of the radically particular; instead, it can be precisely an engagement with comparison and the development of generalizations. The comparisons will ideally be attentive to context, and the generalizations will commonly be limited. Context may include the kind of macrohistorical situations Wallerstein's approach would help ethnographers to identify but might also situate particular cases in relation to a range of other broader phenomena from languages and religions to geography and ecology. The limits on generalization may be clearly recognized or left implicit, to be raised by the kind of statement Wallerstein characterizes as "but not in Pago Pago." What is important is that asking questions like how matrilineal descent organization shapes the accumulation of property is inherently generalizing—even when one starts by asking it only in one place—and inherently open to debate over the scope of application of any relationship identified. This is a crucial reason anthropologists master a range of major ethnographies.

But of course ethnography, at least as most frequently practiced, cannot reveal everything, and to claim it as an exclusive research style is to put on blinders. The upheaval wrought in anthropology a generation (or two) ago by starting to take history, state structures, colonialism, and capitalism more seriously is evidence of this (but not an argument for consigning ethnography to the purely idiographic). Context matters, in other words, and not only immediate context. Many of the most important intellectual disputes are about just what context is most relevant for understanding specific phenomena. Asking how valley states generally relate to mountain "tribes" (possibly even producing them by driving some people from richer to poorer land) thus transforms the way one may look at social organization in any village or region; relating this to ecology and agricultural technologies further interrelates the general and the particular. One can also situate this within the histories of particular states or regions and within more global histories that shape the patterns of local and regional relations. As I think Wallerstein would agree, the issue is not so much scale as the need to consider the multiple relevant contexts.

This points back to why Wallerstein's tripartite division of the social sciences worries me. Many of the most productive questions arise from efforts to relate the seemingly particular to the apparently general. I would hope that any future organization of the social sciences would perpetuate such arguments and provoke them in newly creative ways by forcing researchers with different perspectives and characteristic styles to confront each other's work more often. Put another way, part of the problem with disciplines is precisely that they have developed characteristic styles of work—the "methods" into which students are ritually initiated—and these minimize the frequency of intellectually exciting challenges to conventional opinion and seemingly obvious observations. Economists usually confront only other economists and, at least in the United States, only ones who accept a variety of background assumptions from the neoclassical model (and from the high value placed on mathematicization). Anthropologists too often confront only others who give primacy to face-to-face relationships—understanding language from speech, all social relationships from interpersonal ones, all institutions from the local manifestations. Old ideals of the "four fields" might be trotted out at this point to reassure anthropologists that the discipline still tries to study humanity as a whole, but it is hard to make a realistic case either that the interrelationships among the fields are as the ideology suggests or that the four exhaust the relevant range of approaches. Surely sociology, political science, and economics are as important for a cultural anthropologist (let alone a social anthropologist) as physical anthropology or archaeology.

Wallerstein is right that disciplines are more matters of turf, careerism, and institutional inertia than intellectual principle. He is also right that external pressures on the disciplinary structure are growing. It makes sense for social scientists to seek out a better structure ourselves rather than only defend the existing one. But I hope that whatever develops will be not merely a placement of like with like but a basis for creating fruitful confrontations among different styles of research. These could happen in projects defined around places, theories, or clusters of intellectual problems that are illuminated by several styles of research.

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Wallerstein is to be commended for his continuing efforts to confront social scientists with the stark predicaments we face in organizing our collective activities, and he is certainly right to point here to the uneven developments across the intellectual, institutional, and cultural axes that organize his discussion. Like him, I am an optimist about developments in epistemology that will bring each of us, no matter what our particular methodological commitments, into broader conversations across what once seemed like unbreachable epistemological divides.[1] And, like him, I am struck by the impediments to intellectual work and education posed by the institutional structures and, to a lesser degree, the cultures of the disciplines. His utopian proposal to set the faculty free to regroup under new disciplinary banners thus has a certain appeal.

Yet I am given to wonder whether either eliminating or organizing disciplines is a promising solution. Consider the problem as one of social organization: Bureaucracy as a mode of organization is historically challenged these days, and there are many scholarly analyses that suggest the reasons and implications. Daniel Bell's *The Coming of Postindustrial Society* already pointed in 1973 to social relationships that would play out as "games between people" rather than performance under bureaucratic regimens. At the beginning of the 21st century, emergent social theories are predicated on concepts of networks and relationality rather than bureaucracy. From quite a different perspective, poststructuralists would tell us, the attempt to create solid categories is always going to produce anomalies that don't fit the classification scheme. As Derrida would have it, any structure will require a supplement. Transferred from the realm of text per se to the textual structurations of disciplines, these perspectives suggest the limitations to any solution predicated on trying to align disciplines with intellectual coherence or cultural coherence. As Guenther Roth remarked to me already in the 1970s, sociology (in his example) is just a convenient umbrella where scholars come to stand to keep out of the rain.

These brief allusions to the travails of bureaucracy and classification and the implications of the network metaphor suggest that we need to recast the organization of universities in fundamental ways. Yet when I look at the balkanization that can occur in the humanities, beset as they are by an erosion of coherent rationales of subject matter and a proliferation of programs, the administrative and cultural benefits of relatively eclectic but somehow strong academic units ("departments") in the conventional social sciences looks good by comparison. Certainly their boundaries can and should shift as intellectual rationales change. But if Wallerstein is generally right in his assertions, neither methods nor subject matter can be the basis for defining disciplines. For all the tensions, then, I think there is something healthy about circumstances that encourage people of diverse methodological persuasions and substantive interests to "keep watch over one another" within academic units —call them departments if you will—fighting over hiring, promotions, and tenure.

How, then, to respond to the challenges and possibilities of scholarly networking under postclassificatory circumstances? I will not look at the *longue durée*, but in the very near term, several precepts based on experiences at the University of California, Davis, seem worth elaborating and considering:

1. Topical, theoretical, and methodological course clusters should be grouped and coordinated across academic units, not within them.
2. Undergraduate majors may (typically) be administered within academic units, but any given department might consider offering multiple majors.
3. Undergraduate major course requirements typically should group courses drawn from diverse academic units to form coherent packages, and the names for these majors need not be tied to academic unit names.
4. Doctoral graduate education should be based on "graduate group" degree programs that are not necessarily tied to academic unit names and are subject to periodic review and reconstitution.
5. Graduate students should be encouraged to complete at least one minor program that brings faculty and students together from diverse graduate degree programs according to shared substantive, theoretical, and methodological interests.
6. Graduate course offerings should be designed to encourage enrollment by students and participation by faculty from diverse degree programs.
7. Doctoral substantive examination and dissertation committees generally should follow the model long practiced at some institutions of allowing committee composition in ways not restricted by program or disciplinary boundaries.

This very brief sketch can be only suggestive, but I hope that it advances my general point that academic reform in the human sciences ought to proceed by encouraging new organizational and programmatic solutions that depend neither upon old binary efforts at classifying nor on efforts to rearrange or transcend disciplines.

1. In my account, affinities across seemingly disjoined methodologies constitute the domain of the human sciences as one of "integrated disparity" (see Hall 1999:chap. 9).

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Cultural-social anthropology is of course unremittingly comparative in its approach. Considering it as a "discipline" in all three senses of the term that Wallerstein outlines should, I believe, seem familiar, although in contrary ways, to someone (like myself) brought up in the Hindu (Brahmanical) intellectual tradition and trained as a social anthropologist in the British tradition in the 1950s.

The exact equivalent of the term "discipline" in Sanskrit is *shastra*. Standard dictionaries gloss it as a noun connoting (1) command, precept, rule, (2) teaching, instruction, and (3) a scholarly (religious or secular) treatise. Thus, we have the *shastra* or disciplines of *dharma* (moral law), *artha* (rational pursuit of material ends), and *natya* (dance and music). Corresponding to each *shastra* is a corpus of texts often referred to by a generic name and incorporating the work of one or more authors although attributed to a single authority: Manu's *Manavdharma Shastra* (ca. 100–300 C.E.), Kautilya's *Artha-Shastra* (ca. 400 B.C.E.–100 C.E.), and Bharata's *Natya-Shastra* (ca. 100 B.C.E.–100 C.E.) are well-known examples. These disciplines do not, however, stand for autonomous fields of thought and action. *Dharma*, *artha*, and *kama* (aesthetic/sexual enjoyment) as *purushartha* (goals of action/value orientations) are interdependent although arranged hierarchically, so that the pursuit of a lower goal/value occurs within the ambit of that/those placed above it. Stated conversely, a higher goal/value includes those ranked below it without losing its own priority. One can of course opt out of this goal/value framework and of social life altogether through renunciation (*sannyasa*, "giving up everything"). In short, the concept of *purushartha* is holistic, and the disciplines that ensue from it would not readily translate into a structure of autonomous faculties in an institution of learning. Obviously, the Brahmanical tradition did not entertain the notion of specialized discourses, for this would have amounted to fragmentation of knowledge, which was anathema.

Post-graduate departments of social sciences at Indian universities emerged early in the 20th century. The University of Lucknow (in North India) had departments of history, political science, and economics and sociology from its beginnings in 1921. The founding professor and head of the last-named department, Radhakamal Mukerjee, who shaped its teaching and research programmes, had himself come to economics from history and English literature (at Calcutta University). He sought to hold together economics, sociology, and cultural anthropology, lecturing in all three areas (see Mukerjee 1925). Around the time of his retirement in 1951, he had to witness the trifurcation of his department into independent departments of economics, anthropology, and sociology. Even so, he reaffirmed in 1956 that "a true general theory of society is the corpus of theories, laws, and explanations of social relations and structures derived from all the social sciences. . . . For society is not divisible. Only the social sciences . . . are fractionalized" (p. 9).

So far as anthropology in India is concerned, the notion that it studied "other cultures" was carried over from British and American universities, and therefore Indian anthropologists studied tribal, forest-dwelling communities. When I inquired in 1953 if I could study aspects of my own (non-tribal) community (the Kashmiri Pandits [see Madan 2002]), there was at first some reluctance on the part of my teachers (at Lucknow) to allow it, but eventually they agreed. An unexpected source of support was the British social anthropologist S. F. Nadel, then at the Australian National University, who agreed to supervise my research in the area of marriage and kinship on the basis of fieldwork in a cluster of villages; I myself had grown up in an urban neighbourhood. In the course of time, I formulated my position as one of "distanciation," of treating the familiar as strange—in short, of treating the "other" as a conceptual rather than an empirical category (see Madan 1994:chaps. 6–8).

An important influence in the closing if not the abolition of the gap between social anthropology and sociology in India was M. N. Srinivas (1916–1999), who held doctorates in both sociology (Bombay) and social anthropology (Oxford). He resolutely opposed the ourselves-others dichotomy as colonial baggage (see Srinivas 2002), but his critics have argued that he only succeeded in reducing sociology to social anthropology. It is true that neither he nor many of his students engaged in large-scale studies or handled quantitative data. Indian universities continue to treat sociology and anthropology as separate disciplines.

The point of these comments is that the "putatively reconstructed arena of historical social sciences" that Wallerstein argues for may be more readily realizable in certain intellectual settings, where it may find affinities with an earlier holism, than in others, but its relevance is today universal. The way to its constitution lies in the bridging of a series of breaches, beginning with the dichotomy of nomothetic and idiographic sciences that generated Dilthey's famous nightmare about the academy torn asunder. This would be not an exercise in mechanical eclecticism but the serious effort of making our disciplines (as Wallerstein recommends) less "dubious."

Having said this, I must point out that the task of dismantling disciplinary walls is not going to be easy. Pitted against it are rigid institutional structures, set habits of mind, a disciplinary status system that privileges methodological specialisms, scholarly egos, etc. Those more forthcoming will argue that the best way to avoid drift is to train students in particular disciplines and only then guide them across the divides. This may be pragmatic, but the first thing to do is to recognize the validity of Wallerstein's argument.

Reply

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I am pleased that the respondents all seem to share my concerns. There is, however, a "yes, but" quality to these responses. Calhoun regrets that my vision of the future is a replication of the *Methodenstreit* categories—a world of social science divided three ways (nomothetists, idiographers, and others). This is not my preferred vision but my prediction of where people stand today and therefore how they would spontaneously opt if given the choice. I did say that it would be an improvement to separate the parties this way rather than continue to fight over these issues within each department of each university. Some of the respondents seem to feel that these fights are fruitful. I have never found them so.

So, are there alternatives? No doubt many. Calhoun recommends "confrontations among different styles of research . . . in projects defined around places, theories, or clusters of intellectual problems." This of course has been tried, too. This is what area study programs or urban programs or, indeed, women's studies programs are. I have worked within this kind of structure too, and I am not sure that the results are intellectually happier.

It may be, as Hall suggests, that no grouping of any kind really works very well for very long and all of them result in straitjackets on one kind or another. The concrete proposals he puts forth would lead to a lot of ad hoc arrangements, which we often have already, and perhaps this is the best we can do. There remains the problem of graduate education, however, and students' need to get a credential that results in career possibilities. Here is where the pressure returns for the more permanent structures. And what I hear Brumann saying is that, given all these problems, he guesses he would probably continue to feel more comfortable in an "anthropology" department, perhaps slightly modified, than in anything he sees coming out of mergers, reorganizations, and other forms of fiddling with what we now have.

At this point, our two colleagues from Asia come in to remind us of non-Western realities. Alatas points to—and bemoans—the inbuilt institutional inequalities in the world of knowledge between institutions in the North and those in the South. He also warns us, once again, against the dangerously provincial character of our most general concepts alleged to be universal. And Madan reminds us that we have systematically neglected the social thought of the non-Western world and that this thought may provide us with a fruitful basket of concepts and modes of organizing knowledge of which we have deprived ourselves. Of course they are both right.

I continue to feel that our most urgent collective task as scholars is to expose and reflect upon the very fragile intellectual bases of our existing categorizations of knowledge. I think that we must also soberly assess the pluses and minuses of different modes of organizing knowledge clusters. And I consider it urgent to push towards constructing a common culture of social science based on the presumption that science and philosophy are one rather than opposed modes of knowing. I have no illusions of a smooth ride or even any guarantees that we shall succeed in constructing something better. But we are currently drifting in the open seas, and this is not a very promising position in which to achieve anything useful.

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