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The Heritage of Sociology, The Promise of Social Science

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We are met here to discuss 'Social Knowledge: Heritage, Challenges, Perspectives'. I argue that our heritage is something I call 'the culture of sociology', and I try to define what I think this is. I further argue that, for several decades now, there have been significant challenges precisely to that culture. These challenges essentially consist of calls to unthink the culture of sociology. Given both the persistent reassertion of the culture of sociology and the strength of these challenges, I try finally to persuade you that the only perspective we have that is plausible and rewarding is to create a new open culture, this time not of sociology but of social science, and (most importantly) one that is located within an epistemologically reunified world of knowledge.

We divide and bound knowledge in three different ways: intellectually as disciplines; organizationally as corporate structures; and culturally as communities of scholars sharing certain elementary premises. We may think of a discipline as an intellectual construct, a sort of heuristic device. It is a mode of laying claim to a so-called field of study, with its particular domain, its appropriate methods and, consequently, its boundaries. It is a discipline in the sense that it seeks to discipline the intellect. A discipline defines not only what to think about and how to think about it, but also what is outside its purview. To say that a given subject is a discipline is to say not only what it is but what it is not. To assert therefore that sociology is a discipline is, among other things, to assert that it is not economics or history or anthropology. And sociology is said not to be these other names because it is considered to

have a different field of study, a different set of methods, a different approach to social knowledge.

Sociology as a discipline was an invention of the late 19th century, along-side the other disciplines we place under the covering label of the social sciences. Sociology as a discipline was elaborated more or less during the period 1880–1945. The leading figures of the field in that period all sought to write at least one book that purported to define sociology as a discipline. Perhaps the last major work in this tradition was that written in 1937 by Talcott Parsons, *The Structure of Social Action*, a book of great importance in our heritage, and to whose role I return. It is certainly true that, in the first half of the 20th century, the various divisions of the social sciences established themselves and received recognition as disciplines. They each defined themselves in ways that emphasized clearly how they were different from other neighboring disciplines. As a result, few could doubt whether a given book or article was written within the framework of one discipline or another. It was a period in which the statement, 'that is not sociology; it is economic history, or it is political science' was a meaningful statement.

I do not intend here to review the logic of the boundaries that were established in this period. They reflected three cleavages in objects of study that seemed obvious to scholars at the time, and were strongly enunciated and defended as crucial. There was the cleavage past/present that separated idio-graphic history from the nomothetic trio of economics, political science and sociology. There was the cleavage civilized/other or European/non-European that separated all four of the previous disciplines (which essentially studied the pan-European world) from anthropology and Oriental studies. And there was the cleavage – relevant only, it was thought, to the modern civilized world – of market, state and civil society that constituted the domains respectively of economics, political science and sociology (Wallerstein et al., 1996: Ch. I). The intellectual problem with these sets of boundaries is that the changes in the world-system after 1945 – the rise of the USA to world hegemony, the political resurgence of the non-Western world, and the expansion of the world-economy with its correlative expansion of the world university system – all conspired to undermine the logic of these three cleavages (Wallerstein et al., 1996: Ch. II), such that by 1970 there had begun to be in practice a serious blurring of the boundaries. The blurring has become so extensive that, in the view of many persons, in my view, it was no longer possible to defend these names, these sets of boundaries, as intellectually decisive or even very useful. As a result, the various disciplines of the social sciences have ceased to be disciplines, because they no longer represent obviously different fields of study with different methods and therefore with firm, distinctive boundaries.

The names have not for that, however, ceased to exist. Far from it! For the various disciplines have long since been institutionalized as corporate

organizations, in the form of university departments, programs of instruction, degrees, scholarly journals, national and international associations, and even library classifications. The institutionalization of a discipline is a way of preserving and reproducing practice. It represents the creation of an actual human network with boundaries, a network that takes the form of corporate structures that have entrance requirements and codes providing for recognized paths of upward career mobility. Scholarly organizations seek to discipline not the intellect but the practice. They create boundaries that are far firmer than those created by disciplines as intellectual constructs, and they can outlast the theoretical justification for their corporate limits. Indeed, they have already done so. The analysis of sociology as an organization in the world of knowledge is profoundly different from the analysis of sociology as an intellectual discipline. If Michel Foucault may be said to have intended to analyze how academic disciplines are defined, created and redefined in *The Archaeology of Knowledge*, Pierre Bourdieu's *Homo Academicus* is the analysis of how academic organizations are framed, perpetuated and reframed within the institutions of knowledge.

I am not going to follow either path at the moment. I do not believe, as I have said, that sociology is any longer a discipline (but neither are our fellow social sciences). I do believe they all remain very strong organizationally. And I believe that it follows that we all find ourselves in a very anomalous situation, perpetuating in a sense a mythical past, which is perhaps a dubious thing to do. However, I wish rather to turn my attention to sociology as a culture, that is, as a community of scholars who share certain premises. For I believe that it is in the debates in this domain that our future is being constructed. I argue that the culture of sociology is recent and vigorous, but also fragile, and that it can continue to thrive only if it is transformed.

The Heritage

What can we mean by the culture of sociology? I start with two comments. First, what we normally mean by a 'culture' is a set of shared premises and practices, shared to be sure not by all members of the community all of the time but by most members most of the time; shared openly, but what is even more important shared subconsciously, such that the premises are seldom subject to discussion. Such a set of premises must necessarily be quite simple, and even banal. To the extent that the assertions are sophisticated, subtle, learned, they would be unlikely to be shared by too many, and therefore to be able to create a worldwide community of scholars. I suggest that there exists precisely such a set of simple premises shared by most sociologists, but not necessarily at all by persons who call themselves historians or economists.

Second, I think the shared premises are revealed – revealed, not defined

– by who it is that we present as our formative thinkers. The standard list these days for sociologists around the world is Durkheim, Marx and Weber. The first thing to note about this list is that if one posed the question of formative thinkers to historians, economists, anthropologists or geographers, one would surely come up with a different list. Our list does not contain Michelet or Gibbon, Adam Smith or John Maynard Keynes, John Stuart Mill or Machiavelli, Kant or Hegel, Malinowski or Boas.

So the question becomes, where did our list come from? After all, if Durkheim did call himself a sociologist, Weber did so only in the very last period of his life, and even then ambiguously,[1] and Marx of course never did so. Furthermore, although I have met sociologists who call themselves Durkheimians, and others who call themselves Marxists, and still others who call themselves Weberians, I have never yet met any who said that they were Durkheimian-Marxist-Weberians. So in what sense can these three be said to be founding figures of the field? Yet book after book, and in particular textbook after textbook, says so.[2]

It was not always thus. This grouping is in fact largely the doing of Talcott Parsons and his formative work of the culture of sociology, *The Structure of Social Action* (Parsons, 1949). Of course, as you will recall, Parsons intended that we canonize the trio of Durkheim, Weber and Pareto. Somehow, he was never able to persuade others of the importance of Pareto, who remains largely ignored. And Marx was added to the list, despite Parsons's best efforts to keep him off it. Nonetheless, I attribute the creation of the list essentially to Parsons. And that of course makes the list very recent. It is basically a post-1945 creation.

In 1937, when Parsons wrote, Durkheim was less central to French social science than he had been 20 years earlier and would be again after 1945.[3] And he was not a figure of reference in other major national sociological communities. It is interesting in this regard to look at the Introduction that George E. G. Catlin wrote to the first English edition of *The Rules of Sociological Method*. In 1938, writing for a US audience, Catlin pleaded for Durkheim's importance by classifying him in the same league as Charles Booth, Flexner and W. I. Thomas, and said that, although his ideas were anticipated by Wundt, Espinas, Tönnies and Simmel, he was nonetheless important (Catlin, 1964: xi–xii). This is not exactly the way Durkheim would be presented today. In 1937, Weber was not taught in German universities, and to be fair even in 1932 he was not the commanding figure he is today in German sociology. Nor had he yet been translated into English or French. As for Marx, he was scarcely ever even mentioned in most respectable academic circles.

R. W. Connell has shown in a recent survey what I had long suspected, that the pre-1945 textbooks may have mentioned these three authors, but only alongside a long list of others. Connell calls this 'an encyclopedic, rather

than a canonical, view of the new science by its practitioners' (Connell, 1997: 1514). It is the canon that defines the culture, and this canon had its heyday between 1945 and 1970, a very special period – one dominated by US socio-logical practitioners, one during which structural-functionalism was by far the leading perspective within the sociological community.

The canon must begin with Durkheim, the most self-consciously 'socio-logical' of the three, the founder of a journal called *l'Année Sociologique*, whose centenary we celebrate in 1998 as we celebrate the 50th Anniversary of the International Sociological Association. Durkheim responded to the first and most obvious of questions about which any student of social reality doing empirical work must wonder. How is it that individuals hold particu-lar sets of values, and not others? And how is it that persons with 'similar backgrounds' are more likely to hold the same set of values than persons of dissimilar backgrounds? We know the answer so well that it no longer seems to us a question.

Let us review nonetheless Durkheim's answer. He restates his basic argu-ments very clearly in the 'Preface to the Second Edition' of *The Rules of Sociological Method*, written in 1901. It was meant as a reply to the critics of the first edition, and in it he seeks to clarify what he is saying, since he feels he had been misunderstood. He declares three propositions. The first is that 'social facts must be treated as things', a statement he insists is 'at the very base of our method'. He asserts that he is not thereby reducing social reality to some physical substratum but simply claiming for the social world 'a degree of reality at least equal to that everyone accords' to the physical world. 'The thing [he says] stands in opposition to the idea, just as what is known from the outside stands in opposition to what is known from the inside' (Durkheim, 1982: 35–6). The second proposition is that 'social phenomena [are] external to individuals'.[4] And, finally, Durkheim insists that social con-straint is not the same as physical constraint, because it is not inherent but imposed from the outside.[5] Durkheim further takes note that, for a social fact to exist, there must be individual interactions which result in 'beliefs and modes of behaviour instituted by the collectivity; sociology can then be defined as the science of institutions, their genesis and their functioning' (Durkheim, 1982: 45). Thus we are clearly talking of a social reality that is socially constructed, and it is this socially constructed reality that sociolo-gists are to study – the science of institutions. Durkheim even anticipates our current concern with agency, because it is just at this point that he adds a foot-note, arguing the limits of 'permitted variation'.[6]

These three declarations taken together constitute the argument for Durkheim's 'basic principle, that of the objective reality of social facts. It is . . . upon this principle that in the end everything rests, and everything comes back to it' (Durkheim, 1982: 45).

I do not propose here to discuss my own views on these formulations of

Durkheim. I do wish to suggest that his effort to carve out a domain for soci-ology, the domain of what he calls 'social facts', a domain that is distinctive from the domains both of biology and of psychology, is indeed a basic premise of the culture of sociology. If you then say to me that there are persons among us who call themselves social psychologists, or symbolic interactionists, or methodological individualists, or phenomenologists, or indeed postmodernists, I say to you that these persons have nonetheless decided to pursue their scholarly endeavors under the label of sociology, and not of psychology, or biology, or philosophy. There must have been some intellectual reason for this. I suggest it is their tacit acceptance of the Durkheimian principle of the reality of social facts, however much they would like to operationalize this principle in ways quite different from those which Durkheim proposed.

In the Preface to the first edition, Durkheim discusses how he wishes to be labeled. The correct way, he says, is to not to call him either a 'material-ist' or an 'idealist' but a 'rationalist' (Durkheim, 1982: 32–3). While that term in turn has been the subject of many centuries of philosophical debate and discord, it is certainly a label that almost all sociologists from Durkheim's time to at least 1970 would have embraced.[7] I would like therefore to restate Durkheim's argument as *Axiom No. 1* of the culture of sociology: *there exist social groups which have explicable, rational structures*. Formulated in this simple way, I believe that there have been few sociologists who did not presume its validity.

The problem with what I am calling Axiom No. 1 is not the existence of these groups, but their lack of internal unity. This is where Marx comes in. He seeks to answer the question, how is it that social groups which are sup-posedly a unity (the meaning after all of 'group') in fact have internal struggles? We all know his answer. It is the sentence that opens the first section of the *Communist Manifesto*: 'The history of all hitherto existing society is the history of class struggles' (Marx and Engels, 1948: 9).[8] Of course, Marx was not so naive as to assume that the overt rhetoric of conflict, the explanations of the reasons for the conflict, were necessarily to be taken at face value or were in any sense correct, correct that is from the point of view of the analyst.[9] The rest of Marx's oeuvre is constituted by the elabora-tion of the historiography of the class struggle, the analysis of the mechan-isms of functioning of the capitalist system and the political conclusions one should draw from this framework of analysis. All this together constitutes Marxism, properly speaking, which is of course a doctrine and an analytic viewpoint that has been subject to great controversy within and outside the sociological community.

I do not propose to discuss either the merits of Marxism or the arguments of its opponents. I merely want to ask why it was that Parsons's attempt to exclude Marx from the picture failed so miserably, despite the Cold War, and

despite indeed the political preferences of the majority of the world's sociologists. It seems to me that Marx was discussing something so obviously central to social life that it simply could not be ignored, namely, social conflict.

Marx had a particular explanation of social conflict to be sure, one which centered about the fact that people had different relations to the means of production, some owning them and others not, some controlling their use and others not. It has been very fashionable for some time to argue that Marx was wrong about this, that the class struggle is not the only, or even the primary, source of social conflict. There have been various substitutes offered: status groups, political affinity groups, gender, race. The list goes on. Once again, I do not immediately discuss the validity of these alternatives to class, but confine myself to the observation that every substitute for 'class' presumes the centrality of struggle, and merely juggles the list of combatants. Is there anyone who has refuted Marx by saying, this is all nonsense, since there are no social conflicts?

Take so central an activity to the practice of sociologists as the opinion survey. What is it we do? We usually constitute what is called a representative sample, and we pose to this sample a series of questions about something. Normally, we presume that we will get a range of answers to these questions, although we may not have a clear idea in advance of what the range will turn out to be. If we thought everyone would answer the questions identically, there would be little point in doing the survey. When we get the answers to these questions, what is it we do next? We correlate the answers with a set of basic variables, such as socioeconomic status, occupation, sex, age, education and so on. Why do we do this? It is because we assume that often, even usually, each variable contains a continuum of persons along a certain dimension, and that the wage workers and the businessmen, men and women, the young and the old and so forth will tend to give different answers to the questions. If we did not presume social variation (and most frequently the emphasis has in fact been on variation in socioeconomic status), we would not be engaged in this enterprise. The step from variation to conflict is not a long one, and generally speaking those people who try to deny that variation leads to conflict are suspected of seeking to disregard an obvious reality for purely ideological reasons.

So there we are. We are all Marxists, in the diluted form of what I shall term *Axiom No. 2* of the culture of sociology: *all social groups contain subgroups that are ranked in a hierarchy, and are in conflict with each other*. Is this a dilution of Marxism? Of course it is, indeed a serious dilution. Is this however a premise of most sociologists? Of course it is as well.

Can we stop here? No, we cannot. Having decided that social groups are real and that we can explain their mode of operation (Axiom No. 1), and having decided that they harbor within them repeated conflicts (Axiom No.

2), we face an obvious question: why do not all societies simply blow up, or split apart, or destroy themselves in some other way? It seems clear that, although such explosions do indeed happen from time to time, they do not seem to happen most of the time. There does seem to be a semblance of 'order' in social life, despite Axiom No. 2. Here is where Weber comes in. For Weber has an explanation for the existence of order despite conflict.

We regularly identify Weber as the anti-Marx, one insisting on cultural as opposed to economic explanations, insisting on bureaucratization rather than accumulation as the central driving force of the modern world. But the key concept of Weber that serves to limit the impact of Marx, or at least to modify it seriously, is legitimacy. What does Weber say about legitimacy? Weber is concerned with the basis of authority. Why, he asks, do subjects obey those who give commands? There are various obvious reasons, such as custom and material calculation of advantage. But Weber says they are not enough to explain the commonness of obedience. He adds a third, crucial factor, the 'belief in legitimacy' (Weber, 1968: 213).[10] At this point, Weber delineates his three pure types of authority or legitimate domination: legitimacy based on rational grounds, legitimacy based on traditional grounds and legitimacy based on charismatic grounds. But since, for Weber, traditional authority is the structure of the past and not of modernity, and since charisma, however important a role it plays in historical reality and in Weberian analysis, is essentially a transitional phenomenon, always being eventually 'routinized', we are left with 'rational-legal authority' as the 'specifically modern type of administration' (Weber, 1968: 217).

The picture Weber offers us is that authority is administered by a staff, a bureaucracy, that is 'disinterested', in the sense that it has no *parti pris* either vis-a-vis the subjects or vis-a-vis the state. The bureaucracy is said to be 'impartial', that is, making its decisions according to the law, which is why this kind of authority is called rational-legal by Weber. To be sure, Weber admits that, in practice, the situation is a bit more complicated.[11] Nonetheless, if we now simplify Weber, we have a reasonable explanation for the fact that states are usually orderly, that is, that the authorities are usually accepted and obeyed, more or less, or to a certain extent. We shall call this *Axiom No. 3*, which can be stated as follows: *to the extent that groups/states contain their conflicts, it is in large part because lower-ranked subgroups accord legitimacy to the authority structure of the group on the grounds that this permits the group to survive, and the subgroups see long-term advantage in the group's survival.*

What I have been trying to argue is that the culture of sociology, which we all share, but which was strongest in the period of 1945–70, contains three simple propositions – the reality of social facts, the perennity of social conflict, the existence of mechanisms of legitimation to contain the conflict – which add up to a coherent minimal baseline for the study of social reality. I

have tried to indicate the way in which each of the three propositions was derived from one of the three formative thinkers: Durkheim, Marx and Weber, and I claim that is why we repeat the mantra that this trio represents 'classical sociology'. Once again, I repeat, this set of axioms is not a sophisticated and certainly not an adequate way of perceiving social reality. It is a starting-point, one that most of us have internalized and one that operates largely at the level of unquestioned premises that may be assumed rather than debated. This is what I am calling 'the culture of sociology'. This is, in my view, our essential heritage. But again I repeat, it is a heritage of a construct that is recent, and if vigorous also fragile.

The Challenges

I present six challenges that in my view raise very serious questions about the set of axioms I am calling 'the culture of sociology'. I present them in the order that they began to have an impact on the world of sociology, and more generally on social science, which was sometimes long after they were written. I wish to emphasize at the outset that these are challenges, not truths. Challenges are serious if they put forward credible demands on scholars to re-examine premises. Once we accept that the challenges are serious, we may be stimulated to reformulate the premises in ways that make them less vulnerable to the challenges. Or we may find ourselves forced to abandon the premises, or at the very least to revise them drastically. A challenge is thus part of a process, the beginning and not the end of the process.

The first challenge I present I associate with Sigmund Freud. This may seem surprising. For one thing, Freud was essentially a contemporary of Durkheim and Weber, not someone who came significantly later. For a second thing, Freud has in fact been well incorporated into the culture of sociology. Freud's topology of the psyche – the id, ego and superego – has long been something we use to provide the intervening variables that explain how it is that Durkheim's social facts are internalized inside individual consciousnesses. We may not all use Freud's exact language, but the basic idea is there. In a sense, Freud's psychology is part of our collective assumptions.

I am not however interested now in Freud's psychology but in Freud's sociology. Here, we tend primarily to discuss a few important works, such as *Civilization and its Discontents*, and they are important to be sure. But we tend to ignore the sociological implications of his modes of diagnosis and therapy. I wish to discuss what I think is Freud's implicit challenge to the very concept of rationality. Durkheim called himself a rationalist. Weber made rational-legal legitimation the linchpin of his analysis of authority. And Marx was devoted to pursuing what he called scientific (that is, rational) socialism. Our formative thinkers were all children of the Enlightenment, even when,

as in the case of Weber, they raised gloomy questions about where we were heading. (But the First World War caused much gloom for most of Europe's intellectuals.)

Freud was not at all a stranger to this tradition. Indeed, what was he about? He said to the world, and in particular to the medical world, that behavior that seems to us strange and irrational is in fact quite explicable, provided one understands that much of the individual's mind operates at a level Freud called the unconscious. The unconscious, by definition, cannot be seen or heard, even by the individual himself, but, said Freud, there are indirect ways of knowing what is going on in the unconscious. His first major work, *The Interpretation of Dreams* (first published in 1900), was precisely on this topic. Dreams reveal, said Freud, what the ego is repressing into the unconscious.[12] Nor are dreams the only analytic tool we have at our disposition. The whole of psychoanalytic therapy, the so-called talking cure, was developed as a series of practices that could help both the analyst and the analysand become aware of what was going on in the unconscious.[13] The method is quintessentially one derived from Enlightenment beliefs. It reflects the view that increased awareness may lead to improved decision-making, that is, more rational behavior. But the road to this more rational behavior is by recognizing that so-called neurotic behavior is in fact 'rational', once one understands what the individual intends by this behavior and therefore why it is occurring. The behavior may be in the opinion of the analyst suboptimal, but it is not thereby irrational.

In the history of psychoanalytic practice, Freud and the early analysts treated only, or at least primarily, adult neurotics. But following the logic of organizational expansion, later analysts were ready to analyze children, and even to treat infants who had not reached the age of talking. And still others began to find ways of dealing with psychotics, that is, with persons presumably beyond the capacity to enter into straightforwardly rational discussion. Freud himself has some interesting things to say about acute neurotics and psychotics. In discussing what Freud calls the 'metapsychology of repression', he indicates the multiple forms that repression can take, the various transference neuroses. For example, in anxiety hysteria, there might be first a drawing back from the impulse and then a flight to a substitutive idea, a displacement. But then the person might feel the need to 'inhibit . . . the development of the anxiety which arises from the substitute'. Freud then notes that 'with each increase of instinctual excitation the protecting rampart round the substitutive idea must be shifted a little further outwards' (Freud, 1957: 182). At this point, the phobia becomes still more complicated, leading to ever further attempts at flight.[14]

What is being described here is an interesting social process. Something has caused anxiety. The individual seeks to avoid the negative feelings and consequences by means of a repressive device. This does relieve the anxiety,

but at a price. Freud suggests that the price is too heavy (or is it that it may be too heavy?). What the psychoanalyst is presumably trying to do is to help the individual confront what is causing the anxiety, and thereupon to be able to relieve the pain at a lower price. So, the individual is trying rationally to reduce pain. And the psychoanalyst is trying rationally to lead the patient to perceive that there may be a better way (a more rational way?) to reduce pain.

Is the analyst right? Is this new way a more rational way to reduce pain? Freud ends this discussion of the unconscious by turning to still more difficult situations. Freud exhorts us to see 'how much more radically and profoundly this attempt at flight, this flight of the ego, is put in operation in the narcissistic neuroses' (Freud, 1957: 203). But even here, in what Freud regards as an acute pathology, he still perceives it as the same quest, the same rational quest for the reduction of pain.

Freud is very conscious of the limits of the role of the analyst. In *The Ego and the Id*, he warns quite clearly against the temptation to play 'prophet, saviour and redeemer'.[15] Freud manifests a similar sense of restraint in *Civilization and its Discontents*. He is discussing the impossibility of fulfilling our necessary task of trying to be happy. He says: 'There is no golden rule which applies to everyone: every man [*sic*] must find out for himself in what particular fashion he can be saved' (Freud, 1961: 34). He adds that choices pushed to an extreme lead to dangers, and flights into neurosis, concluding that: 'The man [*sic*] who sees his pursuit of happiness come to nothing in later years can still find consolation in the yield of pleasure of chronic intoxication; or he can embark on the desperate attempt at rebellion seen in a psychosis' (Freud, 1961: 35–6).

I am struck by several things in these passages from Freud. The pathologies he observes in the patient are described as flights from danger. I underline once again how rational it is to flee from danger. Indeed, even the most seemingly irrational flight of all, that into psychosis, is described as 'a desperate attempt at rebellion', as though the person had little alternative. In desperation, he tried psychosis. And finally, there is only so much the analyst can do, not only because he or she is not, may not be, a prophet, but because 'every man [*sic*] must find out for himself in what particular fashion he may be saved'.

We are not in a congress of psychoanalysts. I have not raised these issues to discuss either the functioning of the psyche or the modalities of psychiatric treatment. I have intruded these passages from Freud because of the light they throw on our underlying presupposition of rationality. Something may be described as rational only if there are other things that may be described as irrational. Freud wandered into the arena of what was socially accepted as irrational, neurotic behavior. His approach was to uncover the underlying rationality of this seemingly irrational behavior. He continued into the even more irrational, the psychotic, and found there too an explanation we might

call rational, once again the flight from danger. Of course, psychoanalysis is based on the assumption that there are better and less good modes of dealing with danger. The different responses of the individual exact different prices, to use Freud's economic metaphor.

Pushing, however, the logic of the search for the rational explanation of the seemingly irrational, Freud led us down a path whose logical conclusion is that nothing is irrational from the point of view of the actor. And who is any outsider to say that they are right and the patient is wrong? Freud is wary about how far the analyst should go in imposing his priorities on the patient. 'Every man must find out for himself in what particular fashion he can be saved.' But if nothing is irrational, as seen from someone's point of view, whence the hosannas for modernity, for civilization, for rationality? This is such a profound challenge that I would argue we have not even begun to confront it. The only consistent conclusion we can draw is that there is no such thing as formal rationality; or rather that, in order to decide what is formally rational, one must necessarily spell out in the ultimate detail of complexity and specificity the end that is intended, in which case, everything depends on the point of view and the balance of concerns of the actor. In this sense, postmodernism in its most radical solipsistic versions takes this Freudian premise to its final destination, and without giving Freud the least bit of credit for this in the process, be it noted, probably because they are unaware of the cultural origin of their assertions. But of course, such postmodernists are not taking the Freudian challenge as a challenge, but as an eternal universal truth, the grandest of grand narratives, and with this kind of self-contradiction this extreme position self-destructs.

In the face of Freud's challenge, some have thrown up their hands with glee, and have become solipsistic, and others have fallen back on repeating the mantra of rationality. We can afford to do neither. Freud's challenge to the very operationality of the concept of formal rationality forces us to take more seriously the Weberian pendant concept of substantive rationality, and to analyze it in greater depth than Weber was ready to do himself. What Freud has challenged, what in fact he has perhaps demolished, is the usefulness of the concept of *formal* rationality. Can there be such a thing as abstract formal rationality? Formal rationality is always someone's formal rationality. How then can there be a universal formal rationality? Formal rationality is usually presented as the utilization of the most effective means to an end. But ends are not so easy to define. They invite a Geertzian 'thick description'. And once given that, Freud is hinting, everyone is formally rational. *Substantive* rationality is precisely the attempt to come to terms with this irreducible subjectivity, and to suggest that nonetheless we can make intelligent, meaningful choices, social choices. I return to this theme later.

The second challenge with which I wish to deal is the challenge to Eurocentrism. This is very widespread today. It was seldom mentioned 30 years

ago. One of the first persons to raise this issue publicly and among us was Anouar Abdel-Malek, whose denunciation of 'Orientalism' (1963) predates that of Edward Said by more than a decade, and who has devoted his life work to suggesting what he has called an 'alternative civilizational project' (Abdel-Malek, 1981: xii). I would like to discuss what he has argued, particularly in *Social Dialectics* (1981). I choose to discuss his work because Abdel-Malek goes beyond a mere denunciation of the misdeeds of the West to an exploration of alternatives. Abdel-Malek starts with the assumption that in the transformed geopolitical reality, 'Prepostulated universalism, as a recipe, simply will not do'.[16] In order to arrive at what Abdel-Malek perceives as 'meaningful social theory' (Abdel-Malek, 1981: 43), he suggests we employ a non-reductionist comparativism, comparing what he sees as a world consisting of three interwoven circles – civilizations, cultural areas and nations (or 'national formations'). For him, there are only two 'civilizations', the Indo-Aryan and the Chinese. Each contains multiple cultural areas. The Indo-Aryan contains Egyptian Antiquity, Greco-Roman Antiquity, Europe, North America, Sub-Saharan Africa, the Arab-Islamic and Perso-Islamic zones and major parts of Latin America. The Chinese includes China proper, Japan, Central Asia, Southeast Asia, the Indian subcontinent, Oceania and the Asian-Islamic zone.

If the key factor for Abdel-Malek is 'civilization', the key concept is 'specificity', and this requires, in his words, adding a 'geographical thread' to the historical (Abdel-Malek, 1981: 97). But having said that, he then adds that the central problem in general theory and epistemology is 'to deepen and define the relations between the concept of time and the constellation of notions concerned particularly with the density of time in the domain of human societies' (Abdel-Malek, 1981: 156). Although one can compare civilizations in terms of production, reproduction and social power, the crucial difference is relations with the time dimension, wherein we find the greatest 'density of manifest, explicit specificity. For here we are at the very heart of culture and thought'. He speaks of 'the all-pervading central constitutive influence of the time-dimension, the depth of the historical field' (Abdel-Malek, 1981: 171–2).

The geographical challenge thus turns out to be an alternative concept of time. Remember that, for Abdel-Malek, there are only two 'civilizations' in the sense he is using it, and therefore only two relations to the time dimension. On the one side is the Western vision of time, an 'operational view', which he traces to Aristotle, 'the rise of formal logic, the hegemony of analytical thinking', time as 'a tool for action, not as a conception of man's [*sic*] place in historical duration' (Abdel-Malek, 1981: 179). And 'on the other side of the river', we find a non-analytic concept, where 'time is master', and therefore cannot be 'apprehended as commodity'.[17] He concludes with a call for a 'non-antagonistic yet contradictory dialectical interaction between the

two banks of our common river' (Abdel-Malek, 1981: 185).[18] Where does this leave us? It leaves us with two banks of a common river – not at all the vision of Durkheim, Marx and Weber. It leaves us with irreducible specificities about which we can nonetheless theorize. It leaves us with a civilizational challenge about the nature of time, an issue that was not even an issue for the classical culture of sociology. And this brings us directly to the third challenge.

The third challenge is also about time, not about two visions of time, but about multiple realities of time, about the social construction of time. Time may be the master, but if so, for Fernand Braudel, it is both a master we have constructed ourselves and yet one which it is difficult to resist. Braudel argues there are in fact *four* kinds of social time, but that, in the 19th century and most of the 20th, the overwhelming majority of social scientists perceived only *two* of them. On the one hand, there were those who considered that time was essentially composed of a sequence of events, what Paul Lacombe had called 'histoire événementielle', a term best translated into English as 'episodic history'. In this view, time was the equivalent of a Euclidean line, which had an infinite number of points on it. These points were the 'events', and they were located in a diachronic sequence. This is of course consonant with the ancient view that all is constantly changing at every moment, that explanation is sequential, and that experience is unrepeatable. It is at the basis of what we call idiographic historiography but it is also the basis of atheoretical empiricism, both of which have been widespread in modern social science.

The alternative widespread view of time is that social processes are timeless, in the sense that what explains events are rules or theorems that apply across all of time and space, even if at the present moment we cannot explicate all these rules. In the 19th century, this view was sometimes referred to as 'social physics', in an allusion to Newtonian mechanics which provided the model of this kind of analysis. Braudel referred to this concept of time as 'la très longue durée' (not to be confused with 'la longue durée'). We might call this eternal time. Braudel discussed Claude Lévi-Strauss as his prime example of this approach, but of course the concept has been widely used by others. Indeed, one might say that it constitutes the prevalent usage within the culture of sociology, and is what we usually mean when we speak of 'positivism'. Braudel himself says of this variety of social time: 'if it exists, [it] can only be the time period of the sages' (Braudel, 1972: 35).

Braudel's basic objection to these two concepts of time is that neither of them takes time seriously. Braudel thinks that eternal time is a myth and that episodic time, the time of the event, is, in his famous phrase, 'dust'. He suggests that social reality in fact occurs primarily in two other kinds of time which have been largely ignored by both idiographic historians and nomothetic social scientists. He calls these times that of the *longue durée*, or

structural time, long but not eternal, and that of the *conjoncture*, or cyclical, middle-range time, the time of cycles *within* structures. Both these times are constructs of the analyst, but they are also simultaneously social realities that constrain the actors. Perhaps you feel that Durkheim, Marx and Weber were not entirely resistant to such Braudelian constructs. And to some extent, that is true. They were all three sophisticated and subtle thinkers, and said much that we ignore today at our peril. But as the three were incorporated into what I am calling the culture of sociology, there was no room for socially constructed time, and hence Braudel represents a fundamental challenge to that culture. As the challenge to Eurocentrism forces us into a more complex geography, so the protest against ignoring social time forces us into a far longer time perspective than we have been accustomed to use – but always one, I remind you, that is far less than infinite. No doubt the emergence in the 1970s of what we now call historical sociology was a response, at least in part, to the Braudelian challenge, but it has been absorbed as a specialty within sociology, and the implicit Braudelian demand for greater epistemological reconfiguration has been resisted.

The fourth challenge has come from outside social science. It has come from the emergence of a knowledge movement in the natural sciences and mathematics that today is known as complexity studies. There are a number of important figures in this movement. I concentrate on the one who has in my view stated the challenge most radically, Ilya Prigogine. Sir John Maddox, the former editor of *Nature*, took note of Prigogine's singular importance and asserted that the research community owes him a great debt 'for his almost single-handed persistence over four decades with the problems of non-equilibrium and complexity' (Maddox, 1997). Prigogine is of course a Nobel Prize laureate in chemistry, awarded for his work on so-called dissipative structures. But the two key concepts that resume his perspective are 'the arrow of time' and 'the end of certainties'.[19]

Both concepts seek to refute the most fundamental assumptions of Newtonian mechanics, assumptions that Prigogine thinks survived even the revisions required by quantum mechanics and relativity.[20] The non-Newtonian concepts of entropy and probabilities are to be sure not recent ones. They were at the basis of chemistry, as it developed in the 19th century, and indeed in a sense justified the distinction between physics and chemistry. But, from the point of view of the physicists, the resort to such concepts indicated the intellectual inferiority of chemistry. Chemistry was incomplete, precisely because it was insufficiently deterministic. Not only does Prigogine refuse to accept the lesser merit of such concepts but he goes much further. He wishes to argue that physics itself must be based on them. He is intent on spearing the dragon in its inner rampart, assserting that irreversibility, far from being noxious, is a 'source of order' and 'play a fundamental constructive role in nature' (Prigogine, 1997: 26–7).[21] Prigogine makes it quite clear that he does

not wish to deny the validity of Newtonian physics. It deals with integrable systems, and holds within its 'domain of validity' (Prigogine, 1997: 29). However, this domain is limited, since 'integrable systems are the exception' (Prigogine, 1997: 108).[22] Most systems 'involve both deterministic processes (between bifurcations) and probabilistic processes (in the choice of the branches)' (Prigogine, 1997: 69), and the two processes together create a historical dimension recording the successive choices.

Just as we are not in a congress of psychoanalysts, so we are not in a congress of physicists. If I raise this challenge here among us, it is largely because we have been so accustomed to assuming that Newtonian mechanics represented an epistemological model which we ought to emulate that it is important to recognize that this epistemological model is under severe challenge within the very culture in which it originated. But, even more important, it is because this reformulation of dynamics inverts completely the relation of social science to natural science. Prigogine reminds us of Freud's assertion that humanity has known three successive hurts to its pride: when Copernicus showed that the earth was not the center of the planetary system; when Darwin showed that humans were a species of animal; and when he, Freud, showed that our conscious activity is controlled by our unconscious. To this Prigogine adds: 'We can now invert this perspective: We see that human creativity and innovation can be understood as the amplification of laws of nature already present in physics and chemistry' (Prigogine, 1997: 71). Notice what he has done here. Prigogine has reunited social science and natural science, not on the 19th-century assumption that human activity can be seen as simply a variant of other physical activity, but on the inverted basis that physical activity can be seen as a process of creativity and innovation. This is surely a challenge to our culture, as it has been practiced. Furthermore, Prigogine also speaks to the issue of rationality that we have raised. He calls for a 'return to realism' that is not a 'return to determinism' (Prigogine, 1997: 131).[23] The rationality that is realistic is precisely the rationality that Weber was calling 'substantive', that is, the rationality which is the result of realistic choice.[24]

The fifth challenge which I wish to discuss is that of feminism. Feminists say to the world of knowledge that it has been biased in multiple ways. It has ignored women as subjects of human destiny. It has excluded women as students of social realities. It has utilized a priori assumptions about gender differences which are not based on realistic research. It has ignored the standpoint of women.[25] All of these charges seem to me to be just in terms of the historical record. And the feminist movement, within sociology and within the larger domain of the world of social knowledge, has had some impact in recent decades in rectifying these biases, although of course there is still a long way to go before these issues become non-issues.[26] However, in all this aspect of the work of feminists, they have not been

challenging the culture of sociology. Rather they have been utilizing it, and simply saying that most sociologists (and more broadly, social scientists) have not been respecting the very rules they established for the practice of social science.

This is no doubt a very important thing to have done. Yet, I think there is something even more important, wherein feminists have very definitely been challenging the culture of sociology. This has been the assertion that there been a masculinist bias not only in the domain of social knowledge (where, so to speak, it might have been theoretically expectable) but also in the domain of knowledge of the natural world (where in theory it should not have existed). In this assertion, they have attacked the legitimacy of the claim to objectivity in its *sanctum sanctorum*, a claim that is central to the classical culture of sociology. Just as Prigogine was not satisfied to be permitted to have chemistry as an exception to the determinism of physics, but has insisted that physics itself is not and cannot be deterministic, so feminists are not satisfied with having social knowledge defined as a domain in which social biases are expectable (if undesirable); they are insisting that this applies equally to the knowledge of natural phenomena. I deal with this issue by discussing a few feminist scholars whose background (that is, whose initial training) was in the natural sciences, and who therefore lay claim to be able to speak to this issue with the necessary technical knowledge, training in, and sympathy for natural science.

The three I have chosen are Evelyn Fox Keller, trained as a mathematical biophysicist; Donna J. Haraway, trained as a hominid biologist; and Vandana Shiva, trained as a theoretical physicist. Keller relates her realization in the mid-1970s that what had previously seemed to her a patently absurd question suddenly took precedence in her intellectual hierarchy: 'How much of the nature of science is bound up with the idea of masculinity, and what would it mean for science if it were otherwise?' She then indicates how she will answer this query: 'My subject [is] how the making of men and women has affected the making of science.' Thus far, we are no further than the sociology of knowledge or the sociology of science. And Keller says quite correctly that posing the question merely in this way will result in a 'marginal' impact at most on the culture of natural science. What needs to be shown is that gender affects the 'production of scientific theory' (Keller, 1985: 3–5).

Can this be done? Keller looks to the intervening variable of the psyches of the scientists. She speaks of 'the intra-personal dynamics of "theory choice"' (Keller, 1985: 10).[27] Keller has no difficulty showing how the founders of Baconian science suffused their work with masculinist metaphors, involving a virile mastery and domination of nature, and that the claim of scientists to be different from natural philosophers on the basis that only scientists eschewed the projection of subjectivity simply does not stand the

test of analysis.[28] Keller thus observes 'androcentrism' in science, but refuses to draw the conclusion either of rejecting science per se or of calling for the creation of a so-called radically different science. Rather, she says:

> My view of science – and the possibilities of at least a partial sorting of cognitive from ideological – is more optimistic. And, accordingly, the aim of these essays is more exacting: it is the reclamation, from within science, of science as a human instead of a masculinist project, and the renunciation of the division between emotional and intellectual labor that maintains science as a male preserve. (Keller, 1985: 178)

Donna Haraway starts from her concerns as a hominid biologist and attacks the two somewhat different attempts of R.M. Yerkes and E.O. Wilson to transform biology 'from a science of sexual organisms to one of reproducing genetic assemblages' (Haraway, 1991: 45). The object of both theories, she argues, is human engineering, in two successively different forms, the differences merely reflecting changes in the larger social world. She asks about both theories, human engineering in the interests of whom? She calls her work one 'about the invention and reinvention of nature – perhaps the most central arena of hope, oppression, and contestation for inhabitants of the planet earth in our times' (Haraway, 1991: 1). She insists she is speaking not about nature as it is, but about the stories we are told about nature and experience, in whose telling biologists play a key role.

I do not try to reproduce her arguments here, but simply draw attention to the conclusions she wishes to draw from this critique. Like Keller, she refuses to draw from her critique of 'biological determinism' an exclusively 'social constructionist' view (see Haraway, 1991: 134–5). Rather she sees the social development of the 20th century as one in which we have all become 'chimeras, theorized and fabricated hybrids of machine and organism', to which she gives the name of cyborgs. She says that hers is 'an argument for *pleasure* in the confusion of boundaries and for *responsibility* in their construction' (Haraway, 1991: 150). The boundary breakdowns she sees are those of human and animal, or human plus animal (or organism) and machine; of the physical and non-physical.

She warns against 'universal, totalizing theory' which she calls 'a major mistake that misses most of reality', but she also claims that 'taking responsibility for the social relations of science and technology means refusing an anti-science metaphysics, a demonology of technology' (Haraway, 1991: 181).[29] The theme of responsibility is central to this challenge. She rejects relativism not in the name of 'totalizing visions' but in the name of 'partial, locatable, critical knowledges sustaining the possibility of webs of connections called solidarity in politics and shared conversations in epistemology' (Haraway, 1991: 191).[30]

Vandana Shiva's critique is focused less on scientific methods proper than on the political implications that are drawn from science's position in the

cultural hierarchy. She speaks as a woman of the South, and thus her critique rejoins that of Abdel-Malek.[31]

She opposes to the idea of 'man's empire over nature' the concept of the 'democracy of all life', which she says is the basis of 'most non-Western cultures' (Shiva, 1993: 265). Shiva sees the preservation of biodiversity and the preservation of human cultural diversity as intimately linked, and is therefore particularly concerned about the consequences of the contemporary biotechnological revolution.[32]

I am struck by two constants in the challenge as formulated by Keller, Haraway and Shiva. One is that the critique of natural science as it has been practiced is never translated into a rejection of science as a knowledge activity, but rather into a scientific analysis of scientific knowledge and practice. And second, that the critique of natural science as it has been practiced leads to a call for responsible social judgment. Perhaps you feel that the case for gender bias in natural science is not proven. Here, I think Sandra Harding makes the appropriate response: 'Improbable as [attempts to show how Newton's and Einstein's laws of nature might participate in gender symbolization] may sound, there is no reason to think them in principle incapable of success' (Harding, 1986: 47).[33] The key phrase is 'in principle'. It is on this note of appeal to the most basic practice of science, submitting all claims to empirical verification, that the challenge of feminism to science stands. By its doubts about any a priori assumption that gender is irrelevant to scientific practice, feminism poses a fundamental challenge to the culture of sociology. Whether it poses an equal challenge to the culture of natural science, one they will take into account, remains to be seen.[34]

The sixth and last challenge with which I deal is perhaps the most surprising of all, and the one least discussed. It is that modernity, the centerpiece of all our work, has never really existed. This thesis has been put forth with most clarity by Bruno Latour, the title of whose book is the message: 'We Have Never Been Modern'. Latour starts his book with the same argument as that of Haraway, that impure mixtures are constitutive of reality. He speaks of the proliferation of 'hybrids', what she calls 'cyborgs'. For both, hybrids are a central phenomenon, increasing over time, underanalyzed, and not at all terrifying. What is crucial for Latour is overcoming the scholarly and social segmentation of reality into the three categories of nature, politics and discourse. For him the networks of reality are 'simultaneously real, like nature, narrated, like discourse, and collective, like society' (Latour, 1993: 6).

Latour is often misread as a variety of postmodernist. It is hard to see how an attentive reader could in fact make this error. For he attacks with equal vigor those he calls antimodern, those he calls modern and those he calls postmodern. For him, all three groups assume that the world in which we have been living for the last several centuries and in which we are still living has been 'modern' in the definition that all three groups in common give to

modernity: 'an acceleration, a rupture, a revolution in time [in contrast to] an archaic and stable past' (Latour, 1993: 10).

Latour argues that the word 'modern' hides two sets of quite different practices: on the one hand, the constant creation by 'translation' of new hybrids of nature and culture; and on the other, a process of 'purification', separating two ontological zones, humans and non-humans. The two processes, he argues, are not separate, and cannot be analyzed separately, because paradoxically it is precisely by forbidding hybrids (purification) that it becomes possible to create hybrids, and conversely it is by conceiving of hybrids that we limit their proliferation.[35] To sort out the so-called modern world, Latour recommends an 'anthropology', by which he means 'tackling everything at once'.[36]

Latour conceives of the world in which we live as based on what he calls a Constitution, which renders the moderns 'invincible' by proclaiming that nature is transcendent and beyond human construction, but that society is not transcendent and therefore humans are totally free.[37] Latour believes that, if anything, the opposite is true.[38] The whole concept of modernity is a mistake.

> No one has ever been modern. Modernity has never begun. There has never been a modern world. The use of the present perfect tense[39] is important here, for it is a matter of a retrospective sentiment, of a rereading of our history. I am not saying we are entering a new era; on the contrary we no longer have to continue the headlong flight of the post-post-postmodernists; we are no longer obliged to cling to the avant-garde of the avant-garde; we no longer seek to be even cleverer, even more critical, even deeper into the 'era of suspicion'. No, instead we discover that we have never begun to enter the modern era. Hence the hint of the ludicrous that always accompanies postmodern thinkers; they claim to come after a time that has not even started! (Latour, 1993: 47)

There is something new, however; it is that we have reached a point of saturation.[40] And this brings Latour to the question of time, which as you may see by now is at the center of most of the challenges:

> If I explain that revolutions attempt to abolish the past but cannot do so, I again run the risk of being taken for a reactionary. This is because for the moderns – as for their antimodern enemies, as well as for their false postmodern enemies – time's arrow is unambiguous; one can go forward, but then one has to break with the past; one can choose to go backward, but then one has to break with modernizing avant-gardes, which have broken radically with their own past. . . . If there is one thing we are incapable of carrying out, we now know, it is a revolution, whether it be in science, technology, politics, or philosophy. But we are still modern when we interpret this fact as a disappointment. (Latour, 1993: 69)

We have all, says Latour, never ceased to be 'amoderns' (Latour, 1993: 90). There are no 'cultures', just as there are no 'natures'; there are only 'natures–cultures' (Latour, 1993: 103–4). 'Nature and Society are not two

distinct poles, but one and the same production of successive states of societies–natures, of collectives' (Latour, 1993: 139). It is by recognizing this and making it the center of our analyses of the world that we can go forward.

We are at the end of our recital of the challenges. I remind you that for me the challenges are not truths but mandates for reflection about basic premises. Do you have some doubts about each of the challenges? Most probably. So do I. But together, they constitute a formidable attack on the culture of sociology, and cannot leave us indifferent. Can there be such a thing as formal rationality? Is there a civilizational challenge to the Western/modern view of the world that we must take seriously? Does the reality of multiple social times require us to restructure our theorizing and our methodologies? In what ways do complexity studies and the end of certainties force us to reinvent the scientific method? Can we show that gender is a structuring variable that intrudes everywhere, even into zones that seem incredibly remote, such as mathematical conceptualization? And is modernity a deception – not an illusion, but a deception – that has deceived first of all social scientists?

Can the three axioms, derived as I have suggested from Durkheim/ Marx/Weber, the axioms that constitute what I have called the culture of sociology, deal adequately with these questions, and if not, does the culture of sociology thereby collapse? And if it does, with what can we replace it?

The Perspectives

I should like to deal with the promise of social science in terms of three prospects which seem to me both possible and desirable for the 21st century: the epistemological reunification of the so-called two cultures, that of science and the humanities; the organizational reunification and redivision of the social sciences; and the assumption by social science of centrality in the world of knowledge.

What conclusions can we draw from my analysis of the culture of sociology and the challenges it has been facing? First of all, quite simply, the ultra-specialization that sociology, and indeed all the other social sciences, has been suffering has been both inevitable and self-destructive.[41] We must nonetheless continue to struggle against it, in the hope of creating some reasonable balance between depth and breadth of knowledge, between the microscopic and the synthetic vision. Second, as Smelser has put it so well recently, there are no 'sociologically naive actors'.[42] But do we have sociologically well-informed actors? That is, are our actors rational? And what world do our actors know?

It seems to me that the social facts with which we deal are social in two senses: they are shared perceptions of reality, shared more or less by some medium-large group but with different shadings for every individual viewer.

And they are socially constructed perceptions. But let us be clear. It is not the analyst whose social construction of the world is of interest. It is that of the collectivity of actors who have created social reality by their cumulated actions. The world is as it is because of all that has preceded this moment. What the analyst is trying to discern is how the collectivity has constructed the world, using of course his or her own socially constructed vision.

The arrow of time is thus ineluctable, but also unpredictable, since there are always bifurcations before us, the outcome of which is inherently indeterminate. Furthermore, although there is but one arrow of time, there are multiple times. We cannot afford to neglect either the structural *longue durée* or the cyclical rhythms of the historical system we are analyzing. Time is far more than chronometry and chronology. Time is also duration, cycles and disjunction.

A real world does exist, indubitably. If *it* does not exist, *we* do not exist, and that is absurd. If we do not believe this, we should not be in the business of studying the social world. Solipsists cannot talk even to themselves, since we are all changing at each instant, and therefore, if one adopts the standpoint of a solipsist, our own views of yesterday are as irrelevant to our created visions of today as are the views of others. Solipsism is the greatest of all forms of hubris, greater even than objectivism. It is the belief that our ratiocinations create what we perceive and that we thereby perceive what exists, that which we have created.

But, on the other hand, it is also true that we can only know the world through our vision of it, a collective social vision no doubt, but a human vision nonetheless. This is obviously as true of our vision of the physical world as it is of our vision of the social world. In that sense, we all depend on the glasses with which we engage in this perception, the organizing myths (yes, the grand narratives) that McNeill (1986) calls 'mythistory', without which we are helpless to say anything. It follows from these constraints that there are no concepts that are not plural; that all universals are partial; and that there exists a plurality of universals. And it also follows that all verbs that we use must be written in the past tense. The present is over before we can pronounce it, and all statements need to be located in their historical context. The nomothetic temptation is every bit as dangerous as the idiographic temptation, and constitutes a pitfall into which the culture of sociology has more frequently led too many of us.

Yes, we are at the end of certainties. But what does this mean in practice? In the history of thought, we have been constantly offered certainty. The theologians offered us certainties as seen by prophets, priests and canonized texts. The philosophers offered us certainties as rationally deduced or induced or intuited by them. And the modern scientists offered us certainties as verified empirically by them using criteria they invented. All of them have claimed that their truths were validated visibly in the real world, but that

these visible proofs were merely the outward and limited expression of deeper, more hidden truths for whose secrets and discovery they were the indicated intermediaries.

Each set of certainties has prevailed for some times in some places, but none of them everywhere or eternally. Enter the skeptics and nihilists who pointed to this wide array of contradictory truths and derived from the doubts this sowed the proposition that no claimed truth is more valid than any other. But if the universe is in fact intrinsically uncertain, it does not follow that the theological, the philosophical and the scientific enterprises have no merit, and it surely does not follow that any of them represents merely a gigantic deception. What does follow is that we would be wise to formulate our quests in the light of permanent uncertainty, and look upon this uncertainty not as unfortunate and temporary blindness nor as an insurmountable obstacle to knowledge but rather as an incredible opportunity to imagine, to create, to search.[43] Pluralism becomes at this point not an indulgence of the weak and ignorant but a cornucopia of possibilities for a better universe.[44]

Recently, a group composed largely of physical scientists published a book they entitled a dictionary of ignorance, arguing that science plays a bigger role in creating zones of ignorance than in creating zones of knowledge. I cite the blurb they placed on the back of the book:

> In the process of science enlarging our field of knowledge, we become aware, paradoxically, that our ignorance grows as well. Each new problem we resolve tends to cause the appearance of new enigmas, such that the processes of research and discoveries renew themselves constantly. The frontiers of knowledge seem to widen ceaselessly, giving birth to previously unsuspected questions. But these new problems are salutary. Creating new challenges to science, they oblige it to advance in a perpetual movement without which, perhaps, its light would be quickly extinguished. (Cazenave, 1998)

One of the problems about the creation of new ignorances is that there is no plausible reason to presume that they can be best treated only in or by the narrow domain within which these ignorances were uncovered. The physicist may expose new ignorances which require for their resolution concerns previously designated as biological or philosophical. And this is, as we know, certainly true of the new ignorances sociologists uncover. The protection of one's turf in the face of new ignorances is the worst of scholarly sins, and the greatest possible deterrence to clarity.

It is this issue of turf that underlies the organizational problems of the social sciences. The institutionalization of the nominal divisions of the social sciences is extremely strong today, despite all the genuflection before the rosy glow of 'interdisciplinarity'. Indeed, I would argue that interdisciplinarity is itself a lure, representing the greatest support possible to the current list of disciplines, by implying that each has some special knowledge which it might

be useful to combine with some other special knowledges in order to solve some practical problem.

The fact is that the three great cleavages of 19th-century social science: past/present, civilized/others and state/market/civil society are all three totally indefensible as intellectual markers today. There are no sensible statements one can make in the so-called fields of sociology, economics or political science that are not historical, and there are no sensible historical analyses one can undertake that do not make use of the so-called generalizations that are in use in the other social sciences. Why then continue to pretend that we are engaged in different tasks?

As for civilized/other, the civilized are not civilized and the other are not other. There are of course specificities but they are legion, and the racist simplifications of the modern world are not only noxious but intellectually disabling. We must learn how to deal with the universal and the particular as a symbiotic pair that will never go away, and which must inform all of our analyses.

And finally the distinction of state/market/civil society is quite simply an implausible one, as any real actor in the real world knows. The market is constructed and constrained by the state and the civil society. The state is a reflection of both the market and the civil society. And the civil society is defined by the state and the market. One cannot separate these three modes of expression of actors' interests, preferences, identities and wills into closeted arenas about which different groups of people will make scientific statements, *ceteris paribus*.

I continue, however, to share the Durkheimian premise that psychology and social science are two separate enterprises, and that psychology is closer to, perhaps an intrinsic part of, biology. I note that most psychologists, from the behaviorists to the Freudians, seem to share this view. The group most resistant to this separation is in fact to be found within sociology.

If then none of our existing modes of dividing the social sciences today into separate organizations of knowledge make sense, what shall we do? Those who have studied what is called the sociology of organizations have shown us time and again how resistant organizations are to imposed change, how fiercely and cleverly their leaders act to defend interests that they will not avow but seem very real to those in power. It is difficult to force the pace of transformation. It is perhaps quixotic even to try. On the other hand, there are processes internal to each of our organizations that are destroying the boundaries without the intrusion of any deliberate reform process. Individual scholars are seeking peers with which to create the small groups and networks they find necessary to do their work. And increasingly such networks are paying no attention whatsoever to disciplinary labels.

Furthermore, as specialization proliferates, those who hold the budgetary purse-strings are growing increasingly restive about the seeming irrationality

of the overlaps, especially given the worldwide pressures to reduce rather than increase expenditures on higher education. It is the accountants who may force our pace, and quite possibly in ways that are not intellectually optimal. Thus, it seems to me, it is urgent that the scholars engage in organizational exploration, allowing for wide experimentation and being quite tolerant of each other's efforts, in order to see what kinds of organizational realignments might work best. Perhaps micro–macro should be institutionalized as a mode of organizing groups of scholars. I am not sure. Up to a point, it is in use in the natural sciences already, and in practice (if not in theory), social scientists are using it too. Or perhaps we should be dividing ourselves according to the temporalities of change with which we are dealing – short-term, middle-term, long-term. On none of these dividing lines do I have a fixed view at this point. I feel we should try them out.

What I am very clear about is that we must open ourselves up collectively, and recognize our blinkers. We must read far more widely than we now do, and we must strongly encourage our students to do so. We should recruit our graduate students far more widely than we do, and we must let them play a major role in determining where we can help them grow. And it is crucial for us to learn languages. A scholar who cannot *read* three to five major scholarly languages is severely handicapped. English is surely crucial but English alone means that one has access to at most 50 percent of what is written, and as the decades go by, the percentage will diminish since the areas of greatest growth in the production of scholars will be increasingly non-English in their written production. Increased reading knowledge of languages goes hand in hand with increased internationalization of our corps of scholars, even if they are not identical.

I do not know what kind of restructuring will take place, but I am skeptical that there will ever be a 100th anniversary of any of the existing international social science associations, at least under the same name.

I have saved for the last what I think is the most fascinating perspective of all, and perhaps the most important. Ever since the so-called divorce between philosophy and science consummated in the late 18th century, the social sciences have been the poor relation – neither fish nor fowl, and scorned by both sides in this war of the 'two cultures'. And the social scientists have internalized this image, feeling they had no fate other than to align themselves either with the scientists or with the humanists. Today the situation has radically changed. In the physical sciences, there is a strong and growing knowledge movement, complexity studies, that talks of the arrow of time, of uncertainties, and believes that human social systems are the most complex of all systems. And in the humanities, there is a strong and growing knowledge movement, cultural studies, that believes that there are no essential esthetic canons, and that cultural products are rooted in their social origins, their social receptions and their social distortions.

It seems to me clear that complexity studies and cultural studies have moved the natural sciences and the humanities respectively onto the terrain of social science. What had been a centrifugal field of forces in the world of knowledge has become a centripetal one, and social science is now central to knowledge. We are in the process of trying to overcome the 'two cultures', of trying to reunite into a single domain the search for the true, the good and the beautiful. This is cause for rejoicing, but it will be a very difficult row to hoe.

Knowledge, in the face of uncertainties, involves choices – choices by all matter, and of course choices by social actors, among them the scholars. And choices involve decisions about what is substantively rational. We can no longer even pretend that scholars can be neutral, that is, divested of their social reality. But this in no way means that anything goes. It means that we have to weigh carefully all the factors, in all the domains, to try to arrive at optimal decisions. And that in turn means we have to talk to each other, and to do so as equals. Yes, some of us have more specific knowledge about specific areas of concern than others, but no one, and no group, has all the knowledge necessary to make substantively rational decisions, even in relatively limited domains, without taking into account the knowledge of others outside these domains. Yes, no doubt, I would want the most competent brain surgeon, if I needed brain surgery. But competent brain surgery involves some judgments that are juridical, ethical, philosophical, psychological and sociological as well. And an institution like a hospital needs to bring these wisdoms into a blended, substantively rational view. Furthermore, the views of the patient are not irrelevant. It is the brain surgeon more than anyone else who needs to know this, as does the sociologist, or the poet. Skills do not dissolve into some formless void, but skills are always partial and need to be integrated with other partial skills. In the modern world, we have been doing very little of this. And our education does not prepare us sufficiently for this. Once we realize that functional rationality does not exist, then and only then can we begin to achieve substantive rationality.

This is what I believe Prigogine and Stengers (1984) mean when they speak of the 'reenchantment of the world'.[45] It is not to deny the very important task of 'disenchantment', but to insist that we must put the pieces together again. We dismissed final causes too fast. Aristotle was not that foolish. Yes, we need to look at efficient causes, but we need also to look at final causes. The scientists generalized a tactic useful for disentangling themselves from theological and philosophical control systems into a methodological imperative, and this has been disabling.

Finally, the world of knowledge is an egalitarian world. This has been one of the great contributions of science. Anyone is authorized to challenge the veracity of existing statements of truth, provided that they furnish some empirical evidence for the counterstatement, and offer it to everyone for

collective evaluation. But since the scientists refused to be social scientists, they neglected to observe, or even realize, that this virtuous insistence on egalitarianism in science was not possible, was not even credible, in an inegalitarian social world. To be sure, politics arouses fears in scholars, and they seek safely in insulation. Scholars are afraid of the powerful minority, the minority in power. They are afraid of the powerful majority, the majority who might come into power. It will not be easy to create a more egalitarian world. Nonetheless, to achieve the objective that natural science bequeathed the world requires a far more egalitarian social setting that we now have. The struggles for egalitarianism in science and in society are not two separate struggles. They are one and the same, which points once again to the impossibility of separating the search for the true, the good and the beautiful.

Human arrogance has been humanity's greatest self-imposed limitation. This, it seems to me, is the message of the story of Adam in the Garden of Eden. We were arrogant in claiming to have received and understood the revelation of God, to know the intent of the gods. We were even more arrogant in asserting that we were capable at arriving at eternal truth through the use of human reason, so fallible a tool. And we have been continuously arrogant in seeking to impose on each other, and with such violence and cruelty, our subjective images of the perfect society.

In all these arrogances, we have betrayed first of all ourselves, and closed off our potentials, the possible virtues we might have had, the possible imaginations we might have fostered, the possible cognitions we might have achieved. We live in an uncertain cosmos, whose single greatest merit is the permanence of this uncertainty, because it is this uncertainty that makes possible creativity – cosmic creativity, and with that, of course, human creativity. We live in an imperfect world, one that will always be imperfect and therefore always harbor injustice. But we are far from helpless before this reality. We can make the world less unjust, we can make it more beautiful, we can increase our cognition of it. We need but to construct it, and in order to construct it we need but to reason with each other and struggle to obtain from each other the special knowledge that each of us has been able to seize. We can labor in the vineyards and bring forth fruit, if only we try.

My close collaborator, Terence Hopkins, wrote me a note in 1980, which I take as my conclusion: 'There's no place left to go but up, and up, and up, which translates into higher and higher and higher intellectual standards. Elegance. Precision. Short compass. Being right. Enduring. That's all.'

Notes

1 If one looks at one of the very last articles that Weber wrote, 'Politics as a Vocation', delivered as a speech in 1918, Weber specifically identifies himself in the

second sentence as a 'political economist'. Further on in the text, however, he refers to work that 'sociologists must necessarily undertake'. In this latter sentence, one is not sure to what degree he is referring to himself (Weber, 1946: 129, 134).

2 One recent example is by a Canadian sociologist, Ken Morrison: *Marx, Durkheim, Weber: Formations of Modern Social Thought* (Morrison, 1995). Its blurb reads: 'Every undergraduate course focuses on Marx, Durkheim and Weber as the base of the classical tradition in sociological theory.'

3 On the relative decline of Durkheim, and especially of the *Année Sociologique*, see Clark (1968: 89–91).

4 To the view that society is based on a substratum of individual consciousnesses, Durkheim responds:

> Yet what is so readily deemed unacceptable for social facts is freely admitted for other domains of nature. Whenever elements of any kind combine, by virtue of this combination they give rise to new phenomena. One is therefore forced to conceive of these phenomena as residing, not in the elements, but in the entity formed by the union of these elements. . . .
>
> Let us apply this principle to sociology. If, as is granted to us, this synthesis *sui generis*, which constitutes every society, gives rise to new phenomena, different from those which occur in consciousnesses in isolation, one is forced to admit that these specific facts reside in the society itself that produces them and not in its parts – namely its members. (Durkheim, 1982: 38–40)

5 What is exclusively peculiar to social constraint is that it stems not from the unyieldingness of certain patterns of molecules, but from the prestige with which certain representations are endowed. It is true that habits, whether unique to individuals or hereditary, in certain respects possess this same property. They dominate us and impose beliefs and practices upon us. But they dominate us from within, for they are wholly within each one of us. By contrast, social beliefs and practices act upon us from the outside; thus the ascendancy exerted by the former as compared with the latter is basically very different. (Durkheim, 1982: 44)

6 Despite the fact that beliefs and social practices permeate us in this way from the outside, it does not follow that we receive them passively and without causing them to undergo modification. In thinking about collective institutions, in assimilating ourselves to them, we individualise them, we more or less impart to them our own personal stamp. Thus in thinking about the world of the senses each one of us colours it in his [*sic*] own way, and different people adapt themselves differently to an identical physical environment. This is why each one of us creates to a certain extent *his own* morality, *his own* religion, *his own* techniques. Every type of social conformity carries with it a whole gamut of individual variations. It is nonetheless true that the sphere of permitted variations is limited. It is non-existent or very small as regards religious and moral phenomena, where deviations may easily become crimes. It is more extensive for all matters relating to economic life. But sooner or later, even in this last case, one encounters a limit that must not be overstepped. (Durkheim, 1982: 47, footnote 6)

7 In his recent discussion of rational choice theory, William J. Goode notes:

> Ordinarily, sociologists begin with behavior whose aims and goals seem to be clear enough, and we try to find out which variables explain most of the variance. However,

if those variables fail to predict adequately, if for example people choose consistently to act in ways that lower the likelihood that they will achieve what they claim is their material, moral, or esthetic goal, we do not suppose that these people are irrational. Instead, we look at them more closely to locate the 'underlying rationality' of what they are really seeking. (Goode, 1997: 29)

8 In the 1888 Preface added by Engels, he restates the

> ... fundamental proposition which forms [the] nucleus [of the *Manifesto*]. ... That in every historical epoch, the prevailing mode of economic production and exchange, and the social organization necessarily following from it, form the basis upon which is built up, and from which alone can be explained, the political and intellectual history of that epoch; that consequently the whole history of mankind [*sic*] (since the dissolution of primitive tribal society, holding land in common ownership) has been a history of class struggles, contests between exploiting and exploited, ruling and oppressed classes; that the history of these class struggles form a series of evolutions in which, nowadays, a stage has been reached where the exploited and oppressed class – the proletariat – cannot attain its emancipation from the sway of the exploiting and ruling class – the bourgeoisie – without at the same time, and once and for all, emancipating society at large from all exploitation, oppression, class distinctions, and class struggles. (Marx and Engels, 1948: 6)

9 In discussing what happened in France in the period 1848–51, Marx says:

> And as in private life one differentiates between what a man [*sic*] thinks and says of himself and what he really is and does, so in historical struggles one must distinguish still more the phrases and fancies of parties from their real organizations and their real interests, their conceptions of themselves from their reality. (Marx, 1963: 47)

10   [Custom and material advantage] do not form a sufficiently reliable basis for a given domination. In addition there is normally a further element, the belief in *legitimacy*. Experience shows that in no instance does domination voluntarily limit itself to material or affectual or ideal motives as a basis for its continuance. In addition every such system attempts to cultivate the belief in its legitimacy. But according to the kind of legitimacy which is claimed, the type of obedience, the kind of administrative staff developed to guarantee it, and the mode of exercising authority, will all differ fundamentally. (Weber, 1968: 213)

11   In general, it should be kept clearly in mind that the basis of every authority, and correspondingly of every kind of willingness to obey, is a *belief*, a belief by virtue of which persons exercising authority are lent prestige. The composition of this belief is seldom altogether simple. In the case of 'legal authority', it is never purely legal. The belief in legality comes to be established and habitual, and this means that it is partly traditional. Violation of the tradition may be fatal to it. Furthermore, it has a charismatic element, at least in the negative sense that persistent and striking lack of success may be sufficient to ruin any government, to undermine its prestige, and to prepare the way for charismatic revolution. (Weber, 1968: 263)

12   We have learnt from psycho-analysis that the essence of the process of repression lies, not in putting an end to, in annihilating, the idea which represents an instinct, but in preventing it from becoming conscious. (Freud, 1957: 166)

13   A gain in meaning is a perfectly justifiable ground for going beyond the limits of direct experience. . . .

     Just as Kant warned us not to overlook the fact that our perceptions are subjectively conditioned and must not be regarded as identical with what is perceived though unknowable, so psycho-analysis warns us not to equate perceptions by means of consciousness with the unconscious mental processes which are their object. Like the physical, the psychical is not necessarily in reality what it appears to us to be. (Freud, 1957: 167, 171)

14   The ego behaves as if the danger of a development of anxiety threatened it not from the direction of an instinctual impulse but from the direction of a perception, and it is thus enabled to react against the external danger with the attempts at flight represented by phobic avoidances. In this process repression is successful in one particular; the release of anxiety can to some extent be dammed up, but only at a heavy sacrifice of personal freedom. Attempt at flight from the demands of instinct are, however, in general useless, and in spite of everything, the result of phobic flight remains unsatisfactory. (Freud, 1957: 184)

15   The battle with the obstacle of an unconscious sense of guilt is not made easy for the analyst. Nothing can be done against it directly, and nothing indirectly but the slow procedure of unmasking its unconscious repressed roots, and of thus gradually changing it into a *conscious* sense of guilt. . . . It depends principally on the intensity of the sense of guilt; there is often no counteracting force of a similar order of strength which the treatment can oppose to it. Perhaps it may depend, too, on whether the personality of the analyst allows of the patient's putting him [*sic*] in the place of his ego ideal, and this involves a temptation for the analyst to play the part of prophet, saviour and redeemer to the patient. Since the rules of analysis are diametrically opposed to the physician's making use of his personality in any such manner, it must be honestly confessed that here we have another limitation to the effectiveness of analysis; after all analysis does not set out to make pathological reactions impossible, but to give the patient's ego *freedom* to decide one way or the other. (Freud, 1960: 50–1)

16   The initial inspiration . . . lies and remains deeply rooted in the transformation of the world in our time, in the rise to contemporaneity of the Orient – Asia and Africa, together with Latin America. . . .

     The central difficulty facing social theory at the time of Yalta, the climax of Western hegemony, was how to generate ways and means of tackling the hitherto marginalised societies and cultures belonging within the non-Western civilisational moulds. Prepostulated universalism, as a recipe, simply would not do. It was neither able to interpret, from the inside, the specificities at work, nor was it acceptable to the major formative tendencies within the national schools of thought and action. . . .

     A non-temporal social theory can only obtain in the subjectivist epistemological productions of professional ideologists, divorced from the real concrete world, from the objective dialectics of human societies in given historical periods and places, and from the geo-historical formative influences deeply at work in the hidden part of the iceberg. (Abdel-Malek, 1981: xi, xiii)

17   On the other side of the river, the conceptions of the Orient were structured through a different process realised in a totally different environment.

> If we study the historical-geographical constitution of the nations and societies of the Orient – Asia, around China; the Islamic area in Afro-Asia – it will be clear immediately that we have before us the oldest sedentary and stable societies of socio-economic formations in the history of mankind [*sic*]. A group of societies came to be established around the major rivers, facing wide openings to the ocean and sea, thus enabling the pastoral groups to move towards a more stable, agricultural-sedentary mode of production and social existence. . . . It is crucial here to consider the relevance of 'durability', of 'societal maintenance' through centuries and millennia to these objective basic elements. . . .
>
> Time is master. Therefore the conception of time can be said to have developed as a non-analytical vision, as a unitary, symbiotic, unified and unifying conception. Man could no longer 'have' or 'lack' time; time, the master of existence, could not be apprehended as commodity. On the contrary, man was determined and dominated by time. (Abdel-Malek, 1981: 180–1)

18 Abdel-Malek is not rejecting all of Western modernity. Indeed, he adds this warning to the Orient in its confrontation with the West: 'If the Orient wishes to become master of its own destiny, it would do well to ponder the old saying of the martial arts in Japan: "Do not forget that only he [*sic*] who knows the new things while knowing the ancient things, can become a true master"' (Abdel-Malek, 1981: 185).

19 *The End of Certainty* is the title given to the English translation of his work in 1997. But the original French title was *La Fin des certitudes* (1996), and I think the plural form is more consonant with his argument.

20   As is well known, Newton's law [relating force and acceleration] has been superseded in the twentieth century by quantum mechanics and relativity. Still, the basic characteristics of his laws – determinism and time symmetry – have survived. . . .

By way of such equations [such as Schrödinger's equation], laws of nature lead to certitudes. Once initial conditions are given, everything is determined. Nature is an automaton, which we can control, at least in principle. Novelty, choice, and spontaneous action are real only from our human point of view. . . .

The concept of a passive nature subject to deterministic and time-reversible laws is quite specific to the Western world. In China and Japan, nature means 'what is by itself'. (Prigogine, 1997: 11–12)

Note here the similarity to Abdel-Malek's insistence on two different civilizational relations to the time dimension.

21   Probability plays an essential role in most sciences, from economics to genetics. Still, the idea that probability is merely a state of mind has survived. We now have to go a step further and show how probability enters the fundamental laws of physics, whether classical or quantum. . . .

[Arguments that entropy is a measure of ignorance] are untenable. They imply that it is our ignorance, our coarse graining, that leads to the second law [of thermodynamics]. For a well-informed observer, such as the demon imagined by Laplace, the world would appear as perfectly time-reversible. We would be the father of time, of evolution, and not its children. . . .

Our own point of view is that the laws of physics, as formulated in the traditional way, describe an idealized, stable world that is quite different from the unstable,

evolving world in which we live. The main reason to discard the banalization of irreversibility is that we can no longer associate the arrow of time with an increase in disorder. Recent developments in nonequilibrium physics and chemistry point in the opposite direction. They show unambiguously that the arrow of time is a source of *order*.

The constructive role of irreversibility is even more striking in far-from-equilibrium situations where non-equilibrium leads to new forms of coherence. (Prigogine, 1997: 16–17, 25–6)

22   ... our position is that classical mechanics is incomplete because it does not include irreversible processes associated with an increase in entropy. To include these processes in its formulation, we must incorporate instability and nonintegrability. Integrable systems are the exception. Starting with the three-body problem, most dynamical systems are nonintegrable. (Prigogine, 1997: 108)

23   Our thinking constitutes a return to realism, but emphatically not a return to determinism. . . .

Chance, or probability, is no longer a convenient way of accepting ignorance, but rather part of a new, extended rationality. . . .

In accepting that the future is not determined, we come to the end of certainties. Is this an admission of defeat for the human mind? On the contrary, we believe that the opposite is true. . . .

Time and reality are irreducibly linked. Denying time may either be a consolation or a triumph of human reason. It is always a negation of reality. . . . What we have tried to follow is indeed a narrow path [IW: Heed the words: narrow path] between two conceptions that both lead to alienation: a world rule by deterministic laws, which leaves no place for novelty, and a world ruled by a dice-playing God, where everything is absurd, acausal, and incomprehensible. (Prigogine, 1997: 131, 155, 183, 187–8)

24   It is interesting at this point to return to Braudel to see how his formulations, written three decades earlier, use language that is very similar to that of Prigogine. He wishes to describe his attempts to blend 'unity and diversity in the social sciences' by a term he says he borrows from Polish colleagues, that of 'complex studies' (Braudel, 1980c: 61). He describes *histoire événementielle*, the kind he considers to be dust, as 'linear' history (Braudel, 1980b: 67). And he tells us to embrace Gurvitch's view of global society, in a model that reminds us of bifurcations:

[Gurvitch] sees the future of both [the Middle Ages in the West and our contemporary society] as hesitating between several different destinies, all radically different, and this seems to me a reasonable assessment of the variety of life itself; the future is not a single path. So we must renounce the linear. (Braudel, 1980a: 200)

25   I cite two summary statements of what feminist scholarship is about. Constance Jordan (1990: 1): 'Feminist scholarship is predicated on the assumption that women have experienced life differently from men and that difference is worth studying.' And Joan Kelly (1984: 1): 'Women's history has a dual goal: to restore women to history and to restore history to women.'

26   See Joan Kelly (1984: 1) again:

In seeking to add women to the fundaments of historical knowledge, women's history has revitalized theory, for it has shaken the conceptions of historical study. It has done

this by making problematical three of the basic concerns of historical thought: (1) peri-odization, (2) categories of social analysis, and (3) theories of social change.

27   [Reading laws of nature for their personal content uncovers] the personal investment scientists make in impersonality; the anonymity of the picture they produce is revealed as itself a kind of signature. . . . Attention to the intrapersonal dynamics of 'theory choice' illuminates some of the subtler means by which ideology manifests itself in science – even in the face of scientists' best intentions. . . .

   The fact that Boyle's law is not wrong must, however, not be forgotten. Any effective critique of science needs to take due account of the undeniable successes of science as well as of the commitments that have made such successes possible. . . .

   Boyle's law does give us a reliable description, [one] that stands the tests of experimental replicability and logical coherence. But it is crucial to recognize that it is a statement about a particular set of phenomena, prescribed to meet particular interests and described in accordance with certain agreed-upon criteria of both reliability and utility. Judgments about which phenomena are worth studying, which kinds of data are significant – as well as which descriptions (or theories) of those phenomena are most adequate, satisfying, useful, and even reliable – depend critically on the social, linguistic, and scientific practices of the judgments in question. . . .

    . . . scientists in every discipline live and work with assumptions that feel like constants . . . but are in fact variable, and, given the right kind of jolt, subject to change. Such parochialities . . . can only be perceived through the lens of difference, by stepping outside the community. (Keller, 1985: 10–12)

28   . . . it is a thesis of this book that the ideology of modern science, along with its undeniable success, carries within it its own form of projection: the projection of disinterest, of autonomy, of alienation. My argument is not simply that the dream of a completely objective science is in principle unrealizable, but that it contains precisely what it rejects: the vivid traces of a reflected self-image. (Keller, 1985: 70)

29 For Haraway, this 'means embracing the skilful task of reconstructing the boundaries of daily life, in partial connection with others, in communication with all of our parts. . . . This is a dream not of a common language, but of a powerful infidel heteroglossia' (Haraway, 1991: 181).

30 She concludes that

   bodies as objects of knowledge are material-semiotic generative nodes. Their *boundaries* materialize in social interaction. Boundaries are drawn by mapping processes; 'objects' do not pre-exist as such. Objects are boundary projects. But boundaries shift from within; boundaries are very tricky. What boundaries provisionally contain remains generative, productive of meanings and bodies. Siting (sighting) boundaries is a risky practice.

   Objectivity is not about dis-engagement, but about mutual *and* usually unequal structuring, about taking risks in a world where 'we' are permanently mortal, that is, not in 'final' control. (Haraway, 1991: 200–1)

31   The White Man's Burden is becoming increasingly heavy for the earth and especially for the South. The past 500 years of history reveal that each time a relationship of colonization has been established between the North and nature and people outside the North, the colonizing men and society have assumed a position of superiority, and

thus of responsibility for the future of the earth and for other peoples and cultures. Out of the assumption of superiority flows the notion of the white man's burden. Out of the *idea* of the white man's burden flows the *reality* of the burdens imposed by the white man on nature, women and others. Therefore, decolonizing the South is intimately linked to the issue of decolonizing the North. (Shiva, 1993: 264)

32   While science itself is a product of social forces and has a social agenda determined by those who can mobilize scientific production, in contemporary times scientific activity has been assigned a privileged epistemological position of being socially and politically neutral. Thus science takes on a dual character: it offers technological fixes for social and political problems, but absolves and distances itself from the new social and political problems it creates. . . .

The issue of making visible the hidden links between science technology and society and making manifest and vocal the kind of issues that are kept concealed and unspoken is linked with the relationship between the North and the South. Unless and until there can be social accountability of the science and technology structures and the systems to whose needs they respond, there can be no balance and no accountability in terms of relationships between North and South. . . .

To question the omnipotence of science and technology's ability to solve ecological problems is an important step in the decolonization of the North. (Shiva, 1993: 272–3)

33   In social inquiry we . . . want to explain the origins, forms, and prevalence of apparently irrational but culturewide patterns of human belief and action. . . . Only if we insist that science is analytically separate from social life can we maintain the fiction that explanations of irrational social belief and behavior could not ever, in principle, increase our understanding of the world physics explains. . . .

Counting objects and partitioning a line are common social practices, and these practices can generate contradictory ways of thinking about the objects of mathematical inquiry. It may be hard to imagine what gender practices could have influenced the acceptance of particular concepts in mathematics, but cases such as these show that the possibility cannot be ruled out a priori by the claim that the intellectual, logical content of mathematics is free of all social influence. (Harding, 1986: 47, 51)

34 Jensen says in a review of five books on these questions:

Except primatology, mainstream sciences have virtually ignored feminist attempts to rename nature and reconstruct science. Beyond suggesting models and taxonomies that are less hierarchical, more permeable, and more reflexive than the male prototypes . . . it is not clear what feminist revisions and reconstruction of science will entail. Feminist practices may generate new ways of being in the world . . . and thereby give birth to new ways of knowing and describing the world. Or, perhaps the ultimate achievement of the new epistemologies will be to map the limits of language and knowledge; to chart the embeddedness of knowledge in structures of (gendered) power-relations. (Jensen, 1990: 246)

35   What link is there between the work of translation or mediation and that of purification? This is the question on which I should like to shed light. My hypothesis – which remains too crude – is that the second has made the first possible: the more we forbid

ourselves to conceive of hybrids, the more possible their interbreeding becomes – such is the paradox of the moderns. . . . The second question has to do with premoderns, with the other types of culture. My hypothesis – once again too simple – is that by devoting themselves to conceiving of hybrids, the other cultures have excluded their proliferation. It is this disparity that would explain the Great Divide between Them – all the other cultures – and Us – the westerners – and would make it possible finally to solve the insoluble problem of relativism. The third question has to do with the current crisis: if modernity were so effective in its dual task of separation and proliferation, why would it weaken itself today by preventing us from being truly modern? Hence the final question, which is also the most difficult one: if we have stopped being modern, if we can no longer separate the work of proliferation from the work of purification, what are we going to become? My hypothesis – which, like the previous ones, is too coarse – is that we are going to have to slow down, reorient and regulate the proliferation of monsters by representing their existence officially. (Latour, 1993: 12)

36   If an anthropology of the modern world were to exist its task would consist in describing in the same way how all the branches of our government are organized, including that of nature and the hard sciences, and in explaining how and why these branches diverge as well as accounting for the multiple arrangements that bring them together. (Latour, 1993: 14–15)

The subtitle of the original French version, which was left off the English title, is *Essai d'anthropologie symétrique* (Latour, 1991).

37   Because it believes in the total separation of humans and nonhumans, and because it simultaneously cancels out this separation, the Constitution has made the moderns invincible. If you criticize them by saying that nature is a world constructed by human hands, they will show you that it is transcendent, that science is a mere intermediary allowing access to Nature, and that they keep their hands off. If you tell them that Society is transcendent and that its laws infinitely surpass us, they will tell you that we are free and that our destiny is in our own hands. If you object that they are being duplicitous, they will show you that they never confuse the Laws of Nature with imprescriptible human freedom. (Latour, 1993: 37)

I have corrected a howler of a mistranslation by referring to the French original (Latour, 1991: 57). In the English text, the third sentence reads, quite incorrectly: 'If you tell them that we are free and that our destiny is in our own hands, they will tell you that Society is transcendent and its laws infinitely surpass us.'

38 Latour further clarifies this paradox by looking at its expression in the world of knowledge:

Social scientists have for long allowed themselves to denounce the belief system of ordinary people. They call this belief system 'naturalization'. Ordinary people imagine that the power of gods, the objectivity of money, the attraction of fashion, the beauty of art, come from some objective properties intrinsic to the nature of things. Fortunately, social scientists know better and they show that the arrow goes in the other direction, from society to the objects. Gods, money, fashion and art offer only a surface for the projection of our social needs and interests. At least since Emile Durkheim, such has been the price of entry into the sociology profession.

> The difficulty, however, is to reconcile this form of denunciation with another one in which the directions of the arrows are exactly reversed. Ordinary people, mere social actors, average citizens, believe they are free and that they can modify their desires, their motives and their rational strategies at will....But fortunately, social scientists are standing guard, and they denounce, debunk and ridicule this naive belief in the freedom of the human subject and society. This time they use the nature of things – that is the indisputable results of the sciences – to show how it determines, informs and moulds the soft and pliable wills of the poor humans. (Latour, 1993: 51–3)

39   Again an error in the translation. The English text reads 'past perfect tense', but this is a mistranslation. The French text reads 'passé composé'.

40   . . . the moderns have been victims of their own success. . . . Their Constitution could absorb a few counter-examples, a few exceptions – indeed, it thrived on them. But it is helpless when the exceptions proliferate, when the third estate of things and the Third World join together to invade all its assemblies, *en masse* . . . the proliferation of hybrids has saturated the constitutional framework of the moderns. (Latour, 1993: 49–51)

41   See Deborah Gold (1996: 224):

> For the last several decades, sociology has become a discipline of ultra-specialization. Although sociologists may think we are giving our graduate students a broad sociological education, in truth, by example, we encourage students to narrow their areas of expertise. Unfortunately, this parochialism means that many sociologists are unaware of what is current in specializations not their own. If sociology continues this approach, we can hardly expect to inspire a 21st century Talcott Parsons or Robert Merton who could take a broader perspective. Instead, sociologists in the future are likely to configure their areas of expertise even more narrowly.

It is worthy of note that this peroration was published in a quite specialized journal, *The Gerontologist*.

42   We might even say that the model of sociologically naive actors – as in rational choice and game theoretical models – are misguided for almost all occasions. Our typifications and explanations must involve the continuous interaction of institutionalized expectations, perceptions, interpretations, affects, distortions, and behavior. (Smelser, 1997: 27)

43   Historian, the one who knows? No, the one who searches. (Febvre, 1950: v)

44   It seems to me that uncertainty is the essential issue Neil Smelser was addressing in his 1997 Presidential Address to the American Sociological Association when he discussed 'ambivalence', a term he borrowed from Merton. He discussed it however primarily as a psychological constant in terms of actors' motivations rather than as a structural constant of the physical world. He does however draw a conclusion with which I heartily agree: 'We might even suggest that ambivalence forces us to reason even more than preferences do, because conflict may be a stronger motive for thinking than is desire' (Smelser, 1998: 7).

45   [The concept of the disenchantment of the world] is paradoxically due to the glorification of the earthly world, henceforth worthy of the kind of intellectual pursuit Aristotle reserved for heaven. Classical science denied becoming, natural diversity,

both considered by Aristotle attributes of the sublunar, inferior world. In this sense, classical science brought heaven to earth. . . .

The radical change in the outlook of modern science, the transition toward the multiple, may be viewed as the reversal of the movement that brought Aristotle's heaven to earth. Now we are bringing earth to heaven. (Prigogine and Stengers, 1984: 305–6)