"Social Science and the Quest for a Just Society"

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Macro and micro constitute an antinomy that has long been widely used throughout the social sciences, and indeed in the natural sciences as well. In the last twenty years, the antinomy global/local has also come into wide use in the social sciences. A third pair of terms, structure/agency, has also come to be widely adopted, and is central to the recent literature of cultural studies. The three antinomies are not exactly the same, but in the minds of many scholars they overlap very heavily, and as shorthand phrases they are often used interchangeably.

Macro/micro is a pair which has the tone merely of preference. Some persons prefer to study macrophenomena, others microphenomena. But global/local, and even more structure/agency, are pairs that have passions attached to them. Many persons feel that only the global or only the local make sense as frameworks of analysis. The tensions surrounding structure/agency are if anything stronger. The terms are often used as moral clarion calls; they are felt by many to indicate the sole legitimate rationale for scholarly work.

Why should there be such intensity in this debate? It is not difficult to discern. We are collectively confronted with a dilemma that has been discussed by thinkers for several thousand years. Beneath these antinomies lies the debate of determinism versus free will, which has found countless avatars within theology, within philosophy, and within science. It is therefore not a minor issue, nor is it one about which, over the thousands of years, a real consensus has been reached. I believe that our inability to find a way beyond this opposition constitutes a major obstacle to our collective ability to create a form of knowledge that is adequate for what I expect will be a quite transformed world in the coming century and millennium. I therefore propose to look at how this long-standing debate has been conducted within our community, that is, within the framework of that very recent construct, "social science." I intend to argue that the way the problem has been posed heretofore has made it insoluble. I intend also to argue that we are today at a point where we may be able to overcome the social constructions of the nineteenth century in ways that will allow us to move forward constructively, and collectively, on this question.

Let me start with determinism and free will in theological discourse. The concept that everything is determined seems to derive quite directly from the concept of the omnipotence of God, central to all the monotheistic religions at least. If there is an omnipotent God, then everything is determined by the will of God, and to suggest otherwise would seem to be blasphemous. On the other hand, the churches of the world are in the business of regulating moral behavior. And determinism provides an easy excuse for the sinner. Has God indeed determined that we shall sin? And if so, should we try to counter the will of God? This is a conundrum that has plagued theologians from the beginning. One way out is to argue that God has bestowed upon us free will, that is, the option to sin or not to sin. It is however too easy a solution. Why would it have been necessary or desirable for God to have done this? It makes us seem like God's playthings. Furthermore, it does not provide a logically tight argument. If God has given us free will, can we exercise it in unpredictable ways? If so, is God omnipotent? And if not, can we really be said to have free will?

Let me say once again how impressed I have always been with the astuteness of Calvin's attempt to resolve this dilemma. The Calvinist argument is very simple. Our destinies are indeed not predetermined, not because God could not predetermine everything, but because if humans assert that everything is predetermined, they are thereby limiting God's ability to determine. In effect, Calvin is saying, perhaps we cannot change our minds, but God can, or else God is not omnipotent. Still, as you well know, Calvinists were not persons to countenance immoral behavior. How then could humans be induced to make the necessary effort to behave according to the norms which Calvinists believed they ought to observe? Remember, Calvin was part of the Reformation attempt to refute the doctrine of the Catholic
Church that good deeds are rewarded by God (a view that, by derivation, justified the sale of indulgences). To get out of the box, Calvinists resorted to the concept of negative grace, which is in reality a familiar and very modern device of science, the concept of disproof. While we could not have foreknowledge of who was saved, since that would limit God's decisions, we could have foreknowledge of who was not saved. It was argued that God displays the prospect of damnation in the sinful behavior of humans, as sinful behavior is defined by the church. Those who sin are surely not saved, because God would not permit the saved so to act.

The Calvinist solution is so astute that it was subsequently adopted by its successor expression, the revolutionary movements of the nineteenth and twentieth centuries. The analogous argument went like this. We cannot know for sure who is advancing the revolution but we can know for sure who is not advancing it, those who act in ways that are sinful, that is, in ways that run counter to the decisions of the revolutionary organization. Every member is a potential sinner, even if the militant has behaved appropriately in the past. Members are thus continuously subject to the judgment of the revolutionary authorities as to whether or not they have gone against the will of God, that is, against the will of the revolutionary organization.

Nor was it only the revolutionary organizations that adopted the Calvinist solution. Essentially, modern science adopted it as well. We can never know with certainty whether a scientist has reached truth, but we can know when the scientist has sinned. It is when he has failed to follow the norms of appropriate scientific methods, as defined by the community of scientists, and therefore has ceased to be "rational," that is, when the scientist has stooped to politics, or to journalism, or to poetry, or to other such nefarious activities.

The Calvinist solution is astute, but it has one enormous drawback. It confers inordinate power on those humans church authorities, revolutionary authorities, scientific authorities who are the interpreters of whether or not other human actors are showing signs of negative grace. And who will guard the guardians? Is there then a remedy to this drawback? The consecrated remedy is to proclaim the virtue of human freedom. That good Calvinist, John Milton, wrote a marvelous poem extolling this remedy. It was called *Paradise Lost*. There are many readers who have said that, behind Milton's ostensible vindication of God, his real hero was Lucifer, and that Lucifer's rebellion represented humanity's attempt to rise up against the constraint of the will of an unseeable and unknowable God. But the remedy seems almost as bad as the malady. Shall we praise Lucifer? After all, in whose interests does he act?

I have come to bury Caesar, not to praise him.

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Consider the Enlightenment. What was the sermon? It seems to me the essential message was anticlerical: humans were capable of rational judgment and hence had the ability to arrive at both truth and goodness directly, through their own best efforts. The Enlightenment represented the definitive rejection of religious authorities as judges of either truth or goodness. But who were substituted for them? I suppose one has to say the philosophers. Kant was anxious to take away from the theologians the right to judge either truth or goodness. He found it easy enough to do this for truth, but more difficult to do for goodness. Having decided that one cannot prove laws of morality as though they were laws of physics, he might have conceded goodness to the theologians. But no, he insisted that here too the philosophers could offer the answer, which for Kant was located in the concept of the categorical imperative.

However, in the process of secularizing knowledge, the philosophers enshrined doubt, and this proved to be their own subsequent undoing. For along came the scientists to proclaim that the philosophers were merely disguised theologians. The scientists began to challenge the right of philosophers as well as of theologians to proclaim truth, asserting very stridently that scientists were not philosophers. Is there anything, the scientists asked, that legitimates the speculations, the ratiocinations of the philosophers, anything that allows us to say that they are true? The scientists asserted that they, on the contrary, possessed a firm basis for truth, that of empirical investigation leading to testable and tested hypotheses, to those provisional universals called scientific theorems. The scientists, however, unlike Kant, wiser or perhaps less courageous than Kant, wanted nothing to do with moral laws. They laid claim therefore to only one-half of the task the philosophers had inherited from the theologians. Scientists would search
only for truth. As for goodness, they suggested that it was uninteresting to search for it, asserting that
goodness was incapable of being an object of knowledge as science was defining knowledge.

The claims of the scientists that science represented the unique path to locate truth gained wide cultural
support, and they came to be the preeminent constructors of knowledge in the course of the late
eighteenth and early nineteenth centuries. However, at that very moment, there was a small happening
called the French Revolution, a happening whose protagonists claimed they were acting in the
furtherance of goodness. Ever since, the French Revolution has served as the source of a belief system
at least as powerful as that provided by the rise to cultural predominance of science. As a result, we have
spent the last 200 years trying to reunite the search for truth and the search for goodness. Social science,
as it came to be established during the nineteenth century, was precisely the heir to both searches, and in
some ways offered itself as the ground on which they could be reconciled. I must however admit that
social science has not been very successful in its quest since, rather than reunifying them, it has itself
been torn apart by the dissonance between the two searches.

The centrifugal pressure of the "two cultures" (as we now call them) has been impressively strong. It has
provided the central themes of the rhetoric of public discourse about knowledge. It has determined
the structures of the universities in the course of their being rebuilt and reinvigorated in the nineteenth
century. Its continuing strength explains the persistingly high degree of passion about the antinomies to
which I referred. It explains the fact that social science has never achieved true autonomy as an arena of
knowledge nor ever acquired the degree of public esteem and public support to which it aspires and
which it believes it merits.

The gulf between the "two cultures" was the deliberate construction of Newtonian-Cartesian science.
Science was very sure of itself in this struggle. This is well illustrated by two famous declarations of the
Marquis de Laplace. One was his bon mot in replying to Napoleon's query about the absence of God in
his physics "Sire, I have not found any need for that hypothesis" (cited in Koyré 1957, 276). The other
was his unyielding statement about how much science could know:

The present state of the system of nature is evidently a resultant of what it was in the
preceding instant, and if we conceive of an Intelligence who, for a given moment, embraces
all the relations of beings in the Universe, It will be able to determine for any instant of the
past or future their respective positions, motions, and generally their affections (cited in
Hahn 1967, 15).

Triumphant science was not prepared to admit any doubts or to share the stage with anyone else.

Philosophy, and more generally, what came to called in the nineteenth century the humanities, fell in
public esteem and retreated to a defensive stance. Unable to deny science's capacity to explain the
physical world, they abandoned that domain entirely. Instead, they insisted that there existed another
quite separate domain the human, the spiritual, the moral which was as important as, if not more
important than, the domain of science. That is why, in English at least, they assumed the label of the
humanities. From this human domain they sought to exclude science, or at the very least relegate it to a
very secondary role. As long as the humanities engaged in metaphysics or literature, science was quite
willing to allow itself to be excluded, on the deprecatory grounds that these were non-scientific matters.
But when the subject matter was the description and analysis of social reality, there was no accord, even
a tacit one, between the two camps. Both cultures laid claim to this arena.

A cadre of professional specialists on the study of social reality emerged slowly and, be it said,
unsurely. In many ways, the most interesting story is that of history. Of all the fields that we today call
social science, history has the longest lineage. It was a concept and a term long before the nineteenth
century. But the basis of the modern discipline of history was the historiographical revolution we
associate with Leopold von Ranke. And the modern version of history, which Ranke and his colleagues
called Geschichte and not Historie, was extraordinarily scientific in its fundamental premises. Its
practitioners asserted that social reality was knowable. They asserted that such knowledge could be
objective; that is, that there were correct and incorrect statements about the past, and that historians were
obliged to write history "as it really happened," which is why they gave it the name of Geschichte. They
asserted that scholars must not intrude their biases into the analysis of the data or its interpretation.
Hence they asserted that scholars must offer evidence for their statements, evidence based on empirical
research, evidence subject to control and verification by the community of scholars. Indeed, they even
defined what kind of data would be acceptable evidence (primary documents in archives). In all these
ways they sought to circumscribe the practices of the "discipline" and eliminate from history anything
that was "philosophical," that is, speculative, deductive, mythical. I have called this attitude "history in
search of science" (Wallerstein 1996a).

But historians proved in practice to be timid scientists. They wished to stick extremely close to their data, and to restrict causal statements to statements of immediate sequences immediate particular sequences. They balked at "generalizations," which is what they called either inductions of patterns of behavior from specific instances or assertions of causal sequences in which two variables were less immediately linked in time and space. We may be generous and say they did this because they were sensitive to the thin basis the collected empirical data in the nineteenth century afforded them for sound inductions. In any case, they were haunted by the fear that to generalize was to philosophize, that is, to be antiscientific. And so they came to idolize the particular, the idiographic, even the unique, and thereupon to shun, for the most part, the label of social science, despite the fact that they were "in search of science."

Other practitioners were more audacious. The emerging disciplines of economics, sociology, and political science by and large wrapped themselves in the mantle and the mantra of "social science," appropriating the methods and the honors of triumphant science (often be it noted to the scorn and/or despair of the natural scientists). These social science disciplines considered themselves "nomothetic," in search of universal laws, consciously modeling themselves on the good example of physics (as nearly as they could). They had, of course, to admit that the quality of their data and the plausibility/validity of their theorems were far beneath the level achieved by their confreres in the physical sciences, but they defiantly asserted optimism about future progress in their scientific capacities.

I should like to underline that this great Methodenstreit, as it was called, between idiographic history and the nomothetic trio of "real" social sciences was in many ways huff and puff, since both sides of this disciplinary and methodological debate fully acknowledged the superiority of science over philosophy. Indeed, science might have won the battle for the soul of the social sciences hands down, were the natural scientists not rather snobbish in refusing to accept the importuning social scientists into full membership in the fraternity.

History and the nomothetic trio remained up to 1945 very much social sciences of the civilized world, by the civilized world, and about the civilized world. To deal with the colonized world of what were called primitive peoples, a separate social science discipline was constructed, anthropology, with its separate set of methods and traditions. And the remaining half of the world, that of non-Western, so-called "high civilizations" that is, China, India, the Arabo-Islamic world among others was left to a special group of persons engaged in something that was given the name of "Oriental studies," a discipline that insisted on its humanistic character and refused to be considered part of the social sciences. It is obvious today why a cleavage between a social science of and for the civilized world and a second social science of and for the rest of the world seemed so natural to nineteenth-century European scholars, and why it seems so absurd today. I shall not dwell on this issue (see Wallerstein et al., 1996b). I wish merely to note that both the anthropologists and the Orientalist scholars, by virtue of the logic of engaging in a social science about the others/the non-modern world/the barbarians, felt very much more comfortable on the idiographic side of the Methodenstreit, since the universalist implications of nomothetic social science seemed to leave no place for what they wanted to say.

In the nineteenth century, the idigraphers and the nomothetists were in great competition as to who could be more objective in their work, which had a strange consequence for the macro-micro distinction. If one looks at the earliest works and major figures in each of these emerging disciplines, one notices that they wrote about very large themes, such as universal history or stages of civilization. And the titles of their books tended to be all-encompassing. This fit in very well with the turn that modern thought was taking in that century, the turn to evolution as the fundamental metaphor. These books were very "macro" in the sweep of their subject matter, and they described the evolution of mankind. They were seldom monographic. But this "macro" quality of the research did not seem to last very long.

In the interests of creating corporate structures, the various social science disciplines sought to control the training and career patterns of those who would enter the fraternity. They insisted on both originality and objectivity, and this turned them against macro-scholarship. Originality required that each successive scholar say something new, and the easiest way to do that was to divide up the subject matter into subjects of ever smaller scope, in terms of time, of space, and of variables under consideration. The process of subdivision opened up endless possibilities of not repeating the work of earlier scholars. And by circumscribing the scope, they believed they were making it more possible for scholars to be careful in their collection and analysis of data. It was the mentality of the microscope, and it pushed scholars to using ever more powerful microscopes. It fit in well with a reductionist ethos.

This microscopization of social science reinforced the gulf between idiographic and nomothetic social
science. The two camps were equally in search of objectivity, but pursued diametrically opposite paths to achieving it, because they singled out opposite risks of subjectivity. The idiographic camp had two principal fears. They saw the danger of subjectivity deriving on the one hand from inadequate contextual understanding and on the other hand from the intrusion of self-interest. Insofar as one was dependent upon primary documents, one was obliged to read them correctly, and not anachronistically or from the prism of another culture. This required considerable knowledge of the context: the empirical detail, the definition of boundaries, the use of the language (even in many cases the handwriting), the cultural allusions in the documents. The scholars hence sought to be hermeneutic, that is, to enter into the mentality of persons and groups who were remote from them, and to try to see the world as the persons under study saw it. This required long immersion in the language and culture under observation. For the historians, it seemed easiest therefore to study their own nation/culture, in which they were already immersed. For the anthropologists, who by definition could not follow this path, it required so great an investment to know enough to study a particular group of "others" that it seemed sensible to devote one's life work to the study of one such people. And for the Orientalist scholars, doing well their philological exercises required a lifelong improvement of difficult linguistic skills. There were thus, for each field, objective pressures which led scholars to narrow the scope of their research, and to attain a level of specialization at which there were at most a few other persons in the world who had a matching profile of skills.

The problem of non-involvement was also a serious one for idiographic scholars. The historians solved it first of all by insisting that history could not be written about the present and then by ending the "past" at a point relatively distant from the present. The argument was that we are all inevitably committed politically in the present, but that as we move backwards in time we may feel less involved. This was reinforced by the fact that historians made themselves dependent upon archives, and the states which provided the materials for the archives were (and are) unwilling to make the documents available about current happenings, for obvious reasons. The Orientalist scholars ensured their neutrality by avoiding real intercourse with the civilizations they studied. Theirs being primarily a philological discipline, they were immersed in reading texts, a task they could and largely did conduct in their study. As for the anthropologists, the great fear of the discipline was that some colleagues would "go native," and thereby be unable to continue to play the role of the scientific observer. The main control employed was ensuring that the anthropologist did not stay out in the "field" too long. All of these solutions emphasized remoteness as the mechanism of controlling bias. In turn validity was guaranteed by the interpretative skills of carefully-trained scholars.

The nomothetic trio of economics, political science, and sociology turned these techniques on their head. They emphasized not remoteness but closeness as the road to avoiding bias; but it was a very particular kind of closeness. Objective data were defined as replicable data, that is, precisely data that were not the result of an "interpretation." The more quantitative the data the easier it was to replicate them. But data from the past or from remote parts of the world lacked the infrastructural basis for the necessary guarantees of quality, of "hardness." Quite the opposite: the best data were the most recent, and collected in the countries with the best infrastructure for the recording of data. Older or remoter data were necessarily incomplete, approximate, perhaps even mythical. They might be sufficient for the purposes of journalism or travel reports but not for science. Furthermore, even newly-collected data rapidly became obsolete, since the passing of time brought ever-increased quality of data collection, especially in terms of the comparability of data collected in two or more sites. So the nomothetic trio retreated into the present, even into the immediate and instantaneous present.

Furthermore, insofar as one wanted to perform sophisticated operations on quantitative data, it was optimal to reduce the number of variables, and to use indicators about which one could collect good data, hard data. Thus, reliability pushed these social scientists into constantly narrowing the time and space scope of the analyses and into testing only carefully-limited propositions. One might wonder then about the validity of the results. But the epistemological premises solved this problem. Insofar as one believed that there existed universal laws of human behavior, the locus of the research became irrelevant. One chose sites of data collection according to the quality of the data it was possible to obtain, not because of their superior relevance.

I draw from this the conclusion that the great methodological debates that illustrated the historical construction of the social sciences were sham debates, which distracted us from realizing the degree to which the "divorce" between philosophy and science effectively eliminated the search for the good from the realm of knowledge, and circumscribed the search for truth into the form of a microscopic positivism which took on many guises. The early hopes of social scientists that they could be modern philosopher-kings proved totally vain and social scientists settled into being the handmaidens of governmental reformism. When they did this openly, they called it applied social science. But for the
most part they did this abashedly, asserting that their role was merely to do the research, and that it was up to others, the political persons, to draw from this research the conclusions that seemed to derive from this research. In short, the neutrality of the scholar became the fig-leaf of their shame, in having eaten the apple of knowledge.

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As long as the modern world seemed to be one long success story of technological triumph, the necessary political base to maintain a certain equilibrium in the system continued to exist. Amidst the success, the world of science was carried from honor to honor within this system, as though it were responsible for the triumph. The social sciences were swept along in the tide. No one was seriously questioning the fundamental premises of knowledge. The many maladies of the system from racism to sexism to colonialism as expressions of the manifestly growing polarization of the world, from fascist movements to socialist gulags to liberal formalisms as alternative modes of suppressing democratization were all defined as transitory problems because they were all thought to be capable of being brought under control eventually, as so many turbulent deviations from the norm, in a world in which the trajectory always returned to the curve of linear upward-moving equilibrium. The political persons on all sides promised that goodness was coming at the end of the horizon, a prospect presumably guaranteed by the continual progress in the search for truth.

This was an illusion, the illusion bred by the separation and reification of the two cultures. Indeed the separation of the two cultures was one of the main factors pushing the trajectories far from equilibrium. Knowledge is in fact a singular enterprise, and there are no fundamental contradictions between how we may pursue it in the natural and in the human world, for they are both integral parts of a singular universe. Nor is knowledge separate from creativity or adventure or the search for the good society. To be sure, knowledge will always remain a pursuit, never a point of arrival. It is this very fact, however, that permits us to see that macro and micro, global and local, and above all, structure and agency are not unsurpassable antinomies but rather yin and yang.

There have been two remarkable intellectual developments of the last two decades that constitute an entirely new trend, signs that the world may be now in the process of overcoming the two cultures. These trends are only marginally the doing of social scientists, but they are wonderfully encouraging about the future of social science. I refer to what has been called complexity studies in the natural sciences and what has been called cultural studies in the humanities. I am not going to review the now immense literature in each of these two fields. Rather I shall try to situate each of these fields in terms of their epistemological implications for knowledge and their implications for the social sciences.

Why are complexity studies given that name? It is because they reject one of the most basic premises of the modern scientific enterprise. Newtonian science assumed that there were simple underlying formulas that explained everything. Einstein was unhappy that $e=mc^2$ explained only half the universe. He was searching for the unified field theory that would in an equally simple equation explain everything. Complexity studies argue that all such formulae can at best be partial, and at most explain the past, never the future. (We must of course be careful to distinguish between the dubious belief that truth is simple and the sound methodological injunction of Occam's razor, that we ought always to try to eliminate logical curlicues from our reasoning and include in our equations only the terms necessary to stating them clearly.)

Why is truth complex? Because reality is complex. And reality is complex for one essential reason: the arrow of time. Everything affects everything, and as time goes on, what is everything expands inexorably. In a sense, nothing is eliminated, although much fades or becomes blurred. The universe proceeds, it has a life, in its orderly disorder or its disorderly order. There are of course endless provisional orderly patterns, self-established, holding things together, creating seeming coherence. But none are perfect, because of course perfect order is death, and in any case enduring order has never existed. Perfect order is what we may mean by God, which is by definition beyond the known universe. So the atoms, the galaxies, and the biota pursue their paths, their evolution if you will, until the internal contradictions of their structures move them further and further away from whatever temporary equilibria they enjoy. These evolving structures repeatedly reach points at which their equilibria can no longer be restored, at points of bifurcation, and then new paths are found, new orders established, but we can never know in advance what these new orders will be.
The picture of the universe that derives from this model is an intrinsically non-deterministic one, since the aleatory combinations are too many, the number of small decisions too many, for us to predict where the universe will move. But it does not follow that the universe can therefore move in any direction whatsoever. It is the child of its own past, which has created the parameters within which these new paths are chosen. Statements about our present trajectories can of course be made, and can be made carefully, that is, can be stated quantitatively. But if we try to overdo the accuracy of the data, the mathematicians tell us we get unstable results.\[1\]

[1. The crystal has been shattered, Ivar Ekeland tells us: "The qualitative approach is not a mere stand-in for quantitative methods. It may lead to great theoretical advances, as in fluid dynamics. It also has a significant advantage over quantitative methods, namely, stability" (Ekeland 1988, 73).]

If physical scientists and mathematicians are now telling us that truth in their arena is complex, indeterminate, and dependent on an arrow of time, what does that mean for social scientists? For, it is clear that, of all systems in the universe, human social systems are the most complex structures that exist, the ones with the briefest stable equilibria, the ones with the most outside variables to take into account, the ones that are most difficult to study.

We can only do what the natural scientists can only do. We can search for interpretative patterns, of two sorts. We can search for what might be called formal interpretative patterns, of the kind that state, for example, that all human social systems are historical social systems, not only in the sense that they follow an historical trajectory, but in the sense that they are born or emerge at certain times and places for specific reasons, operate according to specific sets of rules for specific reasons, and come to a close or die or disintegrate at certain times and places because they are unable any longer to handle their contradictions for specific sets of reasons. Such formal interpretative patterns are of course themselves subject to a finite relevance. One day, a given particular formal pattern may no longer operate, though for the moment this day may seem remote.

We can also search however for what might be called substantive interpretative patterns, such as the description of the rules of a particular historical social system. For example, when I term the modern world-system a capitalist world-economy, I am laying claim to the existence of a particular substantive pattern. It is of course a debatable one, and it has been much debated. Furthermore, like a series of boxes within boxes, there are substantive patterns within substantive patterns, such that, even if we all agree that the world in which we live is a capitalist world-economy, we may nonetheless disagree about whether it has had discernible stages, or whether unequal exchange has been its norm, or about endless other aspects of its functioning.

What is crucial to note about complexity studies is that they have in no sense rejected scientific analysis, merely Newtonian determinism. But in turning some premises on their head, and in particular by rejecting the concept of reversibility in favor of the concept of the arrow of time, the natural sciences are taking a giant's step in the direction of the traditional terrain of social science, the explanation of reality as a constructed reality.

If we now turn to cultural studies, let us start with the same question. Why are they called cultural studies? For a group of scholars so taken with linguistic analysis, to my knowledge this question has never been posed. The first thing I note is that cultural studies are not really studies of culture but studies of cultural products. This is the consequence of their deep root in the humanities, and explains in turn the domain of cultural products.

They were also attributed the domain of goodness, but they were very reluctant to seize hold of it. It seemed so political, so uncultural, so fleeting and unsolid, so lacking in eternal continuities. The personal path of Wordsworth from poet of the French Revolution to poet of poetry illustrates the repeated flight of the artists and the scholars of cultural products to the surer ground of "art for art's sake," an aesthetic turning inward. They comforted themselves with Keats's lines in Ode on a Grecian Urn, "®Beauty is truth; truth beauty" that is all Ye know on earth, and all ye need to know."

To be sure, there were always those who asserted that cultural products were a product of the culture, and that this could be explained in terms of the structures of the system. Indeed, cultural studies as we know it today originated in England in the 1950's with persons who were arguing this longstanding theme. They were, let us remember, in search of a workers' culture. But then cultural studies took what has been called a linguistic turn or a hermeneutic turn, but which I think of as a 1968-turn. The revolutions of 1968 were against the liberal center, and put forward the argument that not only was the...
Old Left part of this liberal center, but also that this liberal center was as dangerous as (if not more dangerous than) the true conservatives.

In terms of the study of cultural products, it meant that the enemy became not merely those who would study cultural products according to conservative, traditional aesthetic norms (the so-called canons) but as well against those (the Old Left) who would analyze cultural products in terms of their presumed explanations in the political economy. An explosion followed, in which everything was deconstructed. But what is this exercise? It seems to me the core of it is to assert the absence of absolute aesthetics, to insist that we have to explain how particular cultural products were produced when they were produced and why in that form, and then to proceed to ask how they were and are being received by others, and for what reasons.

We are clearly involved here in a very complex activity, one in which equilibria (canons) are at best transient and one in which there can be no determinate future, since the aleatory elements are too vast. In the process, the study of cultural products has moved away from the traditional terrain of the humanities and onto the terrain of the social sciences, the explanation of reality as a constructed reality. This is of course one of the reasons why so many social scientists have been receptive to it.

The move of natural scientists towards the social sciences (complexity studies) and the move of scholars in the humanities towards the social sciences (cultural studies) has not been without opposition within the natural sciences and within the humanities. The opposition has in fact been ferocious, but it seems to me that it has been largely a rear-guard operation. Nor have the proponents of complexity studies or the proponents of cultural studies defined themselves as moving into the camp of the social sciences. Nor have all (or even most) social scientists analyzed the situation in this way.

But it is time that we all call a spade a spade. We are in the process of overcoming the two cultures via the social scientization of all knowledge, by the recognition that reality is a constructed reality, and that the purpose of scientific/philosophical activity is to arrive at usable, plausible interpretations of that reality, interpretations that will inevitably be transitory but nonetheless correct, for their time, than alternative interpretations. But, if reality is a constructed reality, the constructors are the actors in the real world, and not the scholars. The role of the scholars is not to construct reality but to figure out how it has been constructed, and to test the multiple social constructions of reality against each other. In a sense, this is a game of never-ending mirrors. We seek to discover the reality on the basis of which we have constructed reality. And when we find this, we seek to understand how this underlying reality has in turn been socially constructed. In this navigation amidst the mirrors, there are however more correct and less correct scholarly analyses. Those scholarly analyses that are more correct are more socially useful in that they aid the world to construct a substantively more rational reality. Hence the search for truth and the search for goodness are inextricably linked the one to the other. We are all involved, and involved simultaneously, in both.

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In his latest book, Ilya Prigogine says two things very simply. "The possible is richer than the real. Nature presents us in effect with the image of creation, of the unforeseeable, of novelty." And "science is a dialogue with nature" (Prigogine 1996, 83, 177). I should like to take these two themes as the basis of my concluding remarks.

The possible is richer than the real. Who should know this better than social scientists? Why are we so afraid of discussing the possible, of analyzing the possible, or exploring the possible? We must move not utopias, but utopistics, to the center of social science. Utopistics is the analysis of possible utopias, their limitations, and the constraints on achieving them. It is the analytic study of real historical alternatives in the present. It is the reconciliation of the search for truth and the search for goodness.

Utopistics represents a continuing responsibility of social scientists. But it represents a particularly urgent task when the range of choice is greatest. When is this? Precisely when the historical social system of which we are a part is furthest from equilibrium, when the fluctuations are greatest, when the bifurcations are nearest, when small input has great output. This is the moment in which we are now living and shall be living for the next 25-50 years.{2}
If we are to be serious about utopistics, we must stop fighting about non-issues, and foremost of these non-issues is determinism versus free will, or structure versus agency, or global versus local, or macro versus micro. It seems to me that what we can now see clearly is that these antinomies are not a matter of correctness, or even of preference, but of timing and depth of perspective. For very long and very short time spans, and from very deep and very shallow perspectives, things seem to be determined, but for the vast intermediate zone things seem to be a matter of free will. We can always shift our viewing angle to obtain the evidence of determinism or free will that we want.

But what does it mean to say that something is determined? In the realm of theology, I can understand it. It means we believe that there is an omnipotent God, and that he has determined everything. Even there, we get quickly into trouble as I have suggested. But at least, as Aristotle would have put it, we are dealing with an efficient cause. But if I say that the possibility of reducing unemployment in Europe in the next ten years is determined, who or what is doing this determining, and how far back shall I trace it? Even if you were to convince me that this had some analytical meaning (and that would be difficult), does it have any practical relevance? But does it follow then that it is merely a matter of free will, and that, were Dutch or German or French politicians, or entrepreneurs, or trade-union leaders or someone to do specific things, then I could assure you that unemployment would in fact be reduced? Even if they, or I, knew what these things were, or believed we knew, what would motivate us to do them now when we did not do them previously? And if there were an answer to this, does that mean that our free will is determined by something prior? And if so, what? This is an endless, pointless, sequential chain. Starting with free will, we end up with determinism, and starting with determinism, we end up with free will.

Can we not approach this another way? Let us agree that we are trying to make sense of the complexity, to "interpret" it usefully and plausibly. We could start with the simple task of locating seeming regularities. We could also try provisionally to assess the relative strength of various constraints on individual and collective action. This task we might call locating structures of the longue durée. I call this a simple task, but of course it is not at all an easy task. It is simple rather in the sense that it explains little, and also in the sense that it is a prior task, prior that is to other more complex tasks. If we don't have the structures clearly in mind, we cannot go on to analyze anything more complex, like for example so-called micro-histories, or texts, or voting patterns.

Analyzing structures does not limit whatever agency exists. Indeed, it is only when we have mastered the structures, yes have invented "master narratives" that are plausible, relevant, and provisionally valid, that we can begin to exercise the kind of judgment that is implied by the concept of agency. Otherwise, our so-called agency is blind, and if blind it is manipulated, if not directly then indirectly. We are watching the figures in Plato's cave, and are thinking that we can affect them.

This brings me to Prigogine's second apothegm: "Science is a dialogue with nature." A dialogue has two partners. Who are they in this case? Is science a scientist or the community of scientists or some particular scientific organization(s) or is it everyman insofar as he or she is a thinking being? Is nature a living entity, some sort of pantheistic god, or God omnipotent? I do not think we know for sure who is engaged in this dialogue. The search for the partners in the dialogue is part of the dialogue itself. What we must hold constant is the possibility of knowing more and of doing better. This remains only a possibility, but not an unattainable one. And the beginning of realizing that possibility is ceasing to debate the false issues of the past erected to distract us from more fruitful paths. Science is at its very earliest moments. All knowledge is social knowledge. And social science lays claim to being the locus of self-reflection of knowledge, a claim it makes neither against philosophy nor against the natural sciences, but at one with them.

Much as I think that the next 25-50 years will be terrible ones in terms of human social relations the period of disintegration of our existing historical social system and of transition towards an uncertain alternative I also think that the next 25-50 years will be exceptionally exciting ones in the world of knowledge. The systemic crisis will force social reflection. I see the possibility of definitively ending the divorce between science and philosophy, and as I have told you I see social science as the inevitable ground of a reunited world of knowledge. We cannot know what that will produce. But I can only think, as did Wordsworth about the French Revolution in The Preludes: "Bliss was it in that dawn to be alive. But to be young was very Heaven!"

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