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The Itinerary of World-Systems Analysis; or, How to Resist Becoming a Theory

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The term theory tends to evoke for most people the concept of a set of interconnected ideas that are coherent, rigorous, and clear, and from which one may derive explanations of empirical reality. The term theory however also denotes the end of a process of generalization and therefore of closure, even if only provisional. In the construction of adequate or plausible explanations of complex phenomena, proclaiming that one has arrived at a theory often imposes premature closure on scientific activity, and therefore can be counterproductive. The more complex the reality, the more this tends to be true. What I believe it is often better to do in such cases is to explore empirical reality using spectacles that are informed by theoretical hunches but not bound by them. It is because I believe this is eminently the case in the explanation of historical systems, which are large-scale and long-term, that I have long resisted the appellation of world-systems *theory* for the kind of work I do, insisting that I was engaged instead in world-systems *analysis*. This is thus the story of the itinerary and growth of a non-theory, which I call world-systems analysis.

The story begins for me in the 1950s when I entered the graduate program in sociology at Columbia University. My principal empirical interest was contemporary politics, in the United States and in the world. Columbia sociology at the time was considered to be the center of structural-functional analysis, and the department was particularly proud of pursuing research that combined the theorizing of Robert K. Merton with the methodological approaches of Paul F. Lazarsfeld. What is less often noticed is that Columbia was also the center of a major new subfield of sociology, political sociology.[1] At the time, its faculty (and visitors) included S. Martin Lipset, Daniel Bell, and Johan Galtung, all of whom were prominently associated with political sociology, plus Robert S. Lynd, C. Wright Mills, Herbert Hyman, Ralf Dahrendorf, Daniel Lerner, as well as Lazarsfeld, all of whom in fact did political sociology under other rubrics.

Political sociology was a thriving and growing field. One of the very first research committees of the newly founded International Sociological Association was the one in political sociology. The Social Science Research Council sponsored a multiyear, multivolume project by its Committee on Comparative Politics. I considered it obvious that I would consider myself a political sociologist.[2]

I did have one peculiarity, however. I did not believe the Cold War between the Western "free world" and the Soviet "Communist world" was the primary political struggle of the post-1945 arena. Rather I considered the main conflict to be that the industrialized nations and what came to be called the Third World,[3] also known as the struggle of core vs. periphery, or later still North-South. Because of this belief, I decided to make the study of contemporary social change in Africa my main scholarly pursuit.[4] The 1950s was a period in which the Western world took its first serious look on what was happening outside its own redoubt. In 1955, the Bandoeng conference of Asian and African independent states was the moment of self-assertion by the non-Western world, the moment in which they laid claim to full participation in world politics. And 1960 was the Year of Africa, the year in which sixteen different states became independent; the year also of the Congo crisis, which led to massive United Nations involvement in its civil war, a civil war that was bedeviled by much outside interference.

The year 1960 was also the year in which I came to know Frantz Fanon, an author I had long been reading, and whose theorizing had a substantial influence on my own work. Fanon was a Martinican and a psychiatrist, who went in this latter capacity to Algeria, where he became a militant of the Algerian *Front de Libération Nationale.* His first book, *Black Skin, White Masks* (first published in French in 1952), is about the psychic impact on Blacks of White dominance. It has been widely revived and republished in the 1990s, and is considered highly relevant to the discussions on identity that have become so prevalent. But at the time, it was his fourth and last book, *The Wretched of the Earth* (published in French in 1961 just before his very premature death from leukemia) and prefaced by Jean-Paul Sartre, which made him world-famous. The book became in a sense the manifesto of the world's national liberation movements, as well as of the Black Power movement in the United States.

In the best tradition of both Freud and Marx, Fanon sought to demonstrate that what on the surface was seemingly irrational, notably the use of violence by these movements, was beneath the surface highly rational. The book was therefore not merely a polemic and a call to action but a reflective work of social science, insisting on a careful analysis of the social basis of rationality. I wrote a number of articles at the time, seeking to explain and defend Fanon's work,[5] and I returned to the issue in my discussion of Freud and rationality in my Presidential address to the International Sociological Association in 1998 (Wallerstein 1999c:9–12).

The 1960s was a period of cascading independences in Africa. It was also a period of the first postindependence difficulties—not only the Congo crisis but the beginnings of military coups in a large number of states. Since I was lecturing on and writing about the contemporary scene, I was called upon to explain these multiple new happenings. I came to feel that I was chasing headlines, and that this was not the proper role of a social scientist. During the time that I was doing the fieldwork on the movement for African unity in 1965, I decided to try out a new approach to these issues by expanding the space scope and the time scope of my analyses. I gave three versions of a first cut at this approach at three African universities—Legon in Accra, Ghana; Ibadan in Nigeria; and Dar-es-Salaam in Tanzania.

The interested reception led me to try two things when I returned to Columbia. I created a new course that embodied this expanded scope into the analysis and I found considerable student response to this approach. At the same time, Terence Hopkins and I were asked by the department to give a course on the methodology of "comparative analysis," which we turned into a critique of "the comparative study of national societies." We wrote jointly an article assessing past modes of doing such work (Hopkins and Wallerstein 1967).

At the same time, we undertook a big content analysis project, seeking to extract systematically the propositions to be found in the by then innumerable articles purporting to be comparative in method. We enlisted some twenty graduate students as our readers (in a dozen languages) who were asked to fill in a schedule about each article that we had devised. We never published this gigantic content analysis because we discovered that an extremely large proportion of articles that were "comparative" according to their title compared one somewhat "exotic" country with one the author knew well, since he came from that country (most often the United States). Unfortunately, too many authors compared the data they collected in the exotic country with the remembered or imagined (but not empirically examined) reality of their own. Something, we thought, was very wrong.

About this time, I discovered some wonderful articles by Marian Malowist while roaming through *Africana Bulletin*, an obscure source, since it was the journal of Polish Africanists. Malowist was an economic historian of the fourteenth to seventeenth centuries. He wrote primarily about eastern Europe but he wandered afield to write both about colonial expansion and about the gold trade in the fourteenth to fifteenth centuries between the west coast of Africa and North Africa (Malowist 1964, 1966). The articles had two merits in terms of my further development. They led me to Malowist's other writings. And in the first article, Malowist introduced me to Fernand Braudel's great work on *The Mediterranean*.[6]

It was at this point that my dissatisfactions with the comparative study of national societies combined with my discovery via Braudel of the sixteenth-century world inspired a bad idea which serendipitously turned my work around, and toward world-systems analysis. Since I, along with multiple others, had been describing African and other postcolonial states as "new nations," I said to myself that must mean that there are "old nations." And old nations must at one time have been new nations. So I decided to investigate how old nations (essentially western Europe) had behaved when they were new nations, that is, in the sixteenth century. This was a bad idea, as it was based on premises of modernization theory, which I was to reject so strongly later.[7] Western European states in the sixteenth century were in no way parallel to Third World states in the twentieth century.

Fortunately, I was reading both Braudel and Malowist.[8] What I discovered in Braudel was two concepts that have been central to my work ever since: the concept of the world-economy and the concept of the *longue durée.* What I discovered in Malowist (and then of course in other Polish and Hungarian authors) was the role of eastern Europe as an emergent periphery of the European world-economy in the sixteenth century. I should elaborate on the three discoveries.

What Braudel did in *The Mediterranean* was to raise the issue of the unit of analysis. He insisted that the Mediterranean world was a "world-economy." He got this term from its use in the 1920s by a German geographer, Fritz Rörig, who spoke of *Weltwirtschaft.* Braudel translated this term not as *économie mondiale* but as *économie-monde.* As both he and I were to make explicit many years later, this distinction was crucial: between *économie mondiale* meaning the "economy *of the* world" and *économie-monde* meaning an "economy *that is a* world" (see Braudel 1984, esp. pp. 21–24). The difference was first of all conceptual. In the latter formulation, the world is not a reified entity that is there, and within which an economy is constructed; rather, the economic relationships are defining the boundaries of the social world. The second difference was geographic. In the first usage, "world" equals the globe; in the second usage, "world" means only a large geographic space (within which many states are located), which however can be, and usually is, less extensive than the globe (but also can encompass the entire globe).

I faced one problem immediately. The Romance languages permit making this distinction easily, by using an adjectival noun in place of a true adjective (that is, *économie-monde* as opposed to *économie mondiale*). German doesn't permit the distinction at all orthographically, because one can only use the adjectival noun and it is attached to the noun it is modifying to form a single word. This is why Rörig's usage, which could only be understood contextually, never really received notice. English as a language is in-between. I could translate Braudel's term by inserting a hyphen (thus: "world-economy" instead of "world economy"), the hyphen turning the adjective into an adjectival noun and indicating the indissolubility of the two words which represent thereby a single concept.[9]

I then took Braudel's concept of the "world-economy" and combined it with Polanyi's notion that there were three modes of economic behavior, which Polanyi had called reciprocity, redistribution, and exchange (see Polanyi 1957, 1967, and finally a very clear version, 1977). I decided that reciprocity referred to what I called minisystems (that is, small systems that were not world-systems), and that redistribution and exchange referred to what I called the two varieties of world-systems, world-empires, and world-economies.[10] I then argued that the modern world-system was a capitalist world-economy, that capitalism could only exist within the framework of a world-economy, and that a world-economy could only operate on capitalist principles. I make this case throughout my writings. The earliest (and most widely read) version is Wallerstein (1974b, reprinted in 1979a).

I faced a second problem in orthographics. Both Braudel and I believed that world-economies were organic structures that had lives—beginnings and ends. Therefore, there had to have been multiple world-economies (and of course multiple world-empires) in the history of humankind. Thus I became careful to speak not of world-system analysis but of world-system*s* analysis. This may seem obvious, except that it would become the cornerstone of a fierce attack by Andre Gunder Frank in the 1990s, when he argued that there had been only one world system ever and that it had been covering the Euroasiatic ecumene for twenty-five hundred years at least and the entire world for the last five hundred years (hence no need for either a hyphen or a plural). Obviously, different criteria were being used to define the boundaries of a system. Along with these different criteria came the assertion that the concept of capitalism was irrelevant to the discussion (it either having always existed or never).[11]

If the appropriate unit of analysis of the modern world is that of a world-system, and if there had been multiple world-systems in human history, then Braudel's concept of multiple social temporalities became immediately central. Braudel had built *The Mediterranean* (1949) around an elementary architecture. He would tell the story three times in terms of three temporalities, the short term, the middle term, and the long term. It was only later, however, that he explicitly theorized this fundamental decision in a famous article published in 1958, entitled "History and the Social Sciences: The *longue durée*" (Braudel 1958).[12]

In this article, Braudel speaks not of three temporalities, as we might expect, but rather of four, adding the "*very* long term." He has conceptual names for the four. The short term is *histoire événementielle*, the middle term is *histoire conjoncturelle*, and the long term is *histoire structurelle*. About the very long term he says: "If it exists, it must be the time of the sages" (ibid:76). There are problems with the translation of each of these terms,[13] but the crucial issue to discuss is epistemological. Braudel zeroed in on the fact that, in the last 150 years, the social sciences had seen a split between nomothetic and idiographic modes of knowing, the so-called *Methodenstreit*. Braudel identified this as the split between those who looked only at the eternal truths of social reality (the very long term) and those who thought that everything was particular and therefore nonreplicable (the short term). Braudel wished to assert that the crucial social temporalities were in fact the other two, and first of all that of the *longue durée*—which harbored those structural constraints that have three characteristics: they are not always immediately visible, they are very long-lasting, and very slow to change, but they are *not* eternal.

The most immediate impact on me of this Braudelian imperative—about the priorities scholars should give different social temporalities—was in the conception of how I would write *The Modern World-System.* It became not the search for the eternal truths of comparative organizational analysis, which was the norm in post-1945 sociology (including in political sociology), but rather the story of a singular phenomenon, the modern world-system, informed by a mode of explanation I was calling world-systems analysis. Braudel called this *histoire pensée*, which may best be translated as "analytic history." Braudel's insistence on multiple social times would also lead me later to larger epistemological concerns as well.

What Malowist (and then the larger group of east European historians) did for me was to give sudden flesh to the concept of periphery, as had been initially adumbrated by the Latin American scholars grouped around Raúl Prebisch in the Economic Commission for Latin America (ECLA). The term "second feudalism" to describe what took place in Europe "east of the Elbe" in the sixteenth to eighteenth centuries had long been commonplace. What had not been commonplace, perhaps still isn't, is to see that the "second" feudalism was fundamentally different from the "first" feudalism, and that sharing a common descriptor has done a great disservice to analytic thought.

In the "first" feudalism, the manorial units produced largely for their own consumption and perhaps for that of surrounding small zones. In the so-called "second feudalism," the estates were producing for sale in distant markets. The view that such units were part and parcel of the emerging capitalist world-economy became one of the fundamental themes of my book, and of world-systems analysis. Furthermore, the view that the so-called second feudalism was a feature of a capitalist system had important implications for the prior theorizing, both by Marxists and by liberals, about the nature of capitalism. For a long time, capitalism had been defined in terms of an imagery drawn from the history of nineteenth-century western Europe, of wage-workers in factories (often newly proletarianized and not "owning the means of production") receiving wages (which was their entire income) from an employer who was seeking profits in the market. So strong was this imagery that most analysts refused to categorize as capitalist any enterprise organized in any other mode of labor compensation. Hence, it followed that most of the world could not be considered to be capitalist, or rather was said not *yet* to be capitalist.

Rejecting this nineteenth-century view was a crucial step in the development of world-systems analysis. The classic liberal-Marxist view was based on a theory of stages of development that occurred in parallel ways in units of analysis called states (or societies or social formations). It missed what seemed to us the obvious fact that capitalism in fact operated as a system in which there were *multiple* modes of compensating labor, ranging from wage-labor which was very widely used in the richer, more central zones to various forms of coerced labor very widely used in the poorer, more peripheral zones (and many other varieties in-between). If one did one's analysis state by state, as was the classical method, it would be observed that different countries had different modes of compensating labor and analysts could (and did) draw from this the conclusion that one day the poorer zones might replicate the structure of the richer zones. What world-systems analysis suggested was that this differential pattern across the world-economy was exactly what permitted capitalists to pursue the endless accumulation of capital and was what in fact made the richer zones richer.[14] It was therefore a defining structural element of the system, not one that was transitional or archaic.

Did I theorize this insight? In a way, yes, but diffidently, although I was sure I was on the right track. When I completed *The Modern World-System*, I realized that it was replete with analytic statements, and that there were a whole series of architectonic devices, but that they were nowhere systematically laid out. I worried less about the legitimacy of the exercise than about the potential confusion of the reader. So I added a final chapter, which I called a "Theoretical Reprise." This, plus the "Rise and Demise" article (which was largely a critique of the theorizing of others plus an attempt to show how changing a few premises increased the plausibility of the results), constituted my initial theorizing statements in world-systems analysis.

It wasn't enough for my critics. Many reviewers, even some friendly ones,[15] chided me for insufficiently explicit theorizing—I believe the term is "disprovable hypotheses"—and argued that without it my effort was at most interesting narrative.[16] I was also chided for excessively long footnotes, "winding around the page." To me the long footnotes reflected a deliberate strategy of building my analysis around scholarly discussions on empirical issues, attempting to show how recasting the issues (theorizing?) inserted clarity into what had become for most people murky debates.[17]

I should note that not all the criticism was about the absence of theorizing. There were also important debates about empirical issues. Was Russia really an "external arena" in the sixteenth century, as I asserted, or was it rather a "peripheral zone" just like Poland (see Nolte 1982)? How could I have ignored the Ottoman Empire in the analysis of Charles V and his difficulties in constructing a world-empire? Was the Ottoman Empire really "external" to the European world-economy?[18] While I was ready to defend myself on my empirical choices, such criticisms constantly raised definitional (and therefore theoretical) problems. They forced me to refine my position in order to defend it.

There were two kinds of fundamental theoretical attacks. One came from a Marxist stance, arguing that I had grossly understated the fundamental importance of the class struggle and misdefined capitalism. This was the Brenner critique, suggesting that my view had a "market" bias (sometimes called "circulationism") rather than being a properly "class-based" view of capitalism.[19] In his article, Brenner had attacked not only me but Paul Sweezy and Andre Gunder Frank as well. And the three of us decided that we would not write either a joint reply or separate replies to the article, which was widely read and discussed at the time. I decided to take another path in response to Brenner, whose views struck a resonant note among many persons.

At the same time, a second fundamental critique came from what might be called the Otto Hintze camp. Both Theda Skocpol and Aristide Zolberg launched polemics arguing that world-systems analysis puts into a single arena political and economic phenomena, and that analytically they were separate arenas, operating on separate and sometime contradictory premises.[20] Of course, they were right about what I had done, but I did not think this was an error. Rather I considered it a theoretical virtue. This pair of articles also were widely read.

My substantive answer to both theoretical critiques is to be found in Volume II of *The Modern World-System*, which bore the subtitle *Mercantilism and the Consolidation of the World-Economy, 1600–1750* (Wallerstein 1980). I sought to show in it that, contra a Brenner version of Marxism, there were not multiple forms of capitalism—mercantile, industrial, financial—but rather that these referred to alternate ways for capitalists to make profits, which were better or worse for particular capitalists according to conjunctural shifts in the operations of the world-economy. Furthermore, I argued that the itinerary of Dutch hegemony incarnated a necessary sequence. It was made possible by first achieving supremacy (in terms of efficiency) in productive activities, which led to supremacy in commercial activities, which then led to supremacy in the financial arena; and that the decline of the Dutch followed the same sequence. As for the supposed separate logics of the market and the state, I sought to show that, on the contrary, a singular logic operated in the world-system as a whole and in all of its parts—the core zones, the periphery, and the semiperiphery (whether rising or declining).[21]

What I was also trying to do, as a matter of tactics, became clear to me. Each volume and each chapter of the succeeding volumes was moving forward in time, discussing new empirical issues, and raising further elements of an architectonic scheme. One cannot discuss everything at once. And how all the pieces fit together becomes clear (or clearer) only as one works through the complex empirical data. Furthermore, I had decided on a tactic of overlapping time segments. The second volume starts in 1600 whereas the first ended in 1640, and the third starts in the 1730s whereas the second ended in 1750. And so it will continue to be the case in further volumes. In addition, the chapters within the books had each their own chronological limits, sometimes violating those of the overall book. This is because I came to believe firmly that chronological limits, always difficult to set, are a function of the problem being discussed. The same event belongs in two different chronological limits, depending on the issue. Writing a complex story requires an intelligently flexible schema.

By now I was also writing a large series of articles, published all over the place. If one wishes in an article (talk) both to argue the case for world-systems analysis and to discuss a specific issue, one has to balance the presentation between fundamental premises and particular discussion. I tried to make each important article say at least something worth saying that had not been said before by me. But I had of course also to repeat much of what I had already said, or the audience/readers might not have been able to follow my reasoning. Grouping these articles together in collections had the virtue not merely of making them more available, but of elaborating the theoretical skein.

In the early 1980s, I was asked to give a series of lectures at the University of Hawaii. At the same time, a French publisher asked me to do a short book on "capitalism." I replied that I would write such a book, provided I could call it "*historical* capitalism." The adjective was crucial to me, since I wanted to argue that there was no point in defining in our heads what capitalism is and then looking around to see if it was there. Rather, I suggested we should look at how this system actually worked. Furthermore, I wanted to argue that there has only ever been *one* capitalist system, since the only valid unit of analysis was the world-system, and only one world-economy survived long enough to institutionalize a capitalist system. This is of course the same issue as that discussed above in my rejection of wage-labor as the defining feature of a capitalist system. Is the system a *world*-system or are there as many capitalist systems as there are states?

So I gave the lectures at Hawaii on "historical capitalism" and revised them into a short book. Despite its title, the book has very little empirical/historical data in it. It is a series of analytic statements, assertions about how the system has historically worked, and why. Twelve years later, I was asked to give another series of lectures at the Chinese University of Hong Kong, and I used that occasion to make an overall assessment of the capitalist world-system over its history. I called these lectures "Capitalist Civilization," and there now exists a book in print which puts the two sets of lectures together (Wallerstein 1995a). This book is the closest effort I have ever made to what might pass as systematic theorizing. It is not possible here to summarize the book, but it is the only place in which I tried to cover the whole range of issues I had discussed in other books and essays, and I did try to show how the various parts of the whole fit together.

In 1976, I went to Binghamton University to join my collaborator, Terence Hopkins. We established the Fernand Braudel Center for the Study of Economies, Historical Systems, and Civilizations (FBC),[22] of which I have been the director ever since. There are three things to note about the center: its name, its mode of operation, and its substantive activities.

The use of Braudel's name was intended to indicate our commitment to the study of the *longue durée*, that is, of long-term, large-scale social change. But the rest of the name was taken from a modification of the subtitle of the name of the journal, *Annales*. Its subtitle (at the time) was *E.S.C.*, standing for "economies, societies, and civilizations," all in plural form. We changed, however, "societies" to "historical systems." This was a deliberate theoretical stance. The term *society*—fundamental to general sociological orientations (Merton 1957:81–89)—seemed to us to have led social science in a seriously mistaken direction. In practice, the boundaries of the term *society* have been determined by the adjective placed before it. In the modern world, these adjectives are virtually always the names of states—Dutch society, Brazilian society, and so forth. So the term required that the unit of analysis be state-structured, thereby extending present-day states into their (presumed) historical past. German society was to be seen as the society of the "Germanic peoples" over perhaps two thousand years, although the state itself came into existence only in 1871, and then only in boundaries which were contested and were to change several times thereafter.[23] We insisted instead on the term ***historical system,*** by which we meant an entity that was simultaneously systemic (with boundaries and mechanisms or rules of functioning) and historical (since it began at some point, evolved over time, and eventually came into crisis and ceased to exist). The term ***historical system*** involved for us a more precise specification of the concept of the *longue durée.*

The mode of operation of the FBC was somewhat unusual. It involved an organizational shift that reflected a further theoretical stance. Almost all organized research has been done in one of two ways. One mode is the research program of one (or sometimes several) individuals, either alone or using assistants who are hierarchically subordinate and whose intellectual function is to carry out assigned tasks. Using assistants is simply the expanded version of the functioning of the isolated scholar. The second is the collaborative format, in which several (even very many) scholars (or research institutes) work together (perhaps under the leadership of one person) on a common problem. The outcome is typically a work of many chapters, individually authored, to which someone writes an introduction attempting to show how they fit together.

The FBC sought to institutionalize not collaborative research but collective, unitary research. The mode was to bring together a potential group around a common concern "coordinated" by one or several persons. These groups are called Research Working Groups (RWGs). Each group spends a considerable amount of time defining the research problem and developing a research strategy, at which point the group assigns to its members research tasks. Assignment makes it different from the collaborative project. The assignment process is collective and not hierarchical. Researchers report back to the group regularly, which criticizes their work and sends them out with new group-defined tasks. The results of such work are thus not collections of individual papers but an integrated book written by many hands designed to be read as a monograph.[24] As should be immediately obvious, this approach is the concrete application of the stance advocated in this paper toward theorizing—the avoidance of premature closure.

In addition, it was combined with the assumption that addressing complex intellectual problems requires multiple hands and multiple skills. More than that, these problems require the intrusion of multiple founts of social knowledge, drawn from the multiple social biographies of the participants. It should be noted that typically such RWGs at the Fernand Braudel Center had researchers coming from across the globe and knowing a multiplicity of languages, a crucial element in accumulating multiple kinds of knowledge, including those that are buried in the unconscious psyches of the researchers.

As for the substantive activities, the RWGs have over the years engaged in research on a wide series of major areas which the logic of world-systems analysis suggested needed exploring. And exploration is the key word. Each of the topics was big. Each had enormous problems of locating, in effect creating, appropriate data to utilize. Each resulted in a small step forward in the specification of the integrated theoretical architecture we hoped to build. None contained carefully delineated disprovable hypotheses. Rather each contained somewhat novel conceptualization and the utilization of incomplete and inadequate data (but the best we have presently at our disposition, or at least so we believed). And each sought to rewrite the received canons of presumed theoretical knowledge.

Not every group succeeded even that far. Some research projects had to be abandoned. But those carried through to completion and thereupon published included: the relationship of cyclical rhythms and secular trends of the world-system; the functioning of trans-national commodity chains; hegemony and rivalry in the interstate system; regionality and the semiperiphery; incorporation of the external arena and consequent peripheralization; patterns of antisystemic movements; creating and transforming households; the tension between racism-sexism and universalism; the historical origins and development of social science; the trajectory of the world-system, 1945–2025; the origins of the two cultures and challenges to the epistemology; and currently, a massive project on what others call globalization but which we perceive as "crisis, stability, or transformation?"[25] Each project typically required three to ten years of collective work.

The FBC, like other research structures, constantly sought funds to permit its operation, and therefore submitted projects to multiple foundations. We discovered that when we applied to NSF or even to NEH, we typically received outside evaluations that were evenly balanced between enthusiasm and panning. Few reviewers seemed neutral. Sometimes we got the money and sometimes we didn't. But the panning would always center on methodological questions, on the degree to which the research mode we suggested was insufficiently positivist and therefore in the view of some reviewers insufficiently scientific. We realized some twenty years ago that if one wished to reconstruct the way the analysis of the contemporary world was done, it was insufficient to present data, or even to present data undergirded by a solid theoretical explanation. We had to tackle the question of how one knows what one purports to know, or more properly the appropriate epistemology for social science.

In the 1980s, a second challenge to our work raised its head, coming from that broad current some call cultural studies and others postmodernism or post–other things. For these critics, it was not that we had too few disprovable hypotheses, but that we had far too many. World-systems analysis was said to be just one more "grand narrative," to be cast into the dustbin however recently it had been constructed. We may have had the illusion that we were challenging the status quo of world social science; for these critics we incarnated that status quo. We were said to have committed the fatal sin of ignoring culture.[26]

I turned my attention to these issues, as did the Fernand Braudel Center. I could argue that this was just a matter of our unfolding agenda (one can't do everything at the same time) but no doubt it speeds up the pace of one's agenda when one has the fire beneath one's feet. I suppose it was therefore fortunate, but then there are really no accidents in intellectual history, that it was at this time that I discovered Ilya Prigogine. I heard him speak at a conference in 1981 (not having even known his name before that) and was amazed to hear someone formulate so clearly what I had long been feeling in a confused fashion. And to find that this someone was a Nobel Prize in Chemistry was, to say the least, astonishing, or at least so it was to me at that time.

Prigogine is a chemist by training. The historic relationship of chemists to physicists is one in which the physicists reproached the chemists for being insufficiently Newtonian, that is, for being in fact insufficiently positivist. Chemists were constantly describing phenomena in ways, such as the second law of thermodynamics, that seemed to contradict the premises of classical dynamics, for example, by seeming to deny time-reversibility. Physicists argued that these descriptions/laws must be considered interim formulations, essentially the result of incomplete knowledge, and that eventually what the chemists were analyzing would come to be described in more purely Newtonian terms. Prigogine received his Nobel Prize in 1977 for his work on "dissipative processes" but more generally in fact for being a leader in the analysis of the physics of nonequilibrium processes, central to the emerging large field of "complexity studies." What is more, as he has continued his work, Prigogine has gotten bolder. He is no longer merely saying that nonequilibrium processes exist *as well as* equilibrium processes. He is now saying quite clearly that equilibrium processes are a very special, an *unusual* case, of physical reality, and this can be demonstrated in the heartland of classical physics itself, dynamical systems.[27]

I shall not review the details of his arguments here.[28] What became central for my own analysis, and in my opinion for social science as a whole, are two interrelated elements of the Prigogine construct. The first is the fundamental indeterminacy of all reality—physical and therefore social. One should be clear what one means by indeterminacy. It is *not* the position that order and explanation do not exist. Prigogine believes that reality exists in a mode of "deterministic chaos." That is, he takes the position that order always exists *for a while*, but then inevitably undoes itself when its curves reach points of "bifurcation" (that is, points where there are two equally valid solutions for the equations), and that the choice actually made in a bifurcation *intrinsically* cannot be determined in advance. It is not a matter of our incomplete knowledge but of the *impossibility* of foreknowledge.

I have since argued that Prigogine's position is the call for an "unexcluded middle" (determined order and inexplicable chaos) and is, in this regard, absolutely parallel to that of Braudel, who also rejects the two extremes presented as the exclusive antinomies of particularism and eternal universals, insists on orders (structural time) that inevitably undo themselves and come to an end (Wallerstein 1998b). Prigogine's position had two consequences for world-systems analysis: one was psychologico-political, and the second was intellectual.

The psychologico-political one is not to be underestimated. Nomothetic social science is based on the absolute legitimacy of the Newtonian verities, as a model and a constraint. To have a physical scientist challenge these verities in a plausible way, and to see this challenge become a central part of a serious and substantial knowledge movement within the physical sciences itself undermines the intimidating effect so pervasive within the social sciences of arguments put forward by those who hold on to outmoded scientific methodologies (for example, methodological individualism) when the physicist progenitors of these methodologies are in the process of rethinking them, or rather (as I have insisted) *unthinking* them, that is, of removing them from our internalized and now subconscious assumptions.[29]

The intellectual consequence is nonetheless still more important. Prigogine's work has immediate implications for how one does world-systems analysis, and indeed how one does any kind of social science. It enables one to place precise referents to the concept of the "normal" development of a structure, when the laws of that structure hold and when processes tend to return to equilibrium (what we call the "cyclical rhythms" of the world-system), and to distinguish this period of "normal" development (the development taking the form of "secular trends") from the moments of structural crisis. The moments of structural crisis are those in which the system has moved "far from equilibrium" and is approaching the bifurcation. At that point, one can only predict that the existing system cannot continue to exist, but not which fork it will take. On the other hand, precisely because at a bifurcation the swings of the curve are more violent, every input has more significant impact, the opposite of what happens during "normal" periods, when large inputs result in small amounts of change.

We were now able to take this as a model of transformation of the most complex of all systems, social systems. We could argue, with both Braudel and Prigogine, that such systems have lives—beginnings, normal development, and terminal crises. We could argue that, in terminal crises, the impact of social action was much greater than in periods of normal development. We could call this the period in which "free will" prevails.[30] And we could then apply this to an analysis of the modern world-system. Thus, in the collective work of the Fernand Braudel Center, we argued, on the basis of an analysis of six vectors of the world-system between 1945 and 1990 that the world-system was in structural crisis and was facing a bifurcation (Hopkins and Wallerstein 1996).[31]

The second contribution of Prigogine was to insist that time reversibility was absurd—absurd not only where it seemed obviously absurd, as in heat processes or social processes, but in every aspect of physical reality. He adopted the forgotten slogan of Arthur Eddington, "the arrow of time," and argued the case that even atoms were determined by an arrow of time, not to speak of the universe as a whole. Here, too, he joined forces with Braudel, and here too it was crucial that this theme was coming from a physical scientist. Of course, it added plausibility to our insistence that social systems were *historical* systems, and that no analysis, at any level, can omit taking into account the arrow of time.[32]

We had been thrust into the maelstrom of epistemological debates, which in the end are philosophical as well as scientific questions. These issues moved to the center of world-systems analysis. What we could contribute is to understand the evolution of these debates as a process of the modern world-system, as an integral reflection of its geoculture. I discussed these issues in *Unthinking Social Science*. And in 1993, with a grant from the Gulbenkian Foundation, we set about convening an international commission to study the historical evolution of the social sciences and to look into its possible restructuring.

Constructing the Commission was a key part of the task. We decided to keep it small, in order that it be workable—hence ten persons. We decided we wanted persons from different disciplines in the social sciences. We decided we also wanted to have some physical scientists, and some persons from the humanities. We ended with quotas of 6-2-2. We also decided we wanted persons from all over the world (all five continents), and from different linguistic traditions (we managed four). With a ten-person limit, we couldn't include everything, but we came close. We also wanted persons who had shown prior interest in the large epistemological issues.[33]

The committee's report, *Open the Social Sciences*,[34] contains four chapters. The first is on the historical construction of the social sciences from the eighteenth century to 1945 (Wallerstein et al. 1996). The second deals with three major debates since 1945: the validity of the distinctions among the social sciences; the degree to which the heritage is parochial; and the reality and validity of the distinction between the "two cultures." The third chapter asks, what kind of social science shall we now build? and discusses four issues: humans and nature; the state as an analytic building block; the universal and the particular; and objectivity. The final chapter is a conclusion on restructuring the social sciences.

Aside from the contribution the report tried to make to the understanding of the historical construction and current intellectual dilemmas of the social sciences, it also pointed (albeit in a minor way) to the historical construction of the more enveloping schema, the "two cultures." It seemed to us the next step for world-systems analysis to take was to understand how the very categories of knowledge had come into existence, what role such categories played in the operations of the world-system, and how they shaped the emergence of world-systems analysis itself. Here I can only report on an a work in progress at the FBC, which has taken as its object of its study just that: the reasons why the distinction between "philosophy" and "science" became so central to modern thought in the eighteenth century, for it is easy to show that before then most thinkers thought the two concepts not only were not antagonistic but overlapped (or were even virtually identical). We are also studying why a series of challenges emerged in multiple fields to this distinction in the post-1945 and especially the post-1970 period. We are trying to tie these challenges to the structural crisis of the world-system.[35]

In the Giddens-Turner (1987) volume, I wrote an article on "world-systems analysis," calling for a debate about the paradigm. It opens with the sentences: "'World-systems analysis' is not a theory about the world, or about a part of it.

It is a protest against the ways in which social scientific activity was structured for all of us at its inception in the middle of the nineteenth century."[36] In 1989, I gave a talk on "World-Systems Analysis: The Second Phase" (Wallerstein 1990, 1999a).[37] In that article, I outlined a number of tasks unfinished. I said that the key issue, and "the hardest nut to crack" was how to overcome the distinction of three social arenas: the economic, the political, and the sociocultural. I pointed out that even world-systems analysts, even I myself, although we proclaimed loudly the spuriousness of separating the three arenas that are so closely interlinked, nonetheless continued to use the language of the three arenas and seemed unable to escape it. And in a millennium symposium of the British Journal of Sociology in 2000, I called for sociologists to move forward to the construction of a new and reunified discipline I call "historical social science" (Wallerstein 2000b).

I continue to believe that world-systems analysis is primarily a protest against the ways in which social science is done, including in theorizing. I continue to believe that we must somehow find modes of description that dismisses the very idea of the separation of the three arenas of social action. I continue to believe that the historic categorizations of the disciplines of the social sciences make no intellectual sense any more. But if we continue to protest, it is because we remain in a minority. And if we cannot solve the "key" theoretical conundrum, perhaps we deserve to be. For without solving it, it is hard to convince many of the irrelevance of our consecrated disciplinary categories.

Hence I continue to believe that we are in an uphill battle, but also that this battle is part and parcel of the systemic transformation through which we are living and which will continue for some time yet. Consequently, I continue to believe that it is very worth trying to do what we are doing. But we must be open to many voices and many critics if we are to go further. And that is the reason I continue to believe it is premature to think of what we are doing as a theory.

NOTES

1. For a very brief statement of the cultural importance of this subfield, see Wallerstein (1995b).

2. My M.A. thesis in 1954 was entitled "McCarthyism and the Conservative." My Ph.D. dissertation in 1959 was entitled "The Role of Voluntary Associations in the Nationalist Movements in Ghana and the Ivory Coast." It was later published as *The Road to Independence: Ghana and the Ivory Coast* (Wallerstein 1964). At the first ISA meeting that I attended in Stresa, Italy, in 1959, I spent my time at the meetings of the Committee on Political Sociology. Later, I attended one of the conferences of the SSRC Committee in Frascati, Italy in 1964 and contributed a paper to the volume resulting from the conference (see Wallerstein 1966).

3. See my look backward as of 2000 in Wallerstein (2000a).

4. My first two books, aside from the published dissertation, were *Africa: The Politics of Independence* (Wallerstein 1961) and *Africa: The Politics of Unity* (Wallerstein 1965). In 1973–74, I was elected president of the African Studies Association.

5. See my entries on Fanon (Wallerstein 1968, 1970, 1979b).

6. *La Méditerranée et le monde méditerranéan à l'époque de Phillippe II* was first published in 1949 with a revised edition in two volumes in 1966 (Braudel [1949] 1966). Its English translation, based on the revised version did not appear until 1972 (Braudel 1972).

7. My "manifesto" is found in Wallerstein (1976) delivered in 1975, appearing originally in *The Uses and Controversy of Sociology* and reprinted in Wallerstein (1979a).

8. I acknowledge my debt to both of them in Wallerstein (1974a).

9. I discuss the issue of the hyphen in Wallerstein (1991b).

10. Note the hyphen in all of these formulations. "World empire" (and *Weltreich*) is a term that others have used before me. I felt however that since none of these structures was global, in English the hyphen was required by the same grammatical logic that made it requisite in the case of world-economy.

11. By now Frank has published these arguments in many texts. See especially the early version, Frank (1990), and the mature version, Frank (1999). For my critique of *ReOrient*, see Wallerstein (1999b). The same issue of *Review* also contains critical reviews of Frank by Samir Amin and Giovanni Arrighi.

12. This was republished in Braudel (1969). This has appeared in English in at least four different versions. The reader must beware of the most accessible translation, that found in *On History* (Braudel 1980) since it is inaccurate at points crucial for this discussion.

13. I discuss how best to translate them into English in Wallerstein (1991b).

14. This view is argued in many of my writings. See in particular part 1 of Wallerstein (1979a).

15. See, for example, Hechter (1975:221), who tempers his praise with a critique of shortcomings, two of which revolve around theorizing. "[T]here is no theory to account for the triumph of the European world-economy in the sixteenth century. . . . There is a certain lack of conceptual precision which mars the analysis."

16. See the marvelous discussion of the criticism that Wallerstein has "only one case" in Wulbert (1975).

17. One of the few persons to remark favorably upon this technique, and to explicate clearly the strategy, is Franco Moretti (2000:56–57): "Writing about comparative social history, Marc Bloch once coined a lovely 'slogan,' as he himself called it: 'years of analysis for a day of synthesis'; and if you read Braudel or Wallerstein you immediately see what Bloch had in mind. The text which is strictly Wallerstein's, his 'day of synthesis,' occupies one third of a page, one fourth, maybe half; the rest are quotations (fourteen hundred, in the first volume of *The Modern World-System*). Years of analysis; other people's analysis, which Wallerstein synthesizes into a system."

18. "Is there good reason for considering Poland part of the periphery within Europe's world-economy and regarding the Ottoman empire as part of an external arena?" (Lane 1976:528).

19. "Thus the correct counterposition cannot be production for the market versus production for use, but the class system of production based on free wage labour (capitalism) versus pre-capitalist class systems" (Brenner 1977:50).

20. Theda Skocpol (1977:1079) suggests, like Brenner, who acknowledges seeing her article before publication, that I have ignored the "basic Marxist insight that the social relations of production and surplus appropriation are the sociological key to the functioning and development of any economic system." However, her more fundamental critique has to do with the relation of the economic and political arenas: "[The] model is based on a two-step reduction: first, a reduction of socioeconomic structure to determination by world market opportunities and technological production possibilities; and second, a reduction of state structures and policies to determination by dominant class interests" (ibid:1078–79).

Aristide Zolberg (1981), in his critique of my work specifically recommends Hintze as a more "fruitful avenue for theoretical reflection." He says that Hintze "remains one of the very few scholars who identify the interactions between endogenous processes of various kinds and exogenous *political* processes as a *problématique* for the analysis of European political development." Note the italicization of "political." For Zolberg, as for Skocpol, as indeed for Brenner, I am too "economistic."

21. Core-periphery as an antinomy to be applied to the analysis of the world-economy was first made famous by Raúl Prebisch and his associates in the U.N. Economic Commission of Latin America in the 1950s, essentially to replace the then dominant antinomy of industrialized and agricultural nations. Prebisch was implicitly using a world-systems perspective by insisting that what went on in the two sets of countries was a function of their interrelations more than of social structures internal to

each set of countries. The Prebisch framework was further developed, particularly in its political implications, by what came to be known in the 1960s as dependency theory. In my book *The Modern World-System*, vol. 1 (Wallerstein 1974a), I insisted on adding a third category, the semiperiphery, which I claimed was not merely "in-between" the other two but played a crucial role in making the system work. What the semiperiphery is, and how exactly it can be defined, has been a contentious issue ever since. An early attempt by me to spell this out may be found in Wallerstein (1979a:95–118).

22. See the website: fbc.binghamton.edu.

23. See my discussion of this issue, precisely using the case of Germany to make a general theoretical point, in "Societal Development, or Development of the World-System?" (Wallerstein 1986). This was an address to the *Deutsche Soziologentag* and was published first in *International Sociology* and then republished in *Unthinking Social Science* (Wallerstein 1991a).

24. I have discussed a bit of this organizational history and philosophy in "Pedagogy and Scholarship" (Wallerstein 1998a).

25. The story from 1976–91 can be found in a pamphlet, *Report on an Intellectual Project: The Fernand Braudel Center, 1976–1991.* It is now out of print but can be found on the Web: fbc.binghamton.edu/fbcintel.htm. The annual story since then can be found in the newsletters of the FBC, also on the Web: fbc.binghamton.edu/newsletter.htm.

26. The bulk of the article attacks the uses of "systems theory" for its nomothetic bias, and then draws this inference: "Ideologies of legitimation, questions of cultural domination, etc. take on little or no importance. . . . Wallerstein sees no need to account for the specific development of hegemonic bourgeois democratic ideologies which are already in the process of formation in the period of capitalism's early rise" (Aronowitz 1981:516).

27. "[O]ur 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. . . . We therefore obtain a probabilistic formulation of dynamics by means of which we can resolve the conflict between time-reversible dynamics and the time-oriented view of thermodynamics" (Prigogine 1997:108).

28. The latest and clearest version is to be found in *The End of Certainty*. It should be noted that even here, the issues of orthography intrude themselves. Certainty in the English edition is singular. But the French original is entitled *La fin des certitudes*, and there certainty is plural. I believe the publishers made a serious error in the translation of the title.

29. On the importance of "unthinking" as opposed to "rethinking," see the introduction, "Why Unthink?" to *Unthinking Social Science: The Limits of Nineteenth-Century Paradigms* (Wallerstein 1991a:1–4 and passim).

30. I placed the discussion of "free will" within a fifth social time, not dealt with by Braudel. I called it "transformational time," and suggested that this was the *kairos* discussed by Paul Tillich (1948, esp. pp. 32–51). *Kairos* means "the right time" and Tillich says that "All great changes in history are accompanied by a strong consciousness of a kairos at hand" (ibid:155). See "The Invention of TimeSpace Realities: Towards an Understanding of our Historical Systems," *Unthinking Social Science* (Wallerstein 1991a:146–47), where I specifically tied the concept of transformational time to the discussion by Prigogine of the consequences of "cascading bifurcations."

31. The six vectors are the interstate system; world production; the world labor force; world human welfare; the social cohesion of the states; and structures of knowledge. These six vectors are then summed up in two chapters that I wrote, entitled "The Global Picture, 1945–1990" and "The Global Possibilities, 1990–2025."

32. The importance of the time dimension in the redirecting of sociological theorizing is at the heart of my ISA presidential address (Wallerstein 1999c).

33. The final list of the commission was: Immanuel Wallerstein, chair, sociology, United States; Calestous Juma, science and technology studies, Kenya; Evelyn Fox Keller, physics, United States; Jürgen Kocka, history, Germany; Dominique Lecourt, philosophy, France; V. Y. Mudimbe, Romance Languages, Congo; Kinhide Mushakoji, political science, Japan; Ilya Prigogine, chemistry, Belgium; Peter J. Taylor, geography, United Kingdom; Michel-Rolph Trouillot, anthropology, Haiti. Given the

academic and geographic mobility of scholars, the disciplines listed are those of their doctorates, and the countries those of their identification (via birth or nationality).

34. As of 2002, the report exists in twenty-five editions in twenty-two languages. Others are in process.

35. See a first treatment of this last issue in Richard Lee, "Structures of Knowledge" (1996).

36. The article is reprinted in *Unthinking Social Science* (Wallerstein 1991a). The quote is to be found on p. 237.

37. It was published in *Review*, reprinted in *The End of the World as We Know It: Social Science for the Twenty-first Century* (Wallerstein 1990, 1999a).

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