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SPECIAL SYMPOSIUM ON *THE MODERN WORLD-SYSTEM, VOLS. I-IV,* BY IMMANUEL WALLERSTEIN

Reflections on an Intellectual Adventure

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When I started out to write *The Modern World-System* (MWS) in 1970, I had no idea that forty-one years later I would be publishing its fourth volume and asserting that I needed three more volumes to finish the work. What started out as an attempt to write up, in brief compass, what I had been teaching as a course for a few years became a lifetime intellectual adventure.

To understand this, I have to begin at the beginning. I grew up in New York City in the heyday of Roosevelt's New Deal, the world struggle against fascism, and the Second World War, during which I was just a little too young to be drafted. As I think about the things that might explain the paths I later took, two things stand out.

The first was that I was voraciously interested in everything, and therefore had a very difficult time deciding what might be a career path or even a disciplinary emphasis in college. Fortunately, I went to Columbia for my BA (and later for my MA and PhD). Columbia College was very proud of anchoring its curriculum in "general education," and at that time did not even require that a student "major" in one discipline. So I wandered across the disciplines, and only decided that I would do graduate studies in sociology in my last semester. I chose sociology in fact because I saw it as the least restrictive of the disciplines.

The second particularity, and this goes back to my high school days, was an interest in the non-Western world, not instead of but on an equal level with an interest in the pan-European world. I thought I was going to emphasize India as a focus of work, but the accidents of activity in youth organizations led me to important contacts with Africa (and indeed particularly French-speaking Africa). So I decided to do a doctoral dissertation on an African topic, with the aid of the then new Ford Foundation grants for area studies. Fortunately, once again, the graduate department of sociology at Columbia looked upon this interest with a bemused eye. Why not? they seemed to imply. One more geographical zone for the Columbia sociology department to conquer.

In 1958, I began teaching at Columbia in the college. I had to teach two sections of a required course in the college's general education program and one other course. But what other course? The chair of the college sociology department was then C. Wright Mills. I asked him what he would suggest. And he, typically, said, why not teach your dissertation? So I invented a course which I called "Changing Institutions in New Nations." The next year, it was made a 400-level course, which meant that it was open both for juniors and seniors in the college, and for graduate students.

The second fortuitous event happened in the graduate school. Columbia's graduate department had a very eclectic view of methodology. It insisted that all graduate students take two semesters of methodology courses. But it offered them a choice of six

one-semester courses, both quantitative and qualitative. One of them was called ''Comparative Sociology'' and had been taught by an assistant professor who was in fact an anthropologist by training. His course was based on the Human Relations Area Files that were then in vogue.

But he left the department after three years for a real anthropology department. And the department did not want to lose the option. So, one day, Robert Merton, then the chair, invited Terry Hopkins and me to lunch. Terry and I had joined the department the same year and we were already seen as an intellectual team. Merton suggested that we jointly take over the ''Comparative Sociology'' methodology course. We did, changing it radically, and renaming it ''The Comparative Study of National Societies.''

This was the era of John F. Kennedy, and the department suddenly had a lot of graduate students who had spent two years in the Peace Corps, and were therefore oriented to concerns with what was then called the ''Third World.'' Our new methodology course was just what they were looking for, and it was instantly extremely popular.

There I was, at Columbia, writing about Africa and teaching courses about the Third World. I spent a sabbatical year in Africa in 1965–66, doing research for my book on African unity. I divided my time between Accra in Ghana (then the fount of strong pan-African sentiment) and Dar es Salaam in Tanzania (then the headquarters of the African Liberation Committee of the Organization of African Unity).

Over that year, I gave three public talks—the first in Accra, the second in Ibadan (Nigeria) which I visited, and the third in Dar es Salaam. These talks were in fact an evolving set of reflections about post-independence Africa in the world-system. There turned out to be a great deal of interest in this theme. It was about this time that I discovered Fernand Braudel's books on the Mediterranean, and this had a big impact on how I began to think about the topic.

When I returned to Columbia, I changed my now year-long course on ''new nations'' to one I called ''Social Change: Modernization.'' This was a terrible title in the light of my later views, and the course was invented as I went along. What I did, however, in each successive session was to combine a historical locus (moving forward from the sixteenth century) with a particular theoretical conundrum. I doubt that the course was very good or very clear. But it too seemed attuned to the demands of the times. The graduate students were very responsive.

I had been invited to be a fellow of the Center of Advanced Study in the Behavioral Sciences (CASBS) for 1969–70. But 1968 broke out at Columbia, and I was involved full-time with the student strike, the faculty attempt to mediate between the administration and the students, and then the attempt to create a faculty senate at Columbia. I was so involved that I forgot to accept the CASBS invitation in time. Fortunately, Robert Merton (who was otherwise most unsympathetic to my activities during the 1968 uprising) was still a key figure in the CASBS, and he arranged that I be invited again for 1970–71.

Because of 1968, I took a time-out from writing about Africa to write about the university for two years or so. But then I went to Palo Alto to start my fellowship there in September, 1970. Palo Alto was still then what Dan Bell famously called ''the leisure of the theory class.'' It was an ideal setting for full-time research and writing. I went there with the intention of writing up a small book based on my course on social change. Like the course, it was to combine chronology with theory. It soon became clear to me that the chapter on the sixteenth century would have to be a whole book. And by June 1971, I had basically written what would become Volume I of MWS.

I started at that point to teach at McGill. When the Christmas break came, I realized that I was rather unhappy with Chapter Two of Volume I, so I spent the break rewriting it as well as creating an elaborate index. I may also have done the ''theoretical reprise'' at that time. Now I had a book. It turned out it was not at all easy to get it published. This was a massively footnoted book about the sixteenth century. Who might be interested? I had signed a contract with a previous publisher. But then the publisher rescinded the contract, on the grounds that the book was unsellable. Another publisher refused it on the grounds that some other book he was

publishing (a book now long forgotten) was covering the same ground (it wasn't).

Finally after several other rebuffs, Chuck Tilly, who was then the series editor of a new social science series at Academic Press, decided to "take a chance" on the book. And the imaginative staff editor for the series, Stanley Holwitz, made the crucial (if expensive) decision to put the footnotes at the bottom of the page rather than as endnotes in the rear. We were launched.

The reception was unexpected and remarkable. I describe it in the Prologue to the new edition of MWS I. Three things rescued it from what might have been obscurity. The book in manuscript had been circulating more than I realized, and it came to the attention of Gertrud Lenzer, who persuaded *The New York Times* to let her do a first-page review in December of 1974. In April 1975, Keith Thomas did a review for *The New York Review of Books* that discussed MWS I along with two books by Perry Anderson under the rubric of "jumbo history." And at the 1975 meeting of the American Sociological Association, MWS I was given the award (then called the Sorokin Award) for distinguished scholarship.

The story now shifts to world-systems analysis as a concept and as an intellectual movement. My colleague and co-worker, Terry Hopkins, had been lured away from Columbia by the Sociology Department of SUNY-Binghamton. They wanted to start a graduate program and asked him to create it and run it. After a year or two, they needed an outside evaluation, and Paul Lazarsfeld and I were the team to do it. I was of course very sympathetic to what Terry was establishing and Lazarsfeld was impressed. It was then, I think, that he proclaimed that Terry and I represented "His Majesty's Loyal Opposition"—to the Columbia program he had established with Merton.

Terry then devoted his energies to getting me to join him at Binghamton. With the aid of a sympathetic administrator, I was invited to come in 1976 as chair of the department, which I remained for four years, and director of a research institute that was to be created, the Fernand Braudel Center (FBC), which I remained until 2005.

We established three principles about recruitment to the graduate department. We ignored totally the discipline in which invited faculty had received their degrees. In the process, we acquired faculty from across the disciplines in terms of their training. We established a program of Adjunct Professors (all located outside the United States) who came on a recurrent basis for six weeks each year to give intensive courses. And we recruited students from around the world on the basis of their work and interests in the kind of work we were doing, many of whom joined us after years in the non-university world. Terry had the habit of telling any graduate student applicant who had received offers from us and from some more standard prominent department that, if in the least doubt, they should go to the more standard prominent department.

As for the FBC, the key to our operation was the concept of the Research Working Group (RWG). Such groups had one or more coordinators plus multiple faculty and graduate students (from any department at the university, and sometimes from other universities). The RWGs were organized around some very general theme (say, households or antisystemic movements) and spent the first year or so seeking collectively to define a problem and an approach to doing research, provided the research was done over the *longue durée* and was geographically broad.

The RWGs typically took 3–10 years to do their work, the membership necessarily evolving somewhat over that time. The work was seen as exploratory and not definitive. The data was of every conceivable variety. And the outcome was to be a single book—not a collection of essays, but an argued collective work. Over thirty years, a large number of books of this variety were published.

Funding was of course always an issue. The university paid for minimal infrastructure, but not for these research projects. We of course applied for outside funds to all of the many usual grant-giving agencies. We found that we often had to work for three or four years before we had a project that was "fundable." And we discovered that when we applied for funds to such agencies as the NSF, which had outside reviewers, the reviews came in regularly at two extremes.

Half found the projects wonderful and half thought they were worthless.

It was after a few such experiences that we realized we had to tackle head-on the issue of appropriate methodology for research in what we were calling historical social science. This led the FBC into a new arena of work on what we called the structures of knowledge, which led to other kinds of projects such as *Open the Social Sciences*, the report of the Gulbenkian Commission.

I will not review here all the critiques of world-systems analysis. I do this in the new Prologue to MWS I. But I wish to emphasize one major attempt at steering between Scylla and Charybdis. In all the work associated with world-systems analysis—the work of the FBC, the annual meetings of the Political Economy of the World-System (PEWS) Section of the ASA, the international colloquia the FBC co-sponsored for some twenty years—we tried to avoid two things. On the one hand, we wanted to be open to a range of approaches to world-historical work, not to become in any sense a closed sect. But on the other hand, we wanted to stand for something, not to be diluted in some amorphous whole, such as "global sociology." It has not been easy to do this, but I think that most persons who have been involved in our multiple activities will attest to the fact that we have been able to steer between the shoals.

I wrote in 1998 an article entitled "The Rise and Future Demise of World-Systems Analysis." In it, I argued that the role of challenger or gadfly works only for a while. Either the premises on which we have been operating become mainstream or not. In either case, something called world-systems analysis would probably no longer exist. And the prospects of becoming "mainstream" depend less on the quality or forcefulness of our writings but on the transformed social context within which "mainstreams" are created. I have long argued that the modern world-system is in structural crisis—a crisis whose outcome is both unpredictable and uncertain. It is how this crisis is resolved that will determine the mainstreams of the future.

Finally, I have insisted, much to the despair of even my friends, that there is no such thing as "world-systems *theory*," only a perspective or a mode of analysis. Calling it a theory implies a degree of closure, which I for one do not believe is legitimate. We are an intellectual movement, whose future I have just said is uncertain. But it is one to whose premises I am committed. And the multiple volumes of MWS are the keystone of my own work, which I still regard as an intellectual adventure.

The Emergence of Predominant Capitalism: The Long Sixteenth Century

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The new edition of Immanuel Wallerstein's Volume One of *The Modern World-System*, originally published in 1974, is more beautiful than the original both because of its cover, and because 37 years of subsequent scholarship and world historical events have demonstrated the scientific and practical utility of the theoretical approach developed in this seminal work. If you care about human social change you need to read this book. If you have already read it, you should read it again, as I just have.

The Modern World-System, Vol. I: Capitalist Agriculture and the Origins of the European World-Economy in the Sixteenth Century, by **Immanuel Wallerstein**, Berkeley, CA: University of California Press, 2011. 442pp. $29.95 paper. ISBN: 9780520267572.

The world-systems perspective is a strategy for explaining institutional change that focuses on whole interpolity systems rather than single polities. The tendency in sociological theory has been to think of single national societies as whole systems. This

has led to many errors, because the idea of a system usually implies closure and that the most important processes are endogenous. National societies (both their states and their nations) have emerged over the last few centuries to become the strongest socially-constructed identities and organizations in the modern world, but they have never been whole systems. They have always existed in a larger context of important interaction networks (trade, warfare, long-distance communication) that have greatly shaped events and social change. Well before the emergence of globalization in the popular consciousness, the world-systems perspective developed by Wallerstein, Samir Amin, Andre Gunder Frank, and Giovanni Arrighi focused on the world economy and the system of interacting polities, rather than on single national societies. This has now become taken-for-granted, but when Volume One was written this was not so. This book helped to change the intellectual landscape and to make all the subsequent world-systems research possible.

Wallerstein's new prologue responds to several of the major criticisms that have been made of Volume One. Critics said that the book was too economistic, ignoring politics and culture. Marxists said that Wallerstein ignored class relations. Wallerstein's approach to world history is evolutionary, though he does not use that word. He compares regions and national societies with each other within the same time periods, but he also compares them with earlier and later instances in order to comprehend the long-term trajectories of social change and to explain the qualitative transformation in systemic logic that began to emerge in Europe in the long sixteenth century (1450-1640 CE). His theoretical framework contemplates a "whole system" and how that system has changed or remained the same over time while expanding to become a single Earth-wide integrated network. The questions asked derive from this orientation, but the questions are answered in Volume One by a critical review of controversies among economic historians.[1]

Why did Portugal begin the second wave of European expansion in 1415 CE?[2] What was it about Portugal's position in the European world-economy in the early fifteenth century, its class structure, the nature of the Portuguese state, and its alliance with Genoese finance capitalists, that led it to rewire the long distance trade network with the East by going around Africa? Wallerstein discusses differences in cultural and political institutions and how these interacted with demographic pressures, epidemic diseases, and climate changes that affected the production of "food and fuel."[3] This kind of attention to agriculture, demography, production, and class relations is what is missing in Giovanni Arrighi's version of the evolution of the Europe-centered system as presented in his *The Long Twentieth Century* (1994). But Arrighi's focus on "the shadowy realm" that constitutes the collaboration between finance capital and hegemonic state power is also largely missing in Wallerstein's approach.[4] They complement each other and both need to be read for a complete understanding of the emergence of modern capitalism.

Wallerstein's analysis of East-West similarities and differences that account for the rise of predominant capitalism in Europe and the continued predominance of the tributary logic in East Asia is presented in Chapter One. Summing up his detailed discussion of the main factors that account for the East/West divergence, Wallerstein says:

> The essential difference between China and Europe reflects once again the conjuncture of a secular trend with a more immediate economic cycle. The long-term secular trend goes back to the ancient empires of Rome and China,

[1] The best critical appraisal of Wallerstein's method is Goldfrank (2000).

[2] As Wallerstein notes in Chapter One, the first wave was the European effort to conquer the Holy Land, spurred on by militant Christendom and the Venetian desire to have cheaper access to the goods of the East.

[3] Jason Moore (2003) characterizes Wallerstein's analytic narrative as an environmental history of the emergence of capitalism.

[4] But on pp. 49 and 52 Wallerstein discusses the relationship between the Portuguese state and Genoese finance capital that is the basis of Arrighi's first "systemic cycle of accumulation."

the ways in which and the degree to which they disintegrated. While the Roman framework remained a thin memory whose medieval reality was mediated largely by a common church, the Chinese managed to retain an imperial political structure, albeit a weakened one. This was the difference between a feudal system and a world-empire based on a prebendal bureaucracy. China could maintain a more advanced economy in many ways than Europe as a result of this. And quite possibly the degree of exploitation of the peasantry over a thousand years was less.

To this given, we must add the more recent agronomic thrusts of each, of Europe toward cattle and wheat, and of China toward rice. The latter requiring less space but more men, the secular pinch hit the two systems in different ways. Europe needed to expand geographically more than China did. And to the extent that some groups in China might have found expansion rewarding, they were restrained by the fact that crucial decisions were centralized in an imperial framework that had to concern itself first and foremost with short-run maintenance of the political equilibrium of its world-system.

So China, if anything seemingly better placed *prima facie* to move forward to capitalism in terms of already having an extensive state bureaucracy, being further advanced in terms of the monetization of the economy and possible of technology as well, was nonetheless less well placed after all. It was burdened by an imperial political structure (p. 63).

We now know much more about China because of the careful comparative work of the ''California School'' of world historians (e.g., Kenneth Pomeranz 2001) and Giovanni Arrighi's *Adam Smith in Beijing* (2007) as well as the important collection of essays in Arrighi, Hamashita, and Selden (2003). But Wallerstein's analysis of the main elements explaining the East/West divergence is still the best because of its fruitful combination of millennial and conjunctural time scales.

Those critics who say that Wallerstein ignores class struggle must not have read the book. Not only does he carefully analyze both rural and urban class relations, but he provides a fascinating analysis of the global class structure in the long sixteenth century (pp. 86-87), thereby deflating those in the ''global capitalism'' school who say that his ''state-centric'' analysis ignores system-wide class relations. His analysis of ''coerced cash-crop labor'' (the use of slave and serf labor to produce commodities for the world market) is fundamental to the most important element of the world-systems perspective—that modern capitalism has required an intersocietal hierarchy, an unequal division of labor between a system-wide core and periphery (p. 91). Wallerstein added depth to the analysis of core/periphery relations when he realized that formal colonialism was not the only way in which an unequal international division of labor had been structured. This had already been theorized by the dependency theorists using the idea of neo-colonialism, but Wallerstein discovered a similar case in the way that an unequal division of labor between Poland and Western Europe had underdeveloped Poland in the long sixteenth century. His careful comparison of the ''second serfdom'' in Eastern Europe with the class structures emerging in colonial Latin America in the sixteenth century is fascinating, as is his analysis of the emergence of intermediate forms of labor control in the regions of Europe that were becoming semiperipheral. Elsewhere I have contended that Wallerstein erred in using the ''mode of production'' criteria (capitalism) to spatially bound the Europe-centered system (Chase-Dunn 1998). Europe and its non-core regions were not a separate whole system in the sixteenth century. The European states were still fighting and allying with the Ottoman Empire in ways that greatly influenced the selection of winners and losers within Europe. Europe was a semiperipheral region to the old West Asian core and an instance of what Thomas D. Hall and I have called ''semiperipheral development'' (Chase-Dunn and Hall 1997). But Wallerstein is right that capitalism was emerging to

predominance in the West, and his insightful focus on this evolutionary problem is what makes his approach to world history so useful. Both reading and rereading Volume One is a very rewarding experience.

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Revisiting the Rise of the West

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Immanuel Wallerstein's second volume of the World-System series has been read much less than the first one, though in some ways it should be more crucial because it was from roughly the early seventeenth century to the late eighteenth that the groundwork was laid for the truly revolutionary historical change that was to come afterward. In the late-1500s, Habsburg Spain was still trying to create what Wallerstein calls a "world-empire." In the first volume he made the crucial point that such empires do not generate the internal competition, which can lead to rapid progress. Early modern China, unlike Western Europe, was a large, united empire and not a bunch of warring states spurred on to strengthen their positions in a permanently competitive situation. Spain's use of American precious metals contributed greatly to the West European expansion of the sixteenth century, but the Spanish Empire was only a vast transoceanic plundering scheme that generated neither self-sustaining economic growth for Spain nor the military strength required to bring the rest of Western and Central Europe under its control, so Spain failed.

The Modern World-System, Vol. II: Mercantilism and the Consolidation of the European World-Economy, 1600–1750, by **Immanuel Wallerstein.** Berkeley, CA: University of California Press, 2011. 370pp. $29.95 paper. ISBN: 9780520267589.

Wallerstein's strength is not the discussion of ideas, which he tends to view as mere byproducts of economic systems, but his discussion of the Habsburg Empire leads to a conclusion he avoids making. In the time of Philip II (reigned in Spain from 1556 to 1598), despite the continuing artistic flourishing that was part of Spain's "Age of Gold," his alliance with the Catholic Church and the increasingly bitter attempt to crush not only Protestantism but the free thought and rationalizing science and theology that went with it doomed progress in most Habsburg lands. It so alienated its economically most advanced province, the Netherlands, that it provoked a rebellion that would eventually radically change the balance of power in Europe. The Catholic Counter Reformation backed by the Habsburgs strangled the

expansion of learning that had been centered in Italy and relegated Iberia to intellectual peripherality well before its military decline made it a second-class power. If Spain had succeeded in forcing its will on England and the Netherlands, it would have imposed the kind of intellectual rigidity that would have killed, at least for some time, the rise of capitalism and the ascendancy in the seventeenth century of first the Netherlands and then England. We should remember this in evaluating world-system theory's relevance to our own times.

In contrast, the fifteenth century Ming Empire successfully decreed the end of the long distance fleets that had been expanding Chinese trade to India, Arabia, and even Africa. Why?—to curb the upstart merchants and supposed pirates off the coast of southeast China who profited from this expansion and threatened the existing Confucian cultural, political, and economic hegemony of the Ming. But who were the English and Dutch of the late sixteenth century in the eyes of the Catholic Habsburgs? They were geographically peripheral upstarts, heretical merchants and pirates who threatened the existing Catholic-Habsburg order.

Wallerstein explains how the Habsburgs' failure opened the way to the period covered in Volume II in which a much more trade- and production-based, and more advanced capitalist world-system established itself. Indeed, as Jan de Vries (whose earlier work is much cited by Wallerstein) and Ad van de Woude (1997) have persuasively argued, it was the Dutch economy that was the "first modern economy," not Spain's nor Portugal's. Even England subsequently had to use Dutch technology and capital to turn itself into the world's greatest commercial-maritime power in the late seventeenth and eighteenth centuries.

Volume II makes it clear that we cannot understand how Western Europe came to dominate the world without knowing what happened in this period. Perhaps the book is too state-centered—England had to do this, Sweden tried that, France had no choice but to. . . and so on. But after all, states are the main actors in the modern world, and effectively run, properly taxed states with adequate revenues and the ability to borrow are necessary to secure economic stability and political security. Wallerstein is hardly the only one who has ever said this, but he strongly proves that powerful states in which merchant and industrial entrepreneur (bourgeois in Marxist terminology) interests can guide state policies are required to make markets work. They cannot function on a large scale on their own. What Wallerstein does very well, and what still holds up 30 years after the original publication, is to explain how expanding Dutch, then English and French economic interests gradually incorporated more of the world and began to alter fundamentally social structures everywhere they had commercial interests. This was but a beginning, as the process would greatly accelerate in the nineteenth and twentieth centuries, but Wallerstein's book shows that even before the industrial revolution a gap was starting to open up between the core societies in northwestern Europe and the rest of the world, and that at least for some newly incorporated peripheral areas, this translated into important internal social changes that bound them tightly to the emerging core economies. Wallerstein also shows what classes were the winners and particularly the losers in both the periphery and core. Demonstrating that capitalist progress always produces some losers is something that was sadly neglected in the model of change Wallerstein has spent his career trying to demolish—modernization theory.

In Volume II, as in Volume I, much of the discussion is based on the writing of the best, mostly European, economic and social historians. Many of these were Marxists. Others, if not Marxists, were overwhelmingly more concerned with material changes than with the history of ideas. Wallerstein devotes much space adjudicating their various disputes about what may now seem like fairly arcane historiographic issues; yet, this close textual study of historical works in order to synthesize them into a coherent narrative is one of the aspects that makes these volumes so useful. Today, such historiography is much less fashionable, and especially in its Marxist version, sadly neglected. Wallerstein has variously been accused of being too Eurocentric and

insufficiently attentive to issues of gender and race. That is not really fair. In their time, the European Marxists inspired by the French Annales School, but also quite often by their own political convictions, opened up a whole new understanding of how Europe came to be so rich and powerful, how it took over most of the world, and then proceeded to come close to destroying it. The passage of time has not diminished the important role this particular tradition played in opening up new avenues of research and thinking, and perhaps we would do well to go back to it as we analyze a whole new series of changes in today's world.

Wallerstein's model of how societies operate gives equal weight to class structure, economies, state strength, and position in the world-system. These are not divided into "independent" and "dependent" variables as they are all so closely intertwined. But here, some criticism is in order. For Wallerstein, science, ideology, and philosophy are, in a drastically Marxist way, epiphenomena. One would hardly know from this volume that the period covered is that of the Enlightenment. Early in the volume (p. 7) he brushes this aside in one clause, pointing out that the period he is studying also saw "... the emergence of the presumably 'modern' ideas of Descartes, Leibnitz, Spinoza, Newton, and Locke..." So much for the invention of calculus, the origins of rationalizing Biblical analysis that played such a big role in legitimizing the freeing of minds from church dogma, the origins of modern physics, and the political philosophy of individual rights and freedom that played a critical role in both the American and French revolutions. The scientific and philosophical currents that made the subsequent industrial revolution possible and most importantly legitimized not only capitalism but also evidence-based research is lightly tossed aside, as if only technological innovation mattered, and that was simply the product of competition for control of the world-system.

This is far from a purely Marxist orientation. University of Chicago and Economics Nobel Prize winners Robert Lucas and Gary Becker have proposed that"... the industrial revolution was not exclusively, or even primarily, a technological event." Rather, they say, it was caused by fertility declines that caused greater investment per child, and thus an increase in human capital (Lucas, pp. 169–70). In other words, as Wallerstein believes, it was largely a matter of incentives shifting, though for the Chicago economists, this happened at the individual level rather than, as Wallerstein has it, at the level of social classes (p. 263). In another vein, Kenneth Pomerantz, in a work highly acclaimed by economic historians, has maintained that the reason England industrialized first, and not China, was because England was lucky to have more easily available coal, and that it could exploit the resources of the Americas. Pomerantz does, at least, give limited, passing credit to England's "scientific culture" that developed from 1600 to 1750 (Pomerantz, pp. 44-45), but not much. Yet, Joel Mokyr has persuasively argued that Enlightenment scientific culture, while it did not produce major technological advances in the eighteenth century, did create the base for the astounding leaps in productivity in the nineteenth when scientific progress became increasingly tied to economic growth.

At this point, we get to the question of what Wallerstein's magisterial work means for today's world. We should not neglect the fact that his project has always entailed more than just historical scholarship. In a vast outpouring of essays and lectures he has repeatedly emphasized that the capitalist world-system is ultimately doomed. For a long time he believed that some combination of the socialist advanced economies, the revolutionary periphery, and leftists in the core who supported third-world liberation would create a new socialist world-system. Now, he is more pessimistic and contends that the collapse of American hegemony could bring about the rise of a new East Asian capitalist hegemon dominated by China whose chief rival might be Europe (p. xxiii). In a way, this recapitulates another one of his important contributions to sociology, to remind us that what old-fashioned analysts called the international balance of power matters, and that we need to take it into account.

Wallerstein's personal ideology in the days when both the Soviet Union and

third-world revolutionary regimes promised to overturn capitalism was that his analysis legitimized and helped that trend. Now, what if his more basic historical analysis is correct? Since a socialist world-system seems farther away than ever, can we expect the twenty-first century to be a series of increasingly severe conflicts between a rising China and a failing America (allied with Europe?) punctuated by severe cyclical economic downturns and recurrent crises? There exist many far more benign interpretations of capitalism that do not see it, as Wallerstein does, always driving toward monopolies or the domination of the system by a hegemon. There are also quite different interpretations of where China is heading. But if we take Wallerstein seriously, the almost inevitable conclusion we have to draw from his work is bleak indeed. So, what role, if any, should world-system analysts try to play? Perhaps the stark reality of the situation is one, if not the only, reason why this kind of scholarship has become significantly less visible than in its heyday. Then the future of what he called anti-systemic action seemed to be on the road to success and a whole younger generation of scholars could wax enthusiastic about the coming triumph of Third World socialism.

I think, however, that this is the wrong way to approach Wallerstein's contribution to social analysis. Instead we should concentrate on his having revived a method of analysis that remains as valid today as in Karl Marx's and Max Weber's times. Societies cannot be studied in isolation. All comparative sociology should be grounded in solid historical knowledge. The social sciences are too artificially divided into separate fields and ought to be at least partly reunited. But two additions need to be made by those who would follow in his path.

First, a materialist interpretation of the world is not sufficient. There is also a social world partly determined by ideas that are related to, but not entirely dependent on, class structures and economies. They also drive change, sometimes in ways that materialist theories fail to explain. The struggle over ideas, issues of intellectual freedom, attempts to suppress or foster new thoughts—these are important in determining how societies and the entire world have and will continue to evolve. Existing world-system theory is a major step forward, but to move further requires freeing it from the shackles of narrow materialism. There is a world-system of ideas, too, with its core and periphery (even a semiperiphery), and there are struggles over which kinds of philosophies and ways of thinking will survive or fail. It is closely correlated with, but not identical to the modern economic world-system.

Second, those who wish to continue to expand world-system analysis have to accept something Max Weber tried to emphasize late in his life, that science and politics are distinct enterprises. Because world-system theory ultimately shut out those who did not agree with its political objectives, it lost a lot of its credibility. Some ideological open-mindedness will surely attract the bright young minds it needs to regain its place in the social sciences, and this will enhance rather than damage Wallerstein's long-term legacy as one of the great social scientists of our times.

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Rethinking Bourgeois Revolutions: Transformations of the World-System, 1730-1840s

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The appearance of Volume Four of Immanuel Wallerstein's *The Modern World-System* marks the completion of one of the major scholarly contributions of the past fifty years. The University of California Press is to be congratulated for making the complete work available, especially to a younger generation of graduate students and scholars. *The Modern World-System* is an ambitious if not audacious work that is at once complex and demanding. It attempts to accomplish two things simultaneously. On the one hand, it puts forth the theoretical and methodological foundations for a new unified historical social science. On the other hand, it is a monumental but highly compressed interpretation of the history of the capitalist world-economy, and through that lens, world history, over the past five hundred years. The two tasks are closely related but they are not identical. In the prologue to the new edition, Wallerstein calls attention to what he sees as the major issues entailed in Volume Three, *The Second Era of Great Expansion of the Capitalist World-Economy, 1730s–1840s* and capably defends his positions. Here I am less concerned with his historical interpretations than with discussing the theoretical and methodological implications of his approach.

Wallerstein's work is commonly referred to as world-systems *theory*. However, Wallerstein has argued that his approach is more properly understood as a *perspective* or a *framework for analysis* rather than as a theory. This is more than a case of modesty. It has definite implications for the status of the concept of the modern world-system and for the kind of claims that are made for interpretation and analysis. The world-system perspective entails an active problem-posing, problem-solving approach. It does not attempt to account for all facts, relations, and processes, nor does it attempt to establish general laws or abstract principles. Still less is research viewed as an attempt to prove its theoretical propositions. Rather, this perspective is an open-ended and heuristic approach that attempts to provide adequate *conditions for* a systemic explanation of the decisive economic and political relations forming the modern world.

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The concept of world-system provides the ground for Wallerstein's construction of history and of historical social science. It is a means of cognition. It forms a comprehensive analytical unit that enables him to apprehend theoretically the world as an integrated social whole. It enables him to construct categories or objects of inquiry through their relation to one another within this shared analytical and practical field. Here, objects of inquiry are understood not as things with properties, but as ensembles of changing relations forming configurations that are constantly adapting to one another and to the world around them through definite historical processes. The epistemological and methodological assumption guiding this approach is that the appropriate *unit of analysis* is the capitalist world-system as a whole.

Wallerstein's assumptions turn the logic of conventional social science approaches on their head. Rather than presuming a plurality of discrete, independent, and integral social entities (e.g., societies, states, groups, individuals) with comparable traits, it proposes a single system comprised of diverse constituent elements. These elements relate to one

another as parts of a whole. No sub-unit is like any other. Each is related to the others and each is distinct in time and space. Consequently the usual logic of case comparisons does not apply as, for Wallerstein, each ''case'' is singular in space and time, and is formed through its relation with other such units as parts of the larger world-economy. Instead of comparing presumably discrete and independent units with one another, the explanation refers back to the whole.

The concept of world-system provides procedures that guide inquiry and establish limits for theorization. It is the ground of explanation, both its point of departure and its point of arrival. Analysis begins from the (abstract and general) concept of the world-economy as a whole. Particular relations and processes are taken not as units of analysis but as *units of observation*. They may be various parts of the system or, indeed, the system itself. The key analytical operations here include the differentiation, spatial-temporal bounding, and specification of phenomena within the whole. Successive examination of particular phenomena discloses the specific relations and processes through which they are formed and brings them into relation with the other ''particulars'' forming the world-economic whole. At the same time, such specification of particular phenomena enables us more fully to reconstruct and reinterpret the complex and densely-structured web of relations comprising the world-economy itself. Cognition is understood as a continual process of moving from the whole, to particulars and back again through categories of thought that define the system and are specific to it. The concept of world-system at once orients research and frames analysis and interpretation. Through this procedure the structures and processes constituting the world-economy may be rationally comprehended and the relations among its constituent elements conceptually ordered.

The Second Era of Great Expansion of the Capitalist World-Economy, 1730s–1840s (Volume Three of *The Modern World-System*) engages what is generally regarded as the classic moment in the formation of liberalism, capitalism, and modernity. The key markers for this process are taken to be the Industrial Revolution, the French Revolution, and ''the rise of the bourgeoisie.'' Wallerstein critically examines these concepts from the perspective of the long cycle of world-systemic expansion lasting from 1730 to the 1840s. The book is organized in three distinct movements: struggles for economic and political dominance in the core, the incorporation of new peripheral zones of the world-economy, and settler de-colonization and state formation in the Americas. Each of these interrelated and interdependent movements represents a distinct aspect of the expansion and reformation of the world-economy, and, at the same time, each represents a specific set of analytical problems for Wallerstein's world-system approach.

Wallerstein begins the book by critically examining prevailing interpretations of the Industrial Revolution and the French Revolution. He first evaluates the various explanations of the Industrial Revolution in England. These interpretations generally presume the unique character of the Industrial Revolution and regard it as a break with previous historical development. Wallerstein demonstrates the inadequacy of these accounts and calls into question the analytical utility of the concept of Industrial Revolution itself. First, he argues that the conditions that characterized the Industrial Revolution were not unique to Britain, but existed elsewhere, above all, in France. Secondly, he contends that the Industrial Revolution does not constitute a distinctive historical turning point. Rather, it is an instance of cycles of expansion and innovation that are a recurrent feature of the historic processes forming and reforming the world-system. The real turning point, in his view, occurred with the creation of the system in the sixteenth century.

Wallerstein next addresses the debates over the social interpretation of the French Revolution. This interpretation views the revolutionary events in France between 1789 and 1799 as the struggle of a rising bourgeoisie, with the support of the popular classes, against a feudal order intent on maintaining its privilege. The triumph of this bourgeois revolution initiated the qualitative shift to a new capitalist order in France. Wallerstein rejects the terms in which this debate is posed, but more

importantly he argues that the concept of the French Revolution, like that of the Industrial Revolution, supports a Whig view of history. In contrast to his rejection of Industrial Revolution, he accepts that something of significance did occur in France between 1789 and 1793. But the events of the French Revolution did not constitute either a political revolution or an economic revolution nor did they mark the ascendance of a new social class. Rather, their most important consequences were the transformation of political ideology and a decisive shift in relation between France and Britain. Here too, the significant historical turning point remains the creation of the capitalist world-economy in the sixteenth century.

Having rejected the French and Industrial Revolutions as analytic categories, Wallerstein reinterprets the political and economic development of Britain and France as a struggle for dominance over the world-economy. In the third chapter he shifts the unit of analysis from national societies to the world-system. His concern here is to establish the world-economic and relational character of the particular national histories. He contends that both the ''Industrial Revolution'' and the ''French Revolution,'' as conventionally understood, are artifacts of this long-term struggle for power.

In Wallerstein's approach, the boundaries of national societies become permeable. Instead of a fixed distinction between what is ''internal'' and what is ''external'' to them, national societies appear as particular configurations within the web of systemic relations. At the same time, Wallerstein's use of plural temporalities allows him to integrate multiple levels of structure and agency into a single explanatory account. The long-term expansion of the world-economy creates the conditions for the transformations of the eighteenth and nineteenth centuries, but by itself is of limited explanatory value. Consequently, Wallerstein focuses on the shorter-term economic and political *conjonctures* that occur within the long-term movement. Such intermediate cyclical movements form the immediate contexts of social action, and their identification enables Wallerstein to reconstruct the diverse and changing relations through which both agencies and events are formed. Thus, he does not offer a structuralist account, but incorporates class-conflict, political struggles, and ideology into his explanatory framework. By ordering particular trends, patterns, and events within the analytical framework of the world-economy, he is able to interpret causal relations among historically singular phenomena of diverse duration, tempo, and spatial extension and to account for their significance.

From this perspective Wallerstein deploys the concept of interstate struggle to integrate the ''internal'' and ''external'' histories of France and Britain in a unified analytical field. He is then able to trace the changing position of the two countries through the successive conjunctural cycles of contraction and expansion from 1750 to 1815. This approach enables him to reconstruct the cumulative effects of diverse political and economic processes that increased the gap between Britain and France and restructured the world-economy over the entire period. The struggle between the two countries began on relatively even terms. He particularly calls attention to the political-military victories that increased Britain's advantage over France. Access to overseas, and especially to American markets, combined with an interventionist state, and fluid property rights, allowed Britain to improve its competitive position in agriculture, industry, and trade. British success limited the options available to France, which progressively fell behind Britain. Without adequate outlets for economic expansion, entrenched interests frustrated efforts at agricultural, industrial, and commercial improvement in France. The French state could neither be reformed nor promote reform in other sectors. Rather, it became the source of ongoing fiscal crisis that exacerbated France's problems. The economic upturn that began in the 1790s was marked by global political, military, and ideological conflict between the two powers, including the American, French, and Haitian Revolutions. While the first mass anti-systemic and anti-capitalist movements emerged from these struggles, the French Revolution and Napoleonic Wars sealed France's defeat and secured British hegemony over the world-economy.

Historically, the world-economy is not commensurate with the entire world or

with ''world trade.'' Rather, it refers to definitely structured political economic relations of historical capitalism. Geographical expansion is a fundamental process of the economic and political expansion of the world system. In the third chapter, Wallerstein treats the extension of the world-economy as a systemic process through an examination of the simultaneous incorporation of four separate regions: Africa, Russia, India, and the Ottoman Empire. He is concerned to demonstrate a common sequence of linked systemic processes that are operative in each of these distinct economic, political, social, and cultural configurations. He delineates the historical movement of each from being an external arena, through incorporation, to peripheralization. The concept of ''external arena'' does not refer to a region that is merely outside the world-economy. Rather, it designates a region that already has a relation to the world-economy, generally through trade, but is not part of the world-economic division of labor. Such relations may condition incorporation and systemic expansion, but trade by itself is insufficient to constitute integration into the world-economy. The category of ''incorporation'' serves to organize Wallerstein's analysis of the political and economic mechanisms through which such regions are integrated into the commodity chains constituting the world-economy. Incorporation entails new patterns of production and trade, changes in economic organization and more coercive forms of labor control. While Russia and the Ottoman Empire retained political independence, India and Africa were being colonized. Significantly, the Atlantic slave trade, which had played a significant role in Africa moving from being an external arena to being a peripheral zone of the world-economy, was abolished in the process of incorporation. ''Peripheralization'' refers to the economic and political subordination of these zones and their functional role within the world-economic division of labor. Because his concept of world-economy is a construct for analyzing historical data rather than an explanatory theory, Wallerstein is able to integrate into his explanatory framework the diverse forms these processes took and account for their varied causes and consequences in each instance. He is thereby able to demonstrate how common systemic processes produced distinct local histories.

The final chapter demonstrates both Wallerstein's insistence on historical social science and his sophisticated conceptual framework. Here he analyzes the decolonization of the Americas as an integral part of the expansion and transformation of the world-economy. After 1763, British domination of the Atlantic was matched by commercial expansion in the Pacific and Indian Oceans. At the same time, the French overseas empire contracted. While this informal ''second empire'' served British interests, her North American colonists increasingly found themselves in conflict with the metropolis over trade, agricultural, and industrial policy, and most significantly over expansion on the frontier. Both Spain and Portugal declined in relation to Britain and France. Each became caught up in the Anglo-French rivalry on the Continent, and each became more dependent on their colonial empires as British maritime and commercial power changed the balance of forces. In South America, too, metropolitan reform of colonial policies provoked anti-colonial sentiment. However, the subaltern revolts of Tupac Amaru and of the Comuneros defined the politics of race in Latin America and confined the anti-colonial movement to the Creole elites who steered a course between Spanish colonialism on the one hand and popular revolt on the other. Within this matrix, decolonization played itself out in the years from the American Revolution, through the Haitian Revolution, the Peninsular Wars in Europe, to the final collapse of France in 1815. These events opened the way for decolonization and national independence throughout the Americas. Decolonization of the first peripheral zones in the Americas coincided with the incorporation and colonization of new peripheral zones in Africa and Asia. With the exception of the slave revolution in Haiti and the failed revolution in Ireland, which initiated new anti-systemic movements, this first cycle of decolonization was the achievement of the European settler populations of the Americas. The new republics express the specific position of the Americas in the world-economy, and they remain

distinct from the republican, democratic, or liberal regimes of Europe. Their social composition would distinguish them from the second cycle of mass anti-colonial movements of the twentieth century.

Immanuel Wallerstein's *The Modern World-System* is a pioneering work that opens up new horizons for research, generates new problems, and elaborates new methods. At the same time, it is a difficult work that proceeds not by constructing a history, but by critiquing the categories of existing historiography. For this reason it seems as if the world-system approach is an historicized version of Parsonian structural-functionalism where the social system determines everything. Indeed it has frequently been interpreted in this way. However, this is a one-sided reading that misses the perspective's potential. The volume under review here breaks with liberal and Marxist narratives of capitalism, modernity, and the rise of the bourgeoisie. There is no single "prime mover," whether economic, political, social or cultural. Instead, this approach allows complex historically-grounded causal explanation within the unifying framework of the capitalist world-economy. Such an approach permits fundamental rethinking of the forces that continue to shape the modern world. There is still much to learn from Wallerstein's work.

Liberalism Triumphant—But Where is the World System?

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He is still going strong, the only social scientist who has produced a four-volume work on world development—in his terms the development of the "world system." Immanuel Wallerstein promises a fifth volume soon, and a sixth, he says, "if I can last it out." I sincerely hope he does. Whatever criticisms I might have, it is always a pleasure to grapple with his ideas and to admire the amount and sensitive treatment of empirical research with which he backs up his ideas.

Volume I had the biggest impact on the social and historical sciences, extending our vistas well beyond the nation-state or even the "advanced countries" onto the "world-system," which he said first emerged in the fifteenth century. Volume I had a big impact on me even though I resisted the economism and functionalism I detected there. In Volume II, Wallerstein still emphasized the power of the capitalist world-system but added a considerable emphasis on geopolitics, specifically on Dutch "hegemony" succeeded by Anglo-French rivalry. He noted two cycles in the world-system, one the supposed 60-year Kondratieff economic cycles, the other the slower-paced rise and fall of hegemonic Powers. These two have now become the twin cores of world systems theory in general. But this volume did not attribute rising British power primarily to the strength of its economy, but the strength of its state, a significant departure from his starting point.

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Volume III was a little quirky. Wallerstein attacked the very notion of the "industrial revolution." It was not really revolutionary, he said. True, British economic growth was only about one percent per annum though the fact that this growth continued for most of a century certainly was revolutionary, and so was the cumulative shift to urbanism and industry from agriculture. Wallerstein also rejected both class and revisionist accounts of the French Revolution, since these focused on domestic causes and consequences. Again he emphasized geopolitical causes, that is, French defeat at the hands of the British. In this he was largely correct,

though others have argued that, too. He also observed that there were few revolutionary consequences for France itself, as French historians have also been arguing. This volume also focused on capitalist/imperial expansion across the globe as well as the first phase of decolonization achieved by white settlers. This obviously remained a world-system for him, but it did not any longer seem very economistic or functionalist. But he never really explained where geopolitics and political strength came from.

In Volume IV we see why he had spent so much time on the French Revolution. The Revolution was important, he says, because it led to general acceptance of two great ideas—the normalcy of political change and the irreversibility of popular sovereignty. This in turn made what he calls ''centrist liberalism'' into the dominant ideology of the nineteenth century, defeating its two main rivals, conservatism and radicalism/socialism, and ''taming'' them into adopting its basic principles. Thus centrist liberalism became the dominant presence in what he calls the ''geoculture'' of the nineteenth century world-system. But it was unexpected that he would spend most of the volume discussing Britain and France, which he sees as the main home of centrist liberalism, and very little of it on the rest of the world. Germany, Russia, and the United States have walk-on roles, the periphery appears only as the audience. He promises more of them in Volume V. It was also unexpected that he would focus overwhelmingly on ideology and—after an initial burst of geopolitics—on domestic politics in the two countries. Kondratieff cycles surface as occasional drivers of politics, but on the whole we have to take the world-system for granted. The title and not the sub-title describes the book.

We need to dig a little to find his definition of liberalism. At first he says it seeks to ''achieve in due time the happiness of mankind as rationally as possible'' (p. 11). But so does socialism. So he adds that for this it was necessary ''to engage in conscious, continual, intelligent reformism'' (p. 6) and also that liberals saw the state as ''creating the conditions permitting individual rights to flourish'' (p. 16) rather than as ''protecting traditional rights'' (conservatives) or as ''implementing the general will'' (socialists). He argues convincingly that all three groups only pretended to be against the state—that, for example, laissez-faire barely existed in reality.

Yet he does not define ''centrist liberalism,'' except that, obviously, it is in the center, between conservatism on the right and socialism on the left, a golden mean between reaction and revolution. But its reformism, he says, was eventually accepted by both the right and the left. For the right, fear of the threat coming from below from workers forced them to embrace some reform—though not, I note, for the sake of securing individual rights. They believed reform was necessary to avoid revolution or chaos. This was particularly true of the British Conservatives. As he notes, they and not the Liberals passed most of the progressive legislation of the nineteenth century. For the left, reformism was embraced (though again not for the sake of individual rights) because, he says, working class movements were much too weak to try for revolution and because workers were divided by skill level, religion, ethnicity, and gender. It is hard to argue with this in the cases of Britain and France, and indeed this is now conventional wisdom among historians. Yet Wallerstein does provide a more comprehensive framework of analysis which is innovative as far as the taming of the conservatives is concerned. He becomes even more original when he discusses ethnic and gender issues and also the development of distinct social science disciplines in the nineteenth century (in the second half of Chapter Four and in Chapter Five). Centrist liberals, he says, wanted to keep separate the three domains of the market, the state and civil society, and they achieved this through the emergence of the distinct disciplines of economics, politics, and sociology. This is very provocative.

Centrist liberalism seems all-pervasive in the book. He says that differences between all countries were trivial compared to the overall dominance of centrist liberalism (pp. 179–81). This does not seem plausible for Russia and Germany (where conservatives dominated) nor Italy or Spain (with their patron-client versions of liberalism) nor the United States (liberal but not

centrist). Centrist liberalism, or reformism, was eventually how the West was won, but not until after world wars, the Great Depression, Keynesianism, and the triumph in some countries of social democracy (he would presumably call this centrist liberalism).

He is very interesting on citizenship (in the first half of Chapter Four). From the French Revolution onward, he says, formal equality of citizenship was de rigueur but in substantive terms citizens were not in fact equal and neither conservatives nor liberals wanted them to be. Two anti-systematic movements arose to contest this inequality, social revolutionaries seeking inclusion of the lower classes, and national revolutionaries seeking equality for disprivileged ethnicities, perhaps in their own state (though ethnic minorities are not much discussed). So in response, elites, including liberal elites, sought to "freeze" inequalities among citizens, originally in the form of the class/gender division between "active" and "passive" citizens, then in forms of class and gender franchise limitations, of discrimination against ethnic minorities, and between citizens and aliens. In all these cases, he says, each binary distinction tended to weaken collective anti-systemic action, while the collective identity of the included group preceded that of the excluded group. Thus the bourgeoisie preceded the proletariat, white preceded black, Oriental, and others, masculinity preceded femininity, and the citizen preceded the statuses of alien and immigrant. "Citizenship always excluded as much as it included," he concludes (p. 217). Eventually centrist liberalism effected compromise by conferring civil and political rights on these groups, while resisting socioeconomic equality.

The success of centrist liberalism, he says, was to achieve both a stable order and a long upswing in the world economy. In turn this depended on three pillars, a "strong market," a "strong state," both exemplified by Britain and France, and a "strong interstate system" of which these countries were the core, and through which they were jointly able to impose their liberalism on the world-system—or rather the non-colonial part of it (p. 111). Note again the importance of geopolitics, but in this case it is a dual Anglo-French hegemony, which is not the usual world-systems view of the nineteenth century. But he does not really demonstrate that this was imposed on or accepted by much of the world.

All these arguments are backed up with a wealth of empirical information. There are the Wallerstein trademarks of many quotations from other writers, lengthy footnotes, and an enormous 78-page bibliography. But he has very few references to sociologists or political scientists, and almost none to works published in the twenty-first century. His references are overwhelmingly to historians, mostly of earlier and older generations. Thus, for example, he does not refer to comparative sociological research emphasizing national differences in labor movements, class structures, and states. Nor does he refer to sociologists' writings on citizenship from T.H. Marshall to Rogers Brubaker and beyond.

Overall, my main reservation is that he pins too much onto Britain and France and onto liberalism. Though these two countries did embody much liberalism, the liberal democratic/social democratic path of development did not dominate the West and parts of the Rest until much later. The Meiji Restoration was consolidated in this period with more borrowings from German corporate semi-authoritarianism than from British or French liberalism, while "liberalism" in many countries, including Southern European ones, meant very different things. Second, he does not pay enough attention to the general tendencies of economic and political development and to the internal disagreements among liberalism's rivals, especially the socialists. Industrializing capitalism and urbanism brought the masses on stage and gave them new powers at the level of the nation-state and beyond. That, rather than the influence of liberals, was what frightened both conservatives and liberals into anticipatory reforms. That reformism appeared to be getting the upper hand in working class movements by 1910 was not due primarily to the power of liberalism but to the fact that collective action enabled reformists to make gains while revolutionaries could not.

My main disappointment, however, is that this volume is not about the development of

the world system, not about center, semi-periphery, and periphery. Very little of what he writes about Britain and France presupposes a world-system model. It could have come from many a talented historian. This is an emperor in workaday clothing! I hope that his next volume contains more global finery.

A Liberal Leviathan: The Creation of the Strong State in Nineteenth Century Europe

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In the long-awaited fourth volume of his path-breaking history of the modern world-system, Immanuel Wallerstein focuses on the creation of what he calls a universal geoculture during the "'long' nineteenth century." He defines the word "geoculture" as "values that are very widely shared throughout the world-system, both explicitly and latently" (p. 277). The earlier centuries had already produced capitalism, a global axial division of labor, and a system of core states vying for hegemony over the emerging international political order. The French Revolution introduced two new fundamental cultural considerations into the politics of the capitalist world-system: political change was now seen as normal, and the locus of political sovereignty was now believed to be located not with monarchs but with "something much more elusive, the 'people'" (p. 1). These two momentous changes led to the emergence of "ideologies," which Wallerstein defines as "political metastrategies" aimed at reconciling the striving for expanded popular sovereignty with the elites' desire "to maintain themselves in power and to ensure their continuing ability to accumulate capital endlessly" (ibid.). Three main ideologies developed in the nineteenth century, each one locating itself "in opposition to something else" (p. 11). The first was conservatism, in reaction to the French Revolution; then came liberalism, which began as a negation of conservatism; last came socialism, which positioned itself as a rejection of liberalism. Each ideology proposed a different definition of "the people" and the general will. Conservatives wanted to slow down the pace of now-unavoidable social change, socialists tried to speed it up, and liberals sought a moderate rate. Despite liberalism's leftish beginnings, "its destiny was to assert that it was located in the center" (p. 6). Both of the alternatives to liberalism were, Wallerstein argues, ultimately "tamed" by liberal centrism. In that respect, centrist liberalism became "the prevailing doctrine of the world-system's geoculture" (p. 277).

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Liberalism decisively shaped three crucial spheres. The first was the construction of a strong and liberal state. The absolutist states prior to the nineteenth century "had not been strong states" but "merely the scaffolding within which weak states sought to become stronger" (p. 111). Strong states were those with an "adequate bureaucratic structure and a reasonable degree of popular acquiescence." And it was "only the liberals, who could construct such states in the core zones of the world-system" (pp. 111-112). Wallerstein spends little time discussing the first of these forms of "state strength" (bureaucracy) or any other dimensions of "infrastructural power" (Mann 1988). He does discuss other aspects of state-strengthening, such as the expansion of the electoral suffrage, increased social protection for workers, and the transformation of banks into "key agents of national economic development" during the nineteenth century

(p. 108). However, Wallerstein's discussion of these topics is limpid, summarizing decades of secondary literature in a few clear strokes.

Linked to state strength was the creation of a strong interstate system (p. 111). Wallerstein provides a concise overview of some of the key episodes in the British-dominated international system, detailing the geopolitical *entente cordiale* between Britain and its defeated French rival. These episodes included helping the Belgian, Greek, and Polish uprisings in order to weaken the Ottomans, Austrians, and Russians, which made the year 1830 into a ''watershed in the history of European diplomacy'' (p. 69). Britain and France also cooperated in keeping the peripheries open for trade through a mixture of formal colonialism and informal domination (p. 121). The two powers were able to set their own pace in their patterns of colonial acquisition until the Crimean War and France's ''American Crimea'' in Mexico. By the 1880s, at the latest, all of the other major powers had become free to ''scramble'' in the carving up of Africa as well as the Pacific and other zones.

The second signal change imposed by centrist liberalism, Wallerstein argues, was its attempt to transform the French Revolutionary concept of ''citizen'' into a category of exclusion rather than inclusion. This point is illustrated through incisive discussions of the exclusion of women, workers, and ethnic/racial ''minorities.''

The third change is liberalism's support for the development of the historical social sciences. This discussion connects Wallerstein's *Modern World-System* to the work he has been doing on ''unthinking'' and ''opening'' the social sciences (Wallerstein 1991; Gulbenkian 1997). Here too the key role of liberal centrism guides the analysis, and nineteenth century social science is explained mainly as a containment strategy. Liberalism made a social science of change necessary to preserve elite power. The linkage of social science to reform was not antithetical to the rise of the professionalization of social science and the calls for ''value-freedom'' and ''objectivity''; instead, this was a move away from the practice of direct partisanship to indirect scientific influence on policymaking through expert advice. Wallerstein deals deftly with the creation of economics, sociology, and political science before 1914, that is, up to the moment at which these same fields became academic university disciplines in the core countries. He also discusses the two main ''others'' of these ''nomothetic'' social sciences: history, an idiographic discipline opposed to lawlike generalizations but put to the service of national identity formation in the nineteenth century, and anthropology and Orientalism, which were focused on the nonwestern Other.

The usual criticism of world-system theory is its ''economic reductionism.'' I feel that this critique is off-base, at least for the current volume, which is resolutely focused on other levels—mainly politics. Even in the previous volumes, Wallerstein's accounts of struggles among great powers over who would become the next hegemon often left room for overdetermination, accidents, and intentionality. Arguments for economic determination of politics or culture are quite rare in *Centrist Liberalism Triumphant*. Some of the economic explanatory factors are of course lurking *sotto voce* in the background. Kondratieff cycles finally show up on page 96, for example, and reappear periodically after that. But one has been told in the introduction that the author will not reintroduce concepts that he discussed in earlier volumes. What is sometimes difficult to determine is whether these more economic concepts are always humming in the background—that is, whether they are supposed to be taken for granted.

If there is reductionism in this book, then it is the risk of a *reductio ad politicum*. Most political decisions and cultural changes are traced to strategizing in the international political system. Here we have a whiff of Kenneth Waltz rather than Karl Marx. This sense of political reductionism is reinforced by the fact that every major nineteenth-century event is argued, somewhat relentlessly, to strengthen centrist liberalism—at least until the spell breaks around the 1860s and things start to go wrong for Britain and France.

Compounding the problem of this *politicism* is the absence of an actual theory of politics or culture, the two central arenas of investigation. Activities like social science, culture, and even the state, cannot be understood without analyzing them as *fields of*

difference: fields in which some of the actors are more influential and powerful than others, and in which some of the actors are more *autonomous* than others, with more distance from the influence and demands of external politics and economics. Without a model of cultural and political practices, the danger of turning both into reflections of another external power, be it the state, political strategy, or capitalism, is always lurking. Having myself suffered from this malady of reducing science, politics, ideology, and culture to dependent ''superstructures'' I am aware of its allure (''enjoy your symptom''), but I have also been chastened by social scientists and philosophers for resorting to this shortcut.

An example of this *reductio* is Wallerstein's analysis of social science positivism as the product of liberal political culture. If this is correct, how can we explain this epistemology's dogged persistence in American sociology long after the end of centrist liberalism (Steinmetz 2005)? Or, if we assume that centrist liberalism is still dominant today, why are most of the leading British and French sociologists not imbued with this scientism?

There are also some problems with periodization. Wallerstein explains in the preface that he decided to leave out processes that were not complete, or whose main lines had not been laid down, before 1914. But modern colonialism and modern social science are treated contradictorily. With respect to the former, Wallerstein argues that ''one could not reasonably tell'' the story of the scramble for Africa as though it ''ended somehow in 1914'' (p. xvii). It is of course true that modern colonialism spans the nineteenth and twentieth centuries. But most of the crucial decisions had already been made before 1914: Africa had already been partitioned, the difference between indirect and direct forms of native policy had already crystallized, twentieth-century type social policies had already been introduced in the German colonies as legitimatory devices in 1907, and anticolonial movements and wars were already ubiquitous before WWI.

Of course, Wallerstein promises to return to the ''scramble'' period in his next volume, and including it here would have considerably lengthened this already weighty tome. But just as the main lines of modern colonialism had already been laid down before WWI, the opposite is true of the academic social sciences—which *are* included here. For instance, Wallerstein presents a truncated view of the discipline of sociology, as always fashioning itself as a nomothetic science. This is accuratre even for Germany in the late nineteenth century, as he shows, but that situation was reversed in the Weimar Republic. When the first German sociology professorships were created after 1918, they were located in universities' divisions of Philosophy, Cultural Science, or *Geisteswissenschaften* (e.g., at Berlin, Leipizig, Heidelberg, and Braunschweig). Even today there are entire national fields of sociology not dominated by scientism or positivism (see Abend 2006 on Mexico).

There is nothing at all wrong with overdetermined, multicausal explanations; in fact, they are almost always more appropriate in the human sciences. By introducing alternative determinants at different points in the text, Wallerstein leaves his readers with no idea whether they should substitute the new account for the old one or combine them. Wallerstein discusses Romanticism at two different points in the book. Initially he discusses Romanticism as a product of political culture (pp. 50-57). Later in the book he describes Romanticism as a response to ''scorn by the natural sciences of all that was literary and metaphysical'' (p. 225).

Centrist Liberalism Triumphant is a masterpiece that should be read not only by sociologists but by others well beyond sociology. Part of a series of books, *Centrist Liberalism Triumphant* is not the culmination of it: Wallerstein promises a fifth volume on the period 1873-1968/89 and even suggests the possibility of a sixth volume on the current structural crisis of capitalism. This book presents an analysis of historical change and the importance of the sovereignty of the people. Wallerstein himself has changed his analytic approach over time, foregrounding politics and culture, and he has presented a sovereign grasp of the histories he studies. Reading this book I was intrigued by the foregrounding of the political, and I am looking forward to Volumes Five and Six to see in hindsight the articulation of

the economic, political, and cultural levels of analysis.

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"Field of Forces" and World Culture

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This book of essays tries to put cultural changes in various places and times into an analysis of worldwide "fields of forces," producing rigidity or change differently located in time and place. Both the forces and the cultural outcomes in such models are often fuzzy, and in these essays as well as the other theories of this kind, concrete groups of people, dated times of growth and decay of particular forces and outcomes, places where the mechanisms indeed changed cultures or cultures changed forces, are almost all foggy. (For space reasons I will use "ws" for "world system.")

The book divides into five approximately equal subjects: (1) general intellectual history of academic thought on world history, emphasizing its relation to the ws, (2) general history the of the ws becoming more a system through many kinds of interdependence, mostly since the sixteenth century, (3) varieties of concepts of the ws in political economy, geography, and literature, (4) values intertwined with the ws and their relations to disciplines of the humanities, such as ethics, oppression, and (5) bibliography and organization of the book, index, and so on.

Such frustrations of "field" analysis fuzziness here are analogous to the fuzziness of how world greenhouse gases affect stratospheric jet streams, and polar ice melting, and so creates melted polar water that absorbs rather than reflects solar radiant energy, all making the polar vortex unstable, so that Chicago has more thunderstorms. It all sounds sort of convincing, but concreteness is scattered and unconvincing—vortex and lightning in local clouds from melted ice a thousand miles away? (Similarly, a detective paperback with a lesbian detective but a conventional amount of violence in the plot, as women have more police careers in Europe and the Americas—this is my concreteness, more than we find in most of the essays?) Does the displaced vortex produce the lightning near where we reckon, by counting seconds for the thunder after the flash? Or do Frantz Fanon's (p. 203)

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revolutionary attacks in writings on the racist effects of French imperialism apply equally to Stalin's ethnic cleansing, sending Chechens to Siberia during World War II? Or was it loosely the same forces, except with Soviet ''state capitalism'' running the imperialism? There is no hint here on how we might approach the question, except perhaps that Fanon wrote in a language West Europeans then could read; but we writers of English-language cultural essays learned French but did not learn to read Chechen, and Stalin did not let them publish anyway. Writings in languages few foreigners read are perhaps less forceful in shaping the world cultural fields.

The field-of-forces theorists mentioned more than once in these essays include Immanuel Wallerstein and Pierre Bourdieu. But Wallerstein's examples of world-wide field effect does the work in the sources to document the increasing size and number of Dutch trading vessels carrying grains in the Baltic, then causing Polish agricultural workers to have longer unpaid hours owed to the newly capitalist *Herren*. Such fully developed concreteness pervades his work spottily, giving periodic views of concrete capitalism and concrete exploitation. Pierre Bourdieu has concrete physics professors in the same upper left of his distinction graph as the well-tempered clavier, while the mechanical engineers with the same physics equations in their profession are closer to businessmen and the Blue Danube. Then in the ethnographic distinction extension of the dynamics to the ethnography of children of the physics professor and other such educated elites, some of whom do not make it to a professorship, we find a subculture of arts and crafts and protest-laden music—artsy and intellectual without upper-class dignity. The concrete culture is there to change with distinction of the low income of the adolescents, but to carry cultural elements also in the family line. Such elegant workman-like pictures of concrete field forces creating cultural actions are very scarce here, though Helen Stacy's essay, ''The Legal System of International Human Rights,'' has some.

Overall, these essays seem to me to be on a fruitful intellectual branch, but not ripe with concrete fruit yet. They are a good source of vague ideas to be provided with the elegant concreteness of younger Wallersteins and Bourdieus, along with unstable polar vortexes.