

EVOLUTIONARY PROCESSES IN WORLD POLITICS

THEORY AND METHODOLOGY OF WORLD DEVELOPMENT

The Writings of Andre Gunder Frank

EDITED BY

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AND

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Andre Gunder Frank**

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- (2) Chapter 2 "The Sociology of Development and the Underdevelopment of Sociology," in *Catalyst* #3, 1967.
- (3) Chapter 3 "Transitional Ideological Modes: Feudalism, Capitalism, Socialism," in *Critique of Anthropology*, Vol. 11, #2, 1991, Sage Publications.
- (4) Chapter 4 "A Structural Theory of the Five Thousand Year World System," in Sing C. Chew and David Knottnerus (eds.), *Structure, Culture and History: Recent Issues in Social Theory*. Lanham, MD: Rowman and Littlefield Publishers.
- (5) Chapter 5 "A Plea for World System History," in *Journal of World History*, Vol. 2, #1, 1991, University of Hawaii Press.
- (6) Chapter 6 "The Five Thousand Year World System: An Interdisciplinary Introduction," in *Humboldt Journal of Social Relations*, Vol. 18, #1, 1992, Department of Sociology, Humboldt State University, Arcata, CA 95521.
- (7) Chapter 7 "The World Economic System in Asia before European Hegemony," in *The Historian*, Vol. 56, #2, Winter 1994, Wiley-Blackwell Publishers.
- (8) Chapter 8 "Debunk Mythology, Reorient Reality," unpublished manuscript, Allison Frank, widow of Gunder Frank and literary executor.
- (9) Chapter 9 "Social Movements," in *New Directions in the Study of Justice, Law, and Social Control*, Plenum Press

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P A R T 1

On National Development: The Development of Underdevelopment

He regards it as his task to brush history against the grain.

Walter Benjamin—Illuminations

The phrase, “the development of underdevelopment,” has always been associated with Andre Gunder Frank ever since he published the widely cited article in *Monthly Review* with this title more than forty years ago. At that point in time, the study of socioeconomic and political change, widely known as “development,” was framed around understanding it within the bounds of national territorial boundaries of a nation-state. The dominant theories at that time regarding development were based on stages of growth, diffusionism, and modernization. This modernization-developmental paradigm’s main proponents such as Rostow (1960), Pye (1962), Parsons (1964), Hoselitz (1960), Lerner (1965), and McClelland (1967) contended that socioeconomic progress (or the lack of it) was due to the presence (or absence) of ingredients in each respective country/nation-state that were necessary for development to occur. Their positing of these necessary characteristics of development was based, ipso facto, on an ideal type of developed society conjectured within a value framework that was basically Western. Thus, when a particular country was experiencing difficulties in achieving any progress in socioeconomic and political growth the explanation invariably revolved around the lack of necessary ingredients for development, whether they are cultural, psychological-sociological, economic, or institutional. Therefore, the problems and obstacles to change and development in societies along such a modernization-developmental paradigm tend to be, on the main, rooted internally. However, with an optimistic attitude, these developmentalists

insisted that such handicaps were temporary and that, in the long run, all countries/nation-states will achieve progress and development.

From such a perspective, research was conducted on analyzing the stages particular societies have attained along the continuum from traditional to modern and measurements of the sociopsychological profiles of the social and institutional organizations were carried out. On the whole, however, the modernization-developmental perspective had difficulty explaining the gaps in growth between countries, the lack of autocratic self-sustaining development, the particularized economic specialization of some countries, and the persistent impoverishment of most parts of the globe. Enter Andre Gunder Frank. According to him, such an inability was mainly the consequence of viewing the problems and obstacles to development as concentrated exclusively on conditions internal to the particular country instead of understanding the process as a result of mechanisms tying the country to the economically and technologically advanced centers of the world.

Andre Gunder Frank's timely interventions in the debate on world development belonged to a corpus of radical writings commonly referred to as the "dependency or uneven development" perspective. Early features of this dependency perspective can be seen in the work of Paul Baran (1957), and in the reports of the United Nations Economic Commission for Latin America (ECLA) that centered on the work of Raul Prebisch (1959). Out of this critical interruption, there emerged extensive studies on underdevelopment from Andre Gunder Frank (1967, 1969, 1979), Theotonio Dos Santos (1970), Fernando Cardoso (1972, 1973, 1977), Celso Furtado (1971), Osvaldo Sunkel (1973), and others.

Importantly, not all the dependency studies were wholly influenced by Marx's writings. Some of them were developed mainly as a rebuttal to the modernization-developmental approach. The works of Raul Prebisch (1959, 1976) and Celso Furtado (1971) came under this umbrella. In fact, Gunder Frank (1974) distinguished his approach from that of Prebisch's and Furtado's by suggesting that they belonged to the old school of dependency theory that was nationalist in orientation. In spite of these differences among the proponents of dependency theory this counter-perspective did have some common characteristics, although in some cases the issues or solutions differed. What was most characteristic of these analyses on change and development was the fact that their perspectives were developed from the view of the Third World. These studies challenged the developmentalist's concept of development with its elements of stagism, diffusionism, and dualism by suggesting that these concepts were a consequence of a nonholistic and ahistorical perspective. Chapters one and two clearly outline this critique of the modernization-developmental paradigm. Frank and the *dependentistas* asserted that the onus for the lack of

development (underdevelopment) cannot be blamed on the Third World, but that responsibility must be placed historically on the Western and colonial powers. Therefore, the domination of Third World societies has existed since very early times.

In this regard, Frank (1966) had stated that Third World societies might be originally *undeveloped*, but never *underdeveloped*, and that the root cause can be attributed to the structure of external relations in which these societies were enmeshed. As a result, such a structural relationship generated the domination of their economies by the metropolises (core) and the subordination of their socioeconomic and political growth to the interest of the metropolises (core). According to Frank (1972), this relationship had led to the development of a typical form of class (or national elites), political, and economic structure for a Third World country that he had termed as *lumpen bourgeoisie* and *lumpen development*; though the forms might change for each specific epoch or cultural context. By posing the development of this distorted configuration of social, political, and economic structures, the *dependentistas* challenged the dualist thesis in the modernization–developmentalist paradigm, which suggested that in most later developing societies there was the presence of a dual society whereby the modern industrial enclave existed parallel to a traditional backward society. The explanation for this duality was due either to the lack of progress, communication, or traditional feudal practices. However, what the *dependentistas* argued was that the backwardness and dual society phenomenon was basically caused by the development of the modern enclaves. That is, the development of these urban centers had been parasitical on the underdevelopment of the hinterland, and not because that these hinterland areas were traditionally backward. This thesis, they suggested, can also be applied to the developed nation vis-à-vis the developing one, in the sense that the development of the metropolitan nation (core) was made at the expense of the underdevelopment of the peripheral nation-state.¹ The process of engendering this unequal development, according to Gunder Frank (1979), occurred in the form of unequal trade relationships as well as sociopolitical policies. A classic historical example for this issue can be seen in the underdevelopment of India (Frank 1979).

For the *dependentistas*, their proposals for the causes of underdevelopment were expanded further within a set of strategies for liberation from underdevelopment. Even though they recognized the fact that each country had to work out its own solutions to liberation consistent with its own particular circumstances, there was common agreement that successful development must proceed on two fronts: (1) external change in the structure of the Third World nation's relationships with the imperial metropolitan powers; (2) internal changes within the country contingent

on a transformation of the ruling class. A fair proportion of the Latin American *dependentistas* (depending on their philosophical leanings) stressed for a displacement of the ruling class or comprador elites. By and large, they advocated a socialist revolution. This was a very clear case for Gunder Frank (1969) who envisaged this route as the only way out for the underdeveloped countries then.

Chapters one and two provide to the reader the key pieces that Andre Gunder Frank wrote in the 1960s that formed the basis of his theoretical critique of modernization-developmental paradigm. Even in these earlier studies on development, we can already see the following three main threads that permeated his holistic thinking on world development: the need to consider history, the need to consider the whole, and the need to consider the structure. These three threads would be woven into his lifetime's work in trying to explain what development really means and what it should be. Toward the end of his life he eventually defined the developmental process not as "the development of underdevelopment" as he had originally coined it, but rather as "the underdevelopment of development".¹²

Notes

1. Even though the dependency perspective generated widespread support, it was not without its critics. Critical evaluations have come unexpectedly from the Left and the Right. Two of the most substantive criticisms of the Left are from Ernesto Laclau (1971) and Colin Leys (1977). Mostly, the critics of the Left have challenged and criticized the dependency approach for its eclectic nature and its lack of adherence to Marx's principles. Simply put, the criticisms suggest that dependency theory does not adhere to Marx's theory, and thus is deemed inappropriate for a "correct" analysis of the state of society or for solutions for change to occur. Frank (1974) responded to these criticisms in his article "Dependence Is Dead, Long Live Dependence and Class Struggle."
2. See Frank (1996). In this chapter for his *festschrift*, Gunder Frank summarized his intellectual historical journey.

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CHAPTER ONE

The Development of Underdevelopment

We cannot hope to formulate adequate development theory and policy for the majority of the world's population who suffer from underdevelopment without first learning how their past economic and social history gave rise to their present underdevelopment. Yet most historians study only the developed metropolitan countries and pay scant attention to the colonial and underdeveloped lands. For this reason most of our theoretical categories and guides to development policy have been distilled exclusively from the historical experience of the European and North American advanced capitalist nations.

Since the historical experience of the colonial and underdeveloped countries has demonstrably been quite different, available theory, therefore, fails to reflect the past of the underdeveloped part of the world entirely and reflects the past of the world as a whole only in part. More important, our ignorance of the underdeveloped countries' history leads us to assume that their past and indeed their present resembles earlier stages of the history of the now developed countries. This ignorance and this assumption lead us into serious misconceptions about contemporary underdevelopment and development. Further, most studies of development and underdevelopment fail to take account of the economic and other relations between the metropolis and its economic colonies throughout the history of the worldwide expansion and development of the mercantilist and capitalist system. Consequently, most of our theory fails to explain the structure and development of the capitalist system as a whole and to account for its simultaneous generation of underdevelopment in some of its parts and of economic development in others.

It is generally held that economic development occurs in a succession of capitalist stages and that today's underdeveloped countries are still in a stage, sometimes depicted as an original stage, of history through which the now

developed countries passed long ago. Yet even a modest acquaintance with history shows that underdevelopment is not original or traditional and that neither the past nor the present of the underdeveloped countries resembles in any important respect the past of the now developed countries. The now developed countries were never underdeveloped, though they may have been undeveloped. It is also widely believed that the contemporary underdevelopment of a country can be understood as the product or reflection solely of its own economic, political, social, and cultural characteristics or structure. Yet historical research demonstrates that contemporary underdevelopment is in large part the historical product of past and continuing economic and other relations between the satellite underdeveloped and the now developed metropolitan countries. Furthermore, these relations are an essential part of the structure and development of the capitalist system on a world scale as a whole. A related and also largely erroneous view is that the development of these underdeveloped countries, and within them of their most underdeveloped domestic areas, must and will be generated, or stimulated by diffusing capital, institutions, values, etc., to them from the international and national capitalist metropolises. Historical perspective based on the underdeveloped countries' past experience suggests that on the contrary, economic development in the underdeveloped countries can now occur only independently of most of these relations of diffusion.

Evident inequalities of income and differences in culture have led many observers to see "dual" societies and economies in the underdeveloped countries. Each of the two parts is supposed to have a history of its own, a structure, and a contemporary dynamic largely independent of the other. Supposedly only one part of the economy and society has been importantly affected by intimate economic relations with the "outside" capitalist world; and that part, it is held, became modern, capitalist, and relatively developed precisely because of this contact. The other part is widely regarded as variously isolated, subsistence based, feudal, or precapitalist, and, therefore, more underdeveloped.

I believe on the contrary that the entire "dual" society thesis is false and that the policy recommendations to which it leads will, if acted upon, serve only to intensify and perpetuate the very conditions of underdevelopment they are supposedly designed to remedy.

A mounting body of evidence suggests, and I am confident that future historical research will confirm, that the expansion of the capitalist system over the past centuries effectively and entirely penetrated even the apparently most isolated sectors of the underdeveloped world. Therefore, the economic, political, social, and cultural institutions and relations we now observe there are the products of the historical development of the capitalist system no less than are the seemingly more modern or capitalist features of the national metropolises of these underdeveloped countries.

Analogous to the relations between development and underdevelopment on the international level, the contemporary underdeveloped institutions of the so-called backward or feudal domestic areas of an underdeveloped country are no less the product of the single historical process of capitalist development than are the so-called capitalist institutions of the supposedly more progressive areas. I should like to sketch the kinds of evidence that support this thesis and at the same time indicate lines along which further study and research could fruitfully proceed.

The secretary general of the Latin American Center for Research in the Social Sciences writes in the Center's journal:

The privileged position of the city has its origin in the colonial period. It was founded by the Conqueror to serve the same ends that it still serves today; to incorporate the indigenous population into the economy brought and developed by that Conqueror and his descendants. The regional city was an instrument of conquest and is still today an instrument of domination.¹

The Instituto Nacional Indigenista (National Indian Institute) of Mexico confirms this observation when it notes that "the mestizo population, in fact, always lives in a city, a center of an intercultural region, which acts as the metropolis of a zone of indigenous population and which maintains with the underdeveloped communities an intimate relation which links the center with the satellite communities."² The Institute goes on to point out that "between the mestizos who live in the nuclear city of the region and the Indians who live in the peasant hinterland there is in reality a closer economic and social interdependence than might at first glance appear" and that the provincial metropolises "by being centers of intercourse are also centers of exploitation."³

Thus these metropolis-satellite relations are not limited to the imperial or international level but penetrate and structure the very economic, political, and social life of the Latin American colonies and countries. Just as the colonial and national capital and its export sector become the satellite of the Iberian (and later of other) metropolises of the world economic system, this satellite immediately becomes a colonial and then a national metropolis with respect to the productive sectors and population of the interior. Furthermore, the provincial capitals, which thus are themselves satellites of the national metropolis—and through the latter of the world metropolis—are in turn provincial centers around which their own local satellites orbit. Thus, a whole chain of constellations of metropolises and satellites relates all parts of the whole system from its metropolitan center in Europe or the United States to the farthest outpost in the Latin American countryside.

When we examine this metropolis-satellite structure we find that each of the satellites, including the now underdeveloped Spain and Portugal, serves as an instrument to suck capital or economic surplus out of its own satellites and to channel part of this surplus to the world metropolis of which all are satellites. Moreover, each national and local metropolis serves to impose and maintain the monopolistic structure and exploitative relationship of this system (as the Instituto Nacional Indigenista of Mexico calls it) as long as it serves the interests of the metropolises, which take advantage of this global, national, and local structure to promote their own development and the enrichment of their ruling classes.

These are the principal and still surviving structural characteristics that were implanted in Latin America by the Conquest. Beyond examining the establishment of this colonial structure in its historical context, the proposed approach calls for study of the development—and underdevelopment—of these metropolises and satellites of Latin America throughout the following and still continuing historical process. In this way we can understand why there were and still are tendencies in the Latin American and world capitalist structure that seem to lead to the development of the metropolis and the underdevelopment of the satellite and why, particularly, the satellized national, regional, and local metropolises in Latin America find that their economic development is at best a limited or underdeveloped development.

I believe I have shown in my case studies of the economic and social histories of Chile and Brazil that present underdevelopment of Latin America is the result of its centuries-long participation in the process of world capitalist development.⁴ My study of Chilean history suggests that the Conquest not only incorporated this country fully into the expansion and development of the world mercantile and later industrial capitalist system but that it also introduced the monopolistic metropolis-satellite structure and development of capitalism into the Chilean domestic economy and society itself. This structure then penetrated and permeated all of Chile very quickly. Since that time and in the course of world and Chilean history during the epochs of colonialism, free trade, imperialism, and the present, Chile has become increasingly marked by the economic, social, and political structure of satellite underdevelopment. This development of underdevelopment continues today, both in Chile's still increasing satellization by the world metropolis and through the ever more acute polarization of Chile's domestic economy.

The history of Brazil is perhaps the clearest case of both national and regional development of underdevelopment. The expansion of the world economy since the beginning of the sixteenth century successively converted the Northeast, the Minas Gerais interior, the North, and the Center-South (Rio de Janeiro, Sao Paulo, and Paraná) into export economies and incorporated them into the structure and development of the

world capitalist system. Each of these regions experienced what may have appeared as economic development during the period of its golden age. But it was a satellite development that was neither self-generating nor self-perpetuating. As the market or the productivity of the first three regions declined, foreign and domestic economic interest in them waned and they were left to develop the underdevelopment they live today. In the fourth region, the coffee economy experienced a similar though not yet quite as serious fate (though the development of a synthetic coffee substitute promises to deal it a mortal blow in the not-too-distant future). All of this historical evidence contradicts the generally accepted theses that Latin America suffers from a dual society or from the survival of feudal institutions and that these are important obstacles to its economic development.

During the First World War, however, and even more during the Great Depression and the Second World War, São Paulo began to build up an industrial establishment that is the largest in Latin America today. The question arises whether this industrial development did or can break Brazil out of the cycle of satellite development and underdevelopment that has characterized its other regions and national history within the capitalist system so far. I believe that the answer is no. Domestically the evidence so far is fairly clear. The development of industry in São Paulo has not brought greater riches to the other regions of Brazil. Instead, it has converted them into internal colonial satellites, decapitalized them further, and consolidated or even deepened their underdevelopment. There is little evidence to suggest that this process is likely to be reversed in the foreseeable future except insofar as the provincial poor migrate and become the poor of the metropolitan cities. Externally, the evidence is that although the initial development of São Paulo's industry was relatively autonomous it is being increasingly satellitized by the world capitalist metropolis and its future development possibilities are increasingly restricted.⁵ This development, my studies lead me to believe, also appears destined to limited or underdeveloped development as long as it takes place in the present economic, political, and social framework.

We must conclude, in short, that underdevelopment is not due to the survival of archaic institutions and the existence of capital shortage in regions that have remained isolated from the stream of world history. On the contrary, underdevelopment was and still is generated by the very same historical process that also generated economic development: the development of capitalism itself. This view, I am glad to say, is gaining adherents among students of Latin America and is proving its worth in shedding new light on the problems of the area and in affording a better perspective for the formulation of theory and policy.⁶

The same historical and structural approach can also lead to better development theory and policy by generating a series of hypotheses about

development and underdevelopment such as those I am testing in my current research. The hypotheses are derived from the empirical observation and theoretical assumption that within this world-embracing metropolis-satellite structure the metropolises tend to develop and the satellites tend to underdevelop. The first hypothesis has already been mentioned earlier: that in contrast to the development of the world metropolis, which is no one's satellite, the development of the national and other subordinate metropolises is limited by their satellite status. It is perhaps more difficult to test this hypothesis than the following ones because part of its confirmation depends on the test of the other hypotheses. Nonetheless, this hypothesis appears to be generally confirmed by the nonautonomous and unsatisfactory economic and especially industrial development of Latin America's national metropolises, as documented in the studies already cited. The most important and at the same time most confirmatory examples are the metropolitan regions of Buenos Aires and São Paulo whose growth only began in the nineteenth century, and was, therefore, largely untrammelled by any colonial heritage, but was and remains a satellite development largely dependent on the outside metropolis—first of Britain and then of the United States.

A second hypothesis is that the satellites experience their greatest economic development and especially their most classically capitalist industrial development if and when their ties to their metropolis are weakest. This hypothesis is almost diametrically opposed to the generally accepted thesis that development in the underdeveloped countries follows from the greatest degree of contact with and diffusion from the metropolitan developed countries. This hypothesis seems to be confirmed by two kinds of relative isolation that Latin America has experienced in the course of its history. One is the temporary isolation caused by the crises of war or depression in the world metropolis. Apart from minor ones, five periods of such major crises stand out and are seen to confirm the hypothesis. These are as follows: the European (and especially Spanish) depression of the seventeenth century, the Napoleonic Wars, the First World War, the Depression of the 1930s, and the Second World War. It is clearly established and generally recognized that the most important recent industrial development—especially of Argentina, Brazil, and Mexico, but also of other countries such as Chile—has taken place precisely during the periods of the two world wars and the intervening Depression. Thanks to the consequent loosening of trade and investment ties during these periods, the satellites initiated marked autonomous industrialization and growth. Historical research demonstrates that the same thing happened in Latin America during Europe's seventeenth-century depression. Manufacturing grew in the Latin American countries, and several countries, such as Chile, became exporters of manufactured goods. The Napoleonic Wars gave rise

to independence movements in Latin America, and these should perhaps also be interpreted as in part confirming the development hypothesis.

The other kind of isolation that tends to confirm the second hypothesis is the geographic and economic isolation of regions that at one time were relatively weakly tied to and poorly integrated into the mercantilist and capitalist system. My preliminary research suggests that in Latin America these regions were the ones that initiated and experienced the most promising self-generating economic development of the classical industrial capitalist type. The most important regional cases probably are Tucumán and Asunción, as well as other cities, such as Mendoza and Rosario, in the interior of Argentina and Paraguay during the end of the eighteenth and the beginning of the nineteenth centuries. Seventeenth- and eighteenth-century São Paulo, long before coffee was grown there, is another example. Perhaps Antioquia in Colombia and Puebla and Querétaro in Mexico are other examples. In its own way, Chile was also an example since before the sea route around the Horn was opened this country was relatively isolated at the end of a long voyage from Europe via Panama. All of these regions became manufacturing centers and even exporters, usually of textiles, during the periods preceding their effective incorporation as satellites into the colonial, national, and world capitalist system.

Internationally, of course, the classic case of industrialization through nonparticipation as a satellite in the capitalist world system is obviously that of Japan after the Meiji Restoration. Why, one may ask, was resource-poor but unsatellized Japan able to industrialize so quickly at the end of the century while resource-rich Latin American countries and Russia were not able to do so and the latter was easily beaten by Japan in the War of 1904 after the same forty years of development efforts? The second hypothesis suggests that the fundamental reason is that Japan was not satellized either during the Tokugawa or the Meiji period and, therefore, did not have its development structurally limited as did the countries that were so satellized.

A corollary of the second hypothesis is that when the metropolis recovers from its crisis and reestablishes the trade and investment ties that fully reincorporate the satellites into the system, or when the metropolis expands to incorporate previously isolated regions into the worldwide system, the previous development and industrialization of these regions is choked off or channeled into directions that are not self-perpetuating and promising. This happened after each of the five crises cited earlier. The renewed expansion of trade and the spread of economic liberalism in the eighteenth and nineteenth centuries choked off and reversed the manufacturing development that Latin America had experienced during the seventeenth century, and in some places at the beginning of the nineteenth century. After the First World War, the new national industry of Brazil suffered

serious consequences from American economic invasion. The increase in the growth rate of Gross National Product and particularly of industrialization throughout Latin America was again reversed and industry became increasingly satellized after the Second World War and especially after the post-Korean War recovery and expansion of the metropolis. Far from having become more developed since then, industrial sectors of Brazil and most conspicuously of Argentina have become structurally more and more underdeveloped and less and less able to generate continued industrialization and/or sustained development of the economy. This process, from which India also suffers, is reflected in a whole gamut of balance-of-payments, inflationary, and other economic and political difficulties, and promises to yield to no solution short of far-reaching structural change.

Our hypothesis suggests that fundamentally the same process occurred even more dramatically with the incorporation into the system of previously unsatellized regions. The expansion of Buenos Aires as a satellite of Great Britain and the introduction of free trade in the interest of the ruling groups of both metropolises destroyed the manufacturing and much of the remainder of the economic base of the previously relatively prosperous interior almost entirely. Manufacturing was destroyed by foreign competition, lands were taken and concentrated into latifundia by the rapaciously growing export economy, intraregional distribution of income became much more unequal, and the previously developing regions became simple satellites of Buenos Aires and through it of London. The provincial centers did not yield to satellization without a struggle. This metropolis-satellite conflict was much of the cause of the long political and armed struggle between the Unitarists in Buenos Aires and the Federalists in the provinces, and it may be said to have been the sole important cause of the War of the Triple Alliance in which Buenos Aires, Montevideo, and Rio de Janeiro, encouraged and helped by London, destroyed not only the autonomously developing economy of Paraguay but killed off nearly all of its population unwilling to give in. Though this is no doubt the most spectacular example that tends to confirm the hypothesis, I believe that historical research on the satellization of previously relatively independent yeoman-farming and incipient manufacturing regions such as the Caribbean islands will confirm it further.⁷ These regions did not have a chance against the forces of expanding and developing capitalism, and their own development had to be sacrificed for that of others. The economy and industry of Argentina, Brazil, and other countries that have experienced the effects of metropolitan recovery since the Second World War are today suffering much the same fate, if fortunately still in lesser degree.

A third major hypothesis derived from the metropolis-satellite structure is that the regions that are the most underdeveloped and feudal-seeming today are the ones that had the closest ties to the metropolis in the past.

They are the regions that were the greatest exporters of primary products to and the biggest sources of capital for the world metropolis and were abandoned by the metropolis when for one reason or another business fell off. This hypothesis also contradicts the generally held thesis that the source of a region's underdevelopment is its isolation and its precapitalist institutions.

This hypothesis seems to be amply confirmed by the former super-satellite development and present ultra-underdevelopment of the once sugar-exporting West Indies, Northeastern Brazil, the ex-mining districts of Minas Gerais in Brazil, highland Peru, and Bolivia, and the central Mexican states of Guanajuato, Zacatecas, and others whose names were made world famous centuries ago by their silver. There surely are no major regions in Latin America that are today more cursed by underdevelopment and poverty; yet all of these regions, like Bengal in India, once provided the lifeblood of mercantile and industrial capitalist development—in the metropolis. These regions' participation in the development of the world capitalist system gave them, already in their golden age, the typical structure of underdevelopment of a capitalist export economy. When the market for their sugar or the wealth of their mines disappeared and the metropolis abandoned them to their own devices, the already existing economic, political, and social structure of these regions prohibited autonomous generation of economic development and left them no alternative but to turn in upon themselves and to degenerate into the ultra-underdevelopment we find there today.

These considerations suggest two further and related hypotheses. One is that the latifundium, irrespective of whether it appears today as a plantation or a hacienda, was typically born as a commercial enterprise which created for itself the institutions that permitted it to respond to increased demand in the world or national market by expanding the amount of its land, capital, and labor and to increase the supply of its products. The fifth hypothesis is that the latifundia, which appear isolated, subsistence based, and semifeudal today, saw the demand for their products or their productive capacity decline and that they are to be found principally in the aforementioned former agricultural and mining export regions whose economic activity declined in general. These two hypotheses run counter to the notions of most people, and even to the opinions of some historians and other students of the subject, according to whom the historical roots and socioeconomic causes of Latin American latifundia and agrarian institutions are to be found in the transfer of feudal institutions from Europe and/or in economic depression.

The evidence to test these hypotheses is not open to easy general inspection and requires detailed analyses of many cases. Nonetheless, some important confirming evidence is available. The growth of the latifundium

in nineteenth-century Argentina and Cuba is a clear case in support of the fourth hypothesis and can in no way be attributed to the transfer of feudal institutions during colonial times. The same is evidently the case of the postrevolutionary and contemporary resurgence of latifundia, particularly in the north of Mexico, which produce for the American market, and of similar ones on the coast of Peru and the new coffee regions of Brazil. The conversion of previously yeoman-farming Caribbean islands, such as Barbados, into sugar-exporting economies at various times between the seventeenth and twentieth centuries and the resulting rise of the latifundia in these islands would seem to confirm the fourth hypothesis as well. In Chile, the rise of the latifundium and the creation of the institutions of servitude that later came to be called feudal occurred in the eighteenth century and have been conclusively shown to be the result of and the response to the opening of a market for Chilean wheat in Lima.⁸ Even the growth and consolidation of the latifundium in seventeenth-century Mexico—which most expert students have attributed to a depression of the economy caused by the decline of mining and a shortage of Indian labor and to a consequent turning in upon itself and ruralization of the economy—occurred at a time when urban population and demand were growing, food shortages were acute, food prices were skyrocketing, and the profitability of other economic activities such as mining and foreign trade were declining.⁹ All of these and other factors rendered hacienda agriculture more profitable. Thus, even this case would seem to confirm the hypothesis that the growth of the latifundium and its feudal-seeming conditions of servitude in Latin America has always been and is still the commercial response to increased demand and that it does not represent the transfer or survival of alien institutions that have remained beyond the reach of capitalist development. The emergence of latifundia, which today really are more or less (though not entirely) isolated, might then be attributed to the causes advanced in the fifth hypothesis the decline of previously profitable agricultural enterprises whose capital was, and whose currently produced economic surplus still is, transferred elsewhere by owners and merchants who frequently are the same persons or families. Testing this hypothesis requires still more detailed analysis, some of which I have undertaken in a study on Brazilian agriculture.¹⁰

All of these hypotheses and studies suggest that the global extension and unity of the capitalist system, its monopoly structure and uneven development throughout its history, and the resulting persistence of commercial rather than industrial capitalism in the underdeveloped world (including its most industrially advanced countries) deserve much more attention in the study of economic development and cultural change than they have hitherto received. Though science and truth know no national boundaries, it is probably the new generations of scientists from the underdeveloped countries

themselves who most need to, and best can, devote the necessary attention to these problems and clarify the process of underdevelopment and development. It is their people who in the last analysis face the task of changing this no-longer acceptable process and eliminating this miserable reality.

They will not be able to accomplish these goals by importing sterile stereotypes from the metropolis, which do not correspond to their satellite economic reality and do not respond to their liberating political needs. To change their reality they must understand it. For this reason, I hope that better confirmation of these hypotheses and further pursuit of the proposed historical, holistic, and structural approach may help the peoples of the underdeveloped countries to understand the causes and eliminate the reality of their development of underdevelopment and their underdevelopment of development.

Notes

This essay originally appeared in the September 1966 issue of *Monthly Review*.

1. *América Latina*, Año 6, No. 4 (October–December 1963), p. 8.
2. Instituto Nacional Indigenista, Los centros coordinadores indigenistas (Mexico, 1962), p. 34.
3. *Ibid.*, pp. 3, 3–34, 88.
4. Andre Gunder Frank, "Capitalist Development of Underdevelopment in Chile" and "Capitalist Development of Underdevelopment in Brazil" in *Capitalism and Underdevelopment in Latin America* (New York and London: Monthly Review Press, 1967 and 1969).
5. Also see, Andre Gunder Frank, "The Growth and Decline of Import Substitution," *Economic Bulletin for Latin America*, Vol. IX, No. 1 (March 1964); and Celso Furtado, *Dialectica do Desenvolvimento* (Rio de Janeiro: Fundo de Cultura, 1964).
6. Others who use a similar approach, though their ideologies do not permit them to derive the logically following conclusions, are Aníbal Pinto, *Chile: Un caso de desarrollo frustrado* (Santiago: Editorial Universitaria, 1957); Celso Furtado, *A formação econômica do Brasil* (Rio de Janeiro: Fundo de Cultura, 1959), which was recently translated into English and published as *The Economic Growth of Brazil* by the University of California Press; and Caio Prado Junior, *Historia Econômica do Brasil*, 7th ed. (Sao Paulo: Editora Brasiliense, 1962).
7. See, e.g., Ramiro Guerra y Sánchez, *Azúcar y Población en las Antillas*, 2nd ed. (Havana, 1942), also published as *Sugar and Society in the Caribbean* (New Haven, CT: Yale University Press, 1964).
8. Mario Góngora, *Origen de los "inquilinos" de Chile central* (Santiago: Editorial Universitaria, 1960); Jean Borde and Mario Góngora, *Evolución de la propiedad rural en el Valle del Puango* (Santiago: Instituto de Sociología de la Universidad de Chile, 1959); Sergio Sepúlveda, *El trigo chileno en el mercado mundial* (Santiago: Editorial Universitaria, 1959).
9. Woodrow Borah makes depression the centerpiece of his explanation in "New Spain's Century of Depression," *Ibero-Americana*, No. 35 (Berkeley, 1951). François Chevalier speaks of turning in upon itself in the most authoritative study of the subject, "La formación de los grandes latifundios en México," *Problemas Agrícolas e Industriales de México*, Vol. VIII, No. 1 (1956) (translated from the original French and recently published by the University of California Press). The data that provide the basis for my contrary interpretation are supplied by these authors themselves.
10. Andre Gunder Frank, "Capitalism and the Myth of Feudalism in Brazilian Agriculture," in *Capitalism and Underdevelopment in Latin America* (New York and London: Monthly Review Press, 1967 and 1969).

CHAPTER TWO

Sociology of Development and the Underdevelopment of Sociology

Introduction

This essay examines the sociology of development currently being produced in the developed countries, especially the United States, for export to and use in the underdeveloped countries. On critical examination, this new sociology of development is found to be empirically invalid when confronted with reality, theoretically inadequate in terms of its own classical social scientific standards, and policy-wise ineffective for pursuing its supposed intentions of promoting the development of the underdeveloped countries. Furthermore, the inadequacy grows along with the development of the society that produces it. Like the underdeveloped society to which it is applied, this sociology is becoming increasingly underdeveloped.

To permit a careful and detailed evaluation of this sociology of development, I shall examine the theoretical modes or trends represented by particular writings of selected social scientists. Nonetheless, my critique extends to the whole of this sociology of development. To avoid arbitrary selection, it is convenient to permit representatives of this sociology of development themselves to select the major modes and most of the authors to be examined here. Accordingly, they are given the first word.

Manning Nash, until recently editor of EDCC, has said,¹ "There are, in my view, only three modes of attacking the problem of social change and economic development."

The first mode is the index method: the general features of a developed economy are abstracted as an ideal type and then contrasted with the equally ideal typical features of a poor economy and society. In this mode,

development is viewed as the transformation of one type into the other. Developed examples of this mode are to be found in Hoselitz's *Sociological Factors in Economic Development*,² or Parsons' *Structure and Process in Modern Societies*,³ or in some of the work of the sociologist Marion J. Levy, Jr.⁴

The second mode is the acculturation view of the process of development. The West (taken here as the Atlantic community of developed nations and their overseas outliers) diffuses knowledge, skills, organization, values, technology, and capital to a poor nation, until over time its society, culture, and personnel become variants of that which made the Atlantic community economically successful. Examples of this line of reasoning can be found in Moore and Feldman, *Labor Commitment and Social Change in Developing Areas*⁵ (which also includes essays by Nash and Hoselitz), and in Lerner's *Passing of Traditional Society*,⁶ or in the many accounts of how the Soviet Union and Japan "did it."

The third mode is the analysis of the process as it is now going on in the so-called underdeveloped nations. This approach leads to a smaller-scale hypothesis, to a prospective rather than a retrospective view of social change, to a full accounting of the political, social, and cultural context of development.

Nash's discussion of these currents in contemporary American work on economic development and cultural change is found in his introduction to a collection of essays by, among others, Everett Hagen (who first introduced his thesis in the pages of *EDCC*),⁷ David McClelland (who reviewed Hagen's book in the pages of *EDCC*),⁸ and John H. Kunkel (who recently discussed the third approach in *EDCC*).⁹ Nash describes these authors' essays as representative of the third approach and commends them for their "dialectic of social knowledge, of confrontation of bold assertion against fact in even bolder more elegant assertion."¹⁰ Robert Chin, coeditor of the collection, says that these writers "are performing a pioneering service."¹¹

Nash's classification, summary, and evaluation of the "only three modes of attacking the problem of social change and economic development" can serve as a useful point of departure for our own examination and evaluation of these approaches. Nash is quite mistaken in claiming that these modes exhaust the possibilities of attacking the problems of social change and economic development; he is substantially correct, however, in observing that they virtually exhaust the approaches of American social scientists to these problems of vital contemporary concern.¹²

I propose, therefore, to examine and evaluate the empirical validity, theoretical adequacy, and policy effectiveness of these three approaches to the problems of development. In terms of their relative importance we should begin with the criterion of policy effectiveness, and then consider theoretical adequacy and empirical validity, in that order. For if the recommended policy is ineffective, it renders suspect the theory from which

it is derived; if the theory used is inadequate, it matters relatively little whether the claims made about particular aspects of reality are in fact empirically accurate. Contrary to the logic of the case, however, expository convenience leads me to begin with an examination of the empirical validity of each approach, for this permits us to familiarize ourselves with the approach under review. We will then proceed to the questions of theoretical adequacy and policy effectiveness in turn.

The Ideal Typical Index Approach

The index method is an attempt to attack the problem of economic development and cultural change through the comparative statics of polar ideal types. Referring to the approach of economists generally, and to those of the World Bank in particular, Charles Kindleberger long ago labeled this mode the gap approach: you subtract the ideal typical features or indices of underdevelopment from those of development, and the remainder is your development program.¹³ We may distinguish two major variants of this ideal typical gap approach: the pattern variable approach exemplified by Hoselitz, and the historical stage approach now mostly associated with Rostow. The second variant differs from the first in that it draws on the historical experience of the developed countries to interpose stages into the gap between development and underdevelopment. A further variant of the latter, the historical variations approach of Gerschenkron, which is not examined here, draws on this same historical experience to introduce the possibility of variation into the development stages of the underdeveloped countries. Common to all three variants is the assumption that underdevelopment is an original state which may be characterized by indices of traditionalism, and that, therefore, development consists of abandoning these characteristics and adopting those of the developed countries.

Pattern Variables

This mode is derived not only from Max Weber's conception of the ideal type in general but also from some of Weber's particular ideal types, which were later elaborated and further systematized by Talcott Parsons. Hoselitz takes the pattern variables of Parsons' *Social System*¹⁴ and applies them to the study of economic development and cultural change.

The pattern variables, according to the *Dictionary of Sociology*, are

types of choices open to purposive human beings; they are dichotomies....each representing polar extremes. *Universalism* and

particularism are the names of one. In other words, any individual in a situation requiring choice in his relationships with others must ask himself if he is going to act in terms of a universally accepted precept or one particular to the situation in which he finds himself. Is he going to act according to rule or in terms of particular qualities of the person towards whom he is orienting his action. Another set is termed *achievement* and *ascription* (sometimes referred to as *performance* and *quality*) and here a person in deciding how to act focuses his attention on either the achieved aspects of the other person, e.g., his professional qualifications, or else his ascribed qualities, e.g., sex, age, social class.... Yet another set is known as *specificity* and *diffuseness*, and here the choice takes into account limited and specific factors, e.g., the contrast between a contract entered into, and wider diffuse obligations such as family loyalty.

The point of this scheme of pattern variables is to enable the sociologist to identify the typical choices made, especially of an institutionalized kind.... Pattern variable analysis may be *used* to identify similarities and differences between cultures, or it may be restricted in use to refer to aspects of society, to sub-systems of an institutionalized kind, such as political systems...¹⁵

According to Parsons any social system and any social action can be exhaustively analyzed in terms of only five pairs of pattern variables, which supposedly characterize all possible social action. These five pairs of pattern variables are the three above-defined ones used by Hoselitz, plus *affectivity* and *affective-neutrality* and *self-orientation* and *collectivity-orientation*.

Hoselitz first advanced his theory in 1953 under the title "Social Structure and Economic Growth";¹⁶ he repeated the same thesis again (more penetratingly, he says in a footnote) in 1963 under the title "Social Stratification and Economic Development."¹⁷ He argues that developed countries exhibit the pattern variables of universalism, achievement orientation, and functional specificity, while underdeveloped ones are characterized by their opposites—particularism, ascription, and functional diffuseness. To develop, Hoselitz counsels, underdeveloped countries should eliminate the pattern variables of underdevelopment and adopt those of development. It may be added that *EDCC* has devoted many pages to the diffusion of this approach to the study of economic development and cultural change.¹⁸

Empirical Validity

Hoselitz characterizes the developed countries as universalist and not particularist. They are, as we shall see, normatively universalist. Yet the

reality, the literature, and even the sociological treatment of many developed countries reveal substantial particularism. This is specifically the case for Japan,¹⁹ France,²⁰ and Europe in general,²¹ where the existence of particularism has been demonstrated among both upper and lower classes. Particularism is deep and widespread especially in the working class in both Europe²² and the United States, in recent migrants from the former to the latter, and among nonwhite, rural, or recent rural-urban migrant groups in the United States. Moreover, much of what flies a universalist flag in the United States and other developed countries is little more than the cover for unsavory particularist private interests. We will have occasion later to observe that the developed countries export particularism to the underdeveloped ones, wrapped in such universalist slogans as freedom, democracy, justice, the common good, the economic liberalism of free trade, the political liberalism of free elections, the social liberalism of free social mobility, and the cultural liberalism of free flow of ideas such as the ones we are examining here.²³

Hoselitz also characterizes developed countries as achievement oriented. To examine the counterpart of this pattern variable in reality, it is important to divide it into three subvariables: reward, recruitment, and motivation. In the United States, reward within roles is indeed substantially dependent on achievement. But recruitment into roles, although perhaps substantially a matter of achievement among the middle classes, is very much based on ascription in both the high levels of business management, as Granick has shown in his comparison of American and Soviet management,²⁴ and among the masses of poor in the "other America," as Michael Harrington has so dramatically demonstrated. The ascription of roles, and the consequent reward, to the American Negro speaks silently and eloquently for itself through his contemporary Freedom Movement. Harrington shows, moreover, that far from becoming less ascriptive, American society, both at the top and at the bottom (and perhaps also in the middle), is becoming progressively more ascriptive.²⁵

On the other hand, role recruitment in Japan is very much based on achievement, as Abegglen among others has pointed out.²⁶ However, the assignment of reward within the role, Abegglen argues, is highly ascriptive, being based on such factors as age, family obligations, and so on. The important distinction between recruitment and reward (rarely made in discussions of achievement or ascription) and the obvious differences between Japanese and American practices in this respect would seem to explain a large part of the disagreement on this matter. For example, Bellah²⁷ and Levy,²⁸ who emphasize Japan's achievement orientation as a cause of its development, refer to role recruitment. On the other hand, Abegglen,²⁹ who emphasizes Japan's ascriptive pattern, is apparently thinking of reward within roles. The other achievement variable, individual

achievement motivation or need for) achievement as David McClelland calls it,³⁰ while increasingly confused with the Weberian category of social role assignment and reward, is quite another matter and will be discussed when we come to examine the third mode.

Thirdly, Hoselitz claims that in developed societies roles are functionally specific rather than diffuse and that role specificity helps generate development while role diffuseness does the contrary. To assess this claim, we must first question the relevance of the specificity-diffuseness dichotomy to the structure of interaction that is being examined. Is it useful to distinguish the structure of interaction between ego and alter that is normatively defined in one diffuse role as a complex father-son, teacher-student, general-soldier, and so on relationship, from the structure of interaction in functionally specific roles that are integrated in such a way that ego is systematically father, teacher, general, and so on, and alter is son, student, soldier, and so on? In a word, how important is the difference between role specificity and role diffuseness if the socially significant and dominant specific roles are collected together in one or a few individuals who wear many hats simultaneously or in quick and institutionalized succession? For the latter is the "functionally specific" role structure of the society in which, according to C. Wright Mills, the power elite dominates what President Eisenhower dubbed the military-industrial complex, and in which Douglas Dillon of Dillon Reed & Co. comes to sit in the cabinet as secretary of the treasury; Robert McNamara, president of the Ford Motor Company, becomes secretary of defense—as successor to "Engine Charley" Wilson, who gave us the *bon mot*, "What's good for General Motors is good for the country"; and in which the bulk of military purchases are from a half dozen giant corporations who employ large numbers of retired high-level military officers.³¹

Our own profession is not as isolated from this role structure as Hoselitz's characterization of role specificity might suggest: Roosevelt's and Kennedy's brain trusts co-opted all sorts of American social scientists. Harvard historian Arthur Schlesinger, Jr.'s aid to the development of underdeveloped countries has so far consisted in writing the now famous White Paper on Cuba, which was intended to justify the coming invasion of that country at the Bay of Pigs. He later admitted lying about the invasion in the "national interest." Stanford economist Eugene Staley wrote *The Future of Underdeveloped Countries*³² and then planned it in the renowned Staley—[General Maxwell] Taylor Plan to put 1\$ million Vietnamese in the concentration camps they euphemistically christened "strategic hamlets." Since the failure of that effort at development planning, MIT economic historian Walt Whitman Rostow has escalated the effort by writing *The Stages of Economic Growth: A Non-Communist Manifesto*.³³ He wrote of these stages at the CIA-financed Center for International Studies on the Charles

River and has been operationalizing them on the Potomac as President Kennedy's director of policy and planning in the State Department and President Johnson's chief adviser on Vietnam. It is on behalf of Vietnamese economic growth that Rostow has become the principal architect of escalation, from napalming the South to bombing the North, and beyond. Then, doubtless due to universalist particularism and achieved ascription, Eugene Rostow moves from professing international law at Yale University to practicing it at his brother's side in Washington. Meanwhile, after performing his role as dean of humanities at Harvard University, McGeorge Bundy becomes W. W. Rostow's superior in Washington and goes on television to explain to the misguided and incredulous why this economic development theory and policy is humanitarian (after which he goes on to direct the Ford Foundation and its influence on education and research). In the light of the manifest and institutionalized role-summation and diffuseness of these deans of humane scholarship and professors of applied social science, the clandestine direction of Project Camelot by the Department of Defense and the financing of the United States National Student Association by the CIA pale into the shadows.

However, Hoselitz's and my concern is with the economic development and cultural change of the *underdeveloped* countries. It is, therefore, more important to examine the reality of underdevelopment and Hoselitz's ideal typical mischaracterization of it. Hoselitz characterizes the underdeveloped countries as particularist rather than universalist. Yet normatively, underdeveloped countries are also substantially universalist. A glance at the press, radio, and much of the educational ideology of any underdeveloped country exhibits just as much universalism as do their counterparts in the developed ones. Mexico's most influential newspaper publishes more column inches about the "universalist" United States than the *New York Times* does about the whole world outside of the United States; and one American magazine, the *Reader's Digest*, which excels in getting across the American "universalist" norms and ideology, has a higher circulation in Mexico than the eight largest Mexican magazines combined.³⁴ What makes Hoselitz right in a sense is that this kind of universalism goes no deeper in the underdeveloped countries than it does in the developed ones; for there too it is, instead, a cover for underlying particularism. On the other hand, there are forms of universalism in the underdeveloped countries apart from the superficial façade of the particularly interested organs of public opinion formation. There are general and political strikes, decried by so many of these same observers from the developed countries; militant nationalism, which the same observers frown upon as opposed to the universal good and, therefore, to the particular one of this or that underdeveloped country; and widespread support in underdeveloped countries for the anticolonial and anti-neocolonial movements, which the

developed countries are combating by force of arms and universalistic-sounding propaganda about freedom, and so on, in Vietnam, Malaysia, the Congo, the Dominican Republic, and elsewhere. This evidence suggests that universalism is after all quite widespread and deeply ingrained in the underdeveloped countries among groups that are not the privileged ones in command of the universalist organs of communication.

Hoselitz departs even further from reality when he says that social, economic, and political roles in the underdeveloped countries are distributed almost exclusively in terms of ascriptive norms. He specifically claims that the underdeveloped countries pay little attention to economic achievement in their determination of status and that political leadership is mainly determined by ascriptive norms.³⁵ Someone who has never lived in the universalist castle of American social science would be shocked to find that Hoselitz and many others characterize as ascriptive the national political leadership produced by the interminable military coups in Latin America,³⁶ and by the emerging "national" bourgeoisies all over Africa.³⁷ Yet the unreality of American popular and ostensibly scientific understanding of the world permits Hoselitz and others to suggest that Latin American political power is in the hands of some traditional landed or even feudal oligarchy. They fail to see that in all capitalist underdeveloped countries the power behind the throne, be it military or civil, rests (if it is in national hands at all) with the people who occupy the top roles in the economic organization, and particularly with those who have commercial and financial ties to the developed metropolis.³⁸ This metropolis is increasingly the United States—precisely the vantage point from which these social scientists make their curious observations and characterizations of the underdeveloped part of the world. In supposedly ascriptive Asia, Africa, and Latin America, many present incumbents of these top economic and political roles have achieved their positions, and done so quite recently—often more so than in the achievement-oriented developed countries of Europe and North America.³⁹ Thus, role assignment in the economically and politically most significant roles in underdeveloped countries is decidedly achieved and not ascribed.

It should be pointed out, however, that role assignment by achievement is also common among the lower-level roles in the underdeveloped countries. This has been the case at least since mercantilist and capitalist penetration totally transformed these societies, often centuries ago. Only the social scientists from the invading metropolis seem unable to see how efficiently this penetration integrated these societies into the dominant world system and how universally the latter imposed its social organization and alienation on the people whom Frantz Fanon has called the damned of the earth.⁴⁰ Evidently, the distribution of rewards in underdeveloped countries, at least in high-level roles, is also determined by achievement—as Hoselitz

uses that term. In the monopolistic underdeveloped economies, even more than in the developed ones, financial success is determined by successful speculation and extortion, and the resulting distribution of income is even more unequal. This suggests that, contrary to what Hoselitz says, ascription counts less, and achievement more, in the distribution of reward in the underdeveloped countries.⁴¹ (This assumes that we may call this sort of success "achievement" by our universalist standards, which the present author would not wish to do.)

Finally, Hoselitz says that roles in underdeveloped countries are functionally diffuse rather than specific. This is true in part. The poor in the underdeveloped countries, whether classified as working in the primary, secondary, or tertiary sector, do indeed practice many professions at a time, such as farmer, trader, peddler, artisan, odd jobber, thief, and provider of social security to others, in the attempt to keep body and soul together.⁴² The roles at the other end of the socioeconomic scale are no less diffuse. One need only read the daily press or suffer the consequences of monopoly control in underdeveloped countries to know that the controlling roles are indeed diffuse, as Hoselitz suggests, and also that economic roles predominate in that control, as Hoselitz denies. On the other hand, it is also well to observe that a whole series of intermediate roles in underdeveloped societies, occupied by such members of the middle classes as military officers, government bureaucrats, junior executives, administrators, policemen, and others, are functionally quite specific. Their incumbents serve specific functions of making the whole exploitative system function in the diffused but particular interest of those who have achieved control, in the same sense that the plantation administrator runs the owner's slave plantation for him. It is perhaps not surprising that it is among precisely these middle-role incumbents that universalist values are predominant.⁴³

In a word, if we examine the patterns of social roles in the developed and underdeveloped countries, instead of being blinded by a hand-me-down ideal typical perspective of adulterated Weberian parentage, we conclude that the characteristics Hoselitz and others attribute to developed and underdeveloped countries present a distorted and inadequate conception of social reality. This is, however, the least of the deficiencies of Hoselitz's and allied approaches to economic development and cultural change. That it is so easy to challenge the empirical validity of Hoselitz's conception of development and underdevelopment—that Hoselitz can find some particularism, ascription, and diffuseness in underdeveloped countries, whereas we can easily find universalism, achievement, and specificity there—already suggests that probably neither the one nor the other of the patterns of variables Hoselitz selects for emphasis is important for characterizing, or crucial for determining,

either development or underdevelopment. It raises the suspicion that the important determining factors of development and underdevelopment are not these but others: that is, the theoretical adequacy of Hoselitz's whole approach is cast in doubt.

Theoretical Adequacy

Having disposed of the empirical validity of Hoselitz's claims, we may examine the theoretical adequacy of his thesis in terms of, first, his selection of roles for study; second, his selection of a social system for analysis; and third, and most important, his treatment of the social structure of development and underdevelopment.

It may be best to begin by asking how Hoselitz and I can characterize the pattern of variables or roles in underdeveloped countries so differently. Part of the answer is found in the difference between the roles we deem important for underdevelopment and development. It appears that in Hoselitz's analysis all roles have about the same weight in characterizing and determining underdevelopment. Thus Hoselitz's prescription for development is that the maximum number of roles, almost irrespective of which they are, change from being particularist, ascriptive, and diffuse, and become universalist, achievement based, and functionally specific. The greater this quantitative change of roles from one pattern to the other, it would seem, the greater the development. My review, on the other hand, has lent more emphasis to roles at the top and some at the bottom of the economic and political stratification systems, because they are more important for development than just roles in general.

If social roles do not all carry the same weight or importance for development and underdevelopment, as they evidently do not, then it is not legitimate to assign them the same weights in theory. If, like Hoselitz, we construct ideal-type role patterns for development and underdevelopment (a dubious procedure to begin with) then in constructing the ideal-type we must surely assign more weight to the roles that in fact are more important for development or underdevelopment, even if they be less numerous. Yet in his characterization of both developed and underdeveloped societies, Hoselitz systematically evades the specific examination of the top economic and political roles. If Hoselitz lent these roles the weight they clearly have in the determination of development or underdevelopment, he would be unable to characterize as universalist, achievement based, and functionally specific a society in which the power elite of the industrial-government-military complex pursues particularist ends; or would be unable to characterize as particularist, ascriptive, and functionally diffuse those countries that are governed by oligarchies with economic, political, and military power that is derived from commercial monopoly privileges

and the recurrent recourse to force of arms to protect and augment them. Still less would he be able to rest his theoretical case for development and underdevelopment on this empirical base.

Secondly, we may ask what social universe Hoselitz has in mind when he says that development is characterized by some pattern variables and underdevelopment by others. Hoselitz and many others associate particularism, ascription, and diffuseness in underdevelopment with the extended family, the primitive tribe, the folk community, the traditional sector of a dual society, and with the underdeveloped countries and part of the world in general. But the connection is never made with the developed part of the world nor with the contemporary dominant social organization in the world taken as a whole. Indeed, he seems to be indifferent about where change should take place, since in discussing underdevelopment he moves quite easily and almost imperceptibly from referring to one of these units to talking about another (although never, of course, to the last two). Hoselitz leaves far from clear just which is the social whole whose role patterns he would change from one set of variables to another in order to effect development. Here the theoretical inadequacy is even more glaring, for it contravenes the generally accepted rule of social and all scientific theory to look for and refer to the systemic whole in terms of which the reality (in this case underdevelopment) can be explained and changed. The social system that is today the determinant of underdevelopment certainly is not the family, tribe, community, a part of a dual society, or even, as I shall argue later, any underdeveloped country or countries taken by themselves.

The folk characteristics that were studied by Robert Redfield, and that Hoselitz seems to associate with the pattern variables of underdeveloped society, do not characterize any whole society existing today. At best, they may characterize "tribal societies," few if any of which still remain independent. Redfield himself only spoke in terms of a nontribal folk society when he first studied Yucatan and Tepotzlan, and even then he entitled his book *The Folk Culture of Yucatan*.⁴⁴ When he later began to concentrate his attention on *Peasant Society and Culture*,⁴⁵ he took great pains to point out that peasants with folk characteristics live only in *parts* of societies inasmuch as they are peasants only by virtue of their relation to the city, whose function complements theirs within the same wider social whole that incorporates them both. Furthermore, in his study of the Guatemalan peasant community, Cantel,⁴⁶ Manning Nash himself pointed out that the appearance of the universalist, achievement-oriented, and functionally specific characteristics associated with labor unionism—and their renewed disappearance after the 1954 military coup of which John Foster Dulles was so proud—must be traced beyond the boundaries of the community to the national system. In view of the well-known source of that military coup we might add that it should be traced further to the functioning and

structure of the international system, never mentioned by Hoselitz, but of which Cantel, Guatemala, and all their inhabitants form integral if unhappily determined parts. Therefore, it is not a matter of empirical, theoretical, or policy indifference just which social system is selected for study and change with a view to promoting economic development. Hoselitz's selection is empirically unacceptable because he does not choose to study the system whose characteristics are the determinant ones for development and underdevelopment. Hoselitz's procedure is theoretically unsatisfactory because he does not address himself to the determinant social whole as Redfield counseled that social scientists must do.⁴⁷

Thirdly, Hoselitz's treatment of economic development and cultural change is unsatisfactory on still more important theoretical grounds: his analysis belies its own title, "Social Structure and Economic Growth," by neglecting structure and especially the structure of underdevelopment. The previously discussed empirical and theoretical shortcomings in analyses such as Hoselitz's are of course part and parcel of this neglect. However, the failure of those using this approach to take adequate account of structure is of such far-reaching importance that it requires more specific commentary of its own.

Hoselitz follows the lead of Talcott Parsons who, to commemorate the centenary of the *Communist Manifesto*, explained the theoretical significance and political consequences of his own and "modern sociological theory":

Marx, however, tended to treat the socioeconomic structure of capitalist enterprise as a single indivisible entity rather than breaking it down analytically into a set of the distinct variables involved in it. It is this analytical breakdown which is for present purposes the most distinctive feature of modern sociological analysis.... It results in a modification of the Marxian view.... The primary structural emphasis no longer falls on... the theory of exploitation but rather on the structure of occupational roles...⁴⁸

The felicity of Parsons' analysis of this approach has already been empirically confirmed for us by Hoselitz's aforementioned practice of confining his attention to the arithmetic sum of social roles in general, and of forgetting about the social, political, and economic structure of a particular society under study.

Herein, Parsons, Hoselitz, and recent sociological theorists in general not only modify Marx but also depart from Weber. Parsons' structuralism and holism is confined to the analysis of a wholly abstract model of any and all real or imaginary societies and not with the study of any existing real society. However much Marx and Weber may have relied on theoretical

models and ideal types, neither ever ventured to depart so far from reality. Other recent sociological theorists, mostly social anthropologists of the British structural-functionalist school, who have devoted themselves to the study of existing whole societies, fall short of the standards of classical sociology in other ways. They select small "societies" in Africa and elsewhere for study and analyze them as though they had an isolated existence independent from the imperialist system of which they formed an integral part at the time of study. Hoselitz abandons classical sociology and carries recent sociology still further. He leaves behind the structural holism of Parsons because it is not suited to any but abstract wholes. Yet he does not join the anthropologists on their field trips to study the social structure of social "wholes." Hoselitz is satisfied to abandon both holism and structuralism and to devote his attention to pattern variables. The aforementioned theorists deviate further from classical theory, which is a most serious handicap for those who would study economic development and cultural change. "Modern sociological theory" at best appeals to holism and structuralism to explain the existence of the parts, or merely to demonstrate the relations among them, but not to analyze or account for the existence of the social structure as a whole. Consequently, these theorists, who pretend to analyze economic *development* and cultural *change*, fail to direct their theoretical analysis to the past origins, the present transformations, or the future prospects of the existing social system as a system.

Yet Hoselitz and, as we shall see, the advocates of the second and third modes of analysis, all take another step beyond Parsons—and far beyond what would have occurred to Weber in his moments of wildest fancy. They argue that to eliminate underdevelopment and produce development it is only necessary to change particular variables, roles, or parts of the social system—that it is not necessary to change the structure of the system itself. Logically, Hoselitz and others can take this position only if they maintain one or the other of the following: (1) that underdevelopment and development are associated only with the characteristics of the simple majority of the society's roles, and not with the structure of that society; or (2) granted that development and underdevelopment are associated with the structure of the social system, the system's structure can be changed simply by changing some of its parts or their characteristics. The first violates all standards of social scientific theory; the second is contrary to all empirical reality.

The importance of the empirical and theoretical deficiency of the approach of Hoselitz and others cannot be stressed too much. The empirical evidence, which has been discussed, reveals that this criticism of Hoselitz's and related analyses on theoretical grounds is not based on an isolated appeal to arbitrary theoretical standards. That is, the weight of the scientific standards that such analyses fail to meet lies not so much in their

universal acceptance as it does in their realism and efficacy: if Hoselitz and others had guided their observations and analyses of economic development and cultural change by these standards of structuralism and holism, they could not have come to the empirically erroneous conclusion that ascriptive role assignment in general is keeping underdeveloped countries underdeveloped. They would have seen not only that the crucial political and economic roles in underdeveloped countries are assigned and rewarded by achievement—which is the least of it, since it is not, after all, ascription or achievement that is really important—but also that these roles and their incumbents are no more than some of the manifestations of the real structure of development and underdevelopment of a world-embracing system that gives rise to these roles and whose incumbents in turn serve to maintain the system and underdevelopment in particular.

Policy Effectiveness

Three examples may suffice to indicate that Hoselitz's policy prescriptions do not lead to the consequences he predicts. First, the existence, or the increase (if we would believe C. Wright Mills⁴⁹ or William H. Whyte⁵⁰) of role ascription and diffuseness in business, government, and military circles in the United States has not so far turned that country into an underdeveloped one. A second piece of evidence is that the supposed achievement of functionally specific roles and pursuit of universal standards among, for instance, the business magnates and their military executors in Latin America has not so far developed their countries and still gives no signs of doing so.

Although perhaps not the most important one, a third piece of evidence against Hoselitz's thesis is particularly interesting because it is supplied by Hoselitz himself. As we saw earlier, Hoselitz's pattern variables of development are associated particularly with the rise of the middle classes; and such students of Latin America as John Johnson⁵¹ in the United States and Gino Germani⁵² in Argentina, among many others, have argued that the greater the social mobility and the bigger the middle class the more the development. Yet Hoselitz recently took the initiative to test this thesis by confronting it with the hard facts of reality in Latin America. There he found and wrote that the countries with the largest middle classes, Argentina and Chile, are not at all the ones with the most development.⁵³

Three things, however, are true of middle classes in Latin America. First, their social pattern closely corresponds to the one to which Hoselitz wishes to attribute economic development and cultural change. Secondly, as in Nazi Germany and Fascist Italy, it is precisely these groups that provide the principal "popular" support for the ultrareactionary military dictatorships, as these groups again demonstrated in an impressively

manifest fashion in the 1964 military coup in Brazil.⁵⁴ A third fact, which is not unrelated to the foregoing one or to the unviability of the development prescriptions of Hoselitz, Johnson, Germani, and others, is that throughout the underdeveloped countries (as well as in the United States as Gabriel Kolko has recently shown⁵⁵), when the income of these middle classes rises it does so not at the expense of the rich but at the expense of the large masses of the poor—whose relative and often absolute income in the underdeveloped countries is thereby forced still lower.⁵⁶ Economic development and cultural change of an underdeveloped country through the promotion and rise of the middle classes (or their pattern variables) has not occurred because, among other reasons, it is physically impossible for it to occur given the structure of the system: it only leads to the further underdevelopment of the majority.

Stages of Growth

Within the first ideal-typical mode, which Nash calls the index mode and which I call the gap approach, we may distinguish a second variant. Here the identification of the gap between the characteristics of development and underdevelopment includes the specification of intermediate stages and their characteristics. Although Nash mentioned Rostow in connection with his earlier work on development propensities,⁵⁷ it is preferable to take Rostow's *Stages of Economic Growth* as the example of this variant of the first mode. My review and evaluation of Rostow's and similar "stage" approaches requires less space because first, much of the criticism already made of Hoselitz applies to them as well and, second, Rostow's stages have already come in for much specific criticism from others.⁵⁸ Nonetheless, I submit that Rostow's *Stages of Economic Growth* deserves more fundamental criticism on empirical, theoretical, and policy grounds than it has thus far received.

According to Rostow:

It is possible to identify all societies, in their economic dimensions, as lying within five categories: the traditional society, the preconditions for takeoff, the takeoff, the drive to maturity, and the age of high mass consumption. First, the traditional society. A traditional society is one whose structure is developed within limited production functions, based on pre-Newtonian science and technology, and on pre-Newtonian attitudes towards the physical world. . . . The second stage of growth embraces societies in the process of transition; that is, *the* period when the preconditions for takeoff are developed; for it takes time to transform a traditional society in the ways necessary for it to exploit the fruits of modern science, to fend off diminishing

returns, and thus to enjoy the blessings and choices opened up by the march of compound interest....[T]he stage of preconditions arise[s] not endogenously but from some external intrusion by more advanced societies.... We come now to the great watershed in the life of modern societies: the third stage in this sequence, the takeoff. The takeoff is the interval when the old blocks and resistances to steady growth are finally overcome. The forces making for economic progress, which yielded limited bursts and enclaves of modern activity, expand and come to dominate the society. Growth becomes its normal condition. Compound interest becomes built, as it were, into its habits and institutional structure....[The] takeoff is defined as requiring all three of the following related conditions: (1) a rise in the rate of productive investment from, say, 5 percent or less to over 10 percent of national income (or net national product (NNP)); (2) the development of one or more substantial manufacturing sectors, with a high rate of growth; (3) the existence or quick emergence of a political, social and institutional framework which exploits the impulses to expansion...⁵⁹

Rostow's stages and thesis are incorrect primarily because they do not correspond at all to the past or present reality of the underdeveloped countries whose development they are supposed to guide. It is explicit in Rostow, as it is implicit in Hoselitz, that underdevelopment is the original stage of what are supposedly traditional societies—that there were no stages prior to the present stage of underdevelopment. It is further explicit in Rostow that the now developed societies were once underdeveloped. But all this is quite contrary to fact. This entire approach to economic development and cultural change attributes a history to the developed countries but denies all history to the underdeveloped ones. The countries that are today underdeveloped evidently have had a history no less than have the developed ones. None of them, for example India,⁶⁰ is today the way it was centuries or even decades ago. Moreover, reference to even any schoolboy world history confirms that the history of the now underdeveloped countries has been most intimately related to the history of the now developed ones for at least several centuries.

Indeed, the economic and political expansion of Europe since the fifteenth century has come to incorporate the now underdeveloped countries into a single stream of world history, which has given rise simultaneously to the present development of some countries and the present underdevelopment of others. However, in their attempt to construct theory and policy for the underdeveloped countries, Rostow and others have examined the developed countries as if they had developed in isolation from this stream of world history. It stands to reason that any serious attempt to construct

theory and policy for the development of the now underdeveloped countries has to be based on the examination of the experience of the underdeveloped countries themselves—that is, on the study of their history and of the world historical process that has made these countries underdeveloped. Yet this task of constructing a realistic theory and policy of development has not been pursued by any of the students of economic development and cultural change who employ the modes of approach to the problem that, according to Nash, exhaust all possibilities. We see again, then, that these three approaches to studying and solving the problems of economic development and cultural change only exhaust what is done; but they do not exhaust what can be done, and least of all what must be done.

It is impossible, without closing one's eyes, to find in the world today any country or society that has the characteristics of Rostow's first, the traditional, stage. This is not surprising since the construction of Rostow's stages takes account neither of the history of the now underdeveloped countries nor of their crucial relations with the now developed ones over several centuries past. Rostow's approach obliterates the fact that through these relations, the now developed countries have totally destroyed the preexisting fabric of these societies (be it "traditional" or not). This was most notably the case in India which was deindustrialized;⁶¹ Africa, where the slave trade transformed society long before colonialism did so again;⁶² and Latin America, where the high civilizations of the Incas and the Aztecs were wiped out altogether.⁶³ The relationship between the mercantilist and capitalist metropolis and these colonies succeeded in supplanting the preexisting—or, in the case of the *tabula rasa* situations of Argentina, Brazil, the West Indies, and elsewhere, in implanting the—social, political, and economic structure they now have: that is, the structure of underdevelopment.⁶⁴

This long relationship between the now underdeveloped and now developed countries within the same historical process did not affect only the export enclave in the underdeveloped countries, as the almost universally accepted and just as empirically and theoretically erroneous dual society or economy thesis has it.⁶⁵ On the contrary, this historical relationship transformed the entire social fabric of the peoples whose countries are now underdeveloped, just as in the developed countries.⁶⁶ (I shall return to this problem of the dual society or economy in the section on diffusionism later.)

If Rostow's first, traditional, stage cannot be found in any underdeveloped country today, his second stage, which contains the preconditions for takeoff into economic development, is even more conspicuous by its absence. Characteristic of Rostow's second stage is the penetration of underdeveloped countries by influences created abroad—mostly in the developed countries—and diffused to the underdeveloped ones, where

they destroy traditionalism and simultaneously create the preconditions that will lead to the subsequent takeoff in the third stage. (This, too, is examined in the section on diffusionism.) The factual error of the second stage in Rostow's thesis is so glaring that it may be discussed briefly. As we observed with respect to the first stage, the now underdeveloped Asian, African, and Latin American parts of the world, even if they were traditional in the Rostowian sense before their contact with Europe—a dubious thesis, considering the high civilizations and technological development that had been achieved on all three continents—certainly have been and still are affected by conditions in, and penetrated by influences emanating from, the now developed metropolis. Yet these same metropolitan conditions and influences, which already have a history ranging from one to several centuries, have not brought about economic development, or even led to a takeoff into development, in a single one of the “75 countries,” as they came to be called at the 1964 Geneva Conference on World Trade and Development.

This Conference was called because the nearly two-thirds of the world's population living in these countries feel and know that these metropolitan-imposed second-stage conditions, far from furthering their economic development as Rostow and other metropolitan pundits claim, not only hinder their economic development but even increase their underdevelopment.⁶⁷ The reason for all this is that the reality of underdevelopment, which Rostow's first and second stages obscure and even deny, is that the incorporation of these lands and peoples into the expanding mercantilist and then capitalist world system first initiated their underdevelopment; furthermore, their continued participation in this same system still maintains and even aggravates that underdevelopment.⁶⁸ As Prime Minister Jawaharlal Nehru said, in his *The Discovery of India*, “nearly all our major problems today have developed grown up during British rule and as a direct result of British policy : the princes; the minority problem; various vested interests, foreign and Indian; the lack of industry and the neglect of agriculture; the extreme backwardness in the social services; and, above all, the tragic poverty of the people.”⁶⁹

Rather than countering the authority of Rostow and most of his colleagues from the developed countries only with an appeal to the authority of Nehru and his colleagues from the underdeveloped countries we may also appeal to empirical evidence, which is devastating for the Rostowian thesis. The evidence is from the *tabula rasa* countries that had no population at all before they were incorporated into the developing mercantilist and capitalist system. Today, more than half of both the area and the population of Latin America—especially Argentina, Uruguay, Brazil, and all of the West Indies—occupies regions that, at the time of their incorporation into the European-centered mercantile system, were either entirely

unpopulated or were repopulated after the rapid extermination of the pre-contact population. None of these countries ever experienced Rostow's first stage: the mercantile metropolis did not conquer and settle these regions to institute Rostow's traditionalism but to exploit them through the establishment of exclusively commercial mines, sugar plantations, and cattle ranches. If anything, these regions and peoples entered world history by stepping right into Rostow's second stage. But after more than four centuries, Rostowian second-stage conditions and contact have not led to the third-stage takeoff in these regions, much less to the fourth or fifth stage of development. Today these previously unpopulated regions are just as underdeveloped as are the previously populated ones that were similarly incorporated into the world-embracing capitalist system. Indeed, contrary to Rostow's conception of the second stage—and, as we will see later, contrary to most of the diffusionist thesis—the more intimate the past contact of these regions with the metropolis, the more underdeveloped they are today. Among the many examples are the ex-sugar-exporting regions of the Caribbean and the Brazilian Northeast and the ex-mining export regions of Minas Gerais in the center of Brazil, of Bolivia, and Peru in the Andean Highlands, and of the famous Zacatecas and Guanajuato mining regions in the center of Mexico.⁷⁰

Abundant historical evidence from the underdeveloped countries shows that Rostow's first two stages are fictional. Contemporary evidence from them shows that his last two stages are utopian. After all, if these countries now were to find themselves in the fourth stage of drive toward maturity or in the fifth one of high mass consumption, we would not call them underdeveloped—and Rostow would not have to invent his stages. What is more, while in Rostow's rendition of reality his utopian last two stages are the mere mechanical summation of the fictitious first two stages plus the third, in the unfortunate reality of the underdeveloped countries it is precisely the structure of their underdevelopment—which Rostow whitewashes with his traditionalism and externally created pre-conditions—and their structural relations with the developed countries, which Rostow fails to mention at all, that have for so long prevented the realization of the last two stages. By Rostow's count, we are then left only with the third stage and by my count with the second crucial flaw in Rostow's entire argument.

Rostow would have us believe that in his third stage, the takeoff, he has theoretically synthesized the dynamic qualitative change between the structure of underdevelopment and that of development. However, his theory is not dynamic and he does not isolate structural characteristics or change. Least of all does he incorporate the real structure of underdevelopment and development into his theory. On the contrary, he fails to consider it altogether. Like most, but not all, stage theories of history,

Rostow's is an exercise in comparative statics. While he identifies stages of development, he does not say anything about how to get from one to the other. This is no less the case for the third stage than it is for the four others. The unreality of Rostow's dynamic should not surprise us: for as we have seen, even his statics are entirely unreal; his stages correspond to no reality in the underdeveloped countries at all. How, then, could his development from one stage to another correspond to the underdeveloped world's reality?

That Rostow does not argue from structure is already suggested by the fact that he places the major burden for development in the third stage, on the mere rate of investment and growth. The conclusive evidence of the theoretical inadequacy of Rostow's stages for understanding and eliminating the structure of underdevelopment goes far beyond that, of course. In completely ignoring the history of the underdeveloped countries, Rostow necessarily completely ignores the structure of their underdevelopment. The changes in institutions and investment he posits as the takeoff out of underdevelopment do not begin to affect the real structure of underdevelopment. The proof is that countries such as Argentina,⁷¹ which Rostow claims to be taking off into development, are becoming ever more structurally underdeveloped and that, indeed, no underdeveloped country has ever managed to take off out of its underdevelopment by following Rostow's stages.

Rostow's empirical and theoretical errors extend beyond his analysis of the underdevelopment of the underdeveloped countries to his characterization of the development of the developed ones. While the developed countries are not our topic here, it is necessary at least to point out this faulty characterization of development because, like Hoselitz and others, Rostow bases so much of his policy for the underdeveloped countries on his picture of the developed ones. Rostow is particularly explicit in claiming that England was the first country to industrialize and that it did so by domestically mobilizing its own resources after having experienced certain internal structural changes. Others among the now developed countries, he says, also developed on their own except insofar as the prior development of England and others helped to create the preconditions for their takeoff. Again, Rostow is wrong both on empirical and theoretical grounds. That England and other countries did not develop by relying only on their own efforts has been exhaustively proven. The English Mercantilists, such as Thomas Mun,⁷² had no doubt about it. Neither did Cantillon⁷³ or Marx.⁷⁴ Among our contemporaries, Earl Hamilton,⁷⁵ Eric Williams,⁷⁶ now prime minister of Trinidad and Tobago, and Basil Davidson,⁷⁷ have again demonstrated the crucial role played by the underdeveloped countries in financing the capitalization of the now developed ones. If the now underdeveloped countries were really to follow the stages

of growth of the now developed ones they would have to find still other peoples to exploit into underdevelopment, as the now developed countries did before them.

This misrepresentation of reality by Rostow must, of course, lead to (or does it follow from?) a theoretical error of the first magnitude and of vital importance for development theory and policy. This error is common not only to both variants of the first mode but also to all three modes of approach to economic development and cultural change reviewed here.⁷⁸ They each view the characteristics of development and underdevelopment as *sui generis* to the country concerned. When they proceed to the study of any structure at all, as we have already seen in the case of Hoselitz, they confine themselves to examining only parts of the domestic structure of the country concerned. In none of these modes is there an examination of the actual structure of development and underdevelopment—of the structure of the historical system that gave rise to and includes them both. As to the efficacy of the policy recommended by Rostow, it speaks for itself: no country, once underdeveloped, ever managed to develop by Rostow's stages. Is that why Rostow is now trying to help the people of Vietnam, the Congo, the Dominican Republic, and other underdeveloped countries to overcome the empirical, theoretical, and policy shortcomings of his manifestly noncommunist intellectual aid to economic development and cultural change by bombs, napalm, chemical and biological weapons, and military occupation?⁷⁹

The first or ideal-typical mode of approaching problems of economic development and cultural change turns out upon examination to be empirically invalid, theoretically inadequate, and ineffective policy-wise. The fundamental reason why the whole approach must be rejected by those who would meaningfully understand and solve the problems of economic development and cultural change is that the approach, in all its variations, ignores the historical and structural reality of the underdeveloped countries. This reality is the product of the very same historical process and systemic structure as is the development of the now developed countries: the world-embracing system within which the now underdeveloped countries have lived their history for centuries. It is the structure of this system that constitutes the historical cause and still contemporary determinant of underdevelopment. This structure is ubiquitous; it extends from the most developed part of the most developed country to the most underdeveloped part of the most underdeveloped country. Even if the first approach were to study the structure of underdevelopment on the domestic level of the underdeveloped countries, which as we have seen it does not, it would be unable adequately to analyze and understand that domestic structure—let alone to permit policy formulation that is adequate to change it. Those engaging in the first mode of analysis, and as we will see the second and

third ones as well, resolutely avoid the study of the international structure of development and underdevelopment of which the domestic structure of underdevelopment is only a part. On all grounds then, empirical, theoretical, and policy, the first approach to economic development and cultural change must be rejected as inadequate.

The Diffusionist Approach

The second mode identified by Nash views development as occurring through the diffusion of cultural elements from the developed to the underdeveloped countries. This involves, of course, acculturation to these elements on the part of the underdeveloped countries. The diffusion is seen to spread from the metropolis of the advanced capitalist countries out to the national capitals of the underdeveloped ones, and from these in turn out to their provincial capitals and finally to the peripheral hinterland.

According to this view since development consists of and is promoted by diffusion and acculturation, underdevelopment remains because of obstacles or resistance to this diffusion. Underdevelopment is taken to be the original “traditional” state as much as it is in the first mode. There is even less inquiry into the causes and nature of underdevelopment than in the first mode. In effect, the diffusionists do not suggest to the peoples of the underdeveloped world that they inquire into and remove the causes of underdevelopment; instead they advise them to await and welcome the diffusion of developmental aid from the outside.

Empirical Validity

Nash emphasizes the diffusion of “knowledge, skills, organization, values, technology, and capital” as the primary factors in the second mode’s view of economic development and cultural change. For expository convenience, we shall reclassify these as (1) capital; (2) technology, including knowledge and skills; and (3) institutions, including values and organization.

Capital

With respect to the diffusion of capital the thesis of the second mode begins with the proposition that, being poor, the underdeveloped countries lack investment capital and, therefore, find it difficult or impossible to develop and thereby escape from their poverty. Therefore, the richer developed countries can, should, and do diffuse capital to the underdeveloped ones, thereby promoting their economic development. The acceptability of the initial proposition—that it is poverty which hampers the underdeveloped countries’ efforts at investment and development—has

been strongly challenged on theoretical grounds by Paul Baran,⁸⁰ and this writer has supplied further theoretical and empirical evidence that discounts this proposition.⁸¹ I shall say no more about this proposition here since it is the assumption—or justification—that serves only as the starting point for the diffusionist thesis. Instead, I shall go on to examine the thesis itself, namely that the developed countries diffuse capital to the underdeveloped ones and thereby diffuse aid in their development. This thesis is upheld in the pages of *EDCC* by, among others, Martin Bronfenbrenner,⁸² and by Daniel Garnick,⁸³ who challenges Bronfenbrenner's argument. Whatever the disagreement between them, however, they both agree that the developed countries actually contribute capital to the underdeveloped ones. The variety of views on foreign aid and investment presented under Gerald Meier's editorship in *Leading Issues in Development Economics*, by Raymond Mikesell in *U.S. Private and Government Investment Abroad*,⁸⁴ or by Benjamin Higgins, in his chapter on "Foreign Investment and Foreign Aid" in his *Economic Development*,⁸⁵ exhibit a variety of sharp disagreements. But all these writers, as well as others in *EDCC*,⁸⁶ seem to be in full agreement with the proposition that the flow of capital is from the developed countries to the underdeveloped ones. Again, the only disagreement seems to stem from the facts.

The conservative estimates of the U.S. Department of Commerce show that between 1950 and 1965 the total flow of capital on investment account from the United States to the rest of the world was \$23.9 billion, while the corresponding capital inflow from profits was \$37.0 billion, for a net inflow into the United States of \$13.1 billion. Of this total, \$14.9 billion flowed from the United States to Europe and Canada while \$11.4 billion flowed in the opposite direction, for a net outflow from the United States of \$3.5 billion. Yet, between the United States and all other countries, that is mainly the poor, undeveloped ones, the situation is reversed: \$9.0 billion of investment flowed to these countries while \$25.6 billion profit capital flowed out of them, for a *net inflow from the poor to the rich* of \$16.6 billion.⁸⁷

Other available statistics show exactly the same pattern of net capital flow from the underdeveloped countries to the developed ones.⁸⁸ The only trouble with these data is that they very much understate the actual flow of capital from the poor underdeveloped countries to the rich developed ones. First of all, they understate the capital flow from poor to rich on investment account.⁸⁹ Secondly, they obscure the fact that the largest part of the capital which the developed countries own in the underdeveloped ones was never sent from the former to the latter at all but was, on the contrary, acquired by the developed countries in the now underdeveloped ones.

Thus, according to the U.S. Department of Commerce, of the total capital obtained and employed from all sources by U.S. operations in Brazil in 1957, 26 percent came from the United States and the remainder

was raised in Brazil, including 36 percent from Brazilian sources outside the American firms.⁹⁰ That same year, of the capital in American direct investment in Canada, 26 percent came from the United States while the remainder was also raised in Canada.⁹¹ By 1964, however, the part of American investment in Canada that entered from the United States had declined to 5 percent, making the average American contribution to the total capital used by American firms in Canada during the period 1957–64 only 15 percent. All the remainder of the “foreign investment” was raised in Canada through retained earnings (42 percent), depreciation charges (31 percent), and funds raised by American firms on the Canadian capital market (12 percent). According to a survey of American direct investment firms operating in Canada in the period 1950–59, 79 percent of the firms raised over 25 percent of the capital for their Canadian operations in Canada, 65 percent of the firms raised over 50 percent in Canada, and 47 percent of the American firms with investments in Canada raised all of the capital for their Canadian operations in Canada and none in the United States. There is reason to believe that this American reliance on foreign capital to finance American “foreign investment” is still greater in the poor underdeveloped countries, which are weaker and more defenseless than Canada. This, then, is the source of the flow of capital on investment account from the poor underdeveloped countries to the rich developed ones.

Thirdly, these data take account neither of the well-known decline in the underdeveloped countries’ relative participation in world trade, nor of the deterioration of the terms of trade that is currently costing the underdeveloped countries far more capital than their net or gross receipts of investment and loans from developed ones.⁹² (Net receipts, as was noted earlier, are negative to begin with.) Fourthly, these data on the flow of investment capital leave out of account the still larger flow of capital from the underdeveloped countries to the developed ones on other service accounts. In 1962 Latin America spent fully 61 percent of its foreign exchange earnings on services that were supposedly rendered to it by the developed countries. Half of this, or 30 percent of the total, was accounted for by officially registered profit remittances and debt service. The other half was composed of Latin American payments to the developed countries, which means mostly the United States, for transportation and insurance, travel, other services, donations, transfer of funds, and errors and omissions (in registered capital flows). Moreover, Latin America’s loss of capital on service accounts is increasing over time: while in 1961–63 it was 61 percent, in 1956–60 it had been only 53 percent.⁹³ This capital outflow amounts to 7.3 percent of Latin America’s Gross National Product (GNP), or 10 percent if we add the 3 percent of GNP lost by recent years’ deterioration in the terms of trade; and this equals two and three or more times the capital that “capital poor” Latin America devotes to net investment for

its own development.⁹⁴ Other kinds of capital loss by the underdeveloped countries are not included in these calculations, such as the notorious brain drain, or outflow of human capital that was financed by the poor countries for the subsequent benefit of the rich. Who, we may ask, is diffusing capital to whom?

Beyond the question of the amount and direction of capital diffused, there is the problem of the kind and consequences of foreign aid and investment in underdeveloped countries. That metropolitan investment in and control of primary sector production in underdeveloped countries (in, e.g., sugar, bananas, minerals, and most spectacularly petroleum) has notably failed to develop the underdeveloped countries, but has instead interposed a whole series of obstacles to their development, has by now surely been sufficiently documented to be obvious even when viewed from the developed countries themselves.

Foreign investment in the industrial and service sectors of underdeveloped countries raises further questions. It is far from clear that even this investment helps underdeveloped countries to develop. Nonetheless, with few exceptions, writers from the developed countries have failed to question, much less to analyze, the supposed benefits of this foreign investment to underdeveloped countries. Economists and statesmen from the underdeveloped countries, on the other hand, are increasingly challenging these supposed benefits and are going on to analyze the obstacles to industrialization and economic development created by foreign investment. For example, a congress representing thirty-four Schools of Economics in Latin America recently concluded that

[d]irect foreign investment has many unfavorable effects on the balance of payments, on economic integration and on capital formation in our countries; it determines in great measure the character and direction of our foreign trade, stimulates monopolistic competition, absorbs or subordinates weaker national firms, etc. For all these reasons it is necessary to adopt ways and means that can impede these negative effects.⁹⁵

Arturo Frondizi wrote as follows during his successful electoral campaign for the presidency of Argentina:

It is not amiss to remember that foreign capital usually acts as an agent which perturbs the morality, the politics, and the economy of Argentina. . . . Once established thanks to excessively liberal concessions, foreign capital obtained bank credits which permitted it to expand its operations and therefore its profits. These profits are immediately sent abroad as if all of the investment capital had been

imported by the country. In this way, the domestic economy came to strengthen foreign capitalization and to weaken itself. . . . The natural tendency of foreign capital in our country has been, in the first place, to settle in areas of high profits. . . . When Argentinian effort, intelligence, and perseverance created an independent economic opportunity, foreign capital destroyed it or tried to create difficulties for it. . . . Foreign capital had and has a decisive influence in the social and political life of our country. . . . The press is usually also an active instrument of this process of submission. . . . Foreign capital has had special influence in the political life of our nation, allying itself with the conservative oligarchy . . . those who are tied to foreign capital by economic ties (directors, bureaucratic personnel, lawyers, newspapers that receive advertisements, etc.) and those who, without having economic relations, end up being dominated by the political and ideological climate created by foreign capital,⁹⁶

Octaviano Campos Salas, before he became minister of industry of Mexico, summarized the consequences of foreign investment as follows:

(A) Private foreign capital takes over high profit sectors permanently, expelling or not permitting the entry of domestic capital, by relying on the ample financial resources of its home office and on the political power which it sometimes exercises. (B) The permanent takeover of important sectors of economic activity impedes domestic capital formation and creates problems of balance of payments instability. (C) Private direct foreign investment interferes with anti-cyclical monetary and fiscal policy—it comes when there are expansions and withdraws during depressions. (D) The demands by private foreign investors for concessions to form a “favorable climate” for investment in the receiving countries are unlimited and excessive. (E) It is much cheaper and more consistent with the underdeveloped countries’ aspirations to economic independence to hire foreign technicians and to pay royalties for the use of patents than to accept the permanent control of their economies by powerful foreign consortia. (F) Foreign private capital does not adapt itself to development planning.⁹⁷

It is not, then, indisputably obvious that the underdeveloped countries would be still more underdeveloped if they were not visited by foreign capital.⁹⁸ Evidently, not any and all diffusion, even of capital, let alone of other things, is an aid to economic development.

Technology

Technology is diffused only in part. However, the problem is not, as the diffusionists would have us believe, one of insufficient quantity of technology diffused, and still less one of cultural resistance to its acceptance and employment in technologically backward areas. The problem of technology and its diffusion arises out of the same monopoly structure of the economic system on the world, national, and local levels. During the course of the historical development of the capitalist system on these levels, the developed countries have always diffused out to their satellite colonial dependencies the technology whose employment in the colonial and now underdeveloped countries has served the interests of the metropolis; and the metropolis has always suppressed the technology in the now underdeveloped countries that conflicted with the interests of the metropolis and its own development, as the Europeans did with the irrigation and other agricultural technology and installations in India, the Middle East, and Latin America; or as the English did with industrial technology in India, Spain, and Portugal.⁹⁹ The same is true on the national and local levels in which the domestic metropolis promotes the technology in its hinterland that serves its export interests and suppresses the preexisting individual or communal agricultural and artisan technology that interferes with the use of the countryside's productive and buying capacity and capital for metropolitan development.

Throughout this historical process the metropolis has maintained a high degree of monopoly over industrial production and technology, which it has relinquished only when it had already established an alternative source of monopoly in heavy industry; it is slowly beginning to relinquish the latter in our day, now that it has developed a still newer source of technological monopoly in electronics, synthetics, cybernetics, and automation in general. Far from diffusing more and more important technology to the underdeveloped countries, the most significant technological trend of our day is the increasing degree to which new technology serves as the basis of the capitalist metropolis' monopoly control over its underdeveloped economic colonies.

Some of the facts of technological diffusion, which sharply contrast with most of the diffusionist faith, were recently analyzed by the American business magazine, *Newsweek*, under the title "The U.S. Business Stake in Europe":

To knowledgeable Europeans, in fact, the technical lead of the big U.S. companies is the most disturbing facet of the dollar invasion. In the future, a French study committee recently concluded, competition over prices will give way to competition in innovations, and the pace will be so hot that only firms of international size—that

is, American ones, chiefly—will survive. . . . European industries will function more and more under foreign licensing agreements; they will become subsidiaries of U.S. parent companies, which will sell them their know-how and manage Europe's production. . . . French politicians and publications of the right, left, and center have been accusing the U.S. of economic colonization, satellization, and vassalization. . . . A company chairman in Brussels sums up: "We are becoming pawns manipulated by U.S. giants." . . . An Olivetti executive discussing alternatives to the GE [General Electric] deal. . . . [declared] "But even if we had merged with Machines Bull in France and Siemens in Germany (which later signed a licensing agreement with RCA [*Newsweek*]), we still would have been dwarfed and eventually put out of business by the U.S. giants. . . . Research costs are too high. The transatlantic technological gap is a fact of life. . . . We studied a European solution very carefully. . . . There is no European solution to these problems."¹⁰⁰

Contrary, then, to what the diffusionist would have us believe, the hard fact of technological diffusion, as these members of the developed European business community are well aware, is not the essentially simple matter of diffusing technological development aid from the more developed to the less developed countries. Still less, of course, is the problem of technological diffusion and economic development one of cultural resistance derived from traditionalism or from Hoselitz's pattern variables. If these strong and developed European economies cannot find a European solution to the real developmental problem posed by the technological gap (rather than to the fancied one of the diffusionists), what hope do the weak and underdeveloped economies caught in the same system have to find such a solution?¹⁰¹ It is surely no accident that among European and previously underdeveloped countries, it is only in the socialist countries—the Soviet Union and China—that a "solution to these problems" has been found.

Institutions

The past, present, and future diffusion of institutions and values from developed to underdeveloped areas is a fact beyond question. The construction of an entire theory of economic development on this foundation is another matter. In addition to Manning Nash, who is probably best classified in this category—although he rejects diffusionism in its crudest "pitchforking" form as he calls it—theorists concerned with the developed countries' diffusion of institutions and values, and the underdeveloped recipients' resistance to them, have been well represented in the pages of *EDCC*.¹⁰² Technically, diffusionist theory might deal with the diffusion of any kind of institutions or values. In practice, however,

the diffusionist school has concentrated its attention on the diffusion of old-fashioned or newfangled liberalism (though they rarely call it this)—which is, indeed, most of what has been diffused from the metropolitan to the now underdeveloped countries during the last century. Consequently, I shall concentrate attention on the diffusion of liberalism, in its economic, political, and social forms. Moreover, the pattern variables of universality, achievement orientation, and functional specificity with which Hoselitz identifies economic development are little more than liberalism recast into technical-sounding jargon. This is what Hoselitz apparently would like to see diffused to transform underdevelopment into development. Does diffusionism constitute an adequate development theory, and does the diffusion of liberalism or of anything else serve as an effective economic development policy?

Economic liberalism was and is diffused, not in general, but under very specific and particular circumstances. Its exportation from the metropolis is an expression of the particular interests of those who diffuse it, as its importation by the underdeveloped countries is an expression of the particular interests of those who are acculturating to it. The specific circumstances of and particular interests in the diffusion and acculturation of liberalism, like anything else, were and still are determined by the structure and development of the economic, social, and political system within which it occurs. The German economist Friedrich List reported in the 1840s that an American Supreme Court justice had observed, in regard to one of liberalism's most important tenets, that like most of Great Britain's other products, the free-trade doctrine was produced primarily for export.¹⁰³ A few years later, U.S. President General Ulysses S. Grant observed that

for centuries England has relied on protection, has carried it to extremes, and has obtained satisfactory results from it. There is no doubt that it is to this system that it owes its present strength. After two centuries, England has found it convenient to adopt free trade because it thinks that protection can no longer offer it anything. Very well, then, Gentlemen, my knowledge of my country leads me to believe that within 200 years, when America has gotten all it can out of protection, it too will adopt free trade.¹⁰⁴

President Grant only erred by a century: since the Second World War, that is, since it achieved the unrivaled industrial supremacy and near monopoly in the world that Britain had attained a century before, the United States both directly and through its controlling influence in international agencies, such as GATT, the International Monetary Fund, and the World Bank, has been most adamant in exporting free trade. Free

trade, like free enterprise, is protective monopoly under another name—as Frederick Clairmonte has so well shown.¹⁰⁵

The circumstances and interests leading to the underdeveloped countries' ready acculturation to international free trade and domestic economic liberalism in the nineteenth century—and to free trade in technology and free enterprise in the twentieth century—can be summarized just as clearly:

“The doctrine of liberalism, imported from Europe, thus found fertile ground in our country [Chile] and grew vigorously. It constituted the theoretical basis to re-enforce the interest of the controlling forces, inasmuch as it represented and expressed its desires.”¹⁰⁶

Another more specific and thorough observation is worth quoting at length:

The pressure groups who controlled the economic policy of the country were decidedly freetraders : they were more freetrader than Courcelle-Seneuil, the famous and respected leader of doctrinaire freetrading: they were definitely more Catholic than the Pope.... The mining exporters of the North of the country were free-traders. This policy was not fundamentally due to reasons of doctrine—though they also had these—but rather to the simple reason that these gentlemen were blessed with common sense. They exported copper, silver, nitrates and other minerals... they were paid in pound sterling or dollars.... It is hard to conceive of an altruism or a farsighted or prophetic vision which would lead these exporters to pay export and import duties with a view to the possible industrialization of the country.

Véliz goes on to describe how the agricultural and livestock exporters and the big import houses operated in terms of the same logic. He adds:

Here then is the powerful coalition of strong interests which dominated the economic policy of Chile during the past century and part of the present one. None of these three had the very least interest in Chile industrializing. They monopolized the three powers at all levels; economic power, political power, and social prestige.¹⁰⁷

Aldo Ferter finds the same pattern in nineteenth-century Argentina:

The merchants and livestock owners, who were the dynamic forces in the development of the littoral, were chiefly interested in the expansion of exports. Free trade thus became the philosophy and practical policy of these groups.... Free exports also meant freedom to import.¹⁰⁸

Ferrer returns to discuss the Argentina of our day after its supposed takeoff into industrialization during the 1930s and 1940s, and after the expulsion of Peron and the abrogation of his policy in the 1950s by these same groups and their foreign, now primarily American, allies who instituted the policy of the International Monetary Fund instead:

In January of 1959, Argentina began the application of a stabilization plan. . . . At the same time the exchange rate structure was liberalized, and the peso was devalued. . . . Devaluation has become, moreover, a tool of economic policy explicitly designed to change the domestic price structure in favor of the export sector. . . . The difficulties of this kind of readjustment, in view of the objective conditions obtaining in the Argentinian economy as well as in the world market, are reflected in the fact that stagnation has not been overcome and that the rigidities of the economic system which determine it, far from being on the way to solution, have become even more serious. . . . The financial and monetary policy . . . has been accompanied by a strongly regressive redistribution of income. . . . There has been a strong business contraction. . . . The deficit in the balance of payments and the government budget and the rise in price have not been resolved. . . . In fact, the stabilization plan and the recommendations received from abroad have simply served as a tool in the hands of the sectors who saw their immediate and long term interests served by the impact of the policy followed on the distribution of income and the *backward* structural adjustment of the Argentinian economy.¹⁰⁹

Two additional well-known examples are instructive as to how economic liberalism in the domestic economies of the underdeveloped countries promotes monopoly and thereby the underdevelopment of the majority. One example is the nineteenth-century breakup, in the name of liberalism, of communally held Indian land, its distribution into private ownership and consequent monopoly concentration during the epoch of liberal reform—a concentration which far exceeded that of the autocratic colonial times.¹¹⁰ Another example is the currently ever greater monopoly concentration of finance, commerce, industry, and (still) of land in underdeveloped countries under the aegis of the “free” world’s “free” enterprise.¹¹¹ It is clear then that the diffusion and acculturation of economic liberalism between the developed (or developing) metropolitan countries and their underdeveloped satellites—as well as that within the underdeveloped countries—is a response to interests, and produces consequences that can be summed up in a single word: monopoly. Contrary to the elaborate classical and neoclassical theoretical economic edifice that was carefully built up in Manchester (the first city to enter the modern industrial age!)

and that is still being assiduously exported and imported by interested parties, the diffusion of economic liberalism has quite consistently contributed its significant share to the establishment, maintenance, and strengthening of economic monopoly, both on the national and international levels. Through this monopoly, economic liberalism has contributed to the economic development of *those who diffuse it*; to, as the United Nations Economic Commission for Latin America calls it, the limited “outward-oriented development”¹¹² of the capitals of the underdeveloped countries; and to ever more underdevelopment for the world’s majority who were and are liberally forced to suffer its consequences. The diffusion of political liberalism that accompanied and followed the spread of economic liberalism cannot be said to be very different. Since the consequences of the diffusion of political liberalism are clear in the aforementioned analysis of economic liberalism, and since they are explicit in our daily newspapers, it is unnecessary to rely on Lenin’s analysis of the relations between economic and political power and institutions in his *The State and Revolution*, or to go into it here.¹¹³ The only remark that needs to be made is that the relations between economic and political power—again discussed by President Eisenhower in terms of the “military-industrial complex”¹¹⁴ and by C. Wright Mills¹¹⁵—are even more intimate in the underdeveloped countries than they are in the developed ones, which are discussed by Lenin, Eisenhower, and Mills.

Although it does not go by that name, we may also observe the diffusion of and acculturation to “social liberalism.” This modern liberalism takes the form primarily of promoting “social mobility” and “middle classes” in the underdeveloped countries. Like the others, social liberalism is advertised as leading to a more open, democratic society capable of greater and faster economic development. We have observed earlier that Hoselitz’s pattern variable approach lends support to this thesis, and that Johnson and Germani, among many others, propose the promotion of middle classes and of social mobility as development theory and policy. Johnson diffuses it from the United States,¹¹⁶ and Germani acculturates to it in Argentina when he writes under the title of “A Strategy for Promoting Social Mobility.”¹¹⁷ Like economic and political liberalism, social liberalism is, however, more aptly described as individual liberalism. It is the liberty of a few individuals to move, to monopolize, and thereby restrict the development of the economic, political, and social whole. Those persons in underdeveloped countries who have migrated from country to city or moved from a lower economic and social status to a higher one often say in one way or another that they have made their own individual reform or revolution. In so doing they express not only the conservatism that reflects their desire to maintain their newly gained position but also a fundamental social scientific truth, which seems to escape the attention of diffusionists

and others: "social" mobility is really *individual* mobility and does not transform social structures: rather, a change in the social structure may render possible *social* mobility and economic development.

As with the other liberalisms, the evidence is accumulating (supplied in part by Hoselitz himself, as we saw earlier)¹¹⁸ that the diffusion to the underdeveloped countries of the institutions and values of social liberalism is highly selective at both the diffusing and acculturating ends. The selective diffusion is determined by the structure of the international system, including the structural relations of the sending and receiving societies and subsocieties within it. Far from aiding the development of the underdeveloped countries, social liberalism hinders it. As we noted earlier, social mobility and the rise of the middle classes in the underdeveloped countries renders the distribution of income not more but less equal;¹¹⁹ and it provides economic and political support not for changing but for maintaining and reinforcing the structure of the economic, political, and social status quo.¹²⁰

Theoretical Adequacy

As with our examination of the first mode, our review of the empirical validity of propositions in the second mode offers a good vantage point from which to evaluate their associated theoretical formulations. Like the first mode, the diffusionist approach suffers from serious theoretical shortcomings because of its failure to take adequate account of the determinant structure and development of the social system within which diffusion, acculturation, and economic development and cultural change take place. Perhaps the most important theoretical fault of diffusionism is that it is premised on dualism instead of on structural and developmental holism. In the pages of *EDCC*, the theory of dualism itself has been most explicitly advanced and defended by Benjamin Higgins,¹²¹ who rejects the social dualism of Boeke¹²² only to argue that dualism has a technological and economic basis. Reflecting its widespread acceptance, dualism is explicitly expressed in *EDCC* by writers and reviewers who span the globe.¹²³

Although explicit reliance on the dual society or economy thesis is usually reserved for the analysis of underdeveloped countries alone, the dualist thesis is implicit in the entire analysis of development reviewed here.

All three modes of analysis seek to analyze both the differences between developed and underdeveloped countries as well as the inequalities within the latter by attributing separate and largely independent economic and social structures to the developed and underdeveloped sectors, each with its own separate history and dynamic, if any. (Frequently, as we have seen, the one part is denied any history at all.) Jaques Lambert, for example,

argues in his *Os Dois Brasís* [The Two Brazils] that

the Brazilians are divided into two systems of economic and social organization.... These two societies did not evolve at the same rate... they are separated by centuries.... The dual economy and the dual social structure which accompanies it are neither new nor characteristically Brazilian—they exist in all unequally developed countries.¹²⁴

In this sense, the plantation or mining sector of an underdeveloped country is viewed as an enclave of the developed metropolitan economy on foreign soil. The “enclave” is presumed not to be a real part of the supposedly isolated subsistence economy of the underdeveloped country itself; and it is thought to exercise little if any economic and social influence on this isolated sector in the present, and none in the past.¹²⁵ Similarly, in a supposedly somewhat less underdeveloped country, part of the population, usually the indigenous inhabitants, are said to be outside the market economy and marginal to the national society and to the world as a whole.¹²⁶ This conception of a dual economy and society, whether the duality be attributed to cultural, social, technological, economic, or other causes, then gives rise to the diffusionist theory and policy regarding the diffusion of capital, technology, and institutions.

The dualist theory and the diffusionist and other theses based on it are inadequate because the supposed structural duality is contrary to both historical and contemporary reality:¹²⁷ the *entire* social fabric of the underdeveloped countries has long since been penetrated and transformed by, and integrated into, the world-embracing system of which it is an integral part. The facts of this penetration have been presented and the thesis of the consequent transformation and integration has been persuasively argued for Meso-America by Eric Wolf;¹²⁸ for India by Marx,¹²⁹ Dutt,¹³⁰ and Desai;¹³¹ for China by Owen Lattimore;¹³² for Africa by Woddis,¹³³ Suret-Canale,¹³⁴ and Mamadou Dia;¹³⁵ and even for Indonesia, the birth-place of dualism, by Wertheim and Geertz,¹³⁶ the latter formerly a research associate of Higgins and now a colleague of Hoselitz.

More specifically, as Eric Wolf¹³⁷ has taken great pains to point out for Meso-America and this writer for Brazil,¹³⁸ it is not true, as diffusionists and others implicitly or explicitly maintain, that the isolation of indigenous peoples, peasants, and others, declines over time until they are completely integrated in the national society, which then is no longer dual. On the contrary, the degree of integration and other aspects of the relationship that these peoples have with others at home and abroad varies in ways that are determined primarily by the structure and development of the national and international capitalist system, and secondarily by these

peoples' own very partially successful efforts to defend themselves against the exploitative consequences of this system.

Dualism is not only theoretically inadequate because it misrepresents and fails to analyze the capitalist system on the international, national, and local levels, but also because it fails to adhere to the standards of holism, structuralism, and historicity. Dualists contravene holism in explicitly setting up two or more theoretical wholes to confront a single social whole, which they cannot or will not see. As for structuralism, dualists fall far short because if they see and deal with any structure at all it is at best the structures of the parts. They do not deal with, and even deny the existence of, the structure of the whole system through which the parts are related—that is, the structure that determines the duality of wealth and poverty, of one culture and another, and so on. As to the historical development of the social phenomena they study, dualists and diffusionists either deny any history to one part altogether or, observe its ongoing social change without the historical perspective necessary to interpret it adequately; and they steadfastly abstain, of course, from giving any consideration whatsoever to the historical development of the social system of which diffusing donor and acculturating recipient are but parts. Little wonder then that diffusionists and other dualists who only look at appearances misunderstand their significance and misjudge their consequences for economic development and cultural change.

As Marx said, science would be pointless if the outward appearance of things were to correspond to their inner significance. Thus the task of social scientific theory, which dualist and other advocates of the three modes reviewed here fail to pursue, is not to see how different the parts are, but, on the contrary, to study what relates the parts to each other in order to be able to explain why they are different or dual. If the policy of economic development and cultural change is really meant to eliminate these differences—or the undesirable ones among them—then its task must be to change the relationships that produce these differences: that is, it must change the structure of the entire social system that gives rise to the relations and, therefore, to the differences of the dual society.

The unfortunate, though not inexplicable, fact is that the theory and policy reviewed here is moving away from this task. With their supposedly structural and historical ideal typical approach, the disciples of Weber are leaving their teacher's scientific scope and method behind and dedicating themselves to no more than its cruel caricature. Similarly, dualists and diffusionists—acculturationists are corrupting the vision and work of one of their principal teachers of recent times, Robert Redfield. In creating the ideal type of the folk community, and in analyzing diffusion along the folk-urban continuum,¹³⁹ as well as in his later works on the relations between high and low culture,¹⁴⁰ Redfield, no doubt unintentionally,

encouraged contemporary students of economic development and cultural change to adopt a dualism and diffusionism that he himself rejected in his later years.

Redfield taught that in situations of culture contact diffusion is never a one-way affair. In this respect, then, the diffusionist emphasis on diffusion from the metropolis to the periphery, and the virtual exclusion of the reverse, is a departure from Redfield as well as being unacceptable on other theoretical grounds. Moreover, although Redfield was far from being a structuralist (although he spared no pains to emphasize the need for holism in social scientific theory), he did call our attention to the structural determination of mutual diffusion between, for instance, high and low culture within a single social system. Nonetheless, Redfield's lessons seem not to have come to the attention of that majority of diffusionists who employ his terminology while distorting his ideas.

Finally, it was Redfield more than anyone else in recent times who insisted that there are no peasants without the city to which they are tied and which defines them as peasants, and that there can be no city without its peasants or their equivalent.¹⁴¹ It is clear, then, that at least the later Redfield himself recognized and emphasized the *holistic interdependence and unity* of the dual ideal typical poles and social sectors he made so popular. It may be lamentable that Redfield did not extend this holism to the larger social system and to historical evolution, although his concern with the relations between high and low culture in his last years may have been a step in that direction. It is certainly more than lamentable, however, that so many of his diffusionist and dualist followers have abandoned their mentor's empirical realism and scientific holism and have substituted the most simplistic and crassly nonholistic diffusionism.

Policy Effectiveness

As a policy of economic development and cultural change diffusionism has been largely ineffective. The centuries-long contact and diffusion between the metropolitan countries and the now underdeveloped ones has failed to result in the economic development of the latter. Nor has any diffusion from the capitals to the provinces of the underdeveloped countries brought about the development of these hinterlands. New technology may have increased diffusion beyond that of certain times in the past but surely not beyond the diffusion of initial contact times which, far from initiating the development, initiated the underdevelopment of the now underdeveloped countries. More diffusion, per se, does not generate more development. Moreover, the diffusion that follows in the train of new roads, buses, transistor radios, and so on, is not increasing the economic

development of the recipient regions. Often it has helped to sink them into even deeper and more hopeless underdevelopment.

Conceived in its present form, diffusionism is inherently ineffective as a policy of economic development and cultural change. For it is not so much diffusion that produces a change in the social structure as it is the transformation of the social structure that permits effective diffusion. Development, underdevelopment, and diffusion are all a function of the social structure. In order for the underdeveloped parts of the world to develop, the structure of the world social system must change—on the international-, national-, and local levels. This structural change, however, cannot be brought about by diffusion. On the contrary, the structure of the system itself on all these levels determines the amount, nature, direction, and consequences of the past and present diffusion—a diffusion that has so far produced development only for the few and underdevelopment for the many, and by all indications will continue doing so. Consequently, the structure of this system has to change in order to permit development for all and to permit diffusion to contribute to that development.

The Psychological Approach

Nash introduces the third approach as the one “most profitably pursued,” and that leads to “smaller scale hypotheses, to a prospective rather than a retrospective view of social change.” Furthermore, Nash writes:

These papers I commend to your attention as examples of the dialectic of social knowledge, the confrontation of bold assertion against fact, and the incorporation of more general fact in ever bolder, more elegant assertion.¹⁴²

Nevertheless, a year later, comparing the psychological (and to some extent the first) mode of approach with his own second mode as published in *EDCC*, Nash seems to have had second thoughts:

The ‘specific factor’ analysis of social requisites (like lack of entrepreneurship, low achievement motivation, particularism, capital shortage, etc.) is not likely to provide anything systematically relevant to an understanding of growth. . . .¹⁴³

When Nash says that this mode of analysis leads to smaller-scale hypotheses, he is quite right, as we will see later. However, it should be noted here that the first two modes were seen to be inadequate precisely because

the scale of their theory and hypotheses is already too small to treat adequately the dimension and structure of the social system that gives rise to both development and underdevelopment.

As any historian of social thought will recall, Marx turned Hegel on his head and substituted historical materialism for idealism. Further, he worked with relatively large-scale theory and hypotheses, which he derived from his examination of the capitalist system as a whole as he saw it. Being a true holist, Marx was led—inevitably as Parsons pointed out earlier—to the observation that exploitation is a necessary basis of this system and to the conclusion that such a basis generates the polarization of the system. Since this conclusion was not palatable to Social Democrats such as Weber and Durkheim, whose disciple Parsons became, they set out to construct an alternative theory of the social system by starting with its parts rather than with the whole—a procedure that, as Parsons says, inevitably deemphasizes exploitation and makes the system appear to be not polarizing or disintegrative but integrative instead. Nonetheless, although Weber and Durkheim intentionally and explicitly abandoned the approach, conclusions, and policy of Marx, they still retained strong emphasis on the determinative importance of social structure and, in the case of Weber especially, of history as well. Even Hoselitz, being directly as well as via Parsons a disciple of Weber, and an advocate of the first mode of analysis, retains considerable interest in the role of social structure (he even puts it in his title) despite the attraction that the third mode approach of David McClelland, although apparently not of Everett Hagen, holds for him.¹⁴⁴

The pioneering service, as Nash's coeditor Robert Chin calls it, of these latter students of economic development and cultural change is precisely that they drop all pretense and practice of social scientific structuralism. They "Freudianize" Weber to such an extent that they no longer follow him at all. In fact, they specifically deny the importance of social structure and reject structural analysis. Although Hagen puts the word "social" into his title, he is quite frank in his preface in explaining that his theory is not social at all but rather psychological—or really psychiatric.¹⁴⁵ McClelland, reviewing Hagen's book in *EDCC*, agrees: he calls it "A Psychological Approach to Economic Development," albeit one that he finds to be not up to his own standards.¹⁴⁶ Not to be outdone, McClelland is quite explicit in telling his readers that not the social structure as Weber had it, nor even assignment of and reward in social roles based on achievement (as in Hoselitz's view), but only a high degree of individual motivation or need for achievement is the alpha and omega of economic development and cultural change:

In its most general terms, the hypothesis states that a society with a generally high level of *n* Achievement will produce more energetic entrepreneurs who, in turn, produce more rapid economic development . . . it

must satisfy us to have learned that high *n* Achievement leads people to behave in most of the ways they should behave if they are to fulfill the entrepreneurial role successfully as it has been defined by economists, historians, sociologists. . . . The whole view of history shifts once the importance of the achievement motive is recognized. For a century we have been dominated by Social Darwinism, by the implicit or explicit notion that man is a creature of his environment, whether natural or social. Marx thought so in advocating economic determinism, in arguing that man's psychology is shaped in the last analysis by the conditions under which he must work. Even Freud thought so in teaching that civilization was a reaction to man's primitive urges and to the repressive force of social institutions beginning with the family. Practically all social scientists have in the past generations begun with society and tried to create man in its image. Even Toynbee's theory of history is essentially one of environmental challenges, though he recognizes that states of mind can create internal challenges.¹⁴⁷

In his contribution to the volume edited by Nash and Chin, McClelland goes on to be even more explicit.

What is needed is a glacial shift in Western and particularly American social thinking. Ever since Darwin, social scientists have almost unconsciously started with the premise that the environment is primary and that the human organism somehow learns to adapt to it. . . . Consequently if one wants to change anything really fundamentally, he must start by modifying material arrangements in the environment which in turn will gradually reshape institutions and eventually ideas. Yet the evidence, as in the present instance, is very strong that it is just as often and perhaps more often initiated the other way around. This is just one more piece of evidence to support the growing conviction among social scientists that it is values, motives, or psychological forces that determine ultimately the rate of economic and social development— . . . *The Achieving Society* suggests that ideas are in fact more important in shaping history than purely materialistic arrangements.¹⁴⁸

We have returned full circle to Hegel. Except that McClelland's prescriptions for progress are not quite Hegel's. In his book's final chapter entitled "Accelerating Economic Growth," McClelland summarizes his prescriptions in his subtitles: "Increasing Other-Directedness and Market Morality"; "Increasing *n* Achievement"; "Decreasing Father Dominance"; "Protestant Conversion"; "Catholic and Communist Reform Movements"; "Effects of Education on *n* Achievement"; "Reorganizing Fantasy Life";

“Utilizing Existing *n* Achievement Resources More Effectively”; and he offers a final recommendation:

So we end on a practical note: a plan for accelerating economic growth through mobilizing more effectively the high *n* Achievement resources of a developed country to select and work directly with the scarcer high *n* Achievement resources in underdeveloped countries particularly in small and medium scale businesses located in provincial areas. . . .¹⁴⁹

This new pioneering service was undoubtedly inspired by Weber’s emphasis on values in *The Protestant Ethic and the Spirit of Capitalism*¹⁵⁰ and reinforced by Schumpeter’s emphasis on entrepreneurship in *The Theory of Economic Development*.¹⁵¹ The post-Second World War revival of academic interest in economic development was soon followed by a return to the letter if not to the spirit of Weber and Schumpeter. Books and articles on the role of religion and values in economic development appeared in great numbers, not a few of them in *EDCC*, as cited earlier.¹⁵² Simultaneously, Harvard University set up a Research Center in Entrepreneurial History and a journal, *Explorations in Entrepreneurial History*. Papers on entrepreneurship as a crucial factor in economic development and cultural change were published in *EDCC* and elsewhere.¹⁵³ The increasing evidence against the supposed role of the Schumpeterian entrepreneur in economic development, not only in underdeveloped countries but even in the nineteenth-century United States,¹⁵⁴ has not prevented the psychological idealizers of economic development from going on to advance theories such as those of Hagen and McClelland. Nor has it prevented *EDCC* from following in their footsteps to publish an entire series of studies reinterpreting the world to show the supposed importance of the achievement motive.¹⁵⁵ Furthermore, *EDCC*’s reviewer of *The Achieving Society*, S. N. Eisenstadt, concludes:

the fact that in discussing this book, are confronting it with Weber’s work, is the measure of the importance of the problems raised by McClelland’s endeavor. . . . McClelland has given a very stimulating and important work which anybody interested either in the broader problem of the impact of motivational orientation on society or in the more specific problem of economic development cannot ignore.”¹⁵⁶

To his and *EDCC*’s credit, John H. Kunkel has recently evaluated this “pioneering service”:

As long as man’s activities are considered to be a function of values or personality, little attention need be directed to the immediate

surrounding social environment, since it is not so much the present social structure as that of the past which is most involved in the formation of values and personality. The delineation of societal prerequisites of economic development, according to this view, can accomplish no more than prepare the ground for industrialization years, if not decades, in the future. However, as soon as behavior is considered to be a function largely of the surrounding social structure, both past *and* present, which affects behavior through the continuously operating determination of reinforcing and discriminative stimuli, the present social system takes on great importance. The behavioral prerequisites of economic development can be created only through alterations of the social structure, or certain elements of it, viewed broadly and including the economic system of a society. . . . There is no foundation, on theoretical grounds, for the pessimistic outlook concerning the capacity of the underdeveloped countries to industrialize in a short period of time. Pessimistic conclusions regarding the time necessary for the preparation of the right psychological conditions for economic development are based, essentially, on an incorrect conception of man and on the disregard of principles of behavior formation and maintenance derived from experimental psychology.¹⁵⁷

Nevertheless, in his contribution to the collection of papers edited by Nash and Chin that exemplifies this third mode approach, Kunkel's criticism is based largely on psychological principles and is limited essentially to methodological criticism of the third mode's empirical assertions.¹⁵⁸ So is Eisenstadt's criticism in his review of McClelland's book.¹⁵⁹ Furthermore, Kunkel's proposed alternative in his contribution to *EDCC* is limited to suggesting that behavioristic methodology can overcome the methodological shortcomings of the approach exemplified by Hagen and McClelland.¹⁶⁰ In this connection, Kunkel rightly observes:

Hagen makes much use of personality as an "internal state" of individuals. The characteristics of the "internal state" are derived from psychoanalytic theory, and then used to support the theory and the hypothesized relations among observed facts and inferred characteristics. When psychoanalytic concepts and theories are used in the study of economic development, problems of validating the concepts make any casual generalization difficult to test and accept on bases other than faith. The casual analysis is inadequate. Hagen infers causes from effects, but no evidence is presented to validate the inference made. . . . McClelland postulates a variety of needs as components of a person's "internal state," but this method of analysis involves

inferences from behavior (e.g., the writing of stories based on TAT pictures) which are difficult to validate, in order to explain the data collected by McClelland and his associates.¹⁶¹

Both Kunkel and Eisenstadt find that the work of these students of economic development and cultural change is deficient in that it fails to establish a methodologically adequate efficient cause between the supposedly causative psychological states and the supposedly derivative economic development. Kunkel's purpose in his contribution to *EDCC* is to provide such an efficient causative relation, which is not dependent on untestable inferences about internal states of mind.¹⁶²

Whatever the methodological merits or demerits of Kunkel's resort to behaviorism, it is as limited to generating small-scale hypotheses, as Nash calls them, and to recommending small-scale changes as is the methodology it seeks to substitute. Kunkel himself concludes:

If it is true that striving behavior, like any other, is shaped through differential reinforcement [such as reward and punishment by parents, as Kunkel tells us elsewhere], there is no reason why an internal state... should have to be postulated as an essential element in the analysis of economic development... Various selected elements of the societal environment are amenable to change today, thereby making possible the shaping of behavior patterns necessary for economic development... Since usually only a few aspects of the societal environment can be altered, present efforts to create behavioral prerequisites must begin on a small scale.¹⁶³

This suggests that, to evaluate the theoretical adequacy of the third mode approach, we must bring still other criteria to bear, such as the historicity and holistic structuralism by which we already examined the first two approaches.

As editor of a collection of works that exemplifies the third mode Manning Nash holds that of the three modes he is able to visualize, this third one is "most profitably pursued." One of its profitable aspects is that it leads "to a prospective rather than a retrospective view of social change." That is, as we may infer, Nash thinks that the social scientists working in terms of the third mode are performing a pioneering service not only because they abandon Weber's structuralism, leaving Bert Hoselitz behind as well—and he after all not only retains some structuralism but also is world renowned as an economic historian—but also because in not looking back, these pioneers leave behind them Weber's retrospective and historical approach and analysis.

However, Nash does not confine himself simply to lauding this effort and to recommending that students of economic development and cultural change forget about the past history of the underdeveloped countries concerned. Instead, he goes on to deny that the underdeveloped countries have any history. The third approach, he says, poses three main theoretical problems: "1. To systematically take account of the varieties of *traditional* societies, 2. To seek out the sources of resistance . . . among the various species of *traditionality*, 3. [To study why a society may or may not come] to rest somewhere between its *initial base* and modernity."¹⁶⁴

In other words, underdeveloped societies have no history; they have traditionally been the way they are now, which is underdeveloped. This is indeed a "bold assertion"; but once it faces "confrontation with fact" this claim is clearly revealed to be a falsification. How could Nash make such an assertion after having done the fieldwork for his doctoral dissertation in a community descended from a people who are world renowned for their history, the last seventy years of which he studied, and after having entitled his book *Machine Age Maya*?¹⁶⁵ How is it a pioneering service for the practitioners and champions of the third mode to take less and less account of the history of the underdeveloped countries they presume to study (especially after having delved into it here and there themselves), and finally to end up denying that the underdeveloped countries and underdevelopment even have history? For whom is this a pioneering service?

The answers emerge if we apply the criterion of structural holism to the question of the theoretical adequacy of the third mode approach and if we inquire into the effectiveness of the policy of economic development and cultural change to which this approach gives rise.

Kunkel correctly notes in regard to both the theory and the policy of the third mode that "little attention need be directed to the immediate surrounding social environment since it is not the present social structure which matters." But the critic of this approach is hardly as explicit and clear as its exponent, McClelland himself: "Ideas are in fact more important in shaping history than purely materialistic arrangements . . . of his [man's] environment, whether natural or social." The third mode of approaching economic development and cultural change, then, represents perhaps the ultimate step in pioneering progress away from classical scientific structural holism. The present economic, social, and political structure does not matter at all: There is no need to change the contemporary status quo.

What, then, according to these purveyors of dialectic social knowledge (as Nash terms their service), is to be done; and how effectively and for whom does their policy of promoting economic development and cultural change work? McClelland tells us what is to be done: "Increasing *n*

Achievement" ... "Protestant Conversion" ... "Education" ... "Reorganizing Fantasy Life." As McClelland himself recognizes, not only Marx, but even such progressive students as Spencer, the father of Social Darwinism, Toynbee, the father of neo-Thomism, and Freud, the father of individual psychiatry, and all of their intellectual children, never were progressive enough to believe and maintain that so deeply ingrained a social and economic condition of society could be changed simply by having more of its individuals taught to get a hold of themselves and raise their need for achievement, as McClelland would have it; or by not letting themselves be beaten down by adversity, as Hagen would have it; or even by having teachers and parents tell children more hero stories so that when the latter grow up they might be heroic developers themselves. This degree of progress and progressiveness had to await the coming of David McClelland and his disciples.

McClelland gives credit to one source of co-revelation of his vision of economic development and cultural change: the Communists, particularly the Chinese ones.¹⁶⁶ They receive no credit for following the teachings of Marx or other social scientists, the validity of whose theory McClelland denies; no credit for changing any economic, social, or political structure, the need for which change McClelland denies; nor any credit for making a revolution, which McClelland does not deem worthy of mention. Instead, they receive credit for realizing and putting into practice the truth that ideas and *n* Achievement promote economic development: the Chinese are achieving faster economic development than the Indians, McClelland points out.¹⁶⁷ On the basis of what economic, social, and political structure, he does not say: the Chinese have more *n* Achievement and *n* Power.¹⁶⁸ According to McClelland, it does not matter how that structure determines the distribution of power and the direction of achievement. Despite this generous bow to the Chinese Communists, we need no great insightfulness to discern the allegiance and effectiveness of an economic development policy that—following the example of such highly motivated members of the Cambridge, Massachusetts, academic community as W. W. Rostow,¹⁶⁹ McGeorge Bundy, Arthur Schlesinger, Jr., and perhaps David McClelland himself—promotes *n* Achievement and reorganization of fantasy life within the existing economic, social, and political structure, at home or abroad.

In complimenting the Communists, McClelland fails to give due credit where it really belongs. It is Frank Buchman and his worldwide movement for Moral Rearmament (MRA) who preached precisely the policy of economic development and cultural change now clothed in academic gown by David McClelland. His policy advice to developers is to take their eyes off and leave as is the economic, social, and political structure of the status quo; prepare instead each man for himself to rearm morally and spiritually to face the difficult road of economic development, cultural change, and

social progress that lies ahead. The political character and effectiveness of this development policy is amply demonstrated by its practitioners who include such renowned practical dialecticians, progressive servants, and self-declared MRA supporters as ex-Chancellor Adenauer of Germany, ex-Premier Kishi of Japan, ex-Prime Minister Tshombe of Katanga and the Congo, and the second President of Brazil after the 1964 military coup General Costa e Silva.

Conclusion

Having examined the three modes of approach to and analysis of the problems of economic development and cultural change separately, we can briefly evaluate them conjointly. What first forces itself into view is the wide and deep similarity in the extent of the three modes' empirical inaccuracy, theoretical inadequacy, and ineffectiveness of policy. Yet this similarity should not surprise us. It is no more than the reflection of their fundamental similarity in points of departure, both ideologically and analytically. Thus, the first mode is ideal-typical in that it sets up the supposedly typical characteristics of development. The second mode concerns itself with how these typical characteristics of the first mode are supposedly diffused from the developed countries to the underdeveloped ones. Finally, the third mode, and herein lies its pioneering service, tells us how the typical characteristics that are identified in the first and diffused according to the second mode are to be acculturated by the underdeveloped countries if they wish to develop. This, in a nutshell, is the sum total of this received theory and analysis of economic development and cultural change; it is the alpha and omega of the possibilities that Manning Nash can visualize: it is thanks to this limitation of his, if not of theory and reality, that Nash manages to arrive at the third mode, as he says, "via the argument of residue."

The pioneers of these three modes have progressed; to social dualism, they have added sociological dualism. Their whole theory and theorizing is split down the middle. They see one set of characteristics, take note of one social structure if any; construct one theory for one part of what has been one world economic and social system for half a millennium, and construct another pattern and theory for the other part. And all this in the name of universalism. They argue that one part of the system, Western Europe and Northern America, diffuses and helps the other part, Asia, Africa, and South America, to develop. They similarly argue that those national metropolises of these three continents that have already received the benefits of this diffusion in turn help pull up their own hinterland behind them. They argue that the takeoff by the underdeveloped countries

and their national metropolises is hindered by the drag on them of their slow and backward hinterlands. Curiously, though fortunately, except for the most irresponsible among them, they do not argue similarly that the takeoff and development of the world capitalist metropolis in Europe and North America is hindered by the drag of its underdeveloped hinterland in Asia, Africa, and Latin America. They ask where the capital for the development of the national metropolises of the underdeveloped countries is to come from and say it must and will come from the developed countries; which is wrong, since in fact it comes from the domestic internal colonies of these national metropolises. They ask where the capital for the development of the already developed countries came from and say it came from themselves; which is also wrong since much, and at the time the critical part, of it came from the consequently now underdeveloped countries. As with most of the remainder of the developed countries' universalism, the theoretical universalism of their social science is a pretense and a sham. If we may borrow something from the arsenal of this mode's pioneers, the theorists of all three modes of economic development and cultural change who like to call themselves universally theoretical dualists are intellectual and political schizophrenics.¹⁷⁰

To render the real significance and value of this highly developed conventional wisdom still clearer, we may characterize it—no less exhaustively than Nash summarizes it—by the caricature of the twin mythological supports of the society that produced it, which Steinberg put on the cover of a *New Yorker*: Santa Claus and Sigmund Freud. American society rests on and revolves around these twin gods, Steinberg suggests, and, we may add, so does the ideology of economic development and cultural change which that same society produces and exports. How are the people in the underdeveloped countries to achieve economic development? By waiting for Christmas and then accepting the gift of diffusion from Santa Claus in the North. What gift does Santa Claus bear for the peoples of the underdeveloped countries? The latest message from Sigmund Freud. If only the people of the mythically characterized underdeveloped world will, as we did, learn to worship at the altar of these twin Gods, they too will change culturally and develop economically. Can it be any wonder that the people of the real underdeveloped world must, and will, look beyond what some others dream possible to find a theory of economic development and cultural change that is empirically congruent with, theoretically adequate for, and politically acceptable to, their reality, needs, and desires? The direction in which to look for an alternative theory of economic development and change that is more adequate for the underdeveloped countries is suggested by the common shortcomings of the three-part approach of received theory reviewed here. Firstly, where this approach

is empirically wrong about the past and present reality of the underdeveloped part of the world, the developed part of the world, and the world as a whole, an adequate alternative theory will have to come to terms with the history and contemporary reality of development and underdevelopment. Secondly, where the approach is theoretically inadequate because it cannot identify the determinant social whole, because it takes account neither of the history of the underdeveloped part nor of its relations with the developed part, and least of all of the world as a whole, and because it does not conform to the structure of that world's social system, an alternative theory must reflect the structure and development of the system that has given rise to, now maintains, and still increases both structural development and structural underdevelopment as simultaneous and mutually produced manifestations of the same historical process. Thirdly, where the development policy of this approach is ever more politically conservative and counsels accepting the structural status quo with folded hands while waiting for others' gifts with open hands, an alternative policy for economic development and cultural change will have to be politically ever more revolutionary and help the peoples of the underdeveloped countries to take the destruction of this structure and the development of another system into their own hands. If the developed countries cannot diffuse development, development theory, or development policy to the underdeveloped countries, then the people of these countries will have to develop them by themselves. These three modes of approach are the emperor's clothes, which have served to hide his naked imperialism. Rather than fashioning the emperor a new suit, these people will have to dethrone him and clothe themselves.

Notes

1. Manning Nash, "Introduction, Approaches to the Study of Economic Growth," in "Psychocultural Factors in Asian Economic Growth" (Issue Editors: Manning Nash and Robert Chin), *Journal of Social Issues*, Vol. 29, No. 1 (January 1963), p. 5.
2. Bert F. Hoselitz, *Sociological Factors in Economic Development* (Glencoe: The Free Press, 1960). Hoselitz is the founder and editor of *EDCC*.
3. Talcott Parsons, *Structure and Process in Modern Societies* (Glencoe: The Free Press, 1960).
4. See especially, Marion J. Levy, Jr., "Contrasting Factors in the Modernization of China and Japan," *EDCC*, Vol. 2, No. 3 (October 1953); reprinted in S. Kuznets, W. E. Moore, and J. J. Spengler, eds., *Economic Growth: Brazil, India, Japan* (Durham, NC: Duke University Press, 1955). Levy refers to a related theme in his "Some Aspects of Individualism and the Problem of Modernization in China and Japan," *EDCC*, Vol., 10, No. 3 (April 1962).
5. Wilbert Moore and David Feldman, *Labor Commitment and Social Change in Developing Areas* (New York: Social Science Research Council, 1960).
6. Daniel Lerner, *The Passing of Traditional Society: Modernizing the Middle East* (Glencoe: The Free Press, 1958).

7. Everett Hagen, "The Theory of Economic Development," *EDCC*, Vol. 6, No. 3 (April 1957); also see his *On the Theory of Social Change* (Homewood, IL: Dorsey Press, 1962).
8. David McClelland, "A Psychological Approach to Economic Development," *EDCC*, Vol. 12, No. 3 (April 1964); and *The Achieving Society* (Princeton, NJ: Van Nostrand, 1961).
9. John H. Kunkel, "Values and Behavior in Economic Development," *EDCC*, Vol. 13, No. 3 (April 1965).
10. Nash, "Introduction," in "Psycho-Cultural Factors in Asian Economic Growth," *Journal of Social Issues*, Vol. 29, No. 1 (January 1963), pp. 5-6.
11. Robert Chin, "Preface, a New Social Issue," *Journal of Social Issues*, Vol. 29, No. 1 (January 1963), p. iii.
12. A 111-page essay by Seymour Martin Lipset entitled "Elites, Education, and Entrepreneurship in Latin America" was unfortunately not available to me in time to be included in this review. In this essay, Mr. Lipset, who is probably the most technically skillful and influential contemporary American political sociologist, masterfully constructs a misinterpretation of Latin American development out of all the major and most of the minor empirical, theoretical, and policy errors criticized here. The essay has since been published as Chapter 1, "Values, Education, and Entrepreneurship," in Seymour M. Lipset and Aldo Solari, eds., *Elites in Latin America* (New York: Oxford University Press, 1967).
13. Charles P. Kindleberger, "Review of the Economy of Turkey; the Economic Development of Guatemala; Report on Cuba," *Review of Economics and Statistics*, Vol. 34, No. 4 (November 1952).
14. Talcott Parsons, *The Social System* (Glencoe: The Free Press, 1951).
15. Jeffery Duncan Mitchell, *Dictionary of Sociology* (London: Routledge and Kegan Paul, 1967), pp. 130-131.
16. Bert F. Hoselitz, "Social Structure and Economic Growth," *Economia Internazionale*, Vol. 6, No. 3 (August 1953); reprinted in *Sociological Factors in Economic Development*, Chapter 2. This is not to say, of course, that this approach exhausts the work of Hoselitz, which on the contrary ranges exceptionally widely over the fields of sociology, economics, history, etc. On the other hand, this part of Hoselitz's work organizes and summarizes a very wide range of work by other social scientists.
17. Bert F. Hoselitz, "Social Stratification and Economic Development," *International Social Science Journal*, Vol. 16, No. 2 (1964).
18. In addition to the already cited article by Levy, see e.g., "India's Cultural Values and Economic Development: A Discussion," *EDCC*, Vol. 7, No. 1 (October 1958); Clifford Geertz, "Religious Belief and Economic Behavior in a Central Japanese Town: Some Preliminary Considerations," *EDCC*, Vol. 4, No. 2 (January 1956).
19. James Abegglen, *The Japanese Factory* (Glencoe: The Free Press, 1958).
20. Nicole Deflefortrie-Soubeyroux, *Les dirigeants de l'industrie française* (Paris: Armand Colin, 1961).
21. David Granick, *The European Executive* (Garden City, NY: Doubleday, 1962).
22. Ferdynand Zweig, *The British Worker* (Harmondsworth: Penguin Books, 1952); *The Worker in an Affluent Society: Family Life and Industry* (London: Heinemann, 1962); Raymond Williams, *Culture and Society 1780-1950* (Harmondsworth: Penguin Books, 1961).
23. Frederick Clairmonte, *Economic Liberalism and Underdevelopment—Studies in the Disintegration of an Idea* (Bombay and London: Asia Publishing House, 1960).
24. David Granick, *The Red Executive* (Garden City, NY: Doubleday, 1960).
25. Michael Harrington, *The Other America, Poverty in the U.S.* (New York: Macmillan, 1963); Gabriel Kolko, *Wealth and Power in America: An Analysis of Social Class and Income Distribution* (New York: Praeger, 1962).
26. Abegglen, *The Japanese Factory*.
27. Robert Bellah, *Tokugawa Religion* (Glencoe: The Free Press, 1957).
28. Levy, "Contrasting Factors in the Modernization of China and Japan."
29. Abegglen, *The Japanese Factory*.
30. McClelland, *The Achieving Society*.

31. C. Wright Mills, *The Power Elite* (New York: Oxford University Press, 1956); Fred J. Cook, *The Warfare State* (New York: Macmillan, 1962); also see Tristan Coffin, *The Armed Society* (Baltimore, MD: Penguin Books, 1964).
32. Eugene Staley, *The Future of Underdeveloped Countries* (New York: Harper, 1964).
33. Walt Whitman Rostow, *The Stages of Economic Growth, a Non-Communist Manifesto* (Cambridge: Cambridge University Press, 1962). The recent *New York Times* profile of Rostow observes:
 Since McGeorge Bundy and Bill D. Moyers left the White House, Mr. Rostow, a former professor at the Massachusetts Institute of Technology, has been emerging as the White House spokesman on foreign affairs. . . . He now organizes and attends the President's Tuesday luncheon conferences. Secretary of State Dean Rusk, Secretary of Defense Robert S. McNamara and the White House Press Secretary, George Christian, are usually the only other guests. (*New York Times*, April 13, 1967)
34. Pablo González Casanova, *La Democracia en México* (Mexico: Era, 1965), p. 202.
35. Hoselitz, "Social Stratification and Economic Development."
36. John J. Johnson, ed., *The Role of the Military in Underdeveloped Countries* (Princeton, NJ: Princeton University Press, 1962); *The Military and Society in Latin America* (Stanford, CA: Stanford University Press, 1964); Edwin Lieuwen, *Arms and Politics in Latin America* (New York: Praeger, 1960); *Generals and Presidents, Neo-Militarism in Latin America* (New York: Praeger, 1964).
37. Frantz Fanon, *Les damnés de la terre* (Paris: Maspero, 1961); published and mistranslated as *The Wretched of the Earth* (New York: Grove Press, 1966).
38. José Luis Cecena, *El Capital Monopolista y la Economía de México* (Mexico: Cuadernos Americanos, 1963); Ricardo Lagos, *La Concentración del Poder Económico en Chile* (Santiago: Editorial del Pacífico, 1961); Carlos Malpica, *Guerra a la Muerte al Latifundio* (Lima: Ediciones Voz Rebelde, 1963); Jacinto Oddone, *La Burguesía Terrateniente Argentina* (Buenos Aires: Populares Argentinas).
39. See, e.g., José Luis de Imaz, *Los que Mandan* (Buenos Aires: EUDEBA, 1964).
40. Frantz Fanon, *The Wretched of the Earth*. The degree of capitalist penetration of underdeveloped countries was observed long ago by Rosa Luxemburg in *The Accumulation of Capital* (New York: Monthly Review Press, 1964), especially Section Three, pp. 329–467.
41. United Nations Economic Commission for Latin America, *The Economic Development of Latin America in the Postwar Period* (New York: United Nations, 1963), E/CN. 12/659.
42. United Nations Economic Commission for Latin America, *The Social Development of Latin America during the Postwar Period* (New York: United Nations, 1963), E/CN. 12/660.
43. Theodore R. Crevanna, ed., *Materiales para el Estudio de la Clase Media en América Latina*, 6 vols. (Washington: Unión Panamericana, 1950–51); Marshall Wolfe, *Las Clases Medias en Centro América: Características que Presentan en la Actualidad y Requisitos para su Desarrollo* (New York: United Nations), E/CN. 12/CCE/Rev. 2; and United Nations, *The Social Development of Latin America*; John L. Johnson, *Political Change in Latin America: The Emergence of the Middle Sectors* (Stanford, CA: Stanford University Press, 1958).
44. Robert Redfield, *The Folk Culture of Yucatan* (Chicago, IL: University of Chicago Press, 1941); "The Folk Society," *American Journal of Sociology*, Vol. 52, No. 4 (January 1941).
45. Robert Redfield, *The Little Community and Peasant Society and Culture* (Chicago, IL: University of Chicago Press, 1960); also see *The Primitive World and Its Transformations* (Ithaca, NY: Cornell University Press, 1955).
46. Manning Nash, *Machine Age Maya* (Glencoe: The Free Press, 1958).
47. Redfield, *The Little Community*.
48. Talcott Parsons, "Social Classes and Class Conflict in the Light of Recent Sociological Theory," in *Essays in Sociological Theory*, Rev. ed. (Glencoe: The Free Press, 1954), p. 324.
49. Mills, *The Power Elite*.
50. William H. Whyte, Jr., *The Organization Man* (New York: Simon and Schuster, 1956).
51. Johnson, *Political Change in Latin America*; "The Political Role of the Latin American Middle Sectors," *The Annals of the American Academy of Political and Social Science*, Vol. 334 (March 1961).

52. Gino Germani, *Política y Sociedad en una Epoca de Transición* (Buenos Aires: Paidós, 1962); *Política e Massa* (Belo Horizonte: Publicações de Revista Brasileira de Estudos Politicos, 1960).
53. Bert F. Hoselitz, "Economic Growth in Latin America," *Contributions to the First International Conference in Economic History, Stockholm 1960* (The Hague: Mouton & Co., 1960).
54. Andrew Gunder Frank, "Brazil; the Goulart Ouster," *The Nation*, April 27, 1964.
55. Kolko, *Wealth and Power in America*.
56. Anibal Pinto, "Concentración del Progreso Técnico y de sus Frutos en el Desarrollo Latinoamericano," *El Trimestre Económico*, Vol. 32, No. 125 (January–March 1965). See also his *Chile: Una Economía Difícil* (Mexico: Fondo de Cultura Económica, 1965).
57. Walt Whitman Rostow, *The Process of Economic Growth* (New York: Norton, 1952).
58. Most of the criticism of Rostow's book has, however, been superficial and largely limited to quibbling about details in the characterization of his stages. This superficiality is notably evident in the "Appraisals and Critiques" of "The Rostow Doctrine" by Meier, Kuznets, Cairncross, Habakkuk, and Gerschenkron in Gerald Meier, ed., *Leading Issues in Development Economics* (New York: Oxford University Press, 1964). It is revealing of the narrowness of American economics that Meier, whose book has been very favorably reviewed for its purported breadth of issues and appraisals, did not include the probably most penetrating criticism of Rostow so far by Paul A. Baran and Eric Hobsbawm, "The Stages of Economic Growth," *Kyklos* (Basel), Vol. 14, Fasc. 2 (1961).
59. Rostow, *The Stages of Economic Growth*, pp. 4, 6, 7, 39.
60. R. Palme Dutt, *India Today and Tomorrow* (London: Lawrence & Wishart, 1955); A. R. Desai, *Social Background of Indian Nationalism* (Bombay: Popular Book Depot, 1959); Jawaharlal Nehru, *The Discovery of India* (New York: John Day, 1946); V. B. Singh, *Indian Economy Yesterday and Today* (New Delhi: People's Publishing House, 1964).
61. Ibid.
62. Basil Davidson, *The African Slave Trade* (Boston, MA: Atlantic-Little Brown, 1961); and Jack Woddis, *Africa, the Roots of Revolt* (London: Lawrence & Wishart, 1960).
63. Eric Wolf, *Sons of the Shaking Earth* (Chicago, IL: University of Chicago Press, 1959).
64. Sergio Bagú, *Economía de la Sociedad Colonial. Ensayo de Historia Comparada de América* (Buenos Aires: Ateneo, 1949); Celso Furtado, *The Economic Growth of Brazil* (Berkeley, CA: University of California Press, 1963); Aldo Ferrer, *The Argentinian Economy: An Economic History of Argentina* (Berkeley, CA: University of California Press, 1967); Anibal Pinto, *Chile, Un Caso de Desarrollo Frustrado* (Santiago: Editorial Universitaria, 1958); Gunder Frank, *Capitalism and Underdevelopment in Latin America*; Ramiro Guerra y Sánchez, *Sugar and Society in the Caribbean* (New Haven, NJ: Yale University Press, 1964).
65. J. H. Boeke, *Economics and Economic Policy of Dual Societies* (New York: Institute of Pacific Relations, 1953); Jaques Lambert, *Os Dois Brasis* (Rio de Janeiro: Ministerio da Educação e Cultura, n. d.). See also note 121.
66. Paul A. Baran, *The Political Economy of Growth* (New York: Monthly Review Press, 1957); Gunder Frank, *Capitalism and Underdevelopment in Latin America*. See also note 120.
67. See United Nations Conference on World Trade and Development (Geneva: 1964), UN Document Series, E/CONF. 46, and especially the Report by the Secretary-General cited in note 92.
68. Baran, *The Political Economy of Growth*; Gunnar Myrdal, *Rich Nations and Poor* (New York: Harper & Brothers, 1957), also issued under the title *Economic Theory and Underdeveloped Regions*; Yves Lacoste, *Les pays sous-développés* (Paris: "Que Sais-Je?," Presses Universitaires de France, 1959); Fanon, *Les damnés de la terre*; Gunder Frank, *Capitalism and Underdevelopment in Latin America*.
69. Quoted in Baran, *The Political Economy of Growth*, pp. 149–150.
70. Gunder Frank, "The Development of Underdevelopment."
71. Ferrer, *The Argentinian Economy*; and "Reflexiones Acerca de la Política de Estabilización en la Argentina," *El Trimestre Económico*, Vol. 30, No. 120 (October–December 1963).

Two Argentinian scholars have recently written doctoral dissertations under Professor Walt Rostow at the Massachusetts Institute of Technology, attempting

to identify in the economic history of their own country his series of stages of economic growth. The period of Pre-Conditions, they thought, was completed by 1914 when the railway net was finished and the entire rich area of the Pampas had been brought into pastoral or agricultural use. But somehow development did not follow, and the Take-Off did not occur, again by their reckoning, until 1933. What they did in this situation was to invent a wholly new stage of growth, or rather non-growth, for the Argentinian case, which they called The Big Delay. Even their Take-Off, moreover, has not been followed by rapid progress. Writing in 1959, the experts of the United Nations Economic Commission for Latin America [said],... "Since the time of the great world depression... per capita production has increased at an average rate scarcely half the rate of the increase registered between the beginning of the century and the onset of the depression." It appears, then, that Argentina had in fact attained a relatively high level of income by the earlier part of the century and that in more recent decades... the Argentine experience has been characterized by delay, stagnation, and—to take another word from the ECLA economists—"strangulation." (Carter Goodrich, "Argentina as a New Country," *Comparative Studies in History and Society*, Vol. VII [1964–65], pp. 80–81)

72. Thomas Mun, *England's Treasure by Forraign Trade, or the Balance of Our Forraign Trade Is the Rule of Our Treasure* (Oxford: Basil Blackwell, 1959), first published in 1664.
73. Richard Cantillon, *Essai sur la nature du commerce en général*, edited, with an English translation and other material, by Henry Higgs (New York: Augustus Kelley, 1964).
74. Karl Marx, *Capital*, Vol. III (Moscow: Foreign Languages Publishing House, n. d.).
75. Earl J. Hamilton, "American Treasure and the Rise of Capitalism," *Economica* (London), No. 27 (1929); *American Treasure and the Price Revolution in Spain, 1501–1650* (Cambridge, MA: Harvard University Press, 1934); *War and Prices in Spain 1651–1800* (Cambridge, MA: Harvard University Press, 1947). See also the extension of this work by P. Vilar, "Problems of the Formation of Capitalism," *Past and Present*, November 1956.
76. Eric Williams, *Capitalism and Slavery* (Chapel Hill, NC: University of North Carolina Press, 1944); reprinted by Russell & Russell, New York, 1963; and issued in paperback by Andre Deutsch, London, 1964.
77. Basil Davidson, *The African Slave Trade; Old Africa Rediscovered* (London: Gollancz, 1959).
78. The same error also applies to a further variant, which is associated particularly with Alexander Gerschenkron, *Economic Backwardness in Historical Perspective* (Cambridge, MA: Belknap Press of Harvard University, 1962). Gerschenkron introduces variations into the ideal types of development. He reasons that since the pattern of development of the late-comers, such as Germany, differs from that of those which developed earlier, it is only reasonable to suppose that the pattern of those still later—that is the still underdeveloped countries—will differ even more from the already established pattern and stages of growth. This analysis might indeed seem to be a major advance over the others. But it is not. As with the other proponents of the first mode, there is no hint in Gerschenkron that the underdeveloped countries also have a history that requires study; nor is there any hint that their history and their relations with the now developed countries are much more important for any serious attempt to understand and remove the causes of underdevelopment than is the study of the history of the developed part of the world, whose experience has been quite different. Gerschenkron's variety of the first mode must, therefore, also be judged inadequate.
79. The *New York Times* profile comments: "Mr. Rostow is an architect of the United States policy in Vietnam, and proud of it," *New York Times*, April 13, 1967. "W. W. Rostow once explained the State Department's rationale behind the arms race in the 1950's as forcing the USSR to 'waste' her resources for military purposes and thus denying her the use of these resources to sustain her growth rate," Two Labor Economists, "Tasks of the American Labor Movement," *Monthly Review*, Vol. 18, No. 11 (April 1967), p. 12. Is this also the rationale for the stages of growth Mr. Rostow is proud to impose on Vietnam and China in the 1960s?
80. Baran, *The Political Economy of Growth*.
81. Gunder Frank, *Capitalism and Underdevelopment in Latin America*.

82. Martin Bronfenbrenner, "The Appeal of Confiscation in Economic Development," *EDCC*, Vol. 3, No. 3 (April 1955); "Second Thoughts on Confiscation," *EDCC*, Vol. 11, No. 4 (July 1963).
83. Daniel H. Garnick, "The Appeal of Confiscation Reconsidered: A Gaming Approach to Foreign Economic Policy," *EDCC*, Vol. 11, No. 4 (July 1963); and "Further Thoughts on Confiscation," *EDCC*, Vol. 12, No. 4 (July 1964).
84. Raymond F. Mikesell, ed., *U.S. Private and Government Investment Abroad* (Eugene, OR: University of Oregon, 1962).
85. Benjamin Higgins, "Foreign Investment and Foreign Aid," in his *Economic Development* (New York: Norton, 1959).
86. Chi Ming-Hon, "External Trade, Foreign Investment, and Domestic Development: The Chinese Experience 1840-1937," *EDCC*, Vol. 10, No. 1 (October 1961).
87. Harry Magdoff, *The Age of Imperialism* (New York: Monthly Review Press, 1969), p. 150.
88. Keith B. Griffin and Ricardo French-Davis, "El Capital Extranjero y el Desarrollo," *Revista de Economía* (Santiago), Vols. 83-84 (1964), pp. 16-22; and Chapter 9.
89. *Ibid.*; Cecena, *El Capital Monopolista y la Economía de México*; and Michael Kirdon, *Foreign Investments in India* (London: Oxford University Press, 1965).
90. Claude McMillan, Jr., Richard F. Gonzales, with Leo G. Erickson, *International Enterprise in a Developing Economy. A Study of U.S. Business in Brazil, M.S.U. Business Studies* (East Lansing, MI: Michigan State University Press, 1964), p. 205.
91. This and the following data on Canada are taken or computed from A. E. Safarian, *Foreign Ownership of Canadian Industry* (Toronto: McGraw-Hill Company of Canada, 1966), pp. 235, 241.
92. Report by the Secretary-General of the Conference, "Towards a New Trade Policy for Development," *Proceedings of the United Nations Conference on Trade and Development* (New York: United Nations, 1964), E/CONF.46/141, Vol. II, pp. 9-13, 42, and other documents of the Conference. It should be noted (cf. p. 13) that in comparing the underdeveloped countries' loss of capital due to declining terms of trade with the "net inflow of all types of finance (loans, investments and grants-in-aid)," the United Nations calculates the latter "including private re-investment," that is, including the investment capital that does not flow in at all, net or gross, but is generated in the underdeveloped countries themselves.
93. Andre Gunder Frank "Invisible Foreign Services or National Economic Development?" in pp. 181-191 Andre Gunder Frank, *Latin America: Underdevelopment or Revolution* (New York: Monthly Review Press, 1969).
94. The 7.3 percent is computed from the \$6,195 million service expenditures in *ibid.* as a percentage of the \$84,458 million GNP in 1962 reported in United Nations Economic Commission for Latin America, *Estudio Económico de América Latina 1963* (New York: United Nations, 1964), E/CN.12/696/Rev. 1, p. 6. This document is also the source of all the data used in the computations of the articles cited in note 93. The 3 percent is computed from United Nations Commission for Latin America, *El Financiamiento Externo de América Latina* (New York: United Nations, 1964), E/CN.12/649/Rev. 1, p. 33.
95. *Relatorio de la III Reunion de Facultades y Escuelas de Economía de América Latina, Mexico, June 21-25, 1965*, published in *Presente Económico* (Mexico), Vol. 1, No. 1 (July 1965), p. 63, and in *Comercio Exterior* (Mexico), Vol. 15, No. 6 (June 1965), p. 439; and *Desarrollo* (Colombia), No. 1 (January 1966), pp. 7-9.
96. Arturo Frondizi, *A Luta Antiimperialista* (Sao Paulo: Editora Brasileira, 1958); a translation of *Petroleo y Política* (Buenos Aires: Editorial Raigal, 1955).
97. Quoted in Cámara Textil del Norte, "Las Inversiones Extranjeras y el Desarrollo Económico de México," *Problemas Agrícolas e Industriales de México*, Vol. 9, No. 1-2 (1957).
98. For more detailed analysis of this problem, see Cecena, *El Capital Monopolista y la Economía de México*, Fernando Carmona, *El Drama de América Latina, El Caso de México* (Mexico: Cuadernos Americanos, 1964); Frondizi, *A Luta Antiimperialista*; Silvio Frondizi, *La Realidad Argentina*, 2nd. ed. (Buenos Aires: Praxis, 1967), Vol. I; Hamza Alavi, "U.S. Aid to Pakistan," *Economic Weekly* (Bombay), Special Number (July 1963); Chapters eight and nine of this book; Gunder Frank, *Capitalism and Underdevelopment in Latin America*; and "Foreign Investment in Latin American Underdevelopment from Colonial Conquest to

- Neo-Imperialist Integration," in David Horowitz, ed., *Containment and Revolution* (Boston, MA: The Beacon Press, 1967).
99. Analysis of this process may be found, e.g., for India in the work cited in note 60; for Latin America in note 62; for China in note 132; for Spain, in José Lana, *La Época del Mercantilismo en Castilla (1500-1700)*, 2nd. ed. (Madrid: Atlas, 1943); for Portugal in Alan K. Manchester, *British Preeminence in Brazil, Its Rise and Decline* (Chapel Hill, NC: University of North Carolina Press, 1933).
 100. "The U.S. Business Stake in Europe," *Newsweek*, March 8, 1965, pp. 67-74.
 101. See Gunder Frank, particularly the last part of "Capitalist Development of Underdevelopment in Brazil," in *Capitalism and Underdevelopment in Latin America*. Also see "The Growth and Decline of Import Substitution in Brazil," *Economic Bulletin for Latin America* (New York: United Nations), Vol. 9, No. 1 (March 1964).
 102. Manning Nash, "Social Prerequisites to Economic Growth in Latin America and South East Asia," *EDCC*, Vol. 12, No. 3 (April 1964); Burkhard Strümpel, "Preparedness for Change in Peasant Society," *EDCC*, Vol. 13, No. 2 (January 1965); S. N. Eisenstadt, "Breakdowns of Modernization," *EDCC*, Vol. 12, No. 4 (July 1964); William N. Parker, "Economic Development in Historical Perspective," *EDCC*, Vol. 10, No. 1 (October 1961); S. N. Eisenstadt, "Sociological Aspects of the Economic Adaptation of Oriental Immigrants in Israel—A Case Study in the Problem of Modernization," *EDCC*, Vol. 4 (April 1956); and others.
 103. Friedrich List, *National System of Political Economy* (Philadelphia, 1856).
 104. Quoted in Pedro Santos Martinez, *Historia Económica de Mendoza durante el Virreynato* (Madrid: Universidad Nacional del Cuyo, 1959), p. 125, and retranslated from the Spanish by the author.
 105. Clairmonte, *Economic Liberalism and Underdeveloped Studies in the Disintegration of an Idea*.
 106. Max Nolf, "Industria Manufacturera," in *Geografía Económica de Chile* (Santiago: Corporación de Fomento de la Producción), Vol. III, pp. 162-163.
 107. Claudio Véliz, "La Mesa de Tres Patas," *Desarrollo Económico* (Buenos Aires), Vol. 3, No. 1-2 (April-September 1963), pp. 237-242.
 108. Ferrer, *The Argentinian Economy*, p. 56.
 109. Ferrer, "Reflexiones acerca de la Política de Estabilización en la Argentina," pp. 501-514. Emphasis in the original.
 110. Antonio García, *La Democracia en la Teoría y en la Práctica, Una Tercera Posición Frente a la Historia* (Bogotá: Editorial Iqueima, 1951), and Bases de la Economía Contemporánea, *Elementos para una Economía de Defensa* (Bogotá, 1948); Moisés Gonzales Navarro, ed., *Vallarta en la Reforma* (Mexico: Ediciones de la Universidad Nacional Autónoma, 1956); and *La Colonización en México, 1877-1910* (Mexico, 1960); Jesús Reyes Heróles, *El Liberalismo Mexicano*, 3 Vols. (Mexico: Universidad Nacional Autónoma, Facultad de Derecho, 1957-61).
 111. See works cited in notes 38, 56, and 66.
 112. United Nations Economic Commission for Latin America, *The Economic Development of Latin America in the Postwar Period*, and other publications.
 113. V. I. Lenin, "The State and Revolution," in *Selected Works* (Moscow: Foreign Languages Publishing House, n. d.), Vol. H, Part 1.
 114. Quoted in Cook, *The Warfare State*.
 115. Mills, *The Power Elite*.
 116. Johnson, *Political Change in Latin America*.
 117. Gino Germani, "Estrategia para Estimular La Movilidad Social," *Desarrollo Económico* (Buenos Aires), Vol. 1, No. 3 (1962).
 118. Hoselitz, "Economic Growth in Latin America."
 119. Anibal Pinto, *Chile: Una Economía Difícil*, and his "Concentración del Progreso Técnico y sus Frutos en el Desarrollo Latinoamericano." Also see Gabriel Kolko, for the United States.
 120. Andre Gunder Frank, Destroy Capitalism not Feudalism, in pp.350-361 Andre Gunder Frank, *Latin America: Underdevelopment or Revolution* (New York: Monthly Review

- Press, 1969); Rodolfo Stavenhagen, "Seven Erroneous Theses about Latin America," *New University Thought*, Vol. 4, No. 4 (Winter 1966/67); Claudio Véliz, "Social and Political Obstacles to Reform," *World Today* (London), January 1963, reprinted in Oscar Delgado, ed., *Reformas Agrarias en America Latina* (Mexico: FONDA de Cultura Económica, 1965).
121. Benjamin Higgins, "The 'Dualistic Theory' of Underdeveloped Areas," *EDCC*, Vol. 4, No. 2 (January 1956); also see his *Economic Development*.
 122. J. H. Boeke, *The Structure of the Netherlands Indian Economy* (New York: Institute of Pacific Relations, 1942); *The Evolution of the Netherlands Indies Economy* (New York: Institute of Pacific Relations, 1946); and the definitive *Economics and Economic Policy of Dual Societies*.
 123. P. T. Ellsworth, "The Dual Economy: A New Approach," *EDCC*, Vol. 10, No. 4 (July 1962); Walter Elkan, "The Dualistic Economy of the Rhodesias and Nyasaland," *EDCC*, Vol. 11, No. 4 (July 1963); Samir Das Gupta, "Underdevelopment and Dualism—A Note," *EDCC*, Vol. 12, No. 2 (January 1964); Tsunehiko Watanabe, "Economic Aspects of Dualism in the Industrial Development of Japan," *EDCC*, Vol. 13, No. 3 (April 1965).
 124. Jaques Lambert, *Os Dois Brasis*; see also his new book, *L'Amérique latine* (Paris: Presses Universitaires de France, 1963).
 125. The classic argument of the enclave economy is that of J. H. Boeke.
 126. Pablo González Casanova, *La Democracia en México*, and many other works. The Guatemalan government's "Seminario de Integración Nacional" carries the entire idea in the organization's very name.
 127. See review of Rostow's work earlier, and Andre Gunder Frank, *Capitalism and Underdevelopment in Latin America*, especially the section entitled "Capitalism and the Myth of Feudalism in Brazilian Agriculture." For further criticism of dualism in general and of the particular dualist theses of Jaques Lambert and Celso Furtado about Brazil and of Pablo González Casanova about Mexico, Andre Gunder Frank, "The Mexican Democracy of Pablo Gonzalez Casanova" and "The Brazilian Pre-Revolution of Celso Furtado" in Andre Gunder Frank, *Latin America: Underdevelopment or Revolution* (New York: Monthly Review Press 1969), pp. 318–339.
 128. Wolf, *Sons of the Shaking Earth*.
 129. Karl Marx, "British Rule in India," in *On Colonialism* (Moscow: Foreign Languages Publishing House, n. d.).
 130. Dutt, *India Today and Tomorrow*.
 131. Desai, *The Social Background of Indian Nationalism*.
 132. Owen Lattimore, "The Industrial Impact on China 1800–1950," *First International Conference of Economic History, Stockholm 1960* (The Hague: Mouton & Co., 1960).
 133. Woddis, *Africa, the Roots of Revolt*.
 134. Jean Suret-Canale, *Histoire de l'Afrique Occidentale* (Paris: Editions Sociales, 1961).
 135. Mamadou Dia, *Réflexions sur l'économie de l'Afrique noire* (Paris: Présence Africaine, 1960).
 136. W. F. Wertheim, *Indonesian Society in Transition, a Study of Social Change*, 2nd rev. ed. (The Hague and Bandung: W. van Hoeve Ltd., 1959); and Clifford Geertz, *Agricultural Involvement, the Process of Ecological Change in Indonesia* (Berkeley, CA: University of California Press, 1963).
 137. Erie Wolf, *Sons of the Shaking Earth*, and "Types of Latin American Peasantry," *American Anthropologist*, Vol. 57, No. 3 (June 1955).
 138. Gunder Frank, *Capitalism and Underdevelopment in Latin America*.
 139. Redfield, *The Folk Culture of Yucatan*, and *The Little Community and Peasant Society and Culture*.
 140. Redfield, *Human Nature and the Study of Society*, Papers of Robert Redfield, Margaret Park Redfield, ed. (Chicago, IL: University of Chicago Press, 1962).
 141. Redfield, *Peasant Society and Culture*.
 142. Nash, "Introduction," in "Psycho-Cultural Factors in Asian Economic Growth," pp. 5–6.
 143. Nash, "Social Prerequisites to Economic Growth in Latin America and South East Asia," p. 242.
 144. Bert F. Hoselitz, "Role of Incentives in Industrialization," *Economic Weekly* (Bombay), Vol. 15, Nos. 28, 29, and 30, Special Number (July 1963).

- 145 Everett E. Hagen, *On the Theory of Social Change*.
- 146 McClelland, "A Psychological Approach to Economic Development."
- 147 McClelland, *The Achieving Society*, pp. 205, 238, 391.
- 148 David McClelland, "Motivational Patterns in Southeast Asia with Special Reference to the Chinese Case," *Journal of Social Issues*, Vol. 29, No. 1 (January 1963), p. 17.
- 149 McClelland, *The Achieving Society*, pp. 391–437.
- 150 Max Weber, *The Protestant Ethic and the Spirit of Capitalism* (London: G. Allen & Unwin, 1930).
- 151 J. A. Schumpeter, *The Theory of Economic Development* (Cambridge, MA: Harvard University Press, 1934).
- 152 See note 18 earlier.
- 153 For recent examples see Alec P. Alexander, "Industrial Entrepreneurship in Turkey: Origins and Growth," *EDCC*, Vol. 8, No. 4, Part 1 (July 1960), and Arcadius Kahan, "Entrepreneurship in the Early Development of Iron Manufacturing in Russia," *EDCC*, Vol. 10, No. 4 (July 1962).
- 154 W. Paul Strassman, *Risk and Technological Innovation: American Manufacturing Methods in the Nineteenth Century* (Ithaca, NY: Cornell University Press, 1959); and "The Industrialist," in John J. Johnson, ed., *Continuity and Change in Latin America* (Stanford, CA: Stanford University Press, 1964).
- 155 Norman N. Bradburn and David Berlew, "Need for Achievement and English Industrial Growth," *EDCC*, Vol. 10, No. 1 (October 1961); Juan B. Cortes, "The Achievement Motive in the Spanish Economy between the 13th and 18th Centuries," *EDCC*, Vol. 9, No. 1 (October 1960); James N. Morgan, "The Achievement Motive and Economic Behavior," *EDCC*, Vol. 12, No. 3 (April 1964).
- 156 S. N. Eisenstadt, "The Need for Achievement," *EDCC*, Vol. 11, No. 4 (July 1963), p. 431.
- 157 Kunkel, "Values and Behavior in Economic Development," pp. 276–277.
- 158 John H. Kunkel, "Psychological Factors in the Analysis of Economic Development," in the same issue of *Journal of Social Issues*.
- 159 Eisenstadt, "The Need for Achievement."
- 160 Kunkel, "Values and Behavior in Economic Development."
- 161 Kunkel, "Psychological Factors in the Analysis of Economic Development," pp. 72–73, 82. For a similar criticism also see Eisenstadt, "The Need for Achievement."
- 162 This effort is reminiscent of the famous but unsuccessful attempt to remedy functionalists' accounting for the existence of institutions through reliance on teleology by George C. Homans and David M. Schneider in their *Marriage, Authority, and Final Causes. A Study of Unilateral Cross-Cousin Marriage* (Glencoe: The Free Press, 1955). Rejecting the final cause of societal equilibrium as an explanation for an institution's existence, Homans and Schneider sought to substitute an identifiable efficient cause; though strangely their "efficient cause" was an internal state, that is, another final cause similar to the ones criticized here.
- 163 Kunkel, "Values and Behavior in Economic Development," pp. 275, 277.
- 164 Nash, "Introduction," in "Psycho-Cultural Factors in Asian Economic Growth," p. 4. Emphasis added.
- 165 Nash, *Machine Age Maya*.
- 166 McClelland, "Motivational Patterns in Southeast Asia with Special Reference to the Chinese Case," and *The Achieving Society*, pp. 412–413.
- 167 McClelland, *The Achieving Society*, p. 423.
- 168 McClelland, "Motivational Patterns in Southeast Asia with Special Reference to the Chinese Case."
- 169 "Mr. Rostow's former university colleagues on the old Kennedy White House staff... are savagely critical of his increasing influence, and they condemn his aggressive intellectualism as self-serving opportunism that consoles the President but tends to mislead him, particularly on Vietnam," *New York Times*, April 13, 1967.
- 170 Further theoretical limitations of the functionalist part of this social science theory are examined in Andre Gunder Frank, *Functionalism and Dialectics*, in: Andre Gunder Frank, *Latina America: Underdevelopment or Revolution* (New York: Monthly Review Press 1969), pp. 95–107.

*From National Development to World
Development: The Underdevelopment of
Development*

Reconceptualization of Theory and World History

The shift from conceptualizing development within the bounds of national boundaries or nation-states to understanding it within world systemic lines for Gunder Frank began in the publication of two companion books in the late 1970s: *World Accumulation 1492–1789* (1978a) and *Dependent Accumulation and Underdevelopment* (1978b). Though written much earlier, between 1968 and 1969, they were not published until his return to the Federal Republic of Germany following the military coup in Chile. By this time, three other seminal publications also appeared calling for such a mode of analysis: Samir Amin's *Accumulation on the World Scale* (1974), Eric Wolf's *Europe and the People without History* (1982), and Immanuel Wallerstein's *The Modern World-System Vol. 1* (1974).

The work of Immanuel Wallerstein was the most explicit in this call for analyzing social change and economic transformation within a world-historical context. Methodologically, Wallerstein (1974, 1976, 1979) adduces us to rethink the conceptual schemes and categories of the social sciences. To understand and interpret social change, the false sectorializing tendency of the social sciences has to be abandoned. What was seen previously as five separately recognized social sciences (anthropology, economics, geography, political science, and sociology), with their separate meaningful units of analysis, should be seen as a single subject matter. Analysis of social change and economic transformation in the making of the modern world should consider the totality of social action, and thus triangulate information from the 'separate' social sciences. Notwithstanding this, the mediation of history in the understanding of social occurrences

is considered necessary. This call for a unidisciplinary social science is to be coupled with a macro-level, holistic approach that awards primacy to a historical social system having an effective global division of labor within multiple state structures circumscribed by long cycles of expansion and contraction. The proposal calls into question the orthodox assumption that an understanding of social change within a given nation-state or geographical space in the world-system can be reached by analyzing and interpreting the dynamics and changes occurring within these boundaries alone. The push for a holistic, macro-level analysis of a historical world system encourages us to view social change and economic transformation in the making of the modern world as processes that occur transhistorically within the dynamics of the world-system.

During the earlier time period, Frank partnered with Samir Amin, Giovanni Arrighi, and Immanuel Wallerstein in the publication of two books: *Dynamics of Global Crisis* (1982) and *Transforming the Revolution* (1990), thus agreeing with them on the overall conceptualization of the nature of capitalism and dynamics of the world-system. This intellectual bond was to change starting in the late 1980s when Gunder Frank started to collaborate with Barry Gills in a study of the evolution of the world system and his participation in a UNESCO-sponsored trip commemorating the role of the Silk Roads in the ancient world. Frank himself identified this shift as his Mark II period whereby he disavowed and criticized his own earlier writings on the nature of capitalism, and the historical analyses of economic transformation in the Third World. The chapters in Part 2 provide an overview of this reorientation. In his last published book, *Reorient* (1998), Gunder Frank applied this reconceptualized theoretical framework developed with Barry Gills to the examination of a period in world history thus challenging some commonly received understanding of the said period. *Reorient* was a culminating point in this theoretical shift.

The rethinking of development led Frank to reject the usefulness of utilizing capitalism as a mode of production for analysis, and whether it was historically on the mark to use the '1500' historical breakpoint as the emergence of the capitalist world economy à la Wallerstein, Amin, and others. He declared that modes of production were constructed typically as ideological beliefs, and that they have no basis to historical reality; in this case, development. This foundation thus provided him with the theoretical edifice to suggest how development and transformation occur. According to him, based on an inspection of world history, the world system has a longer evolutionary history than what world-systems' theorists such as Wallerstein and Amin have suggested. The world system is an evolving system with hegemonic (or domination) shifts occurring throughout the course of world history/development. The call is to abandon the concept of capitalism as a distinct mode of production (as well,

the relevancy of mode of production as a heuristic concept) and of transition between modes because these constructs are viewed as obstacles to understanding the essential unity of a world system that has existed for at least five thousand years. What this means for him is that such shifts underlie the dynamics of world development and the centrality of Europe in this developmental trajectory and that the '1500' line in the sand (as proposed by Wallerstein and Amin) has to be reconsidered. This critique led him to attack the Eurocentricity implicit in world-systems analysis as propounded by Wallerstein, Amin, Braudel, and others for their emphases on '1500' as the breakpoint for the making of the modern world with the emergence of the European world-economy. Amin (1999), Arrighi (1999), and Wallerstein (1999) have replied vigorously to this critique.

Every age identifies its "modern" and distinguishes it from a variety of premodern, a cacophony of antimodern, and perhaps even a picture of postmodern times. Such scholarship hinges on the identification of discontinuity, and perhaps quite naturally then focuses on the set of topics that have undergone or are said to be undergoing the hypothesized change. Gunder Frank has dared to ask how much has actually changed relative to that which has stayed the same. His Mark II period depicts an understanding of world history as world development and calls for a non-Eurocentric conceptualization of it. For Gunder Frank, the contemporary world system has had a history of at least five thousand years. By applying the methodology of world historical analysis much farther back in time and space, he and Barry Gills have argued that the process of the accumulation of various forms of capital and associated cycles existed long before (prior to 1500) the emergence of the "capitalist" modern world system. This means that capital accumulation has occurred continuously within a world system over at least the last five thousand years and that the apparent transformation from one social mode of production and organization to another (e.g., the transition from feudalism to capitalism) might rather be the continuation of cyclical changes of the world system. This view rejects the traditional conception of the genesis of capitalism and capital accumulation, and even the positions of Samir Amin (1991) and Immanuel Wallerstein (1991).

The theoretical reformulation was coupled with a plea and the necessity to consider world history in our understanding of world development—a theme Frank had repeatedly stressed also in his earlier publications on dependency. Frank is adamant that only world history will do, as he fondly quotes from Ranke that there is no history other than world history. Such a determined call for world history led Frank to even assert that theory equals history and, in his case, theory equals world history. From this theoretical position, Frank with Gills outlined their theory of a 5,000 year world system with its set of dynamics and structures. Chapters three–five

in Part 2 outline the theoretical reconceptualization of Frank's Mark II period.

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CHAPTER THREE

*Transitional Ideological Modes: Feudalism, Capitalism, Socialism*¹

Introduction to Transitions and Modes in the World System

The present “transition from socialism to capitalism” and the possible future “shift of hegemony from the United States to Japan” are occasions to reexamine several scientific tenets of our politics and political tenets of our social science. Among these are: (1) the “transition from feudalism to capitalism”; (2) the “transition from capitalism to socialism”; (3) the process of “transition” itself; (4) the notion of feudal, capitalist, and socialist “modes of production”; and (5) the hegemonic rise and decline of Europe and the West in the modern world capitalist system. The question arises as to whether any or all of the above are based on scientific analytical categories, or whether they are only derived from fond ideological beliefs. Perhaps both contemporary political reality and available historical evidence should now lead us to abandon some or even all of these positions.

My tentative conclusion is that ideological blinkers have for too long prevented us from seeing that the world political economic system greatly predated the rise of capitalism in Europe and its hegemony in the world. The rise of Europe represented a hegemonic shift from East to West within a preexisting system. If there was any transition then, it was this hegemonic shift within the system rather than the formation of a new system. Now we are again in one of the alternating periods of hegemony and rivalry in the world system, which portends a renewed westward shift of hegemony across the Pacific. To identify the system with its dominant mode of production is a mistake. There was no transition from feudalism

to capitalism as such. Neither was there (to be) an analogous transition from capitalism to socialism. If these analytical categories of "modes of production" prevent us from seeing the real-world political economic system, it would be better to abandon them altogether. These categories of "transition" and "modes" are not essential or even useful tools, but rather obstacles to the scientific study of the underlying continuity and essential properties of the world system in the past. They also shackle our political struggle and ability to confront and manage the development of this same system in the present and future.

A number of recent academic publications offer a good opportunity for such a reexamination of the (un?)holy canons of our historical science and contemporary politics. These publications include *The Brenner Debate* (Aston and Philpin, 1985) on the transition from feudalism to capitalism in Europe; *Before European Hegemony* on the westward shift of hegemony in the thirteenth century by Janet Abu-Lughod (1989); *The Rise and Fall of the Great Powers of Europe and America* by Paul Kennedy (1987); *Long Cycles in World Politics during the Last 500 Years* by George Modelski (1987); *On Global War* during the same period by William Thompson (1988); *Global Formation: Structures of the World-Economy* then and now by Christopher Chase-Dunn (1989a); and other works on hegemonic changes.

Several recent articles by Wallerstein also offer a particularly revealing opportunity to reexamine all of the issues posed in my opening paragraphs. Wallerstein (1989a) looked back on the last, and forward to the next, fifteen years of "World-System Analysis: The Second Phase" at the 1989 annual meeting of the American Sociological Association. Under the title "The West, Capitalism, and the Modern World-System," Wallerstein (1989b) considers "Why in Europe rather than China?" in a contribution to a volume edited by Joseph Needham. In two further articles, Wallerstein (1988, 1989c) hones down the definition of his "modern-world-capitalist-system" and its differentia specifica from all other systems. These articles also offer good occasion for us to reexamine these issues of transitions and modes, as well as those of origins of and hegemony in the modern world capitalist system. I do so in this essay from an historical perspective on a world system history in which Europe was only a Johnny-come-lately and temporary hegemony.

Wallerstein (1989b) examines the possible distinctions between the "modern world-system, the capitalist world-system and capitalism," and finds them nonexistent. Examination of his argument about this distinctiveness will show that it is both internally self-contradictory and externally contradicted by the historical evidence. My own argument is that Wallerstein's interpretation is too limited, indeed, self-limiting, because he fails to take sufficient account of the world system.

I have already made a similar point about feudalism and capitalism. In a debate with Rodolfo Puiggrós in 1965 I stated that “if we are to understand the Latin American problematique we must begin with the world system that creates it and go outside the self-imposed optical and mental illusion of the Ibero-American or national frame” (Frank, 1965, translated in Frank, 1969:231). I now argue that the same imperative also applies to the problematique of transition between feudal and capitalist modes of production in Europe.

In the last generation all sides of the Dobb-Sweezy (recently reprinted in Hilton, 1976) and Brenner (Aston and Philpin, 1985) debates, like generations of “national frame” and other Eurocentric scholars before them, have sought the answer through a change in the mode of production within Europe. Yet if we are to understand this apparently European problematique we must also “begin with the world system that creates it” and abandon the “self-imposed optical and mental illusion of the [European] or national frame.” If we (re)examine Wallerstein’s argument and the historical evidence from a world system perspective, it appears that the world system was not born in 1500; it did not arise in Europe; and it is not exclusively capitalist.

World System Comparisons and Similarities

Wallerstein identifies the most essential characteristics of the “modern world-capitalist-system” variously in one, three, six, and twelve points. The single most important and defining *differentia specifica* is:

It is this ceaseless accumulation of capital that may be said to be its most central activity and to constitute its *differentia specifica*. No previous historical system seems to have had any comparable *mot d'ordre* of social limitlessness.... At the level of this central defining activity of ceaseless growth, the ceaseless accumulation of capital... no other historical system could have been said to have pursued such a mode of social life for more than at most brief moments.... The one thing that seems unquestionable, and unquestioned, is the hyperbolic growth curves—in production, population, and the accumulation of capital—that have been a continuing reality from the sixteenth century.... There was the genesis of a radically new system.... (Wallerstein, 1989b: 9, 10, 26)

However, accumulation has played a central if not the central role in the world system far beyond Europe and long before 1500, as Gills and Frank (1990) emphasize under the title “The Cumulation of Accumulation.” Numerous historical and theoretical objections to this thesis, including

Wallerstein's, are examined in detail and rejected as unfounded in Frank (1990). A small sample of the vast evidence in support of earlier world system accumulation is presented later.

Perhaps the differences become clearer if we compare Wallerstein's "modern-world-capitalist-system" with alternatives on more counts than just one. Elsewhere, Wallerstein distinguishes three different characteristics that supposedly set his system apart: "...this descriptive trinity (core-periphery, A/B [cycle phases], hegemony-rivalry) as a pattern maintained over centuries is unique to the modern world system. Its origin was precisely in the late fifteenth century" (Wallerstein, 1988: 108).

As it happens, and well before reading Wallerstein's above-cited article, Gills and Frank (1990) emphasized the very same trinity of center-periphery, A/B phased cycles, and hegemony/rivalry as the other central, defining characteristics of our world system. Certainly Chase-Dunn (1986), Abu-Lughod (1989), and Wilkinson (1987, 1988) among others have also found these same features earlier and elsewhere. Wallerstein (1989a) himself recognizes this and said so in his above-cited review at the American Sociological Association meetings.

Perhaps we should go into more detail still. Elsewhere, Wallerstein (1989c: 8–10) summarizes six "realities of the evolution of this historical system." Wallerstein (1989a) then helps us by detailing these realities and extending the list to twelve "characteristics presumed to be the description of the capitalist world-economy":

- the ceaseless accumulation of capital as its driving force;
- an axial division of labor in which there is a core-periphery tension, such that there is some form of unequal exchange (not necessarily as defined originally by Arghiri Emmanuel) that is spatial;
- the structural existence of a semi-peripheral zone;
- the large and continuing role of non-wage labor alongside of wage labor;
- the correspondence of the boundaries of the capitalist world-economy to that of an interstate system comprised of sovereign states;
- the location of the origins of this capitalist world-economy earlier than in the nineteenth century, probably in the sixteenth century;
- the view that this capitalist world-economy began in one part of the globe (largely Europe) and later expanded to the entire globe via a process of successive "incorporations";
- the existence in this world-system of hegemonic states, each of whose periods of full or uncontested hegemony has however been relatively brief;
- the non-primordial character of states, ethnic groups, and households, all of which are constantly created and recreated;

- the fundamental importance of racism and sexism as organizing principles of the system;
- the emergence of anti-systemic movements that simultaneously undermine and reinforce this system;
- a pattern of both cyclical rhythms and secular trends that incarnate the inherent contradictions of the system and which accounts for the systemic crisis in which we are presently living. (Wallerstein, 1989b: 3–4)

I contend here that 240 of these 242 words describing the 12 characteristics of the world system after 1500 are equally true of the world economy/system(s) before 1500, whether “capitalist” or not. The two exceptions are under (6) “the origins...probably in the sixteenth century” and under (7) that this world system began in “(largely Europe).” Everything else Wallerstein says about the presumed characteristics of the “capitalist world-economy” and the “modern world-system” was equally true of the medieval and ancient world system.

Thus, if we examine these separate lists, we find that each of them applies equally to other earlier world systems and/or the same world system before 1500. Of course, I do not expect the reader simply to accept this statement. He or she must undertake these comparisons personally. In doing so, however, an excellent guide can be found in Wallerstein himself. For he now has some doubts about his own position and finds “an uncomfortable blurring of the distinctiveness of the patterns of the European medieval and modern world” (1989b: 33). Indeed, Wallerstein is among those who chip away at and in fact question their own “unquestionable” faith in various ways:

Many of these [previous] historical systems had what we might call proto-capitalist elements. That is, there often was *extensive* commodity production. There existed producers and traders who sought profit. There was *investment of capital*. There was wage-labor. There was *Weltanschauungen* consonant with *capitalism*. But none had quite crossed the threshold of creating a system whose primary driving force was the incessant accumulation of capital. (1989b: 35)

We must now renew the question, why did not capitalism emerge anywhere earlier. It seems unlikely that the answer is an insufficient *technological* base.... It is unlikely that the answer is an absence of an *entrepreneurial* spirit. The history of the world for at least two thousand years prior to 1500+ shows an enormous set of groups, throughout multiple historical systems, who showed an aptitude and inclination for *capitalist enterprise*—as producers, as *merchants*, as *financiers*. “Proto-capitalism” was so widespread one might consider it to be a

constitutive element of all the redistributive/tributary world-empires the world has known....Something was preventing it [capitalism]. For they did have the *money* and energy at their disposition, and we have seen in the modern world how powerful these weapons can be. (1989b: 59–60, my emphasis)

Moreover, Wallerstein also negates the uniqueness of his “modern-world-capitalist-system” in numerous other ways. He states, for example, that “All the empirical work of the past 50 years on these other systems has tended to reveal that they had much more extensive commodification than previously suspected....It is of course a matter of degree” (1989b: 19, 20).

After Wallerstein’s own account of (proto)capitalist “elements” in existence long before 1500, it would be tedious for me to repeat my own as set out in Frank (1990) and in more detail in Gills and Frank (1990). Suffice it to observe here that (1) Wallerstein will readily admit that “hyperbolic growth curves in production, population and accumulation of capital” have been *cyclical* since 1500; and (2) Wallerstein and others must also recognize that frequently, and in many places, rapid and massive growth of production, population, and accumulation occurred for much more than “brief” moments long before 1500. Wallerstein himself helps us to observe that this was true, for instance, during the period 1050–1250 in Europe. The same, only much more so, also occurred *at the same time* in Sung China. Some centuries earlier, *capital accumulation* accelerated in Tang China, then in the Islamic caliphate. The same phenomenon can be observed in Gupta India and Sassanian Iran, to cite some other instances.

However, the economy and polity of the ancient and even the archaic world (system) were also characterized by the whole gamut of Wallerstein’s “elements” of (proto)capitalism (capital, money, profit, merchants, wage-labor, entrepreneurship, investment, technology, etc.) and the ones he synthesized for the “modern”-world-capitalist-system (capital accumulation, core—periphery, hegemony, interstate system, cycles, racism, sexism, social movements, and the rest). Simply recall the examples best known to Westerners: Rome, China (great canals and walls), Egypt, and Mesopotamia (irrigation systems and monuments). Moreover, long cyclical ups (and subsequent downs) in *accumulation* may be said to have been world systemic if not world system wide. The important fact is that they were *systemically and systematically related* to each other, for example, in Han China, Gupta India, Parthian and then Sassanian Persia, Imperial and then Byzantine Rome, Axum East Africa, and, of course, “barbarian” Inner Asia, not to mention other parts of the world.

That is, the historical evidence also meets the more difficult test of the specificity of capitalism posed by Maurice Godelier (1990). He makes a

fourfold classification of characteristics similar to Wallerstein's own efforts in that direction. Yet even Godelier remarks that the four characteristics of capitalism he identifies did *not begin* with capitalism; the necessary and sufficient conditions of a new (capitalist) economic structure are their "combination in a new relation" and their "mutual connection" with each other (Godelier, 1990). The historical evidence, however, shows that even the *combination and mutual relation* of Godelier's four or Wallerstein's three, six, or twelve characteristics *did not begin with capitalism in 1500*.

Significantly, however, Wallerstein and the others, with the exception of Wilkinson, are talking only about some similarities with *other* "world" systems. Following them so far, I am arguing only from the old adage that "If it looks like a duck, walks like a duck and quacks like a duck . . . it must be a [world system] duck." But in that case, it or they could just be one or more *other* world system ducks, as Chase-Dunn (1989b) argues. Even Wallerstein might admit this comparison, though the similarities might make him uncomfortable. So, what is this invisible and still unspecified "something" that distinguishes the modern-world-capitalist-system? Perhaps it is only the *Weltanschauung of capitalism itself*, as posed by Smith and Marx then, and Wallerstein and Amin now, which retrospectively sees a qualitative break around 1500 where historically there was none. We observe later that the essential something in this *Weltanschauung* turns out to be the supposed identity of the (capitalist) mode of production and system. According to Smith and Marx, who led me astray in writing my own book two decades ago, the discovery of America and of the passage to the East Indies by the Cape of Good Hope were the greatest events in the history of mankind, and opened up new ground for the bourgeoisie. That is from a European point of view, of course. But from a wider world perspective these two events, as well as others within Europe, were only developments in the unfolding of world history itself. Why were these two new passages to the East and West Indies important, even for Europeans, and why did they want to get there more easily in the first place, if it was not because of what was happening there—and what was to be obtained there—before 1500?

World System Transitions and Continuity

Jacques Garnet (1985: 347–348) proposes an alternative world perspective:

what we have acquired the habit of regarding—according to the history of the world that is in fact no more than the history of the West—as the beginning of modern times was only the repercussion of the upsurge of the urban, mercantile civilizations whose realm extended, before the

Mongol invasion, from the Mediterranean to the Sea of China. The West gathered up part of this legacy and received from it the leaven which was to make possible its own development. The transmission was favored by the crusades of the twelfth and thirteenth centuries and the expansion of the Mongol empire in the thirteenth and fourteenth centuries.... There is nothing surprising about this Western backwardness: the Italian cities... were at the terminus of the great commercial routes of Asia.... The upsurge of the West, which was only to emerge from its relative isolation thanks to its maritime expansion, occurred at a time when the two great civilizations of Asia [China and Islam] were threatened.

In other words, the real issue is not just whether there were *other* world system ducks earlier and elsewhere that had the same one, three, six, or twelve characteristics as Wallerstein's world system duck. Nor is the issue one of transition *between one and the other* such ducks or systems. The real questions are whether there really was a transition to the birth of *this* world system around 1500, or whether the real historical development of *this* same ugly world system duckling reaches further back in time, and whether this system and the motive forces for its "transitions" were based in Europe or *elsewhere in the wider world*.

I believe that what Jacques Hamel and Mohammed Sfia (1990) call a "continuist" perspective is appropriate in answer to these questions. Such a perspective is suggested in Godelier's (1990) and others' examinations of Wallerstein in Hamel and Sfia's (1990) *Sociologie et Sociétés*. From that perspective, the historical record suggests that *this same historical world economic and interstate system is at least five thousand years old*. There was more continuity than discontinuity or even transition of this world (capitalist) economy as an historical system across the supposed divide of the world around 1500. More detailed support for this continuity is presented in Frank (1990) and Gills and Frank (1990). Moreover, therefore, if there really was a "transition to capitalism" in the sixteenth century (which is also subject to debate), it took place *not in Europe nor especially due to changes within Europe* but instead in the long preexisting world system and, importantly, due to changes in the system *outside* Europe. In other words, "to understand the problematique... [of transition *in* Europe] we must begin with the world system that creates it!"

To anticipate some academic-scientific and practical political conclusions we may well recognize the last of Wallerstein's six points about the historical system, which were cited earlier. The system may well have a life cycle, as he says, but this cycle need not, and did not, begin with any transition from feudalism around 1500, as Wallerstein claims, and it need not, and may not, end in 2050–2100 with a transition to socialism, as he

suggests. If we can identify any transitions, each is likely in reality to be a transition between a transition and a transition.

On these issues of transition and/or continuity in the world system, Wallerstein's own account is again helpful, even though—or perhaps because—its short-sighted Eurocentric perspective and internal contradictions seriously undermine his central argument and position. Thus, like Gernet, Abu-Lughod and others, Wallerstein takes note of the Mongols and the crusades, but:

The feudal system in western Europe seems quite clearly to have operated by a pattern of cycles of expansion and contraction of two lengths: circa 50 years [which seem to resemble the so-called Kondratieff cycles found in the capitalist world economy] and circa 200–300 years.... The patterns of the expansions and contractions are clearly laid out and widely accepted among those writing about the late Middle Ages and early modern times in Europe.... It is the long swing that was crucial. Thus 1050–1250+ was a time of the expansion of Europe (the Crusades, the colonizations).... The “crisis” or great contractions of 1250–1450+ included the Black Plague.... (1989b: 33, 34)

Thus, even according to Wallerstein, there was systematic cyclical continuity across his 1500 divide. Moreover, despite his comparison with China, Wallerstein omits to note that 1050–1250 was significantly also the time of the great advances in technology, accumulation, and expansion in Sung China; and that the crisis of 1250–1450 was world (system) wide, including China, as Abu-Lughod (1989) has rightly emphasized. Thus, the clearly laid-out “pattern... of expansions and contractions,” including probably that of “demand and prices” (Wallerstein, 1989b: 14) was not just (west) European, but perhaps world system wide. At the very least, their manifestation in Europe was also a function of their changing center/periphery relations (in trade and hegemony/rivalry) with other parts of the world economy. All these factors not only merit study per se, they require analysis to make any sense out of changes in Europe—or in any other part of Eurasia and Africa, that is, the systemic relations extended far beyond Europe.

Yet even Wallerstein recognizes several additional pieces of the jigsaw puzzle outside Europe. Nonetheless, he is still unable to put it together; because he remains wedded to his old *Weltanschauung*:

The collapse of the Mongols [was a] crucial non-event.... The eleventh-century economic upsurge in the West that we have discussed was matched by a new market articulation in China.... Both linked

up to a Moslem trading ecumene across the Middle East. China's commercialization reinforced this model [why not system?]. . . the Mongol link completed the picture. What disrupted this vast trading world-system was the pandemic Black Death, itself quite probably a consequence of that very trading network. It hurt everywhere, but it completely eliminated the Mongol link. (1989b: 57, 58, my emphasis)

For Wallerstein, the collapse of the Mongols was the last of "four elements in an explanation" of the rise of capitalism in the West out of "the effect of the cumulated collapses." The other three were "the collapse of the seigneurs, the collapse of the states, the collapse of the Church" (1989b: 47). There were political economic factors behind all four collapses: "Most governments became bankrupt. . . incapable of controlling their mercenaries. . . The Church was a major economic actor itself, and was hurt by the economic downturn in the same way that both seigneurs. . . and states. . . were hurt" (1989b: 47–55).

Yet Wallerstein resists and refuses to draw the logical—and historical—conclusions: to put the whole picture in the jigsaw puzzle together, we must liberate ourselves from the imaginary transition within the imaginary system confined to Europe. The solution to the puzzle of the four simultaneous and cumulative collapses and to the "crisis of feudalism in Europe" itself is to be found outside the limited and optically illusory framework of "Feudal Europe." We must look at the real transitions in the real world system and its history as a whole. The resolution of the "crisis of feudalism" involved changing relations within, and further expansion of, the whole world system.

Real World System Issues and Proposals

To understand this and subsequent transitions, therefore, we should:

1) *Abandon the schema of a "European" world (system)* and look outside. Wallerstein and so many others look out of the window from their European house; but they still cannot see its (still marginal) place in the world landscape. Why are the Mongols "the link" in a Chinese-Islamic "trading world-system" before 1500 if Wallerstein and others still refuse to accept the prior existence of this system?

2) *Look at the whole world system.* China, the Mongols, the Islamic world, and Europe, not to mention other parts of the Asiatic-Afro-European ecumene, were linked into a trading and interstate world system in the thirteenth century, à la Abu-Lughod. Should we recognize that this was the world system out of whose crisis hegemonic European capitalism

emerged? Posing the right question is more than half way to the right answer. Wallerstein provides another part of the right answer himself. Of course, however, since he refuses to pose the question, he also does not see the answer. Was the "crucial cycle" limited to Europe? Most probably not. Wallerstein himself suggests some of its extra-European elements. Indeed, all four of the political economic elements of his explanation for the rise of capitalism in Europe include extra-European elements: the Mongols most obviously so, but also the financial crises of the governments, landlords, and Church in Europe. All were related to the development of the 1250–1450 crisis outside Europe and in the world system as a whole. Similarly, the 1050–1250 expansion in Europe had also been part of a world (system)-wide expansion. *The crucial cycle was in the world system itself.*

3) *Recognize long cycles of development in this world system.* Wallerstein recognizes that "it is the long swing that is crucial: 1050–1250 upswing and 1250–1450 downswing ... and 1450–1600 long sixteenth century" (renewed) upswing, before the renewed "seventeenth century crisis." Moreover, Wallerstein recognizes that it was the "crisis" during the 1250–1450 downturn that led to "cumulative collapse" and then to regeneration and a new "genesis." However, Wallerstein and others neglect to ask—and, therefore, to find any answer to—the crucial question: crisis, collapse, new genesis in *what system?* Of course, as George Modelski (who is also incapable of seeing this system; see Modelski, 1987) correctly pointed out to my seminar in person, "in order for us to look for a cycle, we must first be clear about the *system* in which this cycle occurs." So, there are two possibilities: the same European system predates 1500, or Europe was part of a (also the same) world system that predated 1500. Either way Wallerstein's and others' temporal and Eurocentric myopia blinds them to seeing the whole picture of systemic historical reality.

4) *Consider the probability of a continuous cyclical process of development in/of the same single world system.* Of course, if there was a long cycle and it was crucial, the 1050–1250 upswing and the 1250–1400 downswing must have been the cyclical expression and development of *an already existing system.* However, in that case the 1050–1250 upswing may well have been a (re) genesis from a previous crisis/collapse/downswing, which in turn was the culmination of a previous upswing, and so on. how far back? Curiously, Wallerstein sees a single cycle, at least in Europe, but a variety of "unstable" systems around the world, each of which "seldom lasted more than 400–500 years" (1989b: 35). On the other hand, Abu-Lughod (1989) sees a single world system, certainly in the thirteenth century on which she concentrates but also in earlier periods. However, each of her world systems successively, cyclically rise (out of what?) and decline (into what?). Neither Wallerstein nor Abu-Lughod is (yet?) willing to join their insights and see both a single world system and its continuous cyclical development.

5) *Realize that hegemony in the world system did not begin in Europe after 1500, but that it shifted to Europe in the course of hegemonic crises in the East of the same world system.* Even Wallerstein quotes Abu-Lughod (1989) to the effect that “Before European Hegemony, the Fall of the East preceded the Rise of the West.” Abu-Lughod is at pains to show how and why the various parts of the East declined at this time in world systemic terms. Therefore, the root causes of *the rise of the West to hegemony and the transition to capitalism in Europe cannot be found within Europe alone*, but must be sought in the course of the development of the world system—and also within its other parts—as a whole. “If we are to understand the problematique . . . we must begin with the world system that creates it!”

6) *Pursue the origins of the world system*—and of its development in the past half millennium—as far back in time and space as the historical evidence and our ability to analyze it allow. Wallerstein (1989b: 37) writes:

Obviously, any historical occurrence has immediate roots whose derivation can be traced back, *ad infinitum*. However, if we believe that the critical turning point was 500–2,500 years earlier, we are coming up with a cultural-genetic explanation which in effect says that the development of capitalism/“modernity” in the West, and in the West first, had been rendered “inevitable” by this earlier “civilizational” system.

The first sentence is true, and so is the premise in the first half of the second. However, the conclusions in the remainder are totally unwarranted and triply false. Tracing the roots of the present world system backward in no way obliges us to come up with cultural-genetic explanations; still less with civilizational ones; and least of all with the inevitability of the present or future outcome. It is at least equally possible—and, as I argue here, preferable—to come up with a longer and wider historical systemic explanation within which earlier civilizational factors play only a partial role, and inevitability none at all. Therefore, Wallerstein’s otherwise correct rejection of causation by alternative civilizational factors and their various interpretations by others is largely beside the point.

The “explanation” is not to be sought through the civilizational roots of the rise, nor the decline, of Rome, which Wallerstein (1989b: 37–39) discusses after other authors. The same goes for his discussion (39–47) of the “hurrah” for later culture in England and Italy schools. Instead, we should seek the explanations in the development of the world system within which Rome—and its rise and decline—were only regional parts (along with Parthian Iran, Gupta India, Han China, Central Asia, and Africa) and transitional phases. The same goes for Italy and England. This holistic systematic analysis does not, of course, deny the importance of

local, national, regional, or other developments. It only places them in the systemic contexts, which also influence these developments—and are in turn influenced by them. However, the whole is more than the sum of its parts, and the problematique of no part is properly understandable in isolation from the whole of which it is but a part. Wallerstein, of course, understands this truth well—for the period since 1500. But he (still) subjectively refuses to admit it for the time before despite the evidence he himself cites, which objectively supports it. I examine much more evidence for tracing this world system back at least five thousand years and challenge as unfounded the even greater reservations of others against so doing, in Frank (1990) and Gills and Frank (1990).

7) *Discard the ineffective concept of 'proto-capitalism.'* The first supposed resolution of the feudalism-capitalism debate a quarter-century ago was to try to “compromise” on “semifeudalism” going on to become “semi-” “proto-” capitalism. I thought this “compromise” was a nonstarter then; and experience has shown that the “mode of production” debate detracted from better understanding of the world system itself. Wallerstein made his major contribution by taking this avenue himself. It is likely only to befuddle our analysis again to now argue that the essential characteristics of the modern-world-capitalist-system, quoted in 240 of the 242 words of Wallerstein’s 12-point synthesis earlier, are also “proto-capitalist” “elements,” which can be found all around the world in different times and “systems.” It is better to proceed, as Wallerstein does, with the

effort...to establish a continuous pattern of scientific/technological advance, located in many different world regions (China, India, the Near [to us] East, the Mediterranean zone), into which recent western scientific efforts have fit themselves, primarily since the sixteenth century. By underlining the continuities, this argument reduces the distinctiveness of what occurred in western Europe. Furthermore, it has been argued that, in this arena as in many others, Europe had previously been a “backward” or “marginal” zone, implying therefore that any explanation of significant change could not be accounted for exclusively or even primarily in terms of some west European affinity...or tradition. (1989b: 16)

Of course, this means that recourse to the idea of “proto-capitalism” in “different” and “earlier” systems is not at all helpful. Instead, it is much more useful to recognize that technical change and capital accumulation, as well as all other characteristics of Wallerstein’s ‘modern world-system’ also characterized earlier times and system(s). In that case indeed, “we find an uncomfortable blurring of the distinctiveness of the patterns [of

capitalism and proto-capitalism] of the medieval and modern world” (Wallerstein, 1989b: 33). What is it then that makes Wallerstein and others so “uncomfortable”? The answer is that this systemic holistic procedure threatens to pull the rug from under the very foundations of their “scientific” edifice and their fondest ideological beliefs!

8) *Liberate ourselves from the optical illusion of the false identity of ‘system’ and ‘mode of production’.* Samir Amin contends that the system could not have been the same system before 1500 because it did not have the capitalist mode of production, which developed only later. Before 1500, according to Amin and others, modes of production were tributary. My answer is that the system was the same no matter what the mode of production was. The focus on the mode of production blinds us to the more important systemic continuity. Wallerstein makes the same confusion between ‘mode’ and ‘system.’ Indeed the single *differentia specifica* of Wallerstein’s modern-world-capitalist-system is its mode of production. Wallerstein’s identification and also confusion of ‘system’ and ‘mode’ is evident throughout his works and is widely recognized by others. It is also evident in the article I am “dissecting” here, for example:

[T]he difference between capitalism as a mode of production and the multiple varieties of a redistributive or tributary mode of production is surely not, as often asserted... [in] “extra-economic coercion.” For there is considerable extra-economic coercion in our capitalist/”modern” historical system, and markets of some kind have almost always existed in other historical systems. The most we can argue is a distinction that is more subtle. (1989b: 14)

Wallerstein’s system is his mode. So it is for Amin (1989), Brenner (Aston and Philpin, 1985)—and also for their ideological opponents on the Right. It may be appropriate to note that our disagreement has generated long friendly discussions with Brenner and still permits collaboration in our second joint book on contemporary problems with Wallerstein and Amin (Amin et al., 1990). Moreover, both the latter have written responses to my historical arguments in Amin (1990) and Wallerstein (1990). Nonetheless, I maintain that if Wallerstein and Amin cede ground as to the distinctiveness of this mode, they also undermine the scaffolding for the construction of this system in 1500—to the point of the total breakdown of any argument about the *differentia specifica* and the beginning of Wallerstein’s modern-world-capitalist-system. The one, three, six, or twelve essential characteristics of the world system, and its beginning, antedate Wallerstein’s period by far.

We should *separate* our notions of system and mode. Then, we could at least recognize the existence and development of the real world system

over millennia. I believe it is high time to abandon the sacrosanct belief in the ideological formulations about these supposed different modes of production or the supposed transitions between them in such a world system. A transition is a transition between a transition as I learned in Allende's Chile.

Therefore, I agree with Godelier (1990: 35) when he says that there are various ways to be materialist. However, I do not agree with his opinion (1990: 28) that making a theory of the articulation of modes of production or the transitions among them is now a task of the greatest urgency. On the contrary, I believe that materialism, experience, and good sense urge us to abandon this quest and to seek a more fruitful approach based on the material analysis of material world system development.

9) *Therefore, also dare to abandon (the sacrosanct belief in) capitalism* as a distinct mode of production and separate system. What was the ideological reason for my own and Wallerstein's "scientific" construction of a sixteenth-century transition (from feudalism in Europe) to a modern-world-capitalist-economy and system? It was the belief in a subsequent transition from capitalism to socialism; if not immediately in the world as a whole, then at least through "socialism in one country" after another. Traditional Marxists, and many others who debated with us, were even more intent on preserving faith in the prior, but for them more recent, transition from one (feudal) mode of production to another (capitalist) one. Their political/ideological reason was that they were intent on the subsequent transition to still another and supposedly different socialist mode of production. That was (and is?) the position of Marxists, traditional and otherwise, such as Brenner (Aston and Philpin, 1985) and Anderson (1974). That is still the position of Samir Amin (1990) who, like Wallerstein, now wants to take refuge in "proto-capitalism"—and by extension "proto-socialism." (Before he was ousted after the Tiananmen massacre, Chinese Premier Zhao Ziyang came up with the idea that China is now only in the stage of "primary" socialism.) If Maurice Godelier and Samir Amin, among others, would dare to undertake a "transition" from their "scientific" categories, they could spare themselves and their readers some of the political (dis)illusions regarding recent events in the "Second" and "Third" worlds.

Transitional Scientific and Political Conclusions

Is there a scientific/historical/academic justification to meddle with "proto-capitalism" in such a supposed long transition from feudalism to capitalism—or from capitalism to proto-socialism? No, definitely not, as the internal contradictions in Wallerstein's argument amply demonstrate.

So, is there still a political/ideological reason to hold on to the fond belief in a supposed “transition from feudalism to capitalism” around 1800, or 1500, or whenever—to support the fond belief in a “transition to socialism” in 1917, or 1949, or whenever? Is there any such reason still to continue looking for this earlier transition and its hegemonic development only in Europe, while real hegemony is now shifting (no doubt through the contemporary and near-future nonhegemonic interregnum) back toward Asia? No, there is none.

Ironically, Reagan, Bush, Thatcher, Major, Mitterrand, and all the capitalists they represent are equally, or even more, infatuated with the ideology of capitalist distinctiveness, except that they glorify it. Their opponents on the Left disagree in this valuation and still want to overcome capitalism through the transition to socialism. The Right, instead, want to preserve and glorify capitalism and bask in what they see as the self-destruction of Marxism, socialism, and the Evil Empire of the others. However, their ideological faith in the supposedly universally beneficial glories of the “magic” of the market, of course, also lacks scientific foundation in reality. The world system-wide reality is the competitive dog-eat-dog war of all against all (à la Hobbes), in which only the few can win and the many must lose. And so it has been for millennia, thanks to the world system’s unequal structure and uneven process that Wallerstein helps us identify.

We would all do well to see the reality of the globe-embracing structure and the long historical development of the whole world system itself. It is better to recognize this system’s “unity in diversity,” as Mikhail Gorbachev said at the United Nations. That would really be a “transition” in thinking. This “transition” would make us better placed to choose among the diversities which are really available in that world system—*Vives ces différences!* Moreover, this change in thinking could also help us to understand the real transitions that exist and guide us in the continuing struggle for what is good among these differences and against what is socially bad.

Note

1. The following friends have made reflective comments: Christopher Chase-Dunn, Paulo Frank, Barry Gills especially, William McNeill, and Immanuel Wallerstein. However, all have reservations, especially on point nine and my conclusions, to which I have not ceded.

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CHAPTER FOUR

A Structural Theory of the 5,000-Year World System

Our thesis has been articulated in several articles and was outlined in our book, *The World System: Five Hundred Years or Five Thousand?* (Frank and Gills, 1993). Its main theoretical premises are as follows: (1) The existence and development of the world system that stretches back not just for 500 years but for 5,000 years; (2) The (political) world economy is a world system; (3) The process of capital accumulation is the motor force of (world-system) history; (4) The center-periphery structure is one of the characteristics of the world (system); (5) The world system is depicted by hegemony and rivalry of political power although system-wide hegemony has been rare or nonexistent; (6) Long economic cycles of alternating, ascending phases and descending phases underlie economic growth of the world system.

This approach addresses several disciplines or concerns and participates in longstanding controversies within and between them by exploring the connections of our thesis with historiography, civilizationism, archaeology, classicism in ancient history, medievalism, modern history, economic history, macro historical sociology, political geography, international relations, development studies, ecology, anthropology, race, ethnic and gender relations, and so on. Therefore, our thesis also has some important philosophical, social scientific, and political implications.

Main Theoretical Categories and (Operational) Definitions

The World System

Per contra Wallerstein (1974), we believe that the existence and development of the same world system in which we live stretches back 5,000 years

or more (Frank, 1990, 1991a, b; Gills and Frank, 1990/91, 1992; Frank and Gills, 1992, and especially Frank and Gills, 1993). According to Wallerstein and unlike our World System (without a hyphen), World-Systems (with a hyphen and sometimes plural) are in a "world of their own, which need not be even nearly worldwide. Of course however, the "new world" in the "Americas" was home to some world-systems of its own before its incorporation into our (preexisting) world system after 1492. However, these American ones never became dominant in the world, as did the Afro-Eurasian one; that is, therefore, the subject of our attention.

Braudel and Wallerstein stress the difference between world economy/system (without a hyphen) and world-economy/system (with a hyphen). "The world economy is an expression applied to the whole world.... A world-economy only concerns a fragment of the world, an economically autonomous section" (Braudel, 1982: 20–21). A similar "difference a hyphen makes" is stressed by Wallerstein (1991). "Immanuel Wallerstein tells us that he arrived at the theory of the world-economy while looking for the largest units of measurement which would still be coherent" (Braudel, 1982: 70).

In our view, which is shared by Wilkinson, this unit has long been much larger and older than the European-centered "world-economy/system of Braudel and Wallerstein. Wilkinson (1987, 1993) emphasizes political coherence. Therefore, he sees "Central Civilization" as only starting in 1500 BC and spreading out much slower than the economic connections that Wilkinson recognizes as being much earlier and more far-flung. We use the latter as a major criterion for the identification of the world system since at least 3000 BC and, compared to Wilkinson, also see its spread throughout Afro-Eurasia as having been more rapid (Frank and Gills 1993; Frank 1993). For that reason, Chase-Dunn and Hall (1991) usefully suggest that we should refer to a "Central World System."

The distinction between various ancient world-systems, and the one "Central World System," is important. Thus, for instance, Algaze (1993) refers to two different Bronze Age "world-systems" in what is now West Asia/the Middle East. Instead, Gills and Frank, like Wilkinson, insist that we can identify one single world system there already in the Bronze Age. We only differ with Wilkinson in that he dates its origin in 1500 BC and we date it from well over a millennium earlier. Moreover, all three argue that there has been an unbroken historical continuity between the Central Civilization/World System from the Bronze Age to our contemporary "Modern Capitalist World-System." A criterion of systemic participation in a single world system is that no part of this system would be as it is or was if other parts were not as they are or were. The interaction from one part of the system to another may be only indirectly chain-linked. A weaker systemic link would be that the various parts might also have reacted to,

and on, the same global ecological constraints. That system criterion was already proposed in Frank (1990) and in Gills and Frank (1990/91). The latter went on to explicate that surplus extraction and accumulation are "shared" or "interpenetrating" across otherwise discrete political boundaries. Thus, elites participate in each others' system of exploitation vis à vis the producing classes. This participation may be via economic exchange, political relations (e.g., tribute), or through combination of both. All of these relations characterize the millenarian relationship, for instance, between the peoples of China and Inner Asia. This interpenetrating accumulation thus creates a causal interdependence between structures of accumulation and between political entities. Therefore, the structure of each component entity of the world system is saliently affected by this interpenetration.

Despite the aforementioned emphasis on "economic" trade connections to cement the world system, Gills and Frank (1990/91) also explicitly argued that world system connections were established and maintained through recurrent "political" conflict among "societies," a point emphasized by David Wilkinson (1987). The recognition of such conflict as a mark of participation in the same world system is important for the conflict has been over economic resources and control of trade routes. Furthermore, trade in metals and/or weapons could increase military capacity and that, in turn, can enhance control over sources of economic resources, including trade itself. Moreover, political conflict (and shifting alliances and war) has also been the expression of the alternation between hegemony and rivalry within the world system and/or its regional parts.

In summary, the following are criteria of participation in the same world system: extensive and persistent trade connections; persistent or recurrent political relations with particular regions or peoples, including especially center-periphery-hinterland relations, and hegemony/rivalry relations and processes; and sharing major (and minor) economic, political, and perhaps also cultural cycles.

The identification of the geographical extent of near-simultaneity of the up and down phases of these cycles may serve as another important operational definition of the (extent) of the world system. If distant parts of Afro-Eurasia experience economic expansions and then again economic contractions nearly simultaneously, that would seem to be important *prima facie* evidence that they participate in the same world system. Like Gills and Frank (1990/91, 1992), Edens and Kohl (1993) suggest that a major criterion of participation in a single world system is near-simultaneity or synchronism of (cyclical?) expansion and contraction. This suggests

[the] action of an interrelated set of transregional social forces operative over vast regions of western Asia from the mid-third through the

mid-second millennium BC.... The existence of an ancient world system is postulated by the largely synchronous processes of rise and collapse recorded throughout this area; it is difficult to deny that one here is witnessing historically connected processes... (in) the world system. (Edens and Kohl, 1993: 25, 61)

The World Economy

We propose that a world economy has been in existence for a long period of time. We may distinguish two related issues from this proposal. One refers to the existence and significance of production for exchange, through the market, followed with capital accumulation. The other is whether these economic relations comprised a division of labor with specialization and trade coupled with competitive accumulation occurred on a large scale over long distance so as to link distant areas into a single "world" economy. Both propositions are controversial, but we believe that there is ample historical evidence to support these claims.

For evidence of the market/credit economy existing as far back as Assyria, see Mogens-Trolle Larsen (1967, 1976); Adams (1974); Silver (1985); and Rowlands, Larsen, and Kristiansen (1987). In our definition of the world system earlier, regular exchange of surplus does affect the "internal" character of each of the parts of the world system as well. Some scholars, such as Wallerstein (1991), for instance, reject our definition because they do not believe that "mere" trade makes a "system." We do. We not only believe that regular and significant trade provides sufficient ground for speaking of a "system" or of a real "world economy" (without the hyphen), but also that trade integrates "social formations" into something that should be called the "international division of labor," even in the ancient Eurasian world economy. This takes place because trade and production are not separated. The nature of trade directly affects the character of production, as the history of the early modern world system so clearly illustrates. These effects are a consequence of specialization if nothing else, and we contend that they are intimately related to the system of the regular transfer of surplus as well as to specialization. Wallerstein (1993) sets very specific criteria for the level of integration in his international division of labor that, for him at least, precludes considering the pre-1500 division of labor as being in the same formal category. We believe that he has again erected a false dichotomy, the aim of which is axiomatically to preserve the distinctiveness of the "capitalist" world-economy, which is an issue to which we return later.

A related question then is how extensive (and intensive) this division of labor and trade network was. By our aforementioned criteria, as early as the third millennium BC, the world economy/system included Egypt, Mesopotamia, the Arabian Peninsula, the Levant, Anatolia, Iran, the

Indus Valley, Transcaucasia, and parts of Central Asia—all south of the East-West mountain ranges, which transverse much of Asia. However, the analysis of E. N. Chernykh also leads to the inclusion in this world system of the region north of the mountains involving “a whole chain from the Atlantic to the Pacific: the European, Eurasian, Caucasian and Central Asian provinces, along with others outside the USSR” (Chernykh, 1992: 302). He also suggests that “the world system itself has turned out to be far more extensive than appeared earlier” (304).

We find that this world economy/system was formed in the third millennium BC or earlier and that since then it has had a continuous, albeit cyclical, development that incorporated more and more areas of the globe, and which still proceeds today. Although luxury/preciosities did play a significant role in these ancient “external” trade and “internal” political relations—and, therefore, also must do so in our theory and analysis—at that time there were also significant amounts of economically vital trade in bulky necessities: metals, timber, grain, animals and other raw materials and foodstuffs, and textiles and ceramics. For instance, southern Mesopotamia lacked metals and timber and was dependent on their import from Anatolia and the Levant, while it exported grains and textiles.

Capital Accumulation

We regard the process of accumulation as the motor force of (world system) history. Wallerstein and others regard continuous capital accumulation as the *differentia specifica* of the “modern world-system.” We have argued elsewhere that in this regard the “modern” world system is not so different and that this same process of capital accumulation has played a, if not the, central role in the world system for several millennia (Frank, 1991b and Gills and Frank, 1990/91). Amin (1991) and Wallerstein (1991) disagree. They argue that previous world-systems were what Amin and Wolf (1982) call “tributary” or Wallerstein “world empires.” In these, Amin claims that politics and ideology were in command, not the economic law of value in the accumulation of capital. Wallerstein seems to agree.

It is particularly important to clarify our controversial suggestion that “ceaseless accumulation,” which according to Wallerstein is the *differentia specifica* of capitalism, is a feature of the world system throughout its development and is not unique to the modern period. Though there can be no real doubt that industrialization of production played a crucial role in bringing about a quantitative change in the rate of “ceaseless accumulation” in the modern period, in our view this change is essentially a matter of degree. Indeed, Wallerstein himself says, that the difference between so-called proto-capitalism and supposedly full-blown “capitalism” is really a matter of degree. This debate turns on the definition of “ceaseless,”

since Wallerstein also notes the existence and indeed even perhaps the prevalence of capital before the “modern” period. In our view, following Marx (up to a point), “ceaseless” accumulation implies that capital is constantly reinvested into the circuits of production in order to sustain capital accumulation. This ceaselessness is imperative, especially given the facts of competition. The historical evidence suggests to us (Gills and Frank, 1990/91, 1992) that capital accumulation has normally been “competitive” and has involved a continuous reinvestment in the means of production, and indeed in a whole social and political ensemble of sectors, including infrastructure. This investment process is carried out both by private capital and by the state, which is of course essentially the case even today in most modern economies. Then, as now, states lived partly on a “rent” from this international commerce—through direct taxation on trade; partly from “profits” accumulated by their “national” merchants, manufacturers, and money-men; and partly from taxing the national product or income of the general population. Imperialism has provided an additional source of revenue to powerful states throughout history, which often takes the form of “tribute” proper, that is, either extortion or loot acquired through conquest. Indeed the logic of conquest often followed the logic of the trade routes and the sources of materials, and especially precious metal means of payment for them, which were central in that trade (Gills and Frank, 1990/91, also in Frank and Gills, 1993).

There has been a fundamental misconception of the character of the “premodern” economy, particularly of Eurasia, based on the mistaken generalization of the “command economy” or as Anderson (1974) would have it, of the role of “coercion” and determination by the “political instance” rather than by “economics”. In our view, what Samir Amin (1991, 1993) and Eric Wolf (1982) call the “tributary mode” is, more often than not, merely “taxation” by another name. The fact that all historical states have lived by some form of taxation is hardly a revelation to anyone. However, it is not necessarily incompatible with the idea that more often than not, these premodern states coexisted with a vibrant commercial sector in the economy, primarily directed by private merchants and bankers and conducted on a vast international scale. The sheer volume of evidence from each “specialist” history of the various “parts” of Eurasia corroborates the contention of the “centrality” of this world economic commerce again and again (For the earlier period see, e.g., Adams, 1974; Ekholm and Friedman, 1982; Frank, 1993, and for early modern times Abu-Lughod, 1989).

The Center-Periphery Structure

This structure is familiar to analysts of dependence in the “modern” world system and especially in Latin America since 1492. It includes

but is not limited to the transfer of surplus between zones of the world system. However, we now find that this analytical category is also applicable to the early periods of the world system. The structure of this world system does not conform to the "unipolar" model of center-periphery relations, common in most approaches using this concept. We see more "multipolar" center-periphery relations on a world scale. Therefore, the world system is not viewed as having always been composed of a single core and single periphery, but rather of an interlinked set of center-periphery complexes (and also including "hinterland" as discussed in Gills and Frank, 1990/91), joined together in an overall ensemble. Thus, the world system, first in Eurasia before 1500 and globally after 1500, has always been multicentric in structure (Gills, 1994; Frank, 1994). This includes even the period of supposed unipolar European or Western global hegemony in the modern world system. This approach to structuralist analysis allows greater flexibility, since distinct regional, imperial, or market-mediated center-periphery complexes are accepted and yet are all seen as part of a single whole with systemic links to one another.

Yet this multicentricity does not mean "equality" among the various centers or between different center-periphery complexes in the world system. This multicentricity is hierarchically structured. There is a very complex "chain" of "metropole-satellite" relations of extraction and transfer of surplus throughout the whole world system, such as discussed in Frank (1967).

Hegemony-Rivalry

Hegemony is defined as a hierarchical structure of accumulation between classes and states, mediated by force (Gills and Frank, 1990/91, 1992). In this sense, the center-periphery structure of the world system is simultaneously an economic hierarchy as well as a political hierarchy, as hegemony embodies both.

In a supposed alternation between hegemony and rivalry, regional hegemones and rivalries succeed the previous period of hegemony. World system and international relations literature has recently produced many good analyses of alternation between hegemonic leadership and rivalry for hegemony in the world system since 1492, for instance by Wallerstein (1984), or since 1494 by Modelski (1987) and by Modelski and Thompson (1988). We find that hegemony and rivalry also mark world (system) history long before that (Gills and Frank, 1992).

However just as the world economy/system never entirely "falls" but only changes, hegemonic ascent and descent are usually quite gradual and do not occur in a unipolar framework, but rather in a multipolar one. This

world historical process “favours some at a particular time while discriminating against others, and so on through time” (Gills, 1993: 121). Indeed, it is integral to our structural theory of world development, though not unique to only our position of course, that areas that were once “peripheral” may ascend to hegemonic or core status, while areas that have once been core may descend into the periphery. We particularly emphasize how economic rhythms common to the entire world economy/system, such as long cycles of expansion and contraction, affect the relative position of all of the “parts” of the system (Gills and Frank, 1992). The schema of the structure of the world system should perhaps be akin to a “truncated pyramid,” at the “apex” of which there is not usually one sole hegemonic center of political power and capital accumulation, but rather several coexisting and interactive centers. Thus, if one descends from this truncated apex, there is not necessarily a vacuum to be automatically filled by an entirely new ascending center.

Our position is distinguished by the argument that these ascents and declines occur within the same world economy/system. Therefore, we also have serious reservations about received theories of hegemony, such as Modelski and Thompson’s “political” leadership or Wallerstein’s “economic” hegemony. Our reservations rest principally on two bases: the extent of the world system and the essence of hegemony within it. To begin with, the claims that Portugal, the Netherlands, England, and the United States have successively been hegemonic is based on their hegemony in an essentially European/Western-based and centered “world-system.” If we recognize that in the sixteenth to eighteenth centuries the world economy/system was much larger than the “European world-economy/system,” then the claim to hegemony of little Venice, Portugal, and the Netherlands within the whole Afro-Eurasian and American world economy immediately becomes doubtful. All of these economies and their participation in the world were too small in scale to exercise any kind of “hegemony” in the world economy/system. Moreover, they certainly were not the centers of world economic accumulation. By comparison and instead, Ming/Qing China and Moghul India, as well as Ottoman Turkey, and perhaps Safavid Persia political(ly)/economic(ally) far outranked any of the individual West European economies and states, and probably all the European ones added together.

Furthermore, we have previously argued that “hegemony” is a feature of the world economy/system itself, more than of any of its parts, and that it should be defined and measured primarily by the centralization of world economic accumulation in a particular part of the world system (Gills and Frank, 1991, also in Frank and Gills, 1993). By that criterion also, the small European city-state and even national economies were in no sense hegemonic. On the contrary, their very economic success was entirely

derived from their subsidiary participation in an Asian-based world economy in which, as we have seen, accumulation was centralized in India and China. Indeed, the Europeans were able to participate in this world economy at all only by "virtue" of the golden and silver means of payment that they plundered from the Americas, a substantial portion of which they transhipped to the economies of West, South, and East Asia, where the real accumulation took place on the basis of their respective manufacturing superiority and competitiveness.

Therefore, we are led to conclude that not only throughout world-system history but even during the modern period, world economic/systemic hegemony is rare if not nonexistent; incomplete and transitory; and that hegemony tends itself to generate the conditions and competition, which soon undermine one hegemony and replace it with rivalry and an alternative hegemony. The norm of the situation we have called "interlinked hegemonies" (Frank, 1994a).

This is how one arrives at the formulation that global or world hegemony is always shared hegemony, exercised through a complex network composed of class coalitions, alliances, and other forms of association between states, including competitive ones. Furthermore, the world system is characterized by a number of coexisting core powers (or interlinked hegemonic powers) that become increasingly integrated via both conflictual and cooperative relations.

Long (and Short) Economic Cycles

We have already noted the apparent existence of alternating ascending (sometimes denominated "A") phases of economic and political expansion and descending (sometimes denominated "B") phases of political economic crises. In the real world historical process and in its analysis by students of the "modern" world system, these long cycles are also associated with each of the previous categories. That is, an important characteristic of the "modern" world system is that the process of capital accumulation, center-periphery position, world system hegemony and rivalry are all cyclical and occur in tandem with each other. We now find that this same world system cycle and its features also extend back many centuries before 1492.

We now believe that we can identify a cyclical pattern of long ascending (A) and descending (B) phases in the same world system back at least through the Bronze Age third millennium BC. We have already noted that a most revealing operational criterion of the extent of the world system is the participation or not in the approximate 500-year-long economic cycle and the interregional near-synchronization of its approximate 250-year-long (A) and (B) phases. Of course, as we will observe later,

world economic synchronization of shorter cycles and their phases, and particularly crises, are even more revealing.

Our suggested dating of the up (A) and down (B) phases for the entire Bronze Age world system is BC: A: 3000–28/2700, B: 2700–26/2500, A: 2600–2400, B: 24/2300–2000, A: 2000–18/1750, B: 18/1750–16/1500, A: 16/1500–1200, B: 1200–1000, which was the Bronze “Dark Age” Crisis (Frank, 1993). Tentative Iron Age dates are as follows: A: 1000–800?, B: 800–550?, A: 600/550–450/400?, B: 450–350?, A: 350–250/200?, B: 250/200–100/50, A: 200/100 BC–200 AD. B: 150\200–500 AD, A: 500–750\800 AD, B: 750\800–1000\1050 AD, A: 1000\1050–1250\1300 AD, B: 1250\1300–1450 AD, A: 1450–1600 AD.

The dating of periods during the Bronze Age first millennium BC by Andrew and Susan Sherratt (1991) coincided almost exactly with our dating of the up and down phases. Kristian Kristiansen’s work (1993a, 1998) also has a similar dating of expansions and contractions in Europe during the first millennium BC, as does Klav Randsborg’s (1991) for the first millennium AD. Chase-Dunn and Willard’s (1993) analysis, again using Chandler’s data, of city-size growth and decline lend less corroboration to our precise datings; but as already observed, they do confirm the simultaneity of cycle phases between East and West Asia since mid-first millennium BC.

Of course, we should not expect to find complete synchronization nor simultaneity of A and B phases across the entire world system, and still less in its Bronze Age beginnings. It seems enough to be able to demonstrate or even suggest “substantial” synchronization of economic expansion or contraction over very wide areas, which are usually considered to be quite independent of each other. Moreover, other world systemic cyclical characteristics complicate the pattern.

Expansions and contractions seem to begin in one part of the world system, usually in its center core, and then tend to diffuse to other parts, including toward core competitors and the periphery. Dales (1976) observed and Frank (1993) pursued an apparent eastward displacement of cycle phases through West, Central, and South Asia in the third millennium BC. Today, cyclical expansion, and especially contraction, begins in the United States and spreads out from there. Therefore, cyclical decline also tends to spell the relative or even absolute decline of the principal core power.

This decline crisis involves danger and opportunity. It offers opportunities to some rivals, or often even to some peripheral part of the system. Some of them advance both absolutely and relatively, perhaps to even replace the previous central core. Today, we witness this process in Japan and the East Asian NICs relative to the United States.

Another related major concern is the “shorter” more or less fifty-year-long “Kondratieff” type cycles and how they fit into our long(er) cycle, if at

all. How far back these Kondratieff cycles go is still in dispute. Kondratieff, and most researchers who have followed him and Schumpeter (1939), studied these cycles only from an A phase beginning in 1790. Frank (1978a, b) and Goldstein (1988) also identified K cycles back into the seventeenth century, and Modelski and Thompson (1988) have now identified 19 “K waves” beginning in 930 AD. But can any of these cycles be said to have been world economy/system wide? Modelski and Thompson have said so. We certainly do not and would have to find evidence for K waves that include large parts of the still-dominant Asia.

However, Modelski and Thompson also recognize that these shorter “long” cycles probably nested in still longer “long” political economic cycles, which (same or different ones?) we trace back to 3000 BC. Of course, these much longer “long” cycles may also contain other shorter cycles, including perhaps cycles of Kondratieff type duration. Indeed, C. J. Going (1992) has argued that it is possible to identify “Kondratieff type” long cycles in Roman times. Mark Metzler (1994) claims to find them in Japan and maybe in China.

Implications for and Application to Recent World Economic History

Thus, long before the birth of the putative “European world-economy” and still long after its advent, the real world economy had a far-flung division of labor and intricate trade system, which was preponderantly Asian. This also means of course that, as Abu-Lughod (1989) persuasively argues, the city-centered interlinked regions of Asia were dominant in the world economy before European hegemony. However, this Asian dominance was not limited to her “thirteenth century world system.” It also preceded and continued long after that in a world economy, which Europe did not significantly (re)join until then, and in which (Western) Europe did not achieve dominance or hegemony until the nineteenth century. (Instead, as Janet Abu-Lughod [1989: 338] insists “of crucial importance is the fact that the fall of the east precedes the rise of the west.” Even that is true only if we date the rise of the west after the closing date of Braudel’s book in 1800.) Moreover, it signifies that all these “world-economies” in the “West” and “East” were only parts of a single age-old world economy/system, within which this change took place, like all else, only temporarily!

In reality, during the period 1450–1750, sometimes regarded as the period of “primitive accumulation” leading to full capitalism, the world system was still very predominantly under Asian hegemonic influences. The Chinese Ming/Qing, Turkish Ottoman, Indian Moghul, and Persian

Safavid Empires were economically and politically very powerful and only waned vis à vis the Europeans toward the end of this period and thereafter. Therefore, if anything, the modern world economic system was under Asian hegemony, not European. Likewise, much of the real dynamism of the world economy and its primary centers of production and capital accumulation also still lay in Asia throughout this period, not in Europe.

The most important European impact was the injection of new supplies of American bullion—and thereby themselves—into the already well-established Eurasian economy (Blaut, 1992; Frank, 1993). The Europeans did not in any sense “create” either the world economic system itself nor did they create “capitalism.” What the injection of new liquidity into the world economy actually seems to have done was to make important, though also limited, changes in financial flows, trade and production patterns within the world economy, and to permit the Europeans to participate more actively in the same. They specialized in exploiting global differences in resources, production, and prices to maximize their profits as middlemen, and where convenient they used military force and naval forts to enforce their own participation in this exchange. However, Europe itself was not a first-rank power nor an economic core region during these three centuries. The core regions, especially of industrial production, were in China and India. West Asia and Southeast Asia also remained economically more important than Europe. We will try to present estimates of GNP or something like that by major regions before 1800. Braudel uses estimates by Bairoch according to which the Asian economy was still five times larger than the European-American one in 1750.

The introduction of American silver (and to a lesser extent gold) and with it of Europeans into this Afro-Eurasian economy only increased and accelerated quantitative economic growth in an otherwise qualitatively ongoing system. The major producer/exporters of silver bullion were Latin America and Japan, and of gold were Latin America, Southeast Asia, and Africa. Both West and South/East Africa had been a, or the, major source of gold for centuries, but parts of Africa also exported slaves westward and eastward. The other regions were importers of precious metals and copper for their own monetary, coinage, and hoarding use—or for reexport to cover their own deficits.

The major importer and reexporter of both silver and gold bullion was Western and Southern Europe, to cover its own perpetual massive structural deficit with all other regions—except (perhaps) with the Americas and Africa, although the Europeans received African and especially American bullion without giving much in return.

India had a massive surplus with Europe and some with West Asia, based mostly on its more efficient low-cost cotton textile production and export. These went westward to Africa, West Asia, Europe, and from

there on across the Atlantic to the Caribbean and the Americas. In return, India received massive amounts of silver and some gold from the West, directly around the Cape or via West Asia. India also exported cotton textiles to and imported spices from Southeast Asia, and also, via the same, exchanged cotton textiles for silk and porcelain and other ceramics from China.

China had a surplus with everybody (was a “super-accumulator”?), based on its unrivalled manufacturing production and export of silks and porcelain and other ceramics. Therefore, China, which like India had a perpetual silver shortage, was the major net importer of silver and met much of its need for coinage out of imports of American silver, which arrived via Europe, West Asia, India, Southeast Asia, and with the Manila galleons directly from Acapulco. China also received massive amounts of silver and copper from Japan and some through the overland caravan trade across Central Asia.

The complexity of the international division of labor and the network of world trade was of course vastly greater than this simplified summary. However, even this mere summary statement should suffice to indicate that and how all of these world regions were integral parts of a single world economic system between about 1400 and 1800 AD.

Continuing the earlier argument, the changes in the world economic system after the injections of American (and Japanese) bullion were not simply due to Europe, nor were they primarily a diffusion of changes occurring within Europe. Instead, the injection of American bullion (overwhelmingly silver) provided new liquidity and credit formation that facilitated an important, perhaps dramatic, increase in worldwide production, which rose to meet the new monetary demand. This “pull” factor, therefore, encouraged further industrial success and development in China, India, Southeast Asia, and West Asia (including Persia). Even so, the Europeans were able to sell very few manufactures to the East, and instead profited substantially from inserting themselves into the “country trade” within the Asian economy itself.

Conflict and Collaboration with Other World System Theorists

In summary and comparison with some of our colleagues, we find that the principal systemic features of the “modern world system” can also be identified earlier than 1500. Wallerstein (1974, 1984, Modelski (1987), and Amin (1991) argue that the *differentia specifica* of our world system are new since 1500 and essentially different from previous times and places. However, Modelski (1987) includes some leadership before 1500 in his analysis, and

Modelski and Thompson (1988) now trace eighteen Kondratieff cycles back to 930 AD. Christopher Chase-Dunn and Hall (1997) and others find parallels in "other" and prior world systems. Wilkinson (1989) discovers at least some of these features also in his "Central Civilization" and elsewhere. However, he sees historical continuity, but no world system. Abu-Lughod (1989) sees a "thirteenth century world system," but she regards it as different from the world system since 1500 or before 1250. Moreover, she is not so interested in comparing systemic features or characteristics. We combine all of the aforementioned into an analysis, or at least an identification, of the principal features of this world system over several thousand years of its history and development (Frank, 1990, 1991a, b; Gills and Frank, 1990/91, 1992).

The debate between 500 and 5,000 years of world system history is really about how to write a world (system) history. This debate is primarily about continuity versus discontinuity in world history. There are two main positions in this debate. One position is that political/ideological determination of the mode of production or social formation in world history before about 1500 AD, and of ceaseless capital accumulation and economic determination (through the "law of value") at least in the modern capitalist world-system thereafter, makes for a sharp break or discontinuity between the pre-1500 and post-1500 periods. This first position is dominant among most historians and other students of world history; and among world-system theorists it is shared by Wallerstein and his followers, including Amin, who at least therein represent the probably nearly universally accepted received wisdom on this matter.

The real debate/disagreement revolves around the question of what structures constitute a "system" or a "world(-)system" in particular. We contend that a hierarchy of center-periphery (and hinterland) complexes within the world system, in which surplus is being transferred between zones of the hierarchy, necessarily implies the existence of some form of an "international" (though this is not the best term) division of labor. In our view, Amin and Wallerstein continue in the footsteps of Polanyi and Finley and underestimate the importance of capital accumulation via trade and the market in the ancient world system. Therefore, Amin and Wallerstein do not see participation in the system the same way we do and look for the "incorporation" of peoples and their societies and economies into the world-system long after we see them as having been part and parcel of the historical development of the world system.

The other position is that capital accumulation did not begin or become "ceaseless" only after 1500 AD, but has been the motor force of the historical process throughout world-system history. Therefore, there was no such sharp break between different "world-systems" or even "modes of production" around 1500. Thus we believe that the modes of production are not the key to understanding the "transitions" in the history of world

development (Frank, 1991a; Frank and Gills, 1993). Chase-Dunn and Hall (1997) are also critical of these mode-of-production categories, yet they still maintain that 1500 represents a sharp break with the past. We believe that the continuity and developmental dynamic of the world system as a whole is far more important. Furthermore, real "transitions" seem to be more a consequence of larger competitive patterns in the world system than of changes in modes of production. Above all, real transitions seem to be a matter of the role and position a particular entity fills in the world accumulation process.

From this perspective, "hegemonic transitions" have occurred throughout world history and entail not only a shift in the locus of the "concentration" of capital accumulation, but necessarily entail profound changes in social, political, economic, cultural, and ideological aspects of the world system. These phenomena should be analyzed together, as aspects of one overall historical process of change. In this sense, the concept of the "hegemonic transition" could not only be an alternative to that of "modes of production" analysis, but could become a central concept of the analysis of all world history. All of the world system/s approaches have some commonalities, differences, limitations, and offer possibilities for mutual cooperation and extension. We shall concentrate later on some of the limitations of our own approach and how they may be overcome in cooperation with our colleagues, in particular Wilkinson, Chase-Dunn and Hall, and Modelski and Thompson, and their respective approaches.

We are gratified that our continuous 5,000-year world-system scheme of things is gaining increasing acceptance from Wilkinson (1993), that Modelski and Thompson are pushing their own empirical work backward beyond the 1500 AD—their previous 1494—divide, so far to 930 AD and that Chase-Dunn and Hall (1994a, b) are moving in our direction—and we in theirs! They (1997) refer to "the general idea of a single Afro-Eurasian world-system with nearly synchronous phases of growth and decline." They ask whether that is correct; answer that they hesitate to so conclude; but end up with "what are the alternatives?" The only one they offer is that an East Asian world-system may have developed independently of the West Asian one, but that interaction between them—and of both of them on Central Asia—created a dynamic which then affected both simultaneously, at least since the middle of the first millennium BC. Frank (1991a), however, looked into the "Centrality of Central Asia" in this very world system structure and process. Chase-Dunn and Hall agree that climatic changes need further study in this connection. Chase-Dunn and Hall suggest that our 5,000-year world system perspective can also "be used to tease out the real structural and processual differences as well as the similarities across time and across different systems."

As Chase-Dunn and Hall (1997: 258) observe, all of these and other world system approaches “share an emphasis on the interaction of societies as a major source of social change... within societies.” In so doing, they also call into question the very concept and identification of a “society.” What distinguishes one “society” from another and where does one stop and the other begin? Another commonality, which is, however, less common and less extensive, is that these scholars and we increasingly try to extend our studies of the world system and its origins farther and farther back through history and prehistory. Of course, this procedure also conjures up the question of how alike or different the early world system was from the modern and contemporary one. The “continuationists,” such as Wilkinson and ourselves and apparently increasingly Chase-Dunn and Hall (who like we, eschew modes of production and prefer modes of accumulation) and Modelski and Thompson, emphasize the commonalities; and the “transformationists,” especially Wallerstein and Amin, focus on, or only see, the differences, and especially the “mode of production.” Yet what both lack most is a systematic theory of social or historical evolution. In our case, if as Gills put it, it is the same system but it is not the same, then what has made the same system change to become different? We do not have many answers to that question, except the very general—albeit we think important—one that the unequal social (including center-periphery) structure and uneven temporal (cyclical) process of the world system themselves generate change within it and thus its transformation. These days it is increasingly fashionable at least among the more materially—but not for that reason necessarily wrongly—inclined to look at ecology, demography, and technology as major factors in the generation of the social/ historical “evolutionary” dynamic. Our own work has, however, given these factors too short shrift; and we could benefit from technological propositions of others, including Chase-Dunn and Hall (1997) and ecological ones, including those of Chew (1994), and demographic ones, including Goldstone (1991).

Chase-Dunn and Hall (1994b: 6) also observe that “all world-systems pulsate in the sense that the spatial scale of integration, especially by trade, gets larger and then smaller again” and that “all systems experience the rise and fall of hierarchies” (Chase-Dunn and Hall, 1994a: 272). We agree and have found large regions that seem to “drop out” of the world system for long periods of time (India apparently from nearly 1900 to 900 BC and Western Europe significantly from 500 to 1000 AD), in that we do not find evidence of their continued participation in especially the system’s cyclical upswings. However, if a region or a people was an integral part of the world system and was marginalized during a major world economic/political crisis as the two aforementioned ones were, then we should not regard that region as being not a part of the world system

during the time when it is not or less active within it; for paradoxically it was the very participation in the world system that generated the non- or reduced participation. This is a process that we can easily observe happening before our eyes in many parts of the world, and particularly Africa, today. Therefore also, the extent of the world system cannot be interpreted in terms of the amount or degree of interaction within it at any one particular time; since the cyclical rhythm or pulsation of the system itself generates greater or lesser scales of integration, especially by trade. A related issue is that of "internal" vs. "external" influence, causation, or determination. Weberians and Marxists privilege "internal" ones. As observed earlier, world-system theory also stresses influences that are "external" to the particular "society" or "economy" in question; except, of course, that they are internal to the world system, which is of course why we regard the world system so important.

However, world systemic influences and effects may also be more important at some (world system) times than at others: expansive cyclical A phases in the world economy/system, like the rising tide, can raise all, or at least most, individual "societal" political economic boats, as it also strengthens and increases the economic relations among them. The onset of a receding tide B phase crisis also affects all or most boats. However, one of the effects of a B phase is the breakdown of these closer economic relations and a turning-inward-on-itself involution of some or even many "societal" and political parts of the world economy/system, which makes "internal" processes then seem more preponderant, as in "feudal" society. In particular, domestic political processes can become more "determinant" in periods of political economic crisis. If a region or people/s is substantially marginalized from the world system, as in the aforementioned cases of India and Western Europe, their "internal" political processes would seem, *a fortiori*, to be absolutely and relatively even more preponderant. The falling economic tide or shrinking pie also tends to generate fiercer political disputes.

This understanding of world system cyclical expansion and "contraction" as well as commonalities/differences could also help bridge the differences between our larger (central) world system and Wilkinson's smaller Central Civilization, while at the same time allowing us to benefit from his detailed recording and analysis of the rise and decline of polities within the same. Wilkinson (1987, 1993: 235, 240, 241) concurs in the importance of Central Asia; he finds that "civilizations follow oikumenes and 'the flag follows trade' and not the reverse." He also stresses that no endogenous crisis has ever made the central world economy itself collapse, and regards our apparent differences as "not in principle irresolvable." In the meantime, we try to abide by the archeologists' maxim that the absence of evidence is no evidence of absence!

Thus, one further avenue of research, following with David Wilkinson and others, is to clarify how, when, and why each region of Eurasia became integrated into the Eurasian world economy/system. In addition, it is necessary to investigate cases where there may have been "secessions" or "drop outs" from the world system. There is also the even more complex subject of refining the "calibration" of the overall world systemic cycles across all of the regions. The clearest working hypothesis seems to be that world systemic cycles are probably more "sequential" than "simultaneous," though there is also a causal link in the sequentialization. For instance, following Dales (1976), Frank found a sequential eastward shift through West and South Asia of the Bronze Age world system cycle in the third millennium BC. In this regard, we should clarify the "unevenness" even of crisis periods, that is, that even in a general world economic crisis all core areas are neither equally affected nor are all peripheral areas.

Most importantly, the world system approach must be extended by research into the causality of the cycles, both the economic and the hegemonic, and their mutual relations. In this regard and even if they may not be causative, the intervention of climatic, ecological, and demographic change, and their relations with each other and in turn with social structural ones, have received far less attention than they surely merit. This problematique also invites further research into how local conditions interact with systemic-level impulses and stimuli. Specifically, there should also be further research into how local responses affect ascent and decline in the "interlinked hegemonies" hierarchy. Gills' working hypothesis is that "mercantilist" types of "policy" normally accompany a bid for ascent and that "openness" often accompanies already established core/hegemonic status. This hypothesis is general and is intended to refer to the entire development of the world economy/system, and not merely to the modern period, where such a general hypothesis is fairly well supported and widely held by colleagues in the field.

Modelski and Thompson's (1988) temporal and spatial expansion of their empirically grounded cyclical and theoretically sophisticated purview overlaps with ours in several respects and offers opportunities for mutual enrichment and cooperation—as well as criticism. They now also refer to 5,000 years of world history, but refer only to stages of its "evolution" before 1000 AD and do not carry their cyclical analysis farther back. We do; and perhaps they could join us, or use some of our findings in their own work, and then let us benefit from their sophistication to improve our own work. They already offer an analysis of Kondratieff cycles centering in China and the Mongol Empire from 930 to 1350 AD and from then onward in Egypt and Venice until 1500.

Despite our welcome to the spatially widened and temporally deepened scope of Modelski and Thompson's work we also have some serious

reservations, which are grounded in our perception of both theoretical contradictions and corresponding empirical limitations in their work so far. The essential theoretical contradiction is that they now intend and claim to analyze the world system/economy, but in fact do not, but remain essentially Eurocentric! Their very own words as well as their procedure betray their basic working assumptions. We could grant them that the "lead economies are the sparkplugs of the world economy" (100, emphasis in original) and that, as McNeill (1983) already claimed, the lead economy in the eleventh and twelfth centuries was in China. But in that case, when begins "the history of European expansion as the core of the world economy" (102)? In their schema, the shift occurs already in the fifth Kondratieff beginning in 1190, which they see as centered on the Champagne fairs, the sixth on Black Sea trade, the seventh on Venice after 1300, from 1350 on the pepper trade, from 1430 on Guinea gold, from 1494 on Indian spices, 1540 on the Baltic and Atlantic trade, and 1580 on Asian trade. However, although the Black Sea, Guinea, India, and Asia may sound not quite European, it is clear that crucial for Modelski and Thompson is only European trade in these regions. "The principal structural change experienced by the global economy in the fifteenth to eighteenth centuries was the construction of an oceanic trading system... (and) innovations in long-distance trade after 1500... centered around the pioneering of new trading routes... (in) new phases of European imperialism" (101, 104). Yes, indeed—for Europe, but only for Europe and its new American colonies! For Asians, as we noted earlier, these same trade routes were age old. It is empirically quite incorrect, and only a Eurocentric perspective, to claim that an oceanic trading system was constructed—much less that it represented any principal structural change—only from the fifteenth to eighteenth centuries, just as it is incorrect to claim that "Indian textiles became important about the same time as American plantation crops" (117). Perhaps they did so for Europeans, but in Asia the importance of its own textile production and trade was much earlier, as even Palat and Wallerstein (1990) recognize, and remained much greater in the world economy. The same goes for the relatively much less significant "Portugal's innovative economic activities... West African gold and Asian pepper imports" (108) into Europe, but not on the scale of the (Asian-based) world economy/system. So just how, and through what cause or at least mechanism, does the Modelski-Thompson center of gravity in the world economy shift from Sung China westward allegedly all the way over to little (one million population) Portugal, bypassing virtually everything and everybody in between? How was this possible? Simply in that another place gets a new technology, and that is it—not to mention what that new Portuguese and then Dutch technology was? We need more explanation of this crucial process of transition, if it took place at all, which we deny.

Modelski and Thompson's analysis of innovation and leadership is also not unequivocal. Their definition of "leading sector" seems inconsistent. Sometimes it seems to be essentially a "most profitable" sector, for example, gold, while at other times and places it was the "most innovative" sector, for example, a new industry or technology. Yet the authors seem to use these interchangeably. Sugar was surely one of the most profitable "sectors" but not necessarily the most "innovative technology." Moreover, the Modelski-Thompson identification of innovation and leadership is compromised by their Eurocentric nearsightedness. However, this is not a "mere fact," but rather a strictly Eurocentric assumption. Where is the evidence for such a shift? Even Modelski and Thompson contradict this claim when they observe correctly that Genovese wealth and power was derived from plugging into the Asian trade at the Black Sea thanks to Pax Mongolica (238, 248) and that "for another two-three centuries, until and including the time of Columbus... the Chinese market still served as the magnet for world trade" (217). We observed earlier that this remained true in global world economic/system terms and through to the end of the eighteenth century. The Indian textile and the Chinese ceramics industries and their technological sophistication, as well as their respective imperial polities, and world economic accumulation remained far more important than anything in Genoa and Venice, Portugal and Spain, or later even in the Netherlands and Britain. Their "leadership" in Modelski-Thompson terms or "hegemony" in Wallerstein terms were limited to the European "world-economy/system" and by no means extended to the whole world economy/system. So, this Eurocentric limitation of the Modelski-Thompson perspective also casts a long shadow of doubt on the validity and usefulness of their otherwise so fruitful analysis of the shifting loci of economic innovation and political leadership in the world economy/system. We invite Modelski and Thompson to continue bringing their analytic sophistication and empirical knowledge to bear on economic and political cycles in the whole real system/system, and to carry it back as far as the historical evidence permits, perhaps well beyond the 1000 AD date that they now view as the beginning of the "global economy process"—and we would be honored and delighted to be permitted to join them in such a common enterprise.

Chase-Dunn and Hall valiantly come out for comparative analysis, which is exactly why they insist on studying world-systems. Indeed, they are so anxious to do comparative work that they categorize not only all or parts of Eurasia, but also the Wintu Indians in California or "indigenous" Hawaii as "world-systems." We agree that the more comparison we can manage, the better; but we prefer to use the term "world system" without a hyphen and to reserve it for as much of Afro-Eurasia and later the "New World" as can legitimately be viewed as sufficiently interconnected to have been parts of a single world system.

Supposedly, Chase-Dunn and Hall are “splitters,” whereas we are “lumpers,” to use their own preferred language on this matter. The point of being a splitter, *à la* Chase-Dunn and Hall’s project, is precisely to undertake comparative analysis, where the units of analysis being compared are “world-systems,” including even the putative “mini-systems.” This worthy and potentially fruitful project could generate useful abstractions about similar (and different) large-scale, long-term processes of social change and especially about the transformational logic in world system evolution, particularly if the comparisons were among long-lasting large-scale historical world systems, for example, in Mesoamerica, the Andes, and Afro-Eurasia. That is where there is much room for collaboration—and comparison—with Wilkinson and us. However, the Chase-Dunn and Hall, like Marx and still Wolf and Amin, reliance on the concept of a “tributary mode of production” or even of accumulation makes any such structural, let alone transformational, comparisons problematic, especially if the same “mode” was supposed to have been qualitatively unchanged all around the world for over 4,000 years. Moreover, their further insistence to compare these large world-systems with mini-systems such as the Wintu and Hawaii strikes us as comparing more disparate entities than apples and oranges, which are at least fruits of comparable size.

We see three further possible problems or dangers. First is the vast amount of historical data that must first be gathered and analyzed before meaningful comparisons become possible. Second is the temptation to simplify the processes too much, particularly if this takes the form of some kind of economic reductionism. Finally, there is the danger in the Chase-Dunn and Hall model to emphasize evolutionism too much to the detriment of other types of change, for example, conjunctural, retrogression, crisis, and so on. In so doing, they also risk losing the parsimonious elegance and the comparative potential of their original project by going too far in the direction of “lumping” and away from their commitment to “splitting” and comparing.

Of course, we welcome all useful comparisons promoted by Chase-Dunn and Hall (and indeed we do some ourselves) both within this “central” world system and between it or any part of it and other places. We simply think that it is both unnecessary and undesirable to call these other (small) places other “worlds” even regard them as “world-like.”

McNeill has taken our approach as it was intended and has said that he feels he undervalued the importance of international commerce/trade. In McNeill’s own words (from his Foreword to Frank and Gills, 1993), “across the past thirty years or so, my own view has been evolving away from ‘civilization’ and toward ‘world system’ as the best available framework for world history.” However, he also suggests that the concept “communications nets” is preferable to both. McNeill (xii) concludes, “[T]hus

I agree with the authors of this book (*The World System*) in thinking that the rise of specialized occupations producing goods for distant markets was a critical dimension of the deeper human past. Resulting alterations in every day lives were among the most persistent and effective paths of innovation in ancient times as well as more recently.” However, McNeill amends our approach by adding: “[M]arkets and trade constituted only part of the communications network that crossed political, civilizational and linguistic boundaries.” This, quite rightly, is the final area for further extension of our world system research agenda: that is, to broaden the inquiry to really encompass the cultural and political patterns that form an integral part of the world systemic whole.

Conclusion

In summary, our world system approach is based on the rejection of three conventional dichotomies: (A) between the “premodern” and the “modern” economies or between the supposed “political determination” versus the (modern) “economic determination” of economies; (B) between the premodern and modern political cycles: that is, between a premodern “cycle of empires” versus a uniquely modern cycle of (single) hegemonies; and (C) between a “precapitalist” world composed of several distinct world-economies and a unitary “capitalist” world system post-1500. We believe that such a humanocentric history of the world can form the intellectual basis for a new cosmopolitan praxis. Since we reject essentialist views on ethnicity and civilization, in favor of our structuralist approach to ever-changing political economic configurations, our humanocentrism speaks directly to the present era of conflicting nationalisms, localisms, religious identities, and “fragmentation.” From our perspective humanity truly is one, having a true common heritage and sharing a common destiny. We do not propose to return us to the cause of universalism(s), and especially not of the Western-based universalism of “development” or “modernization,” now being sold in the guise of the equation of “democracy” equals the “free market” (Gills, Rocamora, and Wilson, 1993; Frank 1993b). This modern universalism has been inextricably linked with imperialism, and perhaps all universalism must be so to some extent. Modern European colonialism and imperialism, it must be said, was not the first or only attempt to impose universal values.

However, one can and we believe we should propose a defense of cosmopolitanism—in the face of a growing chorus for particularism, methodological individualism, “fundamentalism,” and emotive nationalism. A cosmopolitan praxis, based on a humanocentric understanding of the common historical development of humanity, could

serve to rechannel the impulses of rebellion so prevalent in the present world crisis situation into a more positive direction. The present situation breeds the construction of new separate historical narratives and emphasizes separation, distance, and otherness. Such "historiography," if that is what it can be called, can have little other effect than encouraging conflict and mutual suspicion, even hatred and contempt. If humanity is to truly have a common future on this planet based on mutual trust, acceptance, and even cooperation, it is most imperative that the intellectual underpinnings of a new cosmopolitan praxis be established—and the sooner it is translated into practice the better. Neither "socialism" nor "capitalism" can flourish any longer in a world divided against itself. We must learn to accept our differences while recognizing our common history and working toward our common future. Those who have rejected our world system approach because they believed that it denied all practice in favor of some ahistorical view of unchanging world history have been totally mistaken. On the contrary, our perspective has been intended from the outset to rethink the fundamentals of both political economy and world history (and world development) precisely in order to try to find a broader and better basis for progressive, cosmopolitan praxis.

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CHAPTER FIVE

*A Plea for World System History*¹

Introduction and Procedure

I plead for writing a world history that is as comprehensive and systematic as possible. It should offer a more humanocentric alternative to Western Eurocentrism. This history should seek maximum “unity in the diversity” of human experience and development. Therefore, we should not only make comparisons over time and space, we should also seek more connections among distant and seemingly disparate events at each historical point in time. Moreover, we should systematically seek to systematize both the comparisons and the connections. Thus, our historical inquiry may well find more than comparative commonalities among parts of the whole. We may also discover common features and relations among historical events, which are derived from their common participation in a whole. For the long period before 1492, this “whole” world history should concentrate on the unity and historical interrelations within the Asio-Afro-European “old” “eastern” hemispheric ecumene, stretching from the Pacific to the Atlantic—before Columbus (again) crossed the latter.

The principal idea I advance is the principle, indeed the imperative, of doing a “macro” world *system* history. The main reason to do so is that, as the old adage goes, this historical whole is more than the sum of its parts. This holistic principle does not deny the necessary “micro” history of its parts. However, it is necessary to remember that all the parts are also shaped by—and can only be adequately understood in relation to—their participation in the whole and their relations with other parts. Such “comprehensive” macro attention to the whole and its essential structure and dynamic must, of course, give short shrift to many “micro” details.

However, these can be supplied by specialists, whose also necessary study will in turn help amend and reshape our vision of the whole.

For reasons of expository convenience (for me) and clarity of communication (with the reader), I proceed to pose selected (and numbered) either/or issues. Then, I give my own positions on these issues in the form of theses. Of course, I do not think that all historical reality is so simply reducible to such alternative choices. Nor do I claim to cover all possible or even all important such alternatives and issues. My selection of issues, and their phrasing later, is governed by my own positions—whose arguments I wish to pose for the reader.

Therefore, for reasons of exposition and communication, I frequently resort to brief citations or quotations of arguments of mine, which are elaborated more fully elsewhere. I also “appeal to authority” (and anti-authority by my lights) by citing and quoting authors who have long-standing claim to authority (even if, to their credit, they would disclaim the same). Of course, I do not expect the reader to accept my arguments on the basis of appeals to any authority, least of all my own. On the contrary, my purpose in making these appeals is only to incite readers ever more to “seek truth from (the authority of) facts” and to appeal to “the authority” of their own (re)interpretations of them.

Twenty Issues and Theses

On Eurocentrism and Its Alternatives

1. Should *world* history continue its recent Western Eurocentric bend, or should it seek to liberate the world from it(self)—even in the West? *World* history should be a reflection and representation of the full diversity of human experience and development, which far exceeds the limited and limiting recent bounds of the “West.” Indeed, the “West” does not exist, except by reference to the “East.” Yet the historical existence of “East” and “West” is only a figment of “Western” imagination.

A few generations ago, a different perspective was still counseled even by some Western historians. For instance, in 1918 Frederick Teggart criticized “Eurocentric” history and pleaded for a single “Eurasian” history in which “the two parts of Eurasia are inextricably bound together. Mackinder has shown how much light may be thrown on European history by regarding it as subordinate to Asiatic. . . . The oldest of historians (Herodotus) held the idea that epochs of European history were marked by alternating movements across the imaginary line that separates East from West” (Teggart, 1977: 248).

Yet since Teggart's 1918 plea, Western domination in power and technology has further extended the domain of its culture and Eurocentric Western perspective through proselytizing religion, mass media, language, education, "world" history writing and teaching, and using the (in)famous Mercator projection maps. Nonetheless, homogenization has proceeded less far and fast than some hoped and others feared, and many people around the world are seeking renewed and diverse self-affirmation and self-determination.

2. Should and need Western Eurocentric world history and its distortions be replaced by "equal time" for the history of all cultures? Or need we admit (a variety of competing) other centric histories, be they Islamo-, Nippo-, Sino-, or whatever other centric? No, we can and should all aspire to a nonexclusivist *humanocentric* history. This world history can be more than a historical "entitlement program," which gives all (contemporary) cultures or nationalities their due separate but equal shares of the past. Instead, a humanocentric history can and must also recognize our historical and contemporary unity in and through diversity beyond our ideological affirmations of cultural self. The UNESCO project on the *History of Mankind* (including its *Journal of World History*, published under the main title *Cahiers d'histoire mondiale*, begun in 1954!) and more recently the UNESCO "Integral Study of the Silk Roads" project have made valiant efforts in this direction. Gilbert Allardyce (1990) has reviewed the trials and tribulations of some attempts by UNESCO and others to move "toward world history" through both "entitlement" and "globaloney" programs, at least in the United States and its postwar cultural dependencies. The American leader of the UNESCO project, Louis Gottschalk, finally gave up this "mission impossible." So should fools rush in the footsteps of the daring McNeill (1963, 1990) and Stavrianos (1970) where most angels fear to tread? Why not?

3. If we should not aspire to "equal time" in history of everybody in the world, should such a world history be limited to, or concentrate on, the addition of representative "non-Western civilizations" and cultures to Western ones? Should we limit our study to the comparative examination of their distinctive and common features? This is the procedure of most (literally) so-called courses and textbooks. Some examples of these approaches and their internal contradictions and limitations are examined in Frank (1990a). Two well-known examples examined are the comparative studies of civilizations by Toynbee and Quigley. Another example is the approach to "Civilization as a Unit of World History" by Edward Farmer (1985) and Farmer et al. (1977).

I argue that our world history can and should also make efforts to *connect and relate* the diversity of histories and times to each other. It may be

empirically possible, and in that case it is historically important, to uncover all sorts of historical connections among peoples and places, not only over time but especially at the same time. These connections would lend additional meaning to our comparisons. One cue among others to this kind of historiography is Philip Curtin's *Cross-Cultural Trade in World History* (1984). Another approach was used by Frederick Teggart in his *Rome and China: A Study of Correlations in Historical Events* (1939). Teggart correlated and connected diverse political and economic events (particularly wars, "barbarian" invasions, and interruption or resumption of trade) in these two geographical areas and others in between. Teggart made these connections among contemporaneous events "for the purpose of gaining verifiable knowledge concerning 'the way things work' in the world of human relations... in the spirit of modern scientific work, on the study of World History" (Teggart, 1939: v, xii, and see later). Teggart also proposed a similar inquiry into the possible connections among the often observed almost simultaneous rise in the sixth century B.C. of the religious and other movements associated with Zoroaster, Confucius, Buddha, Ezekiel, and Pythagoras.

On World Historical Comparisons, Connections, Nexuses, and System(s)

4. Need or should world historians then limit themselves to only connecting and comparing different peoples, places, and times as they appear to them at first sight? Or can and should a one world history also seek systematically to systematize these connections and relations, as well as comparisons, into an analysis of a world *system* history? This is now the opinion of our contemporary dean of world history writing, William McNeill (1990). In "The Rise of the West after Twenty-Five Years," he reflects and concludes that

the central methodological weakness of my book is that while it emphasizes interactions across civilizational boundaries, it pays inadequate attention to the emergence of the ecumenical world system within which we live today. Being too much preoccupied by the notion of "civilization," I bungled by not giving the initial emergence of a trans-civilizational process the sustained emphasis it deserved. Somehow an appreciation of the autonomy of separate civilizations (and of all the other less massive and less skilled cultures of the earth) across the past 2,000 years needs to be combined with a portrait of an emerging world system, connecting greater and greater numbers of persons across civilizational boundaries.

To make this a feasible enterprise, one needs a clear and distinct idea of the emergent world system as manifested first in the ancient Middle East and a second time in the modern world, and one must reflect on how these intersected with the more local civilizational and cultural landscapes they impinged upon. . . . In the ancient Middle East, the resulting interactions among peoples living in different landscapes, with diverse languages and other outward signs of civilized diversity, led to the emergence of a cosmopolitan world system between 1700 and 500 B.C. . . . There is a sense, indeed, in which the rise of civilizations in the Aegean (later Mediterranean) coast lands and in India after 1500 B.C. were and remained part of the emergent world system centered on the Middle East. . . . All three regions and their peoples remained in close and uninterrupted contact throughout the classical era. . . . [Moreover] one may, perhaps, assume that a similar [to the modern] primacy for economic exchanges existed also in earlier times all the way back [to] the earliest beginnings of civilization in ancient Mesopotamia. (McNeill, 1990: 9–10, 12–14)

Thirty-five years earlier, Marshall Hodgson had already pleaded as follows:

The point is that *from a world-historical* point of view, what is important is not European history in itself, however important that be for us all; but its role in interregional history. . . . The problem of reorienting ourselves to a more interregional viewpoint, then, is psychologically far-reaching, and must be solved along with that of organizing the historical material.

During the last three thousand years there has been one zone, possessing to some degree a common history, which has been so inclusive that its study must take a preponderant place in any possible world-historical investigation. . . . The various lands of urbanized, literate civilization in the Eastern Hemisphere, in a continuous zone from the Atlantic to the Pacific, have been in commercial and commonly in intellectual contact with each other, mediately or immediately. Not only have the bulk of mankind lived in this zone, but its influence has emanated into much of the rest of the world.

[In] the following approach . . . events may be dealt with in their relation to the total constellation of historical forces of which they are a part—a method not limited to world history, but perhaps likely to be especially appropriate in this case. This means that we are to consider how events reflect interdependent interregional developments. (Hodgson, 1954: 716, 717, 723)

A few years later, Hodgson would add that “few scholarly tasks are more urgent than that of learning to see the various historical backgrounds of our common world in relation to each other” (1960: 879). Allardyce (1990: 62, 67, 69) quotes others to the effect that what world history “needs is a simple, all-encompassing, elegant idea, which offers an adequate conceptual base for a world history.” I suggest that the basic elements of this idea may be found in the foregoing quotations from Hodgson and McNeill. The central concept of this all-encompassing idea is the world *system* and the historical process of its development. What we need is a world system history, please.

The attempt to help advance this “urgent task” is also the main intent of Frank (1990a, b, c, d) and Gills and Frank (1990), although they were largely written before reading these quotations from McNeill and Hodgson. However, the major works by both authors were important inputs. Frank (1990a) concentrates on a critique of many quoted and otherwise cited civilizationists, world and other historians, historical macro sociologists, economic historians, political economists, and others. These scholars mostly do not even consider such a world system history before 1500. Or they consider it, and then deny its practicability or even its utility. Even those few who would welcome a world system history in principle, in their own practice still neglect to pursue it themselves. In each case, I first examine their arguments and procedures. Then, I conclude that their objections or reservations to such a world system history are theoretically invalid and empirically unfounded. Among the authorities, anti-authorities, and others critiqued, and in some cases recommended as partial models, are Abu-Lughod (1989), Amin (1988), Anderson (1974), Chase-Dunn (1986, 1989), Childe (1942), Curtin (1984), Farmer (1985), Farmer et al. (1977), Gernet (1982), Hodgson (1974), Lattimore (1962), Lombard (1975), McNeill (1963, 1982, 1990), Mann (1986), Needham (1961–), Quigley (1961), Schneider (1977), Stavrianos (1970), Taylor (1987–88), Tilly (1984), Toynbee (1946), Wallerstein (1974, 1988), and Wilkinson (1987, 1988).

The conclusions of Frank (1990a) and Gills and Frank (1990) argue why and how such a world system history can and should be undertaken— even if “world history in world-system style is likely to appear . . . as downright subversive” (Allardyce, 1990: 69). But then so have been all new systemic departures. The idea of a world system since 1500 has indeed gained ground in recent years. However, its principal protagonists and others resist the extension of this idea backward before 1500 (for Immanuel Wallerstein, 1974, 1989) or 1250 (for Janet Abu-Lughod, 1989). However, the historical empirical evidence and especially its internally contradictory treatment by these authors vitiate their arguments of a systemic historical break around 1450–1500, as per Wallerstein, or around 1250–1350, as

per Abu-Lughod. This is what I try to demonstrate in Frank (1990b, c) and later. The conclusions derived from my argument challenge the very idea of "transition," especially from the supposed "modes" or "systems" of feudalism to capitalism and on to socialism. No wonder a world system history could appear downright subversive, if it rejects the adequacy of "to each his own" or "equal time to all" cultural histories. It may be even more subversive if it also challenges most people's "scientific" ideologies, according to which their favorite eternal or transitional political economic "mode" or "system" has exceptional virtues—thanks to God.

5. Is world history limited to that of sedentary "civilizations" and their relations? Or must it also include "barbarian" nomads and others, and especially the multifarious relations among the former and the latter? Frank (1990a) follows Lattimore (1962) and others to make a strong plea for more study of central and inner Asian "nomadic" and other peoples, their continuous trade and political relations with their "civilized" neighbors, and the recurrent waves of migratory and invasy incursions from central and inner Asia into east, south, and west Asia, and Europe. Therefore, I argue for greater attention to the possible centrality of central and inner Asia and the dynamics and relations of its peoples with others in world history. Similarly, the nomadic tribes of the Arabian peninsula before the time of Muhammad merit more attention. Moreover, it is time to drop and take exception to the now pejorative term "barbarian." There is much reason to doubt the supposed difference between peoples who have been so labeled and those supposedly more "civilized." There is even reason to doubt the verity and utility of the supposed distinctions between "nomad" and "sedentary" peoples. However that may be, there can be little doubt about the central roles of Central Asia in world (system) history (Frank, 1990d).

Africa has also received less attention than it merits in world (system) history. Curtin has done pioneering work on trade and migration in Africa, but in his *Cross-Cultural Trade in World History* (1984) he has not sought to pursue the African connection in Afro-Asia as far back in history as it may deserve. The southeast Asian peoples and their history were intimately related to and also influential on those of China and India, yet southeast Asia is often largely omitted from even those world histories that give their due to China and India. Relations between the "eastern" and "western" hemispheres, across both the Atlantic and the Pacific, even if they may not have been "systematic," long predate those (re)initiated by Columbus.

Exceptional geographical, topological, ecological, natural, or human resources have lent a select few regions in the world very special strategic, military, political, economic, and cultural importance in the establishment and maintenance of the world system and relations within it. Gills and Frank argue that three magnets of attraction for political economic

expansion stand out. One is sources of human (labor) and/or material inputs (land, water, raw materials, precious metal, etc.) and technological inputs into the process of accumulation. The second is markets to dispose of one zone's surplus production to exchange for more inputs, and to capture stored value. The third, and perhaps most significant, are the most privileged nexuses or logistical corridors of interzonal trade. Bottleneck control over the supply routes of raw materials, especially of metals and other strategic materials, plays a key role in attracting hegemonical powers to such areas, or in providing a basis upon which to make a bid for hegemony. Especially here, economic, political and military conflict and/or cultural, "civilizational," religious and ideological influence also offer special advantages for tapping into the accumulation and the system of exploitation of other zones in benefit of one's own accumulation (Gills and Frank, 1990: 24).

Gills and Frank identify three such corridors and logistical nexuses between the Mediterranean and Asia:

The Nile-Red Sea corridor (with canal or overland connections between them and to the Mediterranean Sea, and open access to the Indian Ocean and beyond).

The Syria-Mesopotamia-Persian Gulf corridor (with overland routes linking the Mediterranean coast through Syria, on via the Orontes, Euphrates and Tigris rivers, to the Persian Gulf, which gives open access to the Indian Ocean and beyond). This nexus also offered connections to overland routes to Central Asia.

The Aegean-Black Sea-Central Asia corridor (connecting the Mediterranean via the Dardanelles and Bosphorus to the overland "Silk Roads" to and from Central Asia, from where connecting routes extended overland to India and China). (Gills and Frank, 1990: 24)

However, there were other such logistical nexuses in various maritime straits, such as those of Ceylon, and overland portages such as Kra on the Malay Peninsula. Along the overland invasory and silk routes in inner Asia, and its connections to China, India, and Persia, other bottleneck and crossroad nexuses played strategic roles. Among these were the Gansu (Haxi) Corridor between China and Dunhuang at the desert's edge, and the Karakorum and other passes across the Pamirs southwestward from Kashgar to Taxila, and across the Tian Shan Mountains northwestward to Samarkand and Bukhara. All of these and other nexuses deserve special attention in the study of world (system) history. Have they been special bones of political and economic contention militating against their long-term control by any one power?

On World Historical Times and Timing

6. Should we treat historical diversity and comparisons as we often do, and as Anderson explicitly defends, by arguing that “there is no such thing as a uniform temporal medium: for the times of the major Absolutism... were precisely, enormously diverse... no single temporality covers it”? (Anderson, 1974: 10). Or can and should the systematization of interregional world history also realize, as Hodgson argued, that “what is important is the recognition... that there has been some sort of developing pattern in which all these interregional developments can be studied, as they are affected by and in turn affect its elements as constituted at any one time” (Hodgson, 1954: 719).

In Frank (1978b) I argued that

Anderson’s apparent attempt to make historiographic virtue out of empirical necessity when he argues that the historical times of events are different though their dates may be the same must be received with the greatest of care—and alarm. For however useful it may be [comparatively] to relate the same thing through different times, the essential (because it is the most necessary and the least accomplished) contribution of the historian to historical understanding is successively to relate different things and places at the same time in the historical process. The very *attempt* to examine and relate the simultaneity of different events in the whole historical process or in the transformation of the whole system—even if for want of empirical information or theoretical adequacy it may be full of holes in its factual coverage of space and time—is a significant step in the right direction (particularly at a time in which this generation must “rewrite history” to meet its need for historical perspective and understanding of the single world historical process in the world today) (Frank, 1978a: 21–22).

Teggart, alas unbeknownst to me, had long since

established (for the first time) the existence of [temporal] *correlations in historical events*... which exhibits the relationship between contemporaneous disturbances in several areas... [and] awareness of the concurrence of events in different regions.... The study of the past can become effective only when it is fully realized that all peoples have histories, that these histories run concurrently and in the same world, and that the act of comparing is the beginning of knowledge.... It at once sets a new problem for investigation by raising the question of

how the correspondences in events are to be accounted for (Teggart, 1939: 239, 243, 245).

7. Did world history discontinuously jump from one place and time to another? The usual Western Eurocentric rendition jumps from ancient Mesopotamia to Egypt, to "classical" Greece and then Rome, to medieval Western Europe, and then on to the Atlantic west, with scattered backflashes to China, India, and so on. Meanwhile, all other history drops out of the story. Or peoples and places never even appear in history, unless they are useful as supposedly direct descendants of development in the West. Instead, any world history should try to trace and establish the historical continuity of developments between then and now in the *world* systemic whole and all its parts. Hodgson and McNeill already emphasized this *continuity*. David Wilkinson (1987) supports Hodgson's early suggestion, which Wilkinson probably did not know. Wilkinson demonstrates convincingly (to me) that "Central Civilization" has a continuous and expanding (I would say world system) history since Mesopotamia and Egypt established relations in about 1500 B.C.

Gills and Frank (1990) argue that these relations extend even farther out and further back. During another millennium from 2500 B.C. or earlier already, peoples established relations with each other around and from the Mediterranean to the Levant, Anatolia, Mesopotamia, the Persian highlands, and the Indus Valley, as well as with many Central Asian "nomads" and others. Gordon Childe (1942) already argued for the recognition and analysis of these and even earlier and more widespread relations. Some two millennia later, China, Manchuria, Korea, and Japan in the northeast, and southeast Asian peoples developed (systematic?) relations with each other and with other peoples across and around Asia. Systemic relations around the beginning of the Christian era among Han China, Kushan Pakistan/India, Parthian Iran, the Roman Empire, and parts of Africa are well documented and analyzed by among others Hudson (1931), Teggart (1939), and with regard to technological diffusion more recently again by Needham (1961). Several recent authors quote Pliny's lament about the fiscal crisis in his native Rome, which was due to its balance of trade deficit with Parthia and through it with China. Teggart went further. He quoted Cicero to the effect that "the credit of the Roman money-market is intimately bound up with the prosperity of Asia; a disaster cannot occur there without shaking our credit to its foundations" (Teggart, 1939: 74). Odani (1990) suggests that, since Roman and Asian coins were of exactly the same weight and, therefore, interchangeable, a single international monetary system may have existed.

Teggart also correlated and compared the timing of wars and barbarian invasions in Rome and China to demonstrate that for the period 58

B.C.–A.D. 107 alone, “even in briefest summary it must be pointed out that, of the wars in the Roman East, eighteen followed wars in Chinese Turkestan, so that of the forty occasions on which outbreaks took place in Europe, twenty-seven were traceable to the policy, or rather changes of policy, of the Han government [in China]” (Teggart, 1939: viii). Teggart pioneered this analysis and suggested that

it is to be seen that peoples in no way concerned with the silk route might yet be connected with the interruptions of trade on that route through the hostilities that the interruptions precipitated between Parthia and Rome. Thus the effects of wars that arose out of interruptions of the great “silk route” through Persia are plainly visible in the internal history of Rome. . . . Seemingly there could be no better illustration of interdependence of nations than the consideration that a decision of the Chinese government should have been responsible for a financial panic in the capital of the Roman Empire. . . . It follows, therefore, that knowledge which is indispensable for an historical account of Roman affairs . . . can be obtained in no other way than by the comparison of events throughout Eurasia. Thus, apart from any wider interest, the comparisons of histories is necessary for a comprehension of what has actually happened within the borders of any national state (Teggart, 1939: X, 241, 243).

Actually, Teggart himself did not limit his inquiry to correlations and comparisons. He also inquired into what he called their “connections” and knowledge concerning “the way things work” in what we might call “the world system.” Moreover, Brooks Adams (1939) long ago pleaded for the recognition of this world historical unity and continuity.

8. However, since when can we accurately refer to “China,” “India,” “Persia,” “Central Asia,” or elsewhere as particular peoples or civilizations? Alternatively, how long were (or still are?) these only geographical loci in and through which different peoples came and went, mixed, and developed cultural, social, political, and economic institutions and relations, which also came and went? Most civilizations, empires, ethnicities, “races,” and of course nations only *temporarily developed here and there out of a mixture of peoples*. Some peoples among them took or gained enough of a temporary upper hand to put their temporary imprint and name on the civilization, dynasty, or empire, and so on. Perhaps the longest still living civilization is that of the Chinese. Yet for half of “China’s” history, it has been ruled by non-Chinese.

Historians conventionally study the “dynastic” history of China. Civilizationists generally focus on this and other (supposedly self-contained)

“civilizations.” Thereby, both have detracted attention from the more important, but often changing, ecological or economic units, empires, states, and (inter)state *systems*, and their *relations* with each other over much of the world. Moreover, the fact that peoples and their institutions have come and gone over the world stage of history does not mean that there was no systemic rhyme or reason to their coming and going. On the contrary, the very coming and going of different peoples, their institutions, and their relations with each other may systematically, and not only exceptionally, have obeyed some systemic “laws” of world system development and history. We should inquire into these.

9. Should we then start our world historical (system) inquiry at some arbitrarily or conveniently selected date? Or should we instead permit the historical evidence *to take us back as far as we can go*? Should we move forward or backward in our historical inquiry? Both! John King Fairbank, the contemporary dean of American historians of China, wrote from his experience that “the rule seems to be, if you want to study the mid-period of a century, begin at the end of it and let the problems lead you back. Never try to begin at the beginning. Historical research progresses backward, not forward” (Fairbank, 1969: ix). This has been my experience as well, and I recommend Fairbank’s rule to others with two reservations. One is that real historical development, of course, moved forward in time, and our scientific rendition of it must respect this fact. The other is that however heuristically useful it may be for us to inquire backward, we can still turn around to relate and present our findings and history itself forward in time.

On Cumulation of Accumulation and Ecology in World System History

10. Is world (system) history only continuous (since when?), or is it also cumulative? Has there been, is there still, a *cumulative historical development*? Civilizationists and cultural historians have long since presented much of human knowledge and culture as cumulative. Childe (1942) and others have also presented technology as substantially cumulative (little reinvention of the wheel). If that is so, can we not theoretically argue and empirically demonstrate that world (system) history includes a long process of economic accumulation, including skills and technology, but also infrastructural, productive, and financial accumulation? That is the argument of Gills and Frank (1990) under the title “The Cumulation of Accumulation.”

11. Is this process of accumulation, and the associated production, trade, finance, and their political organization independent of ecological possibilities and limitations? Just posing this question seems to answer it, especially in this age of heightened ecological degradation and awareness. Human social, economic, and political history have always been *adaptations to ecological circumstances and changes*. Ecological possibilities and limitations helped determine the development of alluvial valley agricultural civilizations such as ancient Sumer and Egypt. Their ecology also affected their needs for commerce and political influence over highland sources of metals and other mineral raw materials and wood. Similarly, ecological realities and their changes also impacted on grassland nomadic and other peoples and their trading, migratory, and invasory relations with sedentary civilizations. Of course, hunting, migration, agriculture, industry, political and military institutions and activities, and many cultural ones have also in turn impinged on and altered the physical environment. Today, but also at some times and places in the past, this human ecological impact has been damaging to the physical environment and to human welfare. A world history must devote more attention to human and social ecology, especially now.

12. Are these ecological and social adaptations and transformations often renewed independent inventions (as of the wheel) at different times and places in the world? Or are many of them also the result of migratory, invasory, trade, political, and cultural *relations and diffusion around the world*? Or both? The easy answer would seem to be both by simple addition of renewed invention here and there and diffusion from here to there. However, "necessity is the mother of invention." Therefore, much of the renewed "(in)dependent" invention and innovation there was also "diffused" from here. That is, invention was stimulated there by the necessity of competition with here, where its use offered a competitive advantage. Moreover, this process of diffusion and emulation of invention and innovation was not limited to things (bronze) or technology (smelting) but extended to social institutions and cultural forms.

Philip Curtin and William McNeill are among those who subscribe to and offer empirical evidence for the diffusionist thesis, both simple and competitive. Every day, archaeologists uncover, and reinterpret, additional evidence for maritime and overland diffusion over the longest distances, and at earlier and earlier times. Diffusion spread, among other things, food-stuffs; agricultural, industrial, transport, and military technology; culture and religion; language and writing; mathematics and astronomy; disease, first plague deaths and then resistance to the same, and medicine; and, of course, genes. See, for instance, McNeill's *Plagues and Peoples*. The more we look for diffusion, the more we find. The place of diffusion in a truly world-embracing history is assured, if we would only admit more of it.

13. A particularly important open question is whether the all too widespread sociocultural institution of *patriarchy* was indigenously invented by many societies or diffused from a few to many. Feminist archaeologists and historians (thank Goddess for them!) have begun to dig up or reinterpret a paleolithic and neolithic past supposedly governed by nonpatriarchal “partnership” relations. However, these relations were found to be “indigenous” particularly in Catal Huyuk and Hacilar in Anatolia, the site of Jericho in the Levant, later in Minoan Crete, and in the Balkans (Eisler, 1987). Figurines that suggest nonpatriarchal goddess worship have also been found farther eastward into India. The feminist scholars argue that these societies, and by extension Western Judeo-Christian society, only switched to patriarchy later after armed invaders from inner and Central Asia brought warfare, military technology, oppression, and therewith the “diffusion” of patriarchy. Thus, these feminist scholars suggest that Western patriarchy is the result of its (unwelcome) diffusion from farther east in inner Asia.

(Re)writing history from a more gender-balanced or feminist perspective is very welcome. We particularly need more “feminist historical materialist” analysis of different and changing gender and family relations, accumulation, politics, and culture/ideology. Much of history has been dominated by men in their own interest and has been written by them from their own perspective. However, the aforementioned feminist version of history seems less than satisfactory. It focuses rather selectively on some circum-Mediterranean societies with supposedly indigenous partnership societies and sees patriarchy as having been only belatedly diffused there from inner Asia. These primarily Euro-Mediterranean-centered feminist historians would do well to expand their scope to that of the world, if not also to the world system, as a whole.

James DeMeo (1987, 1990), for instance, claims that “matrist” (but not matriarchal), democratic, egalitarian, sex-positive, pleasure-oriented, gentle, and nonviolent society was “original” in much more of the world while it was wetter and greener until 6,000 years ago. Then, Arabia and Central Asia dried up about 4000–3500 B.C.; desertification expanded through what he calls the thousand-mile-wide SaharAsian belt stretching 8,000 miles from Africa through inner Asia to China. As a result, many of its inhabitants suffered famines and were obliged to become pastoralist nomads. The harsh and competitive realities of this new lifestyle then fostered “patrism,” including patriarchy, which DeMeo characterizes through at least thirty-five sociocultural variables. These include harsh child rearing and especially infant swaddling and induced cranial deformation to enhance parents’ mobility, sexual repression, patrilocal residence, patrilineal descent and inheritance, various forms of subordination of women,

organized and specialized priesthood, high-class stratification, high bellicosity, and frequent warfare. DeMeo finds these and other characteristics of patrism auto-correlated among each other and correlated with Saharasian and neighboring regions, as well as in some similar regions in the Western hemisphere.

Thus, like the aforementioned feminists, DeMeo also sees the subsequent diffusion of patrism by migrants and invaders escaping from dry regions to other wetter ones with previously matrism societies. However, he also tries to account for patrism as a prior widespread adaptation to changing environmental-economic conditions in the Saharasian belt. Moreover, DeMeo tries to demonstrate how, once it is introduced anywhere, patrism is reproduced, reenforced, and perpetuated intergenerationally, irrespective of subsequent patterns of climate, food supply, or settlement. Perhaps this approach offers additional scope and method for the study of endogenous invention/diffusion of patriarchy and other sociocultural characteristics. On the other hand, like Eisler, DeMeo seems to disregard evidence and theory in support of indigenous development of patriarchy in agriculturally based ancient states and civilizations. Moreover, all those students of Asian nomads whom I have questioned say that on the evidence available to them, the status of women was higher and gender relations were more equal among nomadic than among sedentary peoples. Thus, the question remains open and calls for much more research.

On World System Characteristics and Transitions before and after 1500 A.D.

14. Are systematic and systemic relations of trade, not to mention migration and invasion or military conflict over the same, only recent developments in world (system) history, which bear study merely since the twentieth century, or the nineteenth, or the sixteenth? Or must we more systematically trace all of these political economic relations, no less and maybe even more than cultural ones, back farther and farther in a wider world (system)? I propose the latter and offer some indications on how to proceed in Frank (1990a, b, c, d) and Gills and Frank (1990). For millennia already, these systemic relations of peoples and localities combined a mixture of systematic trade relations and recurrent migrations far beyond the confines of any state or empire. Diplomatic expeditions, military excursions, and shifting alliances among states and empires were expressions of systematic and systemic relations. So were the diffusion and invention or adaptation of technological advances, social institutions, and cultural forms in response to changing ecological, economic, political, and often competitive necessities and opportunities in the wider world system.

15. Can the principal systemic features of the “modern world system” also be identified earlier than 1500 or not? Wallerstein (1988) and Modelski (1987) argue that the *differentiae specifica* of our world system are new since 1500 and essentially different from previous times and places. Christopher Chase-Dunn (1986) and others find parallels in “other” and prior world systems. Wilkinson (1987) discovers at least some of these features in his “Central Civilization” and elsewhere. However, he sees historical continuity, but no world system. Abu-Lughod (1989) sees a “thirteenth-century world system,” but she regards it as different from the world system since 1500 or before 1250. Moreover, she is not so interested in comparing systemic features or characteristics. Gills (1989) and Gills and Frank (1990) combine all of the aforementioned into an analysis, or at least an identification, of the principal features of this world system over several thousand years of its history and development, which are detailed in what follows.

16. According to Wallerstein (1988, 1989, and elsewhere) and many students of world capitalism, the *differentia specifica* of the modern world system is the accumulation of capital: “It is this ceaseless accumulation of capital that may be said to be its most central activity and to constitute its *differentia specifica*. No previous historical system seems to have had any comparable *mot d'ordre*” (Wallerstein, 1989: 9). But was capital accumulation absent or minor or irrelevant elsewhere and earlier? Or, on the contrary, did capital accumulation exist and even define this (or another?) world system before, indeed long before, 1500? Gills and Frank (1990) emphatically argue for this latter position and point to considerable empirical evidence to back up the argument. For millennia and throughout the world (system), there has been capital accumulation through infrastructural investment in agriculture (e.g., clearing and irrigating land) and livestock (cattle, sheep, horses, camels, and pasturage for them); industry (plant and equipment as well as new technology for the same); transport (more and better ports, ships, roads, way stations, camels, and carts); commerce (money capital, resident and itinerant foreign traders, and institutions for their promotion and protection); military (fortifications, weapons, warships, horses, and standing armies to man them); legitimacy (temples and luxuries); and of course the education, training, and cultural development of “human capital.”

The drive to produce, accumulate, distribute, and consume capital provided much of the economic, social, political, and cultural motor force in history. This was the case, for instance, of the development of Song and earlier Tang China, Byzantium, the expansion of Islam, Gupta India, and other regions in “medieval” times. However, the same may be said equally of the earlier “classical” Rome, Parthian Persia, Kushan India, and Han China; of the still earlier Hellenistic world and Persia; and so on back through world history. The mere mention of these “political”

entities, not to mention their many peripheries, hinterlands, and countless nomadic migrants and invaders, should suggest that the same drive to accumulate was instrumental, if not largely determinant, for the competitive economic, political, and military rivalry and occasional opportunist alliances among and within contemporaneous political entities. That is, the quest for achievement and subsequent renewed loss of *competitive advantage (and disadvantage) within the process and pressures of competitive accumulation* have marked the economic, political, social, and cultural development of human and world system history through the ages.

17. Are other characteristics, in particular a *core-periphery* structure, of the modern world system also unique to it since 1500? Or are they also identifiable elsewhere and earlier? In a short list of three main characteristics of his modern world system, Wallerstein (1988) argues that "this descriptive trinity (core-periphery, A/B [cycle phases], hegemony-rivalry) as a pattern maintained over centuries is unique to the modern world-system. Its origin was precisely in the late fifteenth century" (Wallerstein, 1988: 108). Wallerstein (1989) also makes a list of twelve characteristics of his modern world capitalist system since 1500. Frank (1990c) argues why *all* of them also apply earlier. Frank (1990a) and Gills and Frank (1990) argued the same even before seeing Wallerstein's lists of characteristics. To avoid tiring the reader here, however, we limit the present review to Wallerstein's holy trinity alone.

The first characteristic is the core-periphery structure. Christopher Chase-Dunn and Tom Hall (1990) edited a book on *Precapitalist Core/Periphery Relations*. Chase-Dunn (1986) himself has found many examples and so has Gills (1989). Wilkinson (1987) surveys core-periphery relations over 5,000 years of world system history, which Ekholm and Friedman (1982) argued earlier. Therefore, Gills and Frank (1990) contend that core-periphery structures and relations have been prevalent throughout geographical space and historical time. Conceptually, however, they also need to be extended to hinterlands and to a center-periphery-hinterlands (CPH) complex.

The hinterland is not directly penetrated by the extracting classes of the center, but nevertheless it has systemic links with the center-periphery zone and its processes of accumulation. Wallerstein's use of the term hinterland to mean external to the world system is insufficient because it neglects the structural and systemic significance of zones, which are "outside" of, but nonetheless related to, the center-periphery complex. These CPH relationships have been insufficiently analyzed. The CPH complex does not refer to mere geographical position nor only to unequal levels of development. CPH also refers to the relations among the classes, peoples, and "societies" that

constitute the mode of accumulation. The CPH complex is the basic social complex upon which hegemony is constructed in a larger systemic context (Gills and Frank, 1990).

18. Another of the three world system characteristics mentioned by Wallerstein is *hegemony-rivalry*. But is this feature limited to the world since 1500? Or did it also exist elsewhere and earlier? Or, indeed, does it also characterize the same world system earlier? Wallerstein himself discusses the rise and fall of mostly economically based hegemony only since 1500. Modelski (1987) and Thompson (1989) analyze largely politically based and exercised hegemony since 1494. Paul Kennedy (1987) wrote a bestseller about the *Rise and Fall of the Great Powers* but without connecting them in any systematic way. The decline in the hegemony of a great power gives way to an interregnum of competitive economic, political, and military rivalry among others to take its place.

Gills and Frank (1990) argue that hegemony-rivalry has also characterized the world system for thousands of years. As suggested earlier, hegemony is not only political. It is also based on center-periphery relations, which permit the hegemonic center to further its accumulation of capital at the expense of its periphery, hinterland, and its rivals. After a time, not the least through the economic-military overextension signaled by Kennedy, the hegemonic empire loses this power again. After an interregnum of rivalry with other claimants, the previous hegemonical power is replaced by another one. Shifting systems of economic, political, and military alliances, reminiscent of those featured by George Orwell in his 1984, are instrumental in first creating, then maintaining, and finally losing hegemonical imperial power. Gills and Frank (1990) not only argue that there have been numerous and repeated instances of hegemony and rivalry at imperial regional levels. They also suggest that we may be able to recognize some instances of overarching "super-hegemony" and centralizing "super-accumulation" at the world system level before 1500. The Mongol Empire certainly, and Song China perhaps, had a claim to super-hegemony. Thus, very significantly, the later rise to super-hegemony in and of Western Europe, Great Britain, and the United States after 1500 were not unique first instances in the creation of a hegemonic world system. Instead, as Abu-Lughod persuasively argues, "'the fall of the East' preceded the 'Rise of the West'" (Abu-Lughod, 1989: 338) and resulted in a hegemonical shift from East to West. This shift came at a time—and perhaps as a result—of overextension and political economic decline in various parts of the East, which suffered a period of cyclical economic decline so common to them all as to have been world system wide. Thus the "Rise of the West," including European hegemony and its expansion and later transfer to the "new world" across the Atlantic, did not just constitute a

new Modern World Capitalist System. This development also—and even more so—represented a new but continued development and hegemonic shift *within an old world system*.

19. The third characteristic of Wallerstein's world system after 1500 is *long economic cycles of capital accumulation*. Their upward "A" and downward "B" phases generate changes of hegemony and of position in the center-periphery-hinterland structure. These cycles, and especially the Kondratieffs, play important roles in the real development of the world system and in its analysis by Wallerstein (1974), Frank (1978a), Modelski (1987), Goldstein (1988), and Thompson (1989). All emphasize the relations among cycles in the economy, hegemony, and war. However, are these cycles limited to modern times, or do they extend farther back? Frank (1990c) tries to demonstrate that this same cyclical pattern definitely extends back through the eleventh century and that it could be traced further back as well. Gills and Frank (1990) go on to argue that these long cycles extend much farther back in world system history. Even Wallerstein notes that

it is the long swing that was crucial. . . . The feudal system in western Europe seems quite clearly to have operated by a pattern of cycles of expansion and contraction of two lengths: circa 50 years [which seem to resemble the so-called Kondratieff cycles found in the capitalist world economy] and circa 200–300 years. . . . The patterns of the expansions and contractions are clearly laid out and widely accepted among those writing about the late Middle Ages and early modern times in Europe. . . . It is the long swing that was crucial. Thus 1050–1250+ was a time of the expansion of Europe (the Crusades, the colonizations). . . . The "crisis" or great contractions of 1250–1450+ included the Black Plague. (Wallerstein, 1989: 33, 34)

Thus, even according to Wallerstein, there was systematic cyclical *continuity* across his 1500 divide—in Europe. But Abu-Lughod (1989), McNeill (1982), and others offer and analyze substantial evidence that this same cycle was in fact world system wide. Again, even Wallerstein perceives some of the evidence:

The collapse of the Mongols [was a] crucial non-event. . . . The eleventh-century economic upsurge in the West that we have discussed was matched by a new market articulation in China. . . . Both linked up to a Moslem trading ecumene across the Middle East. China's commercialization reinforced this model [why not system?]. . . . The Mongol link completed the picture. What disrupted this vast trading *world-system* was the pandemic Black Death, itself quite probably a

consequence of that very trading network. It hurt everywhere, but it completely eliminated the Mongol link. (Wallerstein, 1989: 57, 58, my emphasis)

Moreover, all these developments were driven by the motor force of capital accumulation. The “crucial long swing” was a cycle of capital accumulation. It seems likely, however, that the rise and decline of different empires in medieval, “classical” Roman-Parthian-Kushan-Han, and even ancient times can and should be fit into such cyclical patterns of their own. Moreover, these regional cycles may in turn fit into, or indeed be partially derivative from, a single world system-wide cycle of capital accumulation, hegemony, and development.

20. So do these characteristic similarities with the “modern-world-capitalist-system” extend only to “other” earlier empires, state systems, regional economies, or different “world systems”? Or do *similar characteristics extend backward through time in the same world system*, which itself also extends much farther back in time? I believe the historical evidence supports, and our analytical categories should promote, this second interpretation. How can we extend the essential features of the “modern-world-capitalist-system” of Wallerstein (1974), Frank (1978a), Modelski (1987), Goldstein (1988), Thompson (1989), and others, and of the “other” world systems and civilizations of Chase-Dunn (1986, 1989), Wilkinson (1987, 1989), and others *back in time through the same world system*? The argument in Frank (1990a) and Gills and Frank (1990) is, in its essence, that this same world system was born at least five thousand years ago out of the confluent relations of several “civilizations” and other peoples. As mentioned earlier, these included at least peoples in Egypt, the Levant, Anatolia, Mesopotamia, Persia, India, and Central Asia. They and other peoples have ever since been continuously and cumulatively related through center-periphery-hinterland structures, relations of hegemony and rivalry, and cycles. These have been regional and probably world system wide. Since Wallerstein’s *differentia specifica* is *not* specific only to modern times, we can and should extend the identification of his single most important defining characteristic of this world system back through time: *Capital accumulation and interpenetrating transfer* of surplus have long characterized and related different parts of the same world system.

Gills and Frank (1990) schematically define this criterion of world system identification and bounding as follows:

The capture by elite A here (with or without its redistribution here) of part of the economic surplus extracted by elite B there means that there is “interpenetrating accumulation” between A and B. This

transfer or exchange of surplus connects not only the two elites, but also their “societies” economic, social, political, and ideological organization. That is, the transfer, exchange or “sharing” of surplus connects the elite A here not only to the elite B there. Surplus transfer also links the “societies”’ respective processes of surplus management, their structures of exploitation and oppression by class and gender, and their institutions of the state and the economy. Thus, the transfer or exchange of surplus is not a socially “neutral” relationship, but rather a profoundly systemic one. Through sharing sources of surplus, the elite A here and the classes it exploits are systemically interlinked to the “mode of production,” and even more important, to the mode of accumulation in B there. By extension, if part of the surplus of elite B here is also traded, whether through equal or more usually unequal exchange, for part of the surplus accumulated by elite C there, then not only B and C but also A and C are systemically linked through the intermediary B. Then A, B and C are systemically connected in the same over-arching system of accumulation. This means that surplus extraction and accumulation are “shared” or “inter-penetrating” across otherwise discrete political boundaries. (Gills and Frank, 1990: 27)

The argument is that these system-defining relations have persisted continuously and grown cumulatively albeit cyclically on a system-wide basis throughout much of the world for thousands of years. For instance, such systemic relations not only characterized, but probably motivated, many Akkadian and Sumerian Mesopotamian economic ties, political institutions, and military excursions into Anatolia and Persia from the time of Sargon in the 2300s B.C. Lattimore (1962), Eberhard (1977), Gernet (1982), and many others have documented and analyzed the later recurrently continuous, systematic, and systemic exchanges of surplus and other relations among sedentary “civilized” people in China and nomadic “barbarian” peoples from Central Asia (and with those who were intermittently one or the other in between). Similar, if perhaps more tenuous or at least less researched, overland and maritime relations developed among Chinese and southeast Asian Peoples. Farther west, the near-simultaneous birth and spread of major religions after 600 B.C. and later Persian-Hellenic rivalry probably responded not only to contemporary similar, but to perhaps also *related*, conditions in different “parts” of the world. As noted earlier, the birth of Christ, expanding systemic relations, and interpenetrating exchange of surplus characterized and *helped shape* all of Han China and its military conquests and economic dependencies through Central Asia, Kushan, and then Gupta south Asia, Parthian Persia, imperial Rome, and its African and European outposts. Indeed, the subsequent near-simultaneous and

coordinated imperial declines from Han China to western Rome and the renewed “barbarian” incursions ultimately emanating out of Central Asia should be analyzed as the interconnected expressions of a single dynamic in a single world system.

Conclusions

To Reject Fashionable Transitions and Modes

Given this argument and the historical evidence to sustain it, is it still possible or sensible to argue that there was a qualitatively different “transition” to and creation of a “modern-world-capitalist-system” around 1500? Or that this “transition” arose essentially out of the “transition from feudalism to capitalism” in Europe? No! and No again! It is time to relegate the latter debate to the parochial European history to which it rightly belongs. We may still wish to debate whether there was a significant “transition” in the world as a whole around 1500, and whether this transition was more “significant” than earlier or subsequent ones. However, in this debate it would be useful and clarifying for all participants to understand that the real world (system) essence of a transition is a transition from a transition to a transition! Then we can see which transitions, if any, are more equal than others, for instance, in the light of the dramatic supposedly “world shaking” transitions taking place, as I write, in Eastern Europe.

Then, is it still sensible to hold on for dear life to the supposedly scientific historical categories of, and ideological preferences for, feudalism, capitalism, socialism—or indeed any such “scientifically” defined “modes of production” or ideologically defined “systems” and “isms”? I believe NOT! (and I argue so in Frank 1990a, c). However, the beliefs in either the virtues or the vices, or both, of “*capitalism*” and also of “socialism” are still very irrationally cherished, strongly held, and widely shared (literally) right and left all around the world. Therefore, scarcely anyone is yet ready to abandon them, no matter how strong the historical evidence nor how logical the argument. Even readers who have followed and may accept my argument through the first twenty points may resist these conclusions. Nonetheless, the historical and contemporary evidence strongly suggests—and may increasingly persuade more people—that these virtues and vices are systematically ingrained in the world system itself, and not in any of its transitionally varying or variably transitional mixed-up “modes.”

Those who still cannot liberate themselves from their “modal” and “modish” thinking should at least examine the historical evidence that all “modes” share virtues *and* sins, even if the shares of some may be more

equal than others. Moreover, the absolute and relative virtues and sins vary over historical time and perhaps over the “life cycles” of “modes” and their implementation or application in (different parts of) the world system. Indeed, it might be said that it is through the virtues and sins of its various and varying “modes” that the system expresses its own structural and dynamic characteristics, operation (“function”), and development (evolution). In that case, however, the insistent reification of “modes” is a case of “misplaced concreteness.” If we want to reify anything, we would do better (less badly) to reify the world system itself (like me?). Yet even then, we should regard the system like a three-legged stool, supported equally by its ecological/economic, political/military, and cultural/religious/ideological legs.

World system history is long (and cyclical!), and I can wait for this idea’s time to come (again!). In the meantime, as throughout world system history in the past, people—today (again) actively including many more women—will unite in a myriad of ever-changing social movements to continue their ever-lasting struggle for their just demands and rights. More power to them! A Luta Continua!

Note

1. Author’s Note: After coauthoring our aforementioned joint article, Barry K. Gills also helped me improve this one.

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P A R T 3

Beyond Eurocentrism, Systems Transformation, and Social Movements

Providing us with an innovative framework for analyzing world-scale structures, processes, and timing of the evolving world system connected to the world-historical events and transformations, Frank extends his reorientation to dealing with economic and political issues. He questions the legitimacy of the extant conflicting assumptions and conclusions about the knowledge and conception of world development, especially in contrast to scholars such as Braudel (1981, 1982, 1984) and Wallerstein (1974–80). From his view, Braudel and Wallerstein continue to perpetuate the Eurocentrism of the development of the modern world. By reexamining world history without acknowledging a break in 1500 of the development of the world, and coupled with his critique of Eurocentrism of the received works of Wallerstein and Braudel, Frank (with Barry Gills) challenged the periodization of the dominance and hegemony of the West that has been commonly accepted. For Frank and Gills it was Asia before Europe. With Frank's celebration of the importance of understanding world transformations via world history, they proceed to argue how our comprehension of world development should change in major disciplines of the scholastic world.

Such a shift in understanding world development would have significant impact on the varied disciplines and theories that have been developed and formulated to understand long-term socioeconomic and cultural patterns. The mapping of commonalities and differences between different theories and disciplines are raised, and future research directions are suggested that should consider the world system history perspective that Frank and Gills propose. The gamut of disciplines and theories identified by Frank include world-systems theory, macro historical sociology, world

historiography, civilizationism, archaeology, classicism, medievalism, economic history, political geography, international relations, international political economy, development studies, ecology, anthropology, ethnic and race relations studies, and gender relations.

Frank's preoccupation with the legitimacy upon which the basis of our knowledge and conception of world development is based occupied a substantial part of his efforts in this reorientation process. He extends his historical perspective on modern world history, particularly focusing on an incisive critique of the important works of Braudel and Wallerstein. His argument is that the evidence Braudel and Wallerstein have presented demonstrate that *modern* world history unfolded in a world economic system, with centers shifting from Asia to Europe after 1800. Frank also addresses the controversy over the European origin of capitalism, and casts doubt on the validity of calling early modern world economy as simply capitalist. In this case, it parallels the theoretical, historical, and methodological critiques Frank made to modernization-development theorists outlined in chapter two; except here, he aims his critique at prominent world systems theorists. In light of the evidence for one world economic system presented by Braudel and Wallerstein, Frank poses four central questions: Where did the world system arise? Where was it centered? Which peoples and regions were inside or outside this global system? Was or is the world system "capitalist"? His answers provide an essential part of the critique of Braudel and Wallenstein's model of a European-based, modern world-system and simply raises crucial ideological, political, geographic, and cultural issues in our understanding of world development.

Frank's *ReOrient: The Global Economy in the Asian Age* provided the historical evidence in support of his theoretical reformulation of his understanding of world development. By no means did he stop then. Moving forward in time by first starting from the deep past he continues his analysis from his last book, *ReOrient*, by extending the research to the nineteenth century. Frank in an unpublished book manuscript, "ReOrient the 19th Century," completed before his passing away, focuses on the nineteenth-century world economy, and compares and contrasts the economic indicators of Europe, specifically those from Great Britain. Since Frank's understanding of world development is overwhelmingly based on an analysis of structures and processes, major aspects of economic indicators including science and technology of Great Britain and Europe come under his scrutiny. Here, he attempts to *debunk* various myths surrounding world development such as British Hegemony, American Exceptionalism, the Changing Nature of Labor, the Dominance of Western Migration, Free Trade, Laissez Faire, the Gold Standard, Imperial Colonialism Does Not Pay, Missionaries, and the Role of Overwhelming Military Power. He contends that neither British nor other European industry was the

battering ram that forced open the walls and doors of the rest of the world, and nor was the Western availability and exercise of vastly superior military power over the East and South was not an overwhelming factor. He stresses that most of the received wisdom about the nature and causes of the wealth of nations during the nineteenth century does not stand up under the weight of the evidence. He suggests that it is time, if not past time, to revise the substantially ideological dogma of Western triumphalism and also the exaggeration of the deformation of “Third World” economies. He provides a scathing critique of ideologically inspired classical, neo-classical, and Keynesian “scientific” analysis and political propaganda by dependence and world-system theory and their alternative analyses. And, he emphasizes how location in an expanding and deepening multilateral world system is a basic, yet often ignored, factor. The unpublished analysis is iconoclastic in nature—truly vintage Frank—focusing on our (mis) understanding of nineteenth-century world development paralleling his very early work on the development of underdevelopment that forms the basis of chapter one in this book.

Frank’s writings in his Mark II period were mainly historical and structural in emphasis. In comparison to his Mark I period, there is little reference to the role of agency in his evaluation of world development. With his late wife, Marta Fuentes in the late 1980s, he identified a series of patterns and themes underlying the rise of social movements leading to global transformations. He and his wife (Fuentes) examine social movements that have appeared, disappeared, and reappeared for centuries and, in some cases, for millennia throughout the world. They explicate the countless social movements such as the Spartacist slave revolts in Rome, religious wars, peasant movements, historical ethnic and nationalist conflict throughout the continent, and women’s movements that unleashed backlashes of witch hunts and more recent forms of repression. They stress that in Asia, the Arab world, and the expansion of Islam, Africa and Latin America, multiple forms of social movements have been the agents of social resistance and transformation throughout history. For them, those movements termed Third World movements typically emerged from world economic crises, as the participants were struggling for sheer physical, economic survival and cultural identity. Frank and Fuentes maintain that virtually all religious, ethnic, and nationalist movements such as working-class and ostensibly radically oriented movements and political parties negate and sacrifice women’s interests (Frank, 1996; Lauderdale and Harris, 2008). Their analysis notes that social movements’ policies or practices that employ hierarchical and dualistic means to their ends are enemies of most women’s movements, particularly in the economically poor countries.

In Frank’s writing on social movements he also exhibited his lifelong concern with stressing equality before efficiency, and human-centricity

instead of Eurocentricity. In the end for him, in discussing world development and transformation he would conclude with the following epitaph: *A Luta Continua*. That also mirrors his lifetime of innovative scholarship.

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CHAPTER SIX

The 5,000-Year World System: An Interdisciplinary Introduction

Introduction

Our thesis is that the contemporary world system has a history of at least five thousand years. The rise to dominance of Europe and the West in this world system are only recent—and perhaps passing—events. Thus, our thesis poses a more humanocentric challenge to Eurocentrism.

Our main theoretical categories are:

1. The world system itself. *Per contra* Wallerstein (1974), we believe that the existence and development of the same world system in which we live stretches back at least five thousand years (Frank, 1990a, 1991a, b; Gills and Frank, 1990–91, 1992; Frank and Gills, 1992). Wallerstein emphasizes the difference a hyphen (-) makes. Unlike our nearly World [wide] System, World-Systems are in a “world” of their own, which need not be even nearly worldwide. Of course however, the “new world” in the “Americas” was home to some world-systems of its own before its incorporation into our (preexisting) world system after 1492.

2. The process of capital accumulation as the motor force of [world system] history. Wallerstein and others regard continuous capital accumulation as the *differentia specifica* of the “modern world-system.” We have argued elsewhere that in this regard the “modern” world system is not so different and that this same process of capital accumulation has played a, if not the, central role in the world system for several millennia (Frank, 1991b and Gills and Frank, 1990–91). Amin (1991) and Wallerstein (1991a) disagree. They argue that previous world-systems were what Amin calls “tributary” or Wallerstein “world empires.” In these, Amin claims that

politics and ideology were in command, not the economic law of value in the accumulation of capital. Wallerstein seems to agree.

3. The center-periphery structure in and of the world [system]. This structure is familiar to analysts of dependence in the "modern" world system and especially in Latin America since 1492. It includes but is not limited to the transfer of surplus between zones of the world system. Frank (1967, 1969) wrote about this among others. However, we now find that this analytical category is also applicable to the world system before that.

4. The alternation between hegemony and rivalry. In this process, regional hegemonies and rivalries succeed the previous period of hegemony. World system and international relations literature has recently produced many good analyses of alternation between hegemonic leadership and rivalry for hegemony in the world system since 1492, for instance, by Wallerstein (1984), or since 1494 by Modelski (1987) and by Modelski and Thompson (1988). However, hegemony and rivalry also mark world [system] history long before that (Gills and Frank, 1992).

5. Long [and short] economic cycles of alternating ascending [sometimes denominated "A"] phases and descending [sometimes denominated "B"] phases. In the real world historical process and in its analysis by students of the "modern" world system, these long cycles are also associated with each of the previous categories. That is, an important characteristic of the "modern" world system is that the process of capital accumulation, changes in center-periphery position within it, and world system hegemony and rivalry are all cyclical and occur in tandem with each other. Frank analyzed the same for the "modern" world system under the titles *World Accumulation 1492-1789* and *Dependent Accumulation and Underdevelopment* (Frank, 1978a, b). However, we now find that this same world system cycle and its features also extend back many centuries before 1492.

Our thesis is elaborated in a forthcoming book, tentatively entitled *The World System: From Five Hundred Years to Five Thousand*, to which this essay is the introduction. In this book, this thesis is introduced by the early contribution of Kaisa Ekhholm and Jonathan Friedman (1982). It is extended by David Wilkinson (1987) who argues that in 1500 B.C. relations between Egypt and Mesopotamia gave rise to what he calls "Central Civilization," which has incessantly spread out through the world ever since. The "one world system" thesis is then elaborated in our chapters.

Amin and Wallerstein critique this thesis and defend their thesis that the "modern world-system" began 500 years ago. They argue in particular that its capitalist mode of production distinguishes it fundamentally from "world empires" and all previous world-systems, which Amin calls "tributary." In his critical reply to us, Wallerstein emphasizes the aforementioned

distinction between his plural “world -systems” with a hyphen and our singular “world system” without an added hyphen. Janet Abu-Lughod (1989), whose work we also review below, contributes a critical discussion of these issues and defends the existence of a “thirteenth century world system,” which she regards as distinguishable as it was distinguished.

Our thesis speaks to several disciplines or concerns and participates in longstanding controversies within and between them. Among these fields and concerns, beyond world-systems theory itself, we here note our challenge to Eurocentrism. Then we outline the connections of our thesis with historiography, civilizationism, archaeology, classicism in ancient history, medievalism, modern history, economic history, macro historical sociology, political geography, international relations, development studies, ecology, anthropology, race and ethnic relations and their studies, gender relations and their study, etc. Our thesis, its similarities and differences with others, and the discussions of the same also have some important philosophical, social scientific, and political implications, which we may briefly note in conclusion.

World System Theory

We ask whether the principal systemic features of the “modern world system” can also be identified earlier than 1500 or not? Wallerstein (1974, 1984, 1989a, b, 1991a), Modelski (1987), and Amin (1991) argue that the *differentia specifica* of our world system are new since 1500 and essentially different from previous times and places. However, Modelski (1991) includes some leadership before 1500 in his analysis. Christopher Chase-Dunn (1986) and others find parallels in “other” and prior world systems. Wilkinson (1989) discovers at least some of these features also in his “Central Civilization” and elsewhere. However, he sees historical continuity, but no world system. Abu-Lughod (1989) sees a “thirteenth century world system,” but she regards it as different from the world system since 1500 or before 1250. Moreover, she is not so interested in comparing systemic features or characteristics. We combine all of the above into an analysis, or at least an identification, of the principal features of this world system over several thousand years of its history and development (Frank, 1990a, 1991a, b; Gills and Frank, 1990–91, 1992).

According to Wallerstein (1989b, c, 1988 and elsewhere) and many students of world capitalism, the *differentia specifica* of the modern world system is the ceaseless accumulation of capital.

It is this ceaseless accumulation of capital that may be said to be its most central activity and to constitute its *differentia specifica*. No previous historical system seems to have had any comparable *mot d'ordre* (1989b: 9).

Samir Amin (1991) also argues that this economic imperative is new and uniquely characterizes only the modern capitalist world system. Of course, this is not the same as arguing that capital accumulation was absent, minor, or irrelevant elsewhere and earlier. On the contrary, capital accumulation did exist and even define this (or another) world system also before, indeed long before, 1500.

Yet, Wallerstein, Amin, and most others argue that there is something very unique and uniquely powerful about modern capital, i.e. an imperative to accumulate “ceaselessly” in order to accumulate at all. We contend that this imperative, both in the familiar money form as well as other forms of capital, is not a unique systemic feature of modern “capitalism.” Rather the imperative of ceaseless accumulation is a characteristic of competitive pressures throughout world system history. Moreover, we note below the existence of cycles in economic growth, both “pre-“ and “post” “capitalist,” in the entire world system (Gills and Frank, 1992). Therefore, something more fundamental than “ceaseless” “capitalist” accumulation in its modern form seems also to be at work in world (system) history throughout the millennia.

That is also the position of Ekholm and Friedman (1982), who find “capital,” as well as the now familiar logic of imperialism to accompany the expansion of capital, already existing from very ancient times in Mesopotamia. L. Orlin (1970), for instance, refers to “Assyrian Colonies in Cappadocia” and Mitchell Allen (1984) to “Assyrian Colonies in Anatolia.” Ekholm and Friedman argue that ancient capital, particularly in its form of the accumulation of bullion (money capital), is essentially the same as capital in later, including modern, times.

In this regard, and to anticipate our review of “archaeology” later, a generation and more ago the perhaps best-known polar opposite positions were represented by Karl Polanyi et al. (1957) and Gordon Childe (1936, 1942). Polanyi is known for his deprecation of the role of markets and by extension of profit-driven accumulation. Yet even Polanyi concluded in a later essay, only posthumously published in 1975 and again in 1977, that

throughout, the external origin of trade is conspicuous; internal trade is largely derivative of external trade, . . . [and] with trade the priority of the external line is evident . . . for what we term “luxuries” were no more than the necessities of the rich and powerful, whose import interest largely determined foreign policy . . . Acquisition of goods from a distance may be practiced by a trader either from . . . (status motive)—or for the sake of gain . . . (profit motive) . . . [There are] many combinations of the two . . . (Polanyi, 1975: 154, 135, 136–137)

Gordon Childe represented the historical materialist and Marxist positions. Yet even so "Childe consistently underestimated the potential surplus that could have been generated by Neolithic economies" according to the archaeologist Philip Kohl (1987: 17). In a related vein, the well-known archaeological student of both Mesopotamia and Meso-America, Robert Adams (1974: 284), suggests "perhaps—to venture still a little further in this direction—we have wrongly deprecated the entrepreneurial element in the historical development of at least the more complex societies."

We also argue for this latter position, which is supported by more and more archeological evidence and analysis, some of which is reviewed by Sherrat (1991) and Algaze (1993). However, we wish to expand the working definition also of capital beyond the confines of current Marxism to encompass much wider manifestations of surplus transfer, both private and public. Therefore, we argue that for millennia already and throughout the world (system) there has been capital accumulation through infrastructural investment in agriculture (e.g., clearing and irrigating land) and livestock (cattle, sheep, horses, camels, and pasturage for them); industry (plant and equipment as well as new technology for the same); transport (more and better ports, ships, roads, way stations, camels, carts); commerce (money capital, resident and itinerant foreign traders, and institutions for their promotion and protection); military (fortifications, weapons, warships, horses and standing armies to man them); legitimacy (temples and luxuries); and of course the education, training, and cultural development of "human capital." Ekholm and Friedman (1982) refer to capital accumulation already in prehistoric times, and it can also be inferred from various archaeologists cited below. Even the drive to accumulate, or the obligation to do so in a competitive world, is not confined to modern capitalism.

Are other characteristics, in particular a core-periphery structure, of the modern world system unique to it since 1500? Or are they also identifiable elsewhere and earlier? In a short list of three main characteristics of his modern world system, Wallerstein (1988b) identifies "this descriptive trinity (core-periphery, A/B [cycle phases], hegemony-rivalry) as a pattern maintained over centuries is unique to the modern world-system. Its origin was precisely in the late fifteenth century" (Wallerstein, 1988b: 108).

Wallerstein also makes lists of six (1989b) and twelve (1989a) characteristics of his modern world capitalist system since 1500. Frank (1991a) argues why all of them also apply earlier. The sections on archaeology, classicism, and medievalism below show how these categories, and particularly core-periphery, are also applicable to prehistory, the ancient world, and premodern history.

Another of the three world system characteristics mentioned by Wallerstein is hegemony/rivalry. But is this feature limited to the world since 1500? Or did it also exist elsewhere and earlier? Or, indeed, does it also characterize

the same world system earlier? Wallerstein himself discusses the rise and fall of mostly economically based hegemony only since 1500.

Modelski (1987) and Modelski and Thompson (1988) as well as Thompson (1989) analyze largely politically based and exercised hegemony since 1494. Paul Kennedy's (1987) bestseller about the *Rise and Fall of Great Powers* went still farther back, but did not connect them in any systematic way.

Wallerstein employs a sequential model of hegemony which refers to productive competitiveness in other core markets, subsequent commercial competitiveness, and financial competitiveness: While this is a useful model of sequential attainment of different dimensions of hegemonic power, it leads to overemphasis on a temporary and fragile "moment" when a core power attains all three advantages simultaneously. It also confines our analysis of global hegemony too much to the single succession of a few such momentary hegemony, to the detriment of analysis of the total phenomena of global hegemony. Even when there is such a momentary hegemon, there are always other interlinked hegemonic powers. Wallerstein distinguishes modern "hegemony" from traditional "imperium." Yet all of his hegemonic powers themselves held colonial possessions and coexisted in a larger system of global hegemony in which other powers exercised imperium. Modelski (1987) and others emphasize political/military hegemony.

Our use of the term hegemony-rivalry refers to the political economic predominance by a center of accumulation, which alternates with periods of rivalry among several such centers of accumulation. Therefore, we argue that hegemony-rivalry has also characterized the world system for thousands of years (Gills and Frank, 1990–91, 1992). As suggested above, hegemony is not only political. It is also based on center-periphery relations, which permit the hegemonic center to further its accumulation of capital at the expense of its periphery, hinterland, and its rivals. After a time, not the least through the economic-military overextension signaled by Kennedy (1987), the hegemonic empire loses this power again. The decline in the hegemony of a great power gives way to an interregnum of competitive economic, political, and military rivalry among others to take its place. After an interregnum of rivalry with other claimants, the previous hegemonic power is replaced by another one. Shifting systems of also economic, political, and military alliances, reminiscent of those featured by George Orwell (1977) in his 1984, are instrumental in first creating, then maintaining, and finally losing hegemonic imperial power.

We argue that there have been not only numerous and repeated instances of hegemony and rivalry at imperial regional levels. We also suggest that we may be able to recognize some instances of overarching "super-hegemony" and centralizing "super-accumulation" at the world system-wide level before 1500 (Gills and Frank, 1990–91, 1992). The

Mongol Empire certainly, and Song China perhaps, had a claim to super-hegemony. Thus, very significantly, the later rise to super-hegemony in and of Western Europe, Great Britain, and the United States after 1500 were not unique first instances in the creation of a hegemonic world system. Instead, as Abu-Lughod (1989: 338) persuasively argues, "the fall of the East" preceded the "Rise of the West" and resulted in a hegemonic shift from East to West. This shift came at a time—and perhaps as a result—of overextension and political economic decline in various parts of the East. It suffered a period of cyclical economic decline so common to them all as to have been world system wide. Thus the "Rise of the West," including European hegemony and its expansion and later transfer to the "New World" across the Atlantic, did not just constitute a new Modern World Capitalist System. This development also—and even more so—represented a new but continued development and hegemonic shift within an old world system.

Janet Abu-Lughod (1989) makes a major contribution to the writing of world history in pushing the starting date for the world system back to 1250. In so doing she has finally cut into the gordian knot of the supposed break in world history at 1500, as per Wallerstein (1974) and others. She denies that the present world system emerged in Europe through the transition from any previous mode of production. She argues instead that whatever mode of production existed in the sixteenth century also existed already in the thirteenth century in Europe—and in the "Middle East," India, and China.

Abu-Lughod shows that eight interlinking city-centered regions were united in a single thirteenth-century world system and division of labor. According to her reading, however, this world system economy experienced its apogee between 1250 and 1350 and declined to (virtual) extinction thereafter, before being reborn in Southern and Western Europe in the sixteenth century. In her words, "of crucial importance is the fact that the 'Fall of the East' preceded the 'Rise of the West.'" She argues that

if we assume that restructuring, rather than substitution, is what happens when world systems succeed one another, albeit after periods of disorganization, then failure cannot refer to the parts themselves but only to the declining efficacy of the ways in which they were formerly connected. In saying the thirteenth-century world system failed, we mean that the system itself devolved. . . . From earliest times, the geographically central "core regions" . . . were Central Asia and the Indian Ocean, to which the Mediterranean was eventually appended. These cores persisted through the classical and thirteenth-century world systems. A decisive reorganization of this pattern did not occur until the sixteenth century. (Abu-Lughod, 1989: 343–345)

It seems at least plausible, if not obvious, then to argue that between the fourteenth-century decline of the East and the fifteenth–sixteenth century rise of the West there occurred a “declining efficacy” and “disorganization” of “the ways in which they were formerly connected.” In that case, consequently there would have been a shift of the center of gravity in the system from East to West—but not a complete failure of the system as a whole. On the contrary, this temporary disorganization and renewed reorganization could and should be read as the continuation and evolution of the system as a whole. Indeed, in our approach all history can and should be analyzed in terms of the shifts in centers of accumulation, as we emphasize in our titles “World System Cycles, Crises and Hegemonial Shifts 1700 BC to 1700 AD” (Gills and Frank, 1992) and “1492 and Latin America at the Margin of World System History: 992–1492–1992 and East-West Hegemonial Shifts” (Frank, 1992a).

Thus, Wallerstein (1989b) sees a single cycle in Europe (albeit “matched by a new market articulation in China...[in] this vast trading world-system,”) and yet a variety of “unstable” systems around the world, each of which “seldom lasted more than 4–500 years” (1989b: 35). On the other hand, Abu-Lughod (1989) sees a single world system, certainly in the thirteenth-century cyclical conjuncture on which she concentrates, but also in earlier periods. Yet, successively each of her world systems cyclically rise (out of what?) and decline (into what?). However, neither Wallerstein nor Abu-Lughod is (yet?) willing to join their insights in the additional step to see both a single world system and its continuous cyclical development.

The third characteristic of Wallerstein’s world system after 1500 is long economic cycles of capital accumulation. Their upward “A” and downward “B” phases generate changes of hegemony and of position in the center-periphery-hinterland structure. These cycles, and especially the Kondratieffs, play important roles in the real development of the world system and in its analysis by Wallerstein (1974), Frank (1978a), Modelski (1987), Goldstein (1988), and Thompson (1989). All emphasize the relations among cycles in the economy, hegemony, and war. However, are these cycles limited to modern times, or do they extend farther back? Wallerstein himself notes that

[i]t is the long swing that was crucial.... The feudal system in western Europe seems quite clearly to have operated by a pattern of cycles of expansion and contraction of two lengths: circa 50 years [which seem to resemble the so-called Kondratieff cycles found in the capitalist world economy] and circa 200–300 years.... The patterns of the expansions and contractions are clearly laid out and widely accepted among those writing about the late Middle Ages and early modern times in Europe.... It is the long swing that was crucial. Thus

1050–1250 + was a time of the expansion of Europe (the Crusades, the colonizations). . . . The “crisis” or great contractions of 1250–1450+ included the Black Plague. . . . (1989b: 33, 34)

Thus, even according to Wallerstein there was systematic cyclical continuity across his 1500 divide—in Europe. But Abu-Lughod (1989), McNeill (1983), and others offer and analyze substantial evidence that this same cycle was in fact world system wide. Wallerstein (1989b: 57, 58) also perceives some of the evidence. Moreover, all these developments were driven by the motor force of capital accumulation. The “crucial long swing” was a cycle of capital accumulation. Frank (1991b) tries to demonstrate that this same cyclical pattern definitely extends back through the eleventh century and that it could well be traced further back as well. Gills and Frank (1992) trace these long cycles much further back to at least 1700 BC in world (system) history.

So do these characteristic similarities with the “modern-world-capitalist-system” extend only to “other” earlier empires, state systems, regional economies, or different “world systems”? Gills and Frank (1990–91, 1992) argue that similar characteristics extend backward through time in the same world system, which itself also extends much farther back in time. That is, we argue for the extension back in time through the same world system of the essential features of the “modern-world-capitalist-system” of Wallerstein (1974), Frank (1978b), Modelski (1987), Goldstein (1988), Thompson (1989), and others, and of the “other” world systems and civilizations of Chase-Dunn (1986, 1989), Wilkinson (1987, 1989), and others. This extension of the world system to at least five thousand years has implications for many disciplines and concerns in history and social science, beginning with historiography and the Eurocentrism that underlies much of its other “scientific” and cultural endeavors.

Eurocentrism and Its Alternatives

Samir Amin (1989) in *Eurocentrism* and Martin Bernal (1987) in his *Black Athena: The Afroasiatic Roots of Classical Civilization* recently criticized Eurocentrism and offered alternative approaches especially on an ideological level, which center on the Eastern Mediterranean and North Africa, respectively. Another alternative to Eurocentrism is the development of “Afrocentrism” by African American historians and others in the United States, which as its name implies centers on Africa, specifically including Sub-Saharan Africa. We believe that these critiques of Eurocentrism are all to the good, but that they are too limited.

Our approach offers the basis for a wider world historic humanocentric alternative to Eurocentrism. World history should be a reflection and

representation of the full diversity of human experience and development, which far exceeds the limited and limiting recent bounds of the "West." Indeed, the "West" does not exist, except by reference to the "inscrutable" "East." Yet their historical existence is only a figment of "Western" imagination. Eurocentrism and other centrisms prevent seeing or even asking how all the "parts" related to the world [system] whole. Therefore, Eurocentrism is also an analytical fetter on world history.

A few generations ago, even some Western historians like Frederick Teggart (1939: 248) in 1918 criticized "Eurocentric" history and pleaded for a single "Eurasian" history in which "[t]he two parts of Eurasia are inextricably bound together. Mackinder has shown how much light may be thrown on European history by regarding it as subordinate to Asiatic.... The oldest of historians (Herodotus) held the idea that epochs of European history were marked by alternating movements across the imaginary line that separates East from West."

Yet since then, Western domination in power and technology has further extended the domain of its culture and Eurocentric Western perspective through proselytizing religion, mass media, language, education, yes, and "world" history writing and teaching, also using the (in)famous Mercator projection maps, etc. Nonetheless, homogenization has proceeded less far and fast than some hoped and others feared; and many people around the world are seeking renewed and diverse self-affirmation and self-determination: "Think globally. Act locally." Some scholars also speak of this problematic in terms of "globalization-localization" (Featherstone, 1991; King, 1991; Lash and Urry, 1987; Robertson, 1990).

Western Eurocentric world history and its distortions need not be replaced by "equal time" for the history of all cultures. Nor need we admit (a variety of competing) other centric histories, be they Islamo-, Nippo-, Sino-, or whatever other centric. No, we can and should all aspire to a nonexclusivist humanocentric history. This world history can be more than an historical "entitlement program," which gives all (contemporary) cultures or nationalities their due separate but equal shares of the past. Instead, a humanocentric history can and must also recognize our historical and contemporary unity in and through diversity beyond our ideological affirmations of cultural self.

World Historiography

Although we should not aspire to "equal time" in the history of everybody in the world, world history also need not just concentrate on adding representative "non-Western civilizations" and cultures to Western ones. Nor should we limit our historical study of cultures and civilizations to

the comparative examination of their distinctive and common features. This is the procedure of most so-called courses and textbooks on "world" history or "comparative civilizations."

Some examples of these approaches and their internal contradictions and limitations are examined in Frank (1990a). Two well-known examples to be examined below are the comparative studies of civilizations by Toynbee and Quigley. Another example is the approach to "Civilization as a Unit of World History" by Edward Farmer (1985) and Farmer et al. (1977) in their *Comparative History of Civilizations in Asia*.

We argue that our world history can and should also make efforts to connect and relate the diversity of histories and times to each other. It may be empirically possible, and in that case it is historically important, to uncover all sorts of historical connections among peoples and places, not only over time but especially at the same time. These connections would lend additional meaning to our comparisons. Frederick Teggart (1939) made such connections, for instance, in his *Rome and China: A Study of Correlations in Historical Events*. Teggart correlated and connected diverse political and economic events (particularly wars, "barbarian" invasions, and interruption/resumption of trade) in these two areas and others in between. Teggart made these connections among contemporaneous events "for the purpose of gaining verifiable knowledge concerning 'the way things work' in the world of human relations...in the spirit of modern scientific work, on the study of World History" (Teggart, 1939: v, xii, and see below).

A one world history should also seek to systematize these connections and relations, as well as comparisons, into an analysis of a world system history. This is now the opinion of our contemporary dean of world history, William McNeill (1990). Recently, he reflected back over "The Rise of the West after Twenty Five Years" and concluded that

[t]he central methodological weakness of my book is that while it emphasizes interactions across civilizational boundaries, it pays inadequate attention to the emergence of the ecumenical world system within which we live today....Being too much preoccupied by the notion of "civilization," I bungled by not giving the initial emergence of a transcivilizational process the sustained emphasis it deserved....In the ancient Middle East, the resulting interactions...led to the emergence of a cosmopolitan world system between 1700 and 500 BC....There is a sense, indeed, in which the rise of civilizations in the Aegean (later Mediterranean) coast lands and in India after 1500 BC were and remained part of the emergent world system centered on the Middle East....All three regions and their peoples remained in close and uninterrupted contact throughout the

classical era....[Moreover] one may, perhaps, assume that a similar [to the modern] primacy for economic exchanges existed also in earlier times all the way back [to] the earliest beginnings of civilization in ancient Mesopotamia.... (McNeill, 1990: 9–10, 12–14)

Thirty-five years earlier, Marshall Hodgson (1954) had already pleaded:

During the last three thousand years there has been one zone, possessing to some degree a common history, which has been so inclusive that its study must take a preponderant place in any possible world-historical investigation.... The various lands of urbanized, literate civilization in the Eastern Hemisphere, in a continuous zone from the Atlantic to the Pacific, have been in commercial and commonly in intellectual contact with each other, mediately or immediately. Not only have the bulk of mankind lived in this zone, but its influence has emanated into much of the rest of the world (Hodgson 1954:716).

[In] the following approach...events may be dealt with in their relation to the total constellation of historical forces of which they are a part.... This means that we are to consider how events reflect interdependent interregional developments. (Hodgson, 1954: 716, 717)

Hodgson (1958: 879) thought that “few scholarly tasks are more urgent.”

This same theme was taken up by L. S. Stavrianos (1970: 3–6) in *The World to 1500: A Global History*. In the “Introduction: Nature of World History” he wrote:

The distinctive feature of this book is that it is a history. It deals with the entire globe rather than some one country or region. It is concerned not with Western man or non-Western man, but with all mankind.... The global approach to history represents a new departure in modern historiography.... The story of man from its very beginnings has a basic unity that must be recognized and respected. Neither Western nor non-Western history may be properly comprehended without a global overview encompassing both. Only then is it possible to perceive the interaction amongst all peoples at all times, and the primary role of that interaction in determining the course of human history....

World history is not the sum of histories of the civilizations of the world.... The structure of world history requires focusing on historical movements that have had major influence on man’s development,

so the geography of world history requires focusing on those regions that initiated those historical movements. When this is done, one land unit stands out uniquely and unchallengeable: Eurasia, the veritable heartland of world history since Neolithic times. . . . To an overwhelming degree, the history of man is the history of these Eurasian civilizations. . . . (Stavrianos, 1970: 3–6)

In Volume I, No. 1 of the new *Journal of World History*, Allerdycy (1990: 62, 67, 69) quoted others to the effect that what world history “needs is a simple, all-encompassing, elegant idea, which offers an adequate conceptual base for a world history.” We suggest that the basic elements of this idea may be found in the foregoing quotations from McNeill, Hodgson, and Stavrianos. The central concept of this all-encompassing idea advanced here is the process of capital accumulation in the world system.

This approach requires the rejection of still another historiographic tradition. We should not treat historical diversity and comparisons like Perry Anderson (1974). He goes beyond comparing the same or similar historical processes and formations such as absolutism at different times. He also argues explicitly that “there is no such thing as a uniform temporal medium: for the times of the major Absolutism . . . were precisely, enormously diverse . . . no single temporality covers it.” Instead, the systematization of interregional world history must realize as Hodgson (1954: 719) argued that “What is important is the recognition . . . that there has been some sort of developing pattern in which all these interregional developments can be studied, as they are affected by and in turn affect its elements as constituted at any one time.”

Frank (1978a: 20) already argued that

Anderson’s apparent attempt to make historiographic virtue out of empirical necessity when he argues that the historical times of events are different though their dates may be the same must be received with the greatest of care—and alarm. For however useful it may be [comparatively] to relate the same thing through different times, the essential (because it is the most necessary and the least accomplished) contribution of the historian to historical understanding is successively to relate different things and places at the same time in the historical process.

Much earlier already, Teggart (1939) had long since

establish[ed] (for the first time) the existence of [temporal] correlations in historical events . . . which exhibits the relationship between contemporaneous disturbances in several areas . . . [and] awareness of

the concurrence of events in different regions.... The study of the past can become effective only when it is fully realized that all peoples have histories, that these histories run concurrently and in the same world, and that the act of comparing is the beginning of knowledge.... It at once sets a new problem for investigation by raising the question of how the correspondences in events are to be accounted for. (Teggart, 1939: 243, 245, 239, emphasis in the original)

Therefore, we should discard the usual Western Eurocentric rendition of history, which jumps discontinuously from ancient Mesopotamia to Egypt, to "classical" Greece and then Rome, to medieval Western Europe, and then on to the Atlantic West, with scattered backflashes to China, India, and so on. Meanwhile, all other history drops out of the story. Or peoples and places never even appear in history, unless they are useful as a supposedly direct descendant of development in the West.

Instead, any world history should try to trace and establish the historical continuity of developments between then and now in the world systemic whole and all its parts. Hodgson and McNeill already emphasized this continuity. David Wilkinson (1987) puts Hodgson's early suggestion into practice and demonstrates convincingly that "Central Civilization" has a continuous and expanding (we would say world system) history since Mesopotamia and Egypt established relations in about 1500 B.C. We return to his thesis below.

We argue that these relations extend even farther out and further back. During another millennium from 2500 B.C. or earlier already, peoples established relations with each other around and through the Mediterranean to the Levant, Anatolia, Mesopotamia, and importantly on to the Persian Highlands and between them and the Indus Valley, as well as with many Central Asian "nomads" and others. Gordon Childe (1942) already argued for the recognition and analysis of these and even earlier and more widespread such relations in neolithic times.

Moreover, world (system) history is not limited to that of sedentary "civilizations" and their relations. It also includes "barbarian" nomads and others, and especially the multifarious relations among the former and the latter. Following Lattimore (1962) and others, we make a strong plea for much more study of Central and Inner Asian "nomadic" and other "peripheral" peoples. We commend special attention to the significance of their continuous trade and political relations with their "civilized" neighbors, and to the timing and causes of the recurrent waves of migratory and invasive incursions from Central/Inner Asia into East, South, and West Asia and Europe. Similarly, the nomadic tribes of the Arabian Peninsula merit more attention long before the time of Mohammed. Moreover, it is high time to drop and take exception to the now pejorative term "barbarian."

The supposed difference between peoples who have been so called and those supposedly more “civilized” are doubtful at best. There is even reason to question many supposed distinctions between “nomad” and “sedentary” peoples. However that may be, there can be little doubt about “*The Centrality of Central Asia*” in world (system) history (Frank, 1992c).

Africa has also received less attention than it merits in world (system) history. Curtin has done pioneering work on trade and migration in Africa, but in his *Cross Cultural Trade in World History* (1984) he has not sought to pursue the African connection in Afro-Asia as far back in history as it may deserve. The South East Asian peoples and their history were long since intimately related to and also influential on those of China and India, if only for the trade and migrations with and between them. Yet South East Asia is often largely omitted from even those world histories, which give their due to China and India.

Civilizationism

Civilizationists and many historians as well as macro sociologists claim to write the history of the world, but without ever attempting to do world history. They distinguish various civilizations or other systems, and sometimes study one problem or another, like ideology, power, economy, technology, etc. Toynbee (1946), Quigley (1961), and more recently Mann (1986) are among them.

Arnold Toynbee (1946: 34–40) finds nineteen or twenty-one separate civilizations, five still living and sixteen dead, though “most of them [were/are] related as parent or offspring to one or more of the others.” He rejects “the egocentric illusion [of] the misconception of the unity of history—involving the assumption that there is only one river of civilization, our own.” We should indeed reject this Euro/Western egocentric illusion, but it is Toynbee’s misconception to assume that there cannot have been or be a single unifying river unless it was “our” Western or another civilizational river. We suggest there was much of a common river and unity of history in a single world system. However, it was multicultural in origin and expression; which has been systematically distorted by Eurocentrism.

Toynbee also rightly rejects “the illusion of ‘the unchanging East.’” “The East” has no historical existence. Indeed, it was a Euro/Western centric invention. Moreover of course, the many peoples and regions of “the East” have been very different and ever changing. This fact and reading of history need and should not, however, exclude these peoples and regions from participation in a common stream of history or historical systemic unity.

Thirdly, Toynbee rightly rejects “the illusion of progress as something which proceeds in a straight line.” Leaving the criterion of progress or

not aside for the moment, we can nonetheless observe cyclical ups and downs in parts of the system and maybe in the whole system itself (Gills and Frank, 1992).

Finally, Toynbee rejects the “very different concept of the unity of history,” as the diffusion of Egyptaic civilization over thousands of years. We accept the rejection of this diffusion, but not his unwarranted rejection of the unity of history or of a single historical world system.

Carroll Quigley (1961) devotes more attention than Toynbee to the interrelations and mutual influences among civilizations and their rise and declines through their seven stages of mixture, gestation, expansion, conflict, universal empire, decay, and invasion. Nonetheless, he still recognizes sixteen separate civilizations. Thus, Quigley also writes a history of the world without attempting to write world history. Instead, he emphasizes their separate internal logics of development through a purportedly “universal” pattern of stages.

David Wilkinson (1987), by contrast, writes a more unitary history about what he calls “Central Civilization.” It began in the West Asian part of the Eurasian landmass and spread eventually to encompass the entire globe: “Central Civilization is the chief entity to which theories of class society, the social system, world-economy, and world systems must apply if they are to apply at all. A suitable theoretical account of its economic process does not yet exist; one for its political process may. . . .” (56–57).

Wilkinson’s subtitles indicate his intent and recommended procedure: “Recognizing Central Civilization as a Reality. . . . Recognizing a single entity in adjacent ‘civilizations’ . . . Recognizing a single entity after civilizations collide. . . . Recognizing a single entity when ‘civilizations’ succeed each other. . . . Did Central Civilization Ever Fall?” (35–39).

Wilkinson’s answer is no, since its birth when Sumer and Egypt joined hands around 1500 B.C. Therefore, Chase-Dunn and Hall (1991) have suggested that we should “adapt Wilkinson’s terminology and call their system the ‘Central World System.’”

However, we are wary about the category of “civilization” itself. “Civilization” is ambiguous as a unit and terribly difficult to bound either in space or in time. When McNeill says he “bungled” by being too preoccupied with civilization as the unit of analysis, it was because it stands in the way of seeing and analyzing world (system) history as a whole.

Archaeology

As already observed in our discussion of capital accumulation and the role of markets and entrepreneurship in ancient history, the field was long dominated by the work of scholars such as Moses Finley (1985, original

1975) and Karl Polanyi et al. (1957). Both deny or downplay the role of the market relations in the ancient economy, and by implication the scope for "capital" accumulation. Ekholm and Friedman (1982) provocatively attempted to expand world system analysis to the ancient economy and to break with this predominant view. They put forward a bold thesis on the continuity of "capital" and imperialism in the ancient world. Archeological critiques of Polanyi, in particular by Silver (1985), Kohl (1989), Woolf (1990), and Sherrat (1991) reexamine the evidence. Archaeologists find ample empirical evidence of capital formation and for the operation of true price-setting markets in the ancient economy. Gills and Frank (1990–91, 1992) rely on this evidence to systematize their reading of the role of capital accumulation and markets in the ancient world system.

Yet, all too often, historians and others have operated with the simplistic assumption that ancient states and empires were purely extractive, expropriating mechanisms. Anderson (1974) emphasizes the primacy of the political/coercive means of extraction of surplus in precapitalist social formations. Amin (1989, 1991) similarly emphasizes the ideological and political-extractive character of surplus extraction in the "tributary" modes of production. We believe that the emphasis on these characterizations of ancient political economy are distorting. There is growing evidence of the vital and widespread role of private merchant capital and "free" imperial cities in generating the revenues on which the state lived in even the most militaristic and coercive of the ancient empires, Assyria, not to mention the more famous Phoenician commercial interests. What holds true for Assyria holds equally true for every other ancient empire and even China, though there perhaps to a somewhat lesser extent. Once this is recognized, the way is open to new studies of the trans-regional economic processes involving the transfer of goods and capital across ancient Eurasia and their effects "within" all the ancient empires.

Nonetheless, much of the work so far remains either civilizational or comparative civilizational in scope and conception. The leap to applying center-periphery and world system conceptual frameworks to the wider geographical, social, and economic contexts we believe to exist has yet to be fully accomplished. There are a few glimmers of light on the horizon in this regard, for instance, Sherrat's (1991) paper on the Bronze Age "world system" and McNeill's (1990) comments on the scope and significance of economic relations in the ancient world system quoted earlier. We believe that there is good reason to encourage this nascent trend to analysis at the largest scale possible given the state of the archeological and historical evidence as the logical extension of the method and theses we advocate over the entire course of world history.

However, a new wave in archaeological studies has recently appeared. It applies center-periphery and/or world systems analysis to the study of

complex societies of the past. Thus, Rowlands, Larsen, and Kristiansen, eds. (1987) entitled a book *Centre and Periphery in the Ancient World*; Champion (1989) edited one on *Centre and Periphery: Comparative Studies in Archaeology*; Chase-Dunn and Hall (1991) on *Pre-Capitalist Core-Periphery Relations*; Greg Woolf (1990) discusses "World-Systems Analysis and the Roman Empire," and Andrew Sherratt writes of "Core, Periphery, and Margin: Perspectives on the Bronze Age" (n.d.) and asks "What Would a Bronze Age World System Look Like?" (1992). Thus, much of this new literature and its very titles about ancient and "precapitalist" societies or "worlds" make the growing recognition explicit that it is not only possible, but analytically fruitful, to apply concepts developed for the analysis of the modern world also to the "premodern" and indeed the "prehistorical" world.

Progress in this direction has, however, been limited by the attempt to apply Wallersteinian categories too rigidly and/or by confining them to "world-systems" of excessively narrow scope. Guillermo Algaze (1993), for instance, comparatively examines "Prehistoric World Systems, Imperialism, and The[ir] Expansion" in each of Egypt, Southern Mesopotamia, and the Indus Valley, as well as Central Mexico. Yet he does not consider the connections among the first three, as well as among them and Northern Mesopotamia, Anatolia, the Levant, Persia, and Central Asia, which are examined in Gills and Frank (1992). George Dales (1976) probed the "Shifting Trade Patterns between the Iranian Plateau and the Indus Valley in the Third Millennium B.C." Hiebert and Lamberg-Karlovsky (1991) in turn examine the relations between "Central Asia and the Indo-Iranian Borderlands." Shereen Ratnagar (1981) explored *Encounters: The Westerly Trade of the Harappan Civilization* with Mesopotamia.

Philip Kohl (1991) also examines the connections between Persia and Transcaucasian Central Asia, and between that in turn and the Indus Valley. He sees parallels and shifts of center of gravity between the latter, but is reluctant to probe possible causal interrelations. Kohl (1987, 1989, 1991) has also written several times about center-periphery relations and "the use and abuse of world systems theory" regarding these areas. He concludes that "these Central Asian materials cannot easily be incorporated into an unmodified Wallersteinian world systems model... Economic development and dependency were not linked phenomena during the Bronze Age... Central Asia clearly interacted with South Asia and Iran in the late third millennium, but it was neither a core, a periphery, nor semi-periphery" (Kohl, 1989: 235, 236, 237). Moreover, among others, Kohl also stresses the maritime connections with Oman.

From our perspective, all of these structures and processes, as well as the specific historical events, can and should be studied as part of a single world system process. It seems particularly opportune to do so when, just

as we write, even the popular press (front-page headline in the *International Herald Tribune*, February 6, 1992) reports the use of satellite observation to make the “major new find... of the Omanum Emporium” at or near “Ancient Arabia’s Lost City” of [Omanian] Ubar, which was the center of the overland and maritime frankincense trade with most of the areas we just discussed. Only extension and adaptation of world system analysis to earlier times can offer analytical categories, which are essential to analyze all this in its then contemporary Bronze Age systemic interrelations. Moreover, we agree with archaeologists such as Kohl who suggest that the age-old inquiry into the origins of the ancient state also must be reoriented to take account of “international relations.” However, these relations were rival and competitive for economic suzerainty, and not only on a bilateral basis but within an “interstate” world system. We return to this matter in our sections on international relations and anthropology below.

Classicism in Ancient History

In Classicism, Eurocentricity as noted earlier, has been recently powerfully criticized by Martin Bernal (1987) and Samir Amin (1989). Both argue that ancient Greece was less the beginning of “Western” than the continuation of “Eastern” civilization and culture. However, we would caution against misuse of Bernal’s work by some of his new “Afrocentrist” interpreters. Similarly, “poly-centrism” can be misused by multiculturalist counterattacks on Eurocentric culture.

On a more material level, the archaeologists Andrew and Susan Sherratt insist similarly about Aegean civilization that “its growth can only be understood in the context if its interaction with these larger economic structures” in the Levant and “behind them stood the much larger urban economies of Mesopotamia and Egypt” where for “already 2000 years... the easterners had the gold, the skills, the bulk, the exotic materials, the sophisticated lifestyle, and the investment capacity” (Sherratt, 1991: 355). Why else, Gills and Frank (1992) ask, would Alexander have turned East to seek his fortune?

Our world system perspective not only reinforces the Amin and Bernal ideological critique of Eurocentrism, but carries it much further still. We also offer an analytic context, indeed a framework, within which to perceive and analyze the “interaction with these larger structures” by Greek, Roman, and other “civilizations” in “classical” times. Thus, our perspective offers a powerful antidote to the Eurocentric classical historians, who were so successful that they imposed their bias upon the ancient world by privileging the role of Graeco-Roman civilization in the story of world history. The contributions of non-Western, and particularly “Oriental,”

societies were systematically denigrated or dismissed as unimportant. Most importantly, Eurocentric classicism distorted the real political and economic position of the "West"—that is, the Graeco-Romans, in the ancient world as a whole. Yet we know that Hellas began its ascendance after a preparatory period of so-called Orientalizing, that is, emulating and integrating with the more advanced and prosperous centers of civilization and commerce in the "East."

The Eurocentric distortions of classicism in ancient history can best be corrected by applying a world system approach in which all the major zones of ancient Eurasia are analyzed on the basis of their participation in a common economic process. Culturalism and the assumption of Western superiority have distorted analyses of the true world historical position and relations of the West European and West Asian (Middle Eastern) regions. A world system framework clarifies that for most of world history, including ancient "classical" history, Europe was ever "marginal" and West Asia ever "central."

The ultimate center of economic gravity in this ancient world remained in the East even after the rise of Hellas, which is well attested to in the history of the Hellenistic kingdoms. It can be argued that, even when Rome ascended to political predominance over these Hellenistic kingdoms, the real economic core of this pan-Mediterranean-Oriental world system nevertheless decidedly remained in the East, whilst Rome itself played a largely parasitic role. The historical evidence corroborates the contention that the real position of the West relative to that of the East has been misunderstood.

Witness the evidence from the ambition of Antony and Cleopatra to rule this world from the East; to the secession of Queen Zenobia in the third century; to the founding of Constantinople as the Eastern capital, and its subsequent centuries-long tenure as the premier economic metropole of the East. Indeed, the so-called fall of the Roman Empire was of course mainly confined to the economically far weaker Western provinces. It was primarily Eurocentric bias and privileging of Graeco-Roman civilization that produced the quite false dichotomy between the "fall" of Rome and the subsequent Byzantine Empire. The latter, of course, was the same Roman Empire; and it only retrenched and regrouped in its economic core in the East.

The true position and relations of the West European and West Asian (Middle Eastern) region have been even less analyzed within the context of the entire Eurasian economic world. Teggart (1939) established a model for how such a task might be accomplished. Such a project would need to incorporate the ancient history of every major region in Eurasia, especially those of China, India, Central Asia, and South East Asia. Our world system history offers a framework to do so. In that framework as in

world historical reality, Europe was ever marginal and West Asia central. Gills and Frank (1991) discuss a Eurasian-wide pattern of correlations in economic expansion and contraction and hegemonic rise and decline during the ancient period. The attempt is to explore the synchronization and sequentialization of these patterns between all the major zones of ancient Eurasia, on the working assumption that they participated in a common world accumulation process.

Medievalism

Most study of medieval history is also extremely Eurocentric. The famous "Dark Ages" refer explicitly to Europe, indeed to Western Europe. However, the implication is that either the rest of the world also experienced centuries of the same; or worse, that it did not exist at all or, if it did so, there were no connections between (Western) Europe and the remainder of the world. All of these theses and their implications are directly challenged by Gills and Frank's (1992) study of the Afro-Eurasian world system during "medieval" times.

In terms of twentieth-century European sociological historiography, the dispute could be summarized through the polar opposite positions of the contemporaries Max Weber and Werner Sombart. The archaeologists Andrew and Susan Sherratt (1991) identify this contrast with regard already to the ancient world. However, it also applies to medieval times; or rather, perhaps it was projected backward by Weber and Sombart from their study of medieval times and indeed from their concern with modern capitalism. Weber and Marx were antagonists in their interpretation of capitalism and in the theoretical apparatuses they bequeathed to twentieth-century social science and history. However, they were tactical allies with regard to their interpretation of the preceding medieval times, from which however differently both sought to distinguish modern capitalism. They saw medieval Europe as sunk in a Dark Age hole of immobility, which was closed in upon itself. For them and for their many and mutually antagonistic followers through most of the twentieth century, Europe was characterized by small-scale and agrarian feudal fiefdoms based on master-serf relations. The most important exponent of similar theses among historians was perhaps Marc Bloch. All of these followed in turn Edward Gibbon's renowned *Decline and Fall of the Roman Empire* from the eighteenth century and European Renaissance writers before that.

A contrary thesis was developed and defended by Sombart (1967, 1969), who laid much greater emphasis on commercial developments, by Alfons Dopsch (1918), and to some extent by Henri Pirenne (1936)

and Henri See (1926). Dopsch emphasized the continued importance of trade after the decline of the Roman Empire in the West and denied that Europe involuted completely. Pirenne recognized the integration at least of Western Europe in the Age of Charlemagne. Though See, like Marx and Weber, was concerned with *The Origins of Modern Capitalism* he identified many medieval commercial precursors, also in the Church. Sture Bolin argued against Pirenne and suggested that without Mohammed—or indeed Rurik, the Swedish invader of Russia—there could have been no Charlemagne. That is, medieval Western Europe was systemically related to Eastern Europe and Islam. (For a discussion of these theses, see Adelson, 1962.) The important place and role of Venice and Genoa in late medieval Europe were derived from their connections with the Byzantines and others in the “East.” The Crusades went there because that was where the action was, while Europe still was in a backwater of world system history.

However, even if we start in Europe as we should not, these observations lead us much farther afield. The importance of the commercial and monetary ties between Europe and Islamic lands is emphasized, among others, by Maurice Lombard (1975). He rightly terms the medieval centuries as *The Golden Age of Islam*. Marshall Hodgson (1974) sees medieval Islam as the veritable center and hub of a flourishing Eurasian ecumene, while (Western) Europe—and by Eurocentric extension the world?—supposedly languished in “dark ages.” K. N. Chaudhuri (1985 and 1990) goes on to analyze the medieval splendor in *Asia before Europe*. Countless historians of China have studied the rise and decline of the Sui, Tang, and Song dynasties; and the world historian William McNeill (1983) ascribes world preeminence to the latter in the late Middle Ages. Christopher Beckwith (1987) insists on the systemic connection among all of these regions and other regions, in particular Central Asia including Tibet, and their polities throughout the medieval period. Gills and Frank (1992) and Frank (1991c) rely heavily on all of these authors to construct their analysis of the world system during the medieval period.

From a world system perspective medieval Europe was socially, politically, and economically quite backward or less developed in comparison with the contemporary cores in the world system, all of which lay to the East. Perhaps no other region in Eurasia suffered so deep and prolonged a retrogression after the classical period. In this sense, medieval Europe was an exception rather than the rule, and Eurocentric preoccupation with feudal social forms distorts our appreciation of real social, political, and economic development in the world as a whole during those centuries. Thus in this regard also, Eurocentrism distorts our understanding of human history.

From Early Modern to Modern History

Early modern history is variously dated more or less from the thirteenth to fifteenth centuries, depending on the specific historical topic under review. These include but are not confined to the following more or less contemporaneous or temporally overlapping events: The War of the Roses in Tudor Britain and/or the Hundred Years War in Europe, the Renaissance in Europe, Norman expansion southward through Europe, the end of the European Crusades, European expansion westward through the Mediterranean into and then across the Atlantic, Mameluk rule in Egypt, the decline of the Byzantine Empire, the rise of the Ottoman Empire and its expansion westward, Mongol expansion in all directions, the Black Death, the rise of the Safavid Empire in Iran, India before and during the Moslem conquest, the Yuan dynasty in China and then its replacement by the Ming dynasty, and farther afield perhaps the Mali Empire in West Africa, the rise of the Incas in Peru and of the Aztecs in Mexico. At best, some of these events or empires are treated comparatively, as in the "Early Modern Seminar" at the University of Minnesota led by Edward Farmer, whose approach was discussed earlier. Yet all of them are treated either independently of each other or at most in relation to their immediate neighbors.

Per contra, in our interpretation of the world system, at least all of these Eurasian events would be supposed, if not treated, as having been interlinked and related to each other. Gills and Frank (1992) do not treat the Mongol expansion and the Black Death as arising *Deus ex Machina* out of nowhere and their impact on and the reactions in China, India, Persia, and Europe as isolated instances. Instead, we treat all of these events and others as integral parts of an integrated Eurasian-wide world system and historical process. Exceptionally, Janet Abu Lughod's (1989) *Before European Hegemony* does the same. She treats eight of these areas as interlinked across Eurasia during the years 1250–1350. We already commented on her work in connection with "world system theory" earlier.

Palat and Wallerstein (1990) speak of an "evolving Indian Ocean world economy," which combined a set of intersecting trade and production linkages from Aden and Mocha on the Red Sea, and Basra, Gombroon, and Hormuz on the Persian Gulf, to Surat and Calicut on the western seaboard and Pulicat and Hughli on the Coromandel and Bengal coasts of India, Melaka on the Malay archipelago; and the imperial capitals such as Delhi and Teheran, connected by caravan trails. They "lived at the same pace as the outside world, keeping up with the trades and rhythms of the globe" (Palat and Wallerstein, 1990: 30–31; also Braudel, 1982: 18).

Nevertheless, Palat and Wallerstein insist that three autonomous historical systems existed: the Indian Ocean world-economy, which centered

around China, and the Mediterranean/European zones, which merely converged at intersections. Yet they note the "swift collapse of these cities once their fulcral positions were undermined." But they would have it that "their riches accumulated from their intermediary role in the trade between different world-systems" rather than acknowledging the existence of a single world economy. Furthermore, Palat and Wallerstein conclude that

despite the temporal contemporaneity of post-1400 expansion of networks of exchange and intensification of relational dependencies in Europe and in the world of the Indian Ocean, the processes of large-scale socio-historical transformation in the two historical systems were fundamentally dissimilar. In one zone, it led to the emergence of the capitalist world-economy. In the other, to an expanded petty commodity production that did not lead to a real subsumption of labour. (Palat and Wallerstein, 1990: 40)

Gills and Frank (1992) regard this as an excessively nearsighted view.

Per contra other students of the world system therefore, if other parts of the world have been the most important players in the same world system earlier on, some of these players still were important in the same world system after 1492 as well. Therefore, it is necessary to rephrase (or repose?) the question of "incorporation" into the system as perceived by Hopkins and Wallerstein in their 1987 issue of *Review* dedicated to "Incorporation into the World-Economy: How the World-System Expands." Moreover, the hegemony first of Iberia in the sixteenth century and then of the Netherlands in the seventeenth, as well as the relative monopolies of trade on which they were based, came at the expense of still operative trading powers, e.g., the Ottomans and Indians, among others.

However, beyond the retreat into greater isolation of China under the Ming at one end of Eurasia, another major reason that this historical development eventually became a more unipolar rather than a multipolar transition is explained by J. M. Blaut (1977) with reference to the other end: The Western European maritime powers conquered the Americas and injected its bullion into their own processes of capital accumulation. The Western powers then used the same to gain increasing control over the trade nexus of the still attractive and profitable Indian Ocean and Asia as a whole. Yet as late as 1680 the English mercantilist Sir Josiah Child still observed that "we obstruct their [Mogul Indian] trade with all the Eastern nations which is ten times as much as ours and all European nations put together" (cited in Palat and Wallerstein, 1990: 26). In that case, what was really in or out of the world system, what were its essential features, and when did these features and the world system itself begin?

In this regard, an argument similar to ours was already made by Jacques Gernet in his *History of China*:

[W]hat we have acquired the habit of regarding—according to the history of the world that is in fact no more than the history of the West—as the beginning of modern times was only the repercussion of the upsurge of the urban, mercantile civilizations whose realm extended, before the Mongol invasion, from the Mediterranean to the Sea of China. The West gathered up part of this legacy and received from it the leaven which was to make possible its own development. The transmission was favored by the crusades of the twelfth and thirteenth centuries and the expansion of the Mongol empire in the thirteenth and fourteenth centuries. . . . There is nothing surprising about this Western backwardness: the Italian cities . . . were at the terminus of the great commercial routes of Asia. . . . The upsurge of the West, which was only to emerge from its relative isolation thanks to its maritime expansion, occurred at a time when the two great civilizations of Asia [China and Islam] were threatened. (Gernet, 1985: 347–348)

Economic History

The same *problématique* marks much of economic history. In recent Eurocentric times, economic history has focused on Europe, its rise, and its expansion worldwide. Far too many books to mention have been written on the whys and wherefores of the “Rise of the West”; and almost all of them have sought the answer in this or that factor or combination of them within Europe. When the rest of the world is there, as for scholars such as Jones (1981), Hall (1985), or Baechler, Hall, and Mann (1988), it is only to be found deficient or defective in some crucial historical, economic, social, political, ideological, or cultural respect in comparison to the West. Therefore, these authors also revert to an internal explanation of the presumed superiority of the West to explain its ascendance over the rest of the world. For all of them, the rise of Europe was a unique “miracle” and not a product of history and shifts within the world (system). The major exception in posing and answering this question is McNeill’s *The Rise of the West*; and it is not an economic but a world history!

As for the others, we may chose *The Rise of the Western World: A New Economic History* by Douglass C. North and Robert Paul Thomas (1973) as an example. The reason is the explicitness of its title, its emphasis on “new,” the renown of the authors, and their revision of received theory. Yet under their subtitles “Theory and Overview: 1. The Issue,” and on the very first

page, they clearly state “the development of an efficient economic organization in Western Europe accounts for the rise of the West” (North and Thomas, 1973: 1, our emphasis). They then trace this institutional change, and especially the development of property rights, to increased economic scarcity, which was generated in turn by a demographic upturn in Western Europe. The rest of the world was not there for them, but we shall return to its demographics in our discussion of macro historical sociology later. Here it is worthy of note, as North and Thomas (1973: vii) emphasize in their preface, that their economic history is “consistent with and complementary to standard neo-classical economic theory.”

Marxist economic history, by contrast, has been dominated by concepts such as “mode of production” and “class struggle.” Yet, both of these concepts have generally also been interpreted within a framework of a single “society” or social formation, or at least a single entity, whether that be a state or a civilization. That is, with regard to “the rise of the West” and “the development of capitalism,” Marxist economic history has been equally or even more Eurocentric than its “bourgeois” opponents. Examples are the famous debate in the 1950s on “the transition from feudalism to capitalism” among Maurice Dobb, Paul Sweezy, Kohachiro Takahashi, Rodney Hilton, and others (reprinted in Hilton, 1976) and the *Brenner Debate* on “European feudalism” (Aston and Philpin, eds., 1985). De Ste Croix (1981) on the class struggles in the ancient “Graeco-Roman” civilization and Anderson (1974) on “Japanese feudalism” also considered these as a particular “society.”

This limitation on the scope of analysis was not inevitable nor laid down by any law. Rather, it was the result of Eurocentrism and a preference for endogenous class-based causal explanatory frameworks. In this preference for the limited and limiting units of analysis, like the national state or society or civilization, “transitions” occur mainly for “internal” “class” reasons. Central to these “transitions” have been the transitions between modes of production, which were usually analyzed as if they occurred wholly within each separate entity according to the development of its internal contradictions.

Thus Anderson (1974) analyzed the “fall” of late Rome in the West as the demise of the slave mode of production and its gradual replacement by the feudal mode of production. Brenner (in Aston and Philpin, eds., 1985) analyzes the transition from feudalism to capitalism in Europe as if it occurred primarily (if not solely) as a consequence of internal class contradictions that brought about a crisis of feudal relations in the European social formation—irrespective of external causes. This was also the central theme of Maurice Dobb (1946), which led to the debate between him and other “productionists” such as Rodney Hilton and others versus the “circulationist” Paul Sweezy—who emphasized the contribution of world

market relations to the transition from feudalism to capitalism in Europe, without, however, yet studying the dynamics of that world economy itself. Kohachiro Takahashi tried to take an intermediate position between the two sides in this debate in the early 1950s (reprinted in Hilton, 1976). The same themes and theses resurfaced a generation later in the Brenner-Wallerstein exchange.

To reexamine the transition from feudalism to capitalism in Western Europe and the simultaneous rise of the "second serfdom" in Eastern Europe, Brenner takes a Dobbsian productionist position; and Wallerstein focuses on the development of the capitalist modern world-system. Denmark and Thomas (1988) review this debate and contend that it is better to maintain a wider system level of analysis and also to pay more attention to the concrete determinants of power within political systems. Denmark and Thomas point to the errors of overly state-centric analysis. Their refutation of Brenner's claims that Poland's relative status was primarily conditioned by its internal structure and not by trade is a useful empirical affirmation of the greater explanatory power of a world system framework of analysis. An illustration of the importance of these long-term and large-scale structural factors is that from his vantage point as a Hungarian Jeno Süzc could observe that in drawing the line between East and West Europe at their meetings in Moscow and Yalta,

[i]t is as if Stalin, Churchill and Roosevelt had studied carefully the *status quo* of the age of Charlemagne on the 1130th anniversary of his death.... [Also] the old Roman limes would show up on Europe's morphological map, thus presaging right from the start the birth of a "Central Europe" within the notion of the "West."

The whole history of the Hapsburg state was an attempt to balance the unbalanceable while being squeezed somewhere between the two extremes of East-Central Europe. The only consequent structural element in that formula... [was] the setting up by the Hapsburgs of a diminished—East-Central European—copy on an "imperial scale" of the division of labour drawn up by the nascent "world economy" on a larger scale... between West (industrial) and East (agricultural)... In the "Hapsburg division of labour," Hungary was cast in the East's role [with its East European hinterland and Austria governing Bohemia in the West's]. (Szücs, 1983: 133, 172, 173)

The issue of how to combine the respective strengths and insights of the global and local levels of analysis is taken up in a forthcoming collection on "neo-structuralism" (Palan and Gills, 1993).

At the center of these still very relevant discussions is a vital methodological issue. Should we take as the primary unit of analysis a single

society (if such a thing can be said to exist!), or a single state, or even a single mode of production (if there ever was one in isolation)? Doing so leads us to privilege production and endogenous factors in formulating our causal explanations of social change. Or should we take on the largest unit of analysis suggested by the material and political-military interactions in which any particular geographical area is involved? That leads us to privilege (or at least to emphasize) accumulation, exchange, and hegemonic influences or rivalries. That is our methodological choice. Of course, we differ from Wallerstein in that we do not see the world system as arising from 1500, but much earlier. Therefore, we do not regard the "transition," if any, as an intra-European process, but more as the consequence of a shift in the economic center of gravity from East to West. That is our argument explicitly in Gills and Frank (1992) and in Frank (1991b, 1992a, b). Thus, we then find "systemic" and conjunctural causal explanations of "transitional" change that appear "external" to Europe and its "internal" relations of production. Since these appear primary to the "production-ists," they, therefore, accuse us of "circulationism." Frank (1991b) in turn inveighs against "Transitional Ideological Modes: Feudalism, Capitalism, Socialism."

In this regard, we may perhaps be permitted a personal but revealing aside. In 1965, one of us debated with Rodolfo Puiggrós in the Sunday supplement of a Mexican newspaper about the transition between feudalism and capitalism in Latin American agriculture (Frank, 1965). The title was "With What Mode of Production Does the Hen Convert Maize into Golden Eggs?" The answer was that the hen's mode of production in agriculture and *a fortiori* Latin America itself was capitalist since its Conquest and incorporation into the capitalist system by the newly hegemonic Europe. Fifteen years later, Frank's then seventeen-year-old son Paulo suddenly said like a bolt out of the sky that "obviously Latin America could not have been feudal, since it was colonized by Europe."

The 1965 article began by inviting the readers to solve a puzzle: connect nine points, which visually seem to form (and enclose) a square, with a single line of four continuous and straight segments. The point was—and still is—that it is impossible to find the solution as long as we stay within the limited frame that the nine points appear to impose on us: "The solution is that we must emerge from the limited and self imposed frame" by going outside it. The argument in 1965 was that "if we are to understand the Latin American problematique we must begin with the world system that creates it and go outside the self-imposed optical and mental illusion of the Ibero-American or national frame" (reprinted in Frank, 1969: 231).

That is still the point, and it applies equally to understanding "the transition from feudalism to capitalism" in Europe and to "the rise of the western world: a new economic history." In the last generation, all sides of

the Dobb-Sweezy debates, the Brenner debates, the Brenner-Wallerstein debates among Marxist and neo-Marxist, as well as the debates between neoclassicists and other Eurocentric scholars before them, have posed all their questions and sought all their answers only or primarily within Europe, be it in its "mode of production," "institutions of property," or otherwise. Yet if we are to understand this apparently European *problématique* we must also "begin with the world system that creates it and go outside the self-imposed optical and mental illusion of the [nine point enclosed square Latin American or European] or national frame."

We recommend the world system as the locus, and the process of accumulation within it as its motor force of development, as the primary determinants of the historical process. In this regard we are very much in agreement with Wallerstein, Amin, Abu-Lughod, and others—as far as they go. However, as noted in our discussion of world system theory earlier, we want also to apply the same methodology much further in space and time. We believe that Marxist and neo-Marxist historiography also should not be confined in its self-imposed "isolationist" orthodoxy. Rather, historical materialist analysis, Marxist or otherwise, should move in ever more holistic and inclusive directions. These were already proposed by earlier materialist economic historians, such as Gordon Childe (1936, 1942) and later by Fernand Braudel's (1953, 1981–84) "total history." Only then can we hope to comprehend the full causal frameworks for transitions—be they in modes, centers of accumulation, or hegemonic power—on the scale of the "world-as-a-whole."

(Macro) Historical Sociology

Both the Marxist heritage and its self-limitations also impinge on macro historical (political) sociology, and so do our critiques thereof from a world system perspective. For example, Michael Mann (1986: 1–2) sums up his approach in two statements. Both could offer justification and basis for a world system historical approach. However, in Mann's hands they do rather the opposite:

Societies are not unitary. They are not social systems (closed or open); they are not totalities. We can never find a single bounded society in geographical or social space. Because there is no system, no totality, there cannot be "sub-systems," "dimensions," or "levels" of such a totality. Because there is no whole, social relations cannot be reduced "ultimately," "in the last instance," to some systemic property of it—like the "mode of material production," or the "cultural" or "normative system," or the "form of military organization." Because

there is no bounded totality, it is not helpful to divide social change or conflict into "endogenous" and "exogenous" varieties. Because there is no social system, there is no "evolutionary" process within it. . . . There is no one master concept or basic unit of "society." . . . I would abolish the concept of "society" altogether.

The second statement flows from the first. Conceiving of societies as multiple overlapping and intersecting power networks gives us the best available entry into the issue of what is ultimately "primary" or "determining" in societies. . . . [There are] four sources of social power: ideological, economic, military, and political (IEMP) relationships.

We can only agree to Mann's proposal to abolish the concept of society and to his rejection of the search for some single ultimately determinant property thereof. For most of Mann's rejection of the premises of orthodox history and social science, Right and Left, also eliminates many underbrush obstacles on the way to the world system history we propose. However, we have some reservations about his *prima facie* rejection of all totality and systemic property as well as to his singular preoccupation with power alone. In particular, we cannot be satisfied by his inquiry only into "the sources of social power" at different times and places, without a systematic attempt to investigate possible connections between here and there, and to trace possible continuities between then and now. Moreover, we suggest that Mann's focus on power itself devotes insufficient attention to the use, if not the motive, of power for ulterior economic ends.

This more materialist perspective is much more pervasive in Jack A. Goldstone (1991). This book is not so much, and certainly not just, another study of revolutions and rebellions. In addition, indeed instead, it offers a demographic/structural and cyclical analysis of economic, political, social, cultural, and ideological factors responsible for state breakdown. The revolutions are only the straw that breaks the camel's back; and the rebellions are those that fail to do so, because the structural conditions are not ripe. "Any claim that such trends were produced solely by unique local conditions is thoroughly undermined by the evidence" (462). To explain, we may best let Goldstone speak for himself:

EARLY MODERN HISTORY: a WORLD HISTORY My primary conclusion is quite beautiful in its parsimony. It is that the periodic state breakdowns in Europe, China and the Middle East from 1500 to 1800 were the result of a single basic process. . . . The main trend was that population growth, in the context of relatively inflexible economic and social structures, led to changes in prices, shifts in resources, and increasing social demands with which the agrarian-bureaucratic

states could not successfully cope. The four related critical trends were as follows: (1) Pressures increased on state finances and inflation eroded state income and population growth raised real expenses. . . . (2) Intra-elite conflicts became more prevalent as larger families and inflation made it more difficult for some families to maintain their status . . . while creating new aspirants to elite positions. . . . (3) Popular unrest grew, as competition for land, urban migration flooded labor markets, declining real wages, and increased youthfulness raised the mass mobilization potential of the populace. . . . (4) The ideologies of rectification and transformation became increasingly salient . . . and turned both elites and middling groups to heterodox religious movements in the search for reform, order, and discipline. The conjunctures of these four critical trends . . . combined to undermine stability on multiple levels of social organization. This basic process was triggered all across Eurasia by periods of sustained population increases that occurred in the sixteenth and early seventeenth centuries and again in the late eighteenth and early nineteenth centuries, thus producing worldwide waves of state breakdown. In contrast, in the late seventeenth and early eighteenth centuries populations did not grow, and the basic process and its four subthemes were absent. Political and social stability resulted. (Goldstone, 1991: 459–460, emphasis in the original)

What lies behind the long cycles of expansion and contraction at least of “economic” growth rates and their political consequences, which are identified by Gills and Frank (1992)? Perhaps demographic changes, due in turn to Eurasian-wide ups and downs in mortality rates, as Goldstone persuasively argues. They could well combine with the long cycles of typically 200 years expansion and contraction, which we identify. Alas, we have not even investigated this possibility—if it is possible to do so with available demographic evidence. However, ecological cycles, as Goldstone also calls them, perhaps based on climactic changes, have also been suggested and investigated by others; and they are discussed in Frank (1990a and Gills and Frank, 1991–92). Goldstone’s kind of analysis could and should be extended beyond the cases he studied.

Goldstone’s demographically based economic, political, and social cycles of course challenge both the view that history is only linearly progressive and that, at least since early modern times, it is determined by the development of capitalism. We agree (Gills and Frank, 1992; Frank, 1991a). Of course Goldstone’s point is even more well taken, if the demographic and political economic cycles extend farther back than the supposed origin of capitalism around 1800, 1500, or whenever. Indeed and although Goldstone himself does not go so far as to say so, his materialist analysis undermines

the very idea of capitalism as a separate and useful category, not to mention system. That is what Frank (1991 b) argues, also on materialist grounds.

A related major case in point is the insistence, against all the evidence, that the class struggle between classes is the motor force of history. Goldstone denies that, and adduces contrary evidence again and again. Alvin Gouldner (1980) already emphasized the contradiction between *The Two Marxisms*. One holds that material economic conditions shape social relations and form consciousness, and the other claims that the class struggle and consciousness thereof drive history. Yet at about the same time, the Polish Marxist Leszek Nowak (1983 translation) pointed out that the transition from slavery to feudalism was not generated by interclass slave revolts against their masters, and the transition from feudalism to capitalism was not due to interclass uprisings by serfs against their lords. In both "transitions," if any, the conflicts and "struggles" were intra-class within the old and emerging new ruling classes, which responded to underlying economic changes. Slave and serf revolts were at best secondary and supplementary. Now Goldstone demonstrates that in each of the cases he analyzes the important conflicts and struggles were among the existing and emerging elites, and not between the "people" and them. "Factional conflict within the elites, over access to office, patronage, and state policy, rather than conflict across classes, led to state paralysis and state breakdown" (Goldstone, 1991: 461), as Gills and Frank (1990–91) also observed. Grassroots social movements from below were supplementary in that they helped further destabilize an already unstable state, if only by obliging it to spend already scarce resources to defend itself; and that the popular movements favored the interests of some elite factions against others. "I know of no popular rebellion that succeeded by itself without associated elite revolts or elite leadership in creating institutional change" (Goldstone, 1991: 11). All this would be obvious, if it were not so frequently denied by those whose ideology leads them to claim to know better.

Gills (1989) also refers to the intra-elite struggles underlying periodic crisis. He sees this pattern virtually everywhere prior to 1500. The pattern is driven not only demographically, but more fundamentally as a cyclical struggle among elites for control over shares of the surplus and state power. The typical pattern, as evident in the history of East Asia, is for privatization of accumulation to grow to a point at which it threatens the stability of the state, whose revenue declines as the rate of exploitation increases. This immiserates the peasantry and impoverishes the economy, and precipitates rebellion. In East Asian history, the timing of major rebellions is closely correlated to the entropic nadir in this cycle of accumulation and hegemony.

These and other revolts and revolutions have been the object of long study by Charles Tilly and his associates. They help fill an important

void in the analysis of world system history, in which people's participation often does not receive the attention it rightfully deserves. Under the suggestive title *Big Structures, Large Processes, Huge Comparisons* Tilly (1984) asks "how can we improve our understanding of the large-scale structures and processes that were transforming the world...?" Tilly answers and argues that "the most pressing theoretical problems are to connect local events to international structures of power and to improve existing models of these international structures."

He considers doing so at the world-historical, the world-systemic, the macro historical, and the micro historical levels. "If the world forms but a single coherent network, the first two levels collapse into one.... How many levels exist and what units define them are partly empirical questions." But "if any connection counts, we will most likely discover that with trivial exceptions the world has always formed a single system." Tilly rightly rejects counting any connection; but he jumps to the unfounded conclusion that, therefore, "only in the last few hundred years, by the criterion of rapid, visible, and significant influences, could someone plausibly argue for all the world as a single system.... [This] implies that human history has seen many world systems, often simultaneously dominating different parts of the globe." Therefore, Tilly argues that we must study many "big structures, large processes, huge comparisons." Yet Tilly's own objectives and alternative criteria to the pernicious postulates also permit alternative plausible arguments. To begin with, there could have been a multicentered and yet single system. Nonetheless, Tilly himself still does not accept these arguments. On the contrary, in private correspondence of July 30, 1989, he suggests that we would have to adopt precise numerical criteria of degrees of influences to measure significance, which in turn we reject as deleterious. Thus, we could say that Tilly's study of social movements breathes welcome life into the baby; but he throws out much of the wider social bath water, all of the systemic bath tub, and leaves the baby perilously suspended in midair.

Political Geography

Political geography as a world-encompassing subject is concerned primarily with analysis of the spatial dimensions of global political economy. Formerly, the dominant form of international political geography was geopolitics, which was preoccupied with strategic studies and power politics. Global power rivalry among the great powers called into being a social science discipline to inform strategists and statesmen. As such, geopolitics was the handmaiden of International Relations, a similarly policy-oriented academic community. Mahan and Mackinder epitomized

the infancy of geopolitics and its strategic obsession, for example, in Mackinder's famous "heartland" theory.

Fortunately, in recent years political geography has been taken in new directions by critical scholarship addressing the spatial dimensions of the modern capitalist world economy. Particularly instrumental therein have been geographers such as Peter Taylor (1989) who also edits the journal *Political Geography*, R. J. Johnson and P. J. Taylor (1986), Richard Peet (1991), and A. D. King (1991). Wallerstein (1991 b) has also contributed in this direction.

The spatial analysis of capitalism on world scale has become more "fluid." It is moving away from notions of fixed territoriality, particularly when addressing questions of nation and nationalism, identity and locality, and the organization of production. The burgeoning literature on globalization/localization, postmodernism and critical human geography, and global culture indicates the still increasing intellectual interest in new ways of incorporating the spatial dimension into analyses of global processes (Soja, 1988; Lash and Urry, 1987; Jameson, 1984; Anderson, 1983; Featherstone, 1991; A. D. King, 1991).

The debates about world system theory and history intersect with these spatial explorations in political geography and critical social theory. Taken to its logical conclusion, the Gills and Frank (1992) approach to cycles of accumulation and hegemony at the scale of the world system as a whole implies a new conceptualization of the spatial dimension of world accumulation/hegemonic processes. The fluidity of the spatial organization of the world system becomes all the more sharply apparent in a perpetual process of restructuring, which has been continuous for not only the past 500 years, but throughout 5,000 years of world system history.

The "geography of imperialism" should be understood not merely territorially, but sequentially, via the shifts in centers of accumulation that occur over time, and which themselves reflect the underlying processes of competitive accumulation that forever restructure the spatial organization of the world economy. In reality, no political geographical/spatial unit or entity, be it nation or state, is fixed. Instead, all have historically been and still are being kaleidoscopically transformed on the wheel of the processes of accumulation in the world system.

International Relations and International Political Economy

Of all the academic disciplines that our world system history should speak to International Relations (IR) and International Political Economy (IPE) are the most obvious candidates. World system analysis established its value

by challenging both disciplines by its very multidisciplinary and holistic approach. By insisting on studying 500 years of world system history, world system analysis broke with the short-term post-1945 self-definition of both IR and IPE. It also broke with the then-predominant state-centric approach in IR, which was mirrored in the modernization approach in Development Studies. World system theory made a case for the superiority of taking the world system as a whole as the unit of analysis. Since its first onslaught on the state-centric approach, conventional IR has been influenced by growing dissatisfaction with traditional realist state centrism. A number of prominent IR theorists have turned their attention instead to IPE (Gilpin, 1987; Keohane, 1984; Krasner, 1983).

Our approach to hegemonic transitions also complements rather than competes with or contradicts the new Gramscian school in IR by, for instance, Stephen Gill (1990) and Robert Cox (1981, 1983, 1987). They use larger frameworks of global hegemony, but also incorporate class and social forces, as well as their relationship to world order. This work complements our insistence on analyzing "interlinking hegemonies" in world historical processes. Gills (1992) attempts new synthesis of the Gramscian and world system approach in an analysis of hegemonic transitions in East Asia. However, most adherents of the new Gramscian approach to IR/IPE do not (yet) extend their analysis back in time beyond the relatively recent modern period.

However, the main point of continuing contact and dialogue between IR theorists and world system theorists has been long cycle theory. Both were concerned with understanding the relationship between economic cycles of expansion and contraction and leadership/hegemonic cycles. These relationships were explored especially in Modelski (1987), and Modelski and Thompson (1988) coming from the "political" IR side; Wallerstein (1974) and Frank (1978a) on the "economic" world system side; the reader on both edited by Thompson (1983); reworking all of the aforementioned and much more in the magistral study on long cycles and war by Goldstein (1988), and reflected in recent discussion of world leadership and hegemony (Rapkin, 1990). In addition to establishing historically grounded empirical studies of long-term cyclical change in the international/world system, they also made a contribution to cumulative social science knowledge, as reviewed by Chase-Dunn (1989).

This dialogue and growing interest in historically grounded IR and IPE theory also led to the establishment of the World Historical Systems (WHS) subsection of the IPE section in the International Studies Association (ISA). However, the 1991 and 1992 meetings of the WHS show that a growing number of its members and others are now applying the study of a combination of both "political" and the "economic" long cycles, and also of center-periphery structures, to world-systems—or like we to the world system—before 1500. Our theses on world accumulation attempt to push the historical agenda of

research even further back in sociohistorical time. Thereby, the established virtues of the world system and long cycle approaches are extended to contribute to the study of world history. Premodern history and archaeology in turn can contribute to and perhaps “redefine” the study of IR and IPE.

The key question we pose to both existing world system theory and to IR and IPE theorists is whether there are fundamental historical cyclical patterns that shape not only the present and the last 500 years, but also much more of human history. Do the patterns of historical cyclical development of the present originate only 500 years ago with the emergence of the “capitalist mode of production” and the “modern inter-state system,” or do they emerge much earlier, as we suggest in Gills and Frank (1992)? If these patterns transcend transitions between modes of production and hegemonic power, as we think the evidence indicates, then the implications for social science are far-reaching indeed. We do not want to fall into some trap of “transhistoricism” by claiming that all world history is the same. We do not deny the reality of constant change and restructuring in the world economy. Far from it; what we seek to establish is that a process of accumulation existed in a world economic system long before the emergence of the “capitalist modern world-system” and that rhythms of expansion and contraction in this world system/economy have a continuity, which long antedate—and indeed contribute to and help account for—the emergence of this “capitalist modern world-system.” These patterns are interlinked with the historical rise and decline of hegemonic powers and shifts in the centers of power, whose fundamental characteristics, as we maintain, also long predate modern states systems.

Our hypotheses not only counter the short-termism and state-centrism of much of IR and some of IPE, they also challenge these disciplines and their concerns to encompass more of the human experience and to analyze it more holistically. Ultimately, our position makes a case for both a macro- and a micro historical sociology as the basis of any IR and/or IPE theory to understand and formulate policy for the modern world. The call for a world historical approach to IR and IPE does not mean that current changes and conditions in the world system are irrelevant or a distraction. The real purpose of world historical approaches is to inform and enrich our understanding of and policy for these ongoing sociopolitical processes in the world today—and tomorrow. We explore some of these social scientific, political, and practical implications later in our conclusions.

Development Studies

Development studies as such was born only after the Second World War and is, not unlike its second cousin “socialist development,” already over

the hill if not downright dead (Seers, 1979; Hirschman, 1981). The present world economic crisis has replaced concern with "development" by that for crisis management in the South and East. Moreover, "development" has been replaced by the new buzzword "democracy," although managing the crisis allows for hardly any democratic control of public policy (Frank, 1992d). On the other hand as we contend, the world system has been around for over 5,000 years already; and its systematic study along these lines has only just begun. However, both the existence and the study of this world system have far-reaching implications for both development studies and "development" itself.

A world system perspective on "development" helps clarify how much—that is how little if at all—the "development" we have known has been good for people. That "[d]evelopment is bad for women," feminists say (Frank, 1991c). If that is true, development is already bad for over half the world's population. However, "development" has also been bad for most men, as Wallerstein explicitly and Amin implicitly point out: Over the five centuries' existence of the modern world-system, as they see it, the growing polarization of income and wealth in the world has not benefitted most men (and still less women) relative to the relatively few who have benefitted. Today, roughly speaking, 20 percent of the population get 80 percent of the world's goodies, and 80 percent have to share the poverty of the remaining 20 percent of the goods. Wallerstein argues that as a result, the majority of the people in the world are also absolutely worse off than they were 500 or even 200 years ago. If now we extend the idea of the world system still much farther back in history the perspective on polarization and "development" becomes dimmer still, even if Amin argues that world-scale polarization only began with the birth of the modern world (capitalist) system.

However that may be, if there is only one world system, then "national" (state) development within it can only be the (temporary) improvement of a region's or a people's position within that system. In that case indeed, the very term "development" makes little sense unless it refers to the development of the whole world system itself, and not just of some part of it (Frank, 1991c). That is, the entire (national state/society) foundation of "modernization" theory and policy, whether "capitalist or "socialist," is challenged by the world system (theory) as well as by the bitter experience of those who put their faith in it and/or were obliged to suffer its costs.

The verity of this discovery is spectacularly illustrated by the experience with "socialist development." To begin with, the "development of socialism" was always little more than misnamed "socialist" development, as distinct from some "other" development, but nonetheless (national/state) development above and before all else. That has now been unmasked as a snare and a delusion. Unfortunately, perhaps even more on the ideological

Right than on the Left, the blame for the failure is falsely attributed to the “socialist” part of this (non)development. In fact, the “socialist Development” was tried and failed exclusively in underdeveloped regions, which had been so for ages and remain the same—for that reason, that is, because of their inherited and still continuing position in the world system, and not because of their supposed “socialism.” To the possible retort that some “capitalist” countries did “develop,” however, the answer is that most “capitalist” countries, regions, and so on in the world also did not “develop” and that they failed to do so for the same reason: not due to their “capitalist” or “socialist” “system,” but due to their position in the world system! So the existence, participation in, and awareness of the world system puts the *problématique* of “development” in a completely different light from that which was mistakenly and ideologically thrown upon it during the four postwar decades.

Development “policy”—and “theory”—has largely been a sham. Very few actors in this drama (farce? tragedy?) have sought anything other than their own profit and enrichment—at the expense of others. That has been true not only of “capitalists” for whom it comes naturally, but also of “socialists” for whom it may come “unnaturally”; but it comes nonetheless. The development “theory” either had “policy” makers as its referent who turned out not to exist, or it had none at all to begin with. How could it have been otherwise, if all are part and parcel of the same dog-eat-dog competitive world system? In that system only a few can win the “development” race at any one time; and apparently they cannot even maintain their lead for long.

If world system theory is an outgrowth of dependence theory, as is often claimed especially by observers who subscribe to neither, then it should not be surprising if “world system” also has implications for “dependence.” Briefly, they are that dependence exists—indeed has existed for millennia within the world system—and that eliminating dependence or being/becoming independent of the world system is impossible. Thus, *dependentistas*, including Frank (1967, 1969) and others, were right in giving structural dependence a central place in their analysis. Indeed, they did not know how right they were; for that dependence cannot be eliminated simply by replacing one “system” by another, because there is only one world system. On the other hand, therefore, the *dependentistas* were wrong in proposing easy solutions for dependence, as Frank (1991c) acknowledges under the title “The Underdevelopment of Development.” It has been an essential part in the center-periphery structure of the world system for thousands of years; and it is not likely to be overcome easily or to disappear soon. Although they are not unrelated, concern about “dependent [under] development” has been shifting to concern for ecologically “sustainable development” (Redclift, 1987).

Ecology

Our thesis also touches on the contemporary and growing globe-embracing ecological threat and worldwide consciousness about the same. We argue that it was ecological considerations that led to the formation of the world system in the first place (Gills and Frank, 1991–92). The initial connections between Mesopotamia and Anatolia, Egypt and the Levant, and so on were forged to overcome ecologically determined regional deficiencies: Mesopotamia had to import metals from Anatolia, and Egypt wood from the Levant. Ecological considerations and changes also underlay many of the migrations and invasions from Central Asia into their neighboring regions to the East, South, and West. The resulting human activity, in turn however, also had far-reaching ecological effects. Some may have been regionally beneficial for, or at least supportable by, the environment. Others, however, caused far-reaching environmental damage and, perhaps in combination with climactic and other environmental changes, led to regional environmental disasters. As a result, entire civilizations disappeared, like the Harappan one in the Indus Valley.

Once formed, the “central” world system as whole survived however. Indeed, it expanded to incorporate ever more of the globe within it. Eventually, technological development, population growth, and of course the exploitation by man both of others and of the environment in the world system led to the growing globe-embracing environmental damages and threats, of which consciousness is only just emerging. However, vast regional environmental damage and consciousness thereof—for instance, in the Americas—occurred before in the world system and as a result of its expansion in what Alfred Crosby (1986) called *Ecological Imperialism: The Biological Expansion of Europe, 900–1900*. Analogous, if perhaps less dramatic, human-caused ecological scourges also occurred earlier in various Eurasian parts of the world system. Now, however, ecological disaster in the world system has itself become altogether global as well. Yet, the existence of the world system generates the causes of this disaster primarily among the rich who most benefit from the system and visits the damages and costs selectively among the poor who can least defend themselves and their meager livelihood against the ecological threat and the structure and operation of the world system. Some of these people(s) have traditionally been the object of study by anthropologists.

Anthropology

Pursuing the world system back over thousands of years also touches some concerns of anthropologists. We have already considered the concern of

archaeologists among them and some of the issues they debate. Evolutionism or neo-evolutionism via White and Steward, fell on hard times among anthropologists. However, there is certainly an overlap of interest with the longer historical view of a world system theory for 5,000 or more years, even if that is perhaps an exceedingly short view. The Lenskis (1982) referred to a 10,000-year world system, and physical anthropologists are of course concerned with more and more millions of years of human kind and its migration. Another issue is that of independent invention vs. diffusion. Emphasis on ties over long distances, not to mention participation in the same system, lends additional credence to diffusion and/or to simultaneous or repeated invention in response to common problems and stimuli.

A related recurrent issue among anthropologists is the question whether the societies they study are or were pristinely independent or related to others and participants in a wider system of societies. Currently, the long widespread thesis that the Kung (Bushmen) led a pristine independent existence in the Kalahari Desert has been the subject of increasing disconfirmation. Like most peoples, they have long participated in broader relations. It may nonetheless not be legitimate to say that these have long included the world system. Nor should it be excluded. However, the longstanding "substantivist" vs. "formalist" debate among economic anthropologists may find its Gordian knot cut when the "societies" they discuss are part of the world system. The formalists argued that the same economic "laws" (e.g., of supply and demand) operate in all societies and times. The substantivists disagreed and countered that most societies were organized around "redistribution" and "reciprocity" instead. Reference in this regard has already been made earlier to the major substantivist writer Polanyi, who has been challenged by new archeological finds. These finds and authors support a 5,000-year world system without however becoming formalists.

The transition from roaming if not nomadic hunters and gatherers to settled agriculturalists has not been as unidirectional as was once claimed. Instead, adaptive "transitions" have gone back and forth in response to ecological but also socioeconomic changes in the areas that particular peoples inhabited, which often formed part of and were subject to the influences of the world system (cf. Lattimore, 1962). Thus, the anthropological concern with kinship-based social organization also appears in a different context, if kinship-based "societies" are viewed as part of the world system.

In particular, political organization that is supposedly derived only or primarily from kinship organization is subject to reinterpretation. Political organization and especially state formation has responded not only or even primarily to "internal" needs within this or that "society" but has been a

function of contacts and rivalries with neighbors and/or invaders from afar within the world system. They in turn often responded to world system-wide circumstances and changes. A survey of the related anthropological literature on state formation based on "internal" factors or on "inter-polity relations" may be found in Cohen (1978). For Central Asia and its relations with its neighbors in East, South, and West Asia, this *problématique* is analyzed among others by the anthropologists Khazanov (1979) and Barfield (1989) and in Frank (1992c). (Barfield, 1989: 6–7) summarizes following Irons (1979: "Among pastoral nomadic societies hierarchical political institutions are generated only by external relations with state societies and never develop purely as a result of internal dynamics of such societies." The anthropologists Talai Asad (1973) in *Anthropology and the Colonial Encounter* and Eric Wolf (1982) in *Europe and the People without History* deal with the relations between colonial powers and indigenous peoples. Although their concern is with relatively recent times, analogous problems also existed during encounters within the world system before modern times. Of particular interest in this regard are the related issues of ethnicity, race, their relations, and study.

Ethnic and Race Relations/Studies

Another vital concern for anthropologists is ethnogenesis and ethnicity, which is of special relevance to ethnic identity, not to mention racial identification, today. The recurrent major and incessant more minor *Völkerwanderungen* in, through, and out of Eurasia have certainly mixed and mixed up ethnicity and race. So how can they be identified today?

Whatever the gaps in our knowledge, or the disputes about, past ethnogenesis and present ethnicity, their fundamentals are clear: ethnogenesis is less traditional than situational, and ethnicity is less of an identity among "us" than a relation with "them." Both the situation and the relation are substantially defined by state and other political power; and the presence, absence, and especially the change in economic welfare occasions changes in the perception of ethnic identity and in the urgency of its expression. The anthropologist Frederick Barth (1969) persuasively argued for the recognition of situational and relational ethnic identity in his *Ethnic Groups and Boundaries*. The same was reiterated in more general terms in Nathan Glazer and Daniel Moynihan's *Ethnicity: Theory and Experience*. Summarizing in the words of Roger Ballard's (1976) review of the latter "ethnicity is then, a political phenomenon, in which material interest unites with moral and emotional bonds." We (1992c) argue that all of these in turn are part and parcel of participation and changing circumstances in the world system, to which ethnic identity and racial

identification are the responses. Therefore, our study of the millennial world system also bears on these vital concerns, which are convulsing the ex-Soviet Union and Yugoslavia as we write. In this regard, we may also recall again the previously cited literature on globalization and localism by, among others, Featherstone (1991), Friedman (1991), Hall (1991), King (1991). To summarize in the words pronounced by Michael Gorbachev before the United Nations, and we believe by Hegel before him: "unity in diversity."

Gender Relations

Feminist archaeologists and historians (thank Goddess for them!) have begun to dig up or reinterpret a paleolithic and neolithic past supposedly governed by nonpatriarchal "partnership" relations. However, these relations were found to be "indigenous" particularly in Catal Huyuk and Hacilar in Anatolia, the site of Jericho in the Levant, later in Minoan Crete, and in the Balkans (Eisler, 1987, following especially Marija Gimbutas, 1980, 1981; and James Mellaart, 1975). Figurines that suggest nonpatriarchal Goddess worship have also been found farther eastward into India. These scholars argue that these societies, and by extension Western Judeo-Christian society, only switched to patriarchy later after armed invaders from Inner and Central Asia brought them warfare, military technology, oppression, and therewith the "diffusion" of patriarchy. Thus, these feminists suggest that Western patriarchy is the result of its (unwelcome) diffusion from farther East in Inner Asia. This thesis is supported by the work of James DeMeo (1987, 1990, 1991). He claims that "matrist" (but not matriarchal) relations were "original" in much of the wetter and greener world before Arabia and Central Asia dried up about 4000–3500 B.C. Then desertification expanded through what he calls the 1,000 mile-wide Saharasian belt stretching 8,000 miles from Africa through Inner Asia to China. As a result, many of its inhabitants suffered famines and were obliged to become pastoralist nomads, whose harsh and competitive realities then fostered "patrism" including patriarchy.

(Re)writing history from a more gender-balanced or feminist perspective is very welcome as all to the good. We particularly need more "feminist historical materialist" analysis of different and changing gender and family relations, accumulation, politics, and culture/ideology. For much of history has been dominated by men in their own interest and written by them from their own perspective. However, the aforementioned feminist version of history seems less than satisfactory and has at least the following four weaknesses and limitations: (1) It focuses rather selectively on some circum-Mediterranean societies with supposedly indigenous partnership

societies and sees patriarchy as having been only belatedly diffused there from Inner Asia; (2) Patriarchy was well established very early even in several societies to the east of the Mediterranean; (3) Patriarchy was not comparatively more evident in Central Asia nomad societies, but rather the contrary. Frank (1992c) reports via Kwanten I have, therefore, tried specifically to ask every professional Central Asianist I have met whether the evidence available to them supports the Eisler and DeMeo theses. Unanimously, they have all said that it does not. According to their evidence on the contrary, Central Asian nomad societies accorded women higher status and had more egalitarian gender relations than their sedentary neighbors in Eurasia. I hesitate to cite the people who could only offer their evidence to me orally. However, I can quote some who have written something about this matter (of which we here reproduce short selections from a sample of two): “Women had more authority and autonomy than their sisters in neighboring sedentary societies....Although the details cannot be confirmed for the entire history of Inner Asia, most visitors made comments [to this effect]” (Barfield, 1989: 25). “Information dating from Mongol times suggests that women in the steppe empires had more rights and independence than their counterparts in sedentary states. These indications are confirmed for the Uighur Empire” (Kwanten, 1979: 58).

Finally to go to the roots of a *worldwide* problem such as patriarchy, these primarily Euro-Mediterranean-centered feminist historians would do well to expand their scope to that of the world, if not also to the world system, as a whole. Beyond DeMeo’s multicultural data, drawn from all around the world, a world *systemic* analysis could perhaps throw some additional light on this worldwide gender problem. For instance, just as emphasis on the competitive process of capital accumulation in the world system puts class and state formation in a different light, so may the same also offer a better perspective on the formation of the gender structure of society.

Some Philosophical, Social Scientific, and Political Implications

This thesis and approach also speaks to the age-old philosophical dilemma about determinism and free will. The formation of and incorporation within the world system may or may not have been necessary and “determined.” However, the world system both limited or “determined” and expanded the options or “free will” once the world system came into existence and/or incorporated a region or people within it. Surely, the formation and expansion of the world system and its “division of labor” increased material possibilities and cultural options for at least those who benefit from the system and probably for those who propagate it. However,

the division of labor also assigned roles and strengthened social structures and historical processes, which limited the options and perhaps determined some of the choices of all participants in the system. Of course, those who are directly exploited and/or oppressed in, not to mention those who are eliminated by, the system have their options limited and perhaps largely determined. However, even those who derive most of the benefits from their positions "on top" of the system probably have some of their behavior "determined" by the exigencies of maintaining and/or furthering their positions within and benefits from the system. Thus the unequal structure and the cyclical process, as well as the "progressive development" of the system simultaneously, expand the "free will" possibilities and "determine" the limited options within the system. However, the "determinism" is not predetermined. The options are determined in and by the structure and process of the system at each point in time. They were not predetermined beforehand by some "invisible hand" and for all time. Like a glacier, the historical process within the system and indeed the world system itself make their own way, both adapting to and changing the ecology.

The recognition and analysis of the system as distinct from its existence independently of its recognition further holism in social science. Many social scientists and historians reject holism in theory, and/or they are not very holistic in (their) practice. We seek to make our analysis as holistic as possible. So do "world-systems" theorists. Yet, we do so in different ways, guided by our respective visions of the "whole." For Amin (1991) and Wallerstein (1989b, 1991a), the important whole system is the modern capitalist world-system. Perhaps for Abu-Lughod (1989) as well; although she also devotes her attention to the "thirteenth century world system." All three also recognize other historical world-systems as do Ekholm and Friedman (1982), who devote more attention to studying ancient ones. We extend the same kind of holism to the study of a single world system and its development over 5,000 years. We suggest that this approach is an appropriate application both of the world system idea or approach and the holist mandate in social science and history. Ekholm and Friedman are receptive thereto; Abu-Lughod is skeptical; and Amin and Wallerstein reject this extension of world-system and use of holism. The latter altogether, and the former partly, argue that before 1500 there were other world-systems, which can and should also be studied holistically but on their own terms. Of course, if our present world system really has had a millennial existence and history as we claim, then our holistic long-view approach is all the more appropriate.

Like our "world-systems" colleagues, we also subscribe to and practice what we call the "three legged stool" approach: like that stool, our study of the social world system is supported equally by three ecological/economic,

political, and cultural/ideological/ethical legs. At one time or another, some of us may concentrate excessively or inadequately on one or two of these legs to the apparent exclusion of the other(s). However, in principle, if not always in practice, we recognize the role of all three legs. The most neglected one, perhaps, is the ecological material of the economic leg. That, unfortunately, is a shortcoming we still share with all too many other students of society.

Our thesis, as well as the related debates reviewed earlier, also have far-reaching political implications. Amin and Wallerstein identify the world system with its mode of production. Our study of the millennial world system and how it operates lead us to demur. Gills insists that the world system must not be confused with its "modes of production." Instead, he sees a complex mixture or articulation of modes at all times in the development of the world accumulation process and the world system and cannot accept the identification of the world system with a single dominant mode. Frank (1991b) goes further and argues that feudalism, and socialism, but also capitalism, are only "ideological modes," which should be excluded from our social scientific analysis altogether.

This issue is perhaps the central political point in the social scientific debate, which Amin and Wallerstein also join. They argue that the modern world-system is uniquely characterized by the capitalist mode of production. That is why they will not accept the proposal that the analysis of this world system can and should be pushed back before 1500. Before that, they argue and are joined by Abu-Lughod, there were other world-systems. Amin and Wallerstein insist, like probably all Marxists and most others, whether or not they see other prior world-systems, that in earlier times other modes of production were dominant. Amin sums them all up as "tributary" modes of production, in which "politics [and ideology] is/was in command" to recall Mao Zsedong. In the modern capitalist world-system, by contrast, the economic law of value is in command; and that on a world system scale.

We insist that this is nothing new. Therefore, Frank also suggests that it would be senseless to call all that previous history throughout most of the world "capitalist." If "capitalism" does not distinguish one "thing" from another, then there is no point in maintaining that label. Amin, Wallerstein, and most others insist that "capitalism" is distinguishable. Of course, today especially the political/ ideological Right finds "capitalism" particularly distinguished and distinguishable from "socialism." Frank denies that any of these categories have any social scientific and/or empirical content and suggests that they serve only ideological "false consciousness" purposes to confuse and confound instead.

The (mis)use and replacement of these categories bear importantly on the analysis and understanding of some major world events today, particularly

the end of “socialism” and of American “hegemony,” albeit not of the “end of history.” We believe that ideological blinkers—or worse, mindset—have too long prevented us from seeing that the world political economic system long predated the rise of capitalism in Europe and its hegemony in the world. The rise of Europe represented a hegemonic shift from East to West within a preexisting system. If there was any transition then, it was this hegemonic shift within the system rather than the formation of a new system. We are again in one of the alternating periods of hegemony and rivalry in the world system now, which portends a renewed westward shift of hegemony across the Pacific. To identify the system with its dominant mode of production is a mistake. There was no transition from feudalism to capitalism as such. Nor was there (to be) an analogous transition from capitalism to socialism. If these analytical categories of “modes of production” prevent us from seeing the real world political economic system, it would be better to abandon them altogether.

We should ask: What was the ideological reason for Wallerstein’s and Frank’s “scientific” construction of a sixteenth-century transition (from feudalism in Europe) to a modern world capitalist economy and system? It was the belief in a subsequent transition from capitalism to socialism, if not immediately in the world as a whole, at least through “socialism in one country” after another. Traditional Marxists and many others who debated with us, even more so, were intent on preserving faith in the prior but for them more recent, transition from one (feudal) mode of production to another (capitalist) one. Their political/ideological reason was that they were intent on the subsequent transition to still another and supposedly different socialist mode of production. That was (and is?) the position of Marxists, traditional and otherwise, such as the aforementioned Brenner (in Aston and Philpin, 1985) and Anderson (1974). That is still the position of Samir Amin (1989) who, like Wallerstein, now wants to take refuge in “proto-capitalism”—and by extension in “proto-socialism.” (Before he was ousted after the Tiananmen massacre, Chinese Premier Zhao Ziyang came up with the idea that China is now only in the stage of “primary” socialism.)

If people would dare to undertake a “transition” from their “scientific” categories, they could spare themselves and their readers some of the political (dis)illusions regarding recent events in the “second” and “third” worlds. These categories of “transition” and “modes” are not essential or even useful tools, but rather obstacles to the scientific study of the underlying continuity and essential properties of the world system in the past. They also shackle our political struggle and ability to confront and manage the development of this same system in the present and future.

We would all do better to see the reality of the globe-embracing structure and the long historical development of the whole world system itself,

full stop. Better recognize this system's "unity in diversity," as Mikhail Gorbachev said at the United Nations. That would really be a "transition" in thinking. This "transition" would help us much better to choose among the diversities which are really available in that world system— *Vives cettres differences!* Moreover, this transition in thinking could also help us to understand the real transitions that there are and to guide us in the struggle for the good and against the socially bad difference.

In particular, we suggest that these labels confuse and confound the real world system issues about which people have to and do dispute and fight. The belief in these labels supports disputes about political "systems" and self-determination, which have little or no real possibilities to be put into practice in the single really existing world system. The same labels serve to misguide or defuse the real social movements. About these, Amin, Frank, and Wallerstein agree enough, despite their disagreements about world system history, to have written a book jointly with Giovanni Arrighi and Marta Fuentes under the title *Transforming the Revolution: Social Movements and the World-System* (1990). Our joint conclusion was—A Luta Continua!

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CHAPTER SEVEN

The World Economic System in Asia before European Hegemony

The recent demise of the “Socialist System,” its “incorporation” into the “capitalist system,” and the increasing wealth of many Asian countries provide a new perspective on the origins and development of a world economic system that spanned the globe. It is an appropriate moment to critically reexamine the work of Fernand Braudel and Immanuel Wallerstein, both of whom have advanced the view that a world-economy emerged in Western Europe by at least 1450, then spread outward from Europe to encompass the rest of the world. Shelves of historical literature have argued that the origin and development of that world economy were due to European “exceptionalism.” Other scholars, however, envision a multicentric Eurasian system by the thirteenth century and question European exceptionalism, pointing out, for example, that the conquest of the Americas, rather than European capitalism, permitted Europeans to take over Asian markets.¹

This essay examines historical perspectives on modern world history, particularly the contradictions between what Braudel and Wallerstein say and what they show. Contrary to their interpretations, Wallerstein and Braudel’s evidence demonstrates that modern history unfolded in a world economic system, the center of which emerged in Asia and only shifted to Europe after 1800. This essay also addresses the controversy over the European origin of capitalism and casts doubt on the validity of calling the early modern world economy “capitalist.” Ideology still blinds too many historians to the nature of the global economy beyond Europe.²

Were there several world-economies, or was there only one world economy? Braudel comments that “[t]here have always been world-economies.” He analyzes several contemporaneous “world-economies” between the

eleventh and eighteenth centuries, in Western Europe, Russia, the Turkish Empire, the Islamic world, India, and China. Yet, he is uncertain whether the last three, including Southeast Asia and Japan, should be considered as three separate economies or as the “greatest of world-economies.” Finally, he considers that the arrival of the Europeans in the East created a “fourth world-economy.”³

Wallerstein’s theory of multiple world-economies looks for the largest coherent units of economic integration. Like Braudel, Wallerstein emphatically denies that there was any single, coherent, world economy until recently. Yet, Braudel’s and Wallerstein’s own data and analysis of their “world-economies” demonstrate that they were not economically autonomous, but had for centuries been intimately connected and dependent on each other. This earlier world system had the very same characteristics as Wallerstein’s “modern-world-system,” but it was not centered on Europe and it was as “capitalist” earlier as later.⁴

Braudel’s economies each had centers that included a dominant capitalist city and a strong, aggressive state. No one city remained dominant for long before being replaced by another. These economies were marked by an internal hierarchy of polarized zones, with the centralization and concentration of wealth and the development of technology setting the core apart from the periphery. Beyond the latter, there were “black holes,” which the trading economy bypassed completely. His economies each had their own long and short cycles of growth and decline.⁵

Braudel’s description can also apply to a single world economy, of which his separate economies were constituent parts. All the characteristics of Wallerstein’s European-based economic system can apply equally to the world economy long before 1500. What these authors observe about economic cycles is revealing, for these cycles reached across their economic boundaries. Braudel and Wallerstein express surprise at this uncomfortable fact, but do not reconsider whether their economic boundaries may have been too narrowly conceived.⁶

The first economy Braudel covers outside Western Europe is Russia, which he regards as “unquestionably a world-economy in itself” that is “remote and marginal.” Yet he contradicts himself as follows:

Russia’s foreign trade was always manipulated by hidden hands in Peking, Istanbul, Isfahan, Leipzig, Lwow, Lubeck, Amsterdam or London....The West took from Russia only raw materials, sending in exchange manufactured articles and currency...[and] the East bought manufactured goods from Russia, and provided in return dyestuffs...luxury goods...[and] cheap silks and cottons. In the early modern era “Russia’s positive balance [of trade] with the West...injected into the Russian economy the minimal monetary

circulation—silver from Europe or China—without which the market activity would scarcely have been conceivable.” A French consul at Elsinore observed that “considerable sums of money in Spanish pieces of eight pass through here on every English vessel bound for St. Petersburg.”⁷

Braudel notes that the exchanges from Europe across Siberia to China “formed a system of interdependence.” Moreover, “at the beginning of the sixteenth century, Russia’s principal foreign market was Turkey,” which Braudel also classifies as a separate “world-economy” “reminiscent of Russia.” Braudel terms the Turkish economy “a fortress,” but also a “source of wealth” and a “crossroads of trade, providing the Turkish Empire with the lifeblood that made it mighty.” The Turkish economy was not any more isolated from the rest of the world than the Russian economy:

A long French report on the Levant trade confirms this impression: “[French] ships carry more goods to Constantinople than to all other ports in the Levant. . . . The surplus funds are transferred to other ports by means of bills of exchange which the French merchants of Smyrna, Aleppo and [Port] Said provide for the Pashas.”

Braudel then asserts that European trade in the Turkish Empire was minimal and “merely passed quickly through... [because] money, the sinews of western trade, usually only made fleeting appearances in the Turkish Empire”: as part went to the sultan’s treasury, part oiled the wheels of top-level trade, and “the rest drained away in massive quantifies to the Indian Ocean.” In that case, Braudel should have asked what intermediary role the Turkish economy played between Europe and India. Then too, Braudel notes that caravan routes ran from Gibraltar to India and China “the whole movement-in-space which made up the Ottoman economy,” which “owed its suppleness and vigour to the tireless convoys which converged from every direction.”⁸

Far from having a self-contained “fortress” economy, then, the Ottoman Empire drew its lifeblood from being a crossroads between other economies, none of which were independent of each other. Of course, the Turks tried to maintain their power, derive maximum benefits from their intermediary position, and bar others from sharing in it as best they could. Turkish merchants, not content with their intermediary role at home, also “invaded Venice, Ferrara, Ancona, even Pesaro, Naples and the fairs of the Mezzogiorno” in Italy and “were soon found all over Europe, in Leipzig fairs, using the credit facilities provided by Amsterdam, and even in Russia or indeed Siberia as we have already seen.” The Turkish Empire hardly sounds like a closed economy.⁹

Braudel calls Asia the “greatest of all world-economies,” which “taken as a whole, consisted of three gigantic world-economies,” Islam, India, and

China. He even allows that "between the fifteenth and eighteenth centuries, it is perhaps permissible to talk of a single world-economy embracing all three." Toward the end of this period he observes that the center of this single economy became stabilized in the East Indies (beyond the boundaries of these three economies) in a network of maritime traffic comparable to that of the Mediterranean or the Atlantic coasts of Europe.¹⁰ Of India he writes that for centuries it had been "subject to a money economy, partly through her links with the Mediterranean world." Gold and silver were "the indispensable mechanisms which made the whole great machine function, from its peasant base to the summit of society and the business world." Braudel suggests that the foundation of Europe's trade with India was the low wages of the "foreign proletariat" there, which produced the cheap exports exchanged for the inflow of precious metals to India.¹¹

As "a historian of the Mediterranean," Braudel declares himself "astonished," to find that Red Sea trade in the late eighteenth century was still the same "vital channel" in the outflow of Spanish-American silver to India and beyond as in the sixteenth century. He might have noted how American silver reached this economy not only via the Red Sea and the Levant, but also around the South African cape, and with the Manila galleons. Braudel did observe that the "influx of precious metal was vital to the movements of the most active sector of the Indian, and no doubt Chinese economy."¹²

According to one historian, the "series of interconnected regional markets dispersed and overlapping around the globe" were really a "world market for silver." Perhaps as much Spanish-American silver crossed the Pacific to Asia, where it competed with Japanese silver, as crossed the Atlantic.

Like exchanges elsewhere, trade in the Far East was based on goods, precious metals and credit instruments. . . . European merchants . . . could apply to the moneylenders in Japan or in India . . . and to every local source of precious metals afforded them by the Far East trade. Thus they used Chinese gold . . . silver from Japanese mines . . . Japanese gold coins . . . Japanese copper exports . . . gold produced in Sumatra and Malacca . . . [and] the gold and silver coins which the Levant trade continued to pour into Arabia (especially Mocha), Persia and north-west India. . . . [The Dutch East India Company] even made use of the silver which the Acapulco galleon regularly brought to Manila.¹³

Temporary shortages of silver had an impact on Asia that may have helped bring down China's Ming dynasty. Prior to 1630, the inflow of silver from Spanish America and Japan promoted the monetization of the Chinese economy. The abrupt decline in silver production during the

world recession after 1630 caused economic turmoil and bankrupted the Ming government, making it an easier prey to the Manchus in 1644. One scholar argues that it was no coincidence that the British monarchy was overthrown in 1640, and the Turkish government nearly fell at about the same time.¹⁴

Moreover, Braudel also finds a *de facto* global if not a world economy beyond the monetary sphere. "Long-term control of the European world-economy evidently called for the capture of its long-distance trade, and therefore of American and Asian products." Braudel wrote as follows:

Who could fail to be surprised that wheat grown at the Cape, in South Africa, was shipped to Amsterdam?...Or that sugar from China, Bengal, sometimes Siam, and, after 1637, Java, was alternately in demand or out of it in Amsterdam, depending on whether the price could compete in Europe with that of sugar from Brazil or the West Indies? When the market in the mother country was closed, sugar from the warehouses in Batavia was offered for sale in Persia, Surat, or Japan. Nothing better demonstrates how Holland in the Golden Age was already living on a world scale, engaged in a process of constant partition and exploitation of the globe....One world-economy (Asia)...[and] another (Europe)...were constantly acting on one another, like two unequally laden trays on a scale: it only took an extra weight on one side to throw the whole construction out of balance.

Few historians have tried to determine whether and how cycles coincided across the supposed boundaries of these economies, yet such evidence could reveal much about whether they formed a single world economy.¹⁵

Braudel himself offers only a few indications of simultaneity across the boundaries of his world-economies. He devotes a special section to conjunctures, considers fifty-year cycles, as well as others that are twice as long and more; of these he writes that "four successive secular cycles can be identified, as far as Europe is concerned." On the one hand Braudel claims that "the world-economy is the greatest possible vibrating surface.... It is the world-economy at all events which creates the uniformity of prices over a huge area, as an arterial system distributes blood throughout a living organism." Yet, on the other hand, Braudel observes that "the influence of the world-economy centered in Europe must very soon have exceeded even the most ambitious frontiers ever attributed to it.... The really curious thing is that the rhythms of the European conjuncture transcend the strict boundaries of their own world-economy." Furthermore, "Prices in Muscovy, in so far as they are known, lined up with those of the

West in the sixteenth century, probably by the intermediary of American bullion, which here as elsewhere acted as a "transmission belt." Similarly, Ottoman prices followed the European pattern for the same reasons.¹⁶

Braudel then demonstrated how such exchange transcended the economic boundaries he describes since the system extends throughout the global economy. Indeed, he observes "knock-on effects" as far away as Macao, even beyond the Manila galleon route. He also remarks that "historians (Wallerstein included) have tended to underestimate this type of exchange." Yet, Braudel underestimates this exchange as well. After reproducing a graph of the yearly fluctuations of Russia's exports and its trade balance between 1742 and 1785, he only observes "two short lived drops in the [trade balance] surplus, in 1772 and 1782, probably as a result of arms purchases." The graph also shows a third big drop in 1762–63. All three coincide with a sharp drop on the graph of Russian exports, whatever may have happened to imports of arms or anything else.¹⁷

These three short periods occurred in Russia in the same years as three world economic recessions, which Braudel discusses at some length in another chapter without making the connection. In still another chapter, Braudel reproduces a graph of Britain's trade balance with its North American colonies between 1745 and 1776 that shows sharp declines in British imports, and lesser declines of exports in the same years, 1761–63 and 1772–73. But again Braudel does not look for connections between these recessions. This omission is curious since about the first of these recessions he writes that "with the currency shortage, the crisis spread, leaving a trail of bankruptcies; it reached not only Amsterdam but Berlin, Hamburg, Altona, Bremen, Leipzig, Stockholm and hit hard in London." Regarding the next recession Braudel observes catastrophic harvests in all of Europe in 1771–72 and famine conditions in Norway and Germany. Moreover, he asks:

Was this the reason for the violent crisis, aggravated possibly by the consequences of the disastrous famine which hit India in the same years 1771–72, throwing into confusion the workings of the East India Company? No doubt these were all factors, but is the real cause not once more the periodic return of a credit crisis? . . . Contemporary observers always connected such crises to some major bankruptcy.

Finally, in the chapter on the North American colonies, Braudel also refers to the Boston Tea Party in 1773. Once again, he makes no connection between this event and those he analyzes elsewhere in the world at the same time. He might not have made such connections because he took his several world-economies too seriously or doubted the wisdom of one historian being able to "bring together in a single analysis the scattered

fragments of a history still insufficiently explored by research.” Wallerstein briefly refers to a “slump” after the Seven Years War in 1763, and in passing to “the immediate postwar trade depression” in the 1780s, but makes no mention at all of the intervening recession in the 1770s.¹⁸

Yet, the Stamp, Quartering, and Townshend Acts were passed because recessionary conditions, the vast war debt, and the elimination of the French threat in North America encouraged the British to increase taxes on the Americans. . . . Probably far more important, though less readily visible in its effects was the 1764 prohibition against the issue of bills of credit and paper money in all of the colonies. This resulted in a deflationary shortage of money and severe hardship to debtors. Such economic conditions and British policies led to the American Revolution, but not before halfway around the world, the British East India Company’s ravaging administration of its newly conquered Indian territories has brought on, simultaneously, the severe famine of 1770–72 and the virtual bankruptcy of the company. . . . [In] Britain the parliament was persuaded to safeguard the national interest and that of the company’s stockholders by finding a suitably profitable market for the stock of otherwise unsalable tea on hand. The result was the Tea Act of 1773, which sought to dump the tea under what amounted to monopoly privileges for the company on the market of the Americans. The Americans reacted by dumping it in Boston Harbor during the Boston Tea Party. The British response, the Quebec and Intolerable Acts of 1774, escalated economic conflict into political repression and rallied support for the Declaration of Independence.¹⁹

In Braudel’s third period of crisis, he notes changes in both British and Russian balances of trade, which were generated by these recessions (and not just increased Russian arms imports). The same recession, however, also had more important repercussions in France, where it led to revolution in 1789, and in the new American confederation. There, hard times in the early 1780s and “the more acute economic downturn of 1785–86 and the [resultant] massive popular political movements, such as Shays’ Rebellion in 1786, renewed and increased political support for the federalists” and the replacement of the Articles of Confederation by the American Constitution in 1787.²⁰ To return to Russia, it hardly seems that its economy could have been so remote if these three crises were connected to simultaneous events in Europe, North America, and India during what should be termed a worldwide economic crisis from 1762 to 1790. Fifty-year, or Kondratieff, cycles in the world economy have been identified elsewhere, with some scholars counting as many as nineteen such cycles. A study of Kondratieff

cycles in Tokugawa Japan since 1600 demonstrates that their timing coincided with those in Europe. Moreover, one scholar notes that "prices in China were very roughly in sync with prices in Europe. . . . If we take this movement of prices as a gauge, China could be considered a part of the 'world economy' at this time."²¹

In a review of the medieval period, Braudel noted how the decline of the Champagne fairs at the end of the thirteenth century coincided with a "series of crises of varying duration . . . affecting the whole of Europe at the time, from Florence to London, heralding what was to become, in conjunction with the Black Death, the great recession of the fourteenth century." Wallerstein also emphasized European cyclical decline from 1250 to 1450 in a pattern "clearly laid out and widely accepted among those writing about the late Middle Ages and early modern times in Europe."²²

It is important to emphasize that this decline was not limited to Europe. One Indian historian asks what accounts for Ceylon's "sudden, catastrophic demise after about A.D. 1236? First of all, let us note that the Sinhalese collapse was not unique. The period from the 1220s to the 1350s was one of deep crisis for many societies in Asia. . . . Were these events all coincidence?"²³

The simultaneity of these catastrophes was no coincidence. Janet Abu-Lughod, who proposes a thirteenth-century world system, finds first a system-wide expansion and then simultaneous economic and demographic crises across all of Eurasia from the early thirteenth century onward. This multipolar world system came and went in little more than a century and was later replaced by another one centered in Europe. Braudel makes a similar argument about Europe: crises "mark the beginning of a process of destructuration: one coherent world system which has developed at a leisurely pace is going into or completing its decline, while another system is being born amid much hesitation and delay." Surely a more coherent argument would be that such European restructuring forms the crisis phase of a long cycle in a global economy with a continuous cyclical development. The economic expansion, noted by Abu-Lughod, started earlier and lasted between about 1050 and 1250 on a global basis, encompassing Sung China, Southeast Asia, India, West Asia, and the Mediterranean. The whole period 1250–1450 brought on a worldwide economic crisis. These expansions and contractions may have been phases in a long cyclical process across all of Eurasia that reaches back at least to 1700 B.C., 3000 B.C., or even earlier. Braudel and others demonstrate that the wealth, prosperity, and trade of the Asian economies, relative to Europe, persisted well into the eighteenth century. Braudel argues that the shift of the center of gravity to the Atlantic required at least two centuries after 1492. "The general decadence comes over the Mediterranean in the seventeenth century. . . . [N]ot in the sixteenth, as is usually claimed."

Moreover, the Asian maritime trade had not displaced the Asian caravan trade or the place of Central Asia in the sixteenth century. In 1600, most silk still moved overland by caravan. Moreover, twice as much spice was brought to Europe by caravan through Central Asia as by ship. Thus, through the sixteenth century, Central Asia and the Turkish Empire continued to maintain their place in overland trade against both the South Asian maritime trade and the West Asian, Mediterranean, and Atlantic trades. The seventeenth-century world economic crisis affected all of these regions, including the Americas. In the eighteenth century trade revived across Central Asia, albeit along a more northerly path, and prospered on the Asian maritime route as well.²⁴

Braudel also writes that "it was only because the accessible markets of the Far East formed a series of coherent economies linked together in a fully operational world-economy, that the merchant capitalism of Europe was able to lay siege to them and to use their own vitality." Europe penetrated these vital markets partly because of its new naval and military technology, but mostly through silver and gold from America after the European conquest incorporated America into the world economy. "In the end, the Europeans had to have recourse to the precious metals, particularly American silver, which was the 'open sesame' of these trades." Braudel continues: "From the start, Spanish America had inevitably been a decisive element in world history." He then asks "Is not America . . . perhaps the true explanation of Europe's greatness?"²⁵

Meanwhile, Braudel notes, "India by expanding eastward was passing on the impact she had felt from the 'far West,' that is the Mediterranean. Is not the connection between Europe and India, an ancient, creative one in every respect?" Before acquiring American silver Europe had scarce means with which to further this connection, but that the ancient connection existed and vitally affected all these economies is beyond dispute. Yet Braudel, Wallerstein, and so many others do dispute it, against the evidence, including that which they themselves provide.²⁶ Braudel notes that the size of the Asian economies far exceeded that of Europe. In 1688, the director of the British East India Company observed that Indian "trade with all the Eastern nations . . . is ten times as much as ours and all the European nations put together." Braudel cites estimates of the comparative gross national products for Western Europe, North America, Russia, and Japan, which he contrasts with the rest of Asia and Latin America. In 1750, the first area had \$35 billion U.S. (in 1960 dollars) and the second \$120 billion. So, after regrouping these regions into an "eastern" economy and a "western" one (whose income was so dependent on Latin American gold and silver) the former was still over five times larger.²⁷

It may be plausible to say that Europe "laid siege" to this richer economy by drawing on its new supply of money, but much less plausible to claim

that Europe “incorporated” it into its own economy, as Wallerstein and others would have it. According to him, it is only from 1733 to 1817 that the European economy “began by incorporating zones which had already been in its external arena since the sixteenth century—most particularly and most importantly, the Indian subcontinent, the Ottoman empire, the Russian empire, and West Africa.”²⁸

Yet, earlier Wallerstein found that by the end of the fourteenth century the Indian subcontinent emerged from this crisis as a core production area of cotton textiles in the world economy and became the beneficiary of a huge inflow of bullion as a result of trade surplus. India’s trade with West Asia increased exponentially over the next several centuries and tied the economic fates of cities on both sides of the Arabian sea closely together. . . . At the same time, the maritime trade of India to the east, connecting to the China-Malay trade, experienced a new resurgence, following Sung China’s decision to lift its earlier ban on merchant trade.

Although economic recovery in this nexus was evident from the mid-fifteenth century, Wallerstein and his collaborators are only willing to speak of an evolving Indian Ocean economy not a world economy.²⁹ Wallerstein acknowledges that these centers dominated trans-regional trade and that they “lived at the same pace as the outside world, keeping up with the trades and rhythms of the globe.” Nevertheless, he insists that there were three autonomous economic systems—the Indian Ocean economy, China, and the Mediterranean-European economy—which merely converged at intersections and that “their riches accumulated from their intermediary role in the trade between different world-systems,” not from the existence of a single world economy. Yet, he notes that the “swift collapse of these cities once their fulcral positions were undermined” occurs at these intersections. Indeed, India used its intermediary role in the global economy to accumulate American silver.³⁰

Wallerstein and his collaborators conclude that despite the temporal contemporaneity of post-1400 expansion of networks of exchange and intensification of relational dependencies in Europe and in the world of the Indian Ocean, the processes of large-scale sociohistorical transformation in the two historical systems were fundamentally dissimilar. In one zone, it led to the emergence of the capitalist world-economy. In the other, it led to an expanded petty commodity production that did not lead to a real subsumption of labor. Wallerstein’s nearsighted view insists on seeing several economies where his own evidence underscores the existence of a single world economy.³¹ In light of the evidence for one world economic system presented by Braudel and Wallerstein, four important questions need to be asked. Where did the world system arise? Where was it centered? Which peoples and regions were inside or outside this global system? Was or is the world system “capitalist”? These questions and Braudel

and Wallerstein's model of a European-based, modern world-system raise important ideological, political, geographic, and cultural issues.

Braudel and Wallerstein believe that the world-system arose and was centered in Western Europe—and almost everybody agrees, if they accept the world system idea, and often even if they do not. The charge of Eurocentrism is dismissed by the “fact” of Western Europe's historical origins and subsequent domination of the world system. Yet is this fact or a mere theoretical construction of a regional economy centered in Western Europe? Braudel himself argues that “long-term control of the European world-economy evidently called for capture of its long-distance trade, and therefore of American and Asian products.” American products of gold and silver were produced under European management, but Asian products had long been produced for a vast, complex world economy whose products and trade the Europeans sought to capture.³²

Once we recognize a world economic system, it cannot have arisen, nor could it have been centered, in Europe. Its origin must have been outside of Europe, somewhere in Asia. This challenges traditional views and raises questions about how the West came to dominate a system created in the East. As one scholar notes, “[T]he fall of the east precedes the rise of the west.” Thus, the “Rise of the West” represents a shift from East to West within the same world economic system. Europeans did not incorporate Asia, but eventually succeeded in changing the terms of their own long-standing marginal attachment to the Asian-dominated global economy. Europe's trump card was American silver. Yet even with this card, it took the Europeans centuries after 1500 to muscle in on the Asian trade. This recognition also challenges the notion of European exceptionalism.³³

For Braudel and Wallerstein the world economic system was centered in Western Europe. Neither state clearly just when and where this hegemony was exercised. For Braudel, hegemonic centers jumped around rather quickly from city to city. For Wallerstein, the hegemonic center moved slowly from Portugal, to Amsterdam, to England, and later to the United States. Periods of hegemony were interrupted by periods of rivalry between powers. This intermittent rivalry implies that European hegemonic power has not been quite as much of a constant as Braudel and Wallerstein claim. Nor have hegemons such as Venice, Lisbon, or Amsterdam, been aggressive or strong states. In the global economy, hegemony has been even more diffuse and uncertain. Moreover, European hegemony arrived later than in Wallerstein's more limited system. In Asia, there was no dominant hegemonic power before the British in the nineteenth century. Even then, it is disputable how strong British hegemony really was and whether it even matched that of the Mongols in the thirteenth century.³⁴

From the perspective of the global economy, any temporary existence of a hegemonic center is more the exception than the rule. Rivalry among regional centers has occurred for thousands of years, as Braudel claims:

[I]n the thousand years or so before the fifteenth century, Far Eastern history is simply a monotonous repetition of the same events; one port would rise to prominence on the shores of the Red Sea, only to be replaced in time by one of its identical neighbors. The same thing happened along the coast of the Persian Gulf or in India, or in the islands and peninsulas of the East Indies. European domination of this rivalry has not been due to any European exceptionalism or even to its technological advantage until the last two centuries. The initial factor, as Braudel seems to recognize, was Western Europe's belated access to American gold and silver, with which it bought into Asian trade and then tried to exclude others (including other Europeans) from it.³⁵

Braudel's and Wallerstein's focus on Western Europe and the Americas is too limited and neglects the active participation of the rest of Europe, Asia, and Africa in a world economy. Moreover, until the 1800s, as Braudel himself shows, economic life in many of these regions remained equally active and richer than in Europe. Asia was not incorporated into the European economy; Europeans bought into and outcompeted Asian rivals with American money. All this places the social relations in and among these Afro-Eurasian societies and "world-economies" in a different perspective. A wider, global economic view throws a rather different light on modern history.³⁶

What does it mean to call European enterprise uniquely "capitalist"? The answer has important ideological and political implications. Both Braudel and Wallerstein address this question, but Braudel differs with Wallerstein and treats the question somewhat more satisfactorily. The Eurocentric debates about the transition from feudalism to capitalism have antihistorical limitations that merit examination.³⁷

According to Braudel "capitalism did not wait for the sixteenth century to make its appearance. We may therefore agree with Marx, who wrote (though he later went back on this) that European capitalism—indeed he even says capitalist production—began in thirteenth-century Italy.... I do not share Immanuel Wallerstein's fascination with the sixteenth century" as the time the world capitalist system emerged in Europe. Braudel is "inclined to see the European world-economy as having taken shape very early on." Indeed he observes "European expansion from the eleventh century" when it was "suddenly covered with towns—more than 3,000 in Germany alone." "This age marked Europe's true

Renaissance." Furthermore, "the merchant cities of the Middle Ages all strained to make profits and were shaped by the strain." Braudel concludes that "contemporary capitalism has invented nothing... By at least the twelfth century... everything seems to have been there in embryo... bills of exchange, credit, minted coins, banks, forward selling, public finance, loans, capitalism, colonialism—as well as social disturbances, a sophisticated labour force, class struggles, social oppression, political atrocities." Braudel also doubts that capitalism was invented in twelfth- or thirteenth-century Venice. "Genoa seems always to have been, in every age, the capitalist city par excellence." Several other Italian cities also had capitalist activities earlier than Venice. In all of them, "money was constantly being invested and reinvested," and "ships were capitalist enterprises virtually from the start." He further notes that "[i]t is tempting too to give Antwerp the credit for the first steps in industrial capitalism, which was clearly developing here and in other thriving towns of the Low Countries" in the sixteenth century.³⁸

Moreover, the term "capitalism" also seems to apply at the most macroeconomic level, for "if today's cycles do in fact have some resemblance to those of the past... there is a certain continuity between ancient regime and modern economies: rules similar to those governing our present experience may have operated in the past."³⁹ Braudel, however, also cast doubt on the idea that capitalism was invented in Western Europe and then exported to Asia: "Everywhere from Egypt to Japan, we shall find genuine capitalists, wholesalers, rentiers of trade, and their thousands of auxiliaries, commission agents, brokers, money-changers, and bankers. As for the techniques, possibilities or guarantees of exchange, any of these groups of merchants would stand comparisons with its western equivalents."⁴⁰

Braudel avers that "the rest of the world... went through economic experiences resembling those of Europe." On the other hand, referring to North and West Africa before the Europeans arrived, he writes that "once more we can observe the profound identity of action between Islam's imperialism and that of the West." Braudel wants to "challenge the traditional image" that describes Asiatic traders as "high-class peddlers." Moreover, after Braudel writes of Asians taking turns in a monotonous repetition for a thousand years of shifts in economic dominance, he concludes that "[f]or all the changes, however, history followed essentially the same course."⁴¹

If we asked what changes occurred in or after 1500 as per Wallerstein, the answer would be not much. Braudel quotes a contemporary French sea captain writing from the Ganges River in India:

The high quality of merchandise made here... attracts and always will attract a great number of traders who send vessels to every part of the Indies from the Red Sea to China. Here one can see the assembly

of nations of Europe and Asia... reach perfect agreement or perfect disunity, depending on the self-interest which alone is their guide.⁴²

No Europeans, including their Portuguese vanguard, added anything of their own, only the money they derived from the conquest of America. A standard work on Asian trade notes that "the Portuguese colonial regime, then, did not introduce a single new element into the commerce of southern Asia.... The Portuguese colonial regime, built upon war, coercion, and violence, did not at any point signify a stage of "higher development" economically for Asian trade. The traditional commercial structure continued to exist."⁴³ Even Wallerstein recognizes "an uncomfortable blurring of the distinctiveness of the patterns of the European medieval and modern world":

Many of these [previous] historical systems had what we might call proto-capitalist elements. That is, there often was extensive commodity production. There existed producers and traders who sought profit. There was investment of capital. There was wage-labor. There was *Weltanschauungen* consonant with capitalism.... "Proto-capitalism" was so widespread one might consider it to be a constitutive element of all the redistributive/tributary world-empires the world has known.... For they did have the money and energy at their disposition, and we have seen in the modern world how powerful these weapons can be. Wallerstein's proto-capitalism also negates the uniqueness of his "modern-world-capitalist-system." He even acknowledges "All the empirical work of the past 50 years on these other systems has tended to reveal that they had much more extensive commodification than previously suspected."⁴⁴

Thus, Europe's incursion into Asia after 1500 succeeded only after about three centuries, when Ottoman, Moghul, and Qing rule was weakened for other reasons. In the global economy, these and other economies competed with each other until Europe won. Historians should concede that there was no dramatic, or even gradual, change to a capitalist economy; and certainly none beginning in Europe in the sixteenth century.

In conclusion it is useful to cite an Indian historian who writes that "the ceaseless quest of modern historians looking for the 'origins' and roots of capitalism is not much better than the alchemist's search for the philosopher's stone that transforms base metal into gold." It is better for historians to abandon the chimera of a uniquely capitalist mode of production emerging in Western Europe. It is far more accurate and important to recognize that the fall of the East preceded the rise of the West, and even that is only true if we date the rise of the West after 1800. The West

and the East were only parts of a single, age-old, world economic system, within which all of these changes took place, then and now. The historian Leopold von Ranke is known for having pleaded for writing history "as it really was," but he also wrote that there is no history but world history.⁴⁵

Notes

1. Fernand Braudel, *The Perspective of the World*, Vol. 3 of *Civilization and Capitalism 15th–18th Century* (Berkeley, CA: University of California Press, 1982); Immanuel Wallerstein, *The Modern World-System*, 3 vols. (New York: Academic Press, 1974–89). For an example of European exceptionalism, see E. L. Jones, *The European Miracle: Environments, Economies and Geopolitics in the History of Europe and Asia* (Cambridge: Cambridge University Press, 1981). For challenges to European exceptionalism, see Janet Abu-Lughod, *Before European Hegemony: The World System A.D. 1250–1350* (New York: Oxford University Press, 1989); K. N. Chaudhuri, *Asia before Europe Economy and Civilisation of the Indian Ocean from the Rise of Islam to 1750* (Cambridge: Cambridge University Press, 1990); J. M. Blaut et al., *1492: The Debate on Colonialism, Eurocentrism, and History* (Trenton, NJ: Africa World Press, 1992); Andre Gunder Frank and Barry Gills, eds., *The World System: Five Hundred Years or Five Thousand?* (New York: Routledge, 1993).
2. Andre Gunder Frank, "A Plea for World System History," *Journal of World History*, Vol. 2, No. 1 (Spring 1991): 1–28; "Transitional Ideological Modes: Feudalism, Capitalism, Socialism," *Critique of Anthropology*, Vol. 11, No. 2 (Summer 1991): 171–188.
3. Braudel, *Perspective*, pp. 24, 69. Both Braudel and Wallerstein stress the difference between "the world economy" (without a hyphen), of which there was only one, and "world-economy" (with a hyphen), of which there were several.
4. Braudel, *Perspective*, p. 70.
5. *Ibid.*, pp. 25–45, 70–88.
6. Andre Gunder Frank, "Bronze Age World System Cycles," *Current Anthropology*, Vol. 34, No. 4 (August–October 1993): 383–429; Barry Gills and Andre Gunder Frank, "World System Cycles, Crises, and Hegemonial Shifts 1700 B.C. to 1700 A.D.," *Review*, Vol. 15 (1992): 621–687.
7. Braudel, *Perspective*, pp. 441, 462, 444, 448, 463.
8. *Ibid.*, pp. 458, 442, 467, 471, 475, 476.
9. *Ibid.*, pp. 480–481.
10. *Ibid.*, pp. 484, 486–487.
11. *Ibid.*, pp. 498, 528, 500, 520–521.
12. *Ibid.*, p. 491.
13. Dennis O. Flynn, "Comparing the Tokugawa Shogunate with Hapsburg Spain: Two Silver-Based Empires in a Global Setting," in James D. Tracy, ed., *The Political Economy of Merchant Empires: State Power and World Trade, 1350–1750* (Cambridge: Cambridge University Press 1991), pp. 332–359.
14. Jack A. Goldstone, *Revolutions and Rebellions in the Early Modern World* (Berkeley, 1991); William S. Atwell, "Some Observations on the 'Seventeenth Century Crisis' in China and Japan," *Journal of Asian Studies*, Vol. 45, No. 2 (May 1986): 223–244.
15. Braudel, *Perspective*, pp. 211, 220.
16. *Ibid.*, pp. 77, 83, 76 (emphasis in original).
17. *Ibid.*, pp. 76, 463.
18. *Ibid.*, pp. 267–273, 419; Wallerstein, *The Modern World System*, pp. 198, 228.
19. Andre Gunder Frank, *World Accumulation 1492–1789* (New York, 1978), pp. 206–208.
20. *Ibid.*, p. 202; Braudel, *Perspective*, p. 275.
21. Gunder Frank, *World Accumulation*; George Modelski and William R. Thompson, "Kondratieff Waves, the Evolving Global Economy, and World Politics: The Problem of Coordination," Paper presented at the N. D. Kondratieff Conference, Moscow, March 17–19, 1992; George Modelski, "Sung China and the Rise of the Global Economy,"

- Manuscript, University of Washington, 1993; Mark Metzler, "Capitalist Boom, Feudal Bust: Long Waves in Economy and Politics in Preindustrial Japan," Manuscript, University of California, Berkeley, 1992, p. 39 (emphasis in original).
22. Braudel, *Perspective*, p. 114; Immanuel Wallerstein, "The West, Capitalism, and the Modern World-System," *Review*, Vol. 15, No. 4 (Fall 1992): 589.
 23. Chaudhuri, *Asia before Europe*, pp. 246–248.
 24. Fernand Braudel, *El Mediterraneo y el Mundo Mediterraneo en la Epoca de Felipe II* (Mexico D.F., 1953), 1, p. 368 Niels Steensgaard, *Carracks, Caravans, and Companies: The Structural Crisis in the European-Asian Trade in the Early 17th Century* (Copenhagen, 1972), pp. 56–57; Andre Gunder Frank, *The Centrality of Central Asia* (Amsterdam, 1992).
 25. Braudel, *Perspective*, pp. 496, 217, 414, 387.
 26. *Ibid.*, p. 524.
 27. *Ibid.*, pp. 494, 534.
 28. Immanuel Wallerstein, *The Modern World-System*, Vol. 3, *The Second Era of Great Expansion of the Capitalist World-Economy 1730–1840s* (New York, 1989), p. 129.
 29. Ravi Arvind Palat and Immanuel Wallerstein, "Of What World System Was Pre-1500 'India' a Part?" Paper presented at the International Colloquium on "Merchants, Companies and Trade," Maison des Sciences de l'Homme, Paris, May 30–June 2, 1990.
 30. *Ibid.*, pp. 30–31; Braudel, *Perspective*, p. 18.
 31. Palat and Wallerstein, "Of What World System Was Pre-1500 'India' a Part?" p. 40.
 32. Braudel, *Perspective*, p. 211.
 33. Abu-Lughod, *Before European Hegemony*, p. 338; Gunder Frank, "A Plea for World System History"; "Transitional Ideological Modes: Feudalism, Capitalism, Socialism"; Gunder Frank and Gills, "The Five Thousand Year World System: An Interdisciplinary Introduction"; "World System Economic Cycles and Hegemonial Shift to Europe 100 B.C. to 1500 A.D."; Gills and Frank, "World System Cydes, Crises, and Hegemonial Shifts."
 34. Barry K. Gills, "Hegemonic Transitions in the World System: Accumulation and the Making of World Order," in Andre Gunder Frank and Barry K. Gills, eds., *The World System: Five Hundred Years or Five Thousand?* (New York, 1993); Gills and Gunder Frank, "World System Cycles, Crises, and Hegemonial Shifts."
 35. Gills, "Hegemonic Transitions in the World System"; Braudel, *Perspective*, p. 485; Gunder Frank, *World Accumulation 1492–1789*; Blaut, "Where Was Capitalism Born?" in 1492.
 36. Gills and Gunder Frank, "World System Cycles, Crises, and Hegemonial Shifts"; Gills, "Hegemonic Transitions in the World System"; Gunder Frank and Gills, "The World System."
 37. Braudel, *Perspective*, p. 57; R. H. Hilton, ed., *The Transition from Feudalism to Capitalism* (London, 1976); T. Aston and C. Philpin, eds., *The Brenner Debate: Agrarian Class Structure and Economic Development in Pre-Industrial Europe* (Cambridge, 1985); Jones, *The European Miracle*; John A. Hall, *Powers and Liberties: The Causes and Consequences of the Rise of the West* (London, 1985); Blaut, "Where Was Capitalism Born?" in 1492.
 38. Braudel, *Perspective*, pp. 57, 92–93, 94, 91; Oliver Cox, *The Foundations of Capitalism* (New York, 1959); Braudel, *Perspective*, pp. 118, 127–128, 157, 129, 156.
 39. *Ibid.*, p. 73.
 40. Braudel, *Perspective*, pp. 387, 434, 485, 486; J. C. van Leur, *Indonesian Trade and Society: Essays in Asian Social and Economic History* (The Hague, 1955), p. 87.
 41. Braudel, *Perspective*, p. 486.
 42. *Ibid.*, p. 511.
 43. Van Leur, *Indonesian Trade and Society*, pp. 117–118.
 44. Wallerstein, "The West, Capitalism, and the Modern World-System," pp. 586–587, 613, 575.
 45. Chaudhuri, *Asia before Europe*, p. 84.

CHAPTER EIGHT

*Debunk Mythology, Reorient Reality*¹

A sad comment on the state of the art on what Ken Pomeranz has called THE GREAT DIVERGENCE in the nineteenth century are the 160 pages of “Conference Proceedings” that reproduce word by word the “Discussions at the Conference” in search of explanations particularly of “World Economic Primacy in the 19th Century.” This major international conference was sponsored by the Luxembourg Institute for European and International Studies in 2002. The speakers at the conference included such luminaries of “world” economic and other history as Moses Abramovitz, Martin Bronfenbrenner, Rondo Cameron, Charles Kindleberger, Angus Maddison, Peter Mathias, William McNeill, Joel Mokyr, Patrick O’Brien, W. W. Rostow, and Jan de Vries, as well as the editors and others. Yet every one of them referred exclusively to characteristics of Britain and other countries in what one of the editors called “a standard discussion” (370) and in which a participant still insisted on an alleged claim that “at the mid-19th century . . . primacy [of Britain] is important and I think unprecedented in its degree” in a half dozen respects with its “structural change is very rapid, idiosyncratic” (351–352). Yet throughout the discussion, not one reference was made to the structure, operation, and transformation of the GLOBAL WORLD political economy within which Britain rose from being a relatively marginal player to momentary “primacy” (but not yet at mid-century) and within a couple of generations declined again. This myopia of our leading lights still in the twenty-first century is truly looking for one’s watch not where it was lost or is to be found, but only where the all-too dim and narrowly focused Euro-American street light is.

(Francois Crouzet and Armand Clesse, eds., 2003: 348–410)

This book is a nineteenth-century sequel to my *Reorient*, which stopped in 1800. I now join Kenneth Pomeranz (2000) when he writes, "I would emphasize an effort to re-think the 19th century," which as he observes "has been abandoned by a whole generation of scholars." Also Edmund Burke (2000: 1) notes, "Why the nineteenth century? Because it seems to me to be the piece that has thus far been left out of the rethinking of modern world history. . . . We're still far from being able to devise a truly world-centered historical framework for the nineteenth century." John F. Kennedy told us that "the great enemy of truth is very often not the lie—deliberate, contrived, and dishonest—but the myth—persistent, persuasive and unrealistic" (Frank, 1998: x). If that is true, then it has certainly been persistently, and persuasively, so, to meld the titles of two books, about THE NATURE AND CAUSES OF THE WEALTH AND POVERTY OF NATIONS (Smith, 1776/1937; Landes, 1997). For the received and still-persistent mythological "explanations" are altogether wide of the mark of how THE GREAT DIVERGENCE, as Pomeranz (2000) calls it, emerged out of the structure, function, and transformation of the world economy in the nineteenth century. It was this GLOBAL ECONOMY that really gave rise to a "single world-wide system which also provided the transfer: along, round about routes . . . [of wealth, income, and entropy] to particularly, the United Kingdom . . . by the much less adequately understood system of multilateral settlements of all classes of international accounts" (Hilgerdt, 1942: xx).

They seem to be even less understood today after six additional decades of mythology than when Folke Hilgerdt analyzed it for the League of Nations. However, it is even more important to understand this global system and process today a century after its breakdown after 1913 during the two World Wars, the intervening Great Depression, and the postwar "American Century," because of the efforts to revive and reconstruct it at the turn of the twenty-first century.

Therefore, as in the previous book, we may suitably begin by challenging and debunking a whole series of myths about THE NATURE AND CAUSES OF THE WEALTH AND POVERTY OF NATIONS now in the nineteenth century, which cannot stand up to the evidence. Unfortunately, many of these myths about the nineteenth century still have wide currency. Of course, in this brief chapter it is possible to confront these myths only with a limited amount of evidence. The bulk of the evidence to bury these myths, as well as to support an alternative explanation, is presented only in the chapters that follow. However, even the limited evidence marshaled in this chapter can serve to introduce an alternative explanatory scheme of things to be presented in Chapter 2 and then applied in the subsequent chapters.²

The tired old mythology about the "European miracle" (Jones, 1981), recently resurrected and popularized by Landes (1998), has already been

duly laid to rest R.I.P. Among others, Blaut (1993), Goody (1996), Wong (1997), Frank (1998), and Pomeranz (2000) have challenged and overturned first its historical veracity, and then the therefrom-derived “scientific” basis in Western historical political and economic social theory. The authors thereof range from Marx to Weber and their followers to even Wallerstein (1974) and Frank (1978). Regarding the period before 1800, the debunking is already substantially done and is not worthy of further attention. However, the nineteenth century remains to be rethought, researched, and rewritten.

Our (mis)understanding of the nineteenth century is still replete with dozens of myths, indeed encased in a whole mythology about *The Rise of the West* that centers on an alleged early “industrial revolution” in Britain spurred by mechanizing the cotton textile industry already in the late eighteenth century, and then carried by coal and steam in the first half of the nineteenth century. By that time, it is alleged that Pax Britannia already “ruled the sea,” was “the workshop of the world,” and had its highest income. So for the past century and a half and still in the aforementioned conference, the almost exclusive question has been “how Britain did it”? But as long as we ask the wrong question, we are certain to get the wrong answer.

Fortunately, some of the myths emerging from and underlying this question have of late been increasingly questioned themselves, and a few have even been laid to rest. But even these have a habit of cropping up again and again, even by writers who momentarily see the light but then inconsistently revert to darkness because they seem unable to kick the habit. However, most of the two dozen-plus myths remain alive and kicking, if only because the main question/mindset/habit remains the same. And excepting the newly formed and baptized “California School,” and a very few colleagues elsewhere (whom we will meet again and again later), no one has yet turned the question around to ask either why the West did not remain like the rest, nor even less WHEN and WHAT happened and HOW and WHY the WORLD political economy turned upside-down to make East and West change places during the nineteenth century. THAT is the question posed and the answers sought in THIS book. The most widespread and in many cases still firmly held myths are the following.

The Gap: When and How Much Did the Divergence Really Take Place?

The previously existing world economic landscape, if not already after 1500 as many claim, including Wallerstein (1974) and Frank (1978), was qualitatively transformed in the period 1750–1800, and certainly soon after

1800, as asserted among many others including Wolf (1982) and Frank (1998). NOT SO.

There is virtual unanimity on the thesis that around 1800 average economic productivity and output and income per capita were substantially the same around the world. The disputes are only about whether the West/East ratio was 2:1, 1:1, or 1:1.2–3, and especially if China was ahead of Europe. A century later, GDP had grown sixfold, and GDP per capita threefold in Western Europe, while they had remained almost stable and even declined in Asia (table 4.1). So when did this gap, or as Pomeranz (2000) calls it *The Great Divergence*, develop for real? The received wisdom had it that this gap began already before 1800, around 1800, and certainly by 1850. But further examination of the evidence in the first half of the nineteenth century finds previous world economic indices continued and relations survived and even expanded (but then so did those of entropy), with the notable and important exception of the place and role of India. The East Asian and Chinese-centered Trade-Tribute Commercial System analyzed by Hamashita (1997) and examined in my *Reorient: Global Economy in the Asian Age*, survived into the nineteenth century. Pritchard (1997: 9) summarizes the growth of the absolute gap in incomes from US\$1,286 to \$12,662 between the few rich and the many poor from 1870 to 1990, or from a gap of 8.7 to 45.2. The 1890s saw an accelerated return to global relations and expanding income differentials analogous to that before 1913. By 1895, one hour of labor in the United States was being exchanged on the world market for 80 hours of labor in India, or already double that of 1980.

An historically correct answer to when the divergence, or the replacement of Asia and especially China by Europe and particularly Britain, is of cardinal importance not only for us to know the past, but also to understand the present and foreseeable future. But the answer seems still to be beclouded by widespread historical amnesia. My book, *Reorient: Global Economy in the Asian Age*, documented and rejected opinions that put particularly China into the historical doghouse, as does Maddison (2000) in his book on THE MILLENNIUM. General MacArthur, as head of an occupation force in Japan, suggested in 1951 that “the gradual rotation of the epicenter of world trade was headed back to the Far East whence it started many centuries ago” (Arrighi et al., 1996: 97). Even latter-day new China hands and sinophiles such as Arrighi and his coauthors still write “after several centuries of Western hegemony, leadership in the global economy is reverting from Western to non-Western hands” (ibid. 99). But the evidence examined in this book suggests that this shift from East to West really started only one century and a half ago and the intervening interregnum mostly lasted only ONE century.

The new place in the world of (part of) Europe and North America was not foreordained nor easily achieved. Michael Geyer and Charles Bright correctly note that:

European initiatives collided, overlapped, and interacted with dynamics of parallel crises in other regions and with strategies of competitive self-improvement that were devised to shore up regional power and to fend off or contain external pressures. Historiographic attention [if any!] focuses on East Asia, but elements of these struggles can be observed in the Indian, Persian, Arab, African and Latin American worlds as well. Geyer and Bright (2000: 61)

This book³ generally, and specifically the remainder of this chapter later and the region-by-region reviews of the period 1750 to the 1810s in Chapter 3 and from the 1810s to the 1870s in Chapter 6, bring further ample evidence to support this statement. They show that resistance to European political economic initiatives was largely successful in all these areas until at least the mid-nineteenth century, and even later until 1870. For now, only a brief summary will have to suffice.

Cohen (2000: 19) is among the revisionists and yet he writes:

Clearly the mid-nineteenth century was a watershed in the history of The East Asian international order over which China had been at least nominally dominant for thousands of years had been shattered. Western power had proved itself superior to that of Asia and had enabled a relative handful of Europeans and Americans to overcome the resistance of hundreds of millions of Asians. Advanced military technology, improvements in military discipline and supply, the co-opting of native forces, especially from the Indian subcontinent, contributed mightily to Western success.

But it is by no means clear that even the watershed we have observed was in the mid-nineteenth century. Many of the changes he names himself occurred later. Indeed, the transformation of the structure and operation of the world economy, including within it the absolute and relative places of the “North” and the “South” really only began around 1850; and it has not been as extensive and as profound as we have been led to believe. That is, more careful examination of nineteenth-century reality, as proposed later, belies the now popular beliefs, and still most “scientific” opinion about the supposed Western penetration of the Asian economy during most of the nineteenth century. Without trying by any means morally to justify or politically to support any and all imperialism and colonialism nor any of its consequences, the time has come to review and, where

appropriate, to revise the substantially ideological dogma of Western triumphalism over alleged “traditionalism” elsewhere. On the evidence simultaneously, we may need also to reconsider much of the nationalist appeal to and “defenses” of “traditional” values and its exaggeration of the deformation of “Third World” economies.

Obviously, the erosion of central power in China and Japan also facilitated Western inroads. By 1870, most of East Asia had been opened to Western goods and the influence of Western ideas. Some of the peoples of the region, most obviously those of Southeast Asia, had lost control of their territory, even of their lives. On the other hand, resistance to the Western-organized international system remained strong. Korea, into which Western influence dribbled indirectly from China, had repelled all efforts to force open its doors to trade with the Europeans and Americans. Japanese central authorities had concluded that a conciliatory—and dilatory—policy was the wisest response to evidence of Western might, but powerful provincial forces were determined to fight, to “expel the barbarians.” China, crippled by internal strife, the Qing dynasty arguably dependent on Western support for its survival, retained its territorial integrity. Chinese students of statecraft and many Chinese officials were already laying the groundwork for a movement to strengthen the country and enable it to stand up to its foreign tormentors. For Siam, appeasement had worked, and skillful leadership gave its people reason to hope for their continued independence. The Vietnamese continued to resist French imperialism vigorously. (Cohen, 2000: 271)

Indeed, even Warren Cohen himself offers a revisionist all-Asia summary that stresses both increasing weaknesses but also continuing strengths. Note, however, that Cohen designates the crucial date of changeover as not earlier than 1870, and even after that resistance remained and there was a renewal of economic growth and even of industry in Asia.

The continued expansion of production and often of income in these regions of Asia summarily reviewed earlier (and examined in much further detail in Chapters 3–5 of *Reorient the 19th Century*) for the century from 1750 to 1850 was accompanied by the also continued growth of intra-Asian trade among these regions. Moreover, that trade also continued to be primarily in Asian, and not European, hands. We have already noted previously that Southeast Asian trade grew within the region and to China, the latter in mostly Chinese ships. European shipping actually declined between India and Southeast Asia (Frank, 1998: XX), but Asian trade may have increased both relatively and absolutely (Reid, 1993). Trade in the North China Sea also continued. Particularly in East Asia, which had been organized by and into the China-centered trade-tribute system (see my *Reorient: Global Economy in the Asian Age* and Hamashita cited therein), the intrusion of the West has never really been more than marginally at

its margins. Except for the Philippines and Indonesia all the region's most important nations...from Japan, Korea, and China to Vietnam, Laos, Kampuchea and Thailand had all been nations linked to one another long before the European arrival...[both] directly or through the Chinese center, by diplomatic and trade relations and held together by shared understanding of the principles, norms, and rules that regulated their mutual interactions as a world among worlds" (Arrighi and Silver, 1999: 287). Moreover, the ever-growing Chinese overseas first laborer and merchant and then overwhelmingly merchant diaspora, although also in conflict with the local peoples as manifested by recurrent pogroms against them, nonetheless has also acted as a cement among these regions, and of course, especially with China itself. Evidence emerging from recent revisionist history, and reviewed region by region in the chapters that follow testifies to continued and even enhanced economic activity and social stability also in the regions of Southeast Asia, Central Asia, and West Asia, including much of the Ottoman Empire and Persia until at least the mid-nineteenth century and later. The decline of India, without whitewashing British colonial rule, now also appears less severe in the eyes of new Indian revisionist history. In much of Africa, production, trade, and income grew as the Atlantic slave trade was replaced by more intra-African slavery applied to production for local and regional markets as well as of vegetable oils and other products for rapidly growing export. Also in many newly independent countries of Latin America, nationalist governments and movements resisted the incursions of the "imperialism of free trade" until the 1860s.

On the other side of the equation, we will see summarily (already in this introductory chapter and much more extensively in Chapters 3 and 6) that European growth before 1850 and even 1870 was nothing like we have been led to believe. Indeed, viewed also from Europe outward around the world, revisionist history has been pushing the effective beginning of British and European suzerainty farther and farther back. For instance, Cain and Hopkins's review (2002) of *BRITISH IMPERIALISM* stress on several occasions that the naval, trade, and financial instruments of British imperial power did not come into effective being until the second half of the nineteenth century and often later. So they note about trade and finance in general (Cain and Hopkins, 2002: 401), invisibles (Cain and Hopkins, 2002: 159ff.), commodities (Cain and Hopkins, 2002: 151ff.), sterling (Cain and Hopkins, 2002: 200–201), the gold standard (Cain and Hopkins, 2002: 137ff.), foreign investment (Cain and Hopkins, 2002: 18ff.), Africa (Cain and Hopkins, 2002: 319), and South America, and so on (Cain and Hopkins, 2002: 272).

The date of inflection, if there be any single one, between Asia and Europe is cropping up more and more as 1870 or even 1880. Cemil Aydin et al. (2005) place it in 1880 for relations with the Ottoman Empire.

Paradoxically, that is also the time at which industrialization began to take hold in the major economies of Asia and specifically in Japan, China, and India, as we will see later. But however that may be it bears repeating that Western domination interrupted the Asian one and itself lasted only less than one century; since by 1945 already economic growth in the East exceeded that in the West, and in the half century since then has been at double the rate in the West (Sugihara, 2003: 78–79).

Economic Growth

The industrial revolution was a revolution in industrial growth that took off first in Britain and then elsewhere in Europe mostly on the basis of new industrial technology and productivity, which was developed there and permitted Europe to outpace the rest of the world from 1760 or at least from 1800 on and to do so primarily on or for the domestic market. NOT SO.

In the home of the industrial revolution, nobody seemed to be aware that what they were living through was one. Certainly not any of the great economists. Not Adam Smith in 1776. Not Malthus in 1798 when he launched his population/land thesis in the absence of technological change. Nor Ricardo in 1816 when he emphasized decreasing returns. Not the renowned French economist Jean Baptise Say who still in 1828 predicted that “no machine will ever be able to transport people or goods around like the worst of horses” (quoted in Bairoch, 1997: 1–270). Perhaps that was because in reality there was not all that much to see. Bayly (2004: 172) now observes that “British historians have been particularly insistent on de-emphasizing industrialization as a turning point in both British and global history.” Moreover, “many economic historians argue that industrialization had little effect outside Britain and Belgium until the mid-1850s” (ibid. 171). The current revisionist economic history of Europe has shown that previous suppositions of rapid growth were vastly exaggerated. Jan de Vries (2001: 183) writes that “revisionist studies have effectively removed the central tenets of brevity and rapid acceleration of economic growth and even questioned the traditional significance of mechanical invention,” and refers to a downward revision by more than half for measured growth rates in Britain between 1760 and 1830. Only after 1870 would the average growth rate in Western Europe change from 1 to 2 percent per year. In Britain, this change occurred earlier; but not yet in 1830, let alone 1800 or 1760.

Table 8.1 on European growth constructed on the basis of Bairoch (1997: 1–592) is indicative:

Table 8.1 European economic growth, 1700–1900.

	1700	1800	1860	1900
Population	67	100	143	200
Manufacturing	20	100	292	814
Export	40	100	600	1670

Source: Bairoch (1997: 1–592)

Summarizing, while population doubled, manufacturing increased eightfold and exports over sixteenfold. However, more than three-fifths of this nineteenth-century growth did not take place until the final two-fifths of the century.

The Asian revisionist history referred to earlier shows that economic growth continued in various parts of Asia during the second half of the eighteenth, and also still or again in the first half of the nineteenth centuries. The Chinese economy continued to grow much faster than previously known or admitted until at least the middle of the nineteenth century.

In Southeast Asia, if Reid and his collaborators are right, economic growth of production and income also per capita must have grown over the century from 1760 to 1850. Although exports and trade generally can and usually do grow more rapidly than production (globally about twice as fast), the smoke of a 4 percent annual growth rate and a 3 percent increase per capita must indicate an underlying productive fire as well. The same goes for West Asia, where commerce and exports as per the aforementioned also grew rapidly in the first half of the nineteenth century.

Capital Formation

Britain achieved its definitive “takeoff” in this period through the abrupt acceleration of capital formation. NOT SO.

The claims to this effect by Walt Whitman Rostow and others have long since been altogether disconfirmed by all evidence that the formation of capital in and by Britain, for example, investment as a percentage of income, remained as slow and low as before. Deane (1969, 1972), Deane and Cole (1967), and Habakkuk (1965), have all shown that Rostow’s magic 10–12 percent rate of (re)investment was not reached until the mid-nineteenth century if then, although Pollard and Crossley (1972) found higher percentages for ABSOLUTE investment, which still do not support the Rostowian and others’ claims. As Francois Crouzet (1972: 19) summarized in his introduction to a volume he edited on this debate, the contrary

theses “have been widely accepted in recent years by economic historians. Correspondingly the Rostowian hypothesis of a sharp and dramatic acceleration in capital formation during the ‘takeoff’ has to be abandoned.” Crouzet (1972) adds, however, that Rostow was wrong not in principle but only in timing, since higher rates of capital formation were reached later. However, Rostow has also shown to be mistaken *in toto* (Frank, 1967). Even if he is partly right about the rate of capital formation later in the nineteenth century, the question still comes on what that investment was based if not on domestic saving.

Investment in capital formation in Britain did not exceed 5 percent of national income any time in the eighteenth century until perhaps in the last decade, when it rose to 7 or 8 percent. Even in the nineteenth century, not more than 1 percent of national income was invested in new industrial and commercial enterprises, and the 10 percent level achieved in the 1840s was primarily in the railway boom (Dean and Pratten, 1965; Cameron 1967). In France, the percentage did not rise to 3 percent until the 1840s, and the same was attracted into railroads. Elsewhere on the Continent, investment hovered between 6 and 8 percent even after mid-century. Everywhere, the state was a principal agent, even if less so in Britain, and more so in Germany and Russia (Cameron, 1967: 313ff.). In the latter “the state’s vastly expanded program of railroad construction was the backbone of domestic industrial growth” both on the supply and the Keynesian demand side. The other major factor was military Keynesianism. That was so, despite—or because—of massive foreign investment, which “after 1880 held over half the new common railroad stock” (Weaver, 1974: 80, 82).

If there was no notable growth in the rate of investment, nor of production, in Britain, and still less so in Continental Europe, during the first half of the nineteenth century, several conclusions inevitably follow: Income, total or per capita, derived from them could not have risen more as claimed by the “optimists” who say it did; or if it really did so, that increased income must have been derived from some other source. That and its role must be explained in large part by income derived from multilateral trade by Britain at the expense of others. That is one of the important and most neglected parts of the inquiry into the nature and causes of the wealth of nations. And, if investment grew in the second half of the nineteenth century despite a chronic deficit in the balance of trade (see later), we must ask how that was possible? The answer—again—must be sought not in domestic saving or anything else, and not only in the so far primary focus on production, but in trade and services as per the thesis of “Gentlemanly capitalism” quoted later, and especially in the almost totally neglected multilateral structure of that trade and related payments.

Income Differentials between the West and the Rest

Differentials of per capita income, including that of the masses, grew rapidly in the first half the nineteenth century and became substantial already by 1850. NOT SO.

A seemingly interminable debate has now raged for decades between “optimists” who saw wages and income rising rapidly in Britain already in the early nineteenth century and the “pessimists” who not only disputed the same but said that they actually fell before beginning to rise only well after mid-century. For Britain, the pessimists seem finally to be gaining the argument; and they are increasingly joined by others regarding other parts of Europe.

That has important implications already of course for income differentials between Europe and other parts of the world; and these are now put even further in question by evidence on income growth elsewhere.

Already reviewing “the whole body of wage and price material currently available in print” on Britain three decades ago, M. W. Flinn (1974: 395) concludes that there were few indications of real wage movements either up or down before 1810–14, and after that until mid-century “the gains in real wages were slight” at less than 1 percent per year. In view of research since then, this estimate must be considered to be among the so-called optimists. However, Bayly (2004: 189) summarizes that “recent work has tended once again to the pessimistic view, that life spans, nutrition, and health declined somewhat during the first stages of industrialization.” So, suppose the “pessimists” are right that the distribution of income became more unequal during this period of rising per capita income in Britain (and by extension then in Western Europe and also in the United States?) and that it remained or became more equal with declining income in India throughout and in China later. Chapter 4 will bring evidence to that effect. Now suppose that the optimists are correct in their claim that there was an improvement in income per capita and of the majority of the population in this period in Britain (but only later elsewhere in Western but still not in Eastern or much of Southern Europe). Any such increased income may have been of micro importance to those who received it; but that still does not signify much change on the world, Asian, and European economic macro scale. But suppose that per capita income was about equal in the East and the West, with a lead by China over Europe, as the early calculations by Bairoch and Maddison suggest, and those by Pomeranz and others now confirm, then at the mid-nineteenth century end of this period, the income of the large majority of Chinese and even of Indians may well have continued to equal or exceed that of the British and *a fortiori* of other Europeans. That is what

Pomeranz also argues, as noted in Chapter 4. The distribution of wealth and income in England was more unequal than in the richest provinces of China, where, therefore, the bulk of the population who were poorer than the richest lived better than in England. Pomeranz (2000) concludes that “the diet of laborers in South China Jiangnan seems at least comparable to England, and the percentage of income spent on basic foodstuffs the same.” In all likelihood the next 45 percent of society lived better in Jiangnan than in England and yet the Yangtze Delta was still richer. Sugihara (2004: 75) adds, “Figures for China in [his] Table 1 suggest a fall in living standards, but this is consistent with the fact that living standards of a large population in coastal China rose.” The same was true in parts of Southeast Asia. In both cases living standards in the richer parts were similar to Japan’s.

Regarding India, Prasanan Parthasarathi (2001) among others has been “Rethinking Wages and Competitiveness in the Eighteenth Century: Britain and South India” and writes:

Contrary to three centuries of received wisdom, this comparison reveals that the Indian weaver was not impoverished. In Britain, weekly earnings bought 40 to 140 pounds of grain, while in South India they purchased between 65 and 160Y. There is reason to believe that spinners’ earnings in South India were even higher. “Their claim to a share of the social product was more secure and it was protected through a variety of means. As a result, labourers in South India were consistently able to maintain a superior standard of living.” South Indian cloth was cheaper because money wages were lower, a product of lower grain prices. The cheapness of grain was a consequence of the superior productivity of South Indian agriculture. . . . With this difference in prices, a given quantity of grain could be purchased in South India with only half the money that was needed in Britain. “Adam Smith, always a keen analyst, put forward this very argument in *THE WEALTH OF NATIONS*.” Strange as it may appear, South India’s competitive advantage in cloth lay in its paddy fields. (Parthasarathi, 2001: 84, 86, 92)

Britain Was the Workshop for the World

Britain’s industrial revolution enabled it to dominate the world’s export markets. NOT SO.

Britain had a structural and permanent merchandise trade deficit, which rose from 10 million pound sterling in 1816 to 175 million around 1905–10 to end at 160 million in 1913. That is, throughout that entire

century was Britain even able or required to export as much merchandise, primarily manufactures except for coal, as it imported? And Britain's capacity to export was not a result so much of any higher productivity or competitiveness as it was on having captive markets, especially in its Indian colony to which it was able to export nearly ten times as much as to China (Bairoch, 1997: 887). Yet even with India, Britain ran an export deficit until mid-century (Bairoch, 1997: 885). Its other most secure export markets remained the Caribbean plantations and Latin America. For Britain's European and North American rivals not only practiced import substitution but also export promotion of their own to compete with Britain in third world countries.

In his *ECONOMIC ELEMENTS IN PAX BRITANNIA*, Albert Imlah (1959) writes to demystify the received wisdom:

Britain was, contrary to common assumption and in spite of her head start in applying machine techniques, an importing country throughout this [nineteenth century] period. No quinquennial period would show a positive balance [of exports]. There seems no escape, therefore, from the conclusions that Britain's new industrial system did not create export surpluses.... It is unmistakably clear that, on balance, Britain was an importing country throughout. (Imlah, 1959: 21, 40)

That appears clearly from Imlah's adjustment of declared values better to reflect "real" values and his estimate of the export deficit and import surplus in Britain's balance of merchandise trade, which rose from about 10 percent of British-produced exports until 1835 to 25 percent in the economically depressed 1840s and in the early 1850s, and then to between 30 and 60 percent of exports in the second half of the nineteenth century (*ibid.* 37–38 and Chapters 5 and 6 later). Nonetheless, Imlah still underestimates imports from colonial countries by some 25 percent as Bagchi (2002) demonstrates and we will examine this later. Moreover, of Britain's exports, from 6 to 14, an average of about 10 percent were always reexports of goods produced by others according to Imlah (1959: 37–38), and still more by other estimates. The deficit in the balance of merchandise trade was made up by a surplus of earnings from "invisible" services, of which shipping accounted from half to two-thirds of the total. So in a sense it was not so much in textiles with which Britain never became really competitive (see 1.6 later), but in ships and shipping that British industry dominated the world market.

Patrick O'Brien (2000: 131) reviews "The Reconstruction, Rehabilitation and Reconfiguration of the British Industrial Revolution" and finds that

“the industrial revolution ‘ain’t what it used to be.’” But what was that? For most orthodoxy it “had run its course from the late eighteenth to the first quarter of the nineteenth century”; but for different experts it had different chronologies, for instance, from 1760–1830 and 1783–1832 or 1783–1802 (O’Brien, 2000: 121). Yet now O’Brien (2000: 123) finds “a modern consensus” on the British industrial revolution instead of “renaissance of gradualism [and] the re-emergence of good fortune” according to which “it is now recognized that the British industrial revolution is among the slowest and evolutionary of ‘transitions’ to an industrial economy.” For by the mid- nineteenth century, agriculture was still the largest, and still unmechanized, sector, and

whole swathes of British industry were not transformed until later in the nineteenth century. These included glass, bricks, mining, furniture, shipbuilding, food processing, finished metallurgy, clothing, et cetera... [that] had not altered their modes of production for decades.... Ratios of capital to labour, the tools and techniques... were the same as they had been in 1700 [or even earlier]. Small units of production remained prevalent and “large parts of the country remained untouched by industrialization,” and continued use of human, animal, wind and water power. (O’Brien, 2000: 124–125)

So the new revisionist history has

- redefined the pace and chronology,
 - re-specified the nature, extent and origins,
 - investigated the antecedents,
 - rejected diffusion to Continental Europe and Asia,
 - emphasized contingency,
 - rediscovered military and naval investment and power, and
 - rejected British and European exceptionalism
- (O’Brien, 2000: 121)

As Britain’s industrial competitiveness on the world market eroded ever more at the turn of the century, demand for protection also increased. Moreover “Tariff Reformers raised the specter of a de-industrialized Britain, where crucial industries like steel, vital for defense and for great power status as well as for wealth creation, were lost: there was a pointed time when Britain could no longer maintain her position in the world and would be faced with a breakdown in social order as industry disintegrated” (Cain and Hopkins, 2002: 191). In other words, loss of Britain’s

position in the world was seen as a threat also to its ability to dissipate social entropy. The answer to this threat was seen and sought in “uniting Britain with her white colonies—whose—populations were expected to expand enormously in the twentieth century—by using their joint power to exploit the underdeveloped parts of—Empire” (Cain and Hopkins, 2002: 191). A principal proponent of this policy was Chamberlain. In other words, the policy clearly sought to continue the dissipation of entropy from Britain and Europe to white-settler regions where the land was still able to absorb it at relatively low cost, and for them jointly with Britain to dissipate their common generation of entropy, both physical and social, to the nonwhite areas of the world under their economic and political control.

Particularly challenging and replacing the primacy of industry is also the thesis underlying the influential but still controversial writings of P. J. Cain and A. G Hopkins (2002) about what they call “Gentlemanly Imperialism.” A few select quotations from their recent summary book *BRITISH IMPERIALISM 1688–2000* can illustrate their thesis:

Our main purpose...was to direct attention to the non-industrial forms of capitalism that, in our view, have been greatly underestimated by Historians of modern Britain [7].... This interpretation runs against both an older, Heroic conception of the (Industrial Revolution: and a newer growth-oriented historiography, which tends to equate development with industrialization [35]...[also] writers in the Marxist tradition...have a slotted and crucial role to industrialization [25].... Industrialists were not at the center of economic policy—making before 1914 [148].... The evidence point to the need to revise some of the central features of the historiography of British imperialism during the classic phase of nineteenth century expansion. We have questioned the widespread, and long-standing assumption linking the “triumph of industry” to imperialist expansion [400].... Recognition needs to be given to the fact that economic development was not synonymous with the Industrial Revolution, and that non-industrial activities, especially those connected with finance and services, were far more important, and independent than standard texts of Economic History have allowed. [646] (Cain and Hopkins, 2002)

Moreover, the revisionist literature since the 1970s claims that foreign influence was more benign than previously imagined (Amsden, 2001: 34), and that Chinese, Indian, Ottoman handloom weaves survived British competition and indeed benefited from being able to use cheap yarn

that was machine produced, first from imports and then from import substitution.

Textiles: The Industrial Forerunner

Textiles that were the avant garde of British and then Continental European industrial and technological “revolution” that transformed the world economy early in the nineteenth century. NOT SO.

“The estimates by Crafts and by Jackson show a turning point in the growth of industrial output in the decades 1780–1801, in both absolute and per capita terms” (Inikori, 2002: 61). Yet, in 1801 the structure of value added by the principal industries was 16–17 percent each for building, leather, and cotton, but still 19 percent for wool (*ibid.*).

During most of the period to mid-century, handlooms still outnumbered power looms, and most of these were still driven by water rather than by steam. Moreover, manufacture of much textile machinery was just that, it was still made by hand. Machines were not yet built by other machines until the end of this period and the beginning of the next. The real key to industrialization, that is machine tooling, was still a long way away. Indeed, although industrialization began with textiles, they were rather unsuited to be a locomotive for other industry in Britain or elsewhere. To begin with, producing textiles by machines, especially handmade ones, was very easily and rapidly replicated elsewhere, unless political economic obstacles were put in the way as in India, Egypt, and Mexico in the 1830s and 1840s. Moreover, the textile industry had only few and weak forward and backward linkages, other than to raw cotton itself. In Britain that cotton had all to be imported, primarily from the southern United States; so the raw cotton input had to be paid for and generated relatively little value addition. By 1851, still only 11 percent of the occupied population in Britain was in the textile industry, and none of downstream clothing was as yet made in factories. Only after 1830 did woolen textiles begin to come out of factories. Agriculture, with 21 percent of the British workforce, still remained by far the largest employer (Ashworth, 1962: 8–9). What bearing this had on the formation of entropy in Britain remains to be seen.

Only spinning was much more efficiently done by machines. Weaving was not, and paradoxically cheap industrial yarn from Britain or from domestic factories encouraged the survival and even growth of domestic weaving elsewhere. Competitive industrial chemical dyes had to await the 1870s and were pioneered in Germany, not Britain. Exports of British textiles, therefore, encountered competition and resistance abroad, when

domestic markets were still or again supplied by domestic, and then by other exporting, producers.

The British Textile Technological Revolution Was Generated at Home

Textiles led the technological revolution. The industry developed by and for the home market, largely from prior domestic technological advances, especially in agriculture. NOT SO.

To begin with, and contrary to the entire history invented by Marx and his multiple followers that capitalism emerged through a transition from feudalism in landed property at home, the British textile revolution was a response to competition on the world market, in particular from India. Moreover as Inikori (2002) ingenuously points out, machine-made textiles were made in poor counties in the north of England that provided their own homegrown labor force to produce them, but NO market to buy them. By contrast, the richer southern English counties in which productivity had risen in agriculture, and, therefore, also witnessed an increase in income, witnessed no textile revolution whatsoever. On the contrary, Inikori (2002: 478) summarizes and I embellish: “the key issues to deal with in relating international trade to the development process in England:” (1) its influence on competing interest groups and through them on the state; (2) the influence on productive utilization of resources in England and the entire Atlantic region that supported it; (3) the role of imported manufactures, many paid for by the profits from African labor and the rest with Latin American silver; and (4) of their substantial reexport, especially to Africans in Africa and the Americas; (5) the provision of raw materials to manufacturers, especially of course of all African slave-produced cotton for England’s premier textile industry, the decline of whose input price itself accounted for one half of the price decline of textile outputs, which is what made them competitive on the world market—to the extent that they were, which was much less than often supposed, especially in Asia; but also of African slave-produced sugar, tobacco, rice, and other products that fed the English population; (6) the role of entrepot trade, for instance, of cowries shipped from the Indian Ocean to England and Amsterdam and then used as payment for slaves in Africa, and the use of Holland as a conveniently located entrepot for English trade with Africa and elsewhere; and (7) in the development of shipping, obviously to carry slaves but also for the “triangular trade.” Actually there were many interlocked triangles, and the Navy to protect them, and finance for all of the aforementioned and more including banking, stock exchange, discount market, marine insurance, and so on but also the use of African gold to support them; and (8) the role of expanding overseas markets for British industry, not only of cotton textiles but also still of woolens and linens and then of metals

manufacture. Inikori himself is intent to demonstrate how essential for each of these were the African slave trade, the African slave plantations in the Americas, and the African sources of slaves and market for British and reexported manufactures to the African Caribbean as well as to Africa itself. But Inikori's "Conclusion" is inescapable: "the industrialization process in England was trade driven... [and] the Atlantic commerce was central to the successful completion of England's industrialization" (Inikori, 2002: 482). "Much of the initial productivity gains in manufacturing were achieved through changes in organization associated with expanding markets. Further productivity gains came as a result of a technological change, which, again, was largely due to expanding markets and increasing scale of production, in the first instance" (Inikori, 2002: 55).

The Industrial Revolution and Industrialization

Rapid industrialization, first in Britain and then elsewhere in Europe and the United States, is what set off The Great Divergence and the Gap in Income between the West and the Rest. NOT SO.

In examining the British alleged hegemony in the nineteenth century, Crouzet (2003) reflects the recent orthodoxy when he writes that "The Second Hundred Years War" helped make Britain the 'First Industrial Nation' and that "Britain's fortuitous economic position flowed from precocious industrialization." Yet further on, the same author observes that for contemporaries, a popular slogan was "ships, colonies, and commerce" (Crouzet, 2003: 83, 28, 68). Note that manufacturing and industry are not among the slogans. Nor were they in reality.

Revisionist history casts ever more doubt on the very existence and concept of an industrial "revolution." But it is noteworthy also that contemporaries, including the great economists, did not see themselves as living in or through one. Under the title "The Industrial Revolution as Fiction," Rondo Cameron (2003: 181) also notes that the "British people between 1750 and 1850 did not know that they were involved in an industrial revolution.... One can read the novels and poetry of Jane Austen, William Blake, the Bronte sisters, Robert Burns, Lord Byron, John Keats, Sir Walter Scott, Percy Shelley and others and discover no hint of industrial upheaval" (Cameron, 2003: 169, 181). Indeed by 1815, it was France who was at the technological frontier, and its diversified industry produced 20 percent of national income (Amsden, 2001: 32). Early industrialization in Britain was concentrated in textiles and only textiles before the use of steam power. But even then, the production of textiles still did not rely on steam. Agriculture declined as a source of employment and output, as it was replaced by agricultural imports from

overseas. But this decline in the share of agriculture was not made up so much by growth of manufacturing as it was by services. For the share of manufacturing (but including coal mining) total employment reached about one-third by 1841, and then remained stable for the remainder of the century. The share of manufacture, mining, and construction in national income put together only rose to 40 percent by 1911. In the meantime, the share of services in employment rose from 26 to 33 percent (Cain and Hopkins, 2002: 112). The most dramatic manufacturing increase was in ship-building, and the most important increase in services was in shipping—and banking—in which Britain did come to dominate the world economy during the last third of the century and up to the First World War.

Indeed Pat Hudson (1992) reviews as per a subtitle the “Writing and Rewriting of History” and finds that

[t]he last decade or so [even already now more than a decade ago] has produced new estimates of occupational structure, living standards, industrial output, GDP growth and productivity change. Together these indicate a more minimal picture of economic change in the industrial revolution than has ever emerged before. Productivity and industrial output were slow to grow, fixed capital proportions, saving and investment changed only gradually, workers’ living standards and personal consumption remained largely unaffected before 1830 and were certainly not squeezed. The macroeconomic indicators of economic transformation were not present, apart from the marked shift of the working population out of agrarian occupations. The existence of any sort of industrial revolution is therefore placed in doubt.

The growth rate of productivity of all input factors [total factor productivity] grew no more than 0.2 percent per year before 1800 and still only 0.4 percent to 1831. Even the elevation of these estimates by Crafts by giving more weight to agriculture [not industry!] let the figures the same for the first period and elevated them to only 0.7 percent for the second one and finally 1 percent per annum from 1830 to 1860. (Hudson, 1992: 37–39)

Thus, “even in Europe and the United States, rapid industrialization occurred only after 1870, more than a century after the new technology had emerged in Britain. What accounts for the slow spread of industrialization?” (Hudson, 1992: 122). As late as 1913, with the United States as 100, manufacturing output per capita was Britain 90; its Dominions about 75; Northwest Europe 40–73; Central Europe 20–30; Russia, Eastern Europe and Balkans 4–13; Southern Europe 15–20; Argentina

and Chile 20; Japan 6; and India (averaged and rounded from Hudson, 1992: 129, based on Lewis, 1978). The index for India does not sound credible, considering the revival of textile production, since the 1870s, then by machines; and China is not listed. More on that in Chapters 6, 7, and 8.

The United States prospered and until 1860 were the leading industry with Ely Whitney's cotton gin, invented in 1793, and behind tariff protection, in an expanding domestic market—but for 200 years was rarely competitive on the world market. Yet “stunning is the fact” that without protection, China, India, and to a lesser extent the Ottomans were competitive at world market prices (Hudson, 1992: 49), until the Japanese knocked India out of, and took over 40 percent of, the Chinese market (Hudson, 1992: 49).

British Exports Conquered the World Market

British exports, particularly of textiles, quickly conquered the world market and battered down the Great Wall of China, as Marx put it. NOT SO.

It is true that in quantitative terms, all British exports did increase quite fast, by nearly eight times in the period from 1816 to 1845, and during the 1830s alone by more than half (Imlah, 1958: 94–95). In the 1830s also British exports, of which textiles still accounted for the largest share, were worth 94 million pounds sterling in total. Of these, however, 36 million went to Europe, 15 million to the United States, 13 million to the West Indies, 10 million to Latin America, and only 13 million to all of Asia (Imlah, 1958: 129).

According to the now well-accepted reestimates of British exports by N. F. R. Crafts (1986), 36 percent of all manufactures and 47 percent of cotton went to Europe in 1834–36 and another 35 percent of all and 20 percent of cotton exports went to North America and the Caribbean slave colonies. Even in 1854–56, 29 percent of all and 29 percent of exports of cottons were to Europe and 28 percent of all and 16 percent of cotton exports crossed the Atlantic to the aforementioned regions. Thus in 1834–36, only 29 percent and in 1854–56 33 percent of all exports; but then already of all exports, 43 percent in the 1830s and 54 percent of cotton were destined to Australia, Africa, Latin America, West Asia and Egypt, and South and East Asia. And over all this time, British exports from its iron and steel industry, most of which went across the Atlantic and to Africa, rose only from 10 to 12 percent of the totals. Almost two-thirds and always more than one-half of British exports were cottons and woolens (Crafts, 1986: 143–145). But Crafts's bunching of these data is misleading. For all of Asia, from even the captive market in India to

the East, still only took a small share of British cotton textile and almost no other exports, and per capita only the tiniest of fractions. On the other hand, echoing Eric Williams's (1943) *CAPITALISM AND SLAVERY*, Pat Hudson (1992) observes that it was "the role of the Atlantic economy in which slavery and the West Indies were for a time the crucial lynchpins ... [and] the slave trade contributed to the external demand for British manufactured goods in West Africa, the Americas and Europe. Finally it is important to examine the contribution this trade made to build-up an efficient commercial, financial and credit infrastructure in Britain" (Hudson, 1992: 196).

So, early on the large bulk of British textile exports were destined to Europe and white immigrant North America, until these regions supplied their own markets through import substitution. In 1840 still, 30 percent of British textile exports went to the major expanding markets of Europe and North America. But that share declined to only 20 percent in 1860, and 10 percent in 1880, as these countries were increasingly producing textiles for their own markets and some also for export (Hobsbawm, 1968: 146–147). In the meantime, the world's largest countries and markets, China and India, were still also the most self-sufficient ones. In the noncaptive market of China, British exports of cotton goods were only 6 percent of its total in 1850 and 8 percent in 1875, for still in 1894 China itself supplied 85 percent of its domestic market of textiles, though it had been importing some cheap mechanized yarn (Arrighi, 1994: 60). For India, Marks (2001: 130, citing Ambriajan) records 20–35 percent of British exports, which seems excessive even for a colonial captive market. Even more so, Hobsbawm (1968: 146, 147) records 30 percent of British textiles in 1850 and 60 percent in 1873 exported to Britain's "captive audience" colony in India. So much for Britain as "the workshop of the world," not to mention Marx's remark about "battering down the Great Wall of China" in the world's most populated East and even colonial South Asia, which had long been and still continued to be its own centers of manufacture! Moreover, British reexports continued to make a substantial and even a rising real contribution to British "exports" and foreign earnings. While in the 1830s and 1840s annual values of reexports hovered around 10 million pounds, they rose to 20 million in 1854, and then reached a maximum of 60 million pounds in 1865, after which they leveled off around 50 million pounds. As a percentage of all exports, in this period reexports hovered around 15 percent, rising to 20 percent in 1865, and then leveling off again to about 17 percent (Imlah, 1958: 33–35). What kind of "workshop of the world" that "ruled the waves" was that in the heyday of "Pax Britannia"?

Coal

Coal was the early and critical power input for Britain. NOT SO.

The principal constraint of land, emphasized by Pomeranz (2001), is amended by Goldstone (2001: 363ff.) and others as energy from land or elsewhere. There is indeed consensus that it was the switch from biological above ground sources of energy from plants, animals, and humans to the below ground mineral ones of first coal and then petroleum that made all the difference. The switchover came earliest where after 1730 the opportunity cost of coal began to decline below that of wood and its charcoal derivative in Britain, where coal deposits happened to be favorably located, but not yet elsewhere in the world. In 1700, Qing China still used twelve times more fuel energy per capita than Britain. Thanks to the switch to coal, by 1850 the proportions were more than reversed in that 18 million people in England used as much energy as 300 million did in China. By the end of the nineteenth century, even after others had also switched to coal, Britain's percent of world population consumed one-quarter of all mined coal. The principal use of coal was to generate steam, first primarily to pump out water to dig ever more and deeper for coal, and then for other purposes. But that would require a further important delay, especially in technological development, and spread; as we will observe immediately later. An apparently serious challenge to Pomeranz is posed by Peer Vries (2001) under the title "Are Coal and Colonies Really Crucial?" Of course he troubles to ask in order time and again to say NO. I find his rebuttal unsatisfactory on many grounds, the principal one being continual reliance on conventional idea that "the Industrial Revolution in Britain in essence was an increase in productivity much more than a windfall in cheap resources" (Vries, 2001: 435–436).

This still-predominant argument is challenged in the examination of still other myths in this chapter and through further examination of the historical evidence in the chapters that follow. Indeed, this dependence on the export of coal in the supposedly industrial leader was cause for some concern at the time that Britain's share of world manufacturing export was declining and especially it was failing to move into exports of technological leadership, into which it was Germany that was moving instead (Cains and Hopkins, 2002: 154).

Steam

The driving force of the new industries was steam from early on. NOT SO.

Steam power made its way rather slowly in and through manufacturing. Steam began to replace water power in spinning after the 1830s, but

not yet in weaving and knitting whose mechanization was more difficult. By 1835, about 1,000 mills with 30,000 horse power (h.p.) were derived from steam, while 10,000 h.p. were still derived from water. But at the same time in 1833, of the 350,000 looms in Britain, 100,000 were power looms and 250,000 were still handlooms. Moreover, these figures are relative, since many looms were only power ASSISTED (Chapman, 1972: 19, 26)]. Even in the newly mechanized cotton regions, weaving remained predominantly by handloom, with 69 percent, or 25 percent, of the labor force, in Lancashire and 20 percent of handlooms in Scotland (Inikori, 2002: 77). Lancashire alone accounted for 56 percent of all power looms in 1835, and 71 percent by 1850 (Inikori, 2002: 450). The rest of Britain languished. That may be because “until the 1830s, coal and steam offered little in the way of cost advantages over windmills, waterwheels, horses, or the hands of women and children to carry out mill and factory work” (Clark, 1997: 90). The efficiency of steam engines remained very low, at 1–2 percent, which Watt raised to 5 percent only after some time, after which it took the whole nineteenth century to raise it to 20 percent, compared to the present 50 percent (Bairoch, 1997: 1–324).

However, steam power and the coal energy used to produce it were the really revolutionary innovations of the industrial revolution, if any. It was not so much a matter that after Watt, thanks to their advantages, they immediately spread through the economy; for they did not. Indeed, for many decades still, their relation was symbiotic: Coal generated steam, and steam engines were used primarily to pump water out of ever deeper shafts to get out more coal. Nor did they quickly spread to and revolutionize factory production, which as per the aforementioned in the principal industry, textiles, they did not. Still in 1840 among the industrializing countries, water power accounted for three-quarters of the energy supply, and even in 1870 it was still more than half (Bairoch, 1997: 1–321). It was most importantly their placement on and powering of moving platforms, first railroads and then ships, that revolutionized transport. That had backward and forward linkages to all sorts of sectors, including especially the movement of agricultural commodities, national, international, and intercontinental and of coal itself. Moreover, steam engines and their manufacture themselves had linkages to other, for example, machining industries; but this development had to await the second half of the nineteenth century.

Railroads

New means of especially steam-powered railroads quickly transformed the British and the North Atlantic economies. NOT SO.

In that land of their birth in the early mid-1830s, the yearly number of railway miles opened was only 40–50, rising to an average of about 200 miles in the late 1830s and early 1840s, before rising to 800 miles in the late 1840s and 1851, after which it declined again to little more than the level of the 1830s (Rostow, 1978: 143). In the United States there was a short burst of short distance railroad construction in the East before the Civil War, but the real epoch of railroad construction in North America had to wait for the 1870s and 1880s. Not until 1869 was the first Western transcontinental route completed. However, in the East, railroads were also complemented by new canals.

Steamships

Steamships rapidly conquered the oceans, and with them “*Britannia Ruled the Waves.*” NOT SO.

Although steam power was installed in some ships, shipping long continued to rely on sail; some now complemented by machines. Of total maritime carrying capacity, steam accounted for only about 15 percent in 1840, and by 1870 still less than 50 percent (Hobsbawm, 1975: 58). Indeed the probably more reliable Ashworth (1962: 68) says that in 1870 only 12 percent of shipping tonnage was carried by steamships and by 1880 still only 25 percent. Imlah (1958: 175) has steam powered as 29 percent of net tons in 1880, 47 percent in 1890, 67 percent in 1900, and 83 percent in 1910. Of these, however, British tonnage multiplied sixteenfold from 1850 to 1880 and the rest of the world’s tonnage increased only four- and a half fold. Thereby, British tonnage grew from about a quarter of the world total in 1840–50 to a third in 1870 and to one half in 1880 (Hobsbawm, 1975: 58). But Imlah (1958) records only 32 percent in 1880 rising to 45 percent in 1890 and 1900, and back to 40 percent in 1900 as other countries increased their own shipping. However, the British share of shipping under steam was higher by about 15 percentage points in the first two years and 5 points in the last two. The next largest shipping fleet in the world was American. Nonetheless, transport costs in this period declined very sharply, for example, from 0.5 pence per pound of cotton shipped from New York to Liverpool in 1823–25 to 0.16 pence in 1851–55. Cost reductions of 50 percent for some products were registered later in the 1850s alone (Kemp, 1976: 63). In a sense during this period, it was ships and shipping that were Britain’s most successful industrial export product.

The Myth of Science and Technology

It was Western and particularly British science that permitted and accounted for the industrial and technological revolutions and their impacts in Britain and the world. NOT SO.

There was no “seventeenth century scientific revolution” (Shapin, 1996), and it did not drive the “industrial revolution” two centuries later in the nineteenth century. Detailed research, including work done industry by industry (Adams, 1996), has shown that science had no input into nor made any other even indirect contribution to technological progress before 1870, when it began to do so primarily in and through the chemical industry, which was not even born before then (Rosenberg and Birdzell, 1986; Frank, 1998). And that was pioneered in Germany, not in Britain. The red thread running through Bairoch’s (1997) 3-volume, 1,500-page economic and social history of the world is technology, to which he attributes almost everything else, including science—much more as a derivative from than as a contributor to technology. So does Robert Adams (1996). Indeed, to a significant extent that is still the case as Purkayashita (2002: 33ff.) points out: Science can have only little input into electrical engineering, but even a minor technological advance in instruments of measurement, an electron microscope or telescopes for instance, can open up vast new vistas to scientific research. For a contrary view, see Margaret Jacob (1992) and Goldstone (2001).

The State

The world economy was organized by and through a world market, and imperialism of free trade operated on its own by laissez-faire without the need for significant state intervention. NOT SO.

Arguably, the archetypical nation-state is the Japanese one, and it underwent no fundamental change from the Tokugawa period to the Meiji Restoration. No European state can match the Japanese one in stateliness, much less in being a national one. Other states in Asia, the big Chinese one, smaller Southeast Asian ones, and West Asian ones in between performed state functions as well or better than European ones. The Ottoman imperial state, even as the supposedly sick man of Europe, performed surely as well or better than the imperial Hapsburg, Russian, and British ones with which it was in recurrent conflict. Indeed, even more state-like have been the colonial states that they themselves set up and ran, as well as the neocolonial ones. In them, their comprador bourgeoisie, as the Chinese

called it or the Lumpenbourgeoisie, its Lumpenstate, and the resulting Lumpenddevelopment in Latin America, as I called it (Frank, 1972), do the bidding of their imperial power with which they are in alliance, but as a junior partner. Moreover, nurturing and using colonial and neo- or semi-colonial states was purposeful metropolitan policy, especially in the last quarter of the nineteenth century as it would also be in the last quarter of the twentieth. And the instrument has been the same. It was and again has been the foreign debt, more than or at least as the basis of gunboat diplomacy. For as the British minister in Persia once observed, "the more we get her into debt, the greater will be our hold and our political influence over her government." (Cain and Hopkins, 2002: 355). And, as a British observer reported about its neighbor using the debt as a handle, "the daily surveillance of which Turkey is the object in her domestic affairs has reduced her sovereign authority to practically zero" (cited in Cain and Hopkins, 2002: 355, 359).

The national state is not the result of nations building states, but of states trying to form nations, and that is why they pay history teachers to invent and inculcate "national" histories. All this was the case in the nineteenth century and it still is today. But ironically it is the imperial/national alliance that promotes this nationalism as a fig leaf for its crass self-interested neocolonial policies. When a few states sought to take an independent developmentist line, like Mohamed Ali's Egypt and Dr. Francia's and the Lopez brothers in Paraguay up to mid-nineteenth century, they were crushed and replaced by neocolonial ones. So it is not true that states arose and exist primarily to manage the people within their borders, nor that the state system was born at the Peace of Westphalia in 1648.

Nor is it the case that the world market functioned and economic development proceeded without, or better without, substantial state intervention. Arguably, interstate relations and competition has been more important than their domestic functions, except to the extent that their domestic control serves international ends. The so-called and much touted "imperialism of free trade" operated through imperial, colonial, and neo-colonial states. They have been major actors in the global economy to promote and protect important economic interests at home and abroad both by peaceful means and military ones. The latter, beyond pursuing their ostensible territorial and trade desires, also intervened, and still do importantly in the development of the productive process. On the supply side, they promote infrastructural and technological developments that are spin-offs from military uses, and on the demand side they elicit Keynesian macroeconomic government spending. In so doing, the Napoleonic Wars promoted industrial development and state formation in parts of Europe. But as the examples of Germany and Italy illustrate, it was the 1873–95 'Great Depression' that generated new or renewed demands for states to

compete in the deflationary regional and world market. Sidney Pollard observes that

[t]he changed circumstances of the 1870s led to strong demands everywhere for aid and protection by the state, in the first instance by the well-tried methods of customs tariffs.... However,... whatever else it might be that divided or united men, in this phase of history it was the state that became the most powerful magnet to attract to itself the loyalties and expectations of aid and defense from more and more sectors of society. (Pollard, 1981: 56)

A summary statement about the state in Latin America is that of Kaplan:

The state acts upon the orientation, structure and functioning of economic activity and the social system to permit and assure the successful operation of the model of dependent development.... The primary productive-export sector... is kept under control.... The availability of the productive resources for the agricultural-mining export sector is maintained and expanded. Property is inviolable.... The state takes on the task of extending the internal frontiers... of open spaces;... or inhabited by indigenous tribes.... One of the most important assignments of the state in this stage is precisely to impede the access of indigenous, criollo and immigrant workers to means of production of their own.... The state aids in domestic capital accumulation (for certain purposes) and attracts foreign funds.... The state constructs and operates public works and services... or grants concessions, guarantees, subsidies and other incentives to foreign companies which thereby are assured high profits.... The benefits of this state intervention flow to the landowners, merchants, investors, speculators, middlemen, and the lawyers of the big companies. (Kaplan, 1997: 174, 197–201)

If that is the place and role of the neocolonial dependent state in “independent” Latin America, then it is *a fortiori* so of the colonial state that the metropolis itself runs for that purpose.

But state intervention has also been reexamined in the industrially most developed countries, beginning with Britain itself. Writing under the subtitle “The Economy and the State,” Pat Hudson (1992: 51–52) specifically challenges “the conventional view [of] minimum state involvement and laissez-faire” and writes that instead the British Empire and military power “was made possible by the emergence of a new kind of British state with a fully reorganized fiscal and military apparatus, heavy taxation,

and a professional bureaucracy. The state and its activities came to affect civilians in every walk of life: it became the largest single actor in the economy with state expenditure rising well above the level of domestic capital formation,” to which, however, it also made a major contribution, especially through the military sector itself. Also Patrick O’Brien (2003) is now insisting on their oft-unrecognized importance. For instance, he observes that between 1688 and 1837, 85 percent of public expenditures were military related, contributing to economic development through both the supply side and the demand side. In a volume devoted to *LEADING THE WORLD ECONOMICALLY* (Crouzet and Clesse, 2003), O’Brien stresses the overlap between the British merchant marine and the Royal Navy and writes:

The cost of servicing national debt, which was mostly military incurred and was 2 ½ times national income by 1821, was much of the budget, and direct military expenditures another 28 percent of budget, after that. . . . Between 1870 and 1914 military expenditures rose over 3 times at falling prices and reached 5 to 7 percent of GNP during the Boer War years 1900–1903. (O’Brien, 2003: 133, 139, 159)

Britain’s followers elsewhere in Europe and in North America, not to mention Japan, had to rely even more on their states to promote industrialization through public expenditures and massive subsidies, other fiscal and monetary policies, as well as military and tariff protection. Indeed, the later they were to industrialize, the more they relied on the state to promote the same.

The Myth of Overwhelming Military Power

If, and insofar as, not British and European industry was the battering ram that forced open the walls and doors of the rest of the world, it was Western availability and exercise of vastly superior military power over the East and South. NOT SO.

“[Patrick] O’Brien tells us...that military power, security, foreign trade and empire did matter, and that Britain did receive real and significant returns on its heavy investment in war” and naval power (Crouzet and Clesse, 2003: 69). Yes BUT: As correctly observed by Gallagher and Robinson (1953: 150), “British [and we may add also other European] policy followed the principle of extending control informally if possible and formally if necessary.” Britain fought fifty colonial wars in the nineteenth century and seventy-two military campaigns in Asia and Africa

after 1837 when India had already been conquered. Then Britain intervened militarily in China in 1839, 1856, and 1859; in Persia in 1856, and Ethiopia and Singapore in 1867. But intervention became more frequent only in the 1880s and 1890s: In Egypt in 1882; Nyasa in 1893; Burma in 1885; Mombasa and Uganda in 1896; Sudan in 1896–97; and South Africa at end-century in the Boer War (Arrighi and Silver, 1999: 224–225). Nevertheless, political excursion and intervention by European powers outside Europe never involved the use of major military force. That was reserved for use against each other in the Crimean War and between the North and the South in the American Civil War. Nor was military power used especially to promote European industrial exports and trade, whose amount to Asia, Africa, and Latin America was never all that much.

Instead, Europeans used relatively small shows of military force, and they did not even have available outside of their European “theater of operations.” Moreover, in the nineteenth century the British forces were manned largely by Indians, 130,000 Indians and 66,000 British troops in 1880, and they still maintained a Gurkha regiment through the twentieth century. Britain obliged the Indians themselves to pay for the troops and their own occupation and “Home Charges.” As Lord Salisbury put it, “India was an English barrack in the Oriental Seas from which we may draw any number of troops without paying for them” (Arrighi and Silver, 1999: 225). The French built up their famous Foreign Legion. So mostly Europeans brought to bear relatively small military force in very selective military/political intervention at the nodes of nerve and circulatory points around the world where and when it was threatened at crucial times and places, both by the “natives” and by rival other European powers. To capture or defend these nodal points a military pinprick and band-aid was often sufficient, and a scalpel; not to say a major military operation was quite unnecessary, and in any case would have been impossible. These crucial nodes were precisely those that were essential to create or maintain this and that nexus of the world economic system of trade and payment imbalances. For that was what supported both the domestic welfare and the further foreign investment of Britain and a few other European powers.

Only in the last quarter of the century was there more frequent and greater military/political intervention and that was a result of interimperialist European rivalry that sought to secure footholds here and there before rival European powers could do so. That rivalry in turn was largely in response to the long economic crisis from 1873 to 1895 in which demand declined and tariff barriers again rose in the North. Prime Minister Lord Palmerston did say that “these half civilized governments, such as those of China, Portugal, Spain, America require a dressing every eight or ten years to keep them in order” (Arrighi, 1994: 36 from Lowe, 1981: 34), but that was militarily more bluster than feasibility; though it may have been

an accurate reflection of would-that-I-could ideology of Britain then, and the U.S. Bush administration now. To that end, and in competition also with European rivals, the British navy was the principal and worldwide predominant instrument of what in later reference to the United States was called “gunboat diplomacy” and that it now also exercises through airpower. So it may be true that as the saying went that in much of the nineteenth century “Britannia rules the waves,” both militarily and commercially as we will see later.

On the other hand, military Keynesianism and direct state support for industries that were important for British development did play a major role that has often been neglected. It is, however, stressed by Patrick O’Brien in the following and other observations:

Between 1688 and 1837, 85 percent of public expenditures were military related, and there was much overlap between British merchant marine and Royal Navy. The cost of servicing the national debt occupied was much of the budget. But the debt itself was mostly incurred for military expenditures and reached 2½ times national income by 1821; and direct military expenditures took up another 28 percent of budget after that. Again between 1870 and 1914, military expenditures rose over 3 times at falling prices and reached 5 to 7 percent of GNP during the Boer War years 1900–1903. (O’Brien, 2003: 16, 133, 139, 154)

Management of the national debt in turn made a major contribution to the organization and operation of British financial capitalism worldwide (Arrighi, 1994).

The Myth of British Hegemony

Britain followed the Netherlands and preceded the United States as A or THE hegemonic power in the world during the nineteenth century. NOT SO.

Simple inspection of the world at large, and Asia in particular, demonstrates the impossibility of the very small Netherlands to exercise any hegemony in the world. And it did NOT. But was the still small but economically, politically, and militarily more powerful Britain able to exercise pervasive hegemonic power around the world? Again, inspection of reality must result in a negative answer. As this chapter demonstrates, certainly no one, and also not Britain, was hegemonic during the first half of the nineteenth century. The period 1850–73 is often said to be the time of maximum British domination. Yet it certainly did not dominate North America, where in the 1860s Canada became independent

and in the United States Britain's southern allies lost the civil war and the northern winners regularly imposed protectionist policies. Also in Latin America, British influence was still being combated by national interests in almost all the states, and/or Britain was challenged by French occupation of Mexico and by American Monroe doctrine expansion. Africans still exercised independent and successful trading positions and the exercise of British political economic power was limited to small parts of Southern Africa and Egypt. In the Ottoman Empire, it is the case that Britain eliminated the challenge of Mohammed Ali in Egypt and forced unequal commercial treaties on the Turks; yet their domestic *Tanzimat*, a sort of analogue to the Chinese self-strengthening, was still quite successful. In Russia and Central Asia the British had to fight their biggest foreign war of the century to defend their interests in the Crimea, which was really fought over access to Turkey and India. In India itself, the British conquest was finally completed, but the Sepoy Mutiny was a serious challenge to British interests and power. Arrighi and Silver (1999) take a glass half-full position in emphasizing that the mutiny as well as the simultaneous Taiping Rebellion in China were put down.

In Southeast Asia, British expansion was only just beginning and in competition with the Dutch in Indonesia and the French in Vietnam and Cambodia. China and Japan certainly remained independent in all respects, except for the loss of Hong Kong to Britain and a series of unequal treaties that gave Britain, but also other European powers and later Japan, some special privileges in a series of Chinese ports, but certainly not inland or on domestic Chinese or Japanese policy.

Moreover, according to the protagonists of the hegemony thesis themselves, British hegemony began its decline again already in the next period from 1873 to 1913. The 1873 crisis marked the beginning and the 1873–96 “depression” its spread, during which even France became a political player again in Africa, and the United States and Germany began their economic challenge to British hegemony. For the latter, it was the time of the famous Berlin to Baghdad railway. Therefore, we must agree with Patrick O’Brien (2001) when he writes of “the myth of British hegemony” and “the myth of two interconnected and evolving hegemonies propagated by historians and social scientists” about Britain and the United States (O’Brien and Cleese, 2001: 49, 55).

The Myth of American Exceptionalism

American economic development in the nineteenth century, and its move from near the margin to near the center of the world economy, was indeed

a marvel to witness, and had its roots primarily in the “distinctive genius” of “the American Way” to its “Manifest Destiny.” NOT SO.

Since much of the emphasis about American economic development relates to its “from-to” path within the world economy, it seems paradoxical that much examination thereof, especially by American historians of America, pay scant attention to the global whole and still much less to the global STRUCTURE WITHIN which the United States moved up and from here to there. Therefore, there has also been virtually no acknowledgment of that and how the resulting beneficial improvements were derived from the position and role of the expanding American economy in the structure of the also expanding world system of trade and payment imbalances. Yet it was the latter that gave a largely unmentioned and even less acknowledged but very important boost to American domestic economic growth, income, and other developments. Immigration, the construction of railroads, and other infrastructure by and for the existing population, new migrants, and their offspring, and the production of export crops of cotton and wheat, later also of meat, and import substitution to supply the domestic market all expanded.

The natural and migratory increase in population and that by the latter’s offspring generated increased production of and demand for consumer goods and investment, particularly of economic infrastructure and housing. There was ever-increasing construction of railroad track and its use to carry freight and passengers. The U.S. mileage of existing railroad track rose from 3,000 miles in 1840 to 9,000 in 1850, 30,000 in 1860. Then construction slowed down during the 1861–65 Civil War and resumed even more so thereafter, to reach 53,000 miles in 1870 and 93,000 by 1880 (Chandler, 1965: 13). In the meantime, the size and carrying capacity of freight cars increased and their number doubled (Woytinsky, 1955: 266). The extension of the network and of its carrying capacity opened up and accelerated access to and among vast areas of the continent that now witnessed new settlement and production particularly of staples for domestic consumption and for export. Equally significant but almost completely neglected was the changing place and role of the American economy within the world economic system of multilateral production, trade and payments, as we will observe later.

The Myth of the Changing Nature of Labor

Industrialization also means free factory labor and proletarianization. NOT SO.

Another Marxist thesis that has passed into general acceptance is that industrialization was accompanied, indeed accomplished, by a free labor force that works for wages, mostly in factories. Peasants and craftsmen

are proletarianized by being divested or divorced from ownership of the means of production that pass into the hands of capital instead. Indeed for much Marxist theory but also in general public opinion that is the *sine qua non* essence of capitalism.

The historical record belies this theory and belief for the nineteenth century of industrialization and before and after that as well. To begin with, wage work already had a history of several millennia in many parts of the world before it was introduced in nineteenth-century Europe. On the other hand, most work, especially by women, is still not waged even today. So if the expansion and predominance of wage work is the criterion of the spread of capitalism, then there is still relatively little capitalism around. Moreover, the industrial revolution used, indeed produced, far more slave, serf, indentured, and bonded than free labor especially but not only in agriculture; not to mention that the women and children who labored around the clock in factories were only “free” in a very peculiar sense of that word. Yet it was slavery that came to be called “peculiar” at the very time that it was as we will observe later still expanding by leaps and bounds in the plantations and mines that produced the material inputs required for industrialization. Finally, but not really final, there are still an estimated 20 million slaves working today especially in India; but they can be found working as well in Los Angeles and New York City.

The Myth of the Dominance of Western Migration

The importance of intercontinental migration can be seen from its vast numbers who left Europe and from the reasons that pushed emigrants out and pulled immigrants in to where they contributed to development. NOT SO.

International migration was only one of the subtopics in the *magnum opus* ON GROWTH AND FLUCTUATION 1870–1913 by Arthur Lewis (1978), which is long since out of print and seems largely forgotten. That amnesia (or simple ignorance?) seems to apply also to what he had to say about migration, which interested him because of its relation to the topic of his title. For Lewis already then wrote that “the movement of Europeans into the countries of temperate settlement [which he counted as 36 million between 1871 and 1915, of which two-thirds to the United States] was outstripped by the movement of Chinese and Indians into the tropical countries.” By 1880, he found 3 million Chinese living abroad, and 8 million by 1922. Eighty percent of these had gone to Southeast Asia, first mostly to Thailand and then to the present Indonesia, but 15 percent to other South Asia, and of course a still significant number of mostly indentured laborers to Oceania and Latin America and the Caribbean. He does

not include North America. And as we will note later, he does not see or count Chinese immigration northward into Manchuria and the Russian Far East. However, Lewis places even more emphasis on international migration out of India, of nearly 16 million, of whom, however, nearly 12 million returned, but still leaving a net out-migration of 4 million between 1871 and 1915. They went primarily to neighboring countries in South Asia, but also to islands in the Pacific and Indian Ocean, as well as to Africa. A relatively few emigrants left Japan for Hawaii, Brazil, and South Asia. The large bulk of these migrants went into work on often European-owned plantations and mining to produce exports for the industrializing countries (Lewis, 1978: 185–186).

So it comes as something of a surprise that Rudolph Vecoli should now have to introduce his edited volume, *A CENTURY OF EUROPEAN MIGRATIONS, 1830–1930*, with the statement, “We need to move beyond the framework of the ‘Atlantic Migration.’ . . . It [has] blinkered us to the global nature of [migration]” (quoted in McKeon, 2003). Quite so, because despite his own failure to do as he says, nineteenth-century migration was indeed global; and North and Southeast Asian destinations were as important, albeit also as neglected, as American ones that have received almost exclusive attention. Out-migration from China was roughly as great as that from Europe.

Since Lewis wrote, further research has revealed that even more Chinese, perhaps tens of millions from South China, went to Southeast Asia and beyond into the Pacific islands and some to the Americas. And other tens of millions of Chinese moved into Manchuria and some beyond that into Siberia and the Russian Far East. There they were joined by migrants from Japan and Korea moving west and surely undercounted Russians moving eastward from west of the Urals. Moreover, “internal” but long-distance migration also characterized China, India, all around the Ottoman Empire, crisscross Africa, and Europe eastward, at least as much as westward in North America. And of course everywhere, rural populations (if they did not move overseas or at least abroad) moved into ever-growing cities. As McKeown (2003) puts it, “Concurrent growth around the world was not coincidental, but linked through an increasingly integrated global economy. It was a world on the move, flowing into factories, construction projects, mines, plantations, agricultural frontiers, and commercial networks across the globe.” Moreover, the flows of migration everywhere, as Lewis already observed, were in tandem with the business cycle at either the sending or receiving end, or both. Indeed, he writes that the still widely used “push and pull are unfortunate terms. There was no pull where there was no push . . . [and] where there was push there was no movement except at times of pull . . . [But for European migration] the dominant factor was pull” (Lewis, 1978: 183).

While the importance accorded to massive migration during the nineteenth century is true, it by no means reflects the total importance of migration. There are at least two other major considerations: One is the expansion and multiplication, "fan-like" as Hildgerdt put it, of the nodal nexuses in and of the world economic system of multilateral imbalances of trade and payments. Migration did not only add segments to the multilateral fan by the expansion of United States and accession of European-settled Argentina, Canada, Australia, New Zealand, and South Africa, not to mention Siberia, North- and South- East Asia. Their new participation also made *The Network of World Trade* more complex and indeed changed its structure and modified its operation for the transfer of income and entropy from each region to all the others. That cemented the hold of the old high-status locations and their occupants in the system of multilateral trade and payments and it added additional at least medium high-status locations, while the majority of the world's population remained relegated to and even more glued to their "low" status locations.

The other additional consideration is the role that migration had in the dissipation of entropy from densely to sparsely settled regions. Europe had regions with high population and many workers relative to their resources, some of which were being increasingly exhausted. So did China. The ability to ship many of these people overseas or even over land permitted a lower rate than otherwise of resource depletion and physical entropy, as well of social entropy from greater political strife, at home. In the receiving or even newly peopled countries, the additional source of labor power converted natural resources into production and income, both for the population in the "new" countries overseas and for those in the old countries. For the latter were now able to import more and at lower prices the products such as wheat and cattle that use much land, which the old countries did not but the new ones did have. So the dissipation of entropy from Europe also took this additional form. In the new countries, the additional use of land and other resources may have been slower in generating physical entropy, some of which has since then also been dissipated abroad, again to the "third world." Thus, Europe and the settlers and already previous immigrants and their offspring in the regions of "new" settlement benefited from this migration and the export/import of cheap goods for their remaining still-growing population. But in the "new" countries, the old native/indigenous/aboriginal populations were forced to absorb untold amounts of physical as well as social entropy and disorder. In the case of the United States furthermore, Mexico was despoiled of half its land and many of its people at the cost also of untold entropy.

Out of all this arises a very important question that has hardly ever been researched and analyzed, or even posed: How did the massive arrival of

migrants affect their recipient regions similarly or differently, and why? Why did this in-migration appear to have had more beneficial effects in and to North America than to South America, and Southeast Asia and Northeast Asia, including Siberia, or did it?

The Myth of Free Trade

The nineteenth century was the golden era of free trade. NOT SO.

The POLICY of free trade was not even born until Britain's abolition of the Corn Laws in 1846. After that, it was practiced at most here and there, and it hardly survived twenty-six years until the Great Depression of 1873–95 buried most of the praxis and most of the policy as well. However, free trade did not go very far even in its heyday. Most significantly, Britain never allowed any free trade between itself and its most important colony, India, nor between the latter and other regions. China did not practice free trade, nor did Japan, even when Western states obliged them to lower tariffs. There was no free trade between Holland and its Indonesian colony nor between it and any place else. However, to the extent that trade was freer into some countries than others, it was precisely the three most important colonies and semi-colonies of India, China, and Indonesia that had the lowest tariffs, while between 1870 and 1914 their colonial masters, Britain and the Netherlands had 3 and 5 percent tariffs and Germany, France, and Japan had tariffs at 12–14 percent and the United States at 33 percent (Nayyar, 2002). Perhaps the most noteworthy therein, and counter to the received wisdom or at least theory, is the inverse correlation between these countries' tariff and their growth rates during this period!

In the United States, Alexander Hamilton had already promoted high tariffs as part of his "Manufacturing System," and they were then debated over between Clay and Calhoun in the 1830s–40s, before the Civil War, which was fought in significant part over whether to maintain them as the industrial North wanted or to reduce them as the cotton-exporting South wanted. After 1873, the United States increased its tariffs again as well. It was the Zollverein (tariff association) sponsored by the Hamiltonian Friedrich List that brought Germany into existence in the first place. It would be tedious to continue listing examples. It would be more useful to inquire when and where what world trade was "free."

The Myth of Laissez Faire

Laissez faire, or the best government is the least government, proved itself superior and gave rise to the West. Private entrepreneurship without state

interference was responsible for most of the progress through the nineteenth century. NOT SO.

The rebuttal of the fable about free trade also gives the lie to that of *laissez faire*, since it was state action that determined how unfree that trade was. It seems curious that French terminology invaded English and other languages in that respect, since particularly the highly centralized and colonial French state did and still does not practice what the saying preaches. However, the “interference” of states and their promotion and finance of all kinds of economic projects was legion everywhere. *Laissez faire* was supposed to mean less “no state intervention,” than no favoritism “in” the intervention that there was. But even that was more theory than praxis (Weaver, 1974: 72). State action was ever active of course when and where European states created and managed colonies, particularly after 1870 in Africa, but already long before that all around the world. But that characterized not only the colonizing but also the colonized or colonial state, whose *raison d'être* (to stick with French) was precisely the exercise of state power in the colonies or neocolonies in the interests of the colonizing state and its capital, often in competition with those of others. That was also the place and task of the newly *de jure* “independent” but *de facto* neocolonial states of Latin America after 1820 and it still is also everywhere else today where international institutions such as the IMF, WB, WTO, and, with or without them, their U.S. masters hold sway and court. Then like now, recourse to state power was an instrument and mark of defense against global political economic forces. William Ashworth looks back:

It is clear that what the governments of the more advanced countries actually did in the economic sphere, that although they may have been moving towards *laissez-faire* in the two middle quarters of the nineteenth century, they achieved just that condition only partially; so partially, in fact, that in much of Europe the long tradition of government regulation cannot be regarded as ever having been really broken, though it was challenged and modified. Even where there was more far-reaching change, as in Britain and the U.S.A., *laissez faire* proved to be an elusive ideal, for in order to extend economic liberty in one way, it was necessary to introduce some new limitations, while the very conditions which seemed to increase the advantages of unrestricted private enterprise also had undesirable consequences, for which it gradually became apparent that only a collective remedy was possible. Into this condition of very incomplete revulsion from thorough government regulation there came, about 1870, fundamental new influences which led to the reversal of the previous trend towards uncontrolled private enterprise. Most

of the important influences were international in scope. (Ashworth, 1962: 137–138)

Throughout the nineteenth century, state military ventures, both bellicose and still pacific, always sustained economic production as they would also continue to do so during the twentieth century. Many states promoted and financed the extension of the agricultural frontier into the interior, the development of industry, and of course the infrastructural investment for all of these, most obviously in transport and communications. Among the former was especially the construction of railroads and canals; and among the latter the telegraph and postage, but perhaps most importantly public education. National state, provincial, and local governments set up or changed an unending stream and ubiquitous web of regulations in the service of any number of interest groups, the least and last of which were the people at whose service they were supposed to be.

The Myth of the Gold Standard

Gold reigned, and the Gold Standard was an important weapon in British economic rule of the world. NOT SO.

Apart from the exaggeration of the extent of British economic rule in the world, the evidence shows that during the period 1813–48/50, the *de facto* silver standard that had been dominant over the previous centuries continued to reign supreme (see my *Reorient: Global Economy in the Asian Age*, particularly Chapter 3). The period from 1848/50 to 1873/80 was marked by a bimetallic silver/gold standard, in which silver continued to weigh heavily, both in the extent of reliance still on silver in the world economy, as well as in the preponderant number of countries that remained on the silver standard. Thus silver continued first supreme and then still very significant throughout three-quarters of the nineteenth century. Gold did not even begin its monetary rule until after 1873 and was only dominant for the four decades until 1913. Interestingly that is also precisely the period in which the gold standard's principal supporter, Britain, was losing its previous dominance in the world economy, which, therefore, covered the mere three decades preceding 1873. That was of course also the date that marked the beginning of "The Great Depression" that lasted until 1895. Yet even during that period, the monetary standard continued to be silver in Mexico, by far the world's largest producer before the new Colorado mines began to produce, and in China and at Britain's own instance India, which still continued as the world's largest importers of silver. Some other large countries, including

Russia and the Ottoman Empire, also remained on the silver standard, and the United States continued on a bimetallic standard until very late in the nineteenth century. Thus and contrary to widespread belief that in the nineteenth century the world was on the gold standard, it's never more than only partial rule was confined to only the last three decades of that century and the first one of the twentieth century. Britain's attempt to return to the gold standard after the First World War proved to be a disaster for Britain; and Roosevelt's tying an ounce of gold to 35 dollars put much of the world on a *de facto* dollar standard until Nixon again separated them and the Bretton Woods arrangements gave way to flexible exchange rates in the early 1970s. Kenwood and Lougheed (1999: 109) summarize: "In short, the international gold standard was in full sway only from perhaps 1897 (some would argue 1880) to 1914, less than 20 (or just over 30) years." Decline in price of silver meant a 50 percent devaluation for silver standard countries, such as China, Chile, India, Japan, Korea, Malaya, Mexico, and Thailand (Kenwood and Lougheed, 1999: 74).

The Myths of the Missionaries

Missionaries went out to spread the word of God. NOT SO.

An African saying had it "first we had the land, and you had the book. Now we have the book, and you have the land." Indeed, however well intentioned many individual missionaries may have been, *de facto* they served as the *avant garde* spearhead all around the world of colonialism and neocolonialism—and still do. At best, they played the "good cop" role opposite the colonial military, political, and administrative "bad cop." In present American military parlance, they served "to soften up" the population for the European or American invasion to come. Even their medical and educational services were part and parcel of a policy and praxis of cultural and ideological, indeed also political, coca cola-colonization and Western assimilation. And that, more often than not, is why they were financed and supported by those who sent them out to proselytize the world. Bayly (2004: 154) observes correctly that "most agents of Christianity were indigenous people, not European missionaries and administrators." But that does not support, but rather counters his conclusion that, therefore, the European role should not be exaggerated. For on the contrary it shows how European missionary influence was extended and multiplied by its indigenous agents and influence. There can be no doubt that the rapid spread of Christianity around the world went along with, indeed often went ahead of, or was part and parcel of the

spread of imperialism. And vice versa, imperialism spread with the alleged word of God, which legitimized the “white man’s burden” of the *mission civilizatrice*.

The Myth that Imperial Colonialism Does Not Pay

European colonialism and imperialism, indeed and all political (colonial) and economic (free trade or other) “North-South” relations made no significant contribution to the development of the North. Calculation of the numbers show that the percentages of direct bilateral foreign trade, payments, and possible profits with or from the “Third World,” or with even all of the world, were too small significantly to affect the development of Britain, Europe, or the United States, as do those of O’Brien (also see his “Balance Sheet for the Acquisition, Retention and Loss of European Empires Overseas” [1998]), Paul Bairoch, and Walt Rostow only to name a few. Bairoch (1997 *passim*) is repeatedly very clear that without risking error (II–673) colonialism was bad for the colonized, but for the colonizers it did no good. NOT SO.

British prime ministers would frankly say what it was all about, and Disraeli explained that the Indian empire should pay for itself and that Indian resources should be available for the British imperial cause (Arrighi, 1994: 47). The key words are the last three, for it was India’s support of the entire system, and not just its bilateral relationship with Britain, that was at issue.

Therefore, all calculations of whether Britain, Europe, and/or the United States benefited or not from their colonies or neocolonies are already vitiated *ab initio*, because in almost every case they take into account only of the bilateral flows of investment, trade, and payments between here and there. The same goes for the assessment of equal or unequal benefits from voluntarily or involuntarily entering into any bilateral exchange relations, as much theory and many analyses presume. On the “left,” on the other hand calculations and often only presumptions on the contrary are that the “North” or some part thereof did benefit, substantially or only marginally, from their colonial, imperialist, or neocolonial and de facto imperialist relations with some part or all of the “South.” However, no matter what the answer to the question “which side are you on,” all these calculations about bilateral relations are largely altogether irrelevant for their own purposes of determining the balance sheet of gains and losses. For these can be determined if at all ONLY by examining the entire system of MULTILATERAL and not simply bilateral or even trilateral imbalances of trade and payments at any one time and also historically. As we will see later, that was unavoidably (even if mostly neglected) the case during our last

period after 1873; but it was to a lesser but nonetheless significant degree already in our first and then second period when also, as the aforementioned Hildgerdt (1942: 45) put it, "the significance of such trade can by no means be gauged by its percentage share in the total."

That is all the more so the case when we consider the opportunity costs lost and gained and the entropy dissipated by this multilateral trade. Two simple examples from the eighteenth and nineteenth centuries will illustrate the matter. The ability for British and other Europeans to import calories from overseas, first in the form of sugar and fish (Atlantic cod) and later of wheat, where their production was relatively cheap for both ecological economic (abundant land and Gulf stream fed water), and socio-political economic reasons (slavery and others), freed Europeans from producing the same calories for their own consumption with their own increasingly scarce land and related resources, which, therefore, could also more profitably be devoted to other uses and/or saved for future generations. Similarly with clothing and its backward linkages. In Britain it was said that "sheep ate men," because the enclosures of common farm land in order to devote it to raising sheep for their wool drove farmers off their land. It also involved deforestation of scarce forests, whose wood was being burnt if not devoted to shipbuilding and so on, to make way for grazing land. The ability of Britain first to import cotton textiles from India, and then to produce them at home but with cotton grown in and imported from India and the southern United States, allowed the British to avoid having to produce that much more wool from sheep that would have required the supply of untold amounts of additional grazing land, and the deforestation and depopulation that would have been involved. The earlier British and European import of cotton textiles was rendered possible only by its payment in silver derived from (also) triangular trades with Spanish America. The later replacement of additional wool clothing by home-produced textile clothing was derived from Britain's privileged position and role at the apex of various trade and payments triangles to be examined later. To have replaced the amount of yarn spun from imported cotton by homegrown wool would in the first trimester of the nineteenth century alone have required more than all of Britain's available pasture and crop land combined. A similar acreage of forest land was saved by the replacement as fuel of wood by coal. Much of the entropy generated by all that, not to mention the entropy that would have been generated by the exhaustion of resources was obviated by the British participation in privileged positions with even only incipient new system of multilateral trade and payments (Pomeranz, 2000: 274–278; Frank, 1998).

After devoting over two decades to proving that colonies do not pay because they cannot by virtue of their far too small place in the trade of the industrializing countries, by now even Patrick O' Brien has come

around a bit to a position he used to dismiss. First, “between 1846–1914 European powers extended, maintained and defended empires overseas ‘on the cheap.’ Indeed, Crouzet notes that now “O’ Brien tells us... that military power, security, foreign trade and empire did matter, and that Britain did receive real and significant returns on its heavy investment in war” (Crouzet, 2002: 69).

Then, industrialization was furthered by being able to export to captive colonial markets and to profit from the associated transport and service earnings. Additionally, “not small” shares of national income can be attributed to the British Empire, between 1, 2, and 4 percent in 1870 and 5–6.2 percent in 1913. But these bilateral calculations miss the essential point. That is that the structure, organization, functioning, and transformation of the global world economy itself and the metropolitan or colonial location within it of any particular unit also accounts for as much or very probably more, as per the titles of Adam Smith and David Landes, of “the wealth and poverty of nations.” By confining analysis almost entirely to bilateral and neglecting multilateral relations of the latter, we are, therefore, also able to convey at best only half or even less of the truth. I leave it to the reader to judge whether a half-truth or less is better or worse than none.

Conclusions

So, if most of the received wisdom about the nature and causes of the wealth of nations during the nineteenth century does not stand up under the weight of evidence, then what is it instead or at least in addition to that we must and should consider? For a more careful examination of nineteenth-century reality belies the now popular beliefs and still most “scientific” opinion about the supposed Western penetration particularly of the Asian economy in most of the nineteenth century. As I stated at the start of this chapter: Without trying by any means morally to justify or politically to support any and all imperialism and colonialism nor any of its consequences, the time has come to review and where appropriate to revise the substantially ideological dogma of Western triumphalism over alleged “traditionalism” elsewhere and simultaneously of much of the nationalist appeal to and “defenses” of “traditional” values and also its exaggeration of the deformation of “Third World” economies. To do so in no way negates the critique of ideologically inspired classical, neo-classical, and Keynesian “scientific” analysis and political propaganda by dependence and world-system theory and their alternative analyses. The reexamination of reality and its still other alternative analysis proposed later may also parallel the denunciation of the received wisdom of both

now “traditional,” and the new dependence as well as world-system theory by their denunciation by recent postmodernist, postcolonial, and subaltern textual “analysis” as far as the latter go, which is not much. For they offer no examination and much less analysis of any political economic reality and its history. Most importantly they have and offer no global perspective, examination, nor political economic history and analysis of the one world economy and system whose own whole globe-encompassing structure and dynamic is so determinant of the possibilities, options, and, therefore, successes and failures of its ever-changing geographic, political, economic, social, and cultural parts.

The at least preliminary answer is how “location, location, location” in an expanding and deepening multilateral world system and its UN level playing field contributed to if not determined the absolute and relative wealth and poverty of nations and the dissipation/absorption of entropy among, not to mention within, them. Indeed, taking a voluntary or even a forced position on and playing from it made the field even more uneven.

Notes

1. Editors' note: This chapter is an unpublished Chapter 1 from an unpublished book manuscript, *ReOrient the 19th Century*, that Gunder Frank was writing prior to his passing away. Other than a few syntax changes, the chapter is presented “as it is.” The book was written as a sequel to his earlier published book, *ReOrient: Global Economy in the Asian Age*.
2. Editors' note: Gunder Frank is referring here to his unpublished book manuscript, *ReOrient the 19th Century*.
3. Editors' note: Gunder Frank is referring to his unpublished book manuscript, *ReOrient the 19th Century*.

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CHAPTER NINE

Social Movements

This chapter will develop the following theses:¹

1. The “new” social movements are not new, even if they have some new features and the “classical” ones are relatively new and perhaps temporary.
2. Social movements display much variety and changeability but have in common individual mobilization through a sense of morality and injustice and social power through social mobilization against deprivation and for survival and identity.
3. The strength and importance of social movements is cyclical and related to long political economic and (perhaps associated) ideological cycles. When the conditions that give rise to the movements change (through the action of the movements themselves and/or more usually because of changing circumstances), the movements tend to disappear.
4. It is important to distinguish the class composition of social movements, which are mostly middle class in the West, popular/working class in the South, and some of each in the East.
5. There are many different kinds of social movements. The majority seeks more autonomy rather than state power and the latter tends to negate themselves as social movements.
6. Although most social movements are more defensive than offensive and tend to be temporary, they are important (today and tomorrow perhaps the most important) agents of social transformation.
7. In particular, social movements appear as the agents and reinterpreters of “delinking” from contemporary capitalism and “transition to socialism.”

8. Some social movements are likely to overlap in membership or be more compatible and permit coalition with others and some are likely to conflict and compete with others. It may be useful to inquire into these relations.
9. However, since social movements, like street theater, write their own scripts—if any—as they go along, any prescription of agendas or strategies, let alone tactics, by outsiders—not to mention intellectuals—is likely to be irrelevant at best and counterproductive at worst.

The “New” Social Movements Are Old

The many social movements in the West, South, and East that are now commonly called “new” are with few exceptions new forms of social movements that have existed through the ages. Ironically, the “classical” working-class and union movements date mostly only from the last century, and they increasingly appear to be only a passing phenomenon related to the development of industrial capitalism. On the other hand, peasant movements, localist community movements, ethnic and nationalist movements, religious movements, and even feminist or women’s movements have existed for centuries and even millennia in many parts of the world. Yet many of these are now commonly called “new,” although European history records countless social movements such as the Spartacist slave revolts in Rome, countless religious wars, the peasant movements, or wars, of sixteenth-century Germany, historic ethnic and nationalist conflicts throughout the continent, and women’s movements that unleashed backlashes of witch hunts and more recent forms of repression. In Asia, the Arab world and the expansion of Islam, Africa, and Latin America, of course, multiple forms of social movements have been the agents of social resistance and transformation throughout history.

Only ecological or green movements and peace movements can more legitimately be termed new, and that is because they respond to social needs that have been more recently generated by world development. Generalized environmental degradation as a threat to livelihood and welfare is the product of recent industrial development and now calls forth largely defensive new ecological, or green, social movements. Recent technological developments in warfare threaten the lives of masses of people and generate new defensive peace movements. Yet even these are not altogether new. World, colonialist, or imperialist capitalist development has caused, or been based on, severe environmental degradation in many parts of the Third World before (as after the conquest of the Americas, the slave wars and trade in Africa, the rape of Bengal, for example) and has aroused

defensive social movements, which included, but were not confined to, environmental issues. Of course, war has also decimated and threatened large populations before and has elicited defensive social movements from them as well. Foreshadowing our times, Aristophanes described a classical Greek women's and peace movement in his play *Lysistrata*.

Whether new or old, the "new social movements" today are by far what mobilizes most people in pursuit of common concerns. Far more than "classical" class movements, the social movements motivate and mobilize hundreds of millions of people in all parts of the world—mostly outside established political and social institutions that people find inadequate to serve their needs, which is why they have recourse to "new," largely noninstitutionalized social movements. This popular movement to social movements is manifest even in responsive behavior outside of membership in the social movements themselves: Millions of people around the world have spontaneously responded to visits by the Pope (beyond the Catholic Church as an institution) and there was massive spontaneous response to Bob Geldof's extrapolitical institutional Band Aid, Live Aid, and Sport Aid appeals against hunger in Africa. The latter was an appeal and a response not only to compassion but also to the injustice of it all.

The Appeal to Justice and Injustice in Social Movements

Varied as these social movements have been and are, if there is a single characteristic that they have in common it is probably their defensive concern with justice or their offensive defense against a shared sense of the injustice, as analyzed in Barrington Moore's *Injustice: The Social Bases of Obedience and Revolt* (1978). This felt concern with (in)justice, perhaps more than the deprivation of livelihood and/or identity through exploitation and oppression through which this sense of (in)justice manifests itself, has probably been the essential motivating and driving force of social movements both past and present. However, this concern with (in)justice refers largely to "us," and the social group perceived as "we" was, and is, very variable as between the family, tribe, village, ethnic group, nation, country, First, Second, or Third world, or humanity, and gender, class, stratification, caste, race, and other groupings, or combinations, of these. What mobilizes us is this deprivation, oppression, or injustice to "us," however "we" define and perceive ourselves. Each social movement then serves not only to combat against deprivation but in doing so also to reaffirm the identity of those active in the movement and perhaps also those "we" for whom the movement is active. Thus, such social movements, far from being new, have characterized human social life in many times and places.

The “classical” working-class and labor union movements can now be seen to be particular social movements, which have arisen and continue to arise in particular times and places. Capitalist industrialization in the West gave rise to the industrial working class and to its grievances, which were expressed through working-class and unionization movements. However, these movements have been defined and circumscribed by the particular circumstances of their place and time—in each region and sector during the period of industrialization and as a function of the deprivation and identity that it generated. “Workers of the world unite” and “proletarian revolution” have never been more than largely empty slogans. With the changing international division of labor, even the slogans have become meaningless. Working-class and union movements are eroding in the West, while they are rising in those parts of the South and East where local industrialization and global development are generating analogous conditions and grievances. Therefore, the mistakenly “classical” working-class social movements must be regarded as both recent and temporary, not to mention that they have always been local or regional and at best national- or state-oriented movements. We will examine their role in the demand for state power when we discuss the latter below.

So what is new in the “new” social movements is perhaps that they now tend to be more single class or stratum movements—middle class in the West and popular working class in the South—than many of them were through the centuries. However, by that criterion of newness, the “classical” old working-class movements are also new and some contemporary ethnic, national, and religious movements are old, as we will observe when we discuss the class composition of social movements below.

Social Movements Are Cyclical

Social movements are cyclical in two senses. First, they respond to circumstances that change as a result of political economic cycles. Second, social movements, their membership, mobilization, and strength tend to be cyclical because the movements usually mobilize most people to respond negatively to circumstances that are themselves cyclical. Political-economic cycles—which one is more determinant is under dispute—generate changing circumstances that affect people detrimentally, or at least more so, particularly during the “downward” economic and “upward” political phases of the cycles. Although it is not beyond dispute, there is growing evidence of long political-economic cycles, often associated with the name of Kondratieff. This long cycle was in an upward phase at the beginning of this century. Then came a long downward “crisis” inter-war phase (where the two world wars belong in the cycle is also under

dispute), a renewed postwar recovery, and again a new downward "crisis" phase beginning in the mid-1960s and becoming more visible since 1973. Social movements appear to have become more numerous and stronger in the downward phase from 1873 to 1896 in the preceding century, during the war and interwar crisis period of this century, and again during the contemporary period of economic, political, social, cultural, ideological, and other crises.

It is useful to examine the historical context, if not determination, of past and contemporary social movements, although many of their members regard themselves as moving autonomously in pursuit of timeless and sometimes universal-seeming ideals, like the true religion, the essential nation, or the real community. The development of the present world political-economic crisis and its multiple ramifications in different parts of the world is generating or aggravating feelings of economic, political, cultural, and identity deprivation and is an affront to the sense of justice for hundreds of millions of people around the world. In many cases, particularly among middle-class people, newly deteriorating circumstances contradict their previously rising aspirations and expectations. More and more people feel increasingly powerless themselves and/or see that their hallowed political, social, and cultural institutions are less and less able to protect and support them. Therefore—and in part paradoxically—they seek renewed or greater empowerment through social movements that are mostly defensive of livelihood and/or identity, like rural and urban local community, ethnic, nationalist, and some religious movements, or often are escapist like the mushrooming religious cult, spiritualist, and some fundamentalist movements. Ecological, peace, and women's movements separately or in combination with the other social movements—also seem to respond to the same crisis-generated deprivation and powerlessness that they seek defensively to stem or redress. Only marginally are these movements offensively in pursuit of betterment, like the women's movement that seeks to improve women's position in society and society itself albeit at a time when the economic crisis is undermining women's economic opportunities.

As social movements come and grow cyclically in response to changing circumstances, so do they go again. Of course, if the demands of a particular social movement are met, it tends to lose force as its *raison d'être* disappears or when it is institutionalized and ceases to be a social movement. More usually, however, the circumstances themselves change only in part, if at all, thanks to the social movement itself—and the movement loses its appeal and force through irrelevance or it is transformed or its members move to another movement with new demands. Moreover—as movements that mobilize people rather than institutionalizing action—even when they are unsuccessful or still relevant to existing circumstances,

social movements tend to lose their force as their capacity to mobilize wanes. This susceptibility to aging and death is particularly true of social movements that are dependent on a charismatic leader to mobilize its members. The various 1968 movements—and most revolutionary and peasant movements—are dramatic examples of this cyclical life cycle of social movements.

Social Movements' Class Composition

The new social movements in the West are predominantly middle-class-based. This class composition of the social movements, of course, reflects in the first instance the changing stratification of Western society from more to less bipolar forms. The relative and now often absolute reduction of the industrial labor force, like the agricultural one before it, and the growth of tertiary service sector employment (even if much of it is low waged) and self-employment increased the relative and absolute pool of middle-class people. The decline in industrial working-class employment has reduced not only the size of this social sector but also its organizational strength, militancy, and consciousness in “classical” working-class and labor union movements. The grievances about ecology, peace, women’s rights, community organization, and identity—including ethnicity and minority nationalism—seem to be felt and related to demands for justice predominantly among the middle classes in the West. However, ethnic, national, and some religious movements straddle class and social strata more. Only nationalist chauvinism and perhaps fundamentalist religiosity (but not religious cultism and spiritualism) seem to mobilize working-class and some minority people more massively than their often nonetheless middle-class leadership. Although most of these people’s grievances may be largely economically based through increased deprivation or reduced or even inverted social mobility, they are mostly expressed through allegiance to social movements that pursue feminist, ecological, peace, community, ethnic, nationalist, and ideological demands.

In the Third World, social movements are predominantly popular or working class. Not only does this class or stratum have more weight in the Third World, but its members are much more absolutely and relatively subject to deprivation and they felt injustice that mobilizes them in and through social movements. Moreover, the international and national or domestic burden of the present world economic crisis falls so heavily on these already low-income people as to pose serious threats to their physical and economic survival and cultural identity. Therefore, they must mobilize to defend themselves—through social movements—in the absence of the availability or the possibility of existing social and political institutions

to defend them. These Third World social movements are at once cooperative and competitive or conflictive. Among the most numerous, active, and popular of these social movements are a myriad of often spontaneous local rural and urban organizations or movements that seek to defend their members' survival through cooperative consumption, distribution, and production. Examples are soup kitchens; distributors, and often producers, of basic necessities such as bread; organizers, petitioners, or negotiators; and sometimes fighters for community infrastructure such as agricultural and urban land, water, electricity, or transport. Recently there were over 1,500 such local community movements in Rio de Janeiro alone, and they are increasingly widespread and active in India's 600,000 villages. However, not unlike working-class and peasant movements before, these popular movements often have some middle-class leadership and now ironically offer some opportunities for employment and job satisfaction to otherwise unemployable middle-class and intelligentsia professionals, teachers, or priests, who offer their services as leaders, organizers, or advisers to these communities and other popular Third World social movements.

More often than not, these local community movements overlap with religious and ethnic movements that lend them strength and promote the defense and assertion of people's identity. However, ethnic, national, and religious movements also straddle class membership more in the Third World. Ethnic, religious, and other "communal" movements in South Asia (Hindu, Moslem, Sikh, Tamil, Assamese, and many others) and elsewhere in the Third World—and perhaps most dramatically and tragically so in Lebanon—also mobilize peoples against each other, however. The more serious the economic crisis—and the political crisis of state and party to manage it and the greater the deception of previous aspirations and expectations, the more serious and conflictive are these communal, sometimes racial, and community movements likely to grow in the popular demand for identity in many parts of the Third World.

The so-called Socialist East is by no means exempt from this worldwide movement to social movements. The 10 million mobilized by Solidarity in Poland and various movements in China are well-known examples, but other parts of Eastern Europe and even the Soviet Union are increasingly visited by similar movements. However, corresponding to the Socialist East's intermediary or overlapping position between the industrial capitalist West and the Third World South—if these categories still have any utility or meaning, which is increasingly doubtful—the social movements in the Socialist East also seem to straddle or combine class or strata membership more than in the West or the South. Ethnic, nationalist, religious, ecological, peace, women's, regional, community, and other protest movements with varied social membership are on the rise both within and outside of the institutional and political structure throughout the socialist countries

for reasons of, and in response to, changing circumstances similar to those in the rest of the world.

Social Movements and State Power

Most social movements do not seek state power. For many participants and observers, this statement is a truism; since not seeking, let alone wielding, state power is a *sine qua non* of a social movement, state power would negate the very essence and purpose of most social movements. This incompatibility between social movement and state power is perhaps most intuitively obvious for the women's movement. On the other hand, for both participants in and observers of social movements, it is hardly satisfactory to define or even describe them in terms of what they are not instead of what they are. The most numerous are individual, small-scale, community-based social movements that, of course, cannot seek state power. However, similarly to the women's movement, the very notion of state or even political party power for them would negate much of their grassroots aims and essence. These community movements mobilize and organize their members in pursuit of material and nonmaterial ends that they often regard as unjustly denied to them by the state and its institutions, including political parties. Among the nonmaterial aims and methods of many local community movements is more participatory democracy and self-determination. These are sensed as being denied to them by the state and its political system. Therefore, the community movements seek either to carve out greater self-determination for themselves within the state or to bypass the state altogether. These community movements have recently mushroomed all over the South and West, although perhaps less so in the latter. Of necessity, in the South the community movements are more cant: rued with material needs, and often survival itself, while in the West many can afford to devote greater attention to local grassroots participatory democracy. Of course, most forces of the national and world economy are beyond their control and severely limit the community movements' room to maneuver. Not even national states have sufficient power and do not protect the communities in the face of world economic forces beyond their control. That is why—perhaps ironically, since they are even more powerless—the local communities attempt protection on a do-it-yourself basis.

The other side of this same coin is, especially during the economic crisis, the increasing disappointment and frustration of many people with the economy itself. So many economic slogans and "solutions," such as "economic growth," "economic development," "economic ends," "economic means," "economic necessities," and "economic austerity," do not

satisfy people's needs for community, identity, spirituality, or often even material welfare. Moreover, political or state institutions are perceived as handmaidens rather than alternatives or even satisfactory directors of these supposed economic imperatives. No wonder that particularly women, who suffer the most at the hands of the economy, are in the forefront of non- and antieconomic extrainstitutional social movements that offer or seek other solutions and rewards.

Many social movements also respond to people's frustration with, and sense of injustice toward, political economic forces beyond their control. Many of these economic forces—sometimes perceived, sometimes not—emanate from the world economy in crisis. Significantly, people increasingly regard the state and its institutions, particularly political parties, as ineffective in the face of these power forces. The state and its political process either cannot or will not face up to, let alone control, these economic forces. In either case, the state and its institutions, as well as the political process and political parties where they exist, leave people at the mercy of forces to which they have to respond by other means—through their own social movements. Accordingly, people form or join largely protective and defensive social movements on the basis of religious, ethnic, national, race, gender, ecological, and peace issues as well as community and various “single” issues. Most of these movements mobilize and organize themselves independently from the state, its institutions, and political parties. They do not regard the state or its institutions—and particularly membership or militancy in political parties—as adequate or appropriate institutions for the pursuit of their aims. Indeed, much of the membership and force of contemporary social movements is the reflection of peoples' disappointment and frustration with—and their search for alternatives to—the political process, political parties, the state, and the capture of state power in the West, South, and East. The perceived failure of revolutionary as well as reformist left-wing parties and regimes in all parts of the world adequately to express people's protests and to offer viable and satisfying alternatives has been responsible for much of the popular movement to social movements. In many cases, however, people's grievances are against the state and its institutions, and in some cases social movements seek to influence state action mostly through outside—much more rarely inside—pressure. Only some ethnic and nationalist (and in the Islamic world, some religious) movements seek a state of their own.

One of the major problems of and with social movements, nonetheless, is their coexistence with national states, their political institutions, processes, and parties. An illustration of this problem is the Green Movement and party in Germany. The originally grassroots ecological movement became a political party in Parliament. The realist *realpolitik* (“*Realo*”)

wing argues that the state, parliament, and political parties are a fact of life that the movement must take into account and use to its advantage. Influence is best exerted by entering these institutions and cooperating with others from the inside. The fundamentalist ("Fundis") wing argues that participation in state institutions and coalitions with other political parties (e.g., Social Democrats) compromises the Green's aims and prostitutes their fundamentals, including that of being a movement. Ethnic, national, religious, and some peace and community movements face similar problems. Whatever they can do outside the state the pressure sometimes becomes irresistible to try to act within the state, as or as part of or through a political party or other state institutions. But then the movement runs the danger of compromising its mission, demobilizing or repelling its membership, and negating itself as a movement. The question arises whether the end justifies the means and is more achievable through other more institutionalized, nonmovement means. Moreover, the question arises whether old social movements—which were often created as mass front organizations of political parties—are now being replaced by new social movements that themselves form or join political parties. But in that case, what difference remains between the old and the new social movements, and what happens to the nonextra antistate and party sentiments and mobilization of many movement members? Perhaps the answer must be sought by shifting the question to the examination of the life cycle of social movements and the replacement of old "new" movements by contemporary movements.

Social movements are important agents of social transformation and new vision, despite their aforementioned defensiveness, limitations, and relations to the state. One reason for the importance of social movements, of course, is the void they fill where the state and other social and cultural institutions are unable or unwilling to act in the interests of their members. Indeed, as we have observed above, social movements step in where institutions do not exist, or where they fail to serve, or violate and contradict, people's interests. Often, social movements step in where angels fear to tread. Although many social movements—and particularly religious ones—invoke the sanctity of traditional ways and values, other social movements are socially, culturally, and otherwise innovative. Nonetheless, if the circumstances that give rise to and support a social movement disappear, so does the movement. If the movement achieves its aims or they become irrelevant, it loses its appeal. It loses steam and fades away, or it becomes petrified.

Much social transformation, cultural change, and economic development, however, occurs as the result of institutions, forces, or relations that are not social movements or the political process in national states. World economic development, industrialization, technological change,

social and cultural “modernization” were and are processes that are hardly driven or directed by social movements or political or state institutions. Their intervention has been more reactive than promotive. Although state intervention should not be underestimated (as it is by the free marketeers), its limitations are ever greater in a world economy whose cycles and trends are largely beyond control. Even “socialist” state ownership and planning is now unable to direct or even to cope with the forces of the world economy.

These circumstances should make for more realism and modesty about the prospects of social movements—or, for that matter, of political institutions—and their policies to counteract or even modify, let alone to escape from, these world economic forces, but they do not. On the contrary, the more powerful and uncontrollable the forces of the world economy are, especially in the present period of world economic crisis, the more they generate social movements (and some political ideological policies) that claim both autonomy and immunity from these world economic forces and promise to overcome them or to isolate their members from them. Much of the attraction of many social movements, of course, comes precisely from their promise to free their members from the deeply felt unjust threat of deprivation of material necessities, social status, and cultural identity. Therefore, objectively irrational hopes of salvation appear as subjectively rational appeals to confront reality and to serve oneself and one’s soul through active participation in social movement. The message becomes the medium, to invert Marshall McLuhan.

Delinking and Transition to Socialism **in Social Movements**

Social movements today and tomorrow may be regarded as offering new interpretations and solutions to the problems of “delinking” from capitalism and “transition to socialism.” For the dependent South, national or state delinking from the world capitalist economy and its cycles proved to be impossible during the postwar period of expansion. Eastern socialist states and their planned economies have been relinked to the world economy and both its cycles and its technological development during the present crisis in the world economy. No national economy or its state and hardly any political parties anywhere in the world today seriously regard delinking a national economy to be a serious practical proposition. Therefore, the thesis about delinking—stop the world, I want to get off—is in for an agonizing reappraisal from those (like the present writers) who have sustained this as an option and a necessity. However, if

the nation or state and the economy are not and cannot be independent today or in the foreseeable future, perhaps the idea of “delinking” can and should be reinterpreted rather than abandoned altogether.

The problems of “delinking” may be reinterpreted through the different and new links that many social movements are trying to forge, both between their members and society and within society itself. The women’s movement and some green ones are examples. Many social movements seek to protect their members physically or spiritually from the vagaries of the cyclical world economy. They propose different kinds of links for their members to the economy and for society, which they also propose to help change. Perhaps “delinking” should be amended to read different linking or changed links. In that case, it is the social movements that are changing some links into different ones for their members today. This would include those religious and spiritualist movements that claim to offer isolation and protection from the traumas of the secular world to their true believers.

Similarly, the problems and prospects of transition to socialism may be reinterpreted in view of the experience with the current form of socialism and contemporary social movements. The current form of socialism has proven unable to delink from the world capitalist economy. Moreover, despite its achievement in promoting extensive growth by mobilizing human and physical resources, it has failed to provide adequately for intensive growth through technological development. Indeed, the same state planning that was an asset for absolute industrial autocratic national growth has proven to be a liability for competitive technological development in a rapidly changing world economy. The related political organization of the current form of socialism has lost its efficacy at home and its attraction abroad. Most important, perhaps, it is becoming increasingly clear that the road to a better “socialist” future replacement of the present capitalist world economy does not lead via the current form of socialism. As the Polish planner Josef Pajestka observed at a recent meeting at the Central School of Planning and Statistics in Warsaw, the current form of socialism is stuck on a side track. The world—as one of the present authors remarked—is rushing by in the express train on the main track, even though, as Pajestka retorted, it may be heading for an abyss.

The real transition to a “socialist” alternative to the present world economy, society, and polity, therefore, may be much more in the hands of the social movements. They must intervene for the sake of survival to save as many people as possible from any threatening abyss. We must also look to the social movements as the most active agents to forge new links that can transform the world in new directions. Moreover, although some social movements are subnational, few are national or international—in

the sense of being between nations or states—and many like the women's, peace and ecological movements could be transnational—that is, nonnational—or people-to-people within the world system. This real socialist transformation, if any, under the agency of the social movements will, however, be more supple and multifarious than any illusionary “socialism in one country” repeated again and again. Thus, even if most movements do not achieve their objectives, their very movement itself helps transform social relations and, therefore, society.

Coalitions and Conflict among Social Movements

It may be useful—without seeking to give any advice—to inquire into likely possibilities of conflict and overlap or coalitions among different kinds of social movements. Aristophanes already remarked on the relation between women and peace in *Lysistrata*. Riane Eisler has traced this same relation even farther back in human society in her *The Chalice and the Blade* (1987). Today, the women's and peace movements share membership and leadership and certainly offer opportunities for coalition. Substantial membership and leadership overlap can also be observed between women's movements and local community movements. At least women are especially—and in Latin America preponderantly—active in community movements, where they acquire some feminist perspectives and press their own demands, which serve to modify these movements, their communities, and, one hopes, society. In the West, there is a similar if lesser overlap between community and peace movements that expresses itself in “nuclear-free” communities, for instance. Again, environmental, ecological, or green movements in the West share compatible goals and membership with women's, peace, environmental, and community movements all of which shy away from pursuit of state power and most entanglements with political institutions—and they offer widespread opportunities for coalitions among social movements.

Other areas of overlap, shared membership, and compatibility or coalition may be observed among some religious and ethnic, national, and sometimes racial movements. The movement led by the Ayatollah Khomeini in Iran and some of his followers elsewhere in the Islamic world is the most spectacular example and has the most massive and successful mobilization of recent times to its credit. The Sikhs in Punjab, the Tamils in Sri Lanka, Solidarity in Poland, Albanians in Yugoslav Kosovo, and Irish Catholics in Northern Ireland are other recent examples. Notably, however, these religious-ethnic-nationalist movements also seek state power or institutional autonomy and sometimes incorporation with a neighboring ethnic or national state. If communities are religiously and ethnically homogeneous, there may be overlap or coalition with these larger movements.

There are also significant areas of conflict and competition among social movements. Of course, movements of different religions and ethnicities or races conflict and compete with each other. However, all of them also seem to conflict and compete with the women's movement and often with the peace movement. In particular, virtually all religious, ethnic, and nationalist movements—such as working-class and Marxist-oriented movements and political parties as well—negate and sacrifice women's interests. Moreover, they successfully compete with women's movements, if any, which lose ground they may already have gained to the onslaught of religious, ethnic, and nationalist movements. Religion and nationalism, and even more so the two combined, seem to be the worst enemies of the women's interests and movements.

The Impropriety of Outside “Good” Advice to Social Movements

As long as the social movements have to write their own scripts as they go along, they cannot use and can only reject as counterproductive any prescriptions from on high or outside as to where they should go or how they should get there. In particular, the social movements cannot use the kind of imaginary blueprints for the future that Smith and Marx avoided but which have been so popular among many of those who claim to speak in their name. For this reason, also, good advice from intellectuals and other well-meaning people is both hard to find and hard to assimilate for the social movements. Most inappropriate, perhaps, is supposed counsel from nonparticipant observers. On the other hand, many social movements can and do benefit from the vision and organizational skill inputs by participants and more rarely from transient outsiders who transfer some vision and/or experience from other movements, parties, and institutions. Many community movements also benefit from, or even depend on, the support of outside institutions, such as the Church, nongovernmental organizations (NGOs), and occasionally even the state. Such aid and dependence also involve dangers of co-optation by these institutions of individual leaders or intermediaries, the leadership and its goals, or even the social movement itself. Nonetheless, what most characterizes social movements is that they must do their own thing in their own way. In fact, perhaps the most important thing that social movements have to offer both to their participants and to others in the world is their own participatory, self-transforming trial-and-error approach and adaptability. Herein is the hope they promise for the future.

Notes

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1. Editors' note: We selected this article to end the book to underscore the fact that Gunder was always committed to encompassing issues of justice and freedom. This chapter was published over twenty years ago and contains ideas and references that do not reflect some of the theoretical reorientation that Gunder Frank had in the mid-1990s, which is evident in the earlier chapters here. We are convinced that if Gunder was still with us, he would have updated this chapter in order to reflect his theoretical reorientation.

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