



4-1-1994

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Recommended Citation

Wescott, Roger Williams (1994) "Civil Systems: A Review of the World-System Theories of Andre Gunder Frank and of Christopher Chase-Dunn and Thomas D. Hall," *Comparative Civilizations Review*: Vol. 30 : No. 30 , Article 8.
Available at: <https://scholarsarchive.byu.edu/ccr/vol30/iss30/8>

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CIVIL SYSTEMS: A REVIEW OF THE WORLD-SYSTEM THEORIES OF ANDRE GUNDER FRANK AND OF CHRISTOPHER CHASE-DUNN AND THOMAS D. HALL¹

Roger Williams Wescott

I am too much an heir of lexicographer Samuel Johnson to have reconciled myself to the term “civilizationist.” But I admit to being both a student of civilization and a comparer of civilizations. In the former role, I am drawn to world history and to world-systems theory. In the latter role, I am drawn to ethnographies and to the comparative (as well as the contrastive) study of civilizations.

If it is objected that this is fence-straddling and that I should make up my mind, I would reply by quoting the late Wallace Notestein of Yale, who reportedly told his students, at the beginning of each year, “All men must be alike, or we wouldn’t know they are men; and they must be different, or we couldn’t tell them apart.” In more general terms, we might rephrase this formula to read: all phenomena are alike, by virtue of their common phenomenality; and they are different, by virtue of their common plurality.

This dichotomy of the one and the many — permanence and change, identity and disparity — was debated by the Greeks from Heraclitus to Plotinus, long before it resurfaced in modern scholarly polemics.² One way in which it has recurred is in the question of the most suitable name for the comparative study of religions. In the Victorian era, it was usually called Comparative Religion; today, it is more often called World Religions. Although I have taught it only under the older title, I doubt that I would experience any difficulty in presenting it under the newer one — or that the content would change greatly as a result of the name change.

Like Chase-Dunn and Hall (hereafter CD & H), I am an “enthusiastic disciplinary trespasser.”³ I concur with them and evidently too with Frank that disciplinary parochialism impedes the study of civilization — as does regional and temporal parochialism. To these three obstacles, I would add a fourth, ideological parochialism, to be discussed below.

Among the strengths of Frank’s Bronze Age article, I think, is his avoidance of regional parochialism, as manifest in his inclusion of central Asia and northern Eurasia in his early world-system.⁴ Interestingly, there is

evidence of northern Eurasian influence not only on southern Eurasia, through conquest, but also on North America, by means not yet elucidated. The evidence is linguistic and consists of Indo-European and Uralic vocabulary in the Penutian languages of California.⁵

Another commendable aspect of Frank's writings, I believe, is his acknowledgement of the uncertainty of Bronze Age dates, to some of which he would be willing to add two centuries.⁶ In my view, there are no firm pre-Alexandrian dates for any region. Where firm dates are lacking, however, my inclination, unlike Frank's, is to reduce time-depth, largely on the grounds that Hellenistic scholars, like Manetho in Egypt and Berossus in Mesopotamia, tended nationalistically to inflate the antiquity of their countries.⁷ The most drastic reduction of ancient chronology is probably that of Gunnar Heinsohn of the University of Bremen, who grants civilization worldwide an antiquity of little more than three thousand years.⁸ Frank himself implicitly admits the plausibility of such time compression when he acknowledges, with some puzzlement, that economic cycles seem to get shorter as time passes. For it is at least possible that the seemingly long cycles of the early Bronze Age were actually no longer than subsequent cycles.⁹

As long as we are on the subject of ancient dating, I should add that it is not only absolute chronology about which I have doubts but relative chronology as well. More precisely, I am not convinced that the conventional Bronze Age-before-Iron Age sequence is as reliable as the Stone Age-before-Metal Age sequence. It is generally admitted that, in some areas, such as Japan, iron was smelted at least as early as bronze.¹⁰ And it seems that iron ore was mined long before copper in Africa, even though the uses to which the ore was put remain debatable.¹¹ Here too, however, Frank concedes a question about the conventional sequence by noting that iron finds in Indo-China precede bronze by over a century.¹²

Frank, I am relieved to note, does not fall into the familiar social science pattern of treating all political and economic cycles as though they were wholly and necessarily endogenous to human society. He acknowledges intrusive factors such as climate, habitat, and disease.¹³ He even cites a catastrophist, Peter James, on the subject of the dramatically sudden collapse of various Bronze Age civilizations.¹⁴ Most congenial to me, he further refers to legendary material on floods and other protohistoric disasters.¹⁵ What seems particularly significant to me about such references is that they highlight a discrepancy that most historians and prehistorians have been too ready, I think, to dismiss or ignore. This discrepancy is the divergence of professional archaeology, which depicts human development as predominantly gradual and progressive, from worldwide oral tradition, which

depicts the past as marked by extensive and traumatic catastrophes that have left our species psychologically damaged.

There is only one matter of fact on which I find Frank in error and that is (what I read as) his separation of Dorians from other Indo-Europeans.¹⁶ The Dorians, being Greek, were as Indo-European as the Romans, the Hittites, the Persians and other early Eurasian speakers of languages related to modern English, Russian, and Hindi.

Predictably, perhaps, I do not perceive as great a difference between the world-system theories of Frank on the one hand and Chase-Dunn and Hall on the other as I think they themselves perceive. Where they do differ, I tend rather to lean toward Frank, for reasons that I hope to make clear below.

Generally speaking, I find CD & H too prone to treat conceptual dichotomies in a discrete rather than a gradient manner. Questions which they treat as either/or matters seem to me to be more-or-less matters. Ten examples follow.

CD & H ask, "Is social change cyclical or transformational?"¹⁷ What they take to be a question of fact, I take to be one of interpretation. An illustrative example is the ancient Roman political shift from kingdom to republic to empire. Those who call this change cyclical can point quite convincingly to the fact that both kingdoms and empires (as domains ruled by emperors) are monarchies. Yet those who call the change transformational can argue, equally persuasively, I think, that imperial Rome was so different from Tarquin Rome that the two should not be equated, even in terms of governance. In other words, the question posed is less one of objective ontology than of subjective hermeneutics, even though the subjectivity involved may be collective rather than individual.

CD & H characterize Frank as belonging among "the radical apostles of continuity."¹⁸ By contrast, they themselves are presumably conservative with regard to the positing of continuity. In concrete terms, Frank gives the world system an antiquity of about 5,000 years, while they give it one of about 3,500 years. To me, Frank's position seems more liberal than radical. A radical construction of systemic continuity, to my mind, would be one that extended it well into the Neolithic Age. But again, this is a matter more of impression than of fact.

CD & H place themselves among "those theorists who study interaction networks rather than distributions of cultural characteristics."¹⁹ Presumably they mean that their focus is on activities, like trade and conquest, rather than on traits, like literacy and urbanism. A cataloguer of traits, however, might well list commercialism and imperialism as cultural traits typifying advanced societies. Yet, conversely, an interactionist might class writing

and city-building as intrinsically interactive behavior patterns. Either way, however, the dichotomy loses its sharpness.

CD & H describe Hawaii as representative of “geographically isolated systems.”²⁰ In literal terms, one can hardly dispute their assertion, since the Italian source-word *isola* means “island.” But the cultural closeness of Hawaii to the rest of Polynesia makes it clear that Hawaii was nautically accessible to sister islands. And recent research seems increasingly to erode the picture, once dear to arm-chair scholars, of oceans as insuperable barriers to travel.

CD & H reject the view that transmission of sweet-potato cultivation from Peru to Hawaii makes these two regions participants “in the same world-system.”²¹ In the absence of a strict definition of world-systems, I think the question moot. All I would readily concede is that, in this case, systemicity (not “systemness,” please) is minimal.

CD & H describe groups of people as “interacting importantly” with immediate neighbors but proceed to insist that “the search for a certain number of separate small-scale systems is a senseless task.”²² Importance, it seems to me, is far more a matter of attitude than of demonstrability. The same goes, to an even greater degree, perhaps, for senselessness.

Before presenting their Working Hypotheses, CD & H pose the provocative question, “Do all world-systems have core/periphery differentiation and/or core/periphery hierarchies?”²³ To this, both they and I answer in the affirmative, although, I suspect, with slightly different rationales. For them, peripherality apparently correlates with powerlessness vis-a-vis a powerful core area. For me, peripherality correlates with stylistic distance from an archetypal core area. If we take Western (or neo-European) civilization as a system, I would rate Germany as more peripheral to it than the Netherlands, Poland as more peripheral than Germany, and Russia as more peripheral than Poland. But CD & H, I infer, would postulate a different sequence, based more on demographic and political than on geographic criteria.

CD & H seek to distinguish those elements in world-systems that “are meaningfully different” from “those that are meaningfully the same.”²⁴ As regards sameness and difference, I would repeat what I observed above: that all comparison involves both. As regards meaningfulness, I would take a relativistic position: that anything that seems meaningful to anyone is, ipso facto, at least minimally meaningful. On the same page, they announce their intention “to compare large numbers of world-systems.” As a comparatist, I share their goal. But, insofar as a world-system is a civilization, I would say that the feasibility of the project depends on the level of discourse and of classification. At the global level, there is and always has been, I would say, only one civilization; whereas, at every less inclusive level, there are

many.²⁵ At the local level, I would add, the number of civilizations may be equated with the number of cities. (My conceptual model here is biological taxonomy, in accordance with which there is now and always has been only one biosphere, though this unit is divisible into both a hierarchy of taxa and a number of biotic zones.)

To avoid simplistic predictions of our collective future, CD & H recommend “examining earlier major transitions.”²⁶ The problem, however, of distinguishing between major and minor transitions is like that of distinguishing between meaningful and meaningless differences. To most historians in the Western world, the transition from the ‘Medieval’ to the ‘Modern’ era has seemed so momentous as to punctuate the history not only of Europe but also of the world at large. To many comparativists, on the other hand, the distinction has seemed relatively parochial. Spengler, in particular, characterized the global Ancient/Medieval/Modern periodization as “incredibly jejune.”

Much the same observation can be made concerning the distinction made by CD & H between “lumpers and splitters” in the study of world-systems.²⁷ Here, as in so many other fields involving classification, whether implicit or explicit, one man’s lumper is another man’s splitter. Among those comparativists who have given explicit counts of civilization, for example, Carroll Quigley listed 16 civilizations. To Toynbee, who (between 1939 and 1961) listed 43 civilizations, Quigley seemed to be a lumper. To Othmar Anderle, however, who listed 7 civilizations, Quigley seemed to be a splitter.²⁸

The principle here is much the same, again, as that in biotaxonomy, where the number of organic kingdoms recognized by systematists varies from two to six, as below.

1. plants
2. animals
3. protists (such as amoebae)
4. monerans (such as bacteria)
5. fungi
6. viruses

Classificatory minimalists list only the first two kingdoms. To them, those who list three or four kingdoms seem to be splitters. Classificatory maximalists, on the other hand, list all six kingdoms. To them, those who list only three or four seem to be lumpers.

As an anthropologist, I am naturally appreciative of Chase-Dunn’s ethnographic sophistication, as evidenced in his writings on indigenous peoples of East Africa and North America.²⁹ One of the few ethnographic matters on which I would be somewhat critical of his position has to do with

social control. As I read his "Typology of World-Systems,"³⁰ those societies which lack overt polities depend for authority structure on kinship systems. Although he mentions age-groups, he seems to treat them solely as biological phenomena. Yet they are in fact important social institutions among most tribal peoples, particularly (and ironically) those of East Africa. For many of these peoples — such as the Sidamo of Ethiopia, the Nandi of Kenya, and the Jie of Uganda — age-groups outweigh kin-groups in power and influence.

Looking back over my remarks thus far, I realize that I may be handicapped by my failure to have read earlier work by world-systematists. The term "world-system" itself leads me almost reflexively to ask, "Whose world? Which system?" Even if we grant that the word 'world' is almost intrinsically imprecise, it seems that we should be able to define the word 'system'. Is it any bounded aggregation, as the phrase 'solar system' suggests? Is it inherently restricted to living aggregates, as the biological sources of Ludwig von Bertalanffy's General Systems Theory imply? Why is it preferentially applied to civil, rather than to precivil, societies? Is it only because Immanuel Wallerstein is a political sociologist and, because of the absence of polities from foraging societies, consequently uninterested in preurban communities? My fellow contributors may well be able to answer these questions — if, indeed, they have not already done so.

To the extent that I still prefer 'comparative civilization' to world-systems theory. I do so for reasons that are more than theoretical. All world-systematists seem to me to write abstractly, preferring concept to percept and logic to experience. Their discourse is marked by polysyllables like 'substantivism', 'accumulationism', and 'continuationism'. What I miss in their publications is the often sensuous texture of actual cultures, both literate and preliterate, which I find in Spengler's *Decline of the West* — perhaps because of his intellectual apprenticeship to ethnologist Leo Frobenius.

Earlier I spoke of disciplinary, regional, and temporal parochialism, expressing my admiration of Frank, Chase-Dunn, and Hall for having avoided all three. To these three parochialisms, however, I added a fourth, ideological parochialism, which I feel that they have not avoided. For them, it seems to me, empires and trade-networks are the essence of world-systems. The resultant political and economic reductionism, as I see it, has led them to neglect artistic and symbolic aspects of civilization and the pervasive element of cultural style. Here again they appear to be at the opposite discursive pole from Spengler, whose sensitivity — perhaps oversensitivity — to stylistic factors led him idiosyncratically to refer to Western civilization as Faustian, Hellenic civilization as Apollinian, and Levantine civilization as Magian.

Many humanists, I believe, would concur in the preceding critique, seeing social scientists generally as too prone to focus on 'hard' political and economic facts at the expense of 'soft' verbal and iconic values. Few, however, would be likely to take the further step proposed by biochemist Rupert Sheldrake, author of the theory of "morphic resonance." Sheldrake's theory might be described as an extension of the theory of "stimulus diffusion" put forward by the anthropologist Alfred Kroeber. Kroeber's contribution to the debate between inventionists and diffusionists was the suggestion that some cultural coincidences — including even that of the Old World and New World pyramids — might be explained by oral transmission of ideas in the absence of migration, conquest, or trade.

Sheldrake, however, went considerably further. Starting with the embryological puzzle of morphogenesis and solving it (to his satisfaction) by postulating a "field of influence" surrounding the fertilized germ-plasm, he extended this notion to other domains, including human culture. In his paradigm, such familiar expressions as "winds of doctrine" and "climate of opinion" are to be taken almost literally, with the result that both concepts and images can be assumed to diffuse even in the absence of verbal contact.³¹ Although most of Sheldrake's scientific colleagues have dismissed his posited process as intrinsically impossible, some ingeniously controlled experiments support his hypothesis. If there is validity in it, morphic resonance must almost inevitably affect all system-formation and, consequently, all systems-theory.

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NOTES

1. The four sources on which I have relied primarily are Christopher Chase-Dunn and Thomas D. Hall, "Comparing World-Systems: Concepts and Working Hypotheses," *Social Forces*, 71, 4, June 1993; Thomas D. Hall and Christopher Chase-Dunn, "The World-Systems Perspective and Archaeology: Forward into the Past," *The Journal of Archaeological Research*, 1, 2, 1993; André Gunder Frank, "Latin America at the Margin of World-System History," *Comparative Civilizations Review*, 28, Spring 1993; and André Gunder Frank, "Bronze Age World-System Cycles," *Current Anthropology*, 34, 3, August-October, 1993. Two of the four are especially relevant to my interest in prehistory.
2. Bertrand Russell, *A History of Western Philosophy*, Simon and Schuster, New York, 1945, pp. 48-55, 68-71, and 288-90.
3. "The World-Systems Perspective and Archaeology" (hereafter WSP), p. 123.
4. "Bronze Age World-System Cycles" (hereafter BAC), pp. 392 and 396.
5. Catherine A. Callaghan, "An 'Indo-European' Type Paradigm in Proto-Eastern Miwok," *American Indian and Indo-European Studies*, Mouton, The Hague, Paris, and New York, 1980; and Otto J. von Sadvoszky, "The New Genetic Relationship and the Paleolinguistics of the Central California Indian Ceremonial Houses," *The LACUS Forum*, 1983, 516-30. Here it should be noted that Callaghan regards the pronominal suffixes of Miwok as only seemingly Indo-European; whereas Sadvoszky treats the Ob-Ugrian lexemes of California Penutian as Uralic cognates rather than, as I do, only as loan-words.
6. Frank, BAC, pp. 397 and 399. Roger W. Wescott, "History, Proto-History, and the Search for Synchronisms," *Catastrophism and Ancient History*, January 1990.
8. Gunnar Heinsohn and Heribert Illig, *Wann Lebten die Pharaonen? Scarabaeus bie Eichborn*, Frankfurt-am-Main, passim.
9. Frank, BAC, p. 405.
10. Percy Knauth, *The Metalsmiths*, Time-Life Books, New York, 1974, p. 13.
11. Raymond A. Dart, Foreword to *The God-Kings and The Titans: The New World Ascendancy in Ancient Times* by James Bailey, St. Martin's Press, New York, 1973, p. 14.
12. Frank, BAC, p. 400.
13. Frank, BAC, pp. 383, 403, and 404.
14. Frank, BAC, p. 398.

15. Frank, BAC, p. 404.
16. Frank, BAC, p. 397.
17. CD & H, CWS, p. 853.
18. CD & H, CWS, p. 857.
19. CD & H, CWS, p. 858.
20. CD & H, CWS, p. 859.
21. CD & H, CWS, p. 861.
22. CD & H, CWS, p. 862.
23. CD & H, CWS, p. 871.
24. CD & H, CWS, p. 875.
25. Roger W. Wescott, "The Enumeration of Civilizations." *History and Theory*, 9, 1, 1970, pp. 70-72 and 79-85.
26. H & CD, WSP, p. 122.
27. H & CD, WSP, p. 126.
28. Roger W. Wescott, *ibid.*, pp. 76-77.
29. CD & H, CWS, p. 879, reference 30; Christopher Chase-Dunn et al., "A Very Small World System in Northern California: The Wintu and Their World System Neighbors," a presentation to the 57th annual meeting of the Society for American Archaeology, Pittsburgh, April 1992.
30. CD & H, CWS, p. 866.
31. Rupert Sheldrake, *The Presence of the Past: Morphic Resonance and the Habits of Nature*, Times Books, Random House, New York and Toronto, 1988.