

## BOOK REVIEWS

Burke, Edmund, III and Kenneth Pomeranz, eds. 2009. *The Environment and World History*. California World History Library, Vol. 9. Berkeley, California: University of California Press. 361 pages, ISBN-13 978-0-520-25687-3 Cloth (\$60.00), ISBN-13 978-0-520-25688-0 Paper (\$24.70).

Edited by two prominent world historians, this book covers different regional cases of state formation or state developmental pressures encouraging their own local, bad, environmental outcomes. The historical data is both material and ideological: it is mostly on state water management policy in different regions combined with how a state's social definition of its environment interacted with material environmental outcomes. Both the distant past and the present are treated in this same 'state developmentalist' model. That is their world historical contribution: to get away from Eurocentric historiographic concepts of epochs based on economics and to analyze instead common civilizational forms of political economy that have historically repeated the same environmental problems in the past or the present. Thus, the historical time periods of the chapters ranges from several thousand years (in the astonishingly good synthesis by Burke of multiple Middle Eastern river-canal agricultural systems created by ancient state developmentalism in this area) to only several hundred years (as in the case of European states' changes to the Rhine River).

Most analyses are single case studies of one region or one major state in a region dominating the environmental context (i.e., chapters on China or Russia), however, there are several comparative chapters like Burke's chapter on multiple state developmentalism in the same region of the ancient Middle East, another one by Michael Adas on comparative rice frontier expansions in Southeast Asia and the environmental effects it had on different delta regions, and somewhat in Sedrez's chapter on Latin America and the environmental contexts created by the policies of different states.

The book is divided into three sections. Part one is called "Overview" though it is really a world historical theory of the editors' own (discussed below), with which some authors in the book's chapters disagree. Part two, "Rivers, Regions, and Developmentalism," has regional or comparative regional studies about water, shockingly leaving out North America and the hegemonic United States despite its ability to materially and ideologically determine much of the world's environmental conditions in different regions for much of modern history after World War II. I think the editors intentionally left out North America/United States because to include it would go against another part of their theory (this part is not demonstrated and is unconvincing, extremist, and artificially dichotomized) that intra-regionality is more tangible and more important than political economic theories of cross-regional pressures in world history. Thus world-embracing theories of dependency theory or world-systems analysis via commodity chains that have defined modernity as an innately cross-regional phenomenon, built states internationally, and defined environmental historiography from the global level inward to the regional is the epistemological 'other' with which they disagree. I am sad to see such a harsh methodological dichotomy 'theorized' between global and regional historical processes of politics because such an excellent regional book can only be improved by including this 'glocal' aspect of regional history. However, the book suffers from its lack of review of this perspective in

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environmental history and typically a lack of addressing it in the regional chapters. Part three, entitled “Landscapes, Conquests, Communities, and the Politics of Knowledge,” deals with contention in social construction of the same regional environment for different uses by different interests, showing how selected definitions, ideologies, and certain interests can dominate the rules of definition of what the environment is to be used for—against other subaltern groups that lose their abilities to define their local environment for their own use.

As Pomeranz writes, the theme of the volume is “commitments to state-building, sedentarization, and intensifying the exploitation of resources [that] we have designated the developmentalist project.” (p. 7). “What then were the concrete manifestations of the developmentalist project? Perhaps the most basic is a continuing increase in incentives and pressures to expand economic production” as a common theme, whether the distant past or the present. Thus state projects of developmentalism or state formation from scratch are what most authors analyze regionally, both for their ideological claims and their material policy power interactively across widely different temporal eras as similar. “We make state making processes drive the global intensification of land use...the latter cannot simply be ascribed to capitalism, much less to an Enlightenment [European epistemology later exported worldwide] that emerged when these processes were already in full swing.” (p. 5) Ergo, shockingly, they edit entirely out any mention of inter-regional domination or mere inter-regional relation from the story of the world history of the environment!

This means they mischaracterize state formation in the modern world since much of state formation proceeded in the past several hundred years because of such world systemic dynamics instead of developing in autarkical isolation. Thus state formation is incapable of being understood without combing the local and international links. Further puzzling is that many other historians for generations have been aware of this interaction of regional and global politics in state formation. For instances of critiques of the editors’ main thesis, Canada’s ‘national’ sovereignty seemed required in the 1860s to justify bond issues being more sellable internationally for investment in a cross provincial network of railroads connected to international extraction regimes (reading any book by the pioneer historian of international raw material flows, Harold Innis). Second, state formation cannot be only a mere regional phenomenon since in oil rich areas of the world state formation and elaboration is keyed into the world system of oil commodity chains—particularly clear in Venezuela (reading Terry Lynn Karl’s *The Paradox of Plenty*) or in Saudi Arabia and Kuwait (both sponsored into existence by British or American oil company aids; reading Brown’s *Oil, God, and Gold: The Story of Aramco and the Saudi Kings* (1999)). Third, international coffee commodity prices and the material specialization of robusta coffee in mass market Brazil in the 1800s led to market specialization being possible for smaller territories globally in finer coffees. This encouraged state elaboration in Central America to arise in a particular time around coffee elites, simultaneously globally dependent upon conditions in the world system of coffee combined with the very different regional specifics: the local environment, previous land tenure relationships, and labor supply. It is impossible to understand Central American state formation without reference to the world, commodity, and price regimes in the world coffee markets (reading Robert G. Williams *States and Social Evolution: Coffee and the Rise of National Governments in Central America* (1994) [Editor’s note: see John M. Talbot’ review essay on coffee, *Journal of World-Systems Research* 16:2:291-301). Since oil and coffee are the first and third most profitable commodities in world trade (second is illegal drugs,

according to the United Nations), this is hardly a small issue in world environmental history if these states were built from the globalized ‘outside in’ instead of the regionalized ‘inside out’.

So the editors’ case is wrong in assuming a huge phenomenological difference where they claim that state formation is a regional phenomenon “opposed” to world system dynamics in more modern times. They seem unaware that the history of state formation in the past few hundred years has been a global phenomenon as well. Moreover, the ‘second generation’ of world-systems analysis and dependency theory have already integrated intra-regional and inter-regional processes into their analysis several decades ago. See Alvin So’s review in *Social Change and Development* (1990).

The editors have another conceit that environmental conflict is very modern and localized: “Whatever one calls it, [state] developmentalism and resistance to it [i.e., its externalities] frame much of the environmental history of the last several centuries.” However, this is another misleading intra-regional limitation on the history of environmentalism, as it ignores international networks of developmentalist pressures and revolts against them (like in Chiapas in Southern Mexico in opposition to international free trade zones); or ignoring that international war instead of local economic pressures is a major degradative force in ‘local regions’ (like in Afghanistan or Iraq with U.S.-led-NATO or U.S. military invasion and occupation for instance); or ignoring the internationalization of environmental movement networks (mentioned in history books that show environmental movements are innately internationalized and cross-cultural influences, seen in Guha’s *Environmentalism: A Global History* (1999) or in Hawken’s *Blessed Unrest* (2007)). Second, others have argued strongly against thinking that there is something exclusively modern about environmental protest. Sing Chew writes that state/social movement contention over environmental conditions is far older than “several centuries,” in his three-volume work of world environmental history starting with *World Ecological Degradation* (2001), which they should know about before thinking environmentalism is entirely a modern phenomenon. My *Ecological Revolution* (2009) came out the same month as their edited book, and I argue something similar to Chew. I cannot agree that environmental social movements are novel phenomena caused by such ‘modern’ scales of state economic intensification when worldwide environmentalist concerns are so deeply connected with the origins of ancient religious protest movements thousands of years ago or in the present.

Third, the editors claim to reject “periodization” strategies of world history though they really only reject periodizations of others. They dislike framing history in economically defined epochs or into Marxist theoretical modes of production, yet adopt their own economic periodizations [1] based on novel energy regimes making a presumed radical break from the past that potentially help us avoid ‘declensionist’ narratives, and [2] how recent state intensification of the economy of only the past several hundred years is responsible for mounting anti-systemic ‘pro-environmental’ pressures. Chew and I argue otherwise: that environmentalism and its cultural effects are a far older state-opposition phenomenon.

In short, they prefer to analyze state developmentalism as a common regional, existential issue worldwide without reference to global issues. “We treat European capitalism and science as culturally specific variants of patterns found much more broadly.” This view would have been justified if they had more detailed European case chapters on this revisionist topic of treating Marxism or capitalism in Amer-European society as a Foucauldian ‘power/discourse’ that has affected the European/American environment’s regional definitions, uses, or conditions. This would be interesting. Alas, they don’t do this very much except for analysis of the ‘capitalist

internationalization' of the Rhine River and its environmental effects, or where Weiner in his chapter argues how Marxist ideology contributed to ignoring of degraded conditions of the ex-Soviet Union, particularly in Maxim Gorki's writings. However, Weiner's chapter on Russia argued that Marxist discourse was not totally to blame for deep-seated Russian degradation since similar harsh state depredations and extractions on the people and environment extended back for a thousand years. What the book sorely misses is a chapter equally critical on how capitalist and state elites defined the U.S. regional environment to their benefit (or defined other people's regional environments worldwide for their region's benefit). However, they do not address the North American region or its regional relations to the rest of the world; nor do they address the Chinese state's modern and massive pull on cross-regional global transformation in African minerals and agricultural economies of states there in a novel 'Cold War' with U.S. influence in Africa (reading Engdahl's *Full Spectrum Dominance* (2009)), despite China being ranked as the number one consumer of materials worldwide for several years now.

Thus, there are many flaws to discuss. To summarize, first despite the book being a great overview of most world regional areas which I was overjoyed to see major world historians address, it has a very disjointed temporal coverage. Only Burke contributes to something that might be called world historical—with his 'longue duree' of the massive Middle East environment, analyzed from 1500 BCE to 2000 CE. Burke's chapter is worth the price of the book in his magisterial summary of much of his (and his mentor Marshall Hodgson's) lifelong work on the cultural heartland of Islam. Other chapters' temporal coverage covers mere 'modern times' of the past several hundred years. Second, mentioned above, the book has a strange lapse spatially by missing the major environmental regions of North America and Australia/New Zealand/Oceania—possibly because to include them makes them difficult to explain seriously without reference to cross-regional analysis. I found it annoying that there was nothing in the material section of the book on Latin American state formation that easily would have qualified the editors' thesis of intra-regionality dominance in state formation. Instead, Latin America is discussed only from the point of view of how historians have socially constructed environmental history there, instead of addressing material constructs there.

Third, this leads to how the book is methodologically flawed in two ways. First by its insistence to discuss only local regional pressures on environmental degradation and definition, yet the editors stretch what seems a biased sample of regional case studies into general principles dominating history simply by ignoring in their narratives much mention of cross-regional pressures in world environmental history. To the contrary, regional state formation and cross-regionality have been successfully integrated together in other historians' work. Therefore, the editors' insistence to place state formation as 'regional,' while cross-regionality is ignored as somehow opposed to state formation, is a false premise.

Thus the book's overview chapters read like an ideological argument framing intra-regional processes as 'the orthodox, real history' and mention of cross-regional ideas in world environmental history are framed as heretical. In the preface, the editors argue, "world history has done better at comparing regional-scale phenomena than at providing new narratives in which the global itself is the unit under consideration." This statement is offered without any scholarly review of the other epistemological views in world history where cross-regional dynamics are more important than internal regional ones, and it is offered without seeing how the division itself between the two levels has been successfully merged in other books above. What was shocking to me is that they fail to mention world systems theory even once, a huge flaw. Moreover, they

openly discusses dependency theory only to lambaste it—using these two theories I believe as an unvoiced foil without letting the reader openly know it is attempting a novel statement of world environmental history without them. Thus there is this tinny ideological tinge to some of the summary chapters or some regional chapters. Dependency theory and its senses that inter-regionality dominates world environmental history get mentioned only to dismiss them as “*narratives of dependency* (it’s just a ‘narrative’ you see), victimhood, and romanticism.” This is pretty hostile stuff from the two American editors, and even the authors from Latin America and India break rank with this view that inter-regional analysis in world environmental history is ‘wrongheaded’ or that ‘declensionist’ thinking makes no sense to their regions. On the contrary, the Latin American chapter says declensionism makes a great deal of sense there, even if this view has unfairly dominated the narrativization of the history of the region opposed to other environmental history topics equally interesting like urban environments or border environmental territories and the competition between Latin American states for them.

So the book becomes an unevenly selected series of autarkic histories of regional state developmentalism attempting to prove the mettle of such a perspective against cross-regional world environmental history. This is hard to justify when much of the book’s temporal discussion is occurring in the midst of the expansion of European empires, their cash crop plantations, and water control measures worldwide. In other words, the editors face a serious disconnect between their grand theoretical views of world history and their sometimes myopic topics discussed in their case studies. The chapter on India is perhaps the only chapter in the book that loudly and openly disagrees with the editors by providing the required synthesis between intra-regional and inter-regional dynamics in its discussion of how the Indian state developmental project’s definitions and strategies changed under the British Empire’s pressures in India. Previously, state developmentalism pressures were kinder on nomadic populations and non-integrated populations that had a detailed, functional, and useful place in a native Indian state system. However, the British Empire’s construction of the purposes of the environment in India was without a place for these issues even if the British were forced in some ways to adapt to them over time to make the Raj work.

In summary, I argue there is little ‘world’ historical about much of their empirical analysis except their theory chapters. A more accurate title of the book would have been “The Environment and Comparative Regional History, with Some World Historical Claims Drawn Exclusively from It.” Despite stating their interest in singular regions comparatively, their theoretical claim is otherwise: it is entirely about global theories of history where intra-regional issues matter far more than cross-regional or world systemic ones. This dichotomy damages their historiography. Mentioned above, other regional state formation and state degradation studies that integrate global contexts show this divide can be removed in global environmental history. Second, if they do have a grand world theory, it is quite modernist: they feel state developmentalism is seen in the past or the present of course though more modern states’ developmentalism has destroyed the “old biological regime” (of limited agricultural/energy production which Braudel or Malthus is seen by them to have described its ‘real’ dynamics). This has brought “new technologies and energy resources” that can potentially lead toward sustainability. “Thus we cannot assume that the developmentalist project invariably leads to environmental degradation,” they say. However, the book imparts the opposite lesson in its regional histories. If they wanted to argue differently in theory like William McNeill does, that there is currently “something new under the sun” (pro-environmental technological movements,

summarized in his book of the same name which they are fond of quoting), they should have provided data to the effect. However, they do not provide that data for such optimism in any of their regional studies. I would have loved to see a good review of many intra-regional material and ideological pressures for sustainability. However, this is missing despite being a crucial argument of the editors. Fourth, this is the other flaw methodologically, and it follows from the first methodological critique: the editors disagree with ‘declensionist’ narratives of environmental decline theoretically (perhaps because these are so closely related to dependency narratives), yet declension and decline are seen throughout their regional cases. It does not make sense that they miss this glaring fault, though they do.

In conclusion, what they could have done was to remove an intellectualized false dichotomy ‘between’ regional and dependency or world-systems views by discussing the expansion of one region worldwide, or the expansion of scale of this constructed regionality worldwide over time, or how world cross-regional interaction created modern state formation and environmental conditions in the first place (mentioned in the books above). Such are the books the editors ought to examine to reevaluate their pointless attempt to differentiate between regional and global processes. I encourage more world historians to tackle environmental issues. The attempt to combine sociological theory of environmental problems with fine-grained regional histories of state degradation is a good start because there has been an analytic separation between much environmental sociology and deep historical analysis and because there has been an analytic separation between the biophysical environment and the social sciences in general. Both divides have been crippling for understanding long-term processual political pressures that create environmental problems. However, after solving the above two divides, the editors’ insert a novel artificial divide between global and regional processes. World historians should take a more historical view of regionality by seeing a regional process as changing in scale over time typically toward a larger regional interaction. More time should be spent building bridges between sociology, history, and environmentalism. This book contributes toward it, though there are more bridges to build that are incomplete in this book.

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**Turchin, Peter, and Sergey A. Nefedov. 2009. *Secular Cycles*. Princeton: Princeton University Press. 362 pages, ISBN: 978-0691136967 Cloth (\$35).**

Biologist Stephen Jay Gould used to tell the story of how physicist Richard Feynman once boasted to him about an amazing new discovery. Feynman had discovered evolution. In 1961 Feynman had the incredible biological insight that the distribution of genetic traits in a population should change over time due to the survival of the fittest. Within a week he had worked out the basics of sexual selection: why peacocks have fancy feathers, etc. He even rediscovered

evolutionary incentives for monogamy in females and polygamy in males. Feynman's idea was certainly brilliant; the only problem was that it was a hundred years too late.

Turchin and Nefedov's cliometric tour de force is reminiscent of Feynman at his best: absolutely brilliant, somewhat quirky, sometimes amateurish, but always provocative. The authors take a neo-Malthusian approach to interpreting the historical development of large-scale agrarian societies including pre-modern England, France, and Russia as well as an extended discussion into ancient Rome. One is tempted to call their approach "neo-Goldstonian," since for Turchin and Nefedov the only historical sociologist is Jack Goldstone (with an occasional nod to Theda Skocpol and Randall Collins). Their idea, in a nutshell, is that the histories of agrarian societies were dominated by demographic booms and busts not just of the general population, but of elites as well. They follow Goldstone in calling this the "demographic-structural" theory of history.

In the demographic-structural approach, demography is not determinative in itself (as for Malthus) but must be examined in conjunction with social structure (as for Goldstone). The authors present a simple model of agrarian societies, which they define as those in which at least 50% of the population and more often over 80%-90% of the population engage in agriculture. They reduce agrarian social structure to two classes (elite and non-elite) and study how the relative demographic weight of each changes over time. They posit four demographic-structural phases:

- Expansion phase: population grows from nadir; elite numbers low;
- Stagflation phase: population is high; elite numbers rise;
- Crisis phase: population begins to decline due to Malthusian pressures; elite overpopulation leads to political conflict;
- Depression phase: population stagnates; elite numbers are decimated by war and economic collapse.

The key driver of capital – H "History" in their model – the elite history of politics and wars, kings and castles – is elite overpopulation. To put it bluntly, as long as there is plenty of empty land to develop (expansion phase) or plenty of peasants to exploit per member of the elite (stagflation phase), elites focus on living the good life. However, once the carrying capacity of the land is reached (crisis phase), intra-elite competition starts to tear apart the fabric of society. Interestingly, Turchin and Nefedov see the rise of elite education and commoner artisanship as signs of crisis: elites seek university degrees and consequent bureaucratic employment only when opportunities to exploit rural peasants are scarce, and commoners turn to craft production for elite markets only when there is no free land available to farm. I had never thought of museum-quality artifacts in this way, but their analysis makes complete sense.

Similarly provocative is the authors' observation that secular cycles of the kind they describe would occur much more rapidly in societies that are characterized by elite polygyny. They cite Ibn Khaldun in noting that elite numbers can grow much more rapidly in traditional Moslem societies than in traditional Christian ones due to the very large number of inheriting children produced by Moslem elites. The result is that medieval Moslem elites soaked up any excess social production almost immediately after a demographic collapse, whereas medieval Christian societies took many generations. Cycles thus had a much shorter time from trough to trough in Moslem agrarian societies than in Christian ones.

The main shortcoming of this book is its lack of sociological grounding. To be fair, the authors are not sociologists – Turchin is a biologist and Nefedov a historian – but their material is fundamentally sociological. After all, if sociology is not the science of history, then what is it? The authors excitedly conclude that they are "optimistic about the future of history as science" (p. 314) but for the rest of us history has existed as a science since the days of Emile Durkheim, Vilfredo Pareto, and Max Weber, not to mention Karl Marx. Moreover, many contemporary sociologists (besides Jack Goldstone) are working on long-term historical dynamics. It is odd that the authors do not so much as cite Janet Abu-Lughod, Chris Chase-Dunn, Gunder Frank, Thomas Hall, Stephen Sanderson, or Immanuel Wallerstein.

This lack of sociological grounding is surely due in part to the authors' own background, but poor editing must also take some of the blame. The authors write as scientists, taking a "just the facts, ma'am" approach to their subject that results in over 300 pages crammed with evidence – and very little else. For example, there is no preface. There is no explanation of why they wrote the book, why the topic is important, or how the authors' arguments fit into larger theoretical debates. There is not even a brief "about the authors" blurb. The text abounds with statements like "Here are some examples:" and "Here are some facts:" followed by long lists of data. These kinds of stylistic problems should have been addressed in the editorial process; it is odd to see a book from a major university press that is so poorly edited.

That said the intellectual content of the book is staggeringly impressive. It is hard to quarrel with Turchin and Nefedov's careful analyses, and their data sources are extensively documented. For anyone interested in applying social theory to historical data on pre-modern Europe, *Secular Cycles* will be a treasure trove of data from obscure sources; the authors have certainly done their homework. Turchin and Nefedov's demographic-structural model also has the potential to spark several Ph.D. theses applying it to societies other than the four studied here. In short, this is a solid and persuasive work, a true scientific monograph. It is certainly not easy going, but highly-motivated scholars will find it extraordinarily rewarding reading.

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Beckwith, Christopher I. 2009. *Empires of the Silk Road: A History of Central Eurasia from the Bronze Age to the Present*. Princeton: Princeton University Press. 472 Pages. ISBN 978-0-691-13589-2 Cloth (\$35).

*Acknowledging the Other*

Over world history, Central Eurasia has often been viewed as providing the conduit (the Silk Road) for trade to flow between the East and the West, and on different occasions, it has impacted on the social, political, economic relations in the East and West with the migrations/invasions of its nomadic tribes. That has been our understanding of world history as told to us by most historical studies of Central Eurasia. Christopher Beckwith's *Empires of the*

*Silk Road* seeks to provide us with a different view of this commonly accepted understanding of the role and function of Central Eurasia. Instead of placing Central Eurasia in its peripheral role, and hence part of the periphery of the world system, Beckwith proposes that Central Eurasia has played a key role in the making of the modern world. The book traced the history of Central Eurasia from the earliest times to the modern period documenting the dynamics of the relations of this region to what Beckwith has defined as the ‘peripheral civilizations’ such as China, India, and Europe. In an effort to break down the myth of Central Eurasia, Central Eurasians of the past according to Beckwith were not only nomads but as well agriculturalists, urbanists, traders, and merchants. Instead of our orthodox impression of these Central Eurasian tribes as invaders and pursuers of luxuries and other goods that they obtained through raids and invasions, Beckwith wants us to view them as tribal groups that charged out of Central Eurasia because their trading interests were restricted during certain periods of world history as a consequence of the geopolitical policies and dynamics of the peripheral civilizations such as China, etc. In short, to Beckwith, Central Eurasians of the past with its tribes and states were no different in terms of economic and political interests than their counterparts (peripheral civilizations) existing on the fringes of Central Eurasia. What this means is that these ‘barbarians’ as most historical accounts have pejoratively defined Central Eurasians, went to war because of trade restrictions like everyone else in the world system and not for lust.

Beckwith’s explanation for the dynamics of the political economy of Central Eurasia over world history rests mainly on a cultural explanation. For him, the organization of the Central Eurasian political economy was around what he has termed: the Central Eurasian Culture Complex. The Complex is the basic social organization of tribal groups in Central Eurasia. The social relationship is that of the lord/ruler surrounded by his comitatus warriors whereby loyalty and fealty are rewarded with gifts from the lord/ruler to his followers. According to Beckwith, this social organizational feature generated a powerful economic need on the ruler/lord’s part to engage in trade so that this basic exchange relationship can be maintained and consolidated. To such an extent, Beckwith even extended this explanation to suggest that the economic engine of the Silk Road was based on internal demand of Central Eurasians for the products of its own people, and that of the peripheral states neighboring Central Eurasia. Such is the crux of Beckwith’s thesis and argument, and the book is replete with a detailed historical exposition of Central Eurasia’s tribes, states, and empires. It is rather refreshing to read another revisionist’s view of world history, and especially one covering a region which has not had much historical accounting. Unfortunately, Beckwith’s history and its explanation for the historical processes, structures, and events remain limited as his narrative is guided by his penchant emphasis on the cultural dimension to explain the trajectory of the evolution of Central Eurasia in world history. Beckwith does not offer any other factors that might also provide plausible explanations for the dynamics of the political economy of Central Eurasia. In an era when the environmental factor has returned to be a useful accounting of socioeconomic and political evolution, Beckwith’s book is quite silent on this dimension. There is no deliberation on whether this factor had conditioned the socioeconomic and political trajectory of Central Eurasia in world history. As recent studies have shown, environmental degradation and climate changes have impacted on the landscape of Central Eurasia, and consequently on the economics and politics of Central Eurasia over world history.

Notwithstanding the above, it is very clear from Beckwith’s accounting that Central Eurasia participated in the system-wide process of capital accumulation in world history. If this is

the case, the dynamics of the world system such as hegemonic shifts and long cycles of economic expansion and contraction must have also shaped the political economy of this region. Beckwith does not pay much attention to these system-wide processes. Other than a few references to economic conditions at the system-wide level, his discussion of Central Eurasian historical structures and events overwhelmingly focus on a descriptive elaboration of conflicts and wars that Central Eurasian polities engaged in, instead of placing such events within long-term cyclical economic considerations.

Despite the above reservations, the book does have some insightful sections examining the spread of science and technology and how Central Eurasia has played a major role in the development of science and technology, the changing importance of land and maritime trade routes in global trade patterns between the rising West and the declining East, the critique of modernity/modernism and the implications it had for the underdevelopment of Central Eurasia in the modern era by the former 'Marxist' Soviet Union and Maoist People's Republic of China, and the orthodox misunderstanding of the Other (Central Eurasia) through the pejorative labeling of the peoples of these lands as 'barbarians'. No doubt, some will question Beckwith's interpretation of historical development in the modern era as it relates to Central Eurasia, especially to what Modernity has done to the underdevelopment of Central Eurasia via the policies of the former Soviet Union and Maoist China. Beckwith's reliance on the cultural/ideological dimension as a propelling force of social change has led him to such an explanatory account with which others might disagree. But at least he tries. This ambitious attempt to provide a broad accounting of the social, political, and economic changes of Central Eurasia over the course of world history to the present has to be commended.

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**Bedford, Kate. 2009. *Developing Partnerships: Gender, Sexuality and the Reformed World Bank*. Minneapolis: University of Minnesota Press. 292 pages, ISBN 978-0-8166-6540-2 Paper, ISBN 978-0-8166-6539-6 Cloth.**

Kate Bedford's *Developing Partnerships* chronicles the shift in global governance from the coercive imposition of neoliberal capitalism on the developing world through structural adjustment programs to a more consensual and cultural means of ensuring neoliberalism's global hegemony. Through a careful analysis of the World Bank's Gender and Development (GAD) projects in Latin America and the Caribbean in the "post-Washington Consensus" era, Bedford illustrates the primary role that gender and sexuality play in embedding neoliberal logic into the cultural fabric of everyday lives in the global south. She demonstrates that it is through the promotion of heteronormative partnerships that the World Bank attempts to stabilize economic markets. As such, the book makes a compelling argument that links the promotion of normative intimacy with shifts in the global political economy.

In response to world-wide critiques and protests against the impact of structural adjustment on the economies and livelihoods of people in the global south, the World Bank

changed its development strategies in the 1990s to focus more on good governance, civil society, social safety nets, poverty eradication, and equity. These new strategies of governance are much more difficult to contest and have been adopted as the “new common sense of our times” (p. xx). The World Bank’s GAD projects essentialize gender binaries, suggest that liberalization naturally leads to a more equitable household division of labor, and promote normative families as the “ultimate marker of good development” (p. 200). Bedford seeks to trouble and disrupt the hegemony of the gender and sexuality regimes being promoted through this post-Washington Consensus.

Bedford argues that in its 1990s transition, the World Bank paid heed to the specifically gendered effects of its 1980s neoliberal policies, namely the crisis of social reproduction produced by the feminization of the labor market, which both overburdened women and led to a “crisis of masculinity” amongst poor men in the global South who, the argument goes, were increasingly driven toward violence and alcoholism with the loss of their identities as breadwinners. The Bank seeks to resolve the tension between unpaid and paid labor through an emphasis on loving partnerships and strong family values. Under this new model, women are encouraged to enter the work force as a means of achieving autonomy and empowerment, whereas men are encouraged to take up the unpaid social reproductive labor women no longer have time to do. In this way, the family becomes a key mechanism for providing the social safety nets seen as crucial for poverty alleviation, and relieves the state of responsibility, thereby ensuring the continued privatization of social reproduction. Drawing on Nikolas Rose’s critique of contemporary neoliberalism (*Powers of Freedom*, 1999, Cambridge), Bedford suggests that the Bank’s GAD policies target women for entrepreneurialization and men for responsabilization – in order to turn them both into citizens who will govern themselves in the wake of the state’s retreat.

Bedford argues that these new World Bank policies are deeply problematic because they are based on unjust characterizations of both poor men and women living in developing countries and because they sideline the important issue of child-care. Let me take each critique in turn. First, assumptions that poor, “Third World” men “act out their wounded masculinity through violence and drunkenness ... echo colonial assumptions about lazy natives and barbaric brown men” (p. 31). In addition, the many unemployed men who contribute to unpaid household labor and the many absent husbands and fathers who migrate in search of work and contribute to the household income through remittances are rendered invisible by the Bank’s assumptions of masculinity in crisis. “Making groups legible requires turning those who do not fit the frame into pathologies or anomalies that need to be transformed” (p. 85). In the chapter detailing the ethnodevelopment loan given to rural indigenous and Afro-Ecuadorian communities in Ecuador, Bedford illustrates the ways in which the Bank’s gender development projects often promote and sustain racialized hierarchies. In addition, these projects make distinctions between appropriate and inappropriate performances of indigeneity based on the extent to which communities are willing to (or are perceived as willing to) assume the ideals of sharing and monogamous partnerships.

The second primary critique Bedford makes of the Bank’s GAD projects is in reference to the promotion of women’s employment as a means of achieving empowerment. In the chapter focusing on Ecuador’s flower industry, Bedford uncovers a fundamental contradiction in the Bank’s policy on women’s employment: “on the one hand regarding women as self-interested autonomous actors enabled through wages to pursue their own erotic destinies, and on the other regarding them as necessarily attached, by enduring love, to specific others with whom they will

altruistically share their income” (p. 109). And in the chapter mentioned above on the Ecuadorian ethnodevelopment loan, Bedford shows how the Bank’s desire to prove that women’s employment leads to empowerment requires Bank employees to ignore the fact that many indigenous women were already working before the loans were introduced (p. 142).

Finally, Bedford points out that the Bank’s new GAD projects sideline the question of child-care. Although the provision of child-care seems the most logical policy response to the crisis of social reproduction (and one which women in developing countries overwhelmingly called for), such an initiative could not be supported by the Bank because it would require governmental intervention and social spending. Husbands become the only agents available to relieve women of their overburdened double duty because they do not cost the state a penny. In addition, engaging in housework and child-care is supposed to make men more “responsible,” thus shielding them from their proclivities toward alcoholism and violence. As such, the World Bank “hails gender balance to intensify the privatization of social reproduction” (p. 203).

With all of these important critiques, it is somewhat surprising that Bedford overlooks another crucial point. While the Bank congratulates itself for incorporating feminist critiques into its post-Washington Consensus gender and development projects, these policies remain grounded in a patriarchal logic which undervalues women’s labor. The Bank accepts the assumption that women deserve to be paid less than men and that the low-wage jobs being created in many developing countries (such as service work, call centers, or sweat shops) *should* be “women’s work.” Bedford could have done more to demonstrate that the feminization of the labor market has occurred, in part, because jobs stigmatized as feminine are underpaid and insecure.

Bedford’s methodological approach combines a discourse analysis of speeches and World Bank reports, interviews with World Bank staff at regional and local levels, and case studies of particular World Bank initiatives in Ecuador and Argentina. As such, she engages in what David Mosse has termed an “ethnography of policy and practice” *Cultivating Development*, 2004, Pluto) examining institutional bureaucracies and practices, the knowledge-production process, staff interactions with and interpretations of policy, and the ways in which these policies are both enacted and contested. Also important is her attention to the World Bank as a knowledge regime. In the second chapter, Bedford shows that the research that goes into World Bank reports and policy documents is often heterogeneous and complex, but “as research travels up the internal hierarchy of texts ... this nuance tends to get lost, as the findings that resonate with certain framings of the gender-policy problem become increasingly prominent” (p. 60). Given an object of analysis as monolithic as the World Bank, Bedford proceeds with great skill and attentiveness, paying heed to the “contingencies” and “frailties” of policy, while simultaneously unveiling hidden assumptions and the operations of hegemony. Such a rich, careful, and multi-layered methodological approach is worthy of emulation and significant in its findings.

In the conclusion, Bedford admits that she has attempted to combine a glass-half-full reading of Bank policies that acknowledges the important ways in which feminists have had a hand in transforming the Bank’s development approach as well as the creative ways that people have used Bank loans to promote feminist projects with a glass-half-empty critique that “pays attention to the dangers of the new common sense we are naturalizing about gender, sexuality, class and race” (p. 213). While I appreciate the difficulties that arise from trying to maintain such a tension throughout the book, there were several points in Bedford’s analysis that called for a harsher critique of the *economic* impact of the Bank’s policies. The maddening contradiction that really characterizes the so-called “reformed” World Bank is that while paying lip service to

poverty alleviation and equity, its bottom line is still the protection of free market liberalism which promotes corporate profit through the super-exploitation of the poor and the steady eradication of all social welfare. The subjectivities forged by the post-Washington Consensus model of development which insist on both entrepreneurial and responsible citizenry allow for the displacement of blame for poverty alleviation. Subjects of this new neoliberal regime are required to assume certain disciplined behaviors in exchange for social safety nets and the price to pay for failure is destitution and neglect. In addition, the Bank's new attempts to respond to the crisis of social reproduction very deftly deflect attention away from the fact that few new jobs have been created and existing jobs are both precarious and poorly remunerated. In fact, I would argue that Bedford's lack of attention to the Bank's primary economic motivation serves to naturalize the hegemony of neoliberalism, as if there really are no (longer) any alternatives.

That being said, Kate Bedford's argument that neoliberalism's global hegemony cannot be fully understood without a lens focused specifically on gender and sexuality is an essential contribution to the literature on international development, globalization, and neoliberal macroeconomics.

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**Cervantes-Rodriguez, Margarita, Ramón Grosfoguel, and Eric Mielants, eds. 2009. *Caribbean Migration to Western Europe and the United States: Essays on Incorporation, Identity, and Citizenship*. Philadelphia: Temple University. 261 pages, ISBN 978-1-59213-954-5 Cloth (\$59.50).**

Midst a flurry of recent scholarly and journalistic accounts of increasing immigration to industrial, capitalist democracies, *Caribbean Migration to Western Europe and the United States* offers a paradigmatic approach to understanding the complexities of immigrant integration and identity. The majority of recent comparative research tends to focus on European and American political responses to immigration rates or the impact of incorporation regimes or other national-level policies on immigrants' economic or cultural integration. While these remain important topics of inquiry, researchers privilege either variation in national policies or variation in the human capital of particular immigrant groups when explaining integration success or failure. Moreover, most studies lump together foreign-born populations when explaining native-born population's attitudinal or behavioral response to immigration. *Caribbean Migration* takes a different approach. In this edited volume, Margarita Cervantes-Rodríguez, Ramón Grosfoguel, and Eric Mielants bring together research that considers the legacies of colonialism on immigrant integration and identity. Ultimately, they provide an analytically refreshing and unique approach to understanding immigrant experiences.

In their introduction to the book, Cervantes-Rodríguez, Grosfoguel, and Mielants make a convincing case for applying the coloniality of power argument to the transnational experiences of Caribbean emigrants. Cervantes-Rodríguez and her colleagues make two important points. First, they argue that methodological nationalism has, ironically, imposed boundaries on

processes that are inherently peripatetic. Second, this has particular implications for migrants from the periphery to the core. Indeed, for centuries, Europeans conquered and colonized the Caribbean, creating a lasting legacy of unequal power-relations among nation-states. Since the post-World War II economic boom, millions of people from these non-independent territories and independent former colonies have migrated to the United States, Netherlands, United Kingdom, and France. Much of this migration is facilitated by the fact that citizens of these islands are also citizens of the Western country that colonized it. Thus, these migrants' experiences, by definition, can be described as transnational because their daily activities and identities are influenced by multiple social fields (Glick Schiller, pg. 29). More importantly, however, the historical relationships between these locales significantly affect migrants' economic, social, and cultural experiences.

The book is organized into three parts, which vary in their scope, empirical evidence, and theoretical contribution. The first section, titled "State Policies and Migrants' Strategies," is the most successful of the three in terms of advancing the coloniality of power argument. Through three separate case analyses, Michel Giraud, Eric Mielants, and Monique Milia-Marie-Luc demonstrate quite effectively the commonality of Caribbean migrants' experiences—regardless of country of origin or destination. This is no small feat. By focusing on the economic and cultural integration of Caribbean immigrants in France, the Netherlands, and the United States, Giraud, Mielants, and Milia-Marie-Luc elucidate the impact of colonialism on native-born attitudes and the incorporation of these populations into European and U.S. societies. One of the most interesting and important insights is that, regardless of citizenship, integration is hindered by colonial legacies of racism. Immigrants from French Guiana, Suriname, or Puerto Rico are legally French, Dutch, or American; yet citizenship does not translate into increased opportunities or likelihood of economic success for these populations. In fact, these migrant groups tend to fare worse than foreign-born from countries without a shared colonial past. Modernization theory and other cultural explanations that look to migrants' human capital in order to understand immigrant integration have missed an important pattern here (Mielants, pg. 67). Giraud, Mielants, Milia-Marie-Luc cite research that shows that human capital cannot fully explain the low status of these immigrant groups. Indeed, racism and discrimination are the common denominators for these immigrants in spite of citizenship and regardless of origin. Native-born populations perceive immigrants from the Caribbean "as 'not completely belonging,' although they are not, strictly speaking, foreigners" (Giraud, pg. 45). Colonization, paternalism, and the historical construction of race prove significant obstacles to integration.

The next section, "Identities, Countercultures, and Ethnic Resilience," provides less evidence in support of the coloniality of power argument and focuses instead on both the common themes and case-specific nuances of transnational cultural processes. Lisa Maya Knauer addresses the ways in which immigrants use cultural practices and social spaces to connect with their "roots" and engage in bicultural identity work. Immigrants engaged in Afro-Cuban culture in New York are able to transcend geographic boundaries and experience "long-distance nationalism;" further, the existence of non-Cuban participants in cultural practices such as the rumba highlight the fact that non-migrants are part of these transnational social fields as well. Livia Sansone, also finding that non-migrants participate in the making of a "new black culture" in the Netherlands, provides supportive evidence of the effect of migration on the cultural practices in Western metropolitan centers. Elizabeth Aranda finds that transnational social spaces facilitate hybrid, or bicultural identity formation. Moreover, according to Aranda, global cities

such as Miami provide ethnic minorities a space to attain cultural citizenship, thereby reducing the stigma associated with being both Latino and Puerto Rican. These essays, although they focus on the idiosyncratic cultural experiences of different immigrant groups, call attention to general processes nonetheless.

The last section of the book is the least theoretically or empirically cohesive. In order to assess whether or not there is a common Latino experience, John R. Logan and Wequen Zhang document the social and economic characteristic of Hispanic groups in the United States as well as the demographic and socioeconomic make-up of immigrant neighborhoods in Miami and New York. They find more differences than similarities across populations and conclude that there is no “average Hispanic.” Laura Oso Casas investigates the social-mobility strategies of female Dominican heads of household in Spain. Although her rich interview data reveals much about this particular group’s experiences, unlike in the previous two sections of the book, there is no other chapter that offers evidence of similar phenomena in other destination countries. This does not mean that the occupational, martial, or educational strategies of Caribbean migrants in other countries differ, but the reader cannot generalize on the basis of Oso Casas’ findings. Finally, Mary Chamberlain explores the personal narratives of migrant families in order to provide insight into the Caribbean transnational family culture and complex network of familial relationships. According to Chamberlain, migration does not break familial ties; in fact, Caribbean migrants show greater reliance on extended kin networks, which include “those distanced genealogically, generationally, or geographically” (Chamberlain, pg. 236). Interview data indicates that this is not only an important strategy but also a point of pride for Caribbean migrants dispersed around the world.

*Caribbean Migration to Western Europe and the United States* offers a compelling look at the transnational experiences of migrants from former colonies and non-independent territories. For the most part the enterprise is quite successful. There are two weaknesses to the project, however. First, the tension between the book’s paradigmatic approach and its case-study methodology is not fully resolved. When taken together, these essays do a good job demonstrating that, due to the historical legacy of colonialism, migrants from the Caribbean have comparable experiences regardless of country of origin or destination; yet some sections and particular essays, which compare multiple migrant populations, do a better job than others. Second, the volume’s theoretical paradigm actually includes two theoretical perspectives: transnationalism and the coloniality of power argument. These approaches are related but also advance different agendas. Further, the coloniality of power argument is theoretically parsimonious, while a focus on transnational processes uncovers idiosyncrasies of particular migrant groups in particular countries. Nevertheless, the book contributes much to the literature, and its essays should be of interest to scholars of immigration across the social sciences.

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Yang, Bin. 2009. *Between Winds and Clouds: The Making of Yunnan Second Century BCE to Twentieth Century CE*. New York: Columbia University Press. 338 Pages, ISBN 9780231142540 Cloth (\$60.00), ISBN 9780231512305 Gutenberg-e, <http://www.gutenberg-e.org/yang/index.html>, copyright 2008 on this version.

The fundamental argument of *Between Winds and Clouds* is that two over millennia of the history of Yunnan and most of Southwest China cannot be understood solely from a Chinese perspective. Nor is it solely a matter of a history of how China incorporated and changed Yunnan. Rather, this history must be understood in a global perspective. Furthermore Yunnan had significant impacts of the development of China, especially its multicultural qualities. Yang further argues that world-system analyses of incorporation are very useful in understanding this history, and notes in several places where the history of the incorporation of Yunnan can add to continuing development of world-systems analysis. This is a complex, yet subtle and nuanced argument, developed over seven substantive chapters. For readers coming from comparative political economy it is useful to note that the style of discourse is that of world history which draws on a large number of primary sources. For those who can read Chinese there is a glossary of romanized terms.

The introductory chapter summarizes the key arguments and gives a general background on the region. Yunnan is an ethnocentric Chinese construct which means approximately the land south of the clouds. The region had at least 25 different ethnic groups which outnumbered Han Chinese immigrants until just a few centuries ago. Yunnan's external connections to Southeast Asia – often known as the lands below the winds – were often as strong as or stronger than its connection to China. Hence, the book's title and a major reason why the region must be studied comparatively as part of a larger world-system.

Yang argues, "Studying how frontiers became peripheral areas in the first place will shed light on the theoretical development of world-system perspectives" (Ch. 1, para 54; p. 15 cloth). Indeed, one can read the entire book as an elaborate exploration of this argument. For traditional scholars of Chinese history an equally important thesis is that no part of China can be studied in isolation – either from the rest of China, or the rest of the world. Because Yunnan had such connections for over two millennia many nuances of creation and modification of a frontier can be examined. An important point is that Yunnan connected to what we now know as Tibet, India, and Southeast Asia via a Southern Silk Road which complemented and supplemented the northern overland Silk Road and the more southerly maritime Silk Road.

The second chapter focuses on the history of the Southern Silk Road. Ironically, silk was not a major trade item along the Southern Silk Road. Goods moved from Yunnan in all directions, thus Yunnan was a major source of information to China about Southeast Asia, especially what we know today as Burma (aka Myanmar). Two products in particular proved very valuable to China: horses and copper. Yunnan is mountainous and highly varied region, one source of its ethnic diversity. It has extensive mineral resources. Because of the altitude of much of the region it was relatively easy to breed horses. Thus, it became an alternative to the northern steppe pastoralists as a source of horses. This capacity was very valuable to China proper which could not breed horses in large numbers due to climatic conditions. The region was finally brought into the Chinese fold via the Mongol conquests in the thirteenth century.

The following chapter examines the many military encounters between Yunnan, or its sub-regions and outsiders, primarily China. Until its conquest, ironically by the Mongols, various

local kingdoms were able to use geographic, climatic, and geopolitical conditions to maintain a degree of autonomy and to negotiate, often favorably, the degree and type of incorporation the region experienced. In lowland areas presence of diseases not common in the north killed many invaders. Mountainous regions presented difficult terrain and at times extremes of cold. Early Han attempts to conquer the region (during the second and first centuries BCE) met with minimal success and were finally abandoned because of the much more serious threats from the Xiongnu in the north. Once the northern frontier was pacified, there were further Han forays into Yunnan, but serious attempts at conquest did not begin again until the third century during the Warring States period. Later one local kingdom, Nanzhoa, was able to play Tang China and Tubo (Tibet) against each other to maintain some autonomy. Early in the tenth century all three kingdoms collapsed. Yang uses this complex history to argue forcefully these interactions formed a regional world-system. This means, among other things, that traditional “national” histories are all misleading. Later interactions with the Song were tempered by Song Dynasty’s dire need for horses.

Subsequent Mongol conquest was also relatively mild. The Mongols were not interested in territorial expansion per se but in trade and access to trade routes. As long as those goals were met, local rule prevailed. The following Ming Dynasty sought stronger conquest of the region so as not to be subject to the dependency that the Southern Song had experienced in their need of horses. The Ming sought “to make Yunnan a permanent part of China” (Ch. 3, para 77; p. 95 cloth).

The next chapter explores why and how rule of the region was primarily indirect through local, native or indigenous, leaders. Yang argues that “Sinicization and indigenization were two sides of the process through which a middle ground was negotiated” (Ch. 4, para 1; p. 102 cloth). Chinese began rule of frontier peoples based on native customs, but with the intentions of “civilizing” (sinicizing) them eventually. This took approximately five centuries in Yunnan hence it is an excellent locale to study these process. Throughout there was a balancing of a need to keep the frontier stable, to continue sinicization, and to use indigenous peoples to aid in frontier defense. While the aim was clear, the execution often was messy or even unsuccessful.

The technique, begun under the Mongols, extended into the succeeding Ming Dynasty. It consisted of a centralized, province wide administration. Then local leaders (princes) were appointed to rule subregions. Third, local leaders (native chieftains) ruled in rural areas where indigenous populations dominated. This constituted a “tripod” system. Local leaders had to pay tribute and meet other obligations. However, these obligations did not preclude their continuing payments to other states in Southeast Asia. During the Ming and Qing Dynasties the tripod was modified by a *gaitu guiliu* policy that transformed native chieftains into a part of the imperial administration. Over time this reversed the domination of ethnicity over administration to ethnicity becoming a subdivision of administration. Gradually the sons of native chieftains were taken to Chinese schools in central provinces and were trained in Chinese language and administrative processes. When they returned home and came to power they were in effect agents of sinicization.

The succeeding chapter (5) is in many ways the heart of the book. Yang shows that sinicization and indigenization were sides of the same coin. They contributed to the emergence of Yunnanese as provincial identity and in turn became an avenue for the absorption of some Yunnanese practices in general Chinese identity and culture. After years of immigration, settling of soldiers, and movement of traders by the end of the Ming the Han became the largest ethnic

population in Yunnan. In short, this is a detailed description of the development of a hybrid society. Yang begins with an overview of the various ethnic groups, then reviews the waves of Chinese immigration into Yunnan. Sinicization grows through bureaucratic administration and education of sons of local indigenous leaders into Confucian practices (Yang notes that Confucian practices are not identical with Han culture which includes many other aspects). Indigenization refers to the introduction of many “barbarian” customs and goods into Chinese culture such as some types of clothes, dances, chairs, and so on. Local climatic conditions forced changes in introduced agricultural practices. The existence of extensive mining communities led to different types of urbanization. Also cowries (shell money) continued in use longer than any other part of China, the topic of the succeeding chapter. Gender imbalances in immigration led to extensive intermarriage. But intermarriage also gave traders and others better access into local networks. This seems to be the main path by which different sexual practices and sexual tools from Yunnan were introduced into Chinese culture. All of these changes contributed to the emergence of a distinctive Yunnanese identity. Yang traces the roots of the *minzu* system (officially recognized minority groups) which is still in operation today: “In essence, the incorporation of Yunnan helped build China as a multiethnic entity” (Ch. 5, para 144; p. 182 cloth). What is clear in the book, but not in this brief summary is Yang’s careful exploration and use of various concepts of ethnicity, and his detailed descriptions of how these changes actually worked.

The penultimate chapter is concerned with money, the redirection of the economy to China, and the increasing, if slow, shift from cowries to copper. Key to these changes was production of silver in Yunnan. By mid nineteenth century Han population outnumbered all non-Han peoples. Copper production reveals how Yunnan was connected to the Chinese world-economy and was incorporated into its administrative hierarchy.

Silver was so abundant that many Buddhist statues were made of it. By the end of the Ming Yunnan produced seventy-five per cent of China’s silver, a scale comparable to New World silver imports. Thus, China became partially dependent on frontier and peripheral production. Cowries were useful for local small scale trade, and used extensively in slave trade. They were useful along the southern Silk Road routes because no one state could implement a currency policy. While cowries began to disappear from China about two millennia ago, they remained important in Yunnan. Cowries disappeared for a while during Han times, but when Han authority waned they returned. As late as the Ming Dynasty cowries were used to pay taxes, and used by the Ming to pay local salaries, and even for large transactions, including donations to monasteries.

By mid seventeenth century copper and silver began to replace cowries. The conventional internal explanation is that as trade increased cowries became less valuable than copper and so fell out of use. The conventional external explanation is that trade with coastal areas was disrupted by arrival of Europeans in Southeast Asia which dried up the source of cowries. Others argued that European commercial capitalism increased the cost of cowries so that Yunnan could no longer afford them. Yang combines these somewhat contradictory explanations and ties long persistence of cowry use to southern Silk Road trade, and their subsequent disappearance was due expansion of the European world-system into Southeast Asia. All in all Yunnan remained a frontier connecting multiple civilizations – China, Southeast Asia, and South Asia. He further notes this is grist for the debates about whether there was one or many world-systems and how they connected and merged.

Immigration, as noted, was gradual but increased during the Qing Dynasty. Migration was possible because of opening of hilly areas, especially through use of New World crops, notably tomatoes and corn. Migration led to population growth from about five million in 1700 to about twenty million by mid nineteenth century and to increased urbanization and industrialization. Furthermore there is evidence that the urbanization and industrialization drove growth in agriculture. There also was a see-saw pattern between peripheral and core population growth. These shifts helped promote the creation of a Yunnanese identity that remained somewhat distinct within overall Chinese identity.

The industrialization was due primarily to the growth in copper mining. Yunnan and Japan were key sources of copper. Curtailment of Japanese copper supply heightened interest in Yunnanese copper. This led to a concentration of miners, something that Chinese administrations sought to avoid in other areas of China because such concentrations had been sources of unrest. However, they were necessary in Yunnan. As would be expected the intensity of copper mining and smelting took a severe toll on local ecology. Lack of readily available charcoal eventually slowed copper production. Yang uses this analysis to critique Skinner's macro-regional analysis of the area. He also notes that unlike some other frontiers, the Yunnan frontier had significant impacts the overall world-system through its various external links.

The final substantive chapter is a fascinating discussion of the formation of Chinese identity which uses the variations in Yunnan's identities to explore the development of Chinese multi-ethnic culture. A key shift is from "barbarian" to imperial subjects, to "younger brothers" in the larger Chinese ethnic "family." The creation and history of the *minzu* (officially recognized ethnic group) is examined from a Yunnan perspective. This is a nuanced discussion which tries to sort out the variations of Chinese Marxism, the drive for Han cultural hegemony, and responses to perceived world pressures to recognize minorities. Throughout these changes the capacity of Yunnan ethnic groups to maintain a modicum of autonomy, and the development of a larger Yunnanese identity played key roles in shaping these policies. It is a striking example of how frontier processes and policies can shape national policies.

A brief conclusion sums up the finding from previous chapters. Among many conclusions are questions about whether or not the Chinese movement into Yunnan, Russian expansion into Siberia, and European expansion into the Americas were part of some "global project" or not at all related. This is a topic worthy of further exploration. But this question could not even be asked without this detailed history of the Yunnan frontier. Another interesting contrast is movement into Xinjiang, which is relatively recent especially with respect of Han migration in contrast with the much longer processes in Yunnan. This difference in time depth is at least a partial explanation of why there is considerable unrest in Xinjiang and little or none in Yunnan. Finally, this study illustrates "how both local (Chinese and non-Chinese) and global factors have made China a multiethnic unity" (Ch. 8, para 24; p. 286 cloth). While the claim that China is a "multiethnic unity" will raise some controversy, Yang clearly demonstrates that whatever Chinese identity is today it was constructed over millennia of complex interactions between local peoples and multiple connections with the outside world.

As lengthy as this summary has been, it barely skims of the surface of many of the issues examined in considerable detail. Yang employs many archival resources and uses accounts from outside China to present a more rounded picture of events. One might argue, based on his discussions, that it is not so much that China has been or ever was isolated, but rather too many Chinese scholars, and even some western scholars of China have been blinded or misled by an

overly ethnocentric view of Chinese history. Along the way Yang points to many fascinating topics worthy of further exploration that no doubt will cause some rethinking and elaboration of world-systems analysis. Further, *Between Winds and Clouds* is an impressive demonstration of the utility of a world-systems analytic approach to a “regional” history. The book is a fascinating read, but not one for the feint hearted. It is a demanding text because the issues it examines are complex. Yang provides sufficient background for non-specialists in Chinese history, but the subject matter itself is difficult for a beginner. Overall, though, the presentation is quite readable. It would be a great case-study for any graduate course that employs world-systems analytic approaches.

A final note on *Between Winds and Clouds* as an electronic publication is useful. Only the electronic version includes the many useful maps. They are not included in the cloth version, nor is their placement even noted. The electronic version is available through Gutenberg-e [<http://www.gutenberg-e.org/>]. If a reader prints from the e-version, the chapters must be downloaded individually and the maps must be downloaded separately. Paragraphs in each chapter are clearly numbered. The cloth version does not have an index. However, the electronic version is searchable, noting every instance of the search term throughout the entire text. It is a bit irritating in that it reports only the sentence in which the term occurs but not the paragraph number. There is no concordance between the cloth version and electronic versions. These advantages and disadvantages are part and parcel of the transition from paper to electronic publication. Overall the e-version is well done.

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Hung, Ho-fung, ed. 2009. *China and the Transformation of Global Capitalism*. Baltimore: John Hopkins University Press. 244 Pages, ISBN 978-0801893070 Cloth (\$50.00), 978-0801893087 ISBN Paper (\$21.37).

Editor Hung has assembled essays on China’s post-Mao rise. They interpret that extraordinary event from perspectives loosely tied to world-systems analysis. This approach assumes that a 1500 economic breakthrough associated with the rise of Europe has created a stable capitalist international order of core, semiperiphery and periphery that can only be humanely transformed by a world socialist revolution.

Other world-systems analyses of the post-Mao-rise of China have described that event as stabilizing a neo-liberal world order. In contrast, these authors interpret the Chinese rise as more on the side of a world socialist transformation. They do so in fundamentally conflicting ways, producing what Hung labels a “cacophony” (p. 188). His concluding chapter, the only part of the book, besides the chapter by Alvin So, which evinces a command of the scholarly literature on China’s economic rise, takes issue with what Hung sees as overly optimistic trajectories imagined by the book’s other contributors.

First, Giovanni Arrighi treats the pre-modern rise of China as peaceful, non-capitalist marketization. He sees the post-WW II Bretton Woods era rise of East Asia under American

hegemony in similar terms. Hence, China's recent rise, in building on both of these earlier phenomena, continues that peaceful non-capitalist market orientation. For Arrighi, world-systems analysis misleads because it is Eurocentric.

To make his argument about a peaceful market versus a war-prone "Western" capitalism, Arrighi recapitulates the CCP official story on the extraordinary 15<sup>th</sup> century voyages of Admiral Zheng He, not mentioning that the Admiral traveled with 30,000 troops as part of a southern imperial Chinese expansion that Indonesian students are taught to see as invasion and aggression. Arrighi also repeats the CCP narrative on the Opium War as making China poor. In contrast, Deng Xiaoping, the political leader of post-Mao reform, blamed China's prior immiseration on the self-sequestration of the late Ming, a self-wounding 15<sup>th</sup> century (after the Zhu Di reign era) policy emulated by Mao Zedong, under whose policies, from 1957 to 1977, China stagnated.

Alvin So reasonably explains China's "Developmental Miracle" as similar to "the East Asian developmental state miracle" (p. 50) which he then contrasts with a supposed "Western neo-liberalism model," as if studies of the global varieties of capitalism do not highlight the social welfare state Rheinisch model of much of continental Europe as antithetical to neo-liberalism. In addition, So's description of "the Maoist state" providing "housing, healthcare, welfare, education, pension...based on need and free of charge to all citizens" (p. 52) ignores the 81 percent who were not on state payrolls.

Editor Hung cites data which contradict So's claim that China has "paid more attention to egalitarianism than its East Asian counterparts" (p. 60). In fact, inequality in China, Hung shows, is approaching Brazilian levels. In contrast to Arrighi's notion of a peaceful Chinese rise, So worries that "When China expands, it will inevitably run into conflict with other hegemonic states" (p. 62).

Richard Applebaum contends that a rising China, as other parts of East Asia, is undercutting the power of European and American retailers by using super-Fordist suppliers which invest heavily in research and product design, thereby creating "a dramatic shift of organizational power within global supply chains" (p. 67). Nike will fade. Yue Yuen will rise (p. 75). This Asian gain, however, is "devastating" for other developing countries.

József Boöröcz imagines China's rise as part of the post-Bretton Woods era success of emerging market economies, that is, India as well as China, with "industrial production...moving away from the [OECD] core areas" (p. 88). The engine of the world economy is now said to be Asia, with the 150 year era of the "West...a brief and relatively insignificant interlude" (p. 100), until recently a minority view in world-systems analysis. Boöröcz imagines "Eastern Europe and northern Eurasia" (p. 102) as the losers, with China and India joining in a "development partnership" which "could invite Russian" partnership.

Boöröcz projects a future "Asian Union" (p. 104). That scenario ignores both the interests of key Asian polities, Japan and Indonesia (ASEAN) and also the tensions and rivalry between China and India.

Paul Ciccantell discusses China's quest for resources which is raising global commodity prices. He knows that world-systems analysts typically describe core powers as stealing the resources of the periphery, constructing what some call a neo-colonial relationship. He argues that China, although doing the same, is different because a late developer pays more, develops mines, and builds transportation. But imperial Japan did the same. "China is following the Japanese model" (p. 117).

Ciccantell then argues that Chinese steel manufacturers are trying to dictate terms to Australian and Brazilian iron ore owners. Although he refers to “raw material peripheries” (p. 126), it is not obvious how his analysis is linked to cores and peripheries. Is Australia usually considered semiperiphery? Is Brazil China’s periphery?

What China is doing, as described by Ciccantell, is better understood in terms of international relations realism. Beijing seeks oil from “Russia, Iran, Sudan and Venezuela...to reduce dependence” on U.S. firms and U.S. navy-controlled sea routes (p. 122). However, the last figure I saw in a Chinese source found that 88 percent of the energy pumped overseas by Chinese enterprises was sold for profit on the world market and was not imported by China. Nonetheless, Ciccantell sees a Chinese attempt to steal raw material sources from America and Japan which could lead to war.

John Gulick claims that no nation can serve as a global economic hegemon in today’s complex world. While China, supposedly wrongfully accused of manipulating its currency to enhance exports, is said actually to be structurally tied to Wall Street and about to suffer greatly from the 2008 forward financial crunch (Hung agrees), America is said to be moving in a protectionist, racist and anti-foreign direction. As a result China will join with Japan to build pan-Asian institutions and free Japan, “a pliant vassal of U.S. imperialism” (p. 137), from subordination to America. China will also partner with Russia to defeat American hegemony. The result, however, will be China and Russia “accorded roles in the trans-national capitalist order” (p. 144). But Gulick then says the change will “constitute the greatest danger to a liberal international order” (p. 146). Gulick does not explain how.

The chapters by Stephanie Luce and Edna Bonacich and by Beverly Silver and Lu Zhan link China’s rise to a world socialist transformation. Luce and Bonacich accept that China manipulates its currency (p. 155). Their careful analysis of job losses caused by China’s rise finds that it is greater for Mexico than for the USA (p. 156). In general, the rise of China, and the dynamics of the post-Bretton Wood era reduce “workers’ power” (p. 157), contributing to “the race to the bottom” (p. 158). The question is, how should global labor respond?

Luce and Bonacich urge the workers of the world to unite. But the authors do not clarify why workers in emerging market economies to which production is being shifted should cooperate with labor in the OECD nations trying to protect jobs from being out-sourced. The goal of global labor standards imposed on the entire system, a policy which the authors embrace, is the position of the AFL-CIO (a group they damn) and is opposed by China, India *et al.* Luce and Bonacich hope that the CCP’s phony national labor union will join with OECD unions to restructure the IMF. Their hope for “gaining power for the working class as a whole in relation to transnational capital” (p. 170) is not based on an extrapolation of on-going dynamics.

Yet Silver and Zhang argue from the perspective of a “militant working class” in China (p. 175). Consequently, to them, “it is not far-fetched to conclude that...China is becoming the epicenter of world labor unrest” (p. 176). Wages therefore will rise in China and Americans will no longer be able to purchase cheap consumer goods. The neo-liberal order will end. “Catastrophic” consequences (p. 184) are possible unless all sectors of the world agree to “a more equal world order” (p. 185).

Hung, in his skepticism about the rise of China, seems suspicious about claims about fundamental global transformations. Assuming a stress on the forces that re-stabilize the world-system, perhaps one should expect a future where a Chinese bubble bursts (as with Japan in 1991

and Indonesia in 1997-98) or where America (or the OECD) and China (or emerging market economies) clash, producing a crash even worse than that of 1929.

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**Tonio Andrade. 2008. *How Taiwan Became Chinese: Dutch, Spanish, and Han Colonization in the Seventh Century*. New York: Columbia University Press. 300 Pages. ISBN 978-0-231-12855-1 Cloth, ISBN 978-0-231-50368-6 E-book.**

In *How Taiwan Became Chinese* (HTBC), Tonio Andrade revisits the history of Western mercantile powers' expansion in maritime Asia between the late sixteenth and seventeenth centuries. Andrade highlights the indispensable contribution of non-European actors in the rise and fall of the Dutch and Spanish colonial settlements in Taiwan (especially the former). These Asian actors—including the Han Chinese and Japanese traders as well as aboriginal people in Taiwan—were no longer Eric Wolf's "people without history" (Wolf 1982: *Europe and the People without History*). Based on findings in Dutch, Spanish and Chinese archives, Andrade's in-depth analysis of formations and liquidations of the Euro-Asian alliances shed light on Andre Gunder Frank's "horizontal integrative macrohistory" paradigm (Frank 1998: *ReOrient*).

Taiwan, or the island of Formosa, became a strategic trading spot in its maritime Asia. Only 80 miles off China's southeastern coast and its Fujian province, the island was close to China's silk supply outlet, but was not so close as to be regulated by Chinese maritime policies. The Ming Empire lifted its maritime ban in 1567 and licensed Chinese seagoing merchants for overseas trade. Many of them skirted state regulations in order to avoid custom tariffs and to trade more freely. To conduct their illicit trade, unregulated traders (referred to as "pirates" in Chinese documents) used Taiwan to trade with Japanese and Europeans merchants there, as well as the island's aborigines. After the Chinese forced the Dutch to abandon its fort in the Pescadores (the Penghu Islands), they settled in Taiwan's southwestern plain in 1624. Two years later, the Spanish, too, set up a foothold on the northern tip of the island (Introduction).

When the Dutch entered Taiwan, the existing population (i.e., its aboriginal people) in the island was "a hundred times larger than the Dutch Republic" (p. 2). The Dutch strategy was to ally with the most active Chinese trading group, the Zheng, to create a new political and economic order. The Zheng group had developed into the paramount intra-Asian trading network beyond official Chinese control. But the leader of the group, Zheng Zhilong, shifted his allegiance to the Chinese Ming Dynasty in 1628. With that shift, Chinese officials authorized Zheng Zhilong the sole right to eliminate the pirates. His group thus dominated Chinese coastal trade. At the same time, in response to the resentment of the Sinkanders (an aboriginal tribe in Southwest Taiwan), whose earlier trade with the Japanese was interrupted after the Dutch arrived, the Dutch prioritized this tribe over other aboriginals on the island. Through the destruction of the traditional inter-tribal rotation of powers, Pax Landica was established. (Ch. 1, 2, and 3).

If the aboriginal people's support was a key to the making of Pax Landica in Southwest Taiwan, the same factor also accounted for the failure of Spanish rule in North Taiwan (1626–1642). First, unlike the predominant hunting-and-gathering society in Southwest Taiwan, trade

and handicrafts constituted the main livelihood of aboriginal tribes in North Taiwan. The latter viewed the Spanish as an economic competitor but not a provider. Second, the success of the Catholic missionaries, while helping the Spanish penetrate the aborigines' lives, also motivated the Spanish to become involved in inter-tribal warfare. Eventually, in 1642, due to Dutch envy of North Taiwan's gold mines and their desire to blockade the Manila trade, the Dutch expelled the Spanish with the assistance of northern aboriginal tribes (Ch. 4 and 5).

After the 1630s, the Dutch recruited Chinese agrarian settlers to migrate from China to Taiwan. Andrade coins the term "co-colonization" to highlight the indispensable Chinese contribution. The Dutch granted Chinese settlers several economic privileges, including property rights to farm land, exclusive permits for commercial deer hunting, and the sole rights to the highest bidder to trade with aboriginal villages, among others. These Chinese settlers were obliged to contribute to production quotas and to allow the Dutch to monopolize the exportation of their products. To effectively manage the growing Chinese community, the *cabassa* system was created, in which the Dutch selected ten Chinese translators to collect tax and sell residential permits. With the Chinese as the cultivators, hunters, and commercial bidders, the aborigines viewed the Chinese as the real challengers. Conflicts between the Chinese and aborigines ensued. The Dutch mediated the conflicts while reaping profits from both sides. For the Chinese, in addition to the required production quotas, the Dutch also imposed residency-permit taxes on them. For the aborigines, the Dutch presented itself as their protector from Chinese expansion. And to be a loyal vassal to the Dutch lordship, the tribes had to submit tributes and control Chinese smuggling and piracy. Financial pressure eventually generated resentments and eventually led to Kuo Huaiyi's leadership of a Chinese anti-Dutch rebellion in 1652. The Dutch pacified the rebellion through an alliance with the aboriginal people (Ch. 6, 7, 8, and 9).

The Dutch regime in Taiwan was ended in late 1661 by Zheng Chenggong, the son of Zheng Zhilong. After the Manchus entered the Ming capital, Beijing, in 1644, Zheng Zhilong shifted his allegiance from the conquered Han Chinese Ming Empire to the Manchus' Qing Dynasty. Known in Western documents as Koxinga, Zheng Chenggong used his family business to finance his anti-Qing mission to restore the Ming Empire. Some historians interpret Koxinga's conquest of Taiwan as an anti-imperialist action, while other scholars view those interpretations as politically distorted. In response, Andrade cites Koxinga's correspondence with the Dutch in 1660, "How can one know my hidden thoughts and tell what are my actual intentions, which have been revealed to nobody?" (p. 211). Andrade goes on to say, "We historians will perhaps do little better than his [Koxinga's] enemies at discerning his true aim" (p. 221). To understand Koxinga's rationale for attacking the Dutch in Taiwan, Andrade reconstructs the tension between Koxinga and the Dutch as a result of the operation of the intra-Asian trade.

The anti-imperialist thesis does not explain why Koxinga did not support the 1652 Chinese anti-Dutch uprising in Taiwan. Relations between Koxinga and the Dutch regime in Taiwan remained amicable prior to the late 1650s, when the Manchus began to implement a series of policies designed to uproot Koxinga's maritime businesses. Beginning in 1656, the Manchus ordered the execution of any Chinese involved in trade with Koxinga. And from 1660, the Manchus imposed a ban on maritime trading. To compensate, Koxinga increased his overseas business, which, however, intensified the competition between the Dutch Batavia and Spanish Manila in the intra-Asian trade. To revenge the capture of his junks by Dutch Batavia, Koxinga imposed a trade embargo against all junks from Dutch Taiwan. Eventually a war broke out in April 1661. Reinforcement from Batavia was too little and too late. And thanks to Sino-Dutch co-

colonization, thousands of Chinese settlers in Taiwan helped establish the first Chinese regime in the colony in 1662 (Ch. 11).

According to Andrade, Western colonialism in early modern Asia did not prove Western superiority in military technology, economic organization, or technological prowess. What distinguished the Europeans and their Asian counterparts was an unusual European motivation to extend their territory beyond what they could effectively control. The Chinese Empire's lack of interest in overseas expansion provided an opportunity for the Dutch and the Spanish to establish their colonial footholds in Taiwan. The Dutch regime lasted longer than the Spanish because they gained indispensable Chinese and aborigines' support, but, when the Chinese became interested in the island, the Dutch had to go.

This book is most welcome among world-system scholars, especially those who have been excited by Frank's *ReOrient* debate. Frank pointed out the importance of the global silk-silver trade in the early modern world system. Andrade further delineates how the trade in maritime Asia developed alongside Euro-Asian co-colonialism in the region. By emphasizing the shared economic rationale between the Europeans and the Han Chinese entrepreneurs, as well as his theoretical insights and historiography of co-colonialism, the author also revisits the myth about the association between Confucian cultural logic and Chinese business practices. At the same time, educators may find that the interactive maps, art work, and hyperlinks in the electronic version of the book make it a user-friendly means of introducing beginners to the Euro-Asian interaction in modern world history.

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Moghadam, Valentine M. 2008. *Globalization and Social Movements: Islamism, Feminism, and the Global Justice Movement*. Lanham: Rowman and Littlefield Publisher, Inc. 180 pages, ISBN 0-7425-5571-2 Cloth (\$70.00), ISBN 0-7425-5572-0 Paper (\$22.95).

Over the past quarter century, critical questions have been raised about the relationship between social movements and globalization. A central challenge facing scholars who enter this terrain of scholarship is how to situate social movements within an ever shifting global context without falling into the traps of world-systemic over-determinism or idiographic particularism. Valentine Moghadam's book, *Globalization and Social Movements: Islamism, Feminism, and the Global Justice Movement*, asks how social movements and networks affect the evolution of globalization, and how globalization has transformed the nature of collective action. And in doing so, provides both a compelling set of substantive arguments regarding the nature of social movements in the contemporary era and a useful conceptual framework for understanding the interrelationship between global and local processes.

The central focus of Moghadam's book is a comparison of three transnational social movements (TSMs) from a world-systems perspective: the Islamist, the feminist, and the Global Justice movements. Moghadam examines the origins, similarities, and differences among the

three movements, situating each within the political, economic, and cultural dimensions of globalization. By situating each within a global context, she comes to a number of interesting conclusions. First, Moghadam states that equal attention must be paid to sub- and supra-state governance processes and nation-states, which continue to be relevant actors on the world scene. Second, the location of a transnational social movement within the uneven political, economic, and geographical context of the world-system affects its subsequent development. These processes span multiple scales, from micro (e.g., access to employment or education), to meso (e.g., area culture or regime type), and macro levels (e.g., neoliberal policies and technological expansion, which vary significantly from place to place). Third, all of these factors mentioned above shape individuals' worldviews, which in turn shape a movement's ideology, opportunities, and tactics to achieve group goals. Finally, a central distinction between groups which utilize violence to achieve their aims and those which eschew it, is the movement's desire to seek state power (p. 6).

Moghadam draws on evidence from case studies to support her arguments. She looks at Islamism, Feminism, and the Global Justice movement cases, and finds that the groups involved responded in different ways to global dynamics. She contrasts the Islamist movement with the feminist and Global Justice movements to illuminate how the heterogeneous economic, political, and cultural restructuring processes of globalization have impacted regions in the world-system differently. She concludes that TSMs have created multiple transnational publics that may overlap to varying degrees, facilitating a loosely interconnected global reach. These publics, which may overlap or diverge entirely, are comprised of actors that consciously communicate, cooperate, and organize across borders (p.121). Although TSMs are reacting to globalization by targeting both states and global order, they use the fruits of globalization, namely the internet, ease of travel, and developing technology, to promote and further their objectives. Collective action, networking, and recruitment have expanded into the virtual field. Ultimately, the polymorphic nature of globalization has yielded both progressive, democratic movements as well as conservative, violent groups (p. 122).

The book makes a number of contributions to TSM scholarship and has numerous strengths. First, its consideration of these three movements side by side leads to a deeper theoretical understanding of how globalization has fostered multiple, loosely connected transnational publics. These publics, in turn, yield both peaceful and violent TSMs based upon that particular public's ideological development and perceptions. Second, it develops an explanatory framework for understanding the complex relationship between globalization and TSMs. Further, it demonstrates how TSMs protest globalization while simultaneously depending on it. Next, it eloquently synthesizes broad bodies of literature to provide an insightful, careful analysis of the dynamic impacts of globalization on the mobilization of social movements. In addition, it thoroughly explores the historical roots and current developments of three empirical cases under the auspices of late stage capitalism. Finally, the book shows how TSMs balance strategic-instrumental considerations with emotional and altruistic ones.

However, despite its various strengths, there are some limitations within the book. First, Moghadam's analysis overemphasizes the presence of violent groups within the Islamist TSM, while understating their prominence in the other two TSMs. For example, the Zapatista movement (part of the Global Justice Movement) initially used armed strategies to achieve its goals. I would have liked to see a section discussing other anti-globalization social movements that currently use violence to further their objectives. Anti-globalization groups that use

repertoires of violence also have multiple publics where transnational dialoguing and networking are taking place. I felt that a brief discussion would have bolstered her points about the polymorphic nature of globalization fostering progressive and violent movements. Next, in relation to violent movements, Moghadam argues that groups utilize violence to seek state power. However, other scholars have found evidence showing groups use violence as a response to government repression. Other variables such as group ideology, identity, etc. may have explanatory power as well.

Despite these limitations, this is a compelling book. Social movement and globalization scholars will appreciate Moghadam's improvement upon both literatures and her explanatory framework she puts forth to remedy the theoretical gaps. The book will also be valuable in a graduate student seminar as an overview of contemporary transnational social movements and globalization. This succinct integration of topics such social movement theory, globalization processes, gender inequality, and religious fundamentalism also makes the information easily accessible to activists and public intellectuals.

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**Babones, Salvatore. 2009. *The International Structure of Income: Its Implications for Economic Growth*. Saarbrücken, Germany: VDM Verlag. 180 Pages. ISBN: 9783639101591 Paper \$105.**

In *The International Structure of Income* Salvatore Babones addresses two interrelated questions. In the first half he addresses the *shape* or structure of the distribution of world-income at both the simulated person-to-person level (Chapter 3) and at the national level à la Arrighi and Dangel (1986, Ch. 4). In the second half he examines the consequences of the structure of the world-economy for economic development. Babones integrates a standard growth model from economics with the structural intuition of the world-systems perspective to suggest that specific growth mechanisms “differ systematically across the three zones of the world-economy” that he identified in the first half of the book (p. 5). The book is an excellent read for world-system scholars who take seriously the notion that social structure matters for economic development. It also provides solid reviews of the global distribution of income, neo-classical theories of growth, and a clear demonstration of simultaneous equation and instrumental variable regressions (which have yet to make it into mainstream sociology, but have been used in economics for over two decades).

Babones's motivation for the book includes a review of the relevant literature on global income inequality. He notes that the only conclusion one can draw from this literature is that inequality has remained fairly stable over the last half of the 20<sup>th</sup> century, after rising precipitously since the industrial revolution. This conclusion is based largely on the major difficulties regarding issues of national income measurement, sample selection, and the inherent ambiguity of summary measures of inequality themselves, all of which lead to different findings regarding the level of global income inequality and its trend overtime. Others have drawn similar

conclusions (Anand and Segal 2008). More importantly for Babones, however, is his conclusion that the stalemate warrants a fresh look from a different vantage point – the shape (or structure) of the entire distribution.

Thus, Chapter 3 examines “the full distribution of income in the world” by simulating a person-to-person (or at least national-sub group to national sub-group) distribution of income plotted against the size of each national-sub group (35). Babones’s point of departure involves a method to generate a simulated distribution and an attempt to characterize the shape of the distribution rather than its variability. He finds a clear bimodal distribution, which casts some doubt on the analytical utility of common summary measures of inequality. Moreover, this finding lends itself immediately to an intuitive question – does the distribution become increasingly polarized over time? In other words, “the distance between the two modes of the global income distribution could be used as a rough indicator of the degree of global inequality” (58). Unfortunately, the intuition is made problematic because the absolute gaps do not give a sense of whether or not a given group is obtaining a different *share* of the income pie, since the absolute gap between the rich and poor will increase even if both grow at the same rate due to the larger initial income of the rich group. Moreover, Babones reaches different conclusions when he compares the gaps based on logged income and raw income. Here the gap declines with the logged data and rises with the raw data. The difference is an artifact of the logarithmic transformation which it picks up the *higher growth rate* of the poor mode (534% from the 1960s to the 1990s) relative to the rich mode (96.1%).

After reviewing several approaches to operationalizing positionality in the world-system (network based, continuum, and income based), Chapter 4 refines and extends the classic work of Arrighi and Drangle (1986) to simulate the distribution of national income, which is used to generate the world-system position of countries. Core countries occupy the wealthiest mode, semiperipheral countries occupy an intermediate mode and peripheral countries occupy the poorest mode. Unsurprisingly, Babones’s findings suggest that a trimodal distribution of national income is extremely stable over time, and that the three modes generate a curvilinear association with within country income inequality a la Kuznets, at least when limited to “organic” zone members. That this measurement produces a stable three-tiered structure is immediately appealing to world-system analysts. However, I think that the review and criticisms of other operationalizations of the world-system were less than complete. Furthermore, the utility of the income approach quickly can lapse into tautology – peripheral countries are poor because they are located in the periphery, but we only know they are located in the periphery because they are poor. Moreover, the apparent curvilinear association with income inequality is plausibly due to cut points in a continuous distribution of national income.

The last two chapters of the book integrate the results from Chapter 4 with mainstream economic theories of growth by assessing the extent to which neo-classical growth mechanisms vary by world-system position. Following an excellent and accessible review of the neo-classical model of labor, physical and human capital, Babones estimates cross-sectional growth regressions separately for core, semiperipheral, and peripheral countries to see if “the structure of neo-classical economic growth differs dramatically across zones of the world-economy” (133). In one set of long-run models, physical and human capital matter for the core but not labor, only physical capital (and maybe labor) matters for the periphery and only labor matters for the semiperiphery. This makes some intuitive sense over the study period (1960-1999) because core countries have a distinctive abundance of physical and human capital; semiperipheral countries

started their developmental trajectories by exploiting abundant labor supply; and perhaps successful peripheral countries distinguish themselves foster development in labor-intensive manufacturing. These results were somewhat fragile to the time period, so one wonders if the apparent differences were due to differences in sample size across zones, which ranged from 9, to 20 to 34 for the core, semiperiphery, and periphery. Indeed, a second set of (medium-run) models that covers a shorter time span (1975-1995) but more countries (88) shows that the effects of neo-classical inputs are not “strongly differentiated by zone of the world economy, after all,” because labor is the most robust input across all three zones even though capital seems to matter only in the periphery (134-5). This latter result prompts an interrogation of physical capital, which is differentiated between foreign and domestic sources in the context of additional growth models. The findings suggest that only domestic investment has a robust association with growth, and this is largely confined to the periphery. Yet, Babones suggests that a plausible explanation is simultaneity bias in the investment – growth link, where growth causes investment rather than the opposite. Indeed, the last empirical chapter conducts structural equation modeling with instrumental variables and shows that while growth tends to lead to both types of investment, the reverse is not true.

In sum, Babones’s book will be intrinsically interesting to world-system analysts because of its explicit structural argument that the distribution of income among individuals and nations, and the process of economic development are patterned by the structure of the world-system. Indeed, I think that this approach provides the most fruitful road forward for world-system analysts because it represents both a willingness to cross-fertilize with other disciplinary perspectives on these issues and moves away from the kind of totalizing (and overly pessimistic) mood that has seemed to dominate world-systems analysis in years past. Moreover, Babones’s work leaves open a number of research avenues. For example, while his method in Chapter 3 may not be the best way to assess the extent of polarization, other methods are available and may provide valuable descriptions of the distribution (Alderson, Beckfield, and Nielsen 2005). Similarly, Chapters 3 and 4 suggest a reasonable set of questions about global inequality that move away from describing variability toward describing the latent structure or shape of the entire distribution, though such analyses would be more compelling if accompanied by statistical tests for departures from “bimodality,” “trimodality,” or any other distributional shape (e.g. Korzeniewicz and Moran 2009). Finally, Babones’s inquiry into differences in developmental paths across world-system zones suggests a whole range of potential inquiries. A plausible first step would be to replicate his analysis using pooled cross-section of time-series regression techniques that would increase the sample size and allow researchers to test explicit hypotheses about differences in the size and direction of coefficients across world-system zones and control for unobserved time-invariant country attributes. If world-system dynamics matter for economic development, then established correlates of economic development must themselves vary by world-system position so that world-system dynamics should have both direct and indirect developmental consequences.

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Dylan, Riley. 2010. *The Civic Foundations of Fascism in Europe: Italy, Spain and Romania, 1870-1945*. Baltimore: John Hopkins University Press. xii + 258 Pages. ISBN: 13: 978-0-8018-9427-5 Cloth (\$ 55.00).

Classical scholarship and intuitive political sense connects democracy with robust civic life and associates fascism with unthinking mobs narrowly concerned with private matters and easy answers. Indeed, the "Tocquevillian thesis" connects robust associational life with democratic government. "Mass society theorists" push the thesis to authoritarian states, arguing that social atomization and weak associational life allow for the political mobilization of an antisocial mass and the imposition of totalitarian rule. In *The Civic Foundations of Fascism in Europe*, Dylan Riley complicates these influential and common views of democracy and fascism by drawing Gramsci into the conversation and reformulating the question in the context of hegemony. This move creates analytic space for human agency, avoiding some of the pitfalls of both ahistorical, mechanistic, variable-testing positivism and suffocating, agentless historical materialism.

To frame and justify his study, Riley poses both an empirical puzzle and logical paradox. "[I]n roughly half of Europe, fascism followed [an] intense wave of associational growth. This outcome," Riley notes "is especially puzzling in countries such as Italy, Spain and Romania, which had well-established liberal institutions by the late nineteenth century" (pg. 1). The empirical puzzle is further complicated by the received wisdom. For Arendt and mass society theorists, fascism "is the result of *both* mass political apathy and fanaticism, and both of these outcomes derive from the collapse of civil society" (pg. 8, emphasis in original). Why did fascism follow the explosive growth of civil society in Italy, Spain, and Romania? Did fascism in these instances mobilize fanatic mobs or manipulate the apolitical mass? More generally, Riley's study raises the question of the relationship among the development of civil society, democracy and state-formation.

Gramsci provides both the missing piece to the puzzle and the resolution to the paradox. Noting that most literature focuses on the relationship between civil society and established liberal democracies, Riley reframes on the question on "the political consequences of civil society development" (pg. 2). Riley uses Gramsci to "focuses on the interaction of civil society and politics, rather than reading one off the other" (pg. 12). With this Gramscian conceptual framing, Riley finds that fascism emerged out of the self-reinforcing cycle of political decay or organic crisis that befell the young liberal states of interwar Italy, Spain and Romania. During their formative periods of national unification in the late 19th century Italy, Spain and Romania all failed to achieve intraclass hegemony: Piedmont conquered and co-opted the rest of Italy as much

as it unified it, failing to bridge the historic divide between north and south; Catalan and Basque elites were never fully integrated into Spain's governing coalition; while "the statist middle class" and old boyar aristocracy that led the Romanian national project failed to incorporate either large landowners or the Jewish bourgeoisie. These incomplete governing coalitions created systems of oligarchic liberalism (known as *transformismo* in Italy and the Canovite system in Spain).

While these nominally liberal regimes rested on restricted electorates, they did provide the necessary legal protections to allow for the development of voluntary associations in rural areas. In Italy, northern Italian liberal elites closely linked to the movement for national unification fostered the development of civil society as a patronage project to create a political base. After the 1890s, however, agrarian socialism and later Catholicism displaced elite-dominated rural associations with an increasingly autonomous civil society. In Spain, civil society had three distinct strands: (1) socialist and republican associationism (2) regional nationalist associationism and (3) Catholic associationism. The dominant position of the Catholic Church and the tendency toward regional nationalism (in Catalonia and to lesser extent Basque Country) created elite-led civic society organizations. In Romania, in contrast, the growth of civil society was a state-led initiative connected to a national policy of industrialization.

In the context of oligarchic liberalism, however, this flowering of civil society tended to further fragment the social elite and deepen organic crisis. While elites attempted to broaden the support for liberal institutions, they did so from a weak base as a result of the previous failure of intraclass hegemony. Attempts to create interclass hegemony only further weakened the support for liberal institutions. In Italy, Prime Minister Giovanni Giolitti stopped using police in labor disputes, and tried to forge an alliance with socialists, but left the south under the heel of the landed elite. In Spain, Primo de Rivera made a similar attempt to establish an alliance with the socialist party and pursue a project of state-led industrialization. Unlike Giolittian Italy, however, the Primoverrista dictatorship systematically destroyed the political parties and fledging liberal institutions. In Romania the attempt to establish interclass hegemony was more complete. The Partidul Național Țărănesc (National Peasant Party or PNT) initiated universal suffrage and land reform, something that both Italian and Spanish elites failed to accomplish. Despite these efforts, the failure to establish intraclass hegemony put obdurate limits on the ability of state managers to open their states to the popular classes, while retaining dominance of the ruling bloc. Giolitti limited effective suffrage to the North. Primo de Rivera's reforms benefited only a small segment of the working class. The PNT's land reform coincided with a new constitution that eased the application of martial law and the manipulation of elections. The growth of civil society, then, benefited Catholicism and socialism and only further discredited liberal institutions, as liberal parties consistently failed to win popular support.

The failure of intraclass and interclass hegemony, in turn, led to the failure of counterhegemony, creating a political vacuum for fascists to fill. Facing a fragmented dominant class with no clear project, democratic forces tended to split and fragment rather than unify. In all three countries, the urban working class, the peasantry and the petty bourgeoisie failed to come together in a meaningful alliance. In Italy, the Italian People's Party and Italian Socialist Party did not come together to create an effective governing coalition and, instead, pursued incompatible programs. In Spain, republicans and socialists remained divided, with the former committed to land reform and the latter wed to doctrinaire anti-clericalism. Internal divisions within the Romanian PNT between the peasantist wing and urban bloc anchored in the petty bourgeoisie resulted in an incoherent platform that alienated the party's base. "The rise of a fascist

movement,” Riley contends “was connected with the tendency of democratic forces to split and fragment. This tendency, in turn, was the result of the absence of a conservative pole with a strong hegemonic project against which the democratic forces could articulate a counter-hegemonic project” (pg. 200).

As a result of the failure of hegemony, fascism emerged as the resolution to the organic crisis that mired Italian, Spanish and Romanian politics in shortsighted, sectarian disputes. Fascism, Riley contends, emerged as authoritarian movement positioned against liberalism but not democracy. To support the controversial and counterintuitive claim that fascism represented authoritarian democratic movements in these cases, Riley notes that fascists relied on the language of civil society to position themselves as the true representatives of nation and co-opted civil society’s social networks as the organizational resources to enliven their movements. Fascists idealized civil society as a zone of spontaneous cooperation that could only fully bloom when freed from the fetters of politics. Where liberal states failed to represent the complex of associations that made up modern societies, fascism sought to directly mobilize interest-based associations to either replace the liberal state or supplement it with more representative but less politicized institutions.

How fascism consolidated is directly related to civil society development. Italian fascism, like Italian civil society, was more autonomous. Terming it party fascism, Riley notes the Partito Nazionale Fascista and affiliated groups exerted pressure on Mussolini that pushed him into more radical positions. For example, fascist insurgency caused Mussolini to abandon his pragmatic alliance among Italian industries, fascist syndicates and reformist socialists. Mussolini capitulated to squadristi, and moved to more radical positions, as Italian fascism emerged from below. In Spain, fascism relied on the social networks of Catholicism and, unlike Italy, had no separate fascist party structure. Riley terms this fascism traditionalist, as Spanish fascism rejected the view of the party as a pedagogical institution and held up the monarchy, the family and the church as the appropriate channels through which to represent the nation. Romanian fascism was divided among two competing tendencies, a party fascism associated with Corneliu Codreanu’s *Liga Apărării Național-Creștine* (League of National Christian Defense) and a statist fascism led by King Carol II. Reflecting Romanian’s state-led civil society, party fascism lacked a popular support base in a union movement or confederation of agrarian cooperatives. Carol’s attempt to create statist fascism from above overtook party fascism, which folded rather than imposing fascism from below.

Before explicitly drawing out the full ramifications of his study, Riley considers two alternative explanations: (1) Barrington Moore’s thesis on fascism as an alliance among the state, a medium-strength bourgeoisie and labor-repressive agrarians under the pressure of economic competition and peasant and worker unrest; and (2) the Weberian thesis, which emphasizes the persistence of an authoritarian old regime in an advanced industrial society as the key element behind fascist regimes. The Weberian thesis quickly falls apart when applied to Riley’s cases, all of which were oligarchic liberal regimes. Italy and Romania, most dramatically, had no “old regime” from which to draw. Moreover, Riley’s three cases pose empirical problems for Moore’s thesis too. In Italy, the labor-repressive agrarians remained sequestered in the politically marginalized south; in Spain, elite factionalism and regionalist nationalism fragmented the elite and prevented any effective coalition; while, in Romania, the PNT’s successful land reform liquidated the labor-repressive agrarians as class. From here, Riley considers positive cases for Moore’s thesis, Germany and Hungary and then applies his own civil society thesis as counter-

explanation. He concludes that Moore's thesis applies well as a description of prewar Hungary and Germany but as an explanation for fascism it falls short. German fascists seized state power only after the Moorian coalition collapsed during Weimer Republic. In Hungary, where this coalition persisted, it acted to block the full development of fascism. Hungary, the only member of the Axis to keep a parliament throughout the war, never became an openly fascist dictatorship. To conclude this discussion, Riley argues that his civil society thesis does more to explain the rise of fascism in Germany, which, like his cases, experienced dramatic growth in civil society and lacked hegemonic organization among the elite, creating an organic crisis which fascism resolved.

Fascism is a slippery subject to approach as it transcends the conventional right/left political dichotomy and presents puzzling empirical problems with its relation to civil society and democratization. Riley's innovative combination of Tocqueville and Gramsci does much to allow us to get a firm grasp on this mercurial subject. While Riley's characterization of fascism as authoritarian democracy may trouble some readers, I feel it is one of the stronger contributions of his work. It acts as a necessary corrective to ideological position that conflates democracy with liberalism. As Riley is correct to note, "A regime is democratic to the extent that its claim to rule rests on a claim to represent the interests of the people" (pg. 207). Fascism could emerge as a solution to the organic crisis exactly because its claim to represent the people was more credible than floundering oligarchic liberal regimes. Riley's addition of Gramsci and hegemonic struggle is essential. He reminds us that political struggle is not a zero-sum game. The failure of more democratic and/or socialist counter hegemony in Italy, Spain and Romania was directly related to the failure of intra- and interclass hegemony. "Gramsci," Riley contends, "gives a positive value to political struggle that is rare not only in the Marxian tradition but also in much political sociology. In short, Gramsci allows one to recognize the value of real political struggle, of hegemonic struggle, for liberal democracy" (pg. 211). Here, the full value of Gramsci becomes clear. Riley's work is exemplary in that it places intentional human action at the center of his analysis. In each of his studies he identifies critical moments of struggle where different outcomes were politically plausible and nearly became reality. This is a quality that is rare in studies of regime types and their origins and deserves to be commended.

One unfortunate shortcoming of Riley's work is its lack of greater world-historical contextualizing. Connections among the various cases emerge during the study (particularly in the Romanian case where fascists looked to both their Italian and German compatriots as models) but are underdeveloped. Is there an iterative quality to fascist regimes? Riley turns to Gramsci's concept of hegemony but he does not deal with his argument concerning the relationship between uneven development and fascism. Instead, Riley presents each case as a distinct time-space. The national and narrowly comparative constraints of the study limit the applicability of Riley's conclusions. Connections among the various cases emerge during the study (particularly in the Romanian case where fascists looked both their Italian and German compatriots as models) but are underdeveloped. Is there an iterative quality to fascist regimes? Is fascism a relevant concept outside of interwar Europe? Beyond the social scientific worth of this question, is a more urgent and political value. In the era of the British National Party, the French Front National, *Ülkücü Gençlik* and *Büyük Birlik Partisi* in Turkey, the Brazilian *Partido da Reedificação da Ordem Nacional*, *Vishva Hindu Parishad* and *Bharatiya Janata Party* in India, the South African *Afrikaner Weerstandsbeweging*, and perhaps even the Tea Party of the United States, the question as to whether a twenty-first century fascism is possible or probable is politically expedient. While

this question is not Riley's primary focus, his analysis tempts the reader to raise many provocative questions about the applicability of his analysis to the present. In such a clearly written and tightly argued book, it is regrettable that Riley chose not to draw out the contemporary relevance of his conclusions.

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**Smil, Vaclav. 2010. *Why America is Not a New Rome*. Cambridge, MA: MIT Press. 226 Pages, ISBN 978-0-262-19593-5 Cloth (\$24.95).**

This book is entirely devoted to uprooting the myth that there are profound similarities between the Ancient Roman Empire and the United States, particularly in terms of the decline of empire—or in other words that America is somehow a “new Rome.” Vaclav Smil is disturbed by the popularization of the analogy in the media, extended by figures such as Joseph Nye, Cullen Murphy, Tom Wolfe, and Peter Bender. In response, he produces a detailed argument on how the analogy is misleading. Comparisons of the two states tend to be based on superficial realities and mistaken understandings, or else universal human tendencies that are certainly not unique to the U.S. and Rome, and therefore not very illuminating when reduced to a Rome-America analogy. Smil is convincing, and he provides fairly comprehensive data to support his assertion that descriptions of Ancient Rome and the U.S. as powerful empires that can be meaningfully compared are drastically misguided.

Smil begins by discussing narratives that draw comparisons between the Roman Empire and states such as Britain and now the United States. He notes that comparisons generally rely on assertions about the populations, intents, and strength or power of the two states. Many commentators accordingly make the provocative suggestion that the U.S. will follow in the footsteps of Rome—predicting a “fall” of massive proportions. But when Smil examines all of these proposed similarities systematically, he finds that there is little foundation for any of it.

I find Smil's treatment of the meaning of empire and its application to the U.S. to be the most fascinating and relevant discussion in the book. This part touches on the academic debate about whether the U.S. is imperial, and begs the question, what would constitute an empire in an integrated and truly global economy? He also explores the concept of the U.S. as a global hegemon, which he finds doubtful. For Smil, an empire exerts political control over a separate political unit (pg. 45). The U.S. does not qualify as an empire according to his examinations — seized territories have always been either incorporated or set free by the United States. Direct military rule by the U.S. has been limited and temporary. The military reach and global hegemonic power of the U.S. have been vastly exaggerated. Instead, Smil suggests that the concept of empire is entirely inappropriate, and that the U.S. is more accurately described as a limited hegemon. Certainly, the power exerted by the U.S. does not operate in the way of the Roman Empire. Further, Smil explains how even the mightiness of the Roman Empire is exaggerated and put out of context. It was neither the most extensive nor longest lasting empire

even among its contemporaries, and as he goes on to detail exhaustively, it was certainly not the most inventive nor productive state of antiquity (pg. 63).

Indeed, in some respects, the differences between the U.S. and Ancient Rome are vast. Smil describes how the Romans were not very impressive inventors or innovators, whereas the U.S. has been a leader of invention and innovation. The Roman Empire did little to advance scientific understanding, and their technical accomplishments occurred slowly over time. Ancient Romans were far more dependent on machines that were powered by people (slaves primarily) and animals. This is “fundamentally incomparable” to the U.S., which is powered overwhelmingly by machines and non-animal based energy (pg. 103). Whereas the Romans’ energy use seriously constrained every aspect of Roman society, America’s high energy availability has created massive opportunities and problems that are not comparable to the Rome of antiquity. Moreover, the U.S. is remarkable compared to contemporary states, whereas Rome’s record in invention was not very impressive compared to Hellenistic Greece or Han China. Differences in quality of life between Ancient Rome and modern America are incommensurable, whether measured as GDP, GDP rate of increase, infant mortality, life expectancy, housing conditions, wealth inequality, reliance on machines, or energy use.

Smil concludes that “whatever lessons can be drawn from the demise of the Western Roman Empire are of little avail in illuminating the global reverberations of any dramatic weakening of America’s standing in the modern world” (pg. 148). In terms of the fall of Rome and the predicted fall of the U.S., Smil maintains that there is little to support comparisons. One could even contest the idea that there was a fall of Rome, since in reality it was more of a slow decline (and applicable mainly to the Western Empire). Ironically, in this the two are reportedly similar: the U.S. has been experiencing “a gradual retreat encompassing all parameters that make a nation a great power” since World War II (pg. 168). It is not likely that its power will abruptly vanish. Smil argues that surmises of the dramatic fall of the U.S. in the near future are rhetorical hyperbole.

While Smil’s book is thoroughly researched and appears adequately objective, I am not convinced that the designation of America as a New Rome is quite so rife within academic discourse to justify devoting an entire book to refuting it. Immanuel Wallerstein has already established that there is a difference between a world-empire and a world-economy, but Smil does not engage the world-systems perspective at all, either to critique or incorporate. Much of his examination of the differences between Rome and the U.S., which takes up about 50% of the book, seems to be overreaching the mark; it should be self-evident that daily conditions were very different between modernity and antiquity, especially with regards to common indicators. It would have been more interesting to me anyway if he had focused on the issue of the U.S. as imperialist, although even those debates generally recognize the vast difference between old world empire and the nature of empire in a modern world-system. At any rate, if I ever had any temptation to refer to America as a “new Rome,” it has now been eradicated effectively.

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