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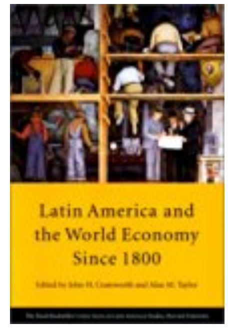

John Coatsworth, Alan Taylor, eds.. *Latin America and the World Economy since 1800.* Cambridge, Mass.: Harvard University Press, 1999. xv + 484 pp. $49.95, cloth, ISBN 978-0-674-51280-1.

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Published on EH.Net (February, 2000)

When I started graduate school in 1973, there were no textbooks on Latin American economic history. Today, depending on your definition of a textbook, there are three or four in English alone. In 1973, we argued about the Asiatic mode of production and precapitalist economic formations. Today we discuss conditional convergence. In 1973, bourgeois economists were the enemy. Today a bourgeois economist is your dissertation supervisor. Welcome to the Cliometric Revolution, Latin style. It's been 25 years in coming, but now that it's come, it's come with a vengeance.

The present anthology is an artifact of that revolution and like all historical artifacts, it requires a bit of study to appreciate its meaning in full. And so to begin, I'm going to quibble with the idea that what you read here is really all that novel. After all, there's always been some cliometric work on Latin America, as the outstanding books of Carlos Dmaz Alejandro on Argentina or Clark Reynolds on Mexico might attest. In my primary field, Mexican history, you could point to things done by Luis Tellez or by Jaime Zabludovsky as recognizably cliometric, but Tellez and Zabludovsky have gone on to major careers in government service rather into careers as economic historians. What's more unusual is to find suitably trained professionals doing purely academic work--doing economic history for a living. For that we can thank, at least partly, a sea change in development ideologies in Latin America, where economists in universities can now spend their time thinking about conditional convergence (whose acquaintance they may have made in some gringo institution) rather than about the Asiatic mode of production. And I think I have some idea why.

For my generation, it was the fall of Allende in 1973 that was critical. For this one, it is the fall of the Berlin Wall. That makes all the difference in the world. You can write sympathetically about the economic history of Cuban sugar mills without espousing the labor theory of value. You can study the history of financial markets in Brazil without being implicated in the overthrow of Joco Goulart in 1964. For now, at least, there are no gangster regimes advocating "market friendly" policies while energetically murdering their own citizens. The ideological and political baggage of the 1960s and 1970s is, for want of a better phrase, just so much history. Hence what we read here by so many relative newcomers to the field. Their authors are students, not prisoners of the past, and that's what makes their scholarship worthwhile. I do have a small bone to pick with the volume's title. This is not a book about Latin America since 1800. It is mostly about Argentina, Brazil and Mexico since 1870, which is not quite the same thing. There are no Indians. There is no Caribbean or Central America. No Andes. But worse, there are really no papers that engage with the period before 1870 and that is a real problem. As John Coatsworth's perceptive essay on the nineteenth century puts it, "the available quantitative evidence shows that Latin America became an underdeveloped region between the early eighteenth and the late nineteenth century" (p. 26). In other words, most of the papers in the volume-Carlos Newland's excepted-do not address the principal issue of Latin America's economic history, namely, the origins of what Lant Pritchett has called "divergence, big time." Even if you argue in reply, that X (what existed before 1870) causes Y (what changed later), the historian is liable to wonder why X occurred when it did and not before, especially if Y is extremely profitable, the proverbial big bill on the sidewalk.

I think I know why. Sensible historians avoid the period before 1870 because it is a Hobbesian world where life, not to mention some of its major actors, was nasty, brutish and short. For most of Spanish America, the era before 1870 (and after Independence in the 1820s) is much, much harder to work in, let alone understand. The archives with which I am familiar (mostly Mexican, to be sure) are a mess-disorganized, uncatalogued, impenetrable-and very nearly impossible to utilize. Of course, the messiness of the sources faithfully reflects the messiness of economic and political life at the time, with unending coups, countercoups, invasions, constitutions, blockades, wars, partitions, regulations, proclamations, declamations, you name it. There's no stable structure for understanding, essentially. Unfortunately, this is where the action is, unless you regard disorder itself as the proximate cause of poor economic performance. As anyone reading this is probably aware, there's really no consensus about that either. For this reason, I take claims made for the cliometric potential of Latin American economic history the way I take tequila: in limited doses, and with many grains of salt. Still, triumphalism only infects the blurbs to the volume, for the "Introduction" by John Coatsworth and Alan Taylor is conspicuously moderate in tone. So maybe I shouldn't complain. Besides, the papers are generally very good and a couple are outstanding. One of the most coherent themes here is the importance of financial markets and institutions in facilitating or accommodating economic growth. This really is a new direction, at least in the Latin American context, for I can think of little in the older historiography that makes this point with any cogency. A very interesting paper by Michael Twomey provides the relevant context in arguing that "[t]he general trend of direct foreign investment [in the twentieth century] has been downward relative to income and, probably, total capital stock" (p. 192). Portfolio investment aside, which Twomey identifies as mainly, until 1990, loans to governments, the implication is that domestic sources of capital were increasingly important between 1913 and 1950, the years when foreign direct investment fell sharply relative to GDP. Twomey's argument frames papers by Stephen Haber, Anne Hanley, Leonard I. Nakamura and Carlos E. J. M. Zarazaga, Gerardo della Paolera and Alan M. Taylor, and Gail D. Triner.

First, Brazil. Anne Hanley's study of business finance and the Sco Paulo Bolsa offers a good point of departure. In the spirit of Twomey's conclusions, Hanley argues that the role of foreign capital in direct investment "while sizeable, mainly played a supporting role in the domestic business formation that was the cornerstone of Sco Paulo's development." The industrial and utilities sectors "actually found their base in the domestic capital market" (both quotations, p. 126). And it was the impersonal mechanism of the stock exchange rather than traditional kin-based finance that fueled "a type of financial Big Bang" between 1905 and 1913 (p. 131). Similarly, Gail Triner finds that the recharter of the Banco do Brasil in 1905 created a "natural infrastructure for financial transactions" (p. 224) that supported a "strong, centralized role for the national government in the economy." And like Hanley, Triner emphasizes that "[t]he banking system increasingly accumulated and reallocated financial resources of the private sector at the expense of either personal or other institutional channels" (p. 226, both quotations). After 1905 the real money supply and the monetized economy grew rapidly, even as the economic predominance of Sco Paulo was consolidated.

The evolution of a modern financial infrastructure for Brazil had measurable implications for the growth of industrial productivity in Brazil after 1890. Stephen Haber's sophisticated analysis of capital market regulation and the development of a securities market argues that "one crucial piece of the puzzle explaining the lack of industrial development before 1890 and rapid industrial growth after 1890 was access to capital" (p. 279). The maturation of debt and equity markets along with the establishment of limited liability laws and mandatory financial disclosure lowered the cost of capital. As a result, the cotton textile industry, which is Haber's focus, grew more quickly than it would have had traditional patterns of kin-based and other less formal avenues of finance been maintained. In short, "entrepreneurs who could best combine the factors of production and choose the optimal output mix were able to mobilize capital that otherwise would not have been available to them" (p. 279).

Argentina has always seemed baffling. Between 1870 and 1900, real per capita product there doubled, but after 1900, it would not do so again until 1958. In other words, the rate of real per capita growth fell from 2.3 percent per year to 1.19 percent per year, which is some slowdown. For Gerardo della Paolera and Alan Taylor, a capital constraint is (part of) the answer. The domestic financial system was simply unable to replace the dwindling supply of British capital after World War I. Caught between the gold standard, international convertibility, and repeated financial crises, the monetary authority, the Caja de Conversisn, was unable to support domestic banks and maintain convertibility at the same time. For this reason, Argentine banks "had to maintain a higher capital cushion" than their foreign counterparts who could borrow abroad much more easily. "[D]omestic banks could not fill the void left by the retreat of foreign capital after 1914" (p. 163). A paper by Leonard I. Nakamura and Carlos E. J. M. Zarazaga raises some questions about this argument by looking at returns to Argentine debt instruments, which don't seem particularly high. Daniel Dmaz Fuentes' chapter on the gold standard in Argentina, Brazil and Mexico reminds us that the Argentine peso was inconvertible between 1914 and 1927, an awkward point for della Paolera and Taylor as well. Nevertheless, their discussion of the non-monetary aspects of financial crises in Argentina is very stimulating. I have heard it said by some historians that there is nothing "new" in the findings of the new economic history of Latin America. I defy them to read della Paolera and Taylor and then tell me that. I doubt the critics have read Bernanke's 1983 paper and the subsequent work it inspired. The remaining papers are somewhat more difficult to characterize because they deal with a wide variety of subjects. Let me give some examples.

Students of Mexican history will welcome the chapters by Graciela Marquez and Aurora Gsmez-Galvarriato. Both make extensive use of archival data and both question commonly held beliefs about Mexico between 1890 and 1920, the last years of the Porfiriato (the dictatorship of Porfirio

Dmaz from 1876 through 1910) and the opening decade of the Mexican Revolution (which lasted until 1920, 1938, 1968, or last week, depending on how you view Mexican history). Marquez shows that it is not enough to simply label Porfirian Mexico a high-tariff country since nominal protection fell sharply during the 1890s. It never recovered its former levels before the outbreak of the Revolution. Gsmez-Galvarriato looks at real wages in the Santa Rosa textile factory in Veracruz. Stability in real wages through 1907 gave way to a sharp decline between 1907 and 1911. A marked recovery occurred between 1911 and 1913, only to fall sharply during the bitterest years of the civil war (1914-1916). From 1917 through 1920, real wages recovered, but did not rise much above their level in 1907. I think Marquez and Gsmez-Galvarriato are saying that the stories we tell about Dmaz and the coming of the Revolution are not likely to hold up under the careful scrutiny of a new historiography informed by detailed industry and firm-level studies. Where this leaves the big studies of the Revolution, such as Alan Knight's, which retells many of the old verities, remains to be seen.

Both William Summerhill and Alan Dye contribute chapters that represent aspects of larger projects. Dye's study of the contracts between sugarcane growers and millers in Cuba lays to rest the myth that the contracts between growers and millers evolved to exploit the growers, upon whom they were coercively imposed. Summerhill's paper on Brazilian railroads concludes that "The direct impact of the railroad in Brazil places it comfortably within the top tier of the cases for which economic historians have constructed social savings estimates" (p. 391). Interested readers can certainly learn more from Dye's *Cuban Sugar in the Age of Mass Production* (Stanford, 1998) or Summerhill's forthcoming *Order Against Progress: Government, Foreign Investment and Railroads in Brazil, 1854-1913* (Stanford, scheduled for Summer 2000).

Papers by Lee Alston, Gary Libecap and Bernardo Mueller; Andri A. Hofman and Nanno Mulder; and Carlos Newland round out the volume. All are well worth reading.

A final observation: It's ironic that economic historians of Latin America stress the study of institutions, a theme that features prominently in this volume as well. For those of us trained in the early 1970s, "institutional history" was something to be avoided, the province of dullards and the unimaginative. It was a matter of faith, enshrined in a famous article by James Lockhart, that the only real historians of Latin America were social historians, and, well, social historians had better things to do than pay attention to, of all things, institutions. Institutions didn't affect the behavior of real people, and real historians studied real (read: ordinary) people. My how times do change. There isn't much doubt about who's doing the interesting history of Latin America these days. Not a few of them are represented in this excellent collection. Now if only I could get them to explain the Asiatic mode of production to me, my life would be complete. Fat chance.

Richard Salvucci teaches at Trinity University. He is co-author with Linda K. Salvucci of "Cuba and the Latin American Terms of Trade in the Nineteenth Century: Old Theories, New Evidence," forthcoming in the *Journal of Interdisciplinary History* in Autumn 2000.

Citation: Richard Salvucci. Review of Coatsworth, John; Taylor, Alan, eds. ***Latin America and the World Economy since 1800.*** EH.Net, H-Net Reviews. February, 2000.

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