Journal of Economic Literature
Vol. XLV (June 2007), pp.400–409

Global Capital Markets in the Long Run: A Review of Maurice Obstfeld and Alan Taylor's *Global Capital Markets*

Jeffrey G. Williamson*

Written by Maurice Obstfeld and Alan Taylor, Global Capital Markets: Integration, Crisis, and Growth *was a much-needed book that will be cited extensively by those with interests in the long run evolution of the world financial capital market. The book does not simply assess changes in the efficiency of global capital markets over the past 150 years, but rather adds significantly to debates about instability and crisis, asymmetry between rich and poor countries in the costs of going open, the Lucas Paradox, the connections between foreign exchange and financial capital market regimes, and much more. The book makes far better use of the comparative evidence generated by the three epochs since 1850—the first global century before 1914, the second global century after 1950, and the autarchy in between—than do competitors that focus solely on one regime, whether the gold standard, post–World War II Breton Woods, or the float since. In addition, while the financial literature rarely assesses in any useful empirical way the connection between financial markets and the real economy, this book makes that connection absolutely clear.* Global Capital Markets *is a stimulating book with a very wide and deep reach.*

1. *Overview*

Maurice Obstfeld and Alan Taylor have written a wonderful book that raises the academic bar, a book that all analysts interested in the operation and impact of financial capital markets should read.[1] There is a long tradition in international finance that combines theory and history and some very good economists have used the combination to improve greatly our understanding of international capital and foreign exchange markets. No doubt their interest in history is driven by the fact that there is little variance in foreign exchange and capital market regimes across countries at any point in time, and because these regimes persist for some time. The obvious advantage of history is that it offers considerable variety in these regimes over time and place, giving the economist a vastly better opportunity to

* Williamson: Harvard University. I am grateful for the comments on an earlier draft by Michael Bordo, Barry Eichengreen, Charles Engel, Chris Meissner, and Ken Rogoff.

[1] Maurice Obstfeld and Alan Taylor, *Global Capital Markets: Integration, Crisis, and Growth* (Cambridge University Press 2004). Hereafter, referred to as Obstfeld and Taylor or "the authors" and *Global Capital Markets.*

isolate what matters. Thus, there were some academic giants in a previous generation that led the way for the authors of *Global Capital Markets: Integration, Crisis, and Growth* (hereafter *GCM*), like Alec Cairncross, Paul Einzig, Milton Friedman, John Maynard Keynes, Charles Kindleberger, Oskar Morgenstern, Anna Schwartz, and others. Furthermore, Obstfeld and Taylor are not alone since they are part of a new generation of economists using history (plus better theory and econometrics) to explore the operation of global capital and foreign exchange markets, like Ben Bernanke (yes, the very same), Michael Bordo, Barry Eichengreen, Niall Ferguson (a historian), Marc Flandreau, Larry Neal, Thomas Sargent, Richard Sylla, Peter Temin, and others. So, why is *GCM* so different? Here are five reasons: the authors take the history of global capital markets from the medieval Champagne fairs to the present, a far bigger historical reach than in previous work (although most of their reach is back to 1850); they cover *world* experience, especially emerging nations struggling to get in to the European mainstream, a dimension almost always missing from previous Euro-centric work (but see Gerardo della Paolera and Taylor 2001; Paolo Mauro, Nathan Sussman, and Yishay Yafeh 2006); they cover a whole range of issues—all of those on the modern agenda—not just one or two; they develop a 150 year, multicountry panel data base (discussed in the *GCM* data appendix), making explicit hypothesis testing possible; and, while their writing is nontechnical and elegant, their economics is about as sophisticated as it comes.

Any book with the sweep, depth, and elegance of this one takes a long time coming, so it's not surprising that it was a decade ago that Obstfeld and Taylor received a Sanwa Bank (now UFJ Bank) grant to start this collaboration. Nor is it surprising that these two authors have been working independently on global capital market issues even longer: Obstfeld's first paper on the topic cited in *GCM* is 1986, with fifteen papers cited in the book after that date; and Taylor's first paper on the topic cited in *GCM* is 1992, with sixteen papers cited in the book after that date. But I want to stress that this book is definitely *not* just a group of related essays pasted between covers. Rather it is a coherent and comprehensive assessment covering all the issues raised individually in those thirty-three prior articles, and much more.

In short, there is no book out there to challenge *GCM* and it should remain the market leader for some time to come.

2. *The* GCM *Landscape and Achievements*

2.1 *Historical Regimes, the* Trilemma, *and Central Issues*

Let me start, as the authors do, by defining the historical regimes and by introducing a central organizing device used in the book.

First, consider the regimes. There have been two global centuries since the early 1800s and there have been three regimes. The first global century lasted until 1914 and World War I. The second global century is the one we live in now, one which started in 1945 as the world slowly resurrected what had died in 1914. Figure 1 plots the resulting secular boom, bust, and boom in world capital markets. As we shall see in a moment, there is a trio of world markets out there and the three are intimately connected—labor markets (mass migration), commodity markets (trade), and financial capital markets. Each of these underwent the same secular boom, bust, and boom in magnitudes and timing, and each of them appears to have obeyed the same laws of political economy motion that created liberal, restrictive and liberal policy. All of this raises two obvious questions: How much of what we observe in figure 1 is due to policy and how much due to domestic market forces (other markets) the world around? Which way does the causality go, from capital market boom to policy liberalization (or capital market bust

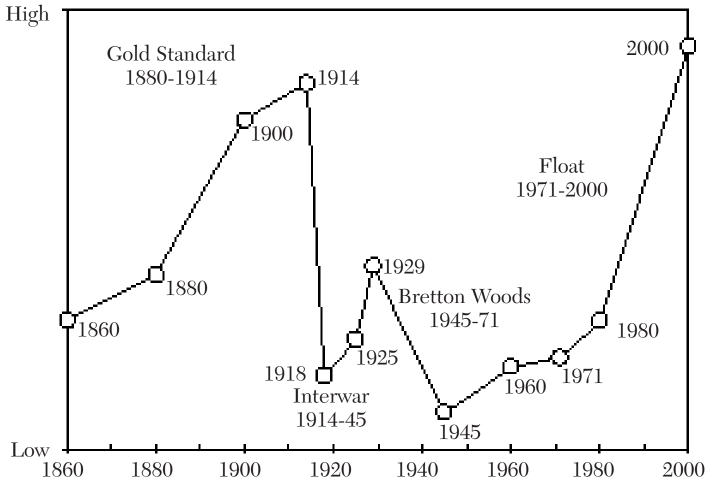

Figure 1. The Evolution of Global Capital Markets, 1860–2000

Source: Obstfeld and Taylor 2004, figure 1.3.

to policy restriction), or from liberalization to boom (or from restriction to bust)? We will return to this query below since I think it is central to all globalization discussions.

Second, consider the central organizing device. Very early in *GCM* (pp. 29–40), the authors introduce the *trilemma* concept, the binding constraint that makes it impossible to have at the same time capital mobility across one's borders, a fixed exchange rate, and an activist monetary policy. This is general equilibrium thinking, and it implies that world capital markets cannot be analyzed independent of foreign exchange regimes and domestic macro policy. It also implies that world capital markets cannot be analyzed independent of world labor markets (migration) and world commodity markets (trade). The trilemma is used with great skill by the authors throughout the book and they actually offer a test of the hypothesis in chapter 5 (pp. 172–94). Whether you find the test persuasive or not, the trilemma is still a demonstrably useful way to think about 150 years of world capital markets and the two transitions between the three regimes.

Along with the demarcation of historical regimes and the use of the trilemma organizing device, the authors also raise a question (pp. 4–15) that helps guide the book, a question which, sadly, is so often totally absent from this literature. What are the benefits and costs of domestic financial market integration with world markets? The benefits have three components. First, uncertainty implies risk, and global capital markets allow countries to insure against that risk: “a basic function of a world capital market is to allow countries with imperfectly correlated income risks to trade them, thereby reducing the global cross-sectional variability in per capita consumption levels” (p. 6). Second, in a certain (and riskless) world, poor and/or economically unstable countries will want to borrow from world capital markets if domestic returns warrant it: “capital markets also reallocate resources over time in ways that can raise efficiency . . . [allowing] countries to smooth out . . . consumption” (pp. 8–9) when income growth and fluctuations are predictable. Once stated this way, the authors have created a path leading to a potential assessment of the gains from open capital markets. In addition, and third, "open capital markets can impose discipline upon governments that might otherwise pursue overexpansionary fiscal or monetary policies or tolerate lax financial practices by domestic financial intermediaries" (p. 9). If countries are politically too immature to impose fiscal and monetary discipline on themselves, global capital markets will do it for them.

What about the costs of open capital markets? First, market discipline is often insufficient to deter poor institutional or poor policy behavior and, in such cases, the market often inflicts "punishments far harsher than the underlying policy 'crimes' would seem to warrant" (p. 10). Indeed, not only can the (structural adjustment) punishment exceed the financial crime, but it often seems that the criminal doesn't even take the punishment. A good example of this is "odious" debt (e.g., Michael Kremer and Seema Jayachandran 2002). If an oligarchic or even despotic political regime incurs the debt before being replaced by rebellious citizens, should the new (democratic) regime be forced to repay the debt? After all, their views on issuing debt and its use were never solicited by the despot, so why should they be responsible for it? Furthermore, the new regime may be fragile and "odious" debt repayment might even shorten its life, a costly result. Second, open capital markets may erode policy autonomy and even induce a race toward the bottom. After all, "if capital is free to emigrate in the face of taxes, then either the burden of providing social services must be shifted toward labor, or those services must be scaled back" (p. 12). While the evidence for a race toward the bottom is not well supported by facts taken from rich advanced countries in the present (Dani Rodrik 1997) or the past (Michael Huberman 2002), it may be much better supported by (missing) facts from poor developing countries.

When and where were the costs of open capital markets so great as to exceed the benefits? Is there, and has there always been, an asymmetry between rich and poor countries in this regard? I am not aware of another book on global capital markets that is so bold and comprehensive in laying out this kind of impressive and policy-relevant agenda.

Consider another issue that the authors place on the table: the evolving change in the mix of capital market activity over the past 150 years. In the authors' words: "globalized capital markets are back, but with a difference. Capital transactions today seem to be mostly a rich–rich affair, consistent with the picture of modern capital flows as mostly 'diversification finance' rather than 'development finance'" (p. 241). This sounds to me like another way of stating the Lucas Paradox (Robert E. Lucas Jr. 1990): If the marginal product of capital is higher in capital scarce poor countries, how come more capital doesn't flow from rich to poor countries?

2.2 *Thinking in General Equilibrium*

Throughout *GCM,* the authors make it clear that financial capital markets, and the policies that affect them, cannot be properly assessed without explicit attention to how these markets interact with others. It might be useful to elaborate on this idea before we proceed any farther. Consider world labor markets. For some time now, economic historians of the first global century have explored the extent to which global capital chased after migrating labor, both heading for abundant third factors, like land and other natural resources (Alan Green and M. C. Urquhart 1976; Michael A. Clemens and Jeffrey G. Williamson 2004). Under such circumstances, the assessment of the impact or determinants of capital flows cannot be made without controlling for the extent of world labor market integration, migration policy and migration flows. So too, no assessment of the impact of migration can be made without controlling for world capital market integration, policies toward across border capital flows, and their magnitude. It has been shown for both the first global century (Taylor and Williamson 1997) and for the second global century (Sarit Cohen and Chang-Tai Hsieh 2000; Timothy J. Hatton and Williamson 2005) that integrated world capital markets can greatly mute the impact of labor migration in the host country. What I have just said about the connection between global capital and global labor markets also applies to the connection between global capital and global commodity markets, that is, between trade and specialization, on the one hand, and capital flows, on the other.[2]

2.3 *Road Map and Achievements*

Let me now offer a road map for the book's complex landscape and stress its many positive achievements along the way.

Chapter 1, the first part of the book, describes the evolution of world capital markets, discusses what should be included in assessing benefits and costs from open capital markets, and elaborates on the trilemma. Part 2 measures the extent to which world capital markets were open and integrated, and the approach is eclectic. In making the assessment about openness, chapter 2 looks at quantities, both the magnitudes of stocks and flows of financial capital across national boundaries, as well as the Feldstein–Horioka (1980) measurement. Martin Feldstein and Charles Horioka (FH) argued that if countries were poorly integrated with world capital markets, they would find their domestic investment constrained by domestic savings, and a regression of the former on the latter would reveal a beta coefficient near unity. While the FH premise has been qualified many times since, it still proves to be a useful device for plotting the state of world capital market integration between 1850 and the present. The FH results conform to figure 1, as do foreign investment (or current account) shares in GDP and foreign asset shares in GDP. This descriptive analysis is impressive and comprehensive, but it has two flaws. First, the authors do not offer a measure of foreign capital flows as a share of domestic investment or foreign-held assets as a share of total assets. In effect, *GCM* assumes constant average and incremental capital–output ratios across countries and over time, an assumption that must surely have been violated, and perhaps in very predictable ways. Just as migration's importance is measured as a share of host country population increase, and as foreign-born importance is measured as a share of total resident population, capital flows and foreign-held assets ought to be measured the same way, as shares of domestic investment and capital stocks. After all, we are interested in the impact of these financial flows on the world distribution of physical capital stocks and investment flows, since it is physical capital that leaves its mark on GDP, output mix, and factor rewards. Second, none of these measures really describe *world* or *global* capital markets; rather, they describe that of European industrial economies and their overseas offshoots (the core, if you will). These *GCM* measures certainly speak to the *intensive margin* in the core, but they say nothing about the *extensive margin.* That is, they say nothing about the periphery and emerging markets. I will return to this issue below since I think most of the "global" capital market action since 1850 has been at the extensive margin, and thus that much of our focus should be there.

Chapter 3 reports how the price evidence speaks to the world capital market integration assessment, and here again the analysis is impressive. The authors document 150 years of world capital market experience with nominal interest parity, purchasing power parity, and real interest rate convergence. The chapter is an empirical tour de force and I can't image any other economists

[2] To offer just one example, there has been very little work which assesses how trade (and its absence) can influence the relative price of imported machines and thus the way that financial capital inflows influence actual accumulation rates in debtor nations (William J. Collins and Williamson 2001).

doing it so well. It must also be added that the authors had to put together a panel data base over the 150 years to perform all of these quantity and price measures of world capital market integration, an impressive data base which will be greatly valued by other scholars as well. Yet, once again and somewhat uncharitably, I must report that *GCM* is not measuring 150 years of *world* or *global* capital market integration, since the "price" (interest rate) data are all taken from the core. As we shall see, we have to wait until the end of the book to learn about the rest of the world, and that will be mostly for the modern era.

While part 2 measures capital market integration in the very long run, part 3 explores the political economy of capital mobility. It starts with an impressive historical narrative in chapter 4 which ties exchange regimes with global capital market integration over a century and a half. Those readers looking for an economically sophisticated and historically informed chronicle of world capital markets from the gold standard heyday to today's floating rates, this is the chapter for you (and the one to assign to your students): it is certainly the best I have read. However, be sure you understand the trilemma before you read this chapter, since it is used extensively in the chronicle. While chapter 4 uses the *trilemma* to organize the history of world capital markets, chapter 5 actually tests whether there really is a stark trade-off between exchange stability, monetary independence, and capital market openness (p. 172). After all, the notion has been challenged both for the modern era (Andrew K. Rose 1996; Guillermo A. Calvo and Carmen M. Reinhart 2001, 2002) and the gold standard era (Bordo and Flandreau 2003). However, *GCM* offers two new and important twists to the assessment. First, the authors measure monetary independence by short-term money-market interest rates, rather than by quantity aggregates. Second, they enlarge the scope of the analysis to cover (comparatively) three major epochs, not just one (as has been common in this literature): the gold standard era (1870–1913), the convertible Bretton Woods years (1959–73), and the modern post–Bretton Woods period of float. Has the trilemma endured in the long run? The authors argue that it has: "Looking at the interest-rate data, we can see the trilemma's lessons borne out over a very broad range of historical experience" (p. 194). My guess is that the debate is not over, but even their critics will have to deal with the careful empirical analysis which Obstfeld and Taylor bring to bear on the issue. Chapter 6 extends this trilemma analysis by looking more closely at the gold standard before and after 1914. The "gold standard era" is, of course, more than just a convenient label for global events in the late nineteenth century, since observers always thought that the gold standard facilitated capital flows and trade (e.g., Herbert Feis 1931). Indeed, an important paper by Michael Bordo and Hugh Rockoff (1996) found that going on the gold standard served as a seal of approval for sovereign debt (reducing perceived risk) and, as a consequence, gold standard countries got cheaper capital. Bordo and Rockoff argued that the market acted as if gold standard countries had given up activist (inflationary) macro policies, except in wartime, and even then they rolled them back in peacetime as soon as possible. The Bordo–Rockoff thesis is not without its critics (Flandreau and Frederic Zumer 2004; Ferguson and Moritz Schularick 2006), but *GCM* offers evidence that seems to confirm the thesis for the pre-1914 gold standard. The chapter also shows that "two key macrofundamentals, the public debt and terms of trade, seem to have mattered little, if at all" (p. 224).[3] Furthermore, the chapter argues that the interwar gold

[3] One wonders whether this modest terms of trade finding would have appeared if Third World experience had been explored by itself. After all, that's where the terms of trade has undergone such great volatility in more than a century since 1870 (Christopher Blattman, Jason Hwang, and Williamson forthcoming).

standard was less credible.[4] This chapter offers a wonderful mix of economic analysis, historical anecdote and econometric analysis. For my money, this is one of the most impressive parts of the book, especially so since it now uses a country sample which truly represents the world.

Since part 4 ("Lessons for Today") concludes the book, chapters 7 and 8, at first sight, seem a wee bit out of place. By this I mean that they are so good at expanding the list of questions, I can't help but wish that the authors had placed some of the content of these chapters somewhere up front, where they would have better helped guide my assessment of global capital markets as I read the book. This is especially true of chapter 7, which I think is the best chapter in *GCM*. The reason I think so is because it is here that the authors pursue what I have called the extensive margin:

> The new financial globalization is for the most part confined to rich countries. A handful of developing countries ("emerging markets") also participate to some degree, but most other developing countries are left out . . . If capital market participation includes some countries but not others . . . why, and with what effects? Exclusion may be the results of market failure, or it could be accounted for by [local] institutions and policies. Inclusion may bring benefits, as well as costs, and the tradeoff can tell us whether policies to promote further integration are advisable, and for whom (p. 230).

I find this a tremendously stimulating statement and the chapter delivers a lot to inform it. For starters, we are shown how capital flows in our current global century differ so greatly from that of the first global century. Before 1914, "the principal flows were long-term investment capital, and virtually unidirectional" (p. 231), and net and gross flows were pretty much the same. Not so in the current global century. For example, "the United States became . . . the world's largest net debtor nation. But while accounting for the biggest national stock of gross foreign liabilities, the United States *also* held the largest stock of gross foreign assets" (p. 231). What makes the recent episode so different from the pre-1914 period is that there is a much larger volume of debt swapping, that is mutual diversification and risk sharing. Why the difference, and does it imply that the gains from modern financial flows have been much more modest than earlier? In addition, the authors show that net capital flows today are largely a North–North affair, with North–South flows pretty modest as a share of the total. While the Lucas Paradox is an attribute of both global centuries, the authors argue that the paradox is more apparent today (see also Clemens and Williamson 2004). Why the difference? Could it be that poor countries today have been less active in liberalizing their financial markets than did poor countries in the first global century? The authors offer some evidence on the role of policy and institutions which leaves the reader with the distinct impression that policy and institutions could very well explain the difference. Many of these themes are continued in the concluding chapter 8, where the stress is on the costs and benefits to going open, and their variance over time and place. I found this a great place to end the book, since it leaves the reader with a spectacular agenda.

3. *Unanswered Questions: A Research Agenda*

I confess that I am a sucker for books that leave an exciting research agenda in their wake and *GCM* excels at that. Let me just list some of these issues.

History has generated two sets of data that are relevant for the questions raised in *GCM*. The first is the country panel data from 1850 to the present, data used with such effectiveness by the authors. But what about the second? Regions have been forming federations for some time now, ones in

[4] There is disagreement between Bordo, Michael Edelstein, and Rockoff (1999) and the authors of *GCM* about whether the "good housekeeping seal" operated in the 1920s and 1930s.

which we think the institutional and policy barriers to financial capital flows are much lower. So, have flows across state borders in the United States, the German Zollverein, Brazil, the European Union and elsewhere had the same or different characteristics than those we observe crossing their common borders to the rest of the world? Did the political economy of capital markets obey different laws of motion within federations as between them? Has the evolution of financial capital markets within countries comoved with global capital markets? If so, why? If not, why not? And, perhaps most important, has the timing of crises, their magnitude, and reform reaction been the same within federations as between them?

A key weakness of the global capital markets literature is that it rarely assesses empirically its impact on the real economy. This seems a bit odd given that the literature on migration and trade is all about impact on the real economy. Nor does it often tell us whether policy, institutions, or other (independent) forces account for most of the instability and trends we observe in the global capital market. It seems to me that economists should be confident enough about their models to be able to decompose the sources of global capital market booms and slumps. This state of affairs also seems a bit odd given that the literature on migration and trade is all about policy impact. Indeed, the trade literature suggests that going open explains only a small share of the trade booms we observe out there.[5] Is the global capital market any different? Does policy lead or follow?

[5] It appears that two-thirds of the OECD trade boom between the late 1950s and the late 1980s is explained by income growth, not by going open (Scott L. Baier and Jeffrey H. Bergstrand 2001). Similarly, it appears that the trade boom between 1870 and 1939 is explained mainly by declining transport costs and income growth, not by changing trade policy (Antoni Estevadeordal, Brian Frantz, and Taylor 2003; David S. Jacks, Christopher M. Meissner, and Dennis Novy 2006). As a final example, it appears that two-thirds of the European overseas trade boom between 1500 and 1800 is explained by income growth and other local forces, while none of it is explained by more pro-global policy (Kevin H. O'Rourke and Williamson 2002).

While the literature on the benefit and cost of having open capital markets is thin, it looks like *GCM* and other new literature will soon change all that. Indeed, a survey by M. Ayhan Kose et al. (2006) reports a recent boom in interest on this question. Here's what the survey reports: There is still no robust evidence that supports the view that broad capital account liberalization benefits growth. However, it appears that equity market liberalization *does* significantly augment growth. In addition, there is little evidence to support the view that financial globalization leads to deeper and more costly crises in developing countries. These findings are, of course, based on very recent evidence. One can only hope that the appearance of *GCM* will provoke economists to look farther back in time to see whether their findings are specific to the modern era and, if so, why.

Exactly how does the kind of financial capital matter? We have no shortage of economic opinion on how and why sovereign debt, private equity and foreign direct investment (FDI) should have different impacts on the real economy, and perhaps even be driven by different factors. This is especially true of FDI, which we think facilitates the transfer of technology. But where is the empirical analysis that makes the comparative assessment between the three and over time? And where is the empirical analysis that deals with the obvious endogeneity of the mix between the three over time?

What about the political economy of financial capital markets? To the extent that unemployment is the central distributional variable that matters, and to the extent that macro policy helps drive the unemployment rate, then the trilemma certainly is a useful tool to assess the trade-offs driving policy. But what about an empirical test of these political economy forces? *GCM* offers 150 years of regime changes constrained by the trilemma, so the opportunities for exploring those forces are abundant, including the importance of who gets to vote. To take one example, I have in mind recent work on who went on the gold standard, when, and why (Meissner 2005). I can only hope that soon we will have a similar literature explaining who went on and off the gold standard in the interwar years (and why), who in the Third World went antiglobal first after the 1940s (and why), who in the Third World went pro-global first after the 1960s (and why), and so on. We need to know more about how the political economy works, although the trilemma is a useful way to start at the most macro level.

4. *Bottom Line*

Global Capital Markets: Integration, Crisis, and Growth is a wonderful book that was badly needed. It will be cited extensively by those with interests in the long run evolution of the world financial capital market. While I may have seemed critical here and there in this review, it is because the authors have tried to answer more and much tougher questions than their predecessors did. This is not just another book about the efficiency of global capital markets, but rather adds significantly to debates about instability and crisis, about asymmetry in the costs of going open, about the Lucas Paradox, about the connections between foreign exchange and financial capital market regimes, and much more. Furthermore, this book makes much better use of the comparative evidence available since 1850 than do competitors which focus solely on one regime—the late nineteenth century classical gold standard, the interwar disaster, post–World War II Breton Woods, or the float since. In addition, the book offers something more. While in the audience listening to papers on global finance in the past, I have often found myself wondering what exactly this paper thinks is the connection between international finance and financial history, on the one hand, and the real economy, on the other. I have always felt that the financial literature rarely assesses that connection in any useful empirical way. This book makes that connection refreshingly clear, even if it opens itself to more criticism by so doing. Few other books on global capital markets stimulate the critical reaction that *Global Capital Markets* does simply because it is a much better book with a much wider and deeper reach. I strongly urge you to read it.

References

Baier, Scott L., and Jeffrey H. Bergstrand. 2001. “The Growth of World Trade: Tariffs, Transport Costs, and Income Similarity.” *Journal of International Economics,* 53(1): 1–27.

Blattman, Christopher, Jason Hwang, and Jeffrey G. Williamson. Forthcoming. “The Impact of the Terms of Trade on Economic Development in the Periphery, 1870–1939: Volatility and Secular Change.” *Journal of Development Economics.*

Bordo, Michael D., Michael Edelstein, and Hugh Rockoff. 1999. “Was Adherence to the Gold Standard a ‘Good Housekeeping Seal of Approval’ during the Interwar Period?” NBER Working Papers, no. 7186.

Bordo, Michael D., and Marc Flandreau. 2003. “Core, Periphery, Exchange Rate Regimes, and Globalization.” In *Globalization in Historical Perspective,* ed. M. D. Bordo, A. M. Taylor, and J. G. Williamson. NBER Conference Report series. Chicago and London: University of Chicago Press, 417–68.

Bordo, Michael D., and Hugh Rockoff. 1996. “The Gold Standard as a ‘Good Housekeeping Seal of Approval.’” *Journal of Economic History,* 56(2): 389–428.

Calvo, Guillermo A., and Carmen M. Reinhart. 2001. “Fixing for Your Life.” In *Brookings Trade Forum 2000,* ed. S. M. Collins and D. Rodrik. Washington, D.C.: Brookings Institution, 1–38.

Calvo, Guillermo A., and Carmen M. Reinhart. 2002. “Fear of Floating.” *Quarterly Journal of Economics,* 117(2): 379–408.

Clemens, Michael A., and Jeffrey G. Williamson. 2004. “Wealth Bias in the First Global Capital Market Boom, 1870–1913.” *Economic Journal,* 114(495): 304–37.

Cohen, Sarit, and Chang-Tai Hsieh. 2000. “Macroeconomic and Labor Market Impact of Russian Immigration in Israel.” Unpublished.

Collins, William J., and Jeffrey G. Williamson. 2001. “Capital-Goods Prices and Investment, 1870–1950.” *Journal of Economic History,* 61(1): 59–94.

Della Paolera, Gerardo, and Alan M. Taylor. 2001. *Straining at the Anchor: The Argentine Currency Board and the Search for Macroeconomic Stability,* 1880–1935. NBER Series on Long-term Factors in Economic Growth. Chicago and London: University of Chicago Press.

Estevadeordal, Antoni, Brian Frantz, and Alan M. Taylor. 2003. “The Rise and Fall of World Trade, 1870–1939.” *Quarterly Journal of Economics,* 118(2): 359–407.

Feis, Herbert. 1931. *Europe, the World's Banker, 1870–1914*. New Haven: Yale University Press.

Feldstein, Martin, and Charles Horioka. 1980. "Domestic Saving and International Capital Flows." *Economic Journal*, 90(358): 314–29.

Ferguson, Niall, and Moritz Schularick. 2006. "The Empire Effect: The Determinants of Country Risk in the First Age of Globalization, 1880–1913." *Journal of Economic History*, 66(2): 283–312.

Flandreau, Marc, and Frederic Zumer. 2004. *The Making of Global Finance 1880–1913.* Development Centre Studies. Paris and Washington, D.C.: Organisation for Economic Co-operation and Development.

Green, Alan, and M. C. Urquhart. 1976. "Factor and Commodity Flows in the International Economy of 1870–1914: A Multi-country View." *Journal of Economic History*, 36(1): 217–52.

Hatton, Timothy J., and Jeffrey G. Williamson. 2005. *Global Migration and the World Economy: Two Centuries of Policy and Performance.* Cambridge: MIT Press.

Huberman, Michael. 2002. "International Labor Standards and Market Integration before 1913: A Race to the Top?" Paper presented to the conference on *The Political Economy of Globalisation: Can the Past Inform the Present?* Trinity College, Dublin (August 29–31).

Jacks, David S., Christopher M. Meissner, and Dennis Novy. 2006. "Trade Costs in the First Wave of Globalization." Unpublished.

Kose, M. Ayhan, Eswar Prasad, Kenneth Rogoff, and Shang-Jin Wei. 2006. "Financial Globalization: A Reappraisal." Unpublished.

Kremer, Michael, and Seema Jayachandran. 2002. "Odious Debt." *Finance and Development*, 39(2): 36–39.

Lucas, Robert E., Jr. 1990. "Why Doesn't Capital Flow from Rich to Poor Countries?" *American Economic Review*, 80(2): 92–96.

Mauro, Paolo, Nathan Sussman, and Yishay Yafeh. 2006. *Emerging Markets and Financial Globalization: Sovereign Bond Spreads in 1870–1913 and Today.* Oxford: Oxford University Press.

Meissner, Christopher M. 2005. "A New World Order: Explaining the International Diffusion of the Gold Standard, 1870–1913." *Journal of International Economics*, 66(2): 385–406.

O'Rourke, Kevin H., and Jeffrey G. Williamson. 2002. "After Columbus: Explaining Europe's Overseas Trade Boom, 1500–1800." *Journal of Economic History*, 62(2): 417–56.

Rodrik, Dani. 1997. *Has Globalization Gone Too Far?* Washington, D.C.: Institute for International Economics.

Rose, Andrew K. 1996. "Explaining Exchange Rate Volatility: An Empirical Analysis of 'The Holy Trinity' of Monetary Independence, Fixed Exchange Rates, and Capital Mobility." *Journal of International Money and Finance*, 15(6): 925–45.

Taylor, Alan M., and Jeffrey G. Williamson. 1997. "Convergence in the Age of Mass Migration." *European Review of Economic History*, 1(1): 27–63.