PROGRESS WITHOUT ORDER: MEXICAN ECONOMIC HISTORY IN THE 1990s

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RESUMEN

Este artículo revisa el desarrollo más reciente en el campo de la historia económica mexicana. Argumenta que en los últimos diez años ha ocurrido un cambio significativo en la metodología, se ha pasado de la teoría de la dependencia y el análisis institucional tradicional, hacia el uso extensivo de técnicas cliométricas e hipótesis tomadas de la Nueva Economía Institucional. Este cambio ha aumentado nuestro conocimiento de diversas claves de la historia económica de México. Ciertamente el área ha experimentado un progreso importante, pero todavía falta un cierto ordenamiento.

ABSTRACT

This article surveys the most recent developments in the field of Mexican economic history. It argues that the last ten years has seen a significant shift in the methodology and focus of the field, away from dependency and traditional institutional analysis, and towards the extensive use of cliometric techniques and hypotheses drawn from the New Institutional Economics. This shift has greatly increased our understanding of several key issues in Mexico's economic history, but much of the work done to date has lacked a coherent focus on a specific set of issues. In other words, the field has recently made astounding progress, but still lacks sufficient order.

Unlike their counterparts in many other countries, Mexican historians have always concerned themselves with questions about long-term eco-

conomic growth. The reasons are not surprising. After all, Mexican history (and modern Mexico) is dominated by social, institutional, and political developments resulting from—or seen as having resulted from—slow rates of economic growth and an extremely unequal division of income. In that sense, Mexican historians have never enjoyed the luxury of their compatriots in Europe and the United States of studying social or political events in an economic vacuum: understanding the causes and consequences of the widespread poverty around them has been too important. This paper is a rough survey of recent developments in the study of Mexico's economic history, and addresses two questions about the recent literature. What have Mexican historians learned from the quantitative approaches to the study of economic growth and change that have dominated American economic history since the 1970s? Conversely, what can scholars of economic growth and the economic effects of institutions learn from studies of Mexican history?

While not lacking motive, until recently the economic historiography of Mexico has lacked method. In other words, although Mexican historians have concerned themselves with fundamentally economic questions, they have (with some important exceptions) failed to effectively apply explicit economic models and the effective use of quantitative data in understanding specific historical problems. This, however, has changed in recent years, as more and more economic historians of Mexico explicitly adopt methods taken from the (no longer so) New Economic History and the (still rather) New Institutional Economics. Nevertheless, the social science revolution in Mexican economic history is not yet complete. In some cases, scholars have adopted the language of social science history without fully understanding the substance of its emphasis on logical consistency, the explicit use of social science theory, data quality, and clearly specified hypotheses. In others, particularly in history departments in the United States, a backlash against the use of techniques in the social sciences has developed.

Mexican history offers an excellent laboratory for the study of several important issues in economics today. First, the question «Why is Mexico poor?» is of intrinsic interest to both current policymakers in Mexico and the US and scholars of economic growth. For example, judging NAFTA's long-term effects requires an understanding of both the economic effects and political origins of the trade policy that preceded it. Second, Mexican history provides an excellent set of natural experiments to test various hypotheses from the New Institutional Economics about the causes and effects of institutional changes. The U.S. annexations of 1848 and the

Mexican Revolution are the most obvious, but the history of Mexico is replete with others.

This essay is structured in the following manner. Section I reviews the «History of Mexican Economic History», and advances some hypotheses about why the field turned away from cliometrics and the use of neoclassical economic theory for so long, and then changed direction again with such rapidity in the 1990s. Section II briefly reviews some of the most recent developments in the literature, broken down not by chronological period (as is traditional) but by *question*: e. g. the origins of labor and land tenure systems, the effects of political stability on growth, the importance of finance, international trade, etc. Section III asks «Where Do We Go From Here?» and attempts to identify some of the most potentially fruitful avenues for future research.

A BRIEF HISTORY OF MEXICAN ECONOMIC HISTORY

Until the 1930s, Mexican economic history was dominated by the study of the colonial period. This is not to say that there were no rigorous studies of the nineteenth and early twentieth centuries. Nevertheless, the paucity is striking. In 1933 the economic historian Luis Chávez Orozco wrote that no serious studies of economic history had been written in Mexico since 1901¹. Two years later Harvard University's bibliography of the *Economic Literature of Latin America* contained no entries under «economic history».

To an extent, this represented a failure of research (or of definition) on the part of Chávez Orozco and the scholars in Cambridge. A number of studies of varying quality had appeared concerning the effects of external debt, the banking system, the railroads, and the ever-present problem of uncertain property rights over agricultural land². Gabriel Mancera's studies of the benefits from the railroad even employed a primitive form of social savings analysis that would not appear foreign to Fogel or Fishlow³. Never-

¹ Chávez Orozco (1933-36), p. 2.

² For studies of the repercussions of external borrowing, see Casasús (1885), Macedo (1905); Díaz Dufoo (1918) and McCaleb (1912). For the banking system, see Labastida (1889), Casasús (1890), Manero (1926), and McCaleb (1930). The most important works on the Mexico's post-colonial land structure, polemical as most of them are, are Molina Enríquez (1909), González Roa and Covarrubias (1917), González Roa (1919), and Phipps (1925).

³ Gabriel Mancera calculated the social savings of his railroad, the Ferrocarril de Hidalgo y Nordeste, assuming a perfectly inelastic demand for transport. Government officials

theless, the use of economic theory in these studies was generally ad-hoc, and the use of data highly selective.

In the USA, economic history experienced a minor revolution in the 1960s with the arrival of what become known as the New Economic History, econometric history, Cliometrics, or simply Clio. Its practitioners applied economic theory and econometric techniques to historical data, and in so doing revolutionized our understanding of American economic history. These New Economic Historians made progress along three fronts.

The first was the measurement of long-term factors in American economic growth. (In fact, one of the most important edited volumes in US economic history is named just that: *Long-Term Factors in American Economic Growth*, edited by Stanley Engerman and Robert Gallman.) This work started with attempts to extend GDP series for the US before World War II, but soon grew to investigate a huge array of questions about the sources of growth, including changes in the capital stock, mortality and fertility rates, immigration, labor markets, transportation technology, nutritional levels, and educational standards. In so doing, these scholars overturned a number of standard interpretations about US growth, and explained a number of long-standing mysteries. For example, Paul David discovered that most US growth in the 19th century was due to increases in the capital stock: TFP growth was surprisingly low. Gavin Wright applied trade theory to argue that capital-intensity after 1870 was simply a complement of natural resource intensity⁴. Robert Fogel (1964) employed social savings analysis to argue that the railroads' contribution to growth was minimal. Christina Romer (1986a, 1986b) argued that business cycle swings were less pronounced before the Great Depression than had been commonly believed. This work soon moved beyond aggregate studies of the entire economy to more detailed studies of productivity growth in specific economic sectors —transport, agriculture, manufacturing— and regional variations.

Soon, these economic historians moved from measuring the sources of growth to investigating the technologies and institutions behind it. Behind changes in savings rates, population distributions, and capital formation lay important technological and organizational innovations. But which innovations were important and which were not? Scholars first began

attacked his estimates on this ground. The debate was surprisingly sophisticated, and is summarized in Coatsworth (1981), pp. 110-13.

⁴ Wright (1990), pp. 651-68.

rigorously asking this question about the effects of slavery on the economic development of the Southern United States and the impact of the United States's peculiarly decentralized financial system on the structure of the national economy, but soon began to pose and test hypotheses about the causes and consequences of inventive activity, consumer credit, industrial organization, racial discrimination, land tenure, and regulatory reforms.

The final front grew naturally from the previous line of investigation: applying economic theory to explain institutional change. In this, economic historians like Douglass North and Lance Davis began to dovetail with the interests of the New Institutional Economics, which grew from the work of Ronald Coase (1988), Mancur Olson (1965), and Oliver Williamson (1993), all of whom theorized about how institutional change makes economic activity more or less efficient by changing uncertainty and transaction costs. Practioners of the New Institutionalism in both economics and political science (where the field is known as positive political economy) increasingly turned to history as the only laboratory available in which to test hypotheses derived from these new theories.

For a brief period in the 1950s and 1960s, it appeared that Mexican economic history might follow in the footsteps of the New Economic History being developed in the United States. As it turned out however, very few economic historians actually followed this lead, Clark Reynolds's book (1970) on the Mexican economy in the 20th century and John Coatsworth's tome (1981) on the railroads being the major exceptions.

As Stephen Haber (1977a) argues, three factors impeded Clio's arrival in Latin America. The first was that few Latin Americans (including Mexicans) who received economics Ph.Ds in the United States or United Kingdom actually entered academia upon their return home. The opportunities available in state service or private enterprise were simply too enticing. Second, computing power was extremely expensive outside the US. To this factor could be added another: data sources in Latin America were (and are) dispersed, archival, and difficult to collect compared to the United States. Not only was crunching numbers more expensive, but getting numbers to crunch was far more difficult. Third, and perhaps most importantly, the spread of dependency theory rejected the use of neoclassical economics and the specification of clearly testable hypotheses.

Dependency theory grew out of a serious criticism of traditional ideas about economic growth. Based on the experience of the Great Depression, Raúl Prebisch (1950) argued that trade was not always good, since a country's static comparative advantage could lock it into producing a good

in which the opportunities for future growth were limited, or nonexistent. W. Arthur Lewis (1954, 1955) argued that flat marginal productivity of labor in the rural sector and capital market imperfections meant that improvements in industrial productivity might not lead to improvements in living standards in underdeveloped economies. Albert Hirschman (1958) argued that dual labor markets and large economies of scale made industrialization impractical unless specific industries were specially targeted with programs to increase their profitability. In addition, Alexander Gerschenkron (1962) influentially argued that high capital requirements due to growing economies of scale and a scarcity of entrepreneurial talent meant that large banks would and the state could and should play a leading role in the industrialization of «latecomer» nations, like Mexico.

These arguments could be thought of as «hypotheses to be tested». Is the marginal productivity of agricultural labor flat in underdeveloped economies? Do underdeveloped countries have a harder time shifting resources from one sector to another than developed economies did at a similar level of development? Did the terms of trade decline over time? How big are economies of scale in new industries? However, that's not what they became. Hirschman and his group rejected explicit modelling of their ideas. At first this was because the modelling techniques of the time were inadequate for their concepts, but their reliance on «suggestive metaphors» and a «relaxed attitude toward internal consistency» meant that their work was vulnerable to being hijacked by others for political purposes. Since dovetailed with a prevailing ideological current and justified growth models that most Latin American governments would have probably been undertaking anyway, these precepts became gospel for a generation of Latin American —and Mexican— historians. Since the movement, and Robert Packenham (1992) has aptly described it as such, was fundamentally political, it rejected both the use of consistent economic models and the presentation of hypotheses in an empirically verifiable manner. In this, however, the dependendistas were merely following in the footsteps of the founders of development theory, and compounding their mistakes.

In fact, when economists and economic historians tried to operationalize and test the implicit hypotheses of dependency, they discovered that the facts did not support the theory. The terms of trade did not systematically decline. Latin American nations, particularly Mexico, subsidized and protected national industry in the 19th century. Mexico tightly regulated foreign capital before the Revolution. Isolation from world markets during world wars or the Great Depression did not prompt sustainable growth or new

investment. The onset of industrialization coincided with an export boom. And so on ⁵.

And so the model collapsed. The collapse was not simply in academia, but also in the real world, as more and more people came to see the import-substituting and state-led industrialization models pursued by Latin American countries as failures. This combined with three other trends to produce the current flowering of New Economic and New Institutional history in Mexico and the rest of Latin America.

The first was an upsurge in interest in questions of economic growth and development by economists in the United States. In part, this was a reaction to «globalization» and the fall of the Soviet empire. On the one hand, more and more countries appeared to adopting institutions resembling those in Western Europe and North America and opening themselves up to international flows of goods and capital. This led economists to wonder what lessons if any might be held in the last great period of globalization before the First World War ⁶. In addition, the growing prominence of «emerging market economies» increased interest in understanding their economic history as it became clear that these nations were not relatively poor due to some putative current factor but because they fell behind in the growth race some 100 or 200 years previously.

The collapse of Communism revived academic interest in institutions and their effect on economic growth, as the world confronted the problem of constructing functioning capitalist institutions in the countries emerging from the rubble of the Soviet Bloc. The economic history of Latin American countries was of particular interest as an example of an area where markets and market institutions did not function well. This dovetailed with a growing academic interest in the effects of institutions on economic performance, as economists sought to use history as a laboratory with which to test hypotheses emerging from growth economics, the New Institutionalism, and advances in game theory (aka positive political economy) ⁷.

In short, development economics collapsed as a separate field from the rest of economics, and became in part a subfield of economic history, as economists like Jeffrey Williamson, Alan Taylor, Rosemary Thorpe, Vincent

⁵ See, for example, Powelson (1979), Cárdenas (1987), Haber (1989), and Beatty (1996).

⁶ See Taylor and Williamson (1997): 27-63, Williamson (1996), Hatton and Williamson (1994), Bordo (1993), Green and Urquhart, (1976), Taylor (1997), Collins, O'Rourke and Williamson (1997), and Obstfeld and Taylor (1997).

⁷ Barzel (1989), Libecap (1993), Alt and Shepsle (1994), Ostrom (1990), Banks and Hanushek (1995), and Landau, Taylor and Wright (1996).

Bulmer-Thomas, Roberto Cortes Conde and others began apply techniques borrowed from Cliometrics and the New Institutionalism to test hypotheses about long-term economic growth in the «Third World»⁸. Two more trends also came together during this period: the cost of computing power fell, and the relative attraction of academic careers in Latin American nations rose. The cost of computing needs no explanation, and I leave the increasing attractiveness of academic careers (at least in Mexico) to a future generation of economic historians⁹. What this means for Mexican economic history is that since the mid-1980s, but particularly in the last five years or so, its practitioners in economics departments in the US, UK, and Mexico have increasingly adopted the rules of «scientific» history. In turn, our understanding of Mexican economic history has greatly advanced, and economics in general has obtained an excellent laboratory for the testing of theory.

If there is one way, however, in which Mexican economic history of the 1990s differs from the New Economic History of the US in the 1960s and 1970s, it is the relatively greater attention given to the role of institutions in reducing (or enhancing) transactions costs and how the specification of property rights creates incentives to improve productivity. This focus is a result of two factors. First, Mexican history is dominated by cases of poorly-specified property rights and *apparently* badly-functioning institutions. Unlike the United States, economic historians naturally gravitate towards explaining the causes and consequences of these phenomena

⁸ Paul Krugman has recently argued that new modelling techniques are allowing «high development theory» to make a comeback. This is an entirely different issue: economists will almost certainly use history as a laboratory to test the hypotheses that emerge from the «new development theory.» As an empirical field, development economics is still a subfield of economics, but it is micro-oriented. Its practioners —Chris Udrey comes to mind— look at issues like household savings and investment at the individual or family level.

⁹ I will, however, advance what may be a rather controversial hypothesis: the simultaneous collapse of the state-led model of economic growth and the PRI's hegemony over Mexican politics opened the door to the serious study of economic history in Mexican universities. Basically, the hypothesis is that before the late 1980s university research was dominated by the state, which would tolerate (indeed, at times encourage) *dependentista*-style criticisms of the world economic order. This is because they simultaneously justified government policies, and allowed the government to safely park leftist critics in university posts. Beginning in the late 1980s, however, private interests began donating substantial sums of money to the private university system, and the government no longer saw the public universities as convenient repositories for potential dissidents. David Lorey's excellent work on Mexican universities supports this hypothesis, but several more tests are needed. Have real academic salaries in the private universities increased? Has the allocation of public research money changed? Has academic productivity in economics departments increased? See Lorey (1993).

—or to determining whether institutions really functioned as badly as the conventional wisdom suggests. Second, it is precisely in testing hypotheses about the effects of institutions and property rights that Mexican history is most useful as a laboratory for economic theory.

RECENT CURRENTS IN MEXICAN ECONOMIC HISTORY

Recently Mexican economic history has rapidly adopted the explicit use of economic theory to frame hypotheses and the use of quantitative techniques to test them. The most promising, and, indeed, the most difficult current in the field is the attempt to apply formal theory to understand how institutions begin, develop, and structure economic change. This line of work has seen its most exciting results in the study of the colonial era. Scholars operating in the New Institutional Economics are finally operationalizing and testing the question «Does Mexico suffer from a colonial heritage?» However, there has also been strong recent research into the effects of political instability on economic growth and industrial structure. Recent work by Aurora Gómez-Galvarriato, Stephen Haber, and Armando Razo has done much not only to enhance our knowledge of Mexico's economic history, but our theoretical understanding of how political instability and property rights regimes interact with economic growth.

Progress is also being made along other fronts. Part is work with a very strong national accounts orientation, aimed at settling debates about the periodization of Mexican economic growth. Richard Salvucci's recent work is in this vein. Similar work has focussed on understanding the effects of particular public policy regimes and regulatory structures on the economy's overall performance: Enrique Cárdenas's work on the policies of President Lázaro Cárdenas (no relation, as far as I am aware) is a case in point, as are studies by Sandra Kuntz and Arturo Grunstein of the political economy of Porfirian regulatory policy.

INSTITUTIONAL CHOICE

One of the oldest debate's in Mexico's economic history is over the nation's putative «colonial heritage». John Coatsworth's (1990) seminal article concluded that all of the putative obstacles to growth established under the Spanish Empire were swept away at independence, with the

exception of two: terrible geographic barriers and insecure property rights. This analysis, however, begged the question of why property rights were insecure in Spanish America, or how the evidently economically dysfunctional institutional order that the nation was to inherit came into being and persisted for so long.

Timothy Yeager (1995) took up part of this question in a recent article about the *encomienda* in colonial New Spain. The *encomienda* was a system under which the Spanish crown assigned a «restricted set of property rights» over Indian labor, under which the *encomendero* could extract a certain amount of tribute in goods or services from the Indians in return for a lump sum tax paid to Madrid and a promise to defend the area and Christianize the natives. The *encomiendas* were not inheritable, nor tradable, nor rentable, nor could the *encomendero* move the Indians to better locations. Yeager wants to explain why the Crown chose the *encomienda* over alternative labor systems. First, he establishes that there is indeed a mystery to be solved. He lays out the revenue maximizing problem faced by the Crown, and using available data demonstrates that the Crown's strategy of charging a (roughly) 20% tax and confiscating *encomiendas* produced less revenue than an alternate strategy of raising tax rates with no confiscations. In addition, he carefully states the logic behind his assertion that the limits on the *encomienda* were of necessity economically inefficient. He then hypothesizes that the Crown preferred the *encomienda* to a more economically «rational» strategy of enslaving the Indians in order to quickly extract rents from the Indian population, without establishing a politically powerful class of perpetual *encomenderos* or slaveowners, who could potentially enjoy the rents from enslavement without passing them on to the Crown. He tests the hypothesis by demonstrating the Crown confiscated the largest *encomiendas* first, since they posed the largest threat to its political control.

Parts of Yeager's logic have come under attack from Mario Pastore, but the usefulness of the approach in attempting to understand institutional choice is clear¹⁰. Stanley Engerman and Kenneth Sokoloff take the approach further. They argue that economies with factor endowments that favored high value export crops like sugar, coffee, tobacco, or cotton, pro-

¹⁰ Pastore (1998). The substance of Pastore's argument is threefold. First, Yeager ignores evidence that free wage labor was viable in the New World from early on. Second, he underestimates the ideological opposition to slavery from the Church. Third, the logic behind Yeager's argument that the Crown confiscated *encomiendas* to maintain its political control is not always clear. Fourth, Yeager underestimates agency costs facing the Crown. Therefore, the *encomienda* was a first-best solution.

duced most efficiently on large plantations using slave labor, gave rise to institutions which exacerbated an unequal distribution of wealth. In the case of Mexico, they argue that the vast «endowments» of native labor gave rise to institutions like the *encomienda*, which was then transformed into a highly unequal pattern of landholding and access to political power. However, they do not provide details over the proposed mechanism through which this might have occurred ¹¹.

Elinor Melville's (1997) recent environmental history of the Valle de Mezquital in the modern-day state of Hidalgo provides some interesting leads into this process. In essence, Melville argues that traditional grazing laws that worked in Spain failed in Mexico. In her words: «Then, as today, common grazing only works when all parties agree to the rules governing the use of specified areas of land». Essentially, she argues that in Mexico the native population had never developed rules governing the use of open grasslands, since they possessed no domesticated grazing animals, and the rules governing the Spanish introduction of sheep grazing were designed for a intricate web of municipal control and traditional land rights that did not exist in the New World. In addition, the Indians had no idea how to deal with grazing animals, even when they retained title to their land and the official right to prevent the animals from entering. The result was the Tragedy of the Commons on a huge scale. Melville musters data to show that the *hacienda* grew not from the *encomienda*, but from the expansion of pastoralism and the resulting precipitous decline in land quality. Given the structure of Spanish land rights, temporarily fallow lands became open for grazers, which quickly destroyed their quality, leading to their abandonment and easy purchase by Spaniards. Eventually the pastoral economy collapsed, leaving a pattern of huge *latifundia* and small, isolated Indian villages.

Of course, this work is only just beginning, but it shows the strength of applying neoclassical and property rights theory to the available data in order to test hypotheses about institutional change. In addition to greatly enhancing our historical knowledge about how and why New Spain's institutions took the form they did, this new literature is beginning to frame the question of Mexico's «colonial heritage» in operationalizable and testable ways.

¹¹ Engerman and Sokoloff (1997). One should note here that Sokoloff and Engerman are directly disputing one of Coatsworth's main assertions: that the unequal structure of land tenure in New Spain did not directly effect the colony's ability to grow after independence.

POLITICAL INSTABILITY AND ECONOMIC GROWTH

Mexican history has seen at least two bouts of extreme political and institutional instability: the half-century following the beginning of the Independence Wars in 1810, and the two decades following the fall of the Porfiriato in 1911. The effects of both periods have been the cause of much polemic in the traditional historiography. More than that, both provide excellent test cases for several of the propositions of the New Institutional Economics, namely that decreases in the stability of property rights should reduce incentives to invest and slow economic growth.

John Coatsworth has argued that New Spain's silver industry was already in decline before independence. However, a recent article by Carlos Ponzio de León (1998) has cast some doubt on this idea. Using new data on corn prices gathered by Richard Garner, Ponzio demonstrates that real production was indeed increasing in Mexico through 1800. He also argues that this upsurge in production was due to royal policies which reduced the cost of production (namely drops in the price of mercury and in the tax burden), meaning that more of the benefits from silver production remained in Mexico, and that this drop in unit costs was greater than the drop in the unit price of silver. Therefore, his calculations of the real increase in silver output provide a minimum estimation of the benefits from the increase production. If silver production was not falling in the last years of the colony, then what provoked the substantial decline in economic output after independence? And to take the question back another step, did incomes really fall? In other words, how bad was the early Nineteenth Century in Mexico?

Enrique Cárdenas and Richard Salvucci (1997) have revisited this question, and their conclusion is that there was indeed a fall in income after independence, and the collapse of the silver mining industry is to blame. Nevertheless, they argue that the result was better described as prolonged stagnation than a severe depression. Salvucci begins by reviewing the best-known estimates of Mexican national income in the late colonial period, most of which (perhaps surprisingly) are based on a 1817 estimate made by José María Quirós, the executive secretary of the Veracruz merchants' guild. He then conceptually dismisses the low estimates of national income from 1836, which severely undercounted domestic production, and relies upon the 1839 estimates of the Instituto de Geografía y Estadística. The Instituto computed national product on the basis of land values, arguing that an increase in land values implies an increase in the stream

of future income associated with that property. Of course, as Salvucci points out, increased risk could mean that property values rose more slowly than risk, but this would imply that the Instituto's estimates provide a floor of 4% per capita growth over the period. To cross-check this, Salvucci conducts a back-of-the-envelope calculation of national income using data on individual expenditure, and comes up with a result of no growth. As for the direct costs of the insurgency, Salvucci argues that they could not have been large, since most of the population of New Spain was at the basic subsistence level to begin with and any major decline in output should have produced mass starvation.

That is not to say that the insurgency had no costs: both Salvucci and Cárdenas argue that the collapse in the mining industry had long-term effects on the economy. In essence, the argument is that the collapse in mining (and capital flight associated either with Spanish exactions to fight Napoleon or the insurgents across Latin America) generated monetary astringency and left little capital available to the rest of the economy. This depressed the volume of trade by creating a scarcity of monetary exchange and continual balance of payments crises. The depressed volume of trade hit exactly those sectors which would have been best equipped to finance reconstruction (or the creation of a modern transport system). Only in the 1860s did recovery commence.

Of course, this analysis begs certain questions. Why, for example, were the domestic capital markets unable to mobilize savings for reconstruction? Carlos Marichal (1997) argues that this was due to the government's financial irresponsibility. Essentially, Mexicans, as in other countries, needed to learn that a piece of paper representing wealth was indeed worth more than the paper upon which it was printed, but that the continuing debt repudiations by governments prevented the emergence of a stable capital market until the early 1880s. Governments were fiscally irresponsible because Mexico was politically unstable: once stability was re-established, fiscal irresponsibility diminished, at least until instability re-emerged after 1910.

Beatriz Armendáriz's (1993) work strengthens this conclusion in a slightly different context. She develops a simple model of bond prices in the absence of current payments and shows that government debt moratoria from 1824 through 1930 significantly raised the cost of capital. In addition to indirectly supporting Marichal's argument, she also casts doubts on some theoretical arguments by Jeffrey Sachs and Carlos Díaz-Alejandro that debt moratoria can raise the price of external debt if the government credibly invests the suspended interest payments. Mexican governments in the

1870s, 1880s, and 1920s arguably did just that, but the price of their debt was not helped, and in addition they may have retarded or set back the development of efficient domestic capital markets. The reason was imperfect information on the part of foreign investors, who did not believe that Mexican governments of any stripe were good credit risks unless they were making current payments. The memory of political instability and fiscal irresponsibility produced lasting effects.

Aurora Gómez-Galvarriato (1998) has studied the effects of poor transportation and a lack of impersonal sources of capital on the textile industry in the 19th century, institutional problems which she links to political instability. Using records of sales of British textile machinery to Mexico, she finds evidence of strong growth in the textile industry from the 1840s onward. In addition, she finds data showing strong growth in spindlage, the number of firms, and the percentage of mills using steam power. However, in striking contrast to the United States, she finds that Mexican textile production was extremely dispersed geographically, in order to overcome high transport costs and internal tariffs. What is not clear, however, is if those barriers to growth had any effect on the industry other than its geographic location. Gómez-Galvarriato asserts that this prevented the industry from taking advantage of agglomeration economies, but why this should be so is not clear from her research. The counterfactual should not be hard to test: more productive producers should not outcompete less productive ones or grow faster, at least until the arrival of the railroad. In addition, one would expect strong regional productivity differences, which should lessen with improving transport.

Armando Razo and Stephen Haber (1998a, 1998b) have used textile census and tax data to calculate productivity for Mexican textile firms, and found that it increased almost continually from the 1840s through the 1930s, sowing doubt upon the traditional interpretation of the 19th century in Mexico. More importantly, their work has also sown doubt on the traditional interpretation of the economic effects of the Mexican Revolution of 1910-20. The authors find no long-lasting effects from the Revolution on either productivity or capacity growth in the textile industry, despite intense political uncertainty (including a long series of attempted coups, political assassinations, and powerful guerrilla movements which lasted until 1928) and the radical institutional changes brought about by the Constitution of 1917, which limited work hours, recognized unions, imposed a minimum wage, and gave the federal government at least the notional right to expropriate property without recompense. The chaos of the Revo-

lution does not appear to have affected economic growth as much as one might predict from the precepts of the New Institutional Economics.

Unfortunately, one huge lacuna in the literature is the relative dearth of economically literate studies of agriculture in the Nineteenth Century, both before and after the arrival of President Porfirio Díaz in 1876. Margaret Chowning (1997) has studied agricultural profitability in Michoacán, and has shown that it rose during the period. Unfortunately, this is due to a decline in the value of estates, and so implies the opposite of her conclusion that political instability had little effect on agriculture. Chowning and others appear to have access to detailed hacienda records which should allow more detailed calculations of agricultural productivity and the factors influencing it. Eric Van Young (1986), Friedrich Katz (1992), and Jan Bazant (1980) are three of the most important historians of Mexican agriculture in the 18th and 19th centuries, but their work, while excellent, is influenced by the French *Annales* school and not cliometrics. They simply do not ask many of the questions an economic historian would want to know. We know far less than we should about 19th century estate agriculture or the effects of political instability on its growth and productivity.

CAPITAL MARKETS AND ECONOMIC GROWTH

In a rather different historical context, work by Carlos Marichal and Stephen Haber on capital markets in the 19th century also try to explain the emergence of institutions, and test hypotheses about their effects. Marichal's work has already been mentioned, and he has contributed much to our understanding of why capital markets failed to develop before the 1880s. Unfortunately, Marichal's analysis of why they remained relatively underdeveloped during the Porfiriato is less convincing; ultimately, he provides no real explanation.

But did this underdevelopment matter? Haber, in a comparison of Brazil and Mexico, ingeniously operationalized the hypothesis that different capital market structures that produced high level of industrial concentration in Mexico. In an industry (like textiles) without economies of scale, one would expect industrial concentration to be a direct function of industry size. The predicted relationship held in Brazil, with liberal capital market legislation; it did not in Mexico. Indeed, concentration worsened after 1890, when Mexico began to tighten its laws regarding bank entry. In addition, the Brazilian industry grew far faster than the Mexican one.

Haber's study is, however, only a beginning. The hypothesized connections between restrictive capital market legislation and industrial growth need to be further developed. For example, were banks involved in industrial finance? Did access to bank credit directly alleviate the liquidity constraints faced by Mexican industrial firms? If so, were the banks «picking winners» —that is, loaning to productive and fast-growing firms— or did access to credit cause faster growth? This is important, for it means the difference between arguing that inefficient capital markets had effects on industrial structure and arguing that it had effects on overall industrial growth. Recent work by Haber, Razo, and the author of this paper (Maurer, 1999) has shed some light on these questions. For example, it appears that joint-stock companies and companies with access to bank credit (all joint-stock companies also enjoyed access to bank credit, but the converse was not true) grew far faster than their competitors, but enjoyed no consistent advantage in total factor productivity, or operated more capital-intensively. In other words, access to impersonal credit alleviated capital constraints, but did not alter or improve firms' technology. There is additional evidence that the credit market did not work perfectly: that is to say, even firms enjoying access to credit faced (lessened) liquidity constraints, and limits on the growth of the banking system limited industrial growth.

This particular question is interesting not only to students of Mexican economic history, but to those interested in the effects of financial systems on economic development. The Mexican case provides an excellent laboratory, for example, for testing general hypotheses about the function of banks in industrialization. Do they alleviate capital constraints faced by firms? Carry out essential monitoring functions? Provide human capital («managerial talent») to the firms they finance? All of these questions are amenable to testing using historical data from Mexico.

RETHINKING PUBLIC POLICY

The use of cliometric techniques and economic theory has also produced an important rethinking of the causes and consequences of public policy. Capital market regulations during the Porfiriato have already been mentioned, but other realms of public policy were equally important. In fact, one might argue that specifying clear property rights in land was one of the most important economic functions of any state, and it is precisely a massive Porfirian program designed to do precisely that which Robert

Holden analyzes in his study of the Terrenos Baldíos program. This program was designed to give companies that agreed to survey «vacant» land one-third of the territories surveyed. In one decade, from 1883 to 1893, Holden finds, these companies received almost 10 per cent of Mexico's land area. However, Holden's careful analysis of the evidence from federal archives indicates that the government initiated the program to insulate itself from the politically risky consequences of the surveys, and that far from acting as the tool of a land-hungry elite willing to subvert institutions to «plunder the peasantry» it generally respected the rights of landholders in disputes with the surveyors. He justifies these conclusions—drawn from an exhaustive review of the records—with a transaction costs argument: «The companies recognized that respecting the claims of property holders was often cheaper than engaging in litigation or administrative processes that could delay the issue of a title for years, not to mention the risk of physical attacks on their engineers by offended landholders»¹². The economic gains the program remain to be quantified, but Holden's study has led us to rethink the putative social costs.

Recent work by Sandra Kuntz (1995, 1996) and Arturo Grunstein (1996) has begun to rethink the history of Mexican railroad policy. Sandra Kuntz has argued that the railroads contributed mightily to Mexican economic development by integrating internal markets and allowing regions to specialize, taking issue with Coatsworth's early contention that the railroads «underdeveloped» Mexico by encouraging a putatively unhealthy concentration in export industries. Many of her propositions about market integration and its benefits need further testing, but her work is part of an ongoing research program that is far from completion, and it represents an important break with the traditional historiography. In addition, Kuntz's careful examination of rate schedules and railroad rate policy have effectively deflated the idea that Porfirian policies unduly aided exporters.

Arturo Grunstein has also begun to apply techniques borrowed from industrial organization to understand the evolution of the competitive structure of the railroads, and government policy. Like Kuntz, he states his hypotheses about the structure of the market in falsifiable manner, borrowing theory from the modern literature on Industrial Organization. Operationalizing these hypotheses awaits further work and theoretical refinement.

Ted Beatty (1996) has made a similar break with the traditional historiography in his study of Porfirian development policies. First, he gathered

¹² Holden (1994), p. 130.

data on Porfirian tariffs, and then calculated effective rates of protection using the 1905 input-output tables from the US Census of Manufactures. He then combined this with a detailed study of the *Industrias Nuevas* program, which provided tax subsidies to selected firms in «new» industries: his data show that the program was relatively ineffective. Firms saved only 5 per cent to 6 per cent of their costs from the subsidies, and most concessions never resulted in functioning firms. Interestingly, he also find—using a probit analysis—that political connection did not seem help firms in obtaining concessions, as result which casts doubt on our traditional interpretation of the Porfirian political economy.

In fact, the new economic historians of Mexico have produced evidence that the roots of Import Substituting Industrialization go back much further than the Porfiriato. Work by Richard Salvucci, Linda Salvucci, and A. Cohen (1994) have shown that Mexico maintained high trade barriers after independence, and use a simple economic model borrowed from Mancur Olson to explain why: simply put, textile producers hurt by British imports after independence found it far easier to mobilize to protect their interests than the miners and farmers hurt by the restrictions¹³.

Enrique Cárdenas (1993) prompted a similar rethinking of the macroeconomic policies of the Lázaro Cárdenas administration (1934-40), which has traditionally been portrayed as populist and economically irresponsible. Cárdenas demonstrated that the central bank's policies had little to do with the monetary expansion of the late 1930s, that the bank worked overtime to arrest the inflation once it started, and that the budget deficits run under President Cárdenas were both small and countercyclical. This is not to say that the Cárdenas presidency was not populist in other ways; but it does break with the traditional view of macroeconomic management during the decade and implies that the relatively stable macroeconomic policies pursued by Mexican governments until the 1970s had their origins in the 1930s, not the 1950s.

WHERE DO WE GO FROM HERE?

To an extent, the answer to this question is clear: we keep identifying historical puzzles which the application of economic theory to historical data could potentially resolve, and interesting test cases that can serve

¹³ Andrien and Johnson (1994).

as laboratories with which to test economic theory. Clearly, economic historians of Mexico have only begun to answer the range of questions that might be posed about the nation's economic growth and change since 1521.

Some puzzles have already begun to be answered. First, it appears that the nation's modern textile industry began much earlier than had been thought, by the 1840s at the latest, and saw impressive productivity growth at least through the Great Depression. Second, the railroads appear to have contributed mightily to the nation's economic growth in the late 19th and early 20th centuries. Third, Porfirian economic policy was protectionist, interventionist, and coherent, and should not be described as «neoliberal» in the modern usage of the term. Fourth, Mexico's recovery from the Great Depression was stronger than had been thought, and due to solid macroeconomic policies, although its industrial growth was more a continuation of previous trends than a genuinely new start. Fifth, a great deal of Mexican backwardness in 20th century can be explained by relatively slow growth—at least until 1876 and outside the textile industry—in the 19th.

This may not seem like a lot compared with the topics that we are just beginning to explore. Such a conclusion would be unwarranted. In fact, the above represents a great deal of progress for a field which is so new, and demonstrates the power of the set of falsifiable hypotheses provided by the application of economic theory to Mexican economic history. We now have a well-defined theoretical basis for future research and a set of tools that can quickly yield dividends for their practitioners. There is much we do not know, especially regarding hypotheses about Mexico's growth (or growth in general) derived from the New Institutionalism, growth theory, and positive political economy. It may be that these approaches only yield a partial explanation of Mexico's relative slow growth performance. But we are far from reaching diminishing returns. This is a growth field.

If I may end on a caveat, however, the field needs to be more focussed in its research agendas. For example, studies of agriculture have lacked a coherent focus on a limited set of questions. Studies of industry and transportation in the nineteenth century have proceeded along a much more orderly line of inquiry, answering several basic questions before moving on to more complex and difficult issues. Twentieth-century economic history suffers from much the same flaw as nineteenth-century agricultural history: there has been too little basic research informed by economic theory into a range of important questions. In other words, Mexican economic history is making astounding progress, but needs a more order.

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