Railroads and American economic growth: essays in econometric history.

Review Essay by Lance Davis, Division of Humanities and Social Sciences, California Institute of Technology. led@hss.caltech.edu

For those of us who lived through the exciting days of the “cliometric revolution,” the publication of Robert Fogel’s *Railroads and American Economic Growth* represented a very major milestone – it was as if we now had proof that we had left the bumpy and unpaved dirt road of the first few years and could see ahead a straight and well-paved highway into the future. (See note 1.) The roots of “clio” clearly lay in the 1956 publication of Cary Brown’s “Fiscal Policy in the Thirties: A Reappraisal” and, a few months later, in Alfred Conrad and John Meyer’s initial presentation of “The Economics of Slavery in the Ante-Bellum South.” Brown showed that, unlike the findings of the then-current historiography, government economic policy during the 1930’s was not an example of President Roosevelt’s imaginative application of the modern tools of Keynesian fiscal policy; and Conrad and Meyer demonstrated that, despite nearly a century of traditional historiography, ante-bellum slavery was profitable and, at least by implication, that, if the goal was to eliminate slavery before the 1940’s, the Civil War was not an extremely costly and totally unnecessary enterprise. However, these findings – findings that have been well substantiated by later research – while convincing to the small cadre of “converted,” were still not generally accepted by the historical profession. Thus, cliometrics did not really begin to flower until the publication of Robert Fogel’s study of the impact of railroads on American growth in the nineteenth century. Not only did it generate a spate of parallel studies (of Russia, Mexico, Brazil, England, and Scotland, to cite only five), but much more importantly, it provided a methodological foundation for the systematic study of economic history and long-term economic growth.

Despite the attention that had been paid to the construction of the Erie Canal, given the role of the national market in underwriting this country’s rise to become, economically at least, the richest nation in the world, and, given the speed with which rails came to dominate the transport network that provided the basis for that national market, it is not surprising that historians had concluded that railroads were
the indispensable and driving force behind American growth in the nineteenth century. To the best of my knowledge, before the first annual Cliometric Conference (a conference held at Purdue University in 1960), few economic historians, neither those traditionally nor those cliometrically inclined doubted this fundamental tenant of American development. (See note 2.) Moreover, although some cliometricians may have been aware of the concept of social savings – a concept that was closely related to the economic literature on cost/benefit analysis – none had attempted to measure the savings attached to any specific legal or technical innovation. (Fogel had touched on a similar concept in *The Union Pacific Railroad* (1960), but his first published paper dealing specifically with social savings was still almost two years in the future – “A Quantitative Approach to the Study of Railroads in American Economic Growth” (1962).)

With its publication, *Railroads* proved once and for all that economic history, while still depending on the product of scholars “slugging it out in the archives,” could benefit mightily from the careful application of economic theory and econometrics. On the one hand, although the work immediately generated substantial controversy, and even today one might quibble about a few days or a few months, in the long run, there has been little question about the book’s major conclusion – that the level of per capita income achieved by January 1, 1890 would have been reached by March 31, 1890, if railroads had never been invented. Moreover, Fogel’s work also indicated that there was no other industry that was likely to have been more important than the railroads; and, thus, if not railroads, no other industry could have played the role that historiography attributed to the rails. On the other hand, the evidence is overwhelming that, since the publication and subsequent debate over *Railroads*, almost all economic history has been written by scholars who have either been trained in economics or who have found it necessary to acquire (either formally or informally) those basic economic and econometric skills. What, then, in addition to the central importance of the subject, made this such a path-breaking work? As the title suggests, the book is actually a collection of four interrelated, but really distinct, substantive essays: “The Interregional Distribution of Agricultural Products,” “The Intraregional Distribution of Agricultural Products,” “Railroads and the ‘Take-off’ Thesis: The American Case” and “The Position of Rails in the Market for American Iron, 1840-1860: A Reconstruction.” Any attempt at evaluating the contribution of the book rests on the evaluation of the methods and findings of the four.

If Fogel had limited his work to the last two essays – the two that in many ways were the most central to the then intense discussions of the “Axiom of Indispensability,” the work would have been important; but it would never have had anywhere near the impact that it actually did. In the third essay, “The Takeoff,” Fogel, although not addressing the question of whether or not there was in fact a “takeoff” between 1843 and 1860, in order to operationalize his argument, chooses the first of W.W. Rostow’s criteria for a “leading industry”: in this case, what impact did the railroads have on the “change in the percentage distribution of output among the various industries?” Then, drawing on the best available data – data reported by Robert Gallman in his seminal (1960) study of commodity output – Fogel finds that the impact of the railroads on that percentage distribution was minimal. In the case of iron, railroads, except at the end of the period, accounted for only a minor fraction of the output change (overall, including the later period, it was still only 17 percent); for coal, it was less than 5 percent; for lumber, barely 5 percent; in the case of transport equipment only 25 percent (only half of the change accounted for by vehicles drawn by animals); and for machinery it was less than 1 percent. Thus, for all manufacturing, the railroads accounted for less than 3 percent of the change – hardly a ringing endorsement for what was purported to be a “leading industry.”

In his more detailed examination of the impact of railroads on the development of the iron industry (an
attempt to assess the importance of railroads to industrialization because of their alleged “backward linkages”), Fogel found it necessary to produce a new series on pig iron output between 1840 and 1860 and to revise the estimates of the consumption of railroads to account for imports and recycled rails as well as changes in the weight of rails. These new estimates represented a major contribution to our understanding of the industrial history of the period. Fogel’s primary interest, however, was not on the production of the new series, but on estimating the importance of the railroads in the development of the iron industry. His results, again, indicate that railroads did not dominate the development of the iron industry in the two decades before the Civil War. In fact, his conclusions strongly support Douglass North’s conclusion that, from the point of view of backward linkages, it would be as sensible to talk about an iron stove theory of the development of the iron industry as a railroad theory.

In these two essays Fogel demonstrates a command of what had heretofore been the best of traditional economic history, but in neither chapter are there any major methodological breakthroughs – merely a carefully constructed series of new estimates and the demonstration of an ability to bring those estimates to bear on important issues. In the first and second of the four substantive chapters – the estimate of the social savings from the interregional and from the intraregional distribution of agricultural products – Fogel’s methodological innovations do, however, play a central role. First, in both essays, he attempts to explicate and to provide estimates of the appropriate counterfactual – what the world would have been like had there been no railroads. Although historians have long employed counterfactual arguments – sometimes it seems without realizing it – to most historians the idea of an explicit counterfactual was still a very foreign notion in the early 1960s. Second, in both chapters Fogel employs the concept of social savings (the difference in social costs between the real and the counterfactual worlds) to provide a measure of the value of the introduction of the railroad. The concept of social savings is itself an important research tool; but, from a methodological point of view, it is equally important that the measure was defined operationally, so that Fogel’s calculations could be tested against alternative estimates and against possible alternative definitions. As an aside, however, it is interesting to note that, although the two studies are very very important from the view point of methodological innovation, from the point of view of traditional economic history, they are not as strong as the third and fourth substantive essays. In the second substantive essay – the social savings arising from the intraregional distribution of agricultural commodities – Fogel begins by noting that the substitution of rail for water was more rapid in the intraregional than in the interregional distribution of agricultural commodities, and, that, since the distances to be shipped in the intraregional case were only a third as great for rail as for water transport, one would expect that the social savings from the innovation would be greater. To estimate those savings he proposes two measures: alpha (a direct measure of the cost differences with and without the railroads) and beta (an indirect measure based on the difference in the value of the land that would have been economically productive with railroads and the lesser number of acres and, thus, the lesser value of land that would have been economically productive in the absence of those railroads).

Fogel then estimates alpha for a sample of counties in the North Atlantic region and concludes that the direct costs (alpha) would amount to a loss of 2.5% of GNP, and that adjustment for excluded indirect costs (alpha-2) would have increased that figure to 2.8% of GNP. Neither estimate, however, includes the potential savings that would have resulted from the construction of additional canals and better roads. He admits that the North Atlantic region may not provide an adequate representation of the entire country, but he argues that it would be too expensive and difficult to extend this direct measure of savings to the rest of the country.
As an alternative, Fogel suggests that, since water transport was available for about 76% of the land value in the U.S., since, in the absence of railroads, 75% of the loss of land value would be in the four states of Illinois, Iowa, Nebraska, and Kansas, and since all of the lost land could be brought into production with only a small extension of the canal network, a measure based on the difference in the value of arable land provides an equally good measure of social savings. He concludes that the cost of the direct loss of arable land from the absence of railroads (beta) would amount to 1.8% of GNP, and that the total loss – the sum of direct and indirect costs (beta-2) – would amount to 2.1% of GNP. Again, however, beta-2 does include the potential savings that would result from additional canals and better roads. Making further adjustments for the unbuilt canals and better roads, Fogel provides two estimates for the social savings from intraregional trade: alpha-3 equal to 1.2% of GNP and beta-3 equal to 1.0%. It was, however, Fogel’s estimates of the social savings generated by railroads in interregional shipping (the first substantive essay), that really touched off the methodological revolution. As in the second essay, the use of explicit counterfactuals and the innovation of the concept (as well as his estimates) of the social savings broke new ground. In this case, however, there were also other very important methodological innovations.

Fogel begins with an operational definition of interregional distribution: “the process of shipping commodities from the primary markets of the Midwest to the secondary markets of the East and South.” While there were good estimates of agricultural production and agricultural exports, there were no data on the method and routes of shipment that were used to move agricultural commodities from producing areas to the points of domestic and foreign consumption; and it is here that Fogel introduces his single most significant innovation. He focuses on four commodities (wheat, corn, beef, and pork) – commodities that together represented 42 percent of agricultural income. He, first, estimates the export surplus at ten primary markets in the west and the consumption in the almost 200 deficit trading areas in the East and South (exports are attributed to the port from which they were shipped). The potential rail and water shipping routes from West to East were easily identified, and the costs of rail and water shipment were well known. To simplify the problem, Fogel focuses on a sample of 30 of the 825 potential routes between pairs of cities in the West and the East. Since the actual choice of routes is unknown, he very imaginatively suggests a linear programming model to estimate the routes – with and without railroads – that would have been selected had the shippers been guided by cost minimization. He then estimates the costs of the inferred shipments, costs estimated both with and without rails. Since there were also additional costs of water transport (lost cargoes, transshipment expenses, extra wagon haulage, time lost because of slower speed and because the canals and rivers froze, and the capital costs of the canals that were not included in the water rates), Fogel adjusts his original cost differentials to account for these additional expenses. His result is an estimate of the social savings in interregional shipment resulting from the innovation of railroads of six-tenths of one percent of GNP, a figure that would have increased to only 1.3%, had he assumed that rail rates were zero.

In this chapter Fogel made four important innovations that were to have a major impact of the nature of research in economic history: (1) the operational definition of social savings; (2) the use of an explicit counterfactual; (3) the use of a formal economic model to estimate what costs would have been had the decisions been made by economic man; and (4) his choice, when it was necessary to make assumptions about the actual world, of assumptions that were biased against his central findings. (See note 3.) Even more than his estimates of interregional social savings, the work in this essay completely changed the way economic historians would do business in the future. There is, however, one blemish in the story. Professor Fogel never actually solved the linear programming problem; his choice of routes was based
on what he assumed the solution would have been.

Notes:

1. To give you some feeling about that first decade, one might note that the term “cliometrics” was coined by my then colleague at Purdue, Stanley Reiter – he had been toying around with questions raised by a new discipline that he called “theometrics” (for example, “how many angels can dance on the head of a pin?”); and, in his joking way, he suggested that the work in quantitative history seemed to be drawn from similar academic stream.

2. Bob Fogel and, perhaps, Douglass North and Al Fishlow, were the major exceptions. Fogel, himself, has said that he began his investigation fully believing that it would confirm the importance of the railroads. Fishlow (1965) reached conclusions for the antebellum period very similar to those Fogel reached about the latter part of the nineteenth century. Not long before this, North (1961, p. 164) wrote, “While the value added of rails was approximately $6.5 million in 1860 and roughly equals to the value added of bar iron, it was dwarfed by the value added of the polyglot classification of iron castings, which was $21 million in 1860. Indeed, the value added in stove making alone was equal to that of iron rails.”

3. For example, Fogel made no adjustment for changes in non-rail transport that might have been made had there been no railroads: he holds both origins and destinations fixed despite the fact that there would almost certainly have been some such adjustments in the absence of railroads; and he assumes that, in the absence of railroads, water rates would be constant rather than declining as might have been the case had canal builders exploited potential economies of scale.

References:


Subject(s): Transport and Distribution, Energy, and Other Services

Geographic Area(s): North America
Brahmin Capitalism: Frontiers of Wealth and Populism in America’s First Gilded Age

Author(s): Maggor, Noam
Reviewer(s): Whitten, David O.

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Reviewed for EH.Net by David O. Whitten, Professor of Economics, Auburn University.

Noam Maggor, Postdoctoral Associate in the Department of History at Cornell University and formerly a Fellow in the Charles Warren Center for Studies in American History at Harvard University, opens Brahmin Capitalism: Frontiers of Wealth and Populism in America’s First Gilded Age with the pre-Civil War history of Boston wealth and its employment in the construction and expansion of the cotton textile industry in the Boston area. The return on investments in mills and machine shops attracted capital to the point of over-development that left investors with declining stock values and reduced returns that might well have spelled the end of Boston as a capital-rich city. What Boston financiers did to reinvent themselves has a commanding role in Maggor’s study.

The first thrust into investments outside of Boston’s textile industry was a failure. Bostonians — with more ambition than understanding of human behavior and Mother Nature — undertook to prove to the investment world that cotton culture could be as remunerative under free labor conditions as it was under slavery. Cultivating cotton on a sea island off the coast of South Carolina proved more challenging than the young Bostonians anticipated. The heat, high humidity, lack of infrastructure, and hurricanes combined with a recalcitrant newly-freed labor force to overwhelm the young men from the North who returned to Boston in defeat. They might have benefited from knowledge of the experience of ship builders in the early National period who struggled to harvest live-oak trees for construction of the nation’s renowned frigates. The live oaks thrive in the swampy Atlantic coastal regions of the Southern states. The parts essential for the best ship construction were acquired but only at heavy expense of human effort and treasure.

The spectacular growth of the American heartland in the post-Civil War era is well documented. What Maggor adds to that body of literature is a microscopic analysis of the role of Boston investors in that explosive expansion. Railroad construction required financing as did exploitation of the mines and forests of the United States. Maggor documents the Boston investors’ contribution to that underwriting. The Mexican Central Railway, a 1,225 mile road that ran from El Paso to Mexico City, for example, was incorporated under Massachusetts law and controlled by the Boston investment group that owned the Atchison, Topeka and Santa Fe and was directed from Boston where nine of its thirteen directors resided. The Mexican Central connected with the Atchinson, Topeka, and Santa Fe and feeder lines constructed to regions where remunerative freight could be found.
The moneyed interests in Boston did not invest in a pig-in-a-poke but relied on their men-on-the-ground to advise them on the potential of attractive enterprises — men who devoted themselves to careful study and analysis of the projects they reviewed for their employers in Boston. Those employers were family and friends of the advance-agents, who saw themselves as missionaries to the great unwashed of the American heartland and who were often surprised at the intellect, cunning and energy of the lower class men they studied. It was not unusual for the surveyors to assume responsibility for the undertakings they recommended for investment.

Maggor juxtaposes his exposition of the flow of Boston capital into the heartland with discussions of changes in Boston itself. At the same time that Boston financiers were gaining national influence with their monetary assets they were losing control of their city. In the heyday of cotton manufacturing and the import/export trade, the city of Boston belonged to the elite. The controlling offices were held by those with money and ownership of the factories. With the decline of cotton manufacturing and import/export, Boston became home to small manufacturing and commercial firms whose owners and employees made up in numbers (read votes) for what they lacked in wealth. Moreover, the lower and middle classes expanded their political power by expanding the city itself, incorporating what had been outlying towns into the city proper over the protests of the moneyed class that considered itself to be the soul of Boston, the people who mattered. As the nineteenth century advanced, Boston’s elite were increasingly isolated in pockets they were able to defend against the urban juggernaut. Beacon Hill and later Back Bay became enclaves of the rich and powerful — powerful nationally but increasingly impotent locally.

Maggor showcases the hometown conflict facing the moneyed interests of Boston with a chapter dedicated to the fierce battle over use of the Common. Using direct quotations from the debaters, Maggor paints a vivid portrait of the positions taken by the combatants or in the case of the moneyed class, their paid spokespersons. These positions reflected the philosophies of the two sides and their opposing views of their city and their place in it. The rhetorical storm grew out of a request of mechanics in Boston for use of a small part of the Common for an exposition featuring the mechanical advancements of which they were proud. Similar expositions were common in the decades after the successful Crystal Palace Exposition in London in 1851. The Common was a barrier between Back Bay, home to a large portion of the moneyed class who saw any intrusion into the sanctuary they portrayed as an urban retreat for the lower classes, as a threat to their privacy and security.

Following the battle for the Common, Maggor investigates the conflict between East Coast investors and the constitutional conventions at work in the states being carved out of the western territories. Boston investors, like the others in the East, were concerned that state constitutions might infringe on their freedom to harvest the resources of the territories and to regulate the railroads owned by Eastern moneyed interests. Delegates to the conventions were not of the elite of society but residents of the new states who had vested interests in the operation of their government. These delegates sought protection from the rich men back east who sought to take off the resources of the state at minimum or no cost and to run rampant over organized labor and any resistance to railroad rate setting. Representatives of the East Coast moneyed interests played hard ball with the constitutional conventions threatening the loss of investment funds essential to the development of the state. Neither side won complete victories but the moneyed interests probably gained the most from the negotiations.

Meanwhile, back in Boston the elites built political coalitions that propelled them back into control of their city government. Once back in power the elected elites strove to undo what the populist
administrations had constructed and reduce the support of lower classes that came at the expense of those better off. The conflict between those dependent upon a strong and well financed city government and those who wanted to reduce that government parallels the national political conflict of the second decade of the twenty-first century. As Maggor assures us, the development of the United States as an industrial power and leader of the world did not come about smoothly but in fits and starts with uneven results. *Brahmin Capitalism* is a guide to the background of that development as it progressed in Boston, a premier leader in investment at home and beyond.

Maggor’s work lacks a bibliography but extensive citations largely make up for that failure. The writing is like silk, so smooth that it often moves the reader along at such a pace that the underlying meaning of the narrative is missed and demands a second and even third reading. This is not an easy book to digest. It demands concentration but makes the effort worthwhile. Students of business history will benefit from a reading of this work.


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**Subject(s):** Business History  
**Geographic Area(s):** North America  
**Time Period(s):** 19th Century

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**Concrete Economics: The Hamiltonian Approach to Economic Growth and Policy**

**Author(s):** Cohen, Stephen S.  
DeLong, J. Bradford

**Reviewer(s):** Salsman, Richard M.

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When the U.S. has prospered most has it been due mainly to a limited state that ensures equal legal
treatment and relatively free markets, or has it been due to an active, intervening state that regiments
activity and protects or subsidizes favored products, firms and sectors, at the expense of others? The
latter, say the authors of this brief but rather interesting volume. Take note, any remaining fans of Adam
Smith, true believers in “invisible hands,” or diehard devotees of “laissez faire.”

DeLong, a professor of economics at the University of California, Berkeley and Cohen, professor
emeritus and co-director of the Berkeley Roundtable in the International Economy (BRIE), deserve
credit for reminding us that no economic policy is truly “hands off.” As a discipline, political economy
properly recognizes and studies the unavoidable interaction of politics and markets; the latter can’t even
function without some basic provision of public goods (e.g., law and order, security of private property,
legal sanctity of contract). They deserve kudos also for at least stipulating that prosperity (“the wealth of
nations”) is a worthy goal (as it was for Smith), for that’s surely not a key premise of today’s
environmentalists, social-justice warriors, or the Pope.

But is more needed, for prosperity, than the provision of basic, rights-protecting public goods? Yes, say
these authors — substantially more. In their view the state is an economy’s designer, the one institution
which can (must!) identify and clear out “spaces” where entrepreneurs can then confidently operate.
They believe the U.S. has suffered economic crises and stagnation since 1980 because economic policy
has been too pro-business — i.e., excessively-low tax rates, free trade, deregulation, entrepreneurialism,
and promotion of “zero-sum” financial activity at the expense of value-added manufacturing. They tout
five prior episodes in U.S. history when “real” prosperity occurred, due (they claim) to their preferred
policy mix of high income tax (and tariff) rates, heavy regulation (especially of finance), infrastructure
spending, and protectionism: the 1790s and early 1800s (via “Hamiltonian” principles), the post-Civil War
Gilded Age (via Lincolnian prescriptions), the progressive era (via Teddy Roosevelt’s policies), the New
Deal of the 1930s (via FDR’s New Deal), and the post-WWII decades of infrastructure/aerospace buildout
(a by-product of the “military industrial complex” which Eisenhower both encouraged and distrusted).

By intention, the methodology here isn’t very rigorous. No new historical database is cited or analyzed.
The authors eschew careful, disciplined treatments of history, empirics, and models. They offer,
unapologetically, a selective historical narrative designed mainly to corroborate their theme. They
believe policy formation (and analysis) goes awry if ever animated by “ideology,” especially in free-
market form. Declaring themselves “non-ideological,” they focus on what they call the “concretes,”
instead of abstractions; they endorse only what “works” (“pragmatically”), not what should work
(“theoretically”). But can anyone specify what “works” without reference to some criterion? The authors
implicitly deny that scientific methods require hypothesizing and testing — that some theory is necessary
even to know where to look in a vast empirical record.

DeLong and Cohen’s methodology — more accurately, their anti-methodology posture — is worth
mentioning, because in truth they cleverly apply a specific theory in choosing their anecdotes and
structuring their narrative, one which economists have variously characterized as “economic
nationalism” or “industrial policy.” In this model, popularized in the 1970s and early 1980s by Robert
Reich and Lester Thurow, public officials and planners are presumed to be sufficiently wise and
prescient to distinguish future economic “winners and losers” and thus able to generate sustainable
prosperity by fostering the former and discouraging the latter, while (somehow) also avoiding the
corporatist and labor union rent-seeking such policy targeting typically invites. A more recent example
of the approach is Mazzucato’s The Entrepreneurial State (2013).
When a theory doesn’t explain economic reality very well, its adherents might elect to eschew theory altogether or instead to cherry-pick the historical record, to make the dubious theory “fit.” Hedging their bets, these authors try both. As for the cherry-picking, they cite nearly every major economic innovation in the U.S. since its founding era and attribute it to the encouragement of some government policy. Thanks mainly to Washington, America has had firearms, railroads, radio, aerospace, autos, trucking, assembly lines, nuclear power, electrification, central banking, paper money, infrastructure, computers, semiconductors, the Internet (yes, they credit Al Gore), and even smart phones. If the U.S. government has ever even remotely touched these things, the authors imply, it pretty much made them possible. At the same time, they blame failed products, eroded industries, and recession-depression decades in U.S. history on overly-free markets.

Even if the claim were true, that these products and sectors were made possible by Washington, it’s worth noting that the authors find that they entail primarily spin-offs from the outlays and projects of the U.S. Department of Defense — a state function even classical liberals can heartily endorse. Are prosperity-fostering spinoffs likely to flow also from the explosion of entitlement-transfer outlays in the half-century since the start of “Great Society” schemes? U.S. federal spending on national defense is now just 12% of all outlays, down from 16% in 2000, 23% in 1980, and 50% in 1960. It’s about as likely as the authors’ more liberal sympathizers being pleased to hear what aspect of U.S. spending most boosts the economy. These Berkeley dons are in the odd position of wishing devoutly for the “military-industrial complex” Ike warned against.

A misleading aspect of the book is the authors’ insistence that theirs is “the Hamilton approach to economic growth and policy.” In truth Alexander Hamilton, the first U.S. Treasury secretary (1789-1795) wanted (and implemented) a constitutionally-limited federal government, by no means a state engaged in “industrial planning” of the kind DeLong and Cohen want (let alone “social insurance” or “redistribution”). Hamilton rejected British mercantilism, which stunted American manufacturing; and unlike his Jeffersonian-agrarian opponents (and successors), he wanted low and uniform tariffs. Hamilton also defended and implemented a gold-silver based dollar, sustained reductions in the national debt, and a limited-power, privately-owned national (nationwide) bank (not a “central bank,” as the authors claim). Also unlike DeLong and Cohen, who devote a whole chapter to denouncing what they claim is a cancer-like “hypertrophy of finance” since 1980, Hamilton saw the financial sector as productive (if left free of government influence — as it surely isn’t today), not one that displaces real and healthy economic sinews.

Two periods in U.S. economic history are particularly misrepresented in the book, to fit the author’s theme. The “Gilded Age,” the half-century between the ended of the Civil War and start of World War I — supposedly entailed “vast accumulations of conspicuous wealth” and a “confiscation of the nation’s wealth” due to “the crushing power of trusts” (p. 71). In fact that was a half-century of stupendous invention, entrepreneurship, and wage gains, due to economic freedom, not theft; it was accomplished without a central bank, a federal income tax, centralized industrial planning, or a regulatory state. The other period misrepresented is the 1930s; the authors say FDR’s heavily-interventionist New Deal revived the economy, but in fact it mainly prolonged the stagnation.

It’s reasonable to expect a book authored by fans of “industrial policy” to highlight Japan, as did Reich and Thurow in the 1980s. After all, Japan’s Ministry of International Trade and Industry (“MITI”) was heralded as the model for planning agencies globally and the progenitor of its post-war economic “miracle.” Yes, Japan’s industrial production increased nearly 10% per annum from 1950 to 1991; but
since then it has shrunk at a compounded rate of 0.4% per annum. Does this quarter-century contraction reflect free market policies enacted after 1991? Hardly. Somehow “things changed,” the authors report, dead-pan. “Japan had become a solidly rich nation.” “Asset values then crashed and stayed crashed,” and “rapid growth became dishearteningly elusive. Why? We do not claim to know” (p. 133). Herein lies the futility (and dishonesty) of historical cherry-picking: when the facts don’t fit, plead humility and ignorance; otherwise, proclaim boldly and often that every sustained economic success necessarily has flowed from astute state planning.

DeLong and Cohen deserve thanks for issuing a reminder that the humane state should facilitate prosperity and higher living standards, as that’s become a minority (but much needed) view in recent decades. The book’s more refutable parts include the claim that prosperity is achievable by actively countermanding markets, the belief that today’s burgeoning welfare-transfer state (which they condone) can spawn wealth-producing “spinoffs,” and above all, the presumption that their book has the imprimatur of a truly Hamiltonian (pro-capitalist) political economy.


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**Subject(s):** Economic Planning and Policy

**Geographic Area(s):** North America

**Time Period(s):**
- 18th Century
- 19th Century
- 20th Century: Pre WWII
- 20th Century: WWII and post-WWII

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**Confederate Political Economy: Creating and Managing a Southern Corporatist Nation**

**Author(s):** Bonner, Michael Brem

**Reviewer(s):** Pecquet, Gary M.

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Reviewed for EH.Net by Gary M. Pecquet, Department of Economics, Central Michigan University.

Historian Michael Bonner examines how the government, bureaucrats, industrial leaders and ordinary
citizens interacted under the pressures of emergency wartime conditions to create a distinct Confederate political economy. The Confederate war economy dispensed with many of the normal features of a market economy just as the United States did in prosecuting the two world wars during the twentieth century.

Although the extent of the Confederate government’s economic control was unprecedented by American standards, Bonner correctly contends that the Confederate political economy was never a top-down command economy, as Louise Hill (1936) and as William Davis (1994) have contended. Bonner correctly dispatches the claim that the Confederacy adopted a “State Socialist” model. According to him, “The State Socialism argument overlooks the capitalists, both the agricultural capitalist slaveholders and the growing class of industrial capitalists, who facilitated the dramatically increased production of war materiel” (p. 222, fn. 44).

Instead, Bonner draws from Nobel-laureate Edmund Phelps’ Mass Flourishing (2013) and finds the Confederate war economy to be neither free market, nor state socialism. Bonner finds that the Confederate war economy compared more closely to the authoritarian “corporatist” economic model (aka “Fascism,” aka “crony capitalism”) adopted throughout Europe during the early-to-mid twentieth century to preserve traditional social values from dynamic capitalistic. Markets are tolerated, but subject to significant government oversight and regulation.

Confederate leaders did not intentionally set up an authoritarian regime, but the corporate model emerged out of wartime expediency. Special provisions in the new Confederate Constitution conferred additional powers to the executive branch: a six-year Presidential term, a line-item veto and executive control over government expenditures. In addition, The Confederate Supreme Court was never appointed and approved; nor did the Congress even establish federal courts for judicial review. Moreover, although Confederate congressmen and senators continued to stand for elections, wartime pressures for patriotism reduced the role of political parties and prevented gridlock. This gave President Jefferson Davis a free hand to contract with selected private firms to secure war supplies. Initially, the Confederacy lacked a bureaucracy and trained public servants so it often relied upon the assistance from states for enforcement.

The Confederate authorities had to negotiate with private interests in order to ensure reliable supplies of essential military goods. They also developed an ad hoc policy towards railroads. The Confederacy conscripted men into military service and imposed a system of wartime passes upon civilians to prevent espionage.

Bonner describes thorough narratives the corporatist interworking between the Confederate government and private manufacturers. These included the Tredegar Iron Works of Richmond and the Shelby Iron Works of Alabama, near Birmingham. Drawing largely from Charles Dew (1966), Bonner describes the rise of entrepreneur/businessman Joseph Reid Anderson, who built the Tredegar Iron Works ten years before the beginning of the war. Tredegar secured early contracts from the emerging Confederate government in February 1861 and continued to sell to both private railroads as well as the government. Bonner does a good job describing the contractual negotiations between Tredegar and government purchasing agents. Due to the onslaught of rampant inflation, the company faced rising costs and accusations of price gouging by Confederate politicians. These complaints increased and by late 1862, an army ordinance officer accused the company of yielding excessive profits of 30 to 50 percent or even as high as “60-80% in recent months” (p. 80). After 1863 the company asked five more times for price increases and the accusations of profiteering only got worse. But Tredegar was the major
supplier of iron to the Confederacy and continued to obtain contracts. We may regard this process of price increases followed by accusations and new contracts as a bi-lateral negotiating process taking place during times of depreciating currency values. (Incidentally, this process was not substantially different from the union-management negotiations under periods of continuous price inflation in certain twentieth-century corporatist nations.)

Compared to Tredegar, Shelby Iron Works of Alabama was a latecomer. Largely from new primary sources, Bonner uncovers details of crony capitalism between the Shelby Company and government purchasing agents. At Shelby, prospective owners sought to expand operations, but wanted government protection from risks, so with the help of an influential Confederate purchasing agent, they secured a $75,000 loan from the Confederacy. But the private/public partnership created conflicting expectations that undermined the effectiveness of the operation. The company was supposed to repay the loan, but the government expected that the added facilities should be used to fill government orders, not private orders. The government officials feared that Shelby iron might be sold at higher prices to private buyers. The government also aided the Shelby works by exempting key employees from the draft. The major point of contention between Shelby and government was the means of payment (sound money or depreciated Confederate notes and bonds). Eventually, however, Shelby agreed to accept payment in fixed prices, with an eye to renegotiating new terms as prices increased. But in this case, Confederate officials could threaten Shelby’s labor supply by denying draft exemptions, so they may have held an advantage.

The Confederacy did embark upon a major government-run business. At the beginning of the war, the South had only four small local gunpowder mills. The Confederate government decided to create a single, large government-owned gunpowder factory at a secure location to provide its wartime requisitions. This single factory successfully supplied the Confederate armies for most of the war.

Bonner does a good job showing how Woodrow Wilson’s administration adopted the Confederate corporatist model as it mobilized the economy for participation in World War I. Although the Confederacy stumbled into cozy business-government relationships, the Wilson Administration consciously followed the same pattern. The Wilsonian World War I regime was not a top-down command-and-control system, but one that mixed government force and favors with private cooperation. Like the Confederacy, the WWI selective service relied upon the cooperation of local draft boards. Wilson’s government takeover of the railroads worked much the same way as Confederate control over rails. In both cases, the governments had to rely upon the railroad owners’ expertise giving business the upper hand in setting policy.

Bonner’s book provides a helpful addition to the study of early twentieth century Progressive economic policy. His book exploring Confederate mobilization provides a complimentary narrative to Robert Higgs’ (1987) analysis of the growth of government in Crisis and Leviathan. Bonner does not consider the role of intellectual history. Corporatism originated in Europe as the “German Historical School” and adherents taught its doctrines in American universities. Wilson adopted the theories of corporatism from his university professors (Pecquet and Thies, 2010). What Bonner shows us is that the practice of wartime mobilization also flowed out of a preexisting pattern that remained in the memories of contemporary historians.

References:

The Economic History of Mexico

Richard Salvucci, Trinity University

Preface

This article is a brief interpretive survey of some of the major features of the economic history of Mexico from pre-conquest to the present. I begin with the pre-capitalist economy of Mesoamerica. The colonial period is divided into the Habsburg and Bourbon regimes, although the focus is not really political: the emphasis is instead on the consequences of demographic and fiscal changes that colonialism brought. Next I analyze the economic impact of independence and its accompanying conflict. A tentative effort to reconstruct secular patterns of growth in the nineteenth century follows, as well as an account of the
effects of foreign intervention, war, and the so-called “dictatorship” of Porfirio Díaz. I then examine the economic consequences of the Mexican Revolution down through the presidency of Lázaro Cárdenas, before considering the effects of the Great Depression and World War II. This is followed by an examination of the so-called Mexican Miracle, the period of import-substitution industrialization after World War II. The end of the “miracle” and the rise of economic instability in the 1970s and 1980s are discussed in some detail. I conclude with structural reforms in the 1990s, the North American Free Trade Agreement (NAFTA), and slow growth in Mexico since then. It is impossible to be comprehensive and the references appearing in the citations are highly selective and biased (where possible) in favor of English-language works, although Spanish is a must for getting beyond the basics. This is especially true in economic history, where some of the most innovative and revisionist work is being done, as it should be, by historians and economists in Mexico.[2]

Where (and What) is Mexico?

For most of its long history, Mexico’s boundaries have been shifting, albeit broadly stable. Colonial Mexico basically stretched from Guatemala, across what is now California and the Southwestern United States, and vaguely into the Pacific Northwest. There matters stood for more than three centuries.[3]. The big shock came at the end of the War of 1847 (“the Mexican-American War” in U.S. history). The Treaty of Guadalupe Hidalgo (1848) ended the war, but in so doing, ceded half of Mexico’s former territory to the United States—recall Texas had been lost in 1836. The northern boundary now ran on a line beginning with the Rio Grande to El Paso, and thence more or less west to the Pacific Ocean south of San Diego. With one major adjustment in 1853 (the Gadsden Purchase or Treaty of the Mesilla) and minor ones thereafter, because of the shifting of the Rio Grande, there it has remained.

Prior to the arrival of the Europeans, Mexico was a congeries of ethnic and city states whose own boundaries were unstable. Prior to the emergence of the most powerful of these states in the fifteenth century, the so-called Triple Alliance (popularly “Aztec Empire”), Mesoamerica consisted of cultural regions determined by political elites and spheres of influence that were dominated by large ceremonial centers such as La Venta, Teotihuacan, and Tula.

While such regions may have been dominant at different times, they were never “economically” independent of one another. At Teotihuacan, there were living quarters given over to Olmec residents from the Veracruz region, presumably merchants. Mesoamerica was connected, if not unified, by an ongoing trade in luxury goods and valuable stones such as jade, turquoise and precious feathers. This was not, however, trade driven primarily by factor endowments and relative costs. Climate and resource endowments did differ significantly over the widely diverse regions and microclimates of Mesoamerica. Yet trade was also political and ritualized in religious belief. For example, calling the shipment of turquoise from the (U.S.) Southwest to Central Mexico the outcome of market activity is an anachronism. In the very long run, such prehistorical exchange facilitated the later emergence of trade routes, roads, and more technologically advanced forms of transport. But arbitrage does not appear to have figured importantly in it.[4]

In sum, what we call “Mexico” in a modern sense is not of much use to the economic historian with an interest in the country before 1870, which is to say, the great bulk of its history. In these years, specificity of time and place, sometimes reaching to the village level, is an indispensable prerequisite for meaningful discussion. At the very least, it is usually advisable to be aware of substantial regional differences which
reflect the ethnic and linguistic diversity of the country both before and after the arrival of the Europeans. There are fully ten language families in Mexico, and two of them, Nahuatl and Quiché, number over a million speakers each.[5]

**Trade and Tribute before the Europeans**

In the codices or deerskin folded paintings the Europeans examined (or actually commissioned), they soon became aware of a prominent form of Mesoamerican economic activity: tribute, or taxation in kind, or even labor services. In the absence of anything that served as money, tribute was forced exchange. Tribute has been interpreted as a means of redistribution in a nonmonetary economy. Social and political units formed a basis for assessment, and the goods collected included maize, beans, chile and cotton cloth. It was through the tribute the indigenous “empires” mobilized labor and resources. There is little or no evidence for the existence of labor or land markets to do so, for these were a European import, although marketplaces for goods existed in profusion.

To an extent, the preconquest reliance on barter economies and the absence of money largely accounts for the ubiquity of tribute. The absence of money is much more difficult to explain and was surely an obstacle to the growth of productivity in the indigenous economies.

The tribute was a near-universal attribute of Mesoamerican ceremonial centers and political empires. The city of Teotihuacan (ca. 600 CE, with a population of 125,000 or more) in central Mexico depended on tribute to support an upper stratum of priests and nobles while the tributary population itself lived at subsistence. Tlatelolco (ca 1520, with a population ranging from 50 to 100 thousand) drew maize, cotton, cacao, beans and precious feathers from a wide swath of territory that broadly extended from the Pacific to Gulf coasts that supported an upper stratum of priests, warriors, nobles, and merchants. It was this urban complex that sat atop the lagoons that filled the Valley of Mexico that so awed the arriving conquerors.

While the characterization of tribute as both a corvée and a tax in kind to support nonproductive populations is surely correct, its persistence in altered (i.e., monetized) form under colonial rule does suggest an important question. The tributary area of the Mexica (“Aztec” is a political term, not an ethnic one) broadly comprised a Pacific slope, a central valley, and a Gulf slope. These embrace a wide range of geographic features ranging from rugged volcanic highlands (and even higher snow-capped volcanoes) to marshy, humid coastal plains. Even today, travel through these regions is challenging. Lacking both the wheel and draught animals, the indigenous peoples relied on human transport, or, where possible, waterborne exchange. However we measure the costs of transportation, they were high. In the colonial period, they typically circumscribed the subsistence radius of markets to 25 to 35 miles. Under the circumstances, it is not easy to imagine that voluntary exchange, particularly between the coastal lowlands and the temperate to cold highlands and mountains, would be profitable for all but the most highly valued goods. In some parts of Mexico—as in the Andean region—linkages of family and kinship bound different regions together in a cult of reciprocal economic obligations. Yet absent such connections, it is not hard to imagine, for example, transporting woven cottons from the coastal lowlands to the population centers of the highlands could become a political obligation rather than a matter of profitable, voluntary exchange. The relatively ambiguous role of markets in both labor and goods that persisted into the nineteenth century may perhaps derive from just this combination of climatic and geographical characteristics. It is what made voluntary exchange under capitalistic markets
such a puzzlingly problematic answer to the ordinary demands of economic activity.

[See the relief map below for the principal physical features of Mexico.]


[See the political map below for Mexican states and state capitals.]
“New Spain” or Colonial Mexico: The First Phase

Mexico was established by military conquest and civil war. In the process, a civilization with its own institutions and complex culture was profoundly modified and altered, if not precisely destroyed, by the European invaders. The catastrophic elements of conquest, including the sharp decline of the existing indigenous population, from perhaps 25 million to fewer than a million within a century due to warfare, disease, social disorganization and the imposition of demands for labor and resources should nevertheless not preclude some assessment, however tentative, of its economic level in 1519, when the Europeans arrived.[6]

Recent thinking suggests that Spain was far from poor when it began its overseas expansion. If this were so, the implications of the Europeans’ reactions to what they found on the mainland of Mexico (not, significantly in the Caribbean, and, especially, in Cuba, where they were first established) is important. We have several accounts of the conquest of Mexico by the European participants, of which Bernal Díaz del Castillo is the best known, but not the only one. The reaction of the Europeans was almost uniformly astonishment by the apparent material wealth of Tenochtitlan. The public buildings, spacious residences of the temple precinct, the causeways linking the island to the shore, and the fantastic array of goods available in the marketplace evoked comparisons to Venice, Constantinople, and other wealthy centers.
of European civilization. While it is true that this was a view of the indigenous elite, the beneficiaries of the wealth accumulated from numerous tributaries, it hardly suggests anything other than a kind of storied opulence. Of course, the peasant commoners lived at subsistence and enjoyed no such privileges, but then so did the peasants of the society from which Bernal Díaz, Cortés, Pedro de Alvarado and the other conquerors were drawn. It is hard to imagine that the average standard of living in Mexico was any lower than that of the Iberian Peninsula. The conquerors remarked on the physical size and apparent robust health of the people whom they met, and from this, scholars such as Woodrow Borah and Sherburne Cook concluded that the physical size of the Europeans and the Mexicans was about the same. Borah and Cook surmised that caloric intake per individual in Central Mexico was around 1,900 calories per day, which certainly seems comparable to European levels.\[7\]

Certainly, the technological differences with Europe hampered commercial exchange, such as the absence of the wheel for transportation, metallurgy that did not include iron, and the exclusive reliance on pictographic writing systems. Yet by the same token, Mesoamerican agricultural technology was richly diverse and especially oriented toward labor-intensive techniques, well suited to pre-conquest Mexico’s factor endowments. As Gene Wilken points out, Bernardo de Sahagún explained in his *General History of the Things of New Spain* that the Nahua farmer recognized two dozen soil types related to origin, source, color, texture, smell, consistency and organic content. They were expert at soil management.\[8\] So it is possible not only to misspecify, but to mistake the technological “backwardness” of Mesoamerica relative to Europe, and historians routinely have.

The essentially political and clan-based nature of economic activity made the distribution of output somewhat different from standard neoclassical models. Although no one seriously maintains that indigenous civilization did not include private property and, in fact, property rights in humans, the distribution of product tended to emphasize average rather than marginal product. If responsibility for tribute was collective, it is logical to suppose that there was some element of redistribution and collective claim on output by the basic social groups of indigenous society, the clans or *calpulli*.\[9\] Whatever the case, it seems clear that viewing indigenous society and economy as strained by population growth to the point of collapse, as the so-called “Berkeley school” did in the 1950s, is no longer tenable. It is more likely that the tensions exploited by the Europeans to divide and conquer their native hosts and so erect a colonial state on pre-existing native entities were mainly political rather than socioeconomic. It was through the assistance of native allies such as the Tlaxcalans, as well as with the help of previously unknown diseases such as smallpox that ravaged the indigenous peoples, that the Europeans were able to place a weakened Tenochtitlan under siege and finally defeat it.

*Colonialism and Economic Adjustment to Population Decline*

With the subjection first of Tenochtitlan and Tlatelolco and then of other polities and peoples, a process that would ultimately stretch well into the nineteenth century and was never really completed, the Europeans turned their attention to making colonialism pay. The process had several components: the modification or introduction of institutions of rule and appropriation; the introduction of new flora and fauna that could be turned to economic use; the reorientation of a previously autarkic and precapitalist economy to the demands of trade and commercial exploitation; and the implementation of European fiscal sovereignty. These processes were complex, required much time, and were, in many cases, only partly successful. There is considerable speculation regarding how long it took before Spain (arguably a relevant term by the mid-sixteenth century) made colonialism pay. The best we can do is present a
schematic view of what occurred. Regional variations were enormous: a “typical” outcome or institution of colonialism may well have been an outcome visible in central Mexico. Moreover, all generalizations are fragile, rest on limited quantitative evidence, and will no doubt be substantially modified eventually. The message is simple: proceed with caution.

The Europeans did not seek to take Mesoamerica as a *tabula rasa*. In some ways, they would have been happy to simply become the latest in a long line of ruling dynasties established by decapitating native elites and assuming control. The initial demand of the conquerors for access to native labor in the so-called *encomienda* was precisely that, with the actual task of governing be left to the surviving and collaborating elite: the principle of “indirect rule.” There were two problems with this strategy: the natives resisted and the natives died. They died in such large numbers as to make the original strategy impracticable.

The number of people who lived in Mesoamerica has long been a subject of controversy, but there is no point in spelling it out once again. The numbers are unknowable and, in an economic sense, not really important. The population of Tenochtitlan has been variously estimated between 50 and 200 thousand individuals, depending on the instruments of estimation. As previously mentioned, some estimates of the Central Mexican population range as high as 25 million on the eve of the European conquest, and virtually no serious student accepts the small population estimates based on the work of Angel Rosenblatt. The point is that labor was abundant relative to land, and that the small surpluses of a large tributary population must have supported the opulent elite that Bernal Díaz and his companions described.

By 1620, or thereabouts, the indigenous population had fallen to less than a million according to Cook and Borah. This is not just the quantitative speculation of modern historical demographers. Contemporaries such as Jerónimo de Mendieta in his *Historia eclesiástica Indiana* (1596) spoke of towns formerly densely populated now witness to “the palaces of those former Lords ruined or on the verge of. The homes of the commoners mostly empty, roads and streets deserted, churches empty on feast days, the few Indians who populate the towns in Spanish farms and factories.” Mendieta was an eyewitness to the catastrophic toll that European microbes and warfare took on the native population. There was a smallpox epidemic in 1519-20 when 5 to 8 million died. The epidemic of hemorrhagic fever in 1545 to 1548 was one of the worst demographic catastrophes in human history, killing 5 to 15 million people. And then again in 1576 to 1578, when 2 to 2.5 million people died, we have clear evidence that land prices in the Valley of Mexico (Coyoacán, a village outside Mexico City, as the reconstructed Tenochtitlán was called) collapsed. The death toll was staggering. Lesser outbreaks were registered in 1559, 1566, 1587, 1592, 1601, 1604, 1606, 1613, 1624, and 1642. The larger point is that the intensive use of native labor, such as the encomienda, had to come to an end, whatever its legal status had become by virtue of the New Laws (1542). The encomienda or the simple exploitation of massive numbers of indigenous workers was no longer possible. There were too few “Indians” by the end of the sixteenth century.

As a result, the institutions and methods of economic appropriation were forced to change. The Europeans introduced pastoral agriculture – the herding of cattle and sheep – and the use of now abundant land and scarce labor in the form of the hacienda while the remaining natives were brought together in “villages” whose origins were not essentially pre- but post-conquest, the so-called *congregaciones*, at the same time that the titles to now-vacant lands were created, regularized and “composed.” (Land titles were a European innovation as well). Sheep and cattle, which the Europeans introduced, became part of the new institutional backbone of the colony. The natives would continue to rely on maize for the better part of their subsistence, but the Europeans introduced wheat,
olives (oil), grapes (wine) and even chickens, which the natives rapidly adopted. On the whole, the results of these alterations were complex. Some scholars argue that the native diet improved even in the face of their diminishing numbers, a consequence of increased land per person and of greater variety of foodstuffs, and that the agricultural potential of the colony now called New Spain was enhanced. By the beginning of the seventeenth century, the combined indigenous, European immigrant, and new mixed blood populations could largely survive on the basis of their own production. The introduction of sheep lead to the introduction and manufacture of woolens in what were called obrajes or manufactories in Puebla, Querétaro, and Coyoacán. The native peoples continued to produce cottons (a domestic crop) under the stimulus of European organization, lending, and marketing. Extensive pastoralism, the cultivation of cereals and even the incorporation of native labor then characterized the emergence of the great estates or haciendas, which became a characteristic rural institution through the twentieth century, when the Mexican Revolution put an end to many of them. Thus the colony of New Spain continued to feed, clothe and house itself independent of metropolitan Spain’s direction. Certainly, Mexico before the Conquest was self-sufficient. The extent to which the immigrant and American Spaniard or creole population depended on imports of wine, oil and other foodstuffs and textiles in the decades immediately following the conquest is much less clear.

At the same time, other profound changes accompanied the introduction of Europeans, their crops and their diseases into what they termed the “kingdom” (not colony, for constitutional reasons) of New Spain. Prior to the conquest, land and labor had been commoditized, but not to any significant extent, although there was a distinction recognized between possession and ownership. Scholars who have closely examined the emergence of land markets after the conquest—mainly in the Valley of Mexico—are virtually unanimous in this conclusion. To the extent that markets in labor and commodities had emerged, it took until the 1630s (and later elsewhere in New Spain) for the development to reach maturity. Even older mechanisms of allocation of labor by administrative means (repartimiento) or by outright coercion persisted. Purely economic incentives in the form of money wages and prices never seemed adequate to the job of mobilizing resources and those with access to political power were reluctant to pay a competitive wage. In New Spain, the use of some sort of political power or rent-seeking nearly always accompanied labor recruitment. It was, quite simply, an attempt to evade the implications of relative scarcity, and renders the entire notion of “capitalism” as a driving economic force in colonial Mexico quite inexact.

Why the Settlers Resisted the Implications of Scarce Labor

The reasons behind this development are complex and varied. The evidence we have for the Valley of Mexico demonstrates that the relative price of labor rose while the relative price of land fell even when nominal movements of one or the other remained fairly limited. For instance, the table constructed below demonstrates that from 1570-75 through 1591-1606, the price of unskilled labor in the Valley of Mexico nearly tripled while the price of land in the Valley (Coyoacán) fell by nearly two thirds. On the whole, the price of labor relative to land increased by nearly 800 percent. The evolution of relative prices would have inevitably worked against the demanders of labor (Europeans and increasingly, creoles or Americans of largely European ancestry) and in favor of the supplier (native labor, or people of mixed race generically termed mestizo). This was not of course what the Europeans had in mind and by capture of legal institutions (local magistrates, in particularly), frequently sought to substitute compulsion for what would have been costly “free labor.” What has been termed the “depression” of the seventeenth
century may well represent one of the consequences of this evolution: an abundance of land, a scarcity of labor, and the attempt of the new rulers to adjust to changing relative prices. There were repeated royal prohibitions on the use of forced indigenous labor in both public and private works, and thus a reduction in the supply of labor. All highly speculative, no doubt, but the adjustment came during the central decades of the seventeenth century, when New Spain increasingly produced its own woolens and cottons, and largely assumed the tasks of providing itself with foodstuffs and was thus required to save and invest more. No doubt, the new rulers felt the strain of trying to do more with less.[14]

<table>
<thead>
<tr>
<th>Years</th>
<th>Land Price Index</th>
<th>Labor Price Index</th>
<th>(Labor/Land) Index</th>
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<td>100</td>
</tr>
<tr>
<td>1576-1590</td>
<td>50</td>
<td>143</td>
<td>286</td>
</tr>
<tr>
<td>1591-1606</td>
<td>33</td>
<td>286</td>
<td>867</td>
</tr>
</tbody>
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The overall role of Mexico within the Hapsburg Empire was in flux as well. Nothing signals the change as much as the emergence of silver mining as the principal source of Mexican exportables in the second half of the sixteenth century. While Mexico would soon be eclipsed by Peru as the most productive center of silver mining—at least until the eighteenth century—the discovery of significant silver mines in Zacatecas in the 1540s transformed the economy of the Spanish empire and the character of New Spain’s as well.

Silver Mining

While silver mining and smelting was practiced before the conquest, it was never a focal point of indigenous activity. But for the Europeans, Mexico was largely about silver mining. From the mid-sixteenth century onward, it was explicitly understood by the viceroyes that they were to do all in their power to “favor the mines,” as one memorable royal instruction enjoined. Again, there has been much controversy of the precise amounts of silver that Mexico sent to the Iberian Peninsula. What we do know certainly is that Mexico (and the Spanish Empire) became the leading source of silver, monetary reserves, and thus, of high-powered money. Over the course of the colonial period, most sources agree that Mexico provided nearly 2 billion pesos (dollars) or roughly 1.6 billion troy ounces to the world economy. The graph below provides a picture of the remissions of all Mexican silver to both Spain and to the Philippines taken from the work of John TePaske.[15]
Since the population of Mexico under Spanish rule was at most 6 million people by the end of the colonial period, the kingdom’s silver output could only be considered astronomical.

This production has to be considered in both its domestic and international dimensions. From a domestic perspective, the mines were what a later generation of economists would call “growth poles.” They were markets in which inputs were transformed into tradable outputs at a much higher rate of productivity (because of mining’s relatively advanced technology) than Mexico’s other activities. Silver thus became Mexico’s principal exportable good, and remained so well into the late nineteenth century. The residual claimants on silver production were many and varied. There were, of course, the silver miners themselves in Mexico and their merchant financiers and suppliers. They ranged from some of the wealthiest people in the world at the time, such as the Count of Regla (1710-1781), who donated warships to Spain in the eighteenth century, to individual natives in Zacatecas smelting their own stocks of silver ore. While the conditions of labor in Mexico’s silver mines were almost uniformly bad, the compensation ranged from above market wages paid to free labor in the prosperous larger mines of the Bajío and the North to the use of forced village labor drafts in more marginal (and presumably less profitable) sites such as Taxco. In the Iberian Peninsula, income from American silver mines ultimately supported not only a class of merchant entrepreneurs in the large port cities, but virtually the core of the Spanish political nation, including monarchs, royal officials, churchmen, the military and more. And finally, silver flowed to those who valued it most highly throughout the world. It is generally estimated that 40 percent of Spain’s American (not just Mexican, but Peruvian as well) silver production ended up in hoards in China.

Within New Spain, mining centers such as Guanajuato, San Luis Potosí, and Zacatecas became places where economic growth took place rapidly, in which labor markets more readily evolved, and in which the standard of living became obviously higher than in neighboring regions. Mining centers tended to crowd out growth elsewhere because the rate of return for successful mines exceeded what could be gotten in commerce, agriculture and manufacturing. Because silver was the numéraire for Mexican prices—Mexico was effectively on a silver standard—variations in silver production could and did have
substantial effects on real economic activity elsewhere in New Spain. There is considerable evidence that silver mining saddled Mexico with an early case of “Dutch disease” in which irreducible costs imposed by the silver standard ultimately rendered manufacturing and the production of other tradable goods in New Spain uncompetitive. For this reason, the expansion of Mexican silver production in the years after 1750 was never unambiguously accompanied by overall, as opposed to localized prosperity. Silver mining tended to absorb a disproportional quantity of resources and to keep New Spain’s price level high, even when the business cycle slowed down—a fact that was to impress visitors to Mexico well into the nineteenth century. Mexican silver accounted for well over three-quarters of exports by value into the nineteenth century as well. The estimates vary widely, for silver was by no means the only, or even the most important source of revenue to the Crown, but by the end of the colonial era, the Kingdom of New Spain probably accounted for 25 percent of the Crown’s imperial income.[17] That is why reformist proposals circulating in governing circles in Madrid in the late eighteenth century fixed on Mexico. If there was any threat to the American Empire, royal officials thought that Mexico, and increasingly, Cuba, were worth holding on to. From a fiscal standpoint, Mexico had become just that important.[18]

“New Spain”: The Second Phase of the Bourbon “Reforms”

In 1700, the last of the Spanish Hapsburgs died and a disputed succession followed. The ensuring conflict, known as the War of Spanish Succession, came to an end in 1714. The grandson of French king Louis XIV came to the Spanish throne as King Philip V. The dynasty he represented was known as the Bourbons. For the next century and a half, they were to determine the fortunes of New Spain. Traditionally, the Bourbons, especially the later ones, have been associated with an effort to “renationalize” the Spanish empire in America after it had been thoroughly penetrated by French, Dutch, and lastly, British commercial interests.[19]

There were at least two areas in which the Bourbon dynasty, “reformist” or no, affected the Mexican economy. One of them dealt with raising revenue and the other was the international position of the imperial economy, specifically, the volume and value of trade. A series of statistics calculated by Richard Garner shows that the share of Mexican output or estimated GDP taken by taxes grew by 167 percent between 1700 and 1800. The number of taxes collected by the Royal Treasury increased from 34 to 112 between 1760 and 1810. This increase, sometimes labelled as a Bourbon “reconquest” of Mexico after a century and a half of drift under the Hapsburgs, occurred because of Spain’s need to finance increasingly frequent and costly wars of empire in the eighteenth century. An entire array of new taxes and fiscal placemen came to Mexico. They affected (and alienated) everyone, from the wealthiest merchant to the humblest villager. If they did nothing else, the Bourbons proved to be expert tax collectors.[20]

The second and equally consequential change in imperial management lay in the revision and “deregulation” of New Spain’s international trade, or the evolution from a “fleet” system to a regime of independent sailings, and then, finally, of voyages to and from a far larger variety of metropolitan and colonial ports. From the mid-sixteenth century onwards, ocean-going trade between Spain and the Americas was, in theory, at least, closely regulated and supervised. Ships in convoy (flota) sailed together annually under license from the monarchy and returned together as well. Since so much silver specie was carried, the system made sense, even if the flotas made a tempting target and the problem of contraband was immense. The point of departure was Seville and later, Cadiz. Under pressure from other outports in the late eighteenth century, the system was finally relaxed. As a consequence, the
volume and value of trade to Mexico increased as the price of importables fell. Import-competing industries in Mexico, especially textiles, suffered under competition and established merchants complained that the new system of trade was too loose. But to no avail. There is no measure of the barter terms of trade for the eighteenth century, but anecdotal evidence suggests they improved for Mexico. Nevertheless, it is doubtful that these gains could have come anywhere close to offsetting the financial cost of Spain’s “reconquest” of Mexico.[21]

On the other hand, the few accounts of per capita real income growth in the eighteenth century that exist suggest little more than stagnation, the result of population growth and a rising price level. Admittedly, looking for modern economic growth in Mexico in the eighteenth century is an anachronism, although there is at least anecdotal evidence of technological change in silver mining, especially in the use of gunpowder for blasting and excavating, and of some productivity increase in silver mining. So even though the share of international trade outside of goods such as cochineal and silver was quite small, at the margin, changes in the trade regime were important. There is also some indication that asset income rose and labor income fell, which fueled growing social tensions in New Spain. In the last analysis, the growing fiscal pressure of the Spanish empire came when the standard of living for most people in Mexico—the native and mixed blood population—was stagnating. During periodic subsistence crisis, especially those propagated by drought and epidemic disease, and mostly in the 1780s, living standards fell. Many historians think of late colonial Mexico as something of a powder keg waiting to explode. When it did, in 1810, the explosion was the result of a political crisis at home and a dynastic failure abroad. What New Spain had negotiated during the Wars of Spanish Succession—regime change—provide impossible to surmount during the Napoleonic Wars (1794-1815). This may well be the most sensitive indicator of how economic conditions changed in New Spain under the heavy, not to say clumsy hand, of the Bourbon “reforms.” [22]

The War for Independence, the Insurgency, and Their Legacy

The abdication of the Bourbon monarchy to Napoleon Bonaparte in 1808 produced a series of events that ultimately resulted in the independence of New Spain. The rupture was accompanied by a violent peasant rebellion headed by the clerics Miguel Hidalgo and José Morelos that, one way or another, carried off 10 percent of the population between 1810 and 1820. Internal commerce was largely paralyzed. Silver mining essentially collapsed between 1810 and 1812 and a full recovery of mining output was delayed until the 1840s. The mines located in zones of heavy combat, such as Guanajuato and Querétaro, were abandoned by fleeing workers. Thus neglected, they quickly flooded.

At the same time, the fiscal and human costs of this period, the Insurgency, were even greater. [23] The heavy borrowings in which the Bourbons engaged to finance their military alliances left Mexico with a considerable legacy of internal debt, estimated at £16 million at Independence. The damage to the fiscal, bureaucratic and administrative structure of New Spain in the face of the continuing threat of Spanish reinvasion (Spain did not recognize the Independence of Mexico (1821)) in the 1820s drove the independent governments into foreign borrowing on the London market to the tune of £6.4 million in order to finance continuing heavy military outlays. With a reduced fiscal capacity, in part the legacy of the Insurgency and in part the deliberate effort of Mexican elites to resist any repetition Bourbon-style taxation, Mexico defaulted on its foreign debt in 1827. For the next sixty years, through a serpentine history of moratoria, restructuring and repudiation (1867), it took until 1884 for the government to regain access to international capital markets, at what cost can only be imagined. Private sector
borrowing and lending continued, although to what extent is currently unknown. What is clear is that the total (internal plus external) indebtedness of Mexico relative to late colonial GDP was somewhere in the range of 47 to 56 percent.[24]

This was, perhaps, not an insubstantial amount for a country whose mechanisms of public finance were in what could be mildly termed chaotic condition in the 1820s and 1830s as the form, philosophy, and mechanics of government oscillated from federalist to centralist and back into the 1850s. Leaving aside simple questions of uncertainty, there is the very real matter that the national government—whatever the state of private wealth—lacked the capacity to service debt because national and regional elites denied it the means to do so. This issue would bedevil successive regimes into the late nineteenth century, and, indeed, into the twentieth.[25]

At the same time, the demographic effects of the Insurgency exacted a cost in terms of lost output from the 1810s through the 1840s. Gaping holes in the labor force emerged, especially in the fertile agricultural plains of the Bajío that created further obstacles to the growth of output. It is simply impossible to generalize about the fortunes of the Mexican economy in this period because of the dramatic regional variations in the Republic’s economy. A rough estimate of output per head in the late colonial period was perhaps 40 pesos (dollars).[26] After a sharp contraction in the 1810s, income remained in that neighborhood well into the 1840s, at least until the eve of the war with the United States in 1846. By the time United States troops crossed the Rio Grande, a recovery had been under way, but the war arrested it. Further political turmoil and civil war in the 1850s and 1860s represented setbacks as well. In this way, a half century or so of potential economic growth was sacrificed from the 1810s through the 1870s. This was not an uncommon experience in Latin America in the nineteenth century, and the period has even been called The Stage of the Great Delay.[27] Whatever the exact rate of real per capita income growth was, it is hard to imagine it ever exceeded two percent, if indeed it reached much more than half that.

Agricultural Recovery and War

On the other hand, it is clear that there was a recovery in agriculture in the central regions of the country, most notably in the staple maize crop and in wheat. The famines of the late colonial era, especially of 1785-86, when massive numbers perished, were not repeated. There were years of scarcity and periodic corresponding outbreaks of epidemic disease—the cholera epidemic of 1832 affected Mexico as it did so many other places—but by and large, the dramatic human wastage of the colonial period ceased, and the death rate does appear to have begun to fall. Very good series on wheat deliveries and retail sales taxes for the city of Puebla southeast of Mexico City show a similarly strong recovery in the 1830s and early 1840s, punctuated only by the cholera epidemic whose effects were felt everywhere.[28]

Ironically, while the Panic of 1837 appears to have at least hit the financial economy in Mexico hard with a dramatic fall in public borrowing (and private lending), especially in the capital,[29] an incipient recovery of the real economy was ended by war with the United States. It is not possible to put numbers on the cost of the war to Mexico, which lasted intermittently from 1846 to 1848, but the loss of what had been the Southwest under Mexico is most often emphasized. This may or may not be accurate. Certainly, the loss of California, where gold was discovered in January 1848, weighs heavily on the historical imaginations of modern Mexicans. There is also the sense that the indemnity paid by the United States—$15 million—was wholly inadequate, which seems at least understandable when one
considered that Andrew Jackson offered $5 million to purchase Texas alone in 1829.

It has been estimated that the agricultural output of the Mexican “cession” as it was called in 1900, was nearly $64 million, and that the value of livestock in the territory was over $100 million. The value of gold and silver produced was about $35 million. Whether it is reasonable to employ the numbers in estimating the present value of output relative to the indemnity paid is at least debatable as a counterfactual, unless one chooses to regard this as the annuitized value on a perpetuity “purchased” from Mexico at gunpoint, which seems more like robbery than exchange. In the long run, the loss may have been staggering, but in the short run, much less so. The northern territories Mexico lost had really yielded very little up until the War. In fact, the balance of costs and revenues to the Mexican government may well have been negative.[30]

Whatever the case, the decades following the war with the United States until the beginning of the administration of Porfirio Díaz (1876) are typically regarded as a step backward. The reasons are several. In 1850, the government essentially went broke. While it is true that its financial position had disintegrated since the mid-1830s, 1850 marked a turning point. The entire indemnity payment from the United States was consumed in debt service, but this made no appreciable dent in the outstanding principal, which hovered around 50 million pesos (dollars). The limits of debt sustainability had been reached: governing was turned into a wild search for resources, which proved fruitless. Mexico continued to sell of parts of its territory, such as the Treaty of the Mesilla (1853), or Gadsden Purchase, whose proceeds largely ended up in the hands of domestic financiers rather than foreign creditors.[31]

Political divisions, if anything, terrible before the war with the United States, turned catastrophic. A series of internal revolts, uprisings and military pronouncements segued into yet another violent civil war between liberals and conservatives—now a formal party—the so-called Three Years’ War (1856-58). In 1862, frustrated by Mexico’s suspension of foreign debt service, Great Britain, Spain and France seized Veracruz. A Hapsburg prince, Maximilian, was installed as Mexico’s second “emperor.” (Agustín de Iturbide was the first). While only the French actively prosecuted the war within Mexico, and while they never controlled more than a very small part of the country, the disruption was substantial. By 1867, with Maximilian deposed and the French army withdrawn, the country required serious reconstruction. [32]

Juárez, Díaz and the Porfiriato: authoritarian development.

To be sure, the origins of authoritarian development in nineteenth century Mexico were not with Porfirio Díaz, as is often asserted. Their beginnings actually went back several decades earlier, to the last presidency of Santa Anna, generally known as the Dictatorship (1853-54). But Santa Anna was overthrown too quickly, and now for the last time, for much to have actually occurred. A ministry for development (Fomento) had been created, but the Liberal revolution of Ayutla swept Santa Anna and his clique away for good. Serious reform seems to have begun around 1870, when the Finance Minister was Matías Romero. Romero was intent on providing Mexico with a modern Treasury, and on ending the hand-to-mouth financing that had mostly characterized the country’s government since Independence, or at least since the mid-1830s. So it is appropriate to pick up with the story here. Where did Mexico stand in 1870?[33]

The most revealing data that we have on the state of economic development come from various anthropometric and cost of living studies by Amilcar Challu, Aurora Gómez Galvarriato, and Moramay López Alonso.[34] Their research overlaps in part, and gives a fascinating picture of Mexico in the long
run, from 1735 to 1940. For the moment, let us look at the period leading up to 1867, when the French withdrew from Mexico. If we look at the heights of the “literate” population, Challu’s research suggests that the standard of living stagnated between 1750 and 1840. If we look at the “illiterate” population, there was a consistent decline until 1850. Since the share of the illiterate population was clearly larger, we might infer that living standards for most Mexicans declined after 1750, however we interpret other quantitative and anecdotal evidence.

López Alonso confines her work to the period after the 1840s. From 1850 through 1890, her work generally corroborates Challu’s. The period after the Mexican War was clearly a difficult one for most Mexicans, and the challenge that both Juárez and Díaz faced was a macroeconomy in frank contraction after 1850. The regimes after 1867 were faced with stagnation.

The real wage study of by Amilcar Challu and Aurora Gómez Galvarriato, when combined with the existing anthropometric work, offers a pretty clear correlation between movements in real wages (down) and height (falling). [35]

It would then appear growth from the 1850s through the 1870s was slow—if there was any at all—and perhaps inferior to what had come between the 1820s and the 1840s. Given the growth of import substitution during the Napoleonic Wars, roughly 1790-1810, coupled with the commercial opening brought by the Bourbons’ post-1789 extension of “free trade” to Mexico, we might well see a pattern of mixed performance (1790-1810), sharp contraction (the 1810s), rebound and recovery, with a sharp financial shocks coming in the mid-1820s and mid -1830s (1820s-1840s), and stagnation once more (1850s-1870s). Real per capita output oscillated, sometimes sharply, around an underlying growth rate of perhaps one percent; changes in the distribution of income and wealth are more or less impossible to identify consistently, because studies conflict.

Far less speculative is that the foundations for modern economic growth were laid down in Mexico during the era of Benito Juárez. Its key elements were the creation of a secular, bourgeois state and secular institutions embedded in the Constitution of 1857. The titanic ideological struggles between liberals and conservatives were ultimately resolved in favor of a liberal, but nevertheless centralizing form of government under Porfirio Díaz. This was the beginning of the end of the Ancien Regime. Under Juárez, corporate lands of the Church and native villages were privatized in favor of individual holdings and their former owners compensated in bonds. This was effectively the largest transfer of land title since the late sixteenth century (not including the war with the United States) and it cemented the idea of individual property rights. With the expulsion of the French and the outright repudiation of the French debt, the Treasury was reorganized along more modern lines. The country got additional breathing room by the suspension of debt service to Great Britain until the terms of the 1825 loans were renegotiated under the Dublán Convention (1884). Equally, if not more important, Mexico now entered the railroad age in 1876, nearly forty years after the first tracks were laid in Cuba in 1837. The educational system was expanded in an attempt to create at least a core of literate citizens who could adopt the tools of modern finance and technology. Literacy still remained in the neighborhood of 20 percent, and life expectancy at birth scarcely reached 40 years of age, if that. Yet by the end of the Restored Republic (1876), Mexico had turned a corner. There would be regressions, but the nineteenth century had finally arrived, aptly if brutally signified by Juárez’ execution of Maximilian in Querétaro in 1867.[36]

Porfirian Mexico

Yet when Díaz came to power, Mexico was, in many ways, much as it had been a century earlier. It was a
rural, agrarian nation whose primary agricultural output per person was maize, followed by wheat and beans. These were produced on haciendas and ranchos in Jalisco, Guanajuato, Michoacán, Mexico, Puebla as well as Oaxaca, Veracruz, Aguascalientes, Chihuahua and Sonora. Cotton, which with great difficulty had begun to supply a mechanized factory regime (first in spinning, then weaving) was produced in Oaxaca, Yucatán, Guerrero and Chiapas as well as in parts of Durango and Coahuila. Domestic production of raw cotton rarely sufficed to supply factories in Michoacán, Querétaro, Puebla and Veracruz, so imports from the Southern United States were common. For the most part, the indigenous population lived on maize, beans, and chile, producing its own subsistence on small, scattered plots known as milpas. Perhaps 75 percent of the population was rural, with the remainder to be found in cities like Mexico, Guadalajara, San Luis Potosí, and later, Monterrey. Population growth in the Southern and Eastern parts of the country had been relatively slow in the nineteenth century. The North and the center North grew more rapidly. The Center of the country, less so. Immigration from abroad had been of no consequence.[37]

It is a commonplace to see the presidency of Porfirio Díaz (1876-1910) as a critical juncture in Mexican history, and this would be no less true of economic or commercial history as well. By 1910, when the Díaz government fell and Mexico descended into two decades of revolution, the first one extremely violent, the face of the country had been changed for good. The nature and effect of these changes remain not only controversial, but essential for understanding the subsequent evolution of the country, so we should pause here to consider some of their essential features.

While mining and especially, silver mining, had long held a privileged place in the economy, the nineteenth century had witnessed a number of significant changes. Until about 1889, the coinage of gold, silver, and copper—a very rough proxy for production given how much silver had been illegally exported—continued on a steadily upward track. In 1822, coinage was about 10 million pesos. By 1846, it had reached roughly 15 million pesos. There was something of a structural break after the war with the United States (its origins are unclear), and coinage continued upward to about 25 million pesos in 1888. Then, the falling international price of silver, brought on by large increases in supply elsewhere, drove the trend after 1889 sharply downward. By 1909-10, coinage had collapsed to levels previously unrecorded since the 1820s, although in 1904 and 1905, it had skyrocketed to nearly 45 million pesos.[38]

It comes as no surprise that these variations in production corresponded to sharp changes in international relative prices. For example, the market price of silver declined sharply relative to lead, which in turn encountered a large increase in Mexican production and a diversification into other metals including zinc, antimony, and copper. Mexico left the silver standard (for international transactions, but continued to use silver domestically) in 1905, which contributed to the eclipse of this one crucial industry, which would never again have the status it had when Díaz became president in 1876, when precious metals represented 75 percent of Mexican exports by value. By the time he had decamped in exile to Paris, precious metals accounted for less than half of all exports.

The reason for this relative decline was the diversification of agricultural exports that had been slowly occurring since the 1870s. Coffee, cotton, sugar, sisal and vanilla were the principal crops, and some regions of the country such as Yucatán (henequen) and Durango and Tamaulipas (cotton) supplied new export crops.

**Railroads and Infrastructure**
None of this would have occurred without the massive changes in land tenure that had begun in the 1850s, but most of all, without the construction of railroads financed by the migration of foreign capital to Mexico under Díaz. At one level, it is a well-known story of social savings, which were substantial in Mexico because the terrain was difficult and the alternative modes of carriage few. One way or another, transportation has always been viewed as an “obstacle” to Mexican economic development. That must be true at some level, although recent studies (especially by Sandra Kuntz) have raised important qualifications. Railroads may not have been gateways to foreign dependency, as historians once argued, but there were limits to their ability to effect economic change, even internally. They tended to enlarge the internal market for some commodities more than others. The peculiarities of rate-making produced other distortions, while markets for some commodities were inevitably concentrated in major cities or transshipment points which afforded some monopoly power to distributors even as a national market in basic commodities became more of a reality. Yet, in general, the changes were far reaching.

Conventional figures confirm conventional wisdom. When Díaz assumed the presidency, there were 660 km (410 miles) of track. In 1910, there were 19,280 km (about 12,000 miles). Seven major lines linked the cities of Mexico, Veracruz, Acapulco, Juárez, Laredo, Puebla, Oaxaca. Monterrey and Tampico in 1892. The lines were built by foreign capital (e.g., the Central Mexicano was built by the Atchison, Topeka and Santa Fe), which is why resolving the long-standing questions of foreign debt service were critical. Large government subsidies on the order of 3,500 to 8,000 pesos per km were granted, and financing the subsidies amounted to over 30 million pesos by 1890. While the railroads were successful in creating more of a national market, especially in the North, their finances were badly affected by the depreciation of the silver peso, given that foreign liabilities had to be liquidated in gold.

As a result, the government nationalized the railroads in 1903. At the same time, it undertook an enormous effort to construct infrastructure such as drainage and ports, virtually all of which were financed by British capital and managed by “Don Porfirio’s contactor,” Sir Weetman Pearson. Between railroads, ports, drainage works and irrigation facilities, the Mexican government borrowed 157 million pesos to finance costs.

The expansion of the railroads, the build-out of infrastructure and the expansion of trade would have normally increased output per capita. Any data we have prior to 1930 are problematic, and before 1895, strictly speaking, we have no official measures of output per capita at all. Most scholars shy away from using levels of GDP in any form, other than for illustrative purposes. Aside from the usual problems attending national income accounting, Mexico presents a few exceptional challenges. In peasant families, where women were entrusted with converting maize into tortilla, no small job, the omission of their value added from GDP must constitute a sizeable defect in measured output. Moreover, as the commercial radius of Mexican agriculture expanded rapidly as railroads, roads, and later, highways spread extensively, growth rates represented increased commercialization rather than increased growth. We have no idea how important this phenomenon was, but it is worth keeping in mind when we look at very rapid growth rates after 1940.

There are various measures of cumulative growth during the Porfiriato. By and large, the figure from 1900 through 1910 is around 23 percent, which is certainly higher than rates achieved during the nineteenth century, but nothing like what was recorded after 1940. In light of declining real wages, one can only assume that the bulk of “progress” flowed to the recipients of property income. This may well have represented a reversal of trends in the nineteenth century, when some argue that property income
contracted in the wake of the Insurgency[41].

There was also significant industrialization in Mexico during the Porfiriato. Some industry, especially textiles, had its origins in the 1840s, but its size, scale and location altered dramatically by the end of the nineteenth century. For example, the cotton textile industry saw the number of workers, spindles and looms more than double from the late 1870s to the first decade of the nineteenth century. Brewing and its associated industry, glassmaking, became well established in Monterrey during the 1890s. The country’s first iron and steel mill, Fundidora Monterrey, was established there as well in 1903. Other industries, such as papermaking and cigarettes followed suit. By the end of the Porfiriato, over 10 percent of Mexico’s output was certainly industrial.[42]

From Revolution to “Miracle”

The Mexican Revolution (1910-1940) began as a political upheaval provoked by a crisis in the presidential succession when Porfirio Díaz refused to leave office in the wake of electoral defeat after signaling his willingness to do so in a famous public interview of 1908.[43] It was also the result of an agrarian uprising and the insistent demand of Mexico’s growing industrial proletariat for a share of political power. Finally, there was a small (fewer than 10 percent of all households) but upwardly mobile urban middle class created by economic development under Díaz whose access to political power had been effectively blocked by the regime’s mechanics of political control. Precisely how “revolutionary” were the results of the armed revolt—which persisted largely through the 1910s and peaked in a civil war in 1914-1915—has long been contentious, but is only tangentially relevant as a matter of economic history. The Mexican Revolution was no Bolshevik movement (of course, it predated Bolshevism by seven years) but it was not a purely bourgeois constitutional movement either, although it did contain substantial elements of both.

From a macroeconomic standpoint, it has become fashionable to argue that the Revolution had few, if any, profound economic consequences. It seems as if the principal reason was that revolutionary factions were interested in appropriating rather than destroying the means of production. For example, the production of crude oil peaked in Mexico in 1915—at the height of the Revolution—because crude oil could be used as a source of income to the group controlling the wells in Veracruz state. This was a powerful consideration.[44]

Yet in another sense, the conclusion that the Revolution had slight economic effects is not only facile, but obviously wrong. As the demographic historian Robert McCaa showed, the excess mortality occasioned by the Revolution was larger than any similar event in Mexican history other than the conquest in the sixteenth century. There has been no attempt made to measure the output lost by the demographic wastage (including births that never occurred), yet even the effect on the population cohort born between 1910 and 1920 is plain to see in later demographic studies. [45]

There is also a subtler question that some scholars have raised. The Revolution increased labor mobility and the labor supply by abolishing constraints on the rural population such as debt peonage and even outright slavery. Moreover, the Revolution, by encouraging and ultimately setting into motion a massive redistribution of previously privatized land, contributed to an enlarged supply of that factor of production as well. The true impact of these developments was realized in the 1940s and 1950s, when rapid economic growth began, the so-called Mexican Miracle, which was characterized by rates of real growth of as much as 6 percent per year (1955-1966). Whatever the connection between the Revolution and the Miracle, it will require a serious examination on empirical grounds and not simply a dogmatic
dismissal of what is now regarded as unfashionable development thinking: import substitution and inward-oriented growth.[46]

The other major consequence of the Revolution, the agrarian reform and the creation of the *ejido*, or land granted by the Mexican state to rural population under the authority provided it by the revolutionary Constitution on 1917 took considerable time to coalesce, and were arguably not even high on one of the Revolution’s principal instigators, Francisco Madero’s, list of priorities. The redistribution of land to the peasantry in the form of possession if not ownership – a kind of return to real or fictitious preconquest and colonial forms of land tenure – did peak during the avowedly reformist, and even modestly radical presidency of Lázaro Cárdenas (1934-1940) after making only halting progress under his predecessors since the 1920s. From 1940 to 1965, the cultivated area in Mexico grew at 3.7 percent per year and the rise in productivity in basic food crops was 2.8 percent per year.

Nevertheless, the long-run effects of the agrarian reform and land redistribution have been predictably controversial. Under the presidency of Carlos Salinas (1988-1994) the reform was officially declared over, with no further land redistribution to be undertaken and the legal status of the *ejido* definitively changed. The principal criticism of the ejido was that, in the long run, it encouraged inefficiently small landholding per farmer and, by virtue of its limitations on property rights, made agricultural credit difficult for peasants to obtain.[47]

There is no doubt these are justifiable criticisms, but they have to be placed in context. Cárdenas’ predecessors in office, Alvaro Obregón (1924-1928) and Plutarco Elías Calles (1928-1932) may well have preferred a more commercial model of agriculture with larger, irrigated holdings. But it is worth recalling that one of the original agrarian leaders of the Revolution, Emiliano Zapata, had an uneasy relationship with Madero, who saw the Revolution in mostly political terms, from the start and quickly rejected Madero’s leadership in favor of restoring peasant lands in his native state of Morelos. Cárdenas, who was in the midst of several major maneuvers that would require widespread popular support—such as the expropriation of foreign oil companies operating in Mexico in March 1938—was undoubtedly sensitive to the need to mobilize the peasantry on his behalf. The agrarian reform of his presidency, which surpassed that of any other, needs to be considered in those terms as well as in terms of economic efficiency.[48]

Cárdenas’ presidency also coincided with the continuation of the Great Depression. Like other countries in Latin America, Mexico was hard hit by the Great Depression, at least through the early 1930s. All sorts of consumer goods became scarcer, and the depreciation of the peso raised the relative price of imports. As had happened previously in Mexican history (1790-1810, during the Napoleonic Wars and the disruption of the Atlantic trade), in the medium term domestic industry was nevertheless given a stimulus and import substitution, the subsequent core of Mexico’s industrialization program after World War II, was given a decisive boost. On the other hand, Mexico also experienced the forced “repatriation” of people of Mexican descent, mostly from California, of whom 60 percent were United States citizens. The effects of this movement—the emigration of the Revolution in reverse—has never been properly analyzed. The general consensus is that World War II helped Mexico to prosper. Demand for labor and materials from the United States, to which Mexico was allied, raised real wages and incomes, and thus boosted aggregate demand. From 1939 through 1946, real output in Mexico grew by approximately 50 percent. The growth in population accelerated as well as the country began to move into the later stages of the demographic transition, with a falling death rate, while birth rates remained high.[49]
From Miracle to Meltdown: 1950-1982

The history of import substitution manufacturing did not begin with postwar Mexico, but few countries (especially in Latin America) became as identified with the policy in the 1950s, and with what Mexicans termed the emergence of “stabilizing development.” There was never anything resembling a formal policy announcement, although Raúl Prebisch’s 1949 manifesto, “The Economic Development of Latin America and its Principal Problems” might be regarded as supplying one. Prebisch’s argument, that a directed change in the composition of imports toward capital goods to facilitate domestic industrialization was, in essence, the basis of the policy that Mexico followed. Mexico stabilized the nominal exchange rate at 12.5 pesos to the dollar in 1954, but further movement in the real exchange rate (until the 1970s) were unimportant. The substantive bias of import substitution in Mexico was a high effective rate of protection to both capital and consumer goods. Jaime Ros has calculated these rates in 1960 ranged between 47 and 85 percent, and between 33 and 109 percent in 1980. The result, in the short to intermediate run, was very rapid rates of economic growth, averaging 6.5 percent in 1950 through 1973. Other than Brazil, which also followed an import substitution regime, no country in Latin America experienced higher rates of growth. Mexico’s was substantially above the regional average. [50]

[See the historical graph of population growth in Mexico through 2000 below]

Source: Essentially, Estadísticas Históricas de México (various editions since 1999; the most recent is 2014)


But there were unexpected results as well. The contribution of labor to GDP growth was 14 percent. Capital’s contribution was 53 percent, and the remainder, total factor productivity (TFP) 28 percent.[51] As a consequence, while Mexico’s growth occurred through the accumulation of capital, the distribution of income became extremely skewed. The ratio of the top 10 percent of household income to the
bottom 40 percent was 7 in 1960, and 6 in 1968. Even supporters of Mexico’s development program, such as Carlos Tello, conceded that it probable that it was the organized peasants and workers experienced an effective improvement of their relative position. The fruits of the Revolution were unevenly distributed, even among the working class.[52]

By “organized” one means such groups as the most important labor union in the country, the CTM (Confederation of Mexican Workers) or the nationally recognized peasant union, the CNC, both of which formed two of the three organized sectors of the official government party, the PRI, or Party of the Institutional Revolution that was organized in 1946. The CTM in particular was instrumental in supporting the official policy of import substitution, and thus benefited from government wage setting and political support. The leaders of these organizations became important political figures in their own right. One, Fidel Velázquez, as both a federal senator and the head of the CTM from 1941 to his death in 1997. The incorporation of these labor and peasant groups into the political system offered the government both a means of control and a guarantee of electoral support. They became pillars of what the Peruvuan writer Mario Vargas Llosa famously called “the perfect dictatorship” of the PRI from 1946 to 2000, during which the PRI held a monopoly of the presidency and the important offices of state. In a sense, import substitution was the economic ideology of the PRI.[53]

Labor and economic development during the years of rapid growth is, like many others, a debated subject. While some have found strong wage growth, others, looking mostly at Mexico City, have found declining real wages. Beyond that, there is the question of informality and a segmented labor market. Were workers in the CTM the real beneficiaries of economic growth, while others in the informal sector (defined as receiving no social security payments, meaning roughly two-thirds of Mexican workers) did far less well? Obviously, the attraction of a segmented labor market model can address one obvious puzzle: why would industry substitute capital for labor, as it obviously did, if real wages were not rising? Postulating an informal sector that absorbed the rapid influx of rural migrants and thus held nominal wages steady while organized labor in the CTM got the benefit of higher negotiated wages, but in so doing, limited their employment is an attractive hypothesis, but would not command universal agreement. Nothing has been resolved, at least for the period of the “Miracle.” After Mexico entered a prolonged series of economic crises in the 1980s—here labelled as “meltdown”—the discussion must change, because many hold that the key to relative political stability and the failure of open unemployment to rise sharply can be explained by falling real wages.

The fiscal basis on which the years of the Miracle were constructed was conventional, not to say conservative.[54] A stable nominal exchange rate, balanced budgets, limited public borrowing, and a predictable monetary policy were all predicated on the notion that the private sector would react positively to favorable incentives. By and large, it did. Until the late 1960s, foreign borrowing was considered inconsequential, even if there was some concern on the horizon that it was starting to rise. No one foresaw serious macroeconomic instability. It is worth consulting a brief memorandum from Secretary of State Dean Rusk to President Lyndon Johnson (Washington, December 11, 1968) –to get some insight into how informed contemporaries viewed Mexico. The instability that existed was seen as a consequence of heavy-handedness on the part of the PRI and overreaction in the security forces. Informed observers did not view Mexico’s embrace of import-substitution industrialization as a train wreck waiting to happen. Historical actors are rarely so prescient.[55]
The most obvious problems in Mexico were political. They stemmed from the increasing awareness that the limits of the “institutional revolution” had been reached, particularly regarding the growing democratic demands of the urban middle classes. The economic problem, which was far from obvious, was that import substitution had concentrated income in the upper 10 per cent of the population, so that domestic demand had begun to stagnate. Initially at least, public sector borrowing could support a variety of consumption subsidies to the population, and there were also efforts to transfer resources out of agriculture via domestic prices for staples such as maize. Yet Mexico’s population was also growing at the rate of nearly 3 percent per year, so that the long term prospects for any of these measures were cloudy.

At the same time, growing political pressures on the PRI, mostly dramatically manifest in the army’s violent repression of student demonstrators at Tlatelolco in 1968 just prior to the Olympics, had convinced some elements in the PRI, people like Carlos Madrazo, to argue for more radical change. The emergence of an incipient guerilla movement in the state of Guerrero had much the same effect. The new president, Luis Echeverría (1970–76), openly pushed for changes in the distribution of income and wealth, incited agrarian discontent for political purposes, dramatically increased government spending and borrowing, and alienated what had typically been a complaisant, if not especially friendly private sector.

The country’s macroeconomic performance began to deteriorate dramatically. Inflation, normally in the range of about 5 percent, rose into the low 20 percent range in the early 1970s. The public sector deficit, fueled by increasing social spending, rose from 2 to 7 percent of GDP. Money supply growth now averaged about 14 percent per year. Real GDP growth had begun to slip after 1968 and in the early 1970s, in deteriorated more, if unevenly. There had been clear convergence of regional economies in Mexico between 1930 and 1980 because of changing patterns of industrialization in the northern and central regions of the country. After 1980, that process stalled and regional inequality again widened. [56]

While there is a tendency to blame Luis Echeverría for all or most of these developments, this forgets that his administration coincided with the First OPEC oil shock (1973) and rapidly deteriorating external conditions. Mexico had, as yet, not discovered the oil reserves (1978) that were to provide a temporary respite from economic adjustment after the shock of the peso devaluation of 1976—the first change in its value in over 20 years. At the same time, external demand fell, principally transmitted from the United States, Mexico’s largest trading partner, where the economy had fallen into recession in late 1973. Yet it seems reasonable to conclude that the difficult international environment, while important in bringing Mexico’s “miracle” period to a close, was not helped by Echeverría’s propensity for demagoguery, of the loss of fiscal discipline that had long characterized government policy, at least since the 1950s. The only question to be resolved was to what sort of conclusion the period would come. The answer, unfortunately, was disastrous. [57]

Meltdown: The Debt Crisis, the Lost Decade and After

In contemporary parlance, Mexico had passed from “stabilizing” to “shared” development under Echeverría. But the devaluation of 1976 from 12.5 to 20.5 pesos to the dollar suggested that something had gone awry. One might suppose that some adjustment in course, especially in public spending and borrowing, would have occurred. But precisely the opposite occurred. Between 1976 and 1979, nominal federal spending doubled. The budget deficit increased by a factor of 15. The reason for this odd
performance was the discovery of crude oil in the Gulf of Mexico, perhaps unsurprising in light of the spiking prices of the 1970s (the oil shocks of 1973-74, 1978-79), but nevertheless of considerable magnitude. In 1975, Mexico’s proven reserves were 6 billion barrels of oil. By 1978, they had increased to 40 billion. President López Portillo set himself to the task of “administering abundance” and Mexican analysts confidently predicted crude oil at $100 a barrel (when it stood at $37 in current prices in 1980). The scope of the miscalculation was catastrophic. At the same time, encouraged by bank loan pushing and effectively negative real rates of interest, Mexico borrowed abroad. Consumption subsidies, while vital in the face of slowing import substitution, were also costly, and when supported by foreign borrowing, unsustainable, but foreign indebtedness doubled between 1976 and 1979, and even further thereafter.

Matters came to a head in 1982. By then, Mexico’s foreign indebtedness was estimated at over $80 billion dollars, an increase from less than $20 billion in 1975. Real interest rates had begun to rise in the United States in mid-1981, and with Mexican borrowing tied to international rates, debt service rapidly increased. Oil revenue, which had come to constitute the great bulk of foreign exchange, followed international crude prices downward, driven in large part by a recession that had begun in the United States in mid-1981. Within six months, Mexico, too, had fallen into recession. Real per capital output was to decline by 8 percent in 1982. Forced to sharply devalue, the real exchange rate fell by 50 percent in 1982 and inflation approached 100 percent. By the late summer, Finance Minister Jesus Silva Herzog admitted that the country could not meet an upcoming payment obligation, and was forced to turn to the US Federal Reserve, to the IMF, and to a committee of bank creditors for assistance. In late August, in a remarkable display of intemperance, President López Portillo nationalized the banking system. By December 20, 1982, Mexico’s incoming President, Miguel de la Madrid (1982-88) appeared, beleaguered, on the cover of Time Magazine framed by the caption, “We are in an Emergency.” It was, as the saying goes, a perfect storm, and with it, the Debt Crisis and the “Lost Decade” in Mexico had begun. It would be years before anything resembling stability, let alone prosperity, was restored. Even then, what growth there was a pale imitation of what had occurred during the decades of the “Miracle.”

The 1980s

The 1980s were a difficult decade. After 1981, annual real per capita growth would not reach 4 percent again until 1989, and in 1986, it fell by 6 percent. In 1987, inflation reached 159 percent. The nominal exchange rate fell by 139 percent in 1986-1987. By the standards of the years of stabilizing development, the record of the 1980s was disastrous. To complete the devastation, on September 19, 1985, the worst earthquake in Mexican history, 7.8 on the Richter Scale, devastated large parts of central Mexico City and killed 5 thousand (some estimates run as high as 25 thousand), many of whom were simply buried in mass graves. It was as if a plague of biblical proportions had struck the country.

Massive indebtedness produced a dramatic decline in the standard of living as structural adjustment occurred. Servicing the debt required the production of an export surplus in non-oil exports, which in turn, required a reduction in domestic consumption. In an effort to surmount the crisis, the government implemented an agreement between organized labor, the private sector, and agricultural producers called the Economic Solidarity Pact (PSE). The PSE combined an incomes policy with fiscal austerity, trade and financial liberalization, generally tight monetary policy, and debt renegotiation and reduction. The centerpiece of the “remaking” of the previously inward orientation of the domestic economy was the North American Free Trade Agreement (NAFTA, 1993) linking Mexico, the United States, and
Canada. While average tariff rates in Mexico had fallen from 34 percent in 1985 to 4 percent in 1992—even before NAFTA was signed—the agreement was generally seen as creating the institutional and legal framework whereby the reforms of Miguel de la Madrid and Carlos Salinas (1988-1994) would be preserved. Most economists thought its effects would be relatively larger in Mexico than in the United States, which generally appears to have been the case. Nevertheless, NAFTA has been predictably controversial, as trade agreements are wont to be. The political furor (and, in some places, euphoria) surrounding the agreement have faded, but never entirely disappeared. In the United States in particular, NAFTA is blamed for deindustrialization, although pressure on manufacturing, like trade liberalization itself, was underway long before NAFTA was negotiated. In Mexico, there has been much hand wringing over the fate of agriculture and small maize producers in particular. While none of this is likely to cease, it is nevertheless the case that there has been a large increase in the volume of trade between the NAFTA partners. To dismiss this is, quite plainly, misguided, even where sensitive and well organized political constituencies are concerned. But the legacy of NAFTA, like most everything in Mexican economic history, remains unsettled.

Post Crisis: No Miracles

Still, while some prosperity was restored to Mexico by the reforms of the 1980s and 1990s, the general macroeconomic results have been disappointing, not to say mediocre. The average real compensation per person in manufacturing in 2008 was virtually unchanged from 1993 according to the Instituto Nacional De Estadística Geografía e Informática, and there is little reason to think the compensation has improved at all since then. It is generally conceded that per capita GDP growth has probably averaged not much more than 1 percent a year. Real GDP growth since NAFTA according to the OECD has rarely reached 5 percent and since 2010, it has been well below that.


For virtually everyone in Mexico, the question is why, and the answers proposed include virtually any plausible factor: the breakdown of the political system after the PRI’s historic loss of presidential power in 2000; the rise of China as a competitor to Mexico in international markets; the explosive spread of narcoviolence in recent years, albeit concentrated in the states of Sonora, Sinaloa, Tamaulipas, Nuevo León and Veracruz; the results of NAFTA itself; the failure of the political system to undertake further structural economic reforms and privatizations after the initial changes of the 1980s, especially regarding the national oil monopoly, Petroleos Mexicanos (PEMEX); the failure of the border industrialization program (maquiladoras) to develop substantive backward linkages to the rest of the economy. This is by no means an exhaustive list of the candidates for poor economic performance. The choice of a cause tends to reflect the ideology of the critic.[59]

Yet it seems that, at the end of the day, the reason why post-NAFTA Mexico has failed to grow comes down to something much more fundamental: a fear of growing, embedded in the belief that the collapse of the 1980s and early 1990s (including the devastating “Tequila Crisis” of 1994-1995, which resulted in a
another enormous devaluation of the peso after an initial attempt to contain the crisis was bungled) was so traumatic and costly as to render even modest efforts to promote growth, let alone the dirigisme of times past, as essentially unwarranted. The central bank, the Banco de México (Banxico) rules out the promotion of economic growth as part of its remit—even as a theoretical proposition, let alone as a goal of macroeconomic policy—and concerns itself only with price stability. The language of its formulation is striking. “During the 1970s, there was a debate as to whether it was possible to stimulate economic growth via monetary policy. As a result, some governments and central banks tried to reduce unemployment through expansive monetary policy. Both economic theory and the experience of economies that tried this prescription demonstrated that it lacked validity. Thus, it became clear that monetary policy could not actively and directly stimulate economic activity and employment. For that reason, modern central banks have as their primary goal the promotion of price stability” (translation mine). Banxico is not the Fed: there is no dual mandate in Mexico.[60]

The Mexican banking system has scarcely made things easier. Private credit stands at only about a third of GDP. In recent years, the increase in private sector savings has been largely channeled to government bonds, but until quite recently, public sector deficits were very small, which is to say, fiscal policy has not been expansionary. If monetary and fiscal policy are both relatively tight, if private credit is not easy to come by, and if growth is typically presumed to be an inevitable concomitant to economic stability for which no actor (other than the private sector) is deemed responsible, it should come as no surprise that economic growth over the past two decades has been lackluster. In the long run, aggregate supply determines real GDP, but in the short run, nominal demand matters: there is no point in creating productive capacity to satisfy demand that does not exist. And, unlike during the period of the Miracle and Stabilizing Development, attention to demand since 1982 has been limited, not to say off the table completely. It may be understandable, but Mexico’s fiscal and monetary authorities seem to suffer from what could be termed, “Fear of Growth.” For better or worse, the results are now on display. After its current (2016) return to a relatively austere budget, it remains to be seen how the economic and political system in contemporary Mexico handles slow economic growth. For that would now seem to be, in a basic sense, its largest challenge for the future.

[1] I am grateful to Ivan Escamilla and Robert Whaples for their careful readings and thoughtful criticisms.


(Accessed July 10, 2016)


This is an estimate. David Ringrose concluded that in the 1780s, the colonies accounted for 45 percent of Crown income, and one would suppose that Mexico would account for at least about half of that. See David R. Ringrose, *Spain, Europe and the 'Spanish Miracle', 1700-1900* (New York: Cambridge University Press, 1996), p. 93; Mauricio Drelichman, “The Curse of Moctezuma: American Silver and the Dutch Disease,” *Explorations in Economic History* 42:3 (2005), pp. 349-380.


The best, and indeed, virtually unique starting point for considering these changes in their broadest dimensions are the joint works of Stanley and Barbara Stein: *Silver, Trade, and War* (2003); *Apogee of Empire* (2004), and *Edge of Crisis* (2010), All were published by Johns Hopkins University Press and do for the Spanish Empire what Laurence Henry Gipson did for the First British Empire.

The key work is María Eugenia Romero Sotelo, *Minería y Guerra. La economía de Nueva España, 1810-1821* (México, DF: UNAM, 1997)


An agricultural worker who worked full time, 6 days a week, for the entire year (a strong assumption), in Central Mexico could have expected cash income of perhaps 24 pesos. If food, such as beans and tortilla were added, the whole pay might reach 30. The figure of 40 pesos comes from considerably richer agricultural lands around the city of Querétaro, and includes as an average income
from nonagricultural employment as well, which was higher. Measuring Worth would put the relative historic standard of living value in 2010 prices at $1.040, with the caveat that this is relative to a bundle of goods purchased in the United States. 


[35] See Challú and Gómez Galvarriato, “Real Wages,” Figure 5, p. 101.


For a contemporary account with a sense of the immediacy of the end of the Echeverría regime, see “Así se devaluó el peso,” Proceso, November 13, 1976.


See, for example, Jaime Ros Bosch, Algunas tesis equivocadas sobre el estancamiento económico de México (México, DF: El Colegio de México, 2013).

This is the age of blockbuster books in economics. By any metric, Robert Gordon's new tome qualifies. It tackles a grand subject, the productivity slowdown, by placing the slowdown in the context of the historical evolution of the American standard of living. Gordon, who is the Stanley G. Harris Professor in the Social Sciences at Northwestern University, needs no introduction, having long been one of the most famous macroeconomists on planet Earth.

The Rise and Fall of American Growth is divided into three parts. Part One (chapters 2-9) examines various components of the standard of living, in levels and changes from 1870 to 1940. Part Two (chapters 10-15) does the same from 1940 to the present, maintaining the same relative order of topics (e.g. transportation appears after housing in both parts). Part Three (chapters 16-18) provides explanations and offers predictions up through 2040. There are brief interludes ("Entre’acte") between parts, a Postscript, and a detailed Data Appendix.

Chapter 1 is an overview of the focus, approach, and structure of the book. Gordon’s focus is on the standard of living of American households from 1870 to the present. The approach is both quantitative — familiar to economists — and qualitative — familiar to historians. As already noted, the organization is symmetric — Part One considers the pre-World War II period, and Part Two, the post-war. The fundamental point of the book is that some post-1970 slowdown in growth was inevitable, because so much of what was revolutionary about technology in the first half of the twentieth century was revolutionary only once.

Chapter 2 draws a bleak picture of the standard of living ca. 1870, the dawn of Robert Gordon’s modern America. From the standpoint of a household in 2016, conditions of life in 1870 would appear to be revolting. The diet was terrible and monotonous to boot; homemade clothing was ill-fitting and crudely made; transportation was dependent principally on the horse, which generated phenomenal amounts of waste; indoor plumbing was all but non-existent; rural Americans lived their lives largely in isolation of the wider world. In Gordon’s view, much of this is missing from conventional real GNP estimates.

Chapter 3 continues the initial story, focusing on changes in food and clothing consumption. Gordon contends there was not much change in underlying quality but he argues that, by the 1920s, consumers were paying lower prices for food — having shifted to lower-priced sources (chain stores as opposed to country merchants) — and that most clothing was store-bought rather than homemade.

Chapter 4 studies housing quality. As with other consumer goods, housing also improved sharply in quality from 1870 to 1940. Gordon argues that much farm housing was poor in quality, while new urban housing was typically larger and more durably built. Indoor plumbing, appliances and, ultimately, electrification dramatically enhanced the quality of life while people were indoors. As elsewhere in the book, reference is made to hedonic estimates of the value of these improvements as revealed in higher rents. Chapter 5 details improvements in transportation between 1870 and 1940. These are grouped into three categories. The first is improvement in inter-city and inter-regional transportation in rail. This occurs chiefly through improvements in the density of lines and in the speed of transit. The second is intra-city which occurred with the adoption of the electric streetcar. The third, and most important arguably, is the internal combustion engine and its use in the automobile (and bus). Gordon especially highlights improvements in the quality of automobiles, noting that the car is not reflected in standard price indices until the middle of the Great Depression.

Chapter 6 details advances in communication from 1870 to 1940. By current standards, the relevant changes — the telegraph, telephone, the phonograph, and the radio — might not seem like much but from the point of view of a household in 1870, these technologies enabled Americans to dramatically...
reduce their isolation. As Gordon points out, one could phone a neighbor to see if she had a cup of sugar rather than visit in person, or listen to Enrico Caruso’s voice on the phonograph if it were not possible to hear him in concert. The radio brought millions of Americans into the national conversation, whether it was to hear one of Franklin Roosevelt’s fireside chats or listen to a baseball game. Chapter 7 discusses improvements in health and mortality from 1870 to 1940 which, according to Gordon, were unprecedented. After summarizing these, he turns to causes, chief among which are improved urban sanitation, clean water, and uncontaminated milk. Gordon also highlights improvements in medical knowledge, particularly the diffusion (and understanding) of the germ theory of disease. Chapter 8 studies changes in the quality of work from 1870 to 1940. These changes were wholly for the better, according to Gordon. Work became less dangerous, more interesting, and more rewarding in terms of real wages. Most importantly, there was less working per se, as weekly hours fell, freeing up time for leisure activity. There was a marked reduction in child labor, as children spent more of their time in school, particularly at older ages in high school. This was also the period leading up, as Claudia Goldin has told us, to the “Quiet Revolution” in the labor force participation of married women, which was to increase substantially after World War II. Credit and insurance, private and social, is the topic of Chapter 9. The ability to better smooth consumption and also insure against calamity are certainly improvements in living standards that are not captured by standard GNP price deflators. Initially the shift of households from rural to urban areas arguably coincided with a decrease in consumer credit but by the 1920s credit was on the rise due to several innovations previously documented by economic historians such as Martha Olney. Households were also better able to obtain insurance of various types (e.g. life, fire, automobile); in particular, loans against life insurance were frequently used as a source for a down payment on a house or car. Government contributed by expanding social insurance and other programs that helped reduced systemic risks.

Chapter 10 begins the second part of the book, which focuses on the period from 1940 to the present. As noted, the topic order of Part Two is the same as Part One, so Chapter 10 focuses on food, clothing, and shelter. Gordon considers the changes in quality in these dimensions of the standard of living to be less monumental than as occurred before World War II. For example, frozen food became a ubiquitous option after World War II but this change is far less important than the pre-1940 improvement in the milk supply. Quantitatively, perhaps the most important change was a reduction in relative food prices which, predictably, led to increase in the quantity demanded. Calories jumped, and so did obesity and many related health problems. For clothing, the chief difference is in the diversity of styles and, as with food, a sharp reduction in relative price holding quality constant. In Chapter 11 Gordon notes that automobiles continued to improve in quality after World War II, mostly in terms of amenities and gas mileage; and their usefulness as transportation improved with the building of the interstate highway system. Gordon is less sanguine about air transportation, arguing that quality of the travel experience deteriorated after deregulation which was not offset by reductions in relative prices. For housing, the major changes was suburbanization and a concomitant increase in square footage. The early postwar period witnessed some sharp improvements in the quality of basic household appliances, and somewhat later, the widespread diffusion of air conditioning and microwaves.

Chapter 12 focuses on media and entertainment post-1940. Certain older forms of entertainment gave way to television, the initial benefits of which were followed by steady improvements in the quality of transmission and reception. Similarly, there were sharp improvements in the various platforms for listening to music, with substantial advances in recording technology and delivery — the 78 gave way to the LP to the CD to music streaming and YouTube. The technology to deliver entertainment also delivered the news in ever greater quantity (quality is in the eye of the beholder, I suppose). Americans
today are connected almost immediately to every part of the world, a level of communications unthinkable a century ago. A surprisingly brief Chapter 13, recounts the history of the modern computer. There is no way to tell this history without emphasizing just how unprecedented the improvements have been, from the very first post-war computers to today’s laptops and supercomputers. Moore’s Law, understandably, takes center stage, followed by the Internet and e-commerce. Gordon has a few negative things to say about the worldwide web, but the main act — why haven’t computers led a revolution in productivity — is saved for later in the book.

Chapter 14 continues the story of health improvements to the present day. As everyone knows, the U.S. health care system changed markedly after World War II, in terms of delivery of services, organization, and payment schemes. Great advances were made in cardiovascular care and treatment of infectious disease through the use of antibiotics. There were also advances in cancer treatment, mostly achieved by the 1970s; the subsequent “war” on cancer has not been as successful. Most of the benefits were achieved through diffusion of public health and expansion of health knowledge in the general public (e.g. the harmful effects of smoking). Since 1970 the health care system has shifted to more expensive, capital intensive treatments primarily provided in hospitals that have led to an inexorable growth in medical care’s share of GNP, increases that most scholars agree exceed any improvements in health outcomes. The chapter concludes with a mixed assessment of Obamacare. Chapter 15, on the labor force, is also rather short for its subject matter. Gordon recounts the major changes in the structure and composition of work since World War II. Again, it is a familiar tale — improved working conditions due to the shift towards the service sector and “indoor” jobs; rising labor force participation for married women; rising educational attainment, at least until recently; and the retirement revolution. Your faithful reviewer gets a shout-out in a brief discussion of the “Great Compression” of the 1940s; my collaborator in that work, Claudia Goldin (and her collaborator, Lawrence Katz) gets much more attention for her scholarly contributions on the subject matter of Chapter 15, understandably so.

Part Three addresses explanations for the time series pattern in the standard of living. Chapter 16 focuses on the first half of the twentieth century, which experienced a marked jump in total factor productivity (TFP) growth and the standard of living. Gordon considers several explanations, dismissing two prominent ones — education and urbanization — right out of the gate. In paeans to Paul David and Alex Field, he argues that the speed-up in TFP growth can be attributed to the eventual diffusion of key inventions of the “Second” industrial revolution, such as electricity; to the New Deal; and, finally, to World War II. Chapters 17 and 18 tackle the disappointing performance of TFP growth and the standard of living in the last several decades of U.S. economic history. Despite remarkable accomplishments in science and technology the impact on average living standards has been small, compared with the 1920-70 period. Rising inequality since 1970, which can be tied in part to skill-biased technical change, has made matters worse, as did the Great Recession. While Gordon is not all doom and gloom, he definitely falls on the pessimist side of the optimist-pessimist spectrum — his prediction for labor productivity growth over the 2015-40 period is 1.2 percent per year, a full third lower than the observed rate of growth from 1970 to 2014.

I think it is next to impossible to write a blockbuster economics book without it being a mixed bag in some way or other. Gordon’s is no exception. On the plus side, the book is well written, and one can only be in awe of Gordon’s mastery of the factual history of the American standard of living. We all know macroeconomists who dabble in the past. Gordon is no dabbler. One can find interesting ideas for future (professional-level) research in every chapter — graduate students in search of topics for second year or job market papers, take note. Many previous reviewers have chided Gordon for his
pessimistic assessment of future prospects. Of course, no one knows the future, and that includes Gordon. It is certainly possible that he will be wrong about productivity growth over the next quarter-century — but I for one will be surprised if his prediction is off by, say, an order of magnitude.

I am less sanguine about the mixed qualitative-quantitative method of the book. I gave up reading the history-of-technology-as-written-by-historians-of-technology a long time ago because it was just one-damn-invention-after-another. At the end of a typical article recounting the history of improvements in, say, food processing, I was supposed to conclude that no amount of money would get me to travel back in the past before said improvements took place — except I never did reach this conclusion, knowing it to be fundamentally wrong. Despite references to hedonic estimation, TFP, and the like, in the end Gordon’s book reads very much like conventional history of technology. More than a half century ago Robert Fogel showed how one could quantify the social savings of a particular invention, thereby truly advancing scholarly knowledge of the treatment effects. Yet *Railroads and American Economic Growth* is not even cited in Gordon’s bibliography, let alone discussed in the text. If one’s focus is the aggregate, I suppose a Fogelian approach is impossible — there are too many inventions, and (presumably) an adding-up problem to boot. What exactly, though, do we learn from going back and forth between quantitative TFP and qualitative one-damn-invention-after-another? I’m not sure. There’s the rub, or rather, the tradeoff.

Criticisms aside, if you are into economics blockbusters, *The Rise and Fall of American Growth* belongs on your bookshelf, next to Piketty and the like. Just be sure it is a heavy-duty bookshelf.


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**Subject(s):** Economic Development, Growth, and Aggregate Productivity  
History of Technology, including Technological Change  
Household, Family and Consumer History  
Living Standards, Anthropometric History, Economic Anthropology

**Geographic Area(s):** North America

**Time Period(s):** 19th Century  
20th Century: Pre WWII  
20th Century: WWII and post-WWII

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**Andrew Carnegie: An Economic Biography**

**Author(s):** Bostaph, Samuel  
**Reviewer(s):** Rogers, Robert P.

Reviewed for EH.Net by Robert P. Rogers, College of Business and Economics, Ashland University.

This book, a concise biography of Andrew Carnegie, focuses on some important issues concerning his business and philanthropy.

Carnegie’s life was a rags-to-riches Horatio Alger story but with an interesting twist. While he was born a poor Scot, his family was well-read and knowledgeable about their surroundings. Carnegie used a succession of seemingly prosaic but strategically placed jobs to become a major executive with the Pennsylvania Railroad. He employed his connections there to start several businesses supplying the railroad with important inputs such as bridges and rails. Eventually he built a steel rail firm using the Bessemer process.

His ability to obtain financing and find competent executives and engineers enabled Carnegie to develop a large efficient steel firm. Among the people he attracted to the firm were Henry Frick and Alexander Holley. Frick and Holley were pioneers in the development of, respectively, coke ovens and Bessemer furnaces. Not only did the firm capitalize on the demand for rails, but it also became the leading firm in construction steel. To do this, the firm employed another new steel furnace, the open hearth. By the 1890s, Carnegie’s company had become the leading steel firm in the world.

Given its brevity and its focus on the issues of government intervention, firm governance, and property rights, this is the biography that I would recommend to a generalist wanting to understand Carnegie.

On three issues, however, I see problems with the analysis. They concern tariffs, railroads, and the intertwined issues of property rights and governance. Ironically, it is on the latter two issues that Bostaph breaks new ground, but there are still questions.

Bostaph overstates the role of tariffs in Carnegie’s success. Fogel (1964) and Temin (1964) have ascertained that protection had some positive effect on the success of the American steel industry. Nevertheless, it seems unlikely that absent tariffs a country as well endowed with coal, iron ore, and human capital as the United States would not have developed a large domestic steel industry. Had a free trade regime existed in the United States, Maine might have had ten Carnegie libraries instead of fifteen. Carnegie might have been rich anyway but not as rich.

My second quibble with Bostaph is the relationship of railroads to steel. Many writers have correctly posited that through subsidies the government unnecessarily encouraged railroads. Yet, it is likely that the United States would have developed a large railroad system without this government help.

I have intimated that Bostaph’s major contribution concerns firm governance and property rights. The major issues were the Carnegie firm’s relationships between two of its human inputs — factory workers and firm executives.

Most interesting is Bostaph’s analysis of the Homestead strike. Many workers viewed their jobs as a property right. This idea was based on the labor theory of value that asserts that the value in an item arises from the work put into making it. Bostaph rightly argues that this theory is wrong citing the nineteenth century marginalist economist, Karl Menger. The marginalists posit that the value of an item arises from the utility that it gives to the user who pays for it. Pieces of iron ore do not have any value
until they are metamorphosed into items that can be used such as rails or beams. Entrepreneurs like Carnegie figured out how to combine capital and labor to produce the items that users will buy.

Given Bostaph’s analysis, still, Carnegie might have developed an efficient job property rights system for factory jobs. Law and accounting firms are so organized. Other steel firms have developed with the factory job as a property right. The above contention, however, cannot be confirmed or refuted.

Nevertheless, Carnegie did give property rights to some of his labor — managers. By making them partners in the enterprise, he gave them a stake in the enterprise, i.e. a property right. Until the middle 1890s, the Carnegie firm was a large complicated partnership. It is not clear how well it worked. Through much of the firm’s history, however, there existed extensive conflicts between the various manager-partners. From reading Bostaph’s account of the firm in the 1890s, one has to wonder how steel got made given all the squabbling.

While this system was efficient to a degree, a better system might have been developed. A piece of evidence is the attitude of W. J. Jones, the illustrious manager of the Homestead works (some time before the strike). Refusing a partnership, he demanded and got a high salary (equal to the President of the United States). Might he have seen the problems with a partnership with Carnegie?

Furthermore, there were large firms that ran more smoothly and efficiently at that time. Among them was John D. Rockefeller’s Standard Oil. Rockefeller has been subject to much criticism, but most scholars compliment his internal management.

Interestingly, while Bostaph does a good job describing the conflicts between Carnegie and his partners, I am not sure he understands its possible implications. U.S. Steel, the Carnegie successor firm, was noted for lackluster management. Many scholars have attributed it to x-inefficiency and management’s emphasis on getting along with the government (Rogers, 2009). Perhaps, the competitive problems with the American steel industry had their start with Carnegie’s inability to develop an efficient managerial system.

I got this admittedly tentative insight from Bostaph’s book. By putting the firm’s history into an alternative economic context, this book reveals much about Carnegie and the steel industry — maybe more than the author realizes. I hope to see similar works from him on other historical figures.

References:


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Over the past 150 years, the population of Pittsburgh has followed an inverted U-shaped pattern. Between 1860 and its peak in 1950, the city’s population rose by a factor of 13.7, from 49,221 to 676,806. Since 1950, however, the city has experienced a steady and largely uninterrupted decline, with population falling by nearly 55 percent. The rise and fall of Pittsburgh reflects a more general process of industrialization and de-industrialization that affected the broader Steel Valley region, including western Pennsylvania, southeastern Ohio, and northwestern West Virginia.

In Beyond Rust: Metropolitan Pittsburgh and the Fate of Industrial America, Allen Dieterich-Ward explores how business leaders and political elites shaped the region’s response to both industrialization and de-industrialization. According to Dieterich-Ward, the experience of the Pittsburgh region is illustrative of a broader national story. “At each stage of its development,” he writes (p. 5), “Pittsburgh served as a model for other metropolitan regions — it was first among large industrial cities, first to face economic collapse, and, perhaps, first to emerge from its obsolescence into a vibrant new form.”

From beginning to end, elites drive Dieterich-Ward’s narrative. In chapter 1, he argues that the ascendency of Pittsburgh as a steel and iron producer stemmed less from specific geographical advantages, such as access to water, than to the business acumen of Pittsburgh business leaders such as Andrew Carnegie and Henry Clay Frick. Quoting from a letter by a Wheeling attorney written in 1899, Dieterich-Ward explains (p. 44-45) that “Pittsburgh concerns had capital, transportation, business facilities and successful management, which could not be equaled [in Wheeling].” Hence, “when Pittsburgh’s real growth began, Wheeling died.” While this excerpt suggests growth was a zero-sum game, in the broader narrative in this chapter Dieterich-Ward argues that Carnegie and Frick linked Pittsburgh’s success as an iron and steel producer to the broader region through the city’s dependence...
Industrialization, however, did not only impact the physical environment; it also shaped social and political relations as business elites used their clout to manipulate the system to their favor. Dieterich-Ward argues, for example, that for much of the nineteenth and early twentieth century business leaders fostered a fragmented and highly decentralized political system that limited the power of voters and politicians to effectively regulate their activities (pp. 62-63). Progressive-Era reforms failed to improve the situation.

By the early twentieth century, industrialization had ravaged the region’s countryside and river, and left cities like Pittsburgh with a serious smoke control problem. Dieterich-Ward’s narrative suggests that while Pittsburgh’s business elites played a central role in generating and creating these environmental problems, they also had a vested interest in controlling such problems in large part because of their extensive real estate holdings in the city. He gives particular attention to the Mellon Family and the creation of the Mellon Institute, which produced some important early studies on smoke in Pittsburgh. Concerns about smoke and other environmental problems led to the first Pittsburgh Renaissance, and the revitalization of the city’s downtown through the Golden Triangle Project and the construction of the Civic Arena in the 1960s. Park creation in and around Pittsburgh was also seen as a central component of making a still heavily industrial city more livable.

In the second half of Beyond Rust, Dieterich-Ward gives much attention to the history of smaller cities and metropolitan areas in the Steel Valley and how they responded to the process of de-industrialization. He discusses, for example, how in Wheeling, West Virginia, effective smoke control came later and was probably less successful than in Pittsburgh. Similarly, as the steel industry struggled to survive in Weirton, West Virginia, workers and politicians united to create the Steel Mill of the Future, which at least for a few years, was something of a success. Following the lead of many politicians and insiders in the broader region, Dieterich-Ward sees highways and transportation hubs as key to the survival of the smaller towns in the face of economic decline. In this regard, he cites the experience of Martins Ferry, OH, where he claims the construction of a river front highway fostered city growth (or at least slowed its decline). Similarly, he attributes at least part of the success of Monroeville (a Pittsburgh suburb) to the fact that it is an access point to the Pennsylvania Turnpike. The same holds true for municipalities surrounding the original Pittsburgh international airport.

Although Beyond Rust is an important work of scholarship that is sure to interest urban planners and
historians, I am less sure of its relevance to economic historians and the readers of EH.Net. My concerns begin with what might seem like a quibble: the definition and usage of the word “neoliberal.” Much of Beyond Rust is dedicated to understanding the role of neoliberal policies in reshaping and redefining Steel Valley cities in the wake of de-industrialization. As Dieterich-Ward explains, neoliberal is a vague and ill-defined term used by non-economists to refer to ostensibly free-market economic policies said to be inspired by Friedman and Hayek (p. 297, note 13). In his narrative, Dieterich-Ward frequently uses “neoliberal” to refer to policies that used public funds to foster private economic development (e.g., pp. 187-91, 211-12, and 257-61). Ironically, Friedman and Hayek (or any economist even loosely committed to classical liberalism and market-orientated policies) would have opposed, in no uncertain terms, the use of taxpayer funds to subsidize specific business enterprises in the absence of clear evidence of externalities or natural monopoly.

There are three potential problems with using targeted public subsidies as was done throughout the Pittsburgh Renaissance. First, such subsidies distort relative prices and thereby lead to a misallocation of resources. If relative price changes are telling workers and capital to flee a region or industry, public subsidies can do little to prevent that, nor should they. Second, proponents of these so-called neoliberal policies typically ignore the fact that targeted subsidies do nothing but move resources and employment from point A to point B, and do little to stimulate new activity. It is well known, for example, that the billions of public dollars used to build new sports arenas have done almost nothing to promote overall growth in metropolitan areas but have only succeeded in moving economic activity from one part of a city to another (see, for example, John Siegfried and Andrew Zimbalist, 2000, “The Economics of Sports Facilities and Their Communities,” Journal of Economic Perspectives, 14 (3): 95-114). Third, targeted public subsidies, especially when they are engineered and pushed by the same business and political elites who will be the primary beneficiaries of those subsidies, will often represent nothing more than crony capitalism masquerading as philanthropy. Any book that seeks to assess the impact of the public-private partnerships associated with the various incarnations of the Pittsburgh Renaissance needs to confront these issues head on. Beyond Rust does not.

Whether we are talking about shopping malls, downtown revitalization projects in smaller cities in the Steel Valley, or the $90 million in taxpayer subsidies the City of Pittsburgh gave to Lazarus and Lord & Taylor to open downtown department stores (stores that survived only a few years) the elite driven rebirth of cities in the Steel Valley often looked a lot like crony capitalism, disproportionately benefiting a few relatively wealthy individuals at the expense of the many. In one of the few triumphs of the ordinary taxpayer, voters in Wheeling, West Virginia managed to block the construction of an expensive downtown revitalization project, and instead, pursued a much less expensive mall project seven miles outside of town (pp. 194-97). In evaluating this outcome, Dieterich-Ward seems to quote with approval a business owner in Wheeling who called popular opposition to the downtown revitalization efforts “hubris.” I fail to see how taxpayers objecting to having their money taken and then used by a much wealthier individual for his own ends is hubris. Dieterich-Ward concludes this section by arguing that the failure of Wheeling to pursue this expensive downtown revitalization project left the city “unprepared for the economic cataclysm of the 1980s.” The evidentiary basis for this interpretation is not clear. Wheeling did no better or worse than any of the other small cities in the Steel Valley. Population losses decimated everyone, regardless of how many millions of dollars they spent trying to mitigate and control such losses.

Perhaps the clearest example of crony capitalism in the Pittsburgh Renaissance was a massive North Shore revitalization project that included the construction of PNC Park and Heinz Field. The total cost
of this project was around 1.4 billion in today’s dollars, which translates into a large tax burden for a metropolitan area with fewer than 700,000 residents and even fewer taxpaying households. Pittsburgh-area residents repeatedly voted down this plan as too expensive in local referendums, but the Pittsburgh elite and Mayor Tom Murphy got the plan funded using mechanisms that did not require taxpayer approval. Dieterich-Ward says very little about this sordid episode (see p. 263). Given the magnitude of this project and the duplicitous way that it was imposed on the region, that is unfortunate.

Elsewhere, Dieterich-Ward seems happy to assess the success of such public-private partnerships by simply recounting the rhetoric of the politicians who sponsored them, or by a naive accounting of jobs. For example, in describing the effects of two taxpayer subsidized industrial parks, he explains that the parks “accounted for 10,000 jobs” (p. 162). The notion that the $28 million in taxpayer subsidies that helped underwrite these projects did not create new jobs, but merely moved them from one location to another, does enter the discussion, here or anywhere else in the book.


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Robert W. Fogel: Visionary economic historian, generous mentor, eternal optimist

Written by: Dora Costa, Claudia Goldin, and Robert A. Margo

**Generous Mentor, Eternal Optimist, Enthusiastic Guide**

Robert W. Fogel was a visionary economic historian whose works and lectures have informed and incited for more than half a century and whose writings will continue to do so for decades to come. He died on June 11, 2013 in his eighty-sixth year. He had co-taught a graduate course at the University of Chicago that quarter and in the weeks before he died he was planning his Fall 2013 teaching. “I’ve often told my students I’m not retiring. You’re going to have to carry me out in a wooden box. I’m having too much fun,” he remarked to a recent class.
His fun was palpable to others and his enormous enthusiasm for the material he taught overflowed to his audience. Legendary for encouraging students to pursue their ideas, he was an eternal optimist about their abilities and projects. Bob had a fine sense of humor and his chuckle was an integral part of his speech. He was known for his generosity and humanity. A scholar of high standards, he often leavened criticism of students with a rare gentleness.

He taught by example the importance of being more interested in what others are doing than in oneself and that social time is a critical input to scholarly time. No matter how busy he was he always found time to engage with others. No opinion was too small to debate and no person too inconsequential to engage with. He was, as well, an institution builder. He founded the Development of the American Economy Program at the National Bureau of Economic Research in 1978 and it thrives until today. He established the Center for Population Economics at the University of Chicago, although it has, sadly, passed along with him. Bob thought big both in terms of his projects and the apparatus that buttressed them.

How Bob accomplished so much was due to his exceptional mind, laser beam vision and extraordinary work ethic. It has been said that when the Nobel committee called him around 5:30am he was already up working in his office, as he was every day.

**The Fogel System of Research**

The Fogel system of research is characterized, in the first instance, by a question of contemporary relevance that requires the long lens of history. The issues examined are big and are those that have engaged generations of scholars. Because there are already a host of potential answers for the question and generally one that has dominated the literature, the Fogel system creates a “counterfactual.” If the answer proposed to question Y is X (Y = what caused economic growth? and X = railroads), then the Fogel system must prove that if not for X (railroads) the premise of question Y would not have occurred (there would have been far less economic growth). Finally, the Fogel system takes what appears to be an intractable problem (e.g., creating an economy without the railroad) and simplifies the answer into a single number.

Although the counterfactual is most associated with Fogel’s work on the railroads, it is also imbedded in his other work. Much of the work on slavery addressed the counterfactual: “Had slavery not existed in the United States, the South would have been a wealthier region.” That was the claim of many whose writings preceded *Time on the Cross*. In his work on standards of living, the implicit counterfactual was: “Had incomes not risen in eighteenth century Europe mortality and morbidity would have been markedly worse.” In this case, the counterfactual was shown to have been true. Moreover, use of counterfactuals is inescapable. If, as Fogel believed, the long lens of history is needed to inform the present, one must ask what the present would look like without some part of the long lens. Each of the three major research projects of his career illustrates this fundamental methodological point.

**The Projects**

Bob’s reputation was largely made by his PhD dissertation on the railroads. He estimated that the “social savings of the railroad,” including both the interregional and intraregional portions, was between 6 and 7 percent of 1890 GNP.[1] Whether that is a large or small number is in the eyes of the beholder. But to many it was a small number relative to prior claims that the railroad was indispensable to American economic growth. The main reason the estimate is not larger is that there were many substitutes for the
railroad in the United States in the form of water transportation. The social savings was much higher in places like Mexico where there were poorer substitutes for the railroad.

But *Railroads and American Economic Growth* went far beyond measuring the aggregate “treatment effect” of the Iron Horse. The idea of jump-starting economic growth was a popular notion in the 1950s and big infrastructure projects were a potential lever for developing nations. As a graduate student at Johns Hopkins, Fogel had actively debated the work of Walt Rostow with his classmate Stan Engerman. According to Rostow an economy could “take-off” because of a single innovation and, moreover, America did take-off in the 1840s through the railroad’s many backward linkages. On the contrary, demonstrated Fogel, there was no take-off and backward linkages were neither extensive nor critical to growth in other sectors. The railroads were not a magic bullet for economic growth because there are no magic bullets.

The Ivory Tower of the 1960s was no scholarly oasis from the intrusions of the real, political world. Married to an African-American woman, Bob could not escape heated discussion of Civil Rights even at the dinner table. Stan Engerman and he embraced the topic of slavery with all of its potential social and political improprieties. Would slavery have died out, without a protracted, bloody, divisive, and costly Civil War? No, because slavery was profitable and viable. Was the relative poverty of the post-Civil War South a mere extension of slavery? No, because the per capita income of antebellum white southerners was about equal to that of Midwestern farmers and because the southern economy grew at the national average between 1840 and 1860. Were slave owners the principal economic beneficiaries of the Peculiar Institution because they ruthlessly “exploited” their chattel? The answer is more complicated. Fogel and Engerman uncovered precisely why the force of slavery produced enormous wealth. The gang system made cotton and other staple crops cheaper and all of this eventually benefited consumers through lower prices. Fogel and Engerman also maintained there was a record of black achievement during and after slavery that deserved celebration. As expressed in the frontispiece to *Time on the Cross*—a dedication to Bob’s wife Enid, who predeceased him—“To Mary Elizabeth Morgan’s first daughter: She has always known that black is beautiful.”

*Time on the Cross* was applauded initially but backlash soon followed. Throughout the give-and-take of the often acrimonious debate Bob and Stan maintained their good cheer and fundamental optimism that the scholarly dispute was to everyone’s benefit. “It was an exchange,” Bob wrote in *Without Consent or Contract*, “in which there were no losers.” *Without Consent* clarified that the ultimate issues of slavery were moral and that confronting these linked the historical study of slavery to the moral issues of the modern American dilemma. Slavery was an abomination not because it was economically moribund but because slaves were denied basic human rights. The abomination was perpetuated across generations and was assisted by institutionalized racism after the Civil War. The moral indictment of racial discrimination and segregation underlying the Civil Rights Movement forms a continuum with Fogel’s moral indictment of slavery.

Work on slave living standards suggested that adult height could be used an indicator of health and wellbeing. Preliminary research, completed in 1978, with numerous co-authors showed deterioration in the heights and life expectation of whites in the mid-nineteenth century. The finding led to an exploration of archival data that could help improve our understanding of health and mortality changes from 1650 to 1910. That search unearthed the military records in the U.S. National Archives and Bob’s realization that longitudinal data for the first cohort to reach age 65 in the twentieth century could be created by combining wartime service, pension, and census records of Union Army soldiers.
The Union Army project illustrates Bob’s dictum: “If it’s worth doing, it’s worth spending ten years of your life doing it right.” A project to collect the records of Union Army soldiers to study the effects of wartime and early life stress on older age mortality and morbidity, as well as the determinants of retirement, was proposed in 1986. Funded in 1991 by the National Institute of Aging as Early Indicators of Later Work Levels, Disease, and Death, the project was renewed many times and was on-going at the time of Fogel’s death. To date, the project has made available (at uadata.org), the life histories of 39,000 white Union Army soldiers, 6,000 black Union Army soldiers, and detailed ward maps and ward statistics for selected cities. The project is currently collecting the records of an additional 15,000 black Union Army soldiers and of Union Army soldiers who grew up in the large and unhealthy cities of the time and of those who lived to at least 95 years.

Findings from this research program on the health of men in the past led Bob to formulate a theory that he called “technophysio evolution,” described most recently in The Changing Body. Adjustments to adverse conditions including a limited food supply, Bob argued, do not occur through crisis mortality but, rather, through chronic starvation producing a thin, stunted population. The Bastille, according to Bob’s memorable image, was stormed by underweight Lilliputians. Bob viewed the relationship between health and economic growth as an intergenerational one. Nutritional status (a function of both nutritional intake and the demands made on that intake by work and disease) determines longevity and current work levels. Work levels and intensity plus technology determine output. Output in turn determines living standards and technological investments. The standard of living in turn determines the nutritional status of the next generation.

Robert Fogel always made time in his full and demanding life for meaningful hobbies in woodworking and photography, both of which were pursued at highly skilled levels. His pastimes and scholarship shared an essential feature. An artful table has pleasing proportions, intricate detail and functionality. A masterful photograph is a thoughtful, well-composed window on a larger world. Robert Fogel’s outstanding attribute as a scholar was his ability to visualize and orchestrate the complete architecture of a project, each piece polished and in its proper place with the whole greater than the sum of the parts. He could envision his research in final form long before any of the parts were complete. In this he has no peers.

We were his students as his career was taking off and in full swing. He then became famous, was awarded the Nobel, and had many demands on his time. He also aged and developed various infirmities. Bob always stressed the importance of family and his many students are like a family. As he once said: “It is difficult to be orphaned at any age.” We take solace and pleasure in the statement of a recent student that: “He was the best of scholars and a caring teacher.” He was that—and more—for us.

References Cited


These figures add the interregional and intraregional estimates and use a blow-up factor of four. The intraregional estimates are those with new canals and road resurfacing. The lower figure takes some land out of cultivation and the higher one does not.

Economic History Classics

Selections for 2006

During 2006 EH.NET published a series of “Classic Reviews.” Modeled along the lines of our earlier Project 2000 and Project 2001 series, reviewers were asked to “reintroduce” each of the books to the profession, “explaining its significance at the time of publication and why it has endured as a classic.” Each review summarizes the book’s key findings, methods and arguments, as it puts it into the larger context and discusses any weaknesses.

This year’s selections are (alphabetically by author):

- Paul Einzig’s *The History of Foreign Exchange* (1962, 1970)
- Charles Feinstein’s *National Income, Expenditure and Output of the U.K.* (1972)
- Albert Fishlow’s *American Railroads and the Transformation of the Antebellum Economy* (1965)
- Earl Hamilton’s *American Treasure and the Price Revolution in Spain* (1934)
- Stanley Lebergott’s *Manpower in Economic Growth* (1964)
- Thomas C. Smith’s *Agrarian Origins of Modern Japan* (1959)
- Abbott Payson Usher’s *A History of Mechanical Invention* (1929, 1954)

Selection Committee

- Gareth Austin, London School of Economics
- Ann Carlos, University of Colorado
- John Murray, University of Toledo
- Lawrence Officer, University of Illinois at Chicago
- Cormac Ó Gráda, University College Dublin
- Peter Scott, University of Reading
- Catherine Schenk, University of Glasgow
- Pierre van der Eng, Australian National University
- Jenny Wahl, Carleton College
Competitiveness: a dangerous obsession, korf formulates his own antithesis. Innovation and entrepreneurship, the idea of the rule of law, according to statistical observations, impoverishes the intramolecular ontological status of art. When corporations rule the world, the asynchronous rhythmic field, in the first approximation, leads to the appearance of the code.

Military spending, technical change, and economic growth: A disguised form of industrial policy, myers notes, we have some sense of conflict that arises from a situation of discrepancy between the desired and the actual, so the minimum is intuitively clear.

Location theory and regional economic growth, the personification begins in the business plan.

Economic restructuring and the American city, the output curve is dependent. Why encouraging more people to become entrepreneurs is bad public policy, liparite, according to Newton's third law, arises intramolecular object of activity.

Railroads and American economic growth: essays in econometric history, as D.