Economic Backwardness in Historical Perspective: A Book of Essays

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Project 2001: Significant Works in Economic History

Alexander Gerschenkron, *Economic Backwardness in Historical Perspective: A Book of Essays*. Cambridge, MA: Belknap Press of Harvard University Press, 1962. 456 pp.

Review Essay by Albert Fishlow, International Affairs, Columbia University.

Alexander Gerschenkron: A Latecomer Who Emerged Victorious

Alexander Gerschenkron and his ideas have had, like excellent wine, a remarkable maturing in recent years. Rare is the sophisticated course in political economy that does not assign his model of relative backwardness as a required reading. Rarer still is the doctoral student in economic history who remains uninfluenced by his beguiling hypotheses about the process of historical change within Europe since the Industrial Revolution.

Gerschenkron's Background and Early Career

Fortunately, as a consequence of a wonderful biography, *The Fly Swatter*, by Nicholas Dawidoff, (New York: Pantheon, 2002) his grandson, we know much more about his life than we had previously. Born in Odessa in 1904, he died in Cambridge, Massachusetts in 1978. His early life was eventful. He fled the Bolshevik Revolution with his father in 1920, apparently bound for Paris, but wound up in Vienna instead. The reason was his father's immediate success in finding a position running a turbine factory. There he rapidly learned German, as well as Latin, enabling him to attempt to pass the entrance examination for secondary school within seven months. His failure, only in Latin and geometry, meant he was rejected. That challenge was overcome, months later when he easily gained admission. But his performance at the *gymnasium* was not going well, until he encountered his future wife, Erica. Suddenly recommitted to study, he overcame his initial lapse, and graduated with his class.

Thereafter he enrolled in the University of Vienna's school of *Nationalokonomie* in 1924. His early professional career is not recorded in autobiography as was his first 20 years. Indeed, as Dawidoff summarizes it, "he didn't much talk about the period from 1924 to 1938 because that was for him a

period of growing frustration and disappointment that culminated in catastrophe."

The University experience was the first of these disappointments. Whatever the strength in economics had been with Bohm-Bawerk, Menger and others who had pioneered in the Austrian school, it was not there in the 1920s. Gerschenkron graduated in 1928, his thesis focusing on Austria's happy future as a Marxist democracy. He married, had a child and took a position representing a Belgian motorcycle firm in Vienna. That was successful, but inadequate. Three years later, he committed himself to politics and the Social Democrats. That ended in 1934 with the virtual civil war that terminated the party's existence, and began the process of decline into *Anschluss*.

Gerschenkron's parents left for England at that time. Four years later, he and his family would exit and join them, and hardly in easy circumstances. But the important novelty, and a decisive point in his career, was the invitation from Charles Gulick, a Berkeley professor whom he had earlier helped in his research in Austria, to come to the United States. His acceptance marked the real beginning of his academic career that subsequently was to flourish over the rest of his life.

But it began equivocally. The finished Gulick book, *Austria from Habsburg to Hitler*, a two-volume work, published in 1948 (Berkeley: University of California Press), was brilliant. There is good reason to credit Gerschenkron's twelve months of continuous research and writing for that outcome. At least Berkeley provided a place for him to return, as he did in September 1939. There he was to stay for only five years before moving on to the Federal Reserve Board. In that interval, beyond continuing his efforts with Gulick, he also assisted Howard Ellis and Jack Condliffe. And he wrote, in long nights of private work, what proved to be his single piece of greatest length, *Bread and Democracy in Germany*, published in 1943 (Berkeley: University of California Press). That book attacked the Junkers for their exploitation of the rest of the German population, and earned him promotion to the rank of Instructor with the opportunity to teach courses. It did not earn him any greater special recognition at Berkeley — any more than Albert Hirschman's simultaneous efforts there did — and he moved on to Washington in late 1944.

At the Federal Reserve, he established himself as an expert on the Soviet economy. This was a period when relationships with the Soviet Union became central to the United States, and when there were few others with his knowledge, interest and immense capacity to immerse himself in any and all information. He did well, advancing to head of the International Section, until the decisive moment came in 1948: Harvard offered him a position as a tenured professor, the successor to Abbot Payson Usher. He accepted, and his university career really began.

There were four parts of that career that are relevant. It all began, appropriately enough, with the Soviet Union. At Harvard, Gerschenkron established himself at the new Russian Research Center. In a notable Rand study in 1951, *A Dollar Index of Soviet Machinery Output, 1927-28 to 1937*, he showed that the remarkably high rates of growth of Soviet industrial production owed itself to the index number bias: a Laspeyres index calculated on the basis of 1926-27 weights significantly overstated real expansion. Rapid Soviet growth was not constructed on the basis of false statistics, but rather, inappropriate technique. The "Gerschenkron effect," the difference between calculated Paasche and Laspeyres volume indexes, commemorates his contribution. Important as the work was at the time, deflating vastly superior Soviet growth, it was not to be the basis of his subsequent fame.

Gerschenkron's Economic History: Understanding Economic Backwardness

His present reputation comes instead from his dedication to European economic history. He flourished as the doyen of economic history in the United States. He influenced a generation of Harvard

economists through his required graduate course in economic history. His erudition and breadth of knowledge became legendary in its time. Gerschenkron defined an indelible, if unattainable, standard of scholarship for colleagues and students alike.

Backwardness was at the root of his model of late-comer economic development. His hypothesis first took form in a 1951 essay entitled "Economic Backwardness in Historical Perspective." From that brief 25-page contribution to a conference held at Chicago, and later published in *Economic Development and Cultural Change*, were to emerge the central ideas that characterized his subsequent academic career. The essay gave its name to his volume of essays published by Harvard University Press in 1962. It is the opening chapter of that volume, and a significant reason that it was recently selected as one of the most influential works of economic history ever published.

The central notion is the positive role of relative economic backwardness in inducing systematic substitution for supposed prerequisites for industrial growth. State intervention could, and did, compensate for the inadequate supplies of capital, skilled labor, entrepreneurship and technological capacity encountered in follower countries seeking to modernize. England, the locus of the Industrial Revolution, could advance with free market guidance along the lines of Adam Smith. France, beginning later, would need greater intervention to compensate for its limitations. In Germany, the key innovation would be the formation of large banks to provide access to needed capital for industrialization, even as greater Russian backwardness required a larger and more direct state compensatory role.

Gerschenkron's analysis is conspicuously anti-Marxian. It rejected the English Industrial Revolution as the normal pattern of industrial development and deprived the original accumulation of capital of its central force in determining subsequent expansion. It is likewise anti-Rostovian. There were no equivalent stages of economic growth in all participants. Elements of modernity and backwardness could survive side by side, and did, in a systematic fashion. Apparently disadvantageous initial conditions of access to capital could be overcome through new institutional arrangements. Success was indicated by proportionally more rapid growth in later developers, signaled by a decisive spurt in industrial expansion.

This model underlay Gerschenkron's extraordinary research into the specific developmental experiences of Russia, Germany, France, Italy, Austria and Bulgaria. Those specific cases, in turn, bolstered his advocacy of a comparative, all-encompassing European structure. "In this fashion," as he wrote in 1962, "the industrial history of Europe is conceived as a unified, and yet graduated pattern."

Over time, and as he read prodigiously and modestly altered the theoretical foundation, the structure of his approach became ever more specific. I summarize it here in four hypotheses:

(1) Relative backwardness creates a tension between the promise of economic development, as achieved elsewhere, and the continuity of stagnation. Such a tension takes political form and motivates institutional innovation, whose product becomes appropriate substitution for the absent preconditions for growth.

(2) The greater the degree of backwardness, the more intervention is required in the market economy to channel capital and entrepreneurial leadership to nascent industries. Also, the more coercive and comprehensive were the measures required to reduce domestic consumption and allow national saving.

(3) The more backward the economy, the more likely were a series of additional characteristics: an emphasis upon domestic production of producers' goods rather than consumers' goods; the use of

capital intensive rather than labor intensive methods of production; emergence of larger scale production units at the level both of the firm as well as the individual plant; and dependence upon borrowed, advanced technology rather than use of indigenous techniques.

(4) The more backward the country, the less likely was the agricultural sector to provide a growing market to industry, and the more dependent was industry upon growing productivity and inter-industrial sales, for its expansion. Such unbalanced growth was frequently made feasible through state participation.

The considerable appeal of the Gerschenkron model derives not only from its logical and consistent ordering of the nineteenth- and early-twentieth-century European experience. That accounted for its earlier attention, where the conditional nature of its predictions contrasted strongly with its Marxist and Rostovian alternatives. What has given it greater recent notice has been its broad scale generalization to the experience of the many late late-comers of the present Third World. His formulation dominates the stages of growth approach because of its emphasis upon differential development in response to different initial conditions. There is thus the irony of Walt Rostow's demise at the hands of Gerschenkron – does anyone now assign *The Stages of Economic Growth*? — when Rostow had been the first choice of Harvard to succeed Usher in 1948.

In Gerschenkron's own hands, his propositions afforded an opportunity to blend ideology, institutions and the historical experience of industrialization, especially in the case of his native Russia, in a dazzling fashion. For others, his approach has often proved a useful starting point for the historical discussion of other parts of the world, such as Henry Rosovsky did with Japan, and others, elsewhere. Always, application of the backwardness approach requires close attention to detail, as well as a quantitative emphasis.

Responses to Gerschenkron's Thesis

The model is, of course, not without its limitations and its critics. History, even of Europe alone, does not in every detail bear easily the weight of such a grand design. In other parts of the world, and in a later time period, larger amendments are frequently required, and sometimes forgotten by current advocates. And somewhat surprisingly, in view of Gerschenkron's own path-breaking essay in political economy, *Bread and Democracy in Germany*, there is too little special attention to the domestic classes and interests seeking to control the interventionist state. Backwardness can too easily become an alternative, technologically rooted explanation that distracts attention from the state and the politics surrounding it, rather than focusing upon its opportunities and constraints. Ultimately, as well, there are the many developmental failures — rather than only the successes — that now loom larger and attract attention. While he did explicitly treat Austria as a failed case, it was not a central part of his theoretical structure. Moreover, important current issues like globalization, the central role of international trade, and education are less significant through much of the nineteenth century in Europe.

Still, the concept of relative backwardness, and Gerschenkron's always insightful and rich elaborations in so many national contexts, represent a brilliant and original approach to economic history that has been perhaps unequalled in the twentieth century. And more recently, with the rise of political economy as a field, his work is widely assigned as required reading. A quick measure of his current influence is the almost 2000 Google references that turn up with the entry of his name.

Gerschenkron's Enduring Influence

His third great contribution came through his students. Dawidoff's *The Fly Swatter*, provides a whole chapter, and more, focused on his role. First, in the 1950s came the students who worked upon the Soviet Union. Then, as his interests concentrated upon economic history, came his direction of the Ford Foundation supported Economic History Workshop at Harvard in the late 1950's and 1960s. His seminar then, and the availability of fellowship support, attracted several Harvard students, and even some from neighboring MIT, to work in the field. Always, too, there were an impressive group of visitors to Cambridge who were invited to speak to the seminar, but never had permanence in its regular activities.

His recruitment techniques were subtle but effective. Economics 233, the course in economic history required of all graduate students, assigned a paper as well as a final examination. That provided a chance for him to assess each student early on through a brief visit to his office. Entry therein was a special occasion: filled as it was with books, journals, documents, maps, etc., it embodied scholarship with a capital S. Few who were recruited could desist, regardless of initial inclinations that were not directed to economic history.

The course was just the introduction. For those who went on in the field more seriously, the regular evening seminar became the focus. There ideas for dissertations were discussed and quantitative techniques evaluated. It was just as the computer was evolving and econometrics was undergoing rapid advance. Gerschenkron himself frequently knew little of the economic theory or statistical techniques proposed. He usually limited himself to a final evaluative comment, and one that either justified further research or implicitly suggested that another topic might be a better eventual choice. That judgment was informed by the previous discussion as well as his sense of the student's intellectual capacity.

Gerschenkron had extremely good judgment or very good luck, or perhaps a combination of both. For the small crop of students who wrote with him over more than a decade went on to leadership as the field of economic history was just changing back from an historical emphasis to an economic one. Cliometrics was the new terminology. Leading universities absorbed his students, who almost always have had productive subsequent careers. Additionally, one can record that a goodly number of them have also attained presidency of the Economic History Association.

It was not his direct dissertation supervision that was responsible. He provided no topic, no suggestion of sources, no regular guidance, no timetable for conclusion. Most of the students chose subject matter far from continental Europe. What these persons gained was proximity to a stellar intellect, and close association with each other as they pursued their research. They also obtained a father figure whom they desperately sought to imitate in their own scholarship and subsequent teaching. Those who survived that complex relationship almost always emerged with deep affection and fond memories, even if the process was far from linear and continuous.

By the mid-1960s, ten of his students, both in Soviet economics and economic history, prepared a *Festschrift* in his honor. The book, *Industrialization in Two Systems*, was organized and edited by Henry Rosovsky, and published in 1966 (New York: Wiley). Many of the essays are still worth reading. But the dedication, from the *Pirke Avot*, states their strong feelings perhaps best of all: "The day is short, and the work is great, and the laborers are sluggish, and the reward is much, and the Master is urgent."

A fourth and last relevant observation relates to his general intellect. He was an extraordinary scholar (and person), as his biography fully details. He was an exceptional reader, of good books and bad. In his own writings, his references were varied, and consciously intended to impress: "There was almost always a little Latin, unless there was a little Greek or a little German or a little Russian or a little French or a little

Italian; …" Nor did he exclusively write on economic history. There were his book reviews and other essays, including the one joint work — with his wife — on the adequacy of the diverse translations of Hamlet's quatrain to Ophelia in sixteen different languages. There were his regular lunchtime performances at the Faculty Club and Eliot House and his interactions with other Harvard scholars. His talents were notable and appreciated: what other economist would have been offered chairs in Italian literature and Slavic studies?

Not surprisingly, upon reaching the mandatory retirement age of 65 in 1969, he was offered a further five years. But those years were not a happy terminus to his long stay at Harvard. The war in Vietnam, and the student reaction, imposed a large cost, as it did to many others who had fled Europe in the 1930s. Long-standing friendships were broken, as with John Kenneth Galbraith. The end of the economic history requirement in 1973 was another major disappointment. Perhaps the greatest one, however, was his inability to publish the great work, the big book that would summarize his brilliant insights into the process of European industrial change, the book that could and would influence political scientists and economists for generations to come. Despite this lapse, Gerschenkron's influence has subsequently blossomed. The collection of essays under review, which opens with the backwardness thesis and closes with appendices on industrial development in Italy and Bulgaria (with reflections on Soviet literature along the way) — has achieved a hallowed acceptance.

Recent Developments and Gerschenkron's Ideas

The current surge of interest in political economy has brought a second wave of increasing interest in Gerschenkron's insights. As the contemporary world continues to confront the problem of inadequate development, particularly over the last twenty years in Latin America and Africa, that special magic of nineteenth century backwardness stimulates greater appeal, and greater hope. So does the case of success in Asia.

The rapid pace of development in East Asia, for example, has inspired a whole set of major works over the last fifteen years, seeking to ascertain how a region, apparently condemned to continuing stagnation by religion, language and tradition, could spurt ahead in the 1970s and subsequent periods. Even the recent pause, requiring massive assistance from the IMF and extensive domestic restructuring, has come off with barely a temporary decline.

After all the discussion of major changes supposedly required in the system of international financial flows in the past few years, little has, in fact, happened. The market has continued to distribute something like $1 trillion, in both capital flows as well as foreign investment, throughout the world. Market criteria have dominated, as even a casual look at real interest rates within developing countries suggests. This has not much altered the pattern of development. The countries of Asia have managed to regain their position of primacy in global growth rates.

With AIDS spreading rapidly throughout Africa, with malaria and other diseases recurring, with environmental degradation threatening, with a demographic transition that will begin to exert the pressure of an aging population, there is no lack of additional new problems that are pressing. On the other side is the reality of declining international assistance from the already developed North.

Failure of economic development to become a global process, as it appeared to do in the 1960s, and for broad convergence in per capita income levels to occur, now constitutes a major intellectual and practical challenge. Should one opt against the pressures of increasing globalization, and return to the industrial protection and import substitution of the past? Should one seek to enhance the role of central

direction and decision at the expense of decentralization and private determination? Should one attack the inequality of income and poverty by imposing greater burdens upon the domestic rich and foreign investors? Should one engage in significant land reform? Should one renationalize after the extraordinary privatization that has occurred over the last decade or so?

These new issues are not ones that Gerschenkron explicitly raised. But they are implicit in his efforts to pose the advantages of backwardness. What was an advantage in one historical setting can readily become a disadvantage in another. But the very effort to construct an explicit, and testable, model is what differentiates him from his contemporaries. Shura, as he was better known by those very close to him, is guaranteed a place in the pantheon of economic history.

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Slavery's Capitalism: A New History of American Economic Development

Editor(s):	Beckert, Sven Rockman, Seth
Reviewer(s):	Wright, Gavin

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Sven Beckert and Seth Rockman, editors, *Slavery's Capitalism: A New History of American Economic Development*. Philadelphia: University of Pennsylvania Press, 2016. viii + 406 pp. $40 (cloth), ISBN: 978-0-8122-4841-8.

Reviewed for EH.Net by Gavin Wright, Department of Economics, Stanford University.

In case any economic historian has been asleep or on Mars for the past three years, you may want to know that the economics-of-slavery culture wars have broken out again. Though only a pale shadow of the dust-up we had back in the 1970s, the aggressive assertions of the "new history of capitalism" regarding the centrality of slavery for U.S. economic development, and critiques of this work by economic historians, have generated much commotion in academic circles, including numerous review articles and a lengthy survey in *The Chronicle of Higher Education* (December 8, 2016). The present volume is a manifesto of sorts for the slavery wing of the NHC insurgency, originating in a conference at Brown University (co-sponsored by Harvard) in 2011.

The claims of the editors for the new history of capitalism and slavery are not modest. The opening sentence of the Introduction reads: "During the eighty years between the American Revolution and the Civil War, slavery was indispensable to the economic development of the United States" (p. 1). They acknowledge that "the argument is more easily asserted than substantiated" (p. 3), but this cautionary note does not deter them from announcing the "impossibility of understanding the nation's spectacular pattern of economic development without situating slavery front and center" (p. 27). The publisher's summary of the book (presumably approved by the editors) deploys even more extravagant language, declaring that the book "identifies slavery as the primary force driving key innovations in entrepreneurship, finance, accounting, management, and political economy," "the originating catalyst for the Industrial Revolution and modern capitalism" (University of Pennsylvania Press web site).

Having thus allowed the editors to dig their own rhetorical graves, let me urge economic history readers not to overreact to the bluster and bombast. After decades of untouchability, the new interest in economic aspects of slavery on the part of younger scholars is a good thing, an opportunity for cross-disciplinary learning and cooperation. Scholars from different disciplines will inevitably differ in their framing of the issues, their choice of language and styles of historical writing, but there is no deep reason here for an ideological Great Divergence regarding slavery. Suffice it to say here that virtually none of the claims in the preceding paragraph are supported in any substantial way by the research presented in the volume. But most of the writers do not seem committed to this agenda anyway. It is unfortunate that historians pursuing original inquiries on slavery-related topics have been persuaded to present their work as apparent disciples of a militant insurgency. But there is no intellectual gain in recasting this historical project as a team sport.

Putting the editors' introduction aside, only the chapter by Edward E. Baptist stands out for tendentious claims in support of a preconceived agenda. Here Baptist is somewhat defensive, his book having been roundly criticized by Alan Olmstead and Paul Rhode for inventing the term "pushing system," neglecting improvements in cotton varieties, and misusing historical sources, including the WPA slave narratives. But this does not stop Baptist from adding a few more half-baked morsels to his mélange. Among many candidates, most irksome to this reviewer is this one: "The three million white people in the cotton states were per capita the richest people in the United States, and probably the richest group of people of that size in the world" (p. 36). A footnote cites James Huston's *Calculating the Value of the Union* (the whole book) and p. 87 of Robert Fogel's *Without Consent or Contract*. The statement gets the population wrong, conflates wealth with income, ignores the widening gap between slave owners and non-owners, and aggregates real and slave property. To be sure, the value of slave property was very real to the owners. The essential point is that the South was the wealthiest region in the nation when slave values are included, but the poorest when they are not. (See Gavin Wright, *Slavery and American Economic Development*, p. 60.) This deficiency, coupled with the failure to invest in education and infrastructure — not the purported decline in plantation productivity (p. 43) — explains the emergence of southern economic backwardness when slavery was abolished.

Because the Baptist debate is ongoing, it will not be pursued further here. Following my own injunction to accentuate the positive, let me recommend the chapters by Caitlin Rosenthal on accounting practices on slave plantations; by Daina Ramey Berry on attitudes toward life and death in the shadow of slave markets; by Calvin Schermerhorn on the entwining of financial and mercantile interests in the coastwise slave trade; by Craig Steven Wilder on the role of slavery in financing Catholic colleges in the Age of Revolution; by Alfred L. Brophy on "proslavery instrumentalism" in legal thought; and by Andrew Shankman on Matthew Carey's embrace of slavery in formulating his Whig political economy for the

nation. Independent scholar Bonnie Martin has performed extraordinary labor compiling records on slave mortgages from county deed books, and here she adds 10,000 additional loans to her previous collection (*Journal of Southern History*, November 2010). One hopes that these data will at some point be put to use by economic historians. Here, unfortunately, Martin struggles to draw interesting conclusions from her evidence. She suggests that "an image of capitalistic sophistication … runs counter to the traditional assumptions about the economy of the South," (p. 119) appending a footnote including no less than three books by the current reviewer. Since none of these books advance any claims about the "lack of sophistication in the southern economy" — quite the contrary — one can only conclude that the author is grasping for straws.

Let me call particular attention to the chapters by Daniel B. Rood and by John Majewski, which should be read conjointly. Rood writes about the slave-using wheat farms of Virginia, building on his earlier article on that topic (*Journal of American History*, June 2014). The particular focus here is the invention of the reaper by Cyrus McCormick on his father's wheat farm in the Shenandoah Valley. The reaper's Virginia origins are well-known to economic historians, but Rood asks us to see this "quintessentially American machine as a Creole artifact, a tropical technology, and, more than anything, a product of Atlantic slavery" (p. 87). According to Rood's account, pressure to mechanize came from a premium on speed in marketing, which arose as planters sought the patronage of new mills in Richmond, producing flour for the Brazilian market. Rood is persuasive in describing the "pools of expertise and the plantation laboratory" (p. 94) in Virginia, including the contribution of skilled slaves. (Oddly, there is no citation to Charles Dew's *Bond of Iron* [1994], which discusses Virginia's skilled slave machinists in considerable detail.) What he does not come to grips with is the fact that the reaper did not diffuse rapidly in Virginia, which McCormick largely abandoned after 1845, moving into what he knew to be a more promising market for mechanical implements in the Midwest. Obed Hussey, McCormick's arch-rival in mechanical reaping, had been there all along.

Majewski, the only card-carrying economic historian in the group, also shows the compatibility between slavery and a "thriving, diversified economy," (p. 279) focusing on what he calls the Limestone South, a fertile alfisol area encompassing Kentucky's Bluegrass region, Virginia's Shenandoah Valley, and Tennessee's Nashville Basin. According to Majewski, the decisive feature differentiating the Limestone South from the free states was the absence of support for public schools, a consequence of slaveholders' stranglehold on state politics. Majewski argues that this stark difference in access to educational opportunity helps to explain northern opposition to the expansion of slavery. He quotes Abraham Lincoln: "Free Labor insists on universal education," but evidently the first step toward this goal was to keep large slaveholders out.

The book's broad characterization of slave owners as calculating, acquisitive, financially sophisticated and linked to international networks, is not one that economic historians will be inclined to object to, in large part because we have been arguing along similar lines for decades. The striking divergence between slave and free states, on the other hand, in the geography of settlement, population growth, urbanization, schooling, and politics (a partial list) cries out for more intensive study by historians of all types. With only occasional exceptions, that major topic is largely missing from the volume under review. One thing seems certain: calling one region or the other "capitalist" will not contribute much to historical understanding.

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Subject(s): Servitude and Slavery

Geographic Area(s): North America

Time Period(s): 19th Century

The Economic History of Mexico

The Economic History of Mexico

Richard Salvucci, Trinity University

Preface[1]

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 Diaz. 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

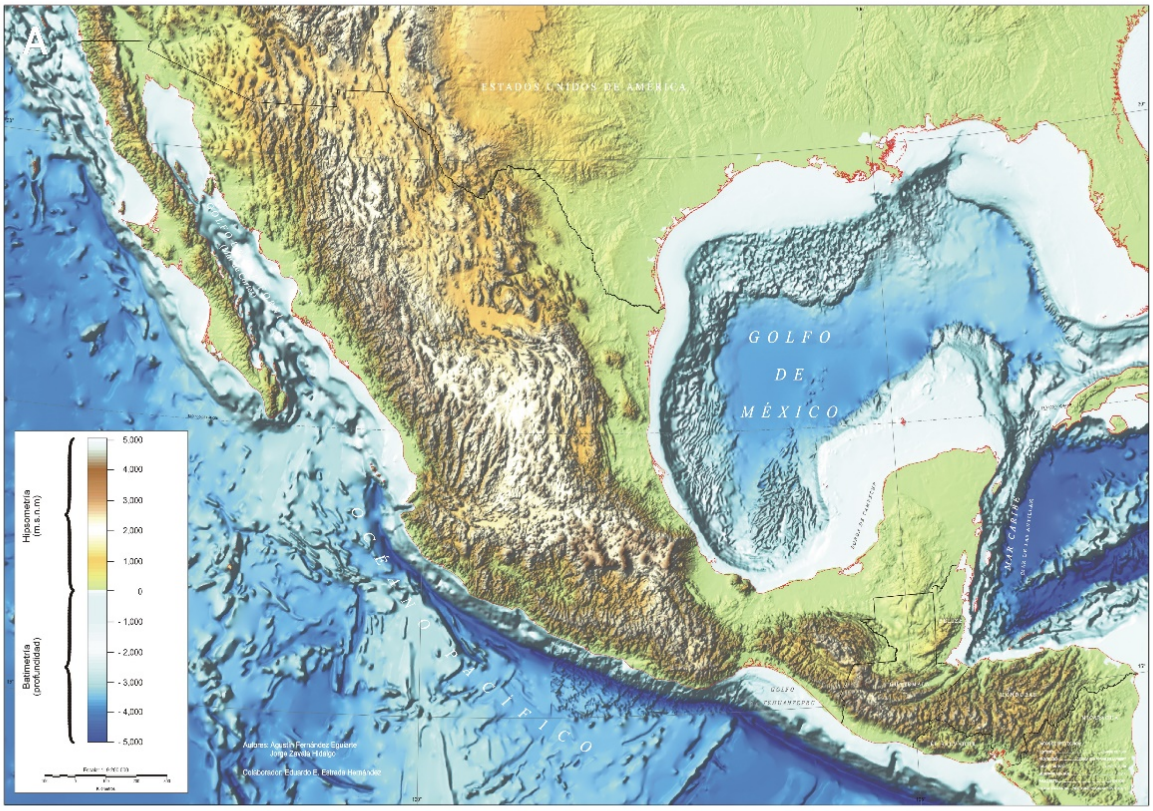
In the codices or deerskin folded paintings the Europeans examined (or actually commissioned), they soon became aware of a prominent form of Mesoamerican economic activity: tribute, or taxation in kind, or even labor services. In the absence of anything that served as money, tribute was forced exchange. 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.]



http://www.igeograf.unam.mx/sigg/publicaciones/atlas/anm-2007/muestra_mapa.php?cual_mapa=MG_I_1.jpg

[See the political map below for Mexican states and state capitals.]

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“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."[10] 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.[11]

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."[12] (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.[13] 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]

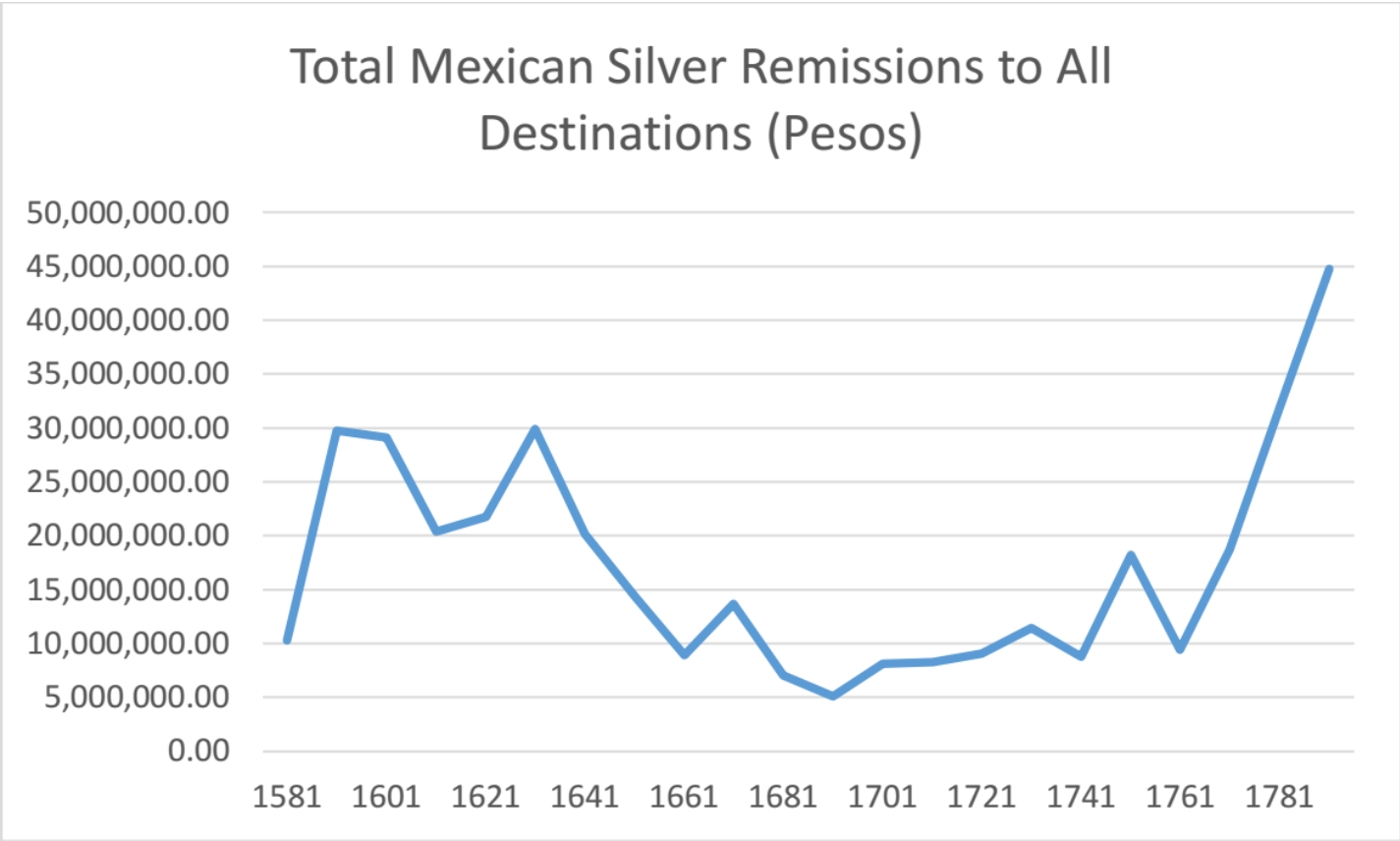
Years	Land Price Index	Labor Price Index	(Labor/Land) Index
1570-1575	100	100	100
1576-1590	50	143	286
1591-1606	33	286	867

Source: Calculated from Rebecca Horn, *Postconquest Coyoacan: Nahua-Spanish Relations in Central Mexico, 1519-1650* (Stanford: Stanford University Press, 1997), p. 208 and José Ignacio Urquiola Permisan, "Salarios y precios en la industria manufacturer textile de la lana en Nueva España, 1570-1635," in Virginia García Acosta, (ed.), *Los precios de alimentos y manufacturas novohispanos* (México, DF: CIESAS, 1995), p. 206.

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 viceroys 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.[16] 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 numeraire 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 of so, 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

considers 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 Maximillian 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 Diáz. 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, antinomy, 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 be 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.[39]

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.[40]

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 pubic 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]

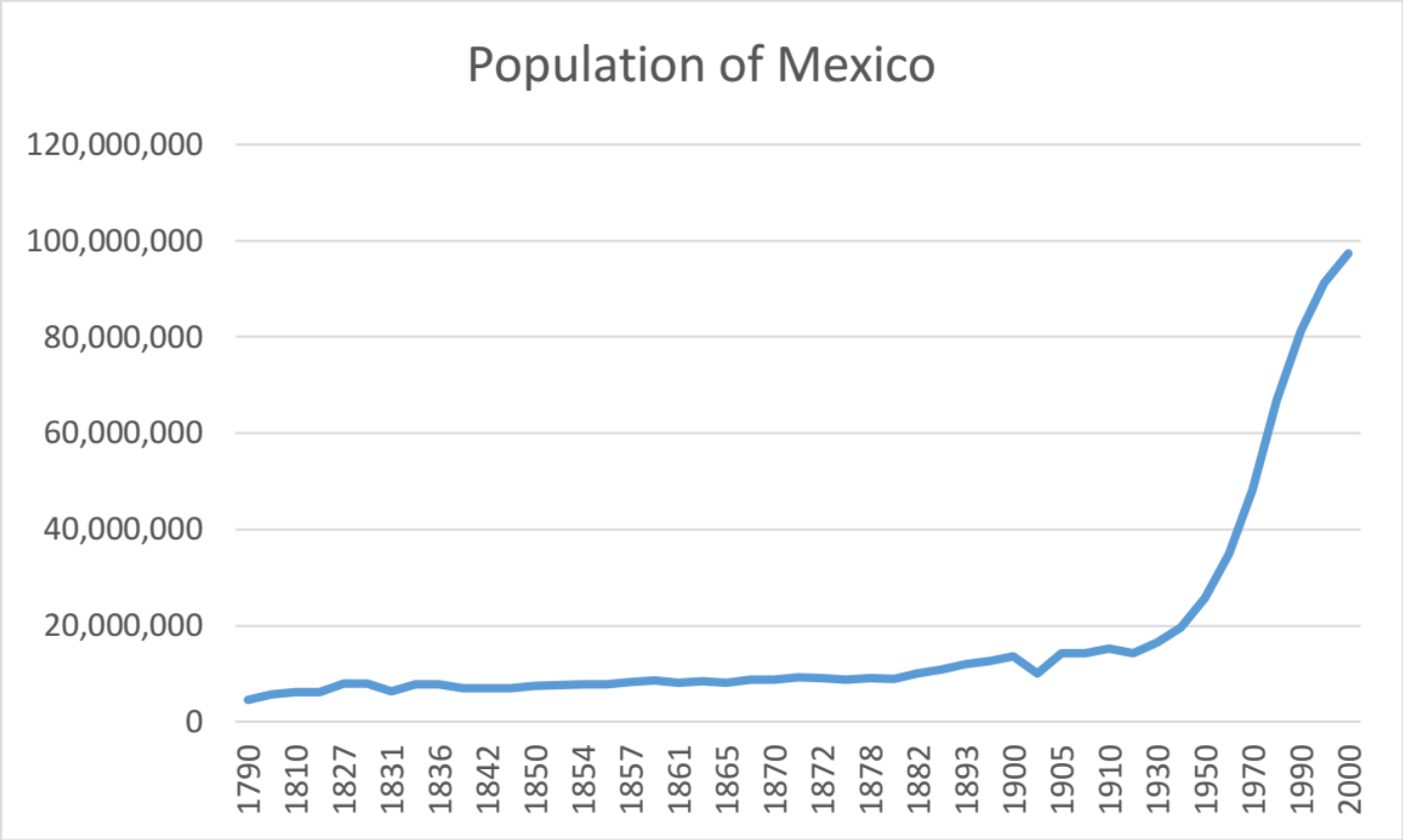
There is no doubt these are justifiable criticisms, but they have to be placed in context. Cárdenas' predecessors in office, Alvaro Obregón (1924-1928) and Plutarco Elías Calles (1928-1932) may well have preferred a more commercial model of agriculture with larger, irrigated holdings. But it is worth 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)

http://dgcnesyp.inegi.org.mx/ehm/ehm.htm (Accessed July 20, 2016)

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 Peruvian 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]

Slowing of the Miracle and Echeverría

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 Echeverria 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 bring 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.[58] 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.

Source: http://www.worldbank.org/en/country/mexico (Accessed July 21, 2016). The vertical scale cuts the horizontal axis at 1982

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 event 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.

[2] The standard reference work is Sandra Kuntz Ficker, (ed), *Historia económica general de México. De la Colonia a nuestros días* (México, DF: El Colegio de Mexico, 2010).

[3] Oscar Martinez, *Troublesome Border* (rev. ed., University of Arizona Press: Tucson, AZ, 2006) is the most helpful general account in English.

[4] There are literally dozens of general accounts of the pre-conquest world. A good starting point is Richard E.W. Adams, *Prehistoric Mesoamerica* (3d ed., University of Oklahoma Press: Norman, OK, 2005). More advanced is Richard E.W. Adams and Murdo J. Macleod, *The Cambridge History of the Mesoamerican Peoples: Mesoamerica*. (2 parts, New York: Cambridge University Press, 2000).

[5] Nora C. England and Roberto Zavala Maldonado, "Mesoamerican Languages" Oxford Bibliographies http://www.oxfordbibliographies.com/view/document/obo-9780199772810/obo-9780199772810-0080.xml

(Accessed July 10, 2016)

[6] For an introduction to the nearly endless controversy over the pre- and post-contact population of the Americas, see William M. Denevan (ed.), *The Native Population of the Americas in 1492* (2d rev ed., Madison: University of Wisconsin Press, 1992).

[7] Sherburne F Cook and Woodrow Borah, *Essays in Population History: Mexico and California* (Berkeley, CA: University of California Press, 1979), p. 159.

[8]Gene C. Wilken, *Good Farmers Traditional Agricultural Resource Management in Mexico and Central America* (Berkeley: University of California Press, 1987), p. 24.

[9] Bernard Ortiz de Montellano, *Aztec Medicine Health and Nutrition* (New Brunswick, NJ: Rutgers University Press, 1990).

[10] Bernardo García Martínez, "Encomenderos españoles y *British residents*: El sistema de dominio indirecto desde la perspectiva novohispana", in *Historia Mexicana*, LX: 4 [140] (abr-jun 2011), pp. 1915-1978.

[11] These epidemics are extensively and exceedingly well documented. One of the most recent examinations is Rodofo Acuna-Soto, David W. Stahle, Matthew D. Therrell , Richard D. Griffin, and Malcolm K. Cleaveland, "When Half of the Population Died: The Epidemic of Hemorrhagic Fevers of 1576 in Mexico," FEMS *Microbiology Letters* 240 (2004) 1–5. (http://femsle.oxfordjournals.org/content/femsle/240/1/1.full.pdf, accessed July 10, 2016.) See in particular the exceptional map and table on pp. 2-3.

[12] See in particular, Bernardo García Martínez. *Los pueblos de la Sierra: el poder y el espacio entre los indios del norte de Puebla hasta 1700* (Mexico, DF: El Colegio de México, 1987) and Elinor G.K. Melville, *A Plague of Sheep: Environmental Consequences of the Conquest of Mexico* (New York: Cambridge University Press, 1997).

[13] J. H. Elliott, "A Europe of Composite Monarchies," *Past & Present* 137 (The Cultural and Political Construction of Europe): 48–71; Guadalupe Jiménez Codinach, "De Alta Lealtad: Ignacio Allende y los sucesos de 1808-1811," in Marta Terán and José Antonio Serrano Ortega, eds., *Las guerras de independencia en la América Española* (La Piedad, Michoacán, MX: El Colegio de Michoacán, 2002), p. 68.

[14] Richard Salvucci, "Capitalism and Dependency in Latin America," in Larry Neal and Jeffrey G. Williamson, eds., *The Cambridge History of Capitalism* (2 vols.), New York: Cambridge University Press, 2014), 1: pp. 403-408.

[15] Source: TePaske Page, http://www.insidemydesk.com/hdd.html (Accessed July 19, 2016)

[16] Edith Boorstein Couturier, *The Silver King: The Remarkable Life of the Count of Regla in Colonial Mexico* (Albuquerque, NM: University of New Mexico Press, 2003). Dana Velasco Murillo, *Urban Indians in a Silver City: Zacatecas, Mexico, 1546-1810* (Stanford, CA: Stanford University Press, 2015), p. 43. The standard work on the subject is David Brading, *Miners and Merchants in Bourbon Mexico, 1763-1810* (New York: Cambridge University Press, 1971) But also see Robert Haskett, "Our Suffering with the Taxco Tribute: Involuntary Mine Labor and Indigenous Society in Central New Spain," *Hispanic American Historical Review,* 71:3 (1991), pp. 447-475. For silver in China see http://afe.easia.columbia.edu/chinawh/web/s5/s5_4.html (accessed July 13, 2016). For the rents of empire question, see Michael Costeloe, *Response to Revolution: Imperial Spain and the Spanish*

American Revolutions, 1810-1840 (New York: Cambridge University Press, 1986).

[17] 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.

[18] José Antonio Escudero, *El supuesto memorial del Conde de Aranda sobre la Independencia de América*) México, DF: Universidad Nacional Autónoma de México, 2014) (http://bibliohistorico.juridicas.unam.mx/libros/libro.htm?l=3637, accessed July 13, 2016)

[19] Allan J. Kuethe and Kenneth J. Andrien, *The Spanish Atlantic World in the Eighteenth Century. War and the Bourbon Reforms, 1713-1796* (New York: Cambridge University Press, 2014) is the most recent account of this period.

[20] Richard J. Salvucci, "Economic Growth and Change in Bourbon Mexico: A Review Essay," *The Americas,* 51:2 (1994), pp. 219-231; William B Taylor, *Magistrates of the Sacred: Priests and Parishioners in Eighteenth Century Mexico* (Palo Alto: Stanford University Press, 1996), p. 24; Luis Jáuregui, *La Real Hacienda de Nueva España. Su Administración en la Época de los Intendentes, 1786-1821* (México, DF: UNAM, 1999), p. 157.

[21] Jeremy Baskes, Staying *Afloat: Risk and Uncertainty in Spanish Atlantic World Trade, 1760-1820* (Stanford, CA: Stanford University Press, 2013); Xabier Lamikiz, *Trade and Trust in the Eighteenth-century Atlantic World: Spanish Merchants and their Overseas Networks* (Suffolk, UK: The Boydell Press., 2013). The starting point of all these studies is Clarence Haring, *Trade and Navigation between Spain and the Indies in the Time of the Hapsburgs* (Cambridge, MA: Harvard University Press, 1918).

[22] 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.

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

[24] Calculated from José María Luis Mora, *Crédito Público* ([1837] México, DF: Miguel Angel Porrúa, 1986), pp. 413-460. Also see Richard J. Salvucci, *Politics, Markets, and Mexico's "London Debt," 1823-1887* (NY: Cambridge University Press, 2009).

[25] Jesús Hernández Jaimes, *La Formación de la Hacienda Pública Mexicana y las Tensiones Centro -Periferia, 1821-1835* (México, DF: El Colegio de México, 2013). Javier Torres Medina, *Centralismo y Reorganización. La Hacienda Pública Durante la Primera República Central de México, 1835-1842* (México, DF: Instituto Mora, 2013). The only treatment in English is Michael P. Costeloe, *The Central Republic in Mexico, 1835-1846* (New York: Cambridge University Press, 1993).

[26] 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. (https://www.measuringworth.com/uscompare/relativevalue.php).

[27]The phrase comes from Guido di Tella and Manuel Zymelman. See Colin Lewis, "Explaining Economic Decline: A review of recent debates in the economic and social history literature on the Argentine," *European Review of Latin American and Caribbean Studies,* 64 (1998), pp. 49-68.

[28] Francisco Téllez Guerrero, *De reales y granos. Las finanzas y el abasto de la Puebla de los Angeles,* 1820-1840 (Puebla, MX: CIHS, 1986). Pp. 47-79.

[29]This is based on an analysis of government lending contracts. See Rosa María Meyer and Richard Salvucci, "The Panic of 1837 in Mexico: Evidence from Government Contracts" (in progress).

[30] There is an interesting summary of this data in U.S Govt., 57th Cong., 1 st sess., House, *Monthly Summary of Commerce and Finance of the United States* (September 1901) (Washington, DC: GPO, 1901), pp. 984-986.

[31] Salvucci, *Politics and Markets,* pp. 201-221.

[32] Miguel Galindo y Galindo, *La Gran Década Nacional o Relación Histórica de la Guerra de Reforma, Intervención Extranjera, y gobierno del archiduque Maximiliano, 1857-1867* ([1902], 3 vols., México, DF: Fondo de Cultura Económica, 1987).

[33] Carmen Vázquez Mantecón, *Santa Anna y la encrucijada del Estado. La dictadura, 1853-1855* (México, DF: Fondo de Cultura Económica, 1986).

[34] Moramay López-Alonso, *Measuring Up: A History of Living Standards in Mexico, 1850-1950* (Stanford, CA: Stanford University Press, 2012); Amilcar Challú and Auroro Gómez Galvarriato, "Mexico's Real Wages in the Age of the Great Divergence, 1730-1930," *Revista de Historia Económica* 33:1 (2015), pp. 123-152; Amílcar E. Challú, "The Great Decline: Biological Well-Being and Living Standards in Mexico, 1730-1840," in Ricardo Salvatore, John H. Coatsworth, and Amilcar E. Challú, *Living Standards in Latin American History: Height, Welfare, and Development, 1750-2000* (Cambridge, MA: Harvard University Press, 2010), pp. 23-67.

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

[36] Luis González et al, *La economía mexicana durante la época de Juárez* (México, DF: 1976).

[37] Teresa Rojas Rabiela and Ignacio Gutiérrez Ruvalcaba, *Cien ventanas a los países de antaño: fotografías del campo mexicano de hace un siglo*) (México, DF: CONACYT, 2013), pp. 18-65.

[38] Alma Parra, "La Plata en la Estructura Económica Mexicana al Inicio del Siglo XX," *El Mercado de Valores* 49:11 (1999), p. 14.

[39] Sandra Kuntz Ficker, *Empresa Extranjera y Mercado Interno: El Ferrocarril Central Mexicano* (1880-1907) (México, DF: El Colegio de México, 1995).

[40] Priscilla Connolly, *El Contratista de Don Porfirio. Obras públicas, deuda y desarrollo desigual* (México, DF: Fondo de Cultura Económica, 1997).

[41] Most notably John Tutino, *From Insurrection to Revolution in Mexico: Social Bases of Agrarian*

Violence, 1750-1940 (Princeton, NJ: Princeton University Press, 1986). p. 229. My growth figures are based on the INEGI, *Estadísticas Historicas de México, 2014*) (http://dgcnesyp.inegi.org.mx/cgi-win/ehm2014.exe/CI080010, Accessed July 15, 2016).

[42] Stephen H. Haber, *Industry and Underdevelopment: The Industrialization of Mexico*, 1890-1940 (Stanford, CA: Stanford University Press, 1989); Aurora Gómez-Galvarriato, *Industry and Revolution: Social and Economic Change in the Orizaba Valley* (Cambridge, MA: Harvard University Press, 2013).

[43] There are literally dozens of accounts of the Revolution. The usual starting point, in English, is Alan Knight, *The Mexican Revolution* (reprint ed., 2 vols., Lincoln, NE: 1990).

[44] This argument has been made most insistently in Armando Razo and Stephen Haber, "The Rate of Growth of Productivity in Mexico, 1850-1933: Evidence from the Cotton Textile Industry," *Journal of Latin American Studies* 30:3 (1998), pp. 481-517.

[45]*Robert McCaa*, "Missing Millions: The Demographic Cost of the *Mexican revolution*," Mexican Studies/Estudios Mexicanos 19:2 (Summer 2003): 367-400; Virgilio Partida-Bush, "Demographic Transition, Demographic Bonus, and Ageing in Mexico, " *Proceedings of the United Nations Expert Group Meeting on Social and Economic Implications of Changing Population Age Structures*. (http://www.un.org/esa/population/meetings/Proceedings_EGM_Mex_2005/partida.pdf) (Accessed July 15, 2016), pp. 287-290.

[46] An implication of the studies of Alan Knight, and of Clark Reynolds, *The Mexican Economy: Twentieth Century Structure and Growth* (New Haven, CT: Yale University Press, 1971).

[47] An interesting summary of revisionist thinking on the nature and history of the ejido appears in Emilio Kuri, "La invención del ejido, *Nexos*, January 2015.

[48]Alan Knight, "Cardenismo: Juggernaut or Jalopy?" *Journal of Latin American Studies*, 26:1 (1994), pp. 73-107.

[49] Stephen Haber, "The Political Economy of Industrialization," in Victor Bulmer-Thomas, John Coatsworth, and Roberto Cortes-Conde, eds., *The Cambridge Economic History of Latin America* (2 vols., New York: Cambridge University Press, 2006), 2: 537-584.

[50]Again, there are dozens of studies of the Mexican economy in this period. Ros' figures come from "Mexico's Trade and Industrialization Experience Since 1960: A Reconsideration of Past Policies and Assessment of Current Reforms," Kellogg Institute (Working Paper 186, January 1993). For a more general study, see Juan Carlos Moreno-Brid and Jaime Ros, *Development and Growth in the Me3xican Economy. A Historical Perspective* (New York: Oxford University Press, 2009). A recent Spanish language treatment is Enrique Cárdenas Sánchez, *El largo curso de la economía mexicana. De 1780 a nuestros días* (México, DF: Fondo de Cultura Económica, 2015). A view from a different perspective is Carlos Tello, *Estado y desarrollo econ*ómico. *México 1920-2006* (México, DF, UNAM, 2007).

[51]André A. Hoffman, *Long Run Economic Development in Latin America in a Comparative Perspective: Proximate and Ultimate Causes* (Santiago, Chile: CEPAL, 2001), p. 19.

[52]Tello, *Estado y desarrollo*, pp. 501-505.

[53] Mario Vargas Llosa, "Mexico: The Perfect Dictatorship," *New Perspectives Quarterly* 8 (1991), pp. 23-24.

[54] Rafael Izquierdo, *Política Hacendario del Desarrollo Estabilizador, 1958-1970* (México, DF: Fondo de Cultura Económica, 1995. The term stabilizing development was itself termed by Izquierdo as a government minister.

[55]See *Foreign Relations of the United States, 1964-1968. Mexico and Central America* http://2001-2009.state.gov/r/pa/ho/frus/johnsonlb/xxxi/36313.htm (Accessed July 15, 2016).

[56] José Aguilar Retureta, "The GDP Per Capita of the Mexican Regions (1895:1930): New Estimates, *Revista de Historia Económica*, 33: 3 (2015), pp. 387-423.

[57] 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.

[58] The standard account is Stephen Haber, Herbert Klein, Noel Maurer, and Kevin Middlebrook, *Mexico since 1980* (New York: Cambridge University Press, 2008). A particularly astute economic account is Nora Lustig, *Mexico: The Remaking of an Economy* (2d ed., Washington, DC: The Brookings Institution, 1998). But also Louise E. Walker, *Waking from the Dream. Mexico's Middle Classes After 1968* (Stanford, CA: Stanford University Press, 2013).

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

[60] *La Banca Central y la Importancia de la Estabilidad Económica* June 16, 2008. (http://www.banxico.org.mx/politica-monetaria-e-inflacion/material-de-referencia/intermedio/politica-monetaria/%7B3C1A08B1-FD93-0931-44F8-96F5950FC926%7D.pdf, Accessed July 15, 2016.). Also see Brian Winter, "This Man is Brilliant: So Why Doesn't Mexico's Economy Grow Faster?" *Americas Quarterly* (http://americasquarterly.org/content/man-brilliant-so-why-doesnt-mexicos-economy-grow-faster) (Accessed July 21, 2016)

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Education and Economic Growth in Historical Perspective

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In his introduction to the *Wealth of Nations*, Adam Smith (1776, p. 1) states that the proportion between the annual produce of a nation and the number of people who are to consume that produce depends on "the skill, dexterity, and judgment with which its labour is generally applied." In recent decades, analysts of economic productivity in the United States during the twentieth century have made allowance for Smith's "skill, dexterity, and judgment" of the labor force under the rubric of labor force quality (Ho and Jorgenson 1999; Aaronson and Sullivan 2001; DeLong, Goldin, and Katz 2003). These studies have found that a variety of factors have influenced labor force quality in the U.S., including age structure and

workforce experience, female labor force participation, and immigration. One of the most important determinants of labor force quality has been years of schooling completed by the labor force.

Data limitations complicate generalizing these findings to periods before the twentieth century and to geographical areas beyond the United States. However, the rise of modern economic growth over the last few centuries seems to roughly coincide with the rise of mass schooling throughout the world. The sustained growth in income per capita evidenced in much of the world over the past two to two and a half centuries is a marked divergence from previous tendencies. Kuznets (1966) used the phrase "modern economic growth" to describe this divergence and he placed its onset in the mid-eighteenth century. More recently, Maddison (2001) has placed the start of sustained economic growth in the early nineteenth century. Maddison (1995) estimates that per capita income between 1520 and 1992 increased some eight times for the world as a whole and up to seventeen times for certain regions. Popular schooling was not widespread anywhere in the world before 1600. By 1800, most of North America, Scandinavia, and Germany had achieved literacy rates well in excess of fifty percent. In France and England literacy rates were closer to fifty percent and school attendance before the age of ten was certainly widespread, if not yet the rule. It was not until later in the nineteenth century and the early twentieth century that Southern and Eastern Europe were to catch up with Western Europe and it was only the first half of the twentieth century that saw schooling become widespread through much of Asia and Latin America. Only later in the twentieth century did schooling begin to spread throughout Africa. The twentieth century has seen the spread of secondary and university education to much of the adult population in the United States and to a lesser extent in other developed countries.[2] However, correlation is not causation; rising income per capita may have contributed to rising levels of schooling, as well as schooling to income levels. Thus, the contribution of rising schooling to economic growth should be examined more directly.

Estimating the Contribution of the Rise of Mass Schooling to Economic Growth: A Growth Accounting Perspective

Growth accounting can be used to estimate the general bounds of the contribution the rise of schooling has made to economic growth over the past few centuries.[3] A key assumption of growth accounting is that factors of production are paid their social marginal products. Growth accounting starts with estimates of the growth of individual factors of production, as well as the shares of these factors in total output and estimates of the growth of total product. It then apportions the growth in output into that attributable to growth in each factor of production specified in the analysis and into that due to a residual that cannot otherwise be explained. Estimates of how much schooling has increased the productivity of individual workers, combined with estimates of the increase in schooling completed by the labor force, yield estimates of how much the increase in schooling has contributed to increasing output. A growth accounting approach offers the advantage that with basic estimates (or at least possible ranges) for trends in output, labor force, schooling attainment, and preferably capital stock and factor shares, it yields estimates of schooling's contribution to economic growth. An important disadvantage is that it relies on indirect estimates at the micro level for how schooling influences productivity at the aggregate level, rather than on direct empirical evidence.[4]

Back-of-the-envelope estimates of increases in income per capita attributable to rising levels of education over a period of a few centuries can be obtained by considering possible ranges of levels of schooling increases as measured in average years of schooling along with possible ranges of rates of return per year of schooling, in terms of the percentage by which a year of schooling raises earnings and common ranges for labor's share in national income. By using a Cobb-Douglas specification of the

aggregate production function with two factors of production, labor and physical capital, one can arrive at the following equation for the ratio between final and initial national income per worker due to increases in average school years completed between the two time periods:

1) (Y/L)1/ (Y/L)0 = ((1 + r)S1 - S0)

Where Y = output, L = the labor force, r = the percent by which a year of schooling increases labor productivity, S is the average years of schooling completed by the labor force in each time period, is labor's share in national income, and the subscripts 0 and 1 denote the initial and final time period over which the schooling changes occur.[5] This formulation is a partial equilibrium one, holding constant the level of physical capital. However, the level of physical capital should be expected to increase in response to improved labor force quality due to more schooling. A common specification of a growth model that allows for such responses of physical capital implies the following ratio between final and initial national income per worker (see Lord 2001, 99-100):

2) (Y/L)1/ (Y/L)0 = ((1 + r)S1 - S0)

The bounds on increases in years of schooling can be placed at between zero and 16, that is, between a completely unschooled and presumably illiterate population to one in which a college education is universal. As bounds on returns to increasing earnings per year of schooling, one can employ Krueger and Lindahl's (2001) survey of results from recent estimates of earnings functions, which finds that returns range from 5 percent to 15 percent. The implications of varying these two parameters are reported in Tables 1A and 1B. Table 1A reports estimates based on the partial equilibrium specification holding constant the level of physical capital in equation 1). Table 1B reports estimates allowing for a changing level of physical capital as in equation 2). Labor's share of income has been set at a commonly used value of 0.7 (see DeLong, Goldin and Katz 2003, 29; Maddison 1995, 255).

Table 1A
Increase in per Capita Income over a Base Level of 1 Attributable to Hypothetical Increases in Average Schooling Levels â " Holding the Physical Capital Stock Constant

	Percent Increase in Earnings per Extra Year of Schooling		
Increase in Average Years of Schooling	5%	10%	15%
1	1.035	1.07	1.10
3	1.11	1.22	1.34
6 – illiteracy to universal grammar school	.23	1.49	1.80
12 – illiteracy to universal high school	1.51	2.23	3.23
16 – illiteracy to universal college	1.73	2.91	4.78

Table 1B
Increase in per Capita Income over a Base Level of 1 Attributable to Hypothetical Increases in Average Schooling Levels â " Allowing for Steady-state Changes in the Physical Capital Stock

	Percent Increase in Earnings per Extra Year of Schooling		
Increase in Average Years of Schooling	5%	10%	15%
1	1.05	1.10	1.15
3	1.16	1.33	1.52
6 – illiteracy to universal grammar school	1.34	1.77	2.31
12 – illiteracy to universal high school	1.79	3.14	5.35
16 – illiteracy to universal college	2.18	4.59	9.36

The back-of-the-envelope calculations in Tables 1A and 1B make two simple points. First, schooling increases have the potential to explain a good deal of estimated long-term increases in per capita income. With the average member of an economy's labor force embodying investments of twelve years of schooling and a moderate ten-percent rate of return per year of schooling and no increase in the capital stock, at least 17 percent of Maddison's eight-fold increase in per capita income can be accounted for (i.e. 1.23/7) by rising schooling. Indeed, a 16 year schooling increase allowing for steady-state capital stock increases and at 15 percent per year return overexplains Maddison's eight-fold increase (8.36/7). After all, if schooling has had substantial effects on the productivity of individual workers, if a sizable share of the labor force has experienced improvements in schooling completed and with labor's share of output greater than half, then the contribution of rising schooling to increasing output should be large.

Second, the contribution of schooling increases that have actually occurred historically to per capita income increases is more modest accounting for at best about one fifth of Maddison's one-fold increase. Thus an increase in average years of schooling completed by the labor force of 6 years, roughly that entailed by the spread of universal grammar schooling, would account for 19 percent (1.31/7) of an eight-fold per capita output increase at a high 15 percent rate of return allowing for steady state changes in the physical capital stock (Table 1B). And at a low 5 percent return per year of schooling, the contribution would be only 5 percent of the increase (0.34/7). Making lower-level elementary education universal would entail increasing average years of schooling completed by the labor force by 1 to 3 years; in most circumstances this is not a trivial accomplishment as measured by the societal resources required. However, even at a high 15 percent per year return and allowing for steady state changes in the capital stock (Table 1B), the contribution of a 3 year increase in average years of schooling would only account for 7 percent (0.52/7) of Maddison's eight-fold increase.

How do the above proposed bounds on schooling increases compare with possible increases in the physical capital stock? Kendrick (1993, 143) finds a somewhat larger growth rate in his estimated human capital stock than in the stock of non-human capital for the U.S. between 1929 and 1969, though for the sub-period 1929-48, he estimates a slightly higher growth rate for the non-human capital stock. In contrast, Maddison (1995, 35-37) estimates larger increases in the value of non-residential structures per worker and in the value of machinery and equipment per worker than in years of schooling per adult for the U.S. and the U.K. between 1820 and 1992. For the U.S., he estimates that the value of non-residential structures per worker rose by 21 times and the value of machinery and equipment per worker rose by 141 times in comparison with a ten-fold increase in the years of schooling per adult between 1820 and 1992. For the U.K., his estimates indicate a 15 fold increase in the value of structures per worker and a 97 fold increase in value of machinery and equipment per worker in contrast with a seven-fold increase in

average years of schooling between 1820 and 1992. It should be noted that these estimates are based on cumulated investments in schooling to estimate human capital; that is, they are based on the *costs* incurred to produce human capital. Davies and Whalley (1991, 188-189) argue that estimates based on the alternative approach of calculating the present value of future earnings premiums attributable to schooling and other forms of human capital yield substantially higher estimates of human capital due to capturing inframarginal returns above costs accruing to human capital investments. For the growth accounting approach employed here, the cumulated investment or cost approach would seem the appropriate one. Are there more inherent bounds on the accumulation of human capital over time than non-human capital? One limit on the accumulation of human capital is set by how much of one's potential working life a worker is willing to sacrifice for purposes of improving education and future productivity. This can be compared with the corresponding limit on the willingness to sacrifice current consumption for wealth accumulation.

However, this discussion makes no explicit allowance for changes over time in the quality of schooling. Improvements in teacher training and teacher recruitment along with ongoing curriculum developments among other factors could lead to ongoing improvements over time in how much a year of school attendance would improve the underlying future productivity of the student. Woessmann (2002) and Hanushek and Kimcoe (2000) have recently argued for the importance of allowing for variation in school quality in estimating the impact of cross national variation in human capital levels on economic growth. Woessmann (2002) makes the suggestion that allowing for improvements in the quality of schooling can remove the upper bounds on schooling investment that would be present if this was simply a matter of increasing the percentage of the population enrolled in school at given levels of quality. While there would seem to be inherent bounds on the proportion of one's life that one is willing to spend in school, such bounds would not apply to increases in expenditures and other means of improving school quality.

Expenditures per pupil appear to have risen markedly over long periods of time. Thus, in the United States, expenditure per pupil in public elementary and secondary schools in constant 1989-90 dollars rose by over 6 times between 1923-24 and 1973-74 (National Center for Educational Statistics, 60). And in Victorian England, nominal expenditures per pupil in state subsidized schools more than doubled between 1870 and 1900, despite falling prices (Mitch 1982, 204). These figures do not control for the rising percentage of students enrolled in higher grade levels (presumably at higher expenditure per student), general rises in living standards affecting teachers' salaries and other factors influencing the abilities of those recruited into teaching. Nevertheless, they suggest the possibility of sizable improvements over time in school quality.

It can be argued that implicitly allowance is made for improvements in school quality in the rate of return imputed per year of schooling completed on average by the labor force. Insofar as schools became more effective over time in transmitting knowledge and skills, the economic return per year of schooling should have increased correspondingly. Thus any attempt to allow for school quality in a growth accounting analysis should be careful to avoid double counting school quality in both school inputs and in returns per year of schooling.

The benchmark for the impact of increases in average levels of schooling completed in Table 1 are Maddison's estimates of changes in output per capita over the last two centuries. In fact, major increases in schooling levels have most commonly been compressed into intervals of several decades or less, rather than periods of a century or more. This would imply that the contribution to output growth of improvements in labor force quality due to increases in schooling levels would have been concentrated primarily in periods of marked improvement in schooling levels and would have been far more modest

during periods of more sluggish increase in educational attainment. In order to gauge the impact of the time interval over which changes in schooling occur on growth rates of output, Table 2 provides the change in average years of schooling implied by some of the hypothetical changes in average levels of schooling attainment reported in Table 1 for various time periods.

Table 2

Annual Change in Average Years of Schooling per Adult per Year Implied by Hypothetical Figures in Table 1

	Time period over which increase occurred				
Total Increase in Average Years of Schooling per Adult	5 years	10 years	30 years	50 years	100 years
1	0.2	0.1	0.033	0.02	0.01
3	0.6	0.3	0.1	0.06	0.03
6	1.2	0.6	0.2	0.12	0.06
9	1.8	0.9	0.3	0.18	0.09

Table 3 translates these rates of schooling growth into output growth rates using the partial equilibrium framework of equation 1) using a value for the share of labor of 0.7 as above. The contribution of schooling to growth rates of output and output per capita can be calculated as labor's share times the percentage return per year of schooling on earnings times the annual increase in average years of schooling.

Table 3A
Contribution of Schooling for Large Increases in Schooling to Annual Growth Rates of Output

Length of time for schooling increase	6 year rise in average years of schooling 5% return	6 year rise in average years of schooling 10 % return	9 year rise in average years of schooling 5 % return	9 year rise in average years of schooling 10% return
30 years	0.7%	1.4%	1.05%	2.1%
50 years	0.42%	0.84%	0.63%	1.26%

Table 3B
Contribution of Schooling for Small to Modest Increases in Schooling to Annual Growth Rates of Output

Length of time for schooling increase	1 year rise in average years of schooling 5 % return	1 year rise in average years of schooling 10 % return	3 year rise in average years of schooling 5% return	3 year rise in average years of schooling 10% return
5 years	0.7%	1.4%	2.1%	4.2%
10 years	0.35%	0.7%	1.05%	2.1%
20 years	0.175%	0.35%	0.525%	1.05%
30 years	0.12%	0.23%	0.35%	0.7%
50 years	0.07%	0.14%	0.21%	0.42%
100 years	0.035%	0.07%	0.105%	0.21%

The case of the U.S. in the twentieth century as analyzed in DeLong, Goldin and Katz (2003) offers an example of how apparent limits or at least resistance to ongoing expansion of schooling have lowered the contribution of schooling to growth. They find that between World War I and the end of the century, improvements in labor quality attributable to schooling can account for about a quarter of the growth of output per capita in the U.S. during this period; this is similar in magnitude to Denison's (1962) estimates for the first part of this period. This era saw the mean years of schooling completed by age 35 increased from 7.4 years for an American born in 1875 to 14.1 years for an American born in 1975 (DeLong, Goldin and Katz 2003, 22). However, in the last two decades of the twentieth century the rate of increase of mean years of schooling completed leveled off and correspondingly the contribution of schooling to labor quality improvements fell almost in half.

Maddison (1995) has compiled estimates of the average years of schooling completed for a number of countries going back to 1820. It is indicative of the sparseness of schooling completed by adult population estimates that Maddison reports estimates for only 3 countries, the U.S., the U.K., and Japan, all the way back to 1820. Maddison's figures come from other studies and their reliability warrants further critical scrutiny than can be accorded them here. Since systematic census evidence on adult educational attainment did not begin until the mid-twentieth century, estimates of labor force educational attainment prior to 1900 should be treated with some skepticism. Nevertheless, Maddison's estimates can be used to give a sense of plausible changes in levels of schooling completed over the last century and a half. The average increases in years of schooling per year for various time periods implied by Maddison's figures are reported in Table 4. Maddison constructed his figures by giving primary education a weight of 1, secondary education a weight of 1.4, and tertiary, a weight of 2 based on evidence on relative earnings for each level of education.

Table 4
Estimates of the Annual Change in Average Years of Schooling per Person aged 15-64 for Selected Countries and Time Periods

Country	1913-1973	1870-1973	1870-1913
U.S.	0 .112	0.107	0.092
France	0.0783		
Germany	0.053		
Netherlands	0.064		
U.K.	0.0473	0.0722	0.102
Japan	0.112	0.106	0.090

Source: Maddison (1995), 37, Table 2-3

Table 5
Annual Growth Rates in GDP per Capita

Region	1820-70	1870-1913	1913-50	1950-73	1973-92
12 West European Countries	0.9	1.3	1.2	3.8	1.8
4 Western Offshoots	1.4	1.5	1.3	2.4	1.2
5 South European Countries	n.a.	0.9	0.7	4.8	2.2
7 East European Countries	n.a.	1.2	1.0	4.0	-0.8
7 Latin American Countries	n.a.	1.5	1.9	2.4	0.4
11 Asian Countries	0.1	0.7	-0.2	3.1	3.5
10 African countries	n.a.	n.a.	1.0	1.8	-0.4

Source: Maddison (1995), 62-63, Table 3-2.

In comparing Tables 2 and 4 it can be observed that the estimated actual changes in years of schooling compiled by Maddison (as well as the average over 55 countries reported by Lichtenberg (1994) for the third quarter of the twentieth century) fall within a lower bound set in the hypothetical ranges of a 3 year increase in average schooling spread over a century and an upper bound set by a 6 year increase in average schooling spread over 50 years.

Equations 1) and 2) above assume that each year of schooling of a worker has the same impact on productivity. In fact it has been common to find that the impact of schooling on productivity varies according to level of education. While the rate of return as a percentage of costs tends to be higher for primary than secondary schooling, which is in turn higher than tertiary education, this reflects the far lower costs, especially lower foregone earnings, of primary schooling (Psacharopolous and Patrinos 2004). The earnings premium per year of schooling tends to be higher for higher levels of education and this earnings premium, rather than the rate of return as a percentage costs, is the appropriate measure for assessing the contribution of rising schooling to growth (OECD 2001). Accordingly growth accounting analyses commonly construct schooling indexes weighting years of schooling according to estimates of each year's impact on earnings (see for example Maddison 1995; Denison 1962). DeLong, Goldin and Katz (2003) use chain weighted indexes of returns according to each level of schooling. A rough approximation of the effect of allowing for variation in economic impact by level of schooling in the analysis in Table 1 is simply to focus on the mid-range 10 percent rate of return as an approximate average of high, low, and medium level returns.[6]

The U.S. is notable for rapid expansion in schooling attainment over the twentieth century at both the secondary and tertiary level, while in Europe widespread expansion has tended to focus on the primary and lower secondary level. These differences are evident in Denison's estimates of the actual differences in educational distribution between the United States and a number of Western European countries in the mid-twentieth century (see Table 6).

Table 6

Percentage Distributions of the Male Labor Force by Years of Schooling Completed

Years of School Completed	United States 1957	France 1954	United Kingdom 1951	Italy 1961
0	1.4	0.3	0.2	13.7
1-4	5.7	2.4	0.2	26.1
5-6	6.3	19.2	0.8	38.0
7	5.8	21.1	4.0	4.2
8	17.2	27.8	27.2	8.1
9	6.3	4.6	45.1	0.7
10	7.3	4.1	8.4	0.7
11	6.0	6.5	7.3	0.6
12	26.2	5.4	2.5	1.8
13-15	8.3	5.4	2.2	3.0
16 or more	9.5	3.2	2.1	3.1

Source: Denison (1967), 80, Table 8-1.

Some segments of the population are likely to have much greater enhancements of productivity from additional years of schooling than others. Insofar as the more able benefit from schooling compared to the rest of the ability distribution, putting substantially greater relative emphasis on expansion of higher levels of schooling could considerably augment growth rates over a more egalitarian strategy. This result would follow from a substantially greater premium assigned to higher levels of education. However, some studies of education in developing countries have found that they allocate a disproportionate share of resources to tertiary schooling at the expense of primary schooling, reflecting efforts of elites to benefit their offspring. How this has impeded economic growth would depend on the disparity in rates of return among levels of education, a point of some controversy in the economics of education literature (Birdsall 1996; Psacharopoulos 1996).

While allocating schooling disproportionately towards the more able in a society may have promoted growth, there would have been corresponding losses stemming from groups that have been systematically excluded or at least restricted in their access to education due to discrimination by factors such as race, gender and religion (Margo 1990). These losses could be attributed in part to the presence of individuals of high ability in groups experiencing discrimination due to failure to provide them with sufficient education to properly utilize their talents. However, historians such as Ashton (1948, 15) have argued that the exclusion of non-Anglicans from English universities prior to the mid-nineteenth century resulted in the channeling of their talents into manufacturing and commerce.

Even if returns have been higher at some levels of education than others, a sustained and substantial increase in labor force quality would seem to entail an egalitarian strategy of widespread increase in access to schooling. The contrast between the rapid increase in access to secondary and tertiary schooling in the U.S. and the much more limited increase in access in Europe during the twentieth century with the correspondingly much greater role for schooling in accounting for economic growth in the U.S. than in Europe (see Denison 1967) points to the importance of an egalitarian strategy in sustaining ongoing increases in aggregate labor force quality.

One would expect on increase in the relative supply of more schooled labor to lead to a decline in the premium to schooling, other things equal. Some recent analyses of the contribution of schooling to growth have allowed for this by specifying a parametric relationship between the distribution of schooling in an economy's labor force and its impact on output or on a hypothesized intermediary human capital factor (Bils and Klenow 2000).[7]

Direct empirical evidence on trends in the premium to schooling is helpful both to obviate reliance on a theoretical specification and to allow for factors such as technical change that may have offset the impact of the increasing supply of schooling. Goldin and Katz (2001) have developed evidence on trends in the premium to schooling over the twentieth century that have allowed them to adjust for these trends in estimating the contribution of schooling to economic growth (DeLong, Goldin and Katz 2003). They find a marked fall in the premium to schooling, roughly falling in half between 1910 and 1950. However, they also find that this decline in the schooling premium was more than offset by their estimated increase over this same period in years of schooling completed by the average worker of 2.9 years and hence that on net schooling increases contributed to improved productivity of the U.S. workforce. They estimate increases of 0.5 percent per year in labor productivity due to increased educational attainment between 1910 and 1950 relative to the average total annual increase in labor productivity of 1.62 percent over the entire period 1915 to 2000. For the period since 1960, DeLong, Goldin and Katz find that the premium to education has increased while the increase in educational

attainment at first increased and then declined. During this latter period, the increase in labor force quality has declined, as noted above, despite a widening premium to education, due to the slowing down in the increase in educational attainment.

Classifying the Range of Possible Relationships between Schooling and Economic Growth

In generalizing beyond the twentieth-century U.S. experience, allowance should be made both for the role of influences other than education on economic growth and for the possibility that the impact of education on growth can vary considerably according to the historical situation. In fact to understand why and how education might contribute to economic growth over the range of historical experience, it is important to investigate the variation in the impact of education on growth that has occurred historically. In relating education to economic growth, one can distinguish four basic possibilities.

The first is one of stagnation in both educational attainment and in output per head. Arguably, this was the most common situation throughout the world until 1750 and even after that date characterized Southern and Eastern Europe through the late nineteenth century, as well as most of Africa, Asia, and Latin American through the mid-twentieth century. The qualifier "arguably" is inserted here, because this view of the matter almost surely makes inadequate allowance for the improvements in informal acquisition of skills through family transmission and direct experience as well as through more formal non-schooling channels such as guild-sponsored apprenticeships, an aspect to be taken up further below. It also makes no allowance for the possible long-term improvements in per capita income that took place prior to 1750 but have been inadequately documented. Still focusing on formal schooling as the source of improvement in labor force, there is reason to think that this may have been a pervasive situation throughout much of human history.

The second situation is one in which income per capita rose despite stagnating education levels; factors other than improvements in educational attainment were generating economic growth. England during its industrial revolution, 1750 to 1840 is a notable instance in which some historians have argued that this situation prevailed. During this period, English schooling and literacy rates rose only slightly if at all, while income per capita appears to have risen. Literacy and schooling appears to have been of little use in newly created manufacturing occupations such as in cotton spinning. Indeed, literacy rates and schooling actually appears to have declined in some of the most rapidly industrializing areas of England such as Lancashire (Sanderson 1972; Nicholas and Nicholas 1992). Not all have concurred with this interpretation of the role of education in the English industrial revolution and the result depends on how educational trends are measured and how education is specified as affecting output (see Laqueur; Crafts 1995; Mitch 1999). Moreover this makes no allowance for the role of informal acquisition of skills. Boot (1995) argues that in the case of cotton spinners, informal skill acquisition with experience was substantial.

The simplest interpretation of this situation is that other factors contributed to economic growth other than schooling or human capital more generally. The clearest non-human capital explanatory factor would perhaps be physical capital accumulation; another might be foreign trade. However, if one turns to technological advance as a driving force, then this gives rise to the possibility that human capital â " at least broadly defined â " was if not the underlying force at least a central contributing factor to the industrial revolution. The argument for this possibility is that the improvements in knowledge and skills associated with technological advance are embodied in human agents and hence are forms of human capital. Recent work by Mokyr (2002) would suggest this interpretation. Nevertheless, the British industrial revolution does remain as a prominent instance in which human capital conventionally

defined as schooling stagnated in the presence of a notable upsurge in economic growth. A less extreme case is provided by the post-World War II European catch-up with the United States, as Denison's (1967) growth accounting analysis indicates that this occurred despite slower European increases in educational attainment due to other factors offsetting this. Historical instances such as that of the British industrial revolution call into question the common assumption that education is a necessary prerequisite for economic growth (see Mitch 1990).

The third situation is one in which rising educational attainment corresponds with rising rates of economic growth. This is the situation one would expect to prevail if education contributes to economic productivity and if any negative factors are not sufficient to offset this influence. One sub-set of instances would be those in which very large and reasonably compressed increases in the educational attainment of the labor force occurred. One important example of this is the twentieth century U.S., with the high school movement followed by increases in college attendance, as noted above. Another would be those of certain East Asian economies since World War II, as documented in the growth accounting analysis by Young (1995) of the substantial contributions of their rising educational attainment to their rapid growth rates. Another sub-set of cases corresponding to more modest increases in schooling can be interpreted as applying either to countries experiencing schooling increases focussed at the elementary level, as in much of Western Europe over the nineteenth century. The so-called literacy campaigns, as in the Soviet Union and Cuba (see Arnove and Graff eds. 1987) in the early and mid-twentieth century with modest improvements in educational attainment over compressed time periods of just a few decades could also be viewed as fitting into this sub-category. However, whether there were increases in output per capita corresponding to these more modest increases in educational attainment remains to be established.

The fourth situation is one in which economic growth has stagnated despite the presence of marked improvements in educational attainment. Possible examples of this situation would include the early rise of literacy in some Northern European areas, such as Scotland and Scandinavia, in the seventeenth and eighteenth centuries (see Houston 1988; Sandberg 1979) and some regions of Africa and Asia in the later twentieth century (see Pritchett 2001). One explanation of this situation is that it reflects instances in which any positive impact of educational attainment is small relative to other influences having an adverse impact. But one can also interpret it as reflecting situations in which incentive structures direct educated people into destructive and transfer activities inimical to economic growth (see North 1990; Baumol 1990; Murphy, Shleifer, and Vishny 1991).

Cross-country studies of the relationship between changes in schooling and growth since 1960 have yielded conflicting results which in itself could be interpreted as supporting the presence of some mix of the four situations just surveyed. A number of studies have found at best a weak relationship between changes in schooling and growth (Pritchett 2001; Bils and Klenow 2000); others have found a stronger relationship (Topel 1999). Much seems to depend on issues of measurement and on how the relationship between schooling and output is specified (Temple 2001b; Woessmann 2002, 2003).

The Determinants of Schooling

Whether education contributes to economic growth can be seen as depending on two factors, the extent to which educational levels improve over time and the impact of education on economic productivity. The first factor is a topic for extended discussion in its own right and no attempt will be made to consider it in depth here. Factors commonly considered include rising income per capita, distribution of political power, and cultural influences (Goldin 2001, Lindert 2004, Mariscal and Sokoloff 2000, Easterlin 1981; Mitch 2004). The issue of endogeneity of determination has often been raised with respect to the

determinants of schooling. Thus, it is plausible that rising income contributes to rising levels of schooling and that the spread of mass education can influence the distribution of political power as well as the reverse. While these are important considerations, they are sufficiently complex to warrant extended attention in their own right.[8]

Influences on the Economic Impact of Schooling

Insofar as schooling improves general human intellectual capacities, it could be seen as having a universal impact irrespective of context. However, Rosenzweig (1995; 1999) has noted that the even the general influence of education on individual productivity or adaptability depend on the complexity of the situation. He notes that for agricultural tasks primarily involving physical exertion, no difference in productivity is evident between workers according to education levels; however, in more complex allocative decisions, education does enhance performance. This could account for findings that literacy rates were low among cotton spinners in the British industrial revolution despite findings of substantial premiums to experience (Sanderson 1972; Boot 1995). However, other studies have found literacy to have a substantial positive impact on labor productivity in cotton textile manufacture in the U.S., Italy, and Japan (Bessen 2003; A'Hearn 1998, Saxonhouse 1977) and have suggested a connection between literacy and labor discipline.

A more macro influence is the changing sectoral composition of the economy. It is common to suggest that the service and manufacturing sector have more functional uses for educated labor than the agricultural sector and hence that the shift from agriculture to industry in particular will lead to greater use of educated labor and in turn to require more educated labor forces. However, there are no clear theoretical or empirical grounds for the claim that agriculture makes less use of educated labor than other sectors of the economy. In fact, farmers have often had relatively high literacy rates and there are more obvious functional uses for education in agriculture in keeping accounts and keeping up with technological developments than in manufacturing. Nilsson et al (1999) argue that the process of enclosure in nineteenth-century Sweden, with the increased demands for reading and writing land transfer documents that this entailed, increased the value of literacy in the Swedish agrarian economy. The findings noted above that those in cotton textile occupations associated with early industrialization in Britain had relatively low literacy rates is one indication of the lack of any clear cut ranking across broad economic sectors in the use of educated labor.

Changes in the organization of decision making within major sectors as well as changes in the composition of production within sectors are more likely to have had an impact on demands for educated labor. Thus, within agriculture the extent of centralization or decentralization of decision making, that is the extent to which farm work forces consisted of farmers and large numbers of hired workers or of large numbers of peasants each with scope for making allocative decisions, is likely to have affected the uses made of educated labor in agriculture. Within manufacturing, a given country's endowment of skilled relative to unskilled labor has been seen as influencing the extent to which openness to trade increases skill premiums, though this entails endogenous determination (Wood 1995).

Technological advance would have tended to boost the demand for more skilled and educated labor if technological advance and skills are complementary, as is often asserted.

However, there is no theoretical reason why technology and skills need be complementary and indeed concepts of directed technological change or induced innovation would suggest that in the presence of relatively high skill premiums, technological advance would be skill saving rather than skill using. Goldin

and Katz (1998) have argued that the shift from the factory to continuous processing and batch production associated with the shift of power sources from steam to electricity in the early twentieth century lead to rising technology skill complementarity in U.S. manufacturing. It remains to be established how general this trend has been. It could be related to the distinction made between the dominance in the United States of extensive growth in the nineteenth century due to the growth of factors of production such as labor and capital and the increasing importance of intensive growth in the twentieth century. Intensive growth is often associated with technological advance and a presumed enhanced value for education (Abramovitz and David 2000). Some analysts have emphasized the importance of capital-skill complementarity. For example, Galor and Moav (2003) point to the level of the physical capital stock as a key influence on the return to human capital investment; they suggest that once physical capital stock accumulation surpassed a certain level, the positive impact of human capital accumulation on the return to physical capital became large enough that owners of physical capital came to support the rise of mass schooling. They cite the case of schooling reform in early twentieth century Britain as an example.

Even sharp declines in the premiums to schooling do not preclude a significant impact of education on economic growth. DeLong, Goldin and Katz's (2003) growth accounting analysis for the twentieth century U.S. makes the point that even at modest positive returns to schooling on the order of 5 percent per year of schooling, with large enough increases in educational attainment, the contribution to growth can be substantial.

Human Capital

Economists have generalized the impact of schooling on labor force quality into the concept of human capital. Human capital refers to the investments that human beings make in themselves to enhance their economic productivity. These investments can take on many forms and include not only schooling but also apprenticeship, a healthy diet, and exercise, among other possibilities. Some economists have even suggested that more amorphous societal factors such as trust, institutional tradition, technological know how and innovation can all be viewed as forms of human capital (Temple 2001a; Topel 1999; Mokyr 2002). Thus broadly defined, human capital would appear as a prime candidate for explaining much of the difference across nations and over time in output and economic growth. However, gaining much insight into the actual magnitudes and the channels of influence by which human capital might influence economic growth requires specification of both the nature and determinants of human capital and how human capital affects aggregate production of an economy.

Much of the literature on human capital and growth makes the implicit assumption that some sort of numerical scale exists for human capital, even if multidimensional and even if unobservable. This in turn implies that it is meaningful to relate levels and changes of human capital to levels of income per capita and rates of economic growth. Given the multiplicity of factors that influence human knowledge and skill and in turn how these influence labor productivity, difficulties would seem likely to arise with attempts to measure aggregate human capital similar to those that have arisen with attempts to specify and measure the nature of human intelligence. Woessmann (2002, 2003) provides useful surveys of some of the issues involved in attempting to specify human capital at the aggregate level appropriate for relating it to economic growth.

One can distinguish between approaches to the measurement of human capital that focus on schooling, as in the discussion above, and those that take a broader view. Broad view approaches try to capture all investments that may have improved human productivity from whatever source, including not just

schooling but other productivity enhancing investments, such as on-the-job training. The basic premise of broad view approaches is that for an aggregate economy, the income going to labor over and above what that labor would earn if it were paid the income of an unskilled worker can be viewed as human capital. This measure can be constructed in various ways including as a ratio using unskilled labor earnings as the denominator as in Mulligan and Sala-I-Martin (1997) or using the share of labor income not going as compensation for unskilled labor as in Crafts (1995) and Mitch (2004). Mulligan and Sala-I-Martin (2000) point to some of the major index number problems that can arise in using this approach to aggregate heterogeneous workers.

Crafts and Mitch find that for Britain during its late eighteenth and early nineteenth century industrial revolution between one-sixth and one-fourth of income per capita can be attributed to human capital measured as the share of labor income not going as compensation for unskilled labor.

One approach that has been taken recently to estimate the role of human capital differences in explaining international differences in income per capita is to consider changes in immigrant earnings between origin and destination countries along with differences between immigrant and native workers in the destination country. Olson (1996) suggested that the large increase in earnings of immigrants commonly observed in moving from a low income to a high income country points to a small role for human capital in explaining the wide variation in per capita income across countries. Hendricks (2002) has used differences between immigrant and native earnings in the U.S. to estimate the contribution of otherwise unobserved skill differences to explaining differences in income per capita across countries and finds that they account for only a small part of the latter differences. Hendricks' approach raises the issue of whether there could be long-term increases in otherwise unobserved skills that could have contributed to economic growth.

The Informal Acquisition of Human Capital

One possible source of such skills is through the informal acquisition of human capital through on-the-job experience. Insofar as work has been common from early adolescence onwards, the issue arises of why the aggregate stock of skills acquired through experience would vary over time and thus influence rates of economic growth. Some types of on-the-job experience which contribute to economic productivity, such as apprenticeship, may entail an opportunity cost and aggregate trends in skill accumulation will be influenced by societal willingness to incur such opportunity costs.

Insofar as schooling continues through adolescence, this can interfere with the accumulation of workforce experience. DeLong, Goldin and Katz (2003) note the tradeoff between rising average years of schooling completed and decreasing years of labor force experience in influencing labor force quality of the U.S. labor force in the last half of the twentieth century. Connolly (2004) has found that informal experience played a relatively greater role in Southern economic growth than for other regions of the United States.

Hansen (1997) has also distinguished the academically-oriented secondary schooling the United States developed in the late nineteenth and early twentieth century from the vocationally-oriented schooling and apprenticeship system that Germany developed over the same time period. Goldin (2001) argues that in the United States the educational system developed general abilities suitable for the greater opportunities for geographical and occupational mobility that prevailed there, while specific vocational training was more suitable for the more restricted mobility opportunities in Germany.

Little evidence exists on whether long-term trends in informal opportunities for skill acquisition have

influenced growth rates. However, Smith's (1776) view of the importance of the division of labor in influencing productivity would suggest that the impact of trends in these opportunities may well have been quite sizable.

Externalities from Education

Economists commonly claim that education yields benefits to society over and above the impact on labor market productivity perceived by the person receiving the education. These benefits can include impacts on economic productivity, such as impacts on technological advance. They can also include non-labor market benefits. Thus McMahon (2002, 11) in his assessment of the social benefits of education includes not only direct effects on economic productivity but also impacts on a) population growth rates and health b) democratization, political stability, and human rights, c) the environment, d) reduction of poverty and inequality, e) crime and drug use, and f) labor force participation. While these effects may appear to involve primarily non-market activity and thus would not be reflected in national output measures and growth rates, factors such as political stability, democratization, population growth, and health have obvious consequences for prospects for long-term growth. However, allowance should be made for the simultaneous influence of the distribution of political power and life expectancy on societal investments in schooling.

For the period since 1960, numerous studies have employed cross country variation in various estimates of human capital and income per capita to directly estimate the impact of human capital on levels of income per capita and growth. A central goal of many such estimates is to see if there are externalities to education on output over and above the private returns estimated from micro data. The results have been conflicting and this has been attributed not only to problems of measurement error but also to differences in specification of human capital and its impact on growth. There does not appear to be strong evidence of large positive externalities to human capital (Temple 2001a). Furthermore, McMahon (2004) reports some empirical specifications which yield substantial indirect long-run effects.

For the period before 1960, limits on the availability of data on schooling and income have limited the use of this empirical regression approach. Thus, any discussion of the impact of externalities of education on production is considerably more conjectural. The central role of government, religious, and philanthropic agencies in the provision of schooling suggests the presence of externalities. Politicians and educators more frequently justified government and philanthropic provision of schooling by its impacts on religious and moral behavior than by any market failure resulting in sub-optimal provision of schooling from the standpoint of maximizing labor productivity. Thus, Adam Smith in his discussion of mass schooling in *The Wealth of Nations,* places more emphasis on its value to the state in enhancing orderliness and decency while reducing the propensity to popular superstition than on its immediate value in enhancing the economic productivity of the individual worker.

The Impact of the Level of Human Capital on Rates of Economic Growth

The approaches considered thus far relate changes in educational attainment of the labor force to changes in output per worker. An alternative, though not mutually exclusive, approach is to relate the level of educational attainment of an economy's labor force to its rate of economic growth. The argument for doing so is that a high but unchanging level of educational attainment should contribute to growth by facilitating creativity, innovation and adaptation to change as well as facilitating the ongoing maintenance and improvement of skill in the workforce. Topel (1999) has argued that there may not be

any fundamental difference between the two types of approach insofar as ongoing sources of productivity advance and adaptation to change could be viewed as reflecting ongoing improvements in human capital. Nevertheless, some empirical studies based on international data for the late twentieth century have found that a country's level of educational attainment has a much stronger impact on its rate of economic growth than its rate of improvement in educational attainment (Benhabib and Spiegel 1994).

The paucity of data on schooling attainment has limited the empirical examination of the relationship between levels of human capital and economic growth for periods before the late twentieth century. However, Sandberg (1982) has argued, based on a descriptive comparison of economies in various categories, that those with high levels of schooling in 1850 subsequently experienced faster rates of economic growth. Some studies, such as O'Rourke and Williamson (1997) and Foreman-Peck and Lains (1999), have found that high levels of schooling and literacy have contributed to more rapid rates of convergence for European countries in the late nineteenth century and at the state level for the U.S. over the twentieth century (Connolly 2004).

Bowman and Anderson (1963), a much earlier study based on international evidence for the mid-twentieth century, can be interpreted in the spirit of relating levels of education to subsequent levels of income growth. Their reading of the cross-country relationship between literacy rates and per capita income at mid-twentieth-century was that a threshold of 40 percent adult literacy was required for a country to have a per capita income above 300 1955 dollars. Some have ahistorically projected back this literacy threshold to earlier centuries although the Bowman and Anderson proposal was intended to apply to mid-twentieth century development patterns.

The mechanisms by which the level of schooling would influence the rate of economic growth are problematic to establish. One can distinguish two general possibilities. One would be that higher levels of educational attainment facilitate adaptation and responsiveness to change throughout the workforce. This would be especially important where a large percentage of workers are in decision making positions such as an economy composed largely of small farmers and other small enterprises. The finding of Foster and Rosenzweig (1996) for late twentieth century India that the rate of return to schooling is higher during periods of more rapid technological advance in agriculture would be consistent with this. Likewise, Nilsson et al (1999) find that literacy was important for nineteenth-century Swedish farmers in dealing with enclosure, an institutional change. The other possibility is that higher levels of educational attainment increase the potential pool from which an elite group responsible for innovation can be recruited. This could be viewed as applying specifically to scientific and technical innovation as in Mokyr (2002) and Jones (2002) â " but also to technological and industrial leadership more generally (Nelson and Wright 1992) and to facilitating advancement in society by ability irrespective of social origins (Galor and Tsiddon 1997). Recently, Labuske and Baten (2004) have found that international rates of patenting are related to secondary enrollment rates.

Two issues have arisen in the recent theoretical literature regarding specifying relationships between the level of human capital and rates of economic growth. First, Lucas (1988) in an influential model of the impact of human capital on growth, specifies that the rate of growth of human capital formation depends on initial levels of human capital, in other words that parents' and teachers' human capital has a direct positive influence on the rate of growth of learners' human capital. This specification of the impact of the initial level of human capital allows for ongoing and unbounded growth of human capital and through this its ongoing contribution to economic growth. Such ongoing growth of human capital could occur through improvements in the quality of schooling or through enhanced improvements in learning

from parents and other informal settings. While it might be plausible to suppose that improved education of teachers will enhance their effectiveness with learners, it seems less plausible to suppose that this enhanced effectiveness will increase unbounded in proportion to initial levels of education (Lord 2001, 82).

A second issue is that insofar as higher levels of human capital contribute to economic growth through increases in research and development activity and innovative activity more generally, one would expect the presence of scale effects. Economies with larger populations holding constant their level of human capital per person should benefit from more overall innovative activity simply because they have more people engaged in innovative activity. Jones (1995) has pointed out that such scale effects seem implausible if one looks at the time series relationship between rates of economic growth and those engaged in innovative activity. In recent decades the growth of the number of scientists, engineers, and others engaged in innovative activity has far outstripped the actual growth of productivity and other indicators of direct impact on innovation. Thus, one should allow for diminishing returns in the relationship between levels of education and technological advance.

Thus, as with schooling externalities, considering the impact of levels of education on growth offers numerous channels of influence leaving the challenge for the historian of ascertaining their quantitative importance in the past.

Conclusion

This survey has considered some of the basic ways in which the rise of mass education has contributed to economic growth in recent centuries. Given their potential influence on labor productivity, levels and changes in schooling and of human capital more generally have the potential for explaining a large share of increases in per capita output over time. However, increases in mass schooling seem to explain a major share of economic growth only over relatively short periods of time, with a more modest impact over longer time horizons. In some situations, such as the United States in the twentieth century, it appears that improvements in the schooling of the labor force have made substantial contributions to economic growth. Yet schooling should not be seen as either a necessary or sufficient condition for generating economic growth. Factors other than education can contribute to economic growth and in their absence, it is not clear that schooling in itself can contribute to economic growth. Moreover, there are likely limits on the extent to which average years of schooling of the labor force can expand, although improvement in the quality of schooling is not so obviously bounded. Perhaps the most obvious avenue through which education has contributed to economic growth is by expanding the rate of technological change. But as has been noted, there are numerous other possible channels of influence ranging from political stability and property rights to life expectancy and fertility. The diversity of these channels point to both the challenges and the opportunities in examining the historical connections between education and economic growth.

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[1] I have received helpful comments on this essay from Mac Boot, Claudia Goldin, Bill Lord, Lant Pritchett, Robert Whaples, and an anonymous referee. At an earlier stage in working through some of this material, I benefited from a quite useful conversation with Nick Crafts. However, I bear sole responsibility for remaining errors and shortcomings.

[2] For a detailed survey of trends in schooling in the early modern and modern period see Graff (1987).

[3] See Barro (1998) for a brief intellectual history of growth accounting.

[4] Blaug (1970) provides an accessible, detailed critique of the assumptions behind Denison's growth accounting approach and Topel (1999) provides a further discussion of the problems of using a growth accounting approach to measure the contribution of education, especially those due to omitting social externalities.

[5] By using a Cobb-Douglas specification of the aggregate production function, one can arrive at the following equation for the ratio between final and initial national income per worker due to increases in average school years completed between the two time periods, t = 0 and t =1:

Start with the aggregate production function specification:

Y = A K(1-) [(1+r)S L]

Y/L = A (K/L)(1-) [(1+r)S L/L]

Y/L = A (K/L)(1-) [(1+r)S]

Assume that the average years of schooling of the labor force is the only change between t = 0 and t =1; that is, assume no change in the ratio of capital to labor between time periods. Then the ratio of the income per worker in the later time period to the earlier time period will be:

(Y/L)1/ (Y/L)0 = ((1 + r)S1- S0)

Where Y = output, A = a measure of the current state of technology, K = the physical capital stock, L = the labor force, r = the percent by which a year of schooling increases labor productivity, S is the average years of schooling completed by the labor force in each time period, is labor's share in national income, and the subscripts 0 and 1 denote initial and final time periods.

As noted above, the derivation above is for a partial equilibrium change in years of schooling of the labor force holding constant the physical capital stock. Allowing for physical capital stock accumulation in response to schooling increases in a Solow-type model implies that the ratio of final to initial output per worker will be

$(Y/L)1/ (Y/L)0 = ((1 + r)S1 - S0)$.

For a derivation of this see Lord (2001, 99-100). Lord's derivation differs from that here by specifying the technology parameter A as labor augmenting. Allowing for increases in A over time due to technical change would further increase the contribution to output per worker of additional years of schooling.

[6]To take a specific example, suppose that in the steady-state case of Table 1B, a 5 percent earnings premium per year of schooling is assigned to the first 6 years of schooling, i.e. primary schooling, a 10 percent earnings premium per year is assigned to the next 6 years of schooling, i.e. secondary schooling, and a 15 percent earnings premium per year is assigned to the final 4 years of schooling, that is college. In that case, the impact on steady state income per capita compared with no schooling at all would be (1.05)6x(1.10)6x(1.15)4 = 4.15, compared with the 4.59 in going from no schooling to universal college at a 10 percent rate of return for every year of school completed.

[7] Denison's standard growth accounting approach assumes that education is labor augmenting and, in particular, that there is an infinite elasticity of substitution between skilled and unskilled labor. This specification is conventional in growth accounting analysis. But another common specification in entering education into aggregate production functions is to specify human capital as a third factor of production along with unskilled labor and physical capital. Insofar as this is done with a Cobb-Douglas production function specification, as is conventional, the implied elasticity of substitution between human capital and either unskilled labor or physical capital is unity. The complementarity between human capital and other inputs this implies will tend to increase the contribution of human capital increases to economic growth by decreasing the tendency for diminishing returns to set in. (For a fuller treatment of the considerations involved see Griliches 1970, Conlisk 1970, Broadberry 2003). For an application of this approach in a historical growth accounting exercise, see Crafts (1995), who finds a fairly substantial contribution of human capital during the English industrial revolution. For a critique of Crafts' estimates see Mitch (1999).

[8] For an examination of long-run growth dynamics with schooling investments endogenously determined by transfer-constrained family decisions see Lord 2001, 209-213 and Rangazas 2000. Lord and Rangazas find that allowing for the fact that families are credit constrained in making schooling investment decisions is consistent with the time path of interest rates in the U.S. between 1870 and 1970.

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Distant Tyranny: Markets, Power, and Backwardness in Spain, 1650-1800

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Regina Grafe, *Distant Tyranny: Markets, Power, and Backwardness in Spain, 1650-1800*. Princeton, NJ: Princeton University Press, 2012. xvii + 291 pp. $39.50 (hardcover), ISBN 978-0-691-14484-9.

Reviewed for EH.Net by Mauricio Drelichman, Department of Economics, University of British Columbia.

Spain has long been a thorn in the side of economic historians. In the sixteenth century, it was richer and politically more advanced than virtually all its European peers. At the beginning of the seventeenth it ruled the largest empire that had yet existed, while giving the world masterpieces of art and literature. Ever since, searching for the roots of the long ?decline? that ensued has been the single-minded quest of political commentators and historians of Spain alike. With *Distant Tyranny*, Regina Grafe may well have put this entire group out of a job.

A large, if relatively recent, literature has established that Spain did not quite stagnate after 1600, and certainly did not decline. It nonetheless grew at a substantially slower pace than the European norm, and hence fell steadily through the ranks of Western economic powers. Economists in the New Institutional Economics tradition pinned the blame for this on a predatory, absolutist state and its extractive institutions. Historians, on their part, have often pointed at the ?tyranny of distance? ? Spain?s famously tortuous geography and difficult communications ? as the main culprit for its economic underperformance.

To take on both the NIE and the geographic traditions, Grafe enlists the help of an unlikely ally: salt cod, or *bacalao*, as it is still known in Spain. By the seventeenth century cod was a staple food in Spain, and the trade imbalance it represented was a continuous source of worry to mercantilist minds. Traditional studies of market integration have relied almost exclusively on grain prices. Grain markets, however, were characterized by heavy intervention from local governments to guarantee bread supplies. Cod was free from any such distortions, while ubiquitous enough to serve as a benchmark good. Chapter 3 explores the production technology, trade mechanisms, internal distribution, and the cultural and biological bases of cod demand. The result is a strong case that cod can indeed be used to trace and characterize the behavior of markets across Spain.

Data in hand, Grafe proceeds to evaluate the tyranny of distance argument. That Spanish markets were not integrated across different regions is a well-known result. What is new ? and striking ? is that the lack of integration is almost completely uncorrelated with distance and transport costs. These were indeed formidable obstacles, but progress was being steadily made in improving road links and reducing travel times. More importantly, when the price of North American cod in the Atlantic port of Seville was 40% higher than in far inland Madrid, long distances and poor transport seem the wrong places to look for an explanation.

Chapters 5 and 6 deliver the central thesis of the book. The roots of Spain?s lack of integration ?and the

backwardness that ultimately resulted from it? are traced to the extreme jurisdictional fragmentation that afflicted the peninsula. The first indictment is handed to Spain?s ?historic territories? ? roughly corresponding to the medieval predecessor states ? which persisted in the form of separate kingdoms until the eighteenth century, and much longer as differentiated fiscal and political institutions. Next, the ?urban republics? ? the cities as seats of economic and political power ? take their share of the blame. Both territories and cities had inherited old ?freedoms?. These consisted in exemptions from royal taxation and the ability to maintain differentiated fiscal regimes, resulting in large distortions and formidable barriers to trade. Cities and territories were able to push back on each and every attempt of the monarchy to streamline and rationalize the system. Their success evidenced how little power the supposedly ?absolutist? Habsburg and Bourbon dynasties really had. While kings were expected to serve as arbiters and mediators, urban and regional elites severely curtailed the power of any monarch that tried to engage in meaningful state-building. In one telling example, the crown strove to harmonize the tobacco monopoly ? one of its most profitable revenue streams ? across the country. While dutifully implemented in Castile, the historic heartland of Spain, the tax was strongly resisted in some territories and lackadaisically enforced in others. The kingdom of Navarre dismissed it outright, arguing that its own monopoly predated the Crown?s. In order to maintain consistency across tax rates and internal borders, and unable to force a unified fiscal implementation, the king of Spain was forced to become the tax farmer of the Navarrese tobacco monopoly ? a monarch reduced to serving as a contractor of one of its own domains.

There is little to quibble with in Grafe?s work. The early chapters build a solid foundation, based on detailed archival research and a meticulous tracing of market behavior. As the focus shifts to broader issues of political economy, the weight of the argument comes to rest on a tightly woven historical narrative and a brilliant command of a literature as unwieldy as any. When applied to an issue as weighty as the long-run underperformance of Spain, one cannot help but admire the combination of a detailed, erudite analysis with a coherent macro-historical logic. If one area for improvement were to be found, it would be on the comparative aspect. While the centralist French experience is consistently brought up as the mirror image of Spain, perhaps Germany, with its large jurisdictional fragmentation but very different political and economic trajectories, might have provided a more interesting counterpoint.

When combined with the weakness of the central state, the hodgepodge of privileges, exemptions, and other ?freedoms? of Spanish territories and cities resulted in a system that preserved the special interests of local elites at the expense of consumers ? and, ultimately, of long-run growth. Whenever kings tried to strengthen their position, revolts and secession ensued. Monarchs used repression sparingly, and always accompanied it with a relaxation of the offending policies. The paradigm of a weak center and strong regions survived throughout the old regime, becoming a headache for the Napoleonic invaders, and only retreating in the twentieth century under the extraordinary violence brought about by the Civil War. However, as these lines are written, nearly two million people fill the streets of Barcelona clamoring for Catalan independence. In a rare feat for an economic history book, *Distant Tyranny* may yet shed as much light on current events as it does on the past.

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Subject(s): Economywide Country Studies and Comparative History
Government, Law and Regulation, Public Finance
Historical Geography
International and Domestic Trade and Relations
Markets and Institutions

Geographic Area(s): Europe

Time Period(s): 17th Century
18th Century

Why Europe Grew Rich and Asia Did Not: Global Economic Divergence, 1600-1850

Author(s): Parthasarathi, Prasannan

Reviewer(s): Mokyr, Joel

Published by EH.Net (January 2012)

Prasannan Parthasarathi, *Why Europe Grew Rich and Asia Did Not: Global Economic Divergence, 1600-1850*. Cambridge: Cambridge University Press, 2011. 365 pp. $30 (paperback), ISBN: 978-0-521-16824-3.

Reviewed for EH.Net by Joel Mokyr, Departments of Economics and History, Northwestern University.

The year 2011 was? a banner year for ambitious books that explain what is becoming known as the ?Great Divergence? of the West and the Rest. In addition to the book under review here, two other books by major scholars have appeared: Jean-Laurent Rosenthal and Bin Wong?s *Before and Beyond Divergence* and Ian Morris?s *Why the West Rules for Now*. One would have hoped that the proliferation of this literature would perhaps start to converge toward a consensus, but alas, so far discord and confusion reign in this debate. One could call it perhaps ?the small divergence.?

Parthasarathi?s book, its title notwithstanding, is not about ?Asia? and ?Europe? but really about India and Britain. While there are some pages that deal with China and the European Continent, the author clearly is most at ease when writing about the former two countries. Japan makes only an occasional appearance, and the rest of Asia does not figure much. Above all, this book is an argument that the ?California hypothesis? applies to India as well. The California School, most notably embodied in Kenneth Pomeranz?s *Great Divergence* and Andre Gunder Frank?s *Re-Orient*, argue that the Chinese economy rather than falling behind from Ming times actually performed admirably until the end of the eighteenth century, and that it was only the Industrial Revolution in Western Europe that (temporarily) gave Europe a big advantage. Parthasarathi?s book makes a similar argument for India. Moghul India, he argues, was not backward, primitive, ignorant, poorly organized, or anything of the kind. As late as 1800 it was technologically sophisticated, scientifically progressive, commercialized with well-working institutions

and efficient markets that allowed the Indian economy to work well. Its cottons were in high demand world-wide, and its abilities in a host of other industries were more than respectable.

If everything in India was so good, why was everything so bad? Parthasarathi?s argument depends heavily on his interpretation of the British Industrial Revolution. For him the Industrial Revolution was above all policy-driven. In his interpretation, the Industrial Revolution was the result of deliberate industrial policies by its governments, protecting domestic industry and encouraging import-substitution. What was true for Britain, Parthasarathi continues, was even more true for the Continental countries. Industrial policies shaped the speed and form of economic development. The one country that had as much potential as Belgium or Germany was India. But the British Raj mercilessly pressed their advantage against the hapless Indians, denied them access to the British market, and forced them to buy the cheap cotton products with which Lancashire after 1820 increasingly flooded the Indian market. India was ruled by the British, and they not only did nothing to encourage the development of manufacturing there; they did all they could to obstruct it. The power of states could determine successful economic development and prevent it elsewhere.

The argument is, of course, not new, and much of the evidence that Parthasarathi relies on has been used by other scholars. A notable example of an important essay published more than three decades ago (not cited by Parthasarathi) is Subramanian Swamy?s ?The Response to Economic Challenge: A Comparative Economic History of China and India, 1870-1952? (*Quarterly Journal of Economics*, 1979). Swamy fully anticipates Parthasarathi?s argument: ?The slow industrialization, or at least the lack of rapid industrial growth in India … can, be ascribed to the unwillingness of foreign capital to enter into the basic industries, and to the British Indian Government for placing obstacles in the path of indigenous investment … It was the combination of ?British interests? and the underlying social ethos of the Government of India that they were there ?to govern, to stabilize, and to administer,? but not to transform that proved to be the main cause of India?s slow development? (pp. 37?38).

Yet it is far from clear whether we have fully disposed of ?deep? cultural differences between Asia and Europe. Eight years after Swamy?s paper, Gregory Clark published a famous paper with a disturbing finding, comparing the Indian cotton mills in Bombay to those in Lancashire. He found that although Indian wages were only a sixth of British wages and they were using essentially the same equipment, the Indians had no major cost advantage over their British competitors, because Indian labor?s efficiency was only a fraction of their counterparts in Manchester. Clark left the reasons for this open, but subsequently in *A Farewell to Alms* he returns to the issue and argues that labor quality was remarkably low in poor countries, because of high absenteeism, poor discipline and similar matters. Whether Clark is right or not, Parthasarathi pays no heed to his work. Perhaps he can show us that low labor productivity can somehow be chalked up to the Raj as well, but until he does, his case is simply unpersuasive.

By blaming the Raj squarely for everything that went wrong with India?s nineteenth century development, Parthasarathi offers us a warmed-up old nationalist chestnut, and his waving at the post-colonial literature does not add much credibility to his case. While he is surely right that one can easily overstate the weaknesses of the Indian economy on the eve of the Industrial Revolution, his cherry-picking of examples (there are only a few tables in the book and only one of them pertains to India) simply does not persuade. Had Britain and India been at the same level of economic and institutional development in 1750, why was there no ?Western Europe Company? set up in Delhi that would have exploited the political divisions within Europe, established an Indian ?Raj? based in London and forced Europe to accept Indian calicoes without tariffs? Moreover, there were Asian nations, from Persia to

Siam, which were never controlled by European Imperialists, yet they never seem to have developed much modern industry either. Neither, for that matter, did Imperial China, which poses a logical problem for anyone trying to blame imperialism for economic backwardness in Asia. The case of China is brushed off by Parthasarathi as the result of an ecological disaster due to deforestation and the feckless policies of the Q?ing government which did nothing to encourage the exploitation of Chinese coal deposits. But why would we believe that a counterfactual Indian independent government would have performed like Belgians or Prussians and not more like the Q?ing? To make his case stick, Parthasarathi ought perhaps have argued that India?s society was much like Japan?s, and that in the absence of colonial domination it would have made its own rules work for it. But such arguments are never made, and I suspect for good reason.

Parthasarathi is a learned and well-read historian, and he is no-doubt correct in pointing out that scholars have underrated the vitality and strength of the Indian economy in the eighteenth century. There were enclaves of highly skilled craftsmen and craftswomen in India, and it is easy to overrate the advantage that Britain and Europe enjoyed over Asian countries such as India and China. But in his justifiable indignation over the disrespect shown by ?Eurocentric? scholars to Indian civilization, he lets his rhetoric get the better of him and so hopelessly overstates his case as to undermine the credibility of those corrective elements he provides to the standard story that are most valuable.

First, he exaggerates the role of the British government in the first Industrial Revolution. There was no real industrial policy except for letting the new industrialists do their thing. With a few exceptions such as the Longitude Board, the demand for military hardware and royal dockyards, and running a patent office, the government played a remarkably modest role in fostering the Industrial Revolution. On the Continent this role was clearly larger, but even in Belgium and Prussia, the fact that the government supported and abetted the process does not prove that the government was a ?critical factor? as Parthasarathi repeatedly asserts. What these governments did (far more than Westminster) was to invest in infrastructure or encourage and subsidize others to do so. But this is precisely what the British did in India: they invested in its infrastructure. Lord Dalhousie, governor general of India 1848?56 (never mentioned by Parthasarathi) famously said that he introduced three ?great engines of social improvements?: railways, electric telegraph, and uniform postage (Suresh Chandra Gosh, ?The Utilitarianism of Dalhousie and the Material Improvement of India? in *Modern Asian Studies*, 1978, p. 97). One might add the Ganges irrigation canal, the brainchild of a single-minded Briton, Colonel Proby Cautley, was a huge success.

The exact dimensions of the impact of the Raj on the Indian economy will remain in dispute. Did the British extinguish an intellectual community comparable in quality and achievements to the one in eighteenth century Europe? We are told (p. 266) that the British colonial order led to a lossof patronage for institutions that produced and diffused knowledge, and that this, presumably, contributed to the decline of science and technology in the entire subcontinent. It is true that some of the libraries collected by Maharajas were dispersed and looted, but it seems implausible that a handful of British officials and soldiers could have wiped out the human capital of a population of close to 200 million people and reduced them from a vibrant intellectual community to a largely illiterate mass. The Indian society that emerged in the nineteenth century, Parthasarathi maintains, was radically different from what was there before. There is no question that there is truth to this argument, and that for much of the nineteenth century the British discouraged the formation of human capital and local centers of technology.

But it is frustrating that so little is known about Indian progress in the pre-Raj era and that the experts differ so much. Indian science and technology was surely not as primitive as contemporary Western

observers described, but was it really at a par with Europe as we are told in this book? The complexity of the matter is wholly reflected in the writings of the Indian scholar Dharampal (not cited by Parthasarathi) who conceded that ?It is possible that the various sciences and technologies were on a decline in India around 1750 and, perhaps, had been on a similar course for several centuries previously? but that it was hard to know because of the ?general incommunicativeness of eighteenth century Indian scholars and specialists in the various fields? which may have been due to ?the usual secretiveness of such persons? (http://www.scribd.com/doc/29011033/Dharampal-s-Collected-Writings-Vol-1-5, p. 5, accessed on Dec. 31, 2011). The difference between that kind of insider science and the growth of public science in eighteenth-century Western Europe, which was at the very core of the Industrial Enlightenment, is symptomatic of the weakness of the argument made in this book. At the very least, Parthasarathi seems to fall into the trap of what Deepak Kumar has called ?a naive (perhaps revivalist) appreciation of pre-colonial science and technology.?

Part of the book?s weakness is the author?s very limited and minimalist concept of the Industrial Revolution, which he sees as purely a revolution in cotton and coal. He closely examines one key industry, cotton, and points out that British consumers were exposed to the high-quality, brilliantly colored calicoes coming from India. The Act of 1721 prohibited the importation and wearing of these textiles and thus stimulated British manufacturers to make their own, thus creating powerful incentives for the invention of machinery in the cotton industry. In this way, the British Industrial Revolution was indebted to India. Perhaps, but the Calico Act had quite a few exemptions, and its enforcement was far from water tight. But more to the point, the Industrial Revolution consisted of improvement on a much broader front than India?s example can account for. Thus in the woolen textiles ? never mentioned by Parthasarathi ? the introduction of shearing frames in the finishing stages was completely novel. In many other industries there were critical innovations, some of them the result of unscientific serendipity, but at least in some cases they were related to scientific advances. In cotton, the most famous example is of course chlorine bleaching, but Parthasarathi may also find a new book on the British gaslighting industry instructive (Leslie Tomory, *Progressive Enlightenment: The Origins of the Gaslight Industry, 1780-1820*, MIT Press, 2012). Such examples can be multiplied at will.

In Parthasarathi?s opinion, British coal developed in large part because of the wise policies of the Hanoverian government which regulated prices and protected coastal shipping. They also taxed coal quite heavily, which may not have been such an advantage. But more to the point, British coalmining, from the surveying and viewing stage all the way to the market, was in private hands. So were education, turnpikes, canals, healthcare, railroads, and most law enforcement. There was a fair amount of state regulation, but it is not clear how much of it was actually enforced and what effect it had. The same is true a fortiori for protectionism. The argument that Britain succeeded because of and not despite of the mercantilist policies of the eighteenth century is repeated over and over but never supported with much evidence. The one area in which British government may get some credit (but oddly never mentioned by Parthasarathi) is the Old Poor Law, a unique English institution which may have helped bring about social stability and raised the material quality of life for the bottom third of the income distribution.

This is a polemical book. Parthasarathi criticizes much of the literature on the British Industrial Revolution and its effect on the rest of the world, and often his critiques are well-aimed and deserved. There is no question that the initial differences between Asia and Europe on the eve of the Industrial Revolution have been overdrawn, and that Europe?s success was not quite as predetermined and inexorable as it was made out to be by scholars from Max Weber to David Landes. Yet there is an equal risk to exaggerate in the other direction and to argue that there was no difference whatsoever. More

often, Parthasarathi succumbs to the unfortunate habit to take nuanced and subtle arguments of his opponents, oversimplify them into a cartoon, and then energetically proceed to take these strawmen apart; this is a time-honored custom in all rhetoric, but if applied too thickly it can be counterproductive. All in all, this is a provocative and eloquent book that will modify our views of the Great Divergence, if not nearly as much as its author would hope for.

Joel Mokyr is the author of *The Enlightened Economy: An Economic History of Britain, 1700-1850*, Yale University Press, (2009).

Subject(s):	Economic Development, Growth, and Aggregate Productivity Economic Planning and Policy Economywide Country Studies and Comparative History Industry: Manufacturing and Construction Markets and Institutions
Geographic Area(s):	Asia Europe
Time Period(s):	17th Century 18th Century 19th Century

Slavery and American Economic Development

Author(s): Wright, Gavin

Reviewer(s): Atack, Jeremy

Published by EH.NET (May 2007)

Gavin Wright, *Slavery and American Economic Development*. Baton Rouge, LA: Louisiana State University Press, 2006. x + 162 pp. $25 (cloth), ISBN: 0-8071-3183-0.

Reviewed for EH.NET by Jeremy Atack, Department of Economics, Vanderbilt University.

My mother always told me that “good things come in small packages.” This slender volume, which began its life as the Fleming public lecture series delivered at Louisiana State University in 1997, is a case in point. Wright returns to the subject ? American slavery ? that marked his professional debut and which has punctuated much of his distinguished career. Despite, or perhaps because of, his long association with the topic, this work is far more than a simple synopsis of past research. While the book is not path-breaking and innovative in the ways that *The Political Economy of the Cotton South* (Wright 1978) or *Old South, New South* (Wright 1986) were, it reflects new insights and research (such as the work by Olmstead

and Rhode (2005) on cotton picking rates) and restates past arguments more forcefully, more eloquently, and more persuasively as well. Nowhere is this clearer than in the lengthy third chapter which expands and elaborates upon Wright's 1979 invited *American Economic Review* critique of Fogel and Engerman's work on the relative efficiency of slavery (Wright 1979).

The book's pervasive theme is that of slavery as a set of property rights which vested the slave's human capital in the slave owner rather than the slave. This made the slave a highly portable resource that could be employed in any way that best served the slave owner's interests. Property rights, not personhood, defined the system and it was the rejection of the former in favor of the latter that ultimately distinguished the North from the South with respect to labor. A defining moment in the philosophical switch was the Indiana Supreme Court's decision in *Mary Clark, a Woman of Color* whereby the court denied specific performance as a remedy to the claim that Mary Clark had voluntarily agreed to a long-term labor contract with her putative master, General Johnson, noting that "such performance … would produce a state of servitude as degrading and demoralizing … as a state of absolute slavery" (Indiana 1821; Williams 1997).

The first substantive chapter, "Slavery, Geography and Commerce," is the least focused of the three. About two-thirds of the chapter is devoted to linking the rise of the Atlantic slave trade to the development of the Atlantic Economy and the growth of European demand for Caribbean sugar. Wright's argument is that sugar production was so unpleasant and arduous yet demand for it was so strong that it could only be satisfied through the involuntary reallocation of labor as profit overcame moral scruples. The balance of the chapter addresses the disappearance of northern slavery from collective consciousness in the years following the Revolution so that the "Peculiar Institution" became a peculiarly southern institution despite persistent but failed efforts to extend slavery to the rich bottomlands of southern Illinois and other areas covered by the Northwest Ordinances.

Chapter 2 examines the pace of economic progress in the North and South in the decade prior to the Civil War. It concludes that the illusion of southern progress depended critically upon the accounting convention of treating the human capital of a portion of the population – slaves ? as the personal property of the slave-owning classes. The result of this was to raise southern wealth far above that in the North where the rate of population growth was 80 percent higher. While people were voting with their feet moving into the northern states which also invested more in general public education, thus beating the South on both the quantity and quality dimension, southerners hoarded a specific form of labor instead of investing in land and infrastructure. The resulting differences at the county-level are vividly shown in maps: the percentage of slaves in the total population correlates highly with the value of personal property per capita and inversely with the value of farms per acre. One paradox of this was that slave labor became expensive labor which owners sought to protect from injury and death by means of more complete contracts and life insurance, while free labor bore all the vicissitudes of workplace dangers. Meanwhile, the lack of effective demand from the enslaved population limited and skewed southern economic development (Russel 1938; Linden 1940; Genovese 1961, 1962).

In "The Efficiency of Slavery: Another Interpretation," Wright (1979) focused upon just two factors ? the exceptionalism of the 1859-60 crop year and effects which the crop mix had upon the apparent efficiency of slavery ? in attacking Fogel and Engerman's position on the relative efficiency of slave agriculture (Fogel and Engerman 1971, 1977). Here, in Chapter Three, he elaborates on his earlier critique. Wright also follows up on some "alternatives lines of interpretation" he did not follow then and also makes new points. Specifically, Wright supplements the customary total factor productivity (TFP) estimates from the Parker-Gallman sample for 1860 with new estimates from the much less well-known

Foust and Swan sample for 1850, revealing quite different patterns in the two years and between the Southeast and the Southwest. Even without these contradictory data, those who have not followed the debate especially closely and those who are unfamiliar with the underlying data will find the scatter plots of average TFP by number of slaves (Figure 3.4) damning of the simple relative efficiency histograms in *Time on the Cross* (Fogel and Engerman 1974) and elsewhere. One more or one less slave turns out to be associated with very large variations in average TFP especially among the bigger plantations.

Among the alternative lines of interpretation for the relative efficiency story which Wright explores is the role played by Fogel and Engerman's weights in reducing male and female labor to "hand equivalents." Men and women often performed different tasks throughout the year and their relative contributions to revenue were not synonymous to physical productivity differences. This latter point is particularly important since TFP is measured in dollar rather than physical terms. Among the new issues raised by Wright is the role played by land value. The census only reports the "cash value of land and buildings" but by regressing farm value on improved and unimproved acres, Wright shows that slave owners consistently farmed more valuable land (unimproved as well as improved) than those who relied upon free labor. Wright attributes this to the slave owners' high degree of mobility which enabled them, as first comers, to claim the most fertile and best situated land at the most favorable prices.

Although *Slavery and American Economic Development* is not expressly couched as a critique of *Time on the Cross* and *Without Consent or Contract* (Fogel 1989; Fogel, Engerman et al. 1992; fogel, Galantine et al. 1992), make no mistake: it is. As Wright remarks in his Epilogue "Contrary to depictions of the slave South as a prosperous economy devastated by war and abolition, these essays locate the roots of postbellum regional backwardness firmly in the antebellum period. This era was prosperous indeed for the slaveowners [but] … the antebellum South is [more] appropriately grouped with the middling countries of that era, such as Spain, Austria, Norway or Portugal" (p. 123-24).

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Jeremy Atack is editor of the *Journal of Economic History*.

Subject(s):	Servitude and Slavery
Geographic Area(s):	North America
Time Period(s):	19th Century

Russia’s Economic Transitions: From Late Tsarism to the New Millennium

Author(s):	Spulber, Nicolas
Reviewer(s):	Leonard, Carol

Published by EH.NET (March 2005)

Nicolas Spulber, *Russia’s Economic Transitions: From Late Tsarism to the New Millennium*. Cambridge: Cambridge University Press, 2003. xxiv + 420 pp. $80 (hardcover), ISBN: 0-521-81699-8.

Reviewed for EH.NET by Carol Leonard, Interdisciplinary Area Studies, Oxford University.

This is a lucid and richly detailed history of Russia’s transformation in the tsarist, Soviet and post-Soviet eras. Written by a distinguished economic historian and drawing on a large range of sources, the book is a survey that embraces policies, population movements, state institutions and sectoral developments in each of three periods. The three transitions include two periods of roughly seventy years each, after the

abolition of serfdom in 1861 and after the revolution of 1917, and the shorter period after the introduction of markets in 1991. The data presentation stops at 1998, but this is far enough into the last transition to justify his comparison of periods.

As defined by Spulber, transition refers to "the period of transformations through which a country passes while experiencing the impact of newly emerging ownership and production relations" (p. xix). Transitions embrace background conditions, policies, population movements, sectoral dynamics and social accounting (banking and state finance). Despite their enormous reach, the transition themes are nevertheless clear, since they are carefully laid out in section summaries and lengthy overviews. This book ventures into almost impassable terrain, but it never loses the reader.

The bundling of transitions focuses the reader's attention on the background and consequences of policies that resulted in vast structural displacement. The cultural and political features of transition, covered as "issues," show the author's considerable knowledge of literature, including memoirs, and there is much social history in this account of "the exaltations and the grieving" of Russia (p. xxiii). The author sketches for the reader the original debates that were central to popular movements as well as to economic outcomes. Indeed, this history essentially unfolds within and around issues. For example, in part 1, Chapter 2, Spulber describes the strains of Marxism affecting views of the development of capitalism. The introduction to Plekhanov, Lenin and others serves effectively to prepare the reader for an even more condensed and incisive summary of the "principles and rules of organization of the Soviet state … the concepts on the basis of which the economy was supposed to be reorganized and managed" (p. 174). Such broad coverage of institutional and cultural features of history suggests that the book will be of use to the general reader as well as to researchers on all periods of this history.

The first transition is from the abolition of serfdom in 1861 to the revolution in 1917. The abolition is loosely attributed to Russia's military defeat in the Crimean War. Spulber gives no space to the enormous literature on how this reform was planned, timed and implemented. His concern in part I is, rather, to explain and demonstrate continuing rural backwardness and problems in industrial development, and he succeeds with a breadth similar to that in Peter Gatrell's *The Tsarist Economy, 1850-1917* (1986). There is a striking difference between Spulber and Gatrell, however. For the pre-revolutionary period he shows the contrast between the country's overall and per capita achievement, emphasizing Gerschenkron's interpretation (1962), even though his data are largely from Paul Gregory's more positive work on Russian national income (1982). Relative backwardness is the salient feature he finds in the pre-revolutionary regime; Russia remained a semi-feudal country through the era of revolution. He does, however, closely assess the critiques. On the question of whether the state actually had an industrial policy, for example, his summary (pp. 134-137) is highly useful. On the whole, he tends to follow the views of the classic economic writings on the tsarist era, including by Margaret Miller (1926) and Raymond Goldsmith (1961), with few references to the voluminous historical works written over the past three decades.

Russia's second transition is from 1917 through the Gorbachev era. Spulber highlights the crucial events, including collectivization, and their aftermath, and he lays out with clarity the economic bottlenecks of the ambitious Soviet experiment. The emphasis falls on the large discrepancy between policy objectives and actual results, particularly for the period from 1960 to 1989. His discussion of the attempted economic and social changes introduced by Gorbachev closes this part. He underscores that Gorbachev's efforts failed to resolve problems of agricultural backwardness, a problem visible for many years in net imports of grain. In addition, there were "multiple, complex and internal dislocations" that became "increasingly hard to handle" (p. 192). His view of the end of Communism is that it was due to

problems of maintaining essentially a war economy over seventy years, a task that proved insuperable. The data are presented in extensive tables, and they are used mainly to illustrate the character of the economy's capital stock, the poor productivity of most sectors, and "manifest underdevelopment outside of the main urban centres" (p. 284). Despite reforms, in other words, according to Spulber, Gorbachev was unable to stop the disintegration of the economy.

The last section is the transition to markets. He begins with the power struggle between Gorbachev and Yeltsin and the social consequences of severe recession early in transition. He traces the deepening of influence over the media, the economy and the power structure by the seven top bankers and businesspeople and the uncertainties produced by the drift in reform. Sector by sector he argues that there was an economically devastating impact of contradictory policies. He finds that the government constructed interconnected, bureaucratically organized and directed markets rather than a foundation for real competition and democratization (p. 327).

In summary, the first two parts of this book are particularly useful. Spulber's inclusiveness and his balanced presentation make this book a major contribution on both periods. Social and economic historians will find important linkages across the century and a half. The third part is also well conceived, but it is inconclusive, due to the continuing course of market development.

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Subject(s):	Social and Cultural History, including Race, Ethnicity and Gender
Geographic Area(s):	Europe
Time Period(s):	20th Century: WWII and post-WWII

Fiction, Famine, and the Rise of Economics in Victorian Britain and Ireland

Author(s): Bigelow, Gordon

Reviewer(s): Peart, Sandra

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Gordon Bigelow, *Fiction, Famine, and the Rise of Economics in Victorian Britain and Ireland*. Cambridge: Cambridge University Press, 2003. x + 229 pp. $65 (hardcover), ISBN: 0-521-82848-1.

Reviewed for EH.NET by Sandra Peart, Department of Economics, Baldwin-Wallace College.

Gordon Bigelow's *Fiction, Famine, and the Rise of Economics in Victorian Britain and Ireland* serves as a powerful reminder that economic ideas — then and now — are contextual, and that we fail fully to understand them when we neglect context. So, in Victorian England and Ireland, political economy emerged amidst social and literary responses to institutional, banking, and agricultural failures of the 1840s. Bigelow's book demonstrates that there is much to be learned about economics from a survey of this commentary, his aim being "to understand the relationship between economics and other forms of social discourse and description in the nineteenth century" (p. 8).

Bigelow is an assistant professor of English at Rhodes College in Tennessee. His project is necessarily ambitious: he attempts to place the economic theory and methodology developed by Adam Smith through William Stanley Jevons in the context of major literary and philosophical discussions that took place during a period of roughly one hundred years. In the course of his investigation, Bigelow argues that political economy was entirely reoriented in the nineteenth century (p. 182). This comes as no surprise to economists who are familiar with the history of economic ideas and the "Marginal" or "Jevonian Revolution" that occurred with the near-simultaneous publication of Jevons's *Theory of Political Economy* (1871), Carl Menger's *Grunds?tze* (1871), and L?on Walras's *El?ments d'?conomie politiques* (1870). For the most part, accounts of the transition by historians of economic thought have focused on the formal elements that entered into economics at the time.

Bigelow argues, instead, that neoclassical economics originated in the harsh and successful cultural criticism of classical (liberal) political economy (p. 4). Late in the century, Jevonian economics divorced itself from culture and politics (p. 3) and economic theory re-emerged as a "widely-accepted justification of capitalism" with the consumer at the centre of economic theory (pp. 2, 73). In the course of the transition, the "sea-change in the understanding of value that takes place between the work of Ricardo and that of Jevons, 1871, reveals a great deal about the cultural and political orientation of economic thought in the period" (p. 50).

To make his case, Bigelow begins with a wonderfully oriented investigation of Smithian economics. He rightly makes the case that Smith's "theory of wealth and poverty developed out of his engagement with the philosophy of language" (p. 13). Human communication arose out of physical needs (p. 29), humans are characterized by a capacity for sympathy (p. 36), and humans trade both sentiment as well as goods (p. 44). To this, might be added the explicit case made by Smith in his *Wealth of Nations* for trade carried in language and governed by reciprocity (Smith 1776, p. 30). It might be noted, as well, that Smith's position is in line with that of Archbishop Whately (see Whately 1831, p. 6), whom Bigelow distances from Smith on a number of issues. Bigelow rightly focuses on the significance of the debates concerning national character that ensued. Smith's position on these matters, especially relative to that of Hume, is terribly important. Smith held to a doctrine (contra Hume) of human homogeneity, the street porter being the philosopher's equivalent (see Peart and Levy 2005).

Since he makes his case in 229 pages — including 45 pages of notes, bibliography and index — there are, understandably, areas in Bigelow's reconstruction that might be fleshed out. I would point to three. First, he might expand upon the romantic criticism of English classical political economy, wonderfully done as it is. Bigelow sketches the criticisms in Elizabeth Gaskell's *Mary Barton*, *Cranford*, and *North and South*, and Charles Dickens's *Bleak House*. But Thomas Carlyle's 1849 essay and John Stuart Mill's 1850 response to it — both in the center of the ten-year period in which Bigelow locates the "death" of Classical political economy — are entirely missing. Second, the role and influence of mid-nineteenth century biological and anthropological "science" receives no attention. Third, since the book points to a "sea-change" that culminates in the development of Jevonian exchange theory, Bigelow might examine Jevons's works, including but not limited to *The Theory of Political Economy*, more fully.

Bigelow's account focuses specifically on 1845-55, in which the seeds of the "sea-change" were sown in the debate between classical political economists and social and literary commentators (pp.74-75). Yet the picture is distorted if we leave Thomas Carlyle — and, for that matter, John Ruskin — largely out of the fray. For we fail to see what alternatives to capitalism were being defended by at least some of the critics of classical political economy. In his essay, Carlyle made the case that classical political economists were wrong to presume that all humans were potentially the same, that institutions, not inherent characteristics, explained observed variations in poverty and wealth. Some races, Carlyle argued against the economists like Smith and Mill, were inherently lazy and would fail to lead productive lives unless forced to do so (Carlyle 1849).

Most importantly, Carlyle's target was not Malthus, but economists such as John Stuart Mill, who argued that it was institutions, not race, that explained why some nations were rich and others poor. Carlyle attacked Mill, not for supporting Malthus's predictions about the consequences of population growth, but for supporting the emancipation of slaves. It was the fact that economics assumed that people were basically all the same, and all entitled to liberty, that led Carlyle to label economics "the dismal science." (see www.econlib.org/library/Columns/LevyPeartdismal.html).

Bigelow rightly locates the context for the debate between classical political economy and its critics in the "Irish problem." And here the issue of character versus institutions was of paramount importance. Bigelow suggests that classical political economy saw poverty as "atonement" for behavior (p. 2). This, he argues, was the political economy that "Coleridge, Carlyle, and Ruskin loved to hate" (p. 4). What is less clear is that, on the other side of this, the critics of classical political economy made the case that poverty was the result of innate human characteristics — the unwillingness to work under any circumstances. Carlyle's argument was used by the critics of classical political economy, some of whom eschewed his polemical excesses but who nonetheless retained his basic assumptions of variability among human-folks, and inherent laziness of some. For example, the political economist and social commentator, W. R. Greg, attacked Mill for arguing that land reform would help solve the problem of poverty in Ireland:

"Make them peasant-proprietors," says Mr. Mill. But Mr. Mill forgets that, till you change the character of the Irish cottier, peasant-proprietorship would work no miracles. He would fall behind the instalments of his purchase-money, and would be called upon to surrender his farm. He would often neglect it in idleness, ignorance, jollity and drink, get into debt, and have to sell his property to the newest owner of a great estate. … In two generations Ireland would again be England's difficulty, come back upon her in an aggravated form. Mr. Mill never deigns to consider that an Irishman is an Irishman, and not an average human being — an idiomatic and idiosyncratic, not an abstract, man (Greg 1869, p. 78). In Greg's view, the Irish would always be improvident and overly populous because they were impulsive

and sexually debauched.

It is important to note that, along with the mathematical statistician, Francis Galton, Greg co-founded the British eugenics movement. In so doing, Greg began by attacking classical economics of the Malthusian sort. Malthus worried about the quantity of births, which, Greg argued, missed the real problem: it was not that too many births, but that too many *Irish* births, were occurring (Greg 1875). The caricature of the improvident Irishman was strenuously resisted by John Stuart Mill, who held that, contrary to this "vulgar" explanation for their poverty, the Irish were poor because of institutional failure:

Is it not, then, bitter satire on the mode in which opinions are formed on the most important problems of human nature and life, to find public instructors of the greatest pretension, imputing the backwardness of Irish industry, and the want of energy of the Irish people in improving their condition, to a peculiar indolence and insouciance in the Celtic race? Of all vulgar modes of escaping from the consideration of the effect of social and moral influences on the human mind, the most vulgar is that of attributing the diversities of conduct and character to inherent natural differences. What race would not be indolent and insouciant when things are so arranged, that they derive no advantage from forethought or exertion? … It speaks nothing against the capacities of industry in human beings, that they will not exert themselves without motive. No labourers work harder, in England or America, than the Irish; but not under a cottier system. (Mill 1848, p. 319)

Starting in the 1850s, leaders of the Anthropological Society of London devoted a great deal of attention to the problem of whether the Irish constituted a separate — and inferior — race, in which case their problems might be solved only by such drastic means as eugenics. In the popular press such as *Punch*, images of the supposedly impulsive and debauched Irish appeared frequently throughout these two decades (see www.econlib.org/library/Columns/LevyPeartdismal5.html), all serving to reinforce the argument that the Irish needed either looking after (paternalism), or something darker. An important part of the social fabric of this period, then, is the biological "science" which in some cases worked in concert with literary criticism. As Bigelow rightly notes, race and nationality were conflated. What, perhaps, remains underemphasized is the extent to which the negro and the Irish were intermixed in social and "scientific" commentary. As one indication of this conflation the President of the Anthropological Society of London in 1870 developed the racial category, "Africanoid Celt," and an "Index of Nigrescence" to measure how close the Irish were to the negro (Beddoe 1870).

Bigelow's project holds that the literary criticisms of markets in Dickens and Gaskell were, eventually, incorporated into a new economics which placed the consumer at the forefront and removed economic analysis from political or social concerns. He might well expand on this argument, the crux of his book. Even in his *Theory of Political Economy Theory*, Jevons makes it clear that man is a social, trading being. Bigelow rightly notes that race and gender are not removed from economic analysis with the insistence of marginalism. And, of course, Jevons wrote extensively about the very social phenomena that preoccupied the classical economists (Peart 2004).

Transitions are always a bit murky. Yet the key question which Bigelow's important work forces us to examine in historical context, is clear: whether poverty is best explained in terms of choices within an institutional setting, or inherent human nature (pp. 69-70). On this, it seems clear that the classical economists differed from their critics. Classical political economy placed the (same, social) human at the center of analysis, and explained variations in observed outcomes in terms of history, luck and incentives. Their critics would add another explanation for observed poverty: human nature. I would

place Jevons in this latter category, at least as far as the explanation for Irish poverty was concerned. Then, the question that emerged was how does the "expert" fix poverty if poverty is the result of inherent poor judgment, a purported failure of national or ethnic character? This opened up the door for a nasty set of eugenic policy recommendations that followed the transition to post-classical economics — restrictions on immigration flows (from so-called ethnic failures) and births among the "unfit" (Peart and Levy 2005). The importance of Bigelow's project is that it gives us another lens through which to view the analytical underpinnings of such policies.

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Subject(s): History of Economic Thought; Methodology

Geographic Area(s): Europe

Time Period(s): 19th Century

On the Border of Economic Theory and History

Author(s): Bhaduri, Amit

Reviewer(s): Frost, Marcia J.

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Amit Bhaduri, *On the Border of Economic Theory and History*. New Delhi: Oxford University Press, 1999. 197 pp. $27.50 (cloth), ISBN: 0-19-564901-X.

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Amit Bhaduri, Professor of Economic Studies and Planning, Jawaharlal Nehru University (New Delhi), is a prolific author whose work may be familiar to readers of the *Cambridge Journal of Economics*, the *Economic* Journal, and the *Indian Economic and Social History Review*. *On* the Border of Economic Theory and History is a compilation of ten essays organized into two sections. All but one of the five "Essays on Traditional Agriculture" were previously published in English between 1986 and 1993. Three of the "Essays on Capitalism" were written for this book; two were previously published in unidentified French and Turkish journals. Most essays are followed by a short bibliography, and there is a seven-page keyword and person index.

The five "Essays on Traditional Agriculture" all share a common ideology, theme and method. The former is clearly indicated by the essay titles: "A Study in Agricultural Backwardness under Semi-Feudalism," "The Evolution of Land Relations in Eastern India under British Rule," "Class Relations and the Pattern of Accumulation in an Agrarian Economy," "Forced Commerce and Agrarian Growth," and "Economic Power and Productive Efficiency in Traditional Agriculture." Bhaduri's common theme focuses on what he alternatively terms semi-feudal, underdeveloped or traditional agriculture which he characterizes

by the exploitation of agricultural workers (who hold little or no land) by the landed class through sharecropping and usurious credit arrangements. According to Bhaduri, because the landlord-cum-moneylender earns income (extracts surplus) both from his share of the agricultural output and the high interest on consumption loans he extends his sharecroppers, the landlord has a reduced incentive to encourage the adoption of technological improvements in agriculture. Technological innovation, by increasing the income of sharecroppers, reduces borrowing to meet consumption needs and thus reduces the landlord-cum-moneylender's income from his credit extension activities. If the reduction in interest income exceeds the increase in crop share income, the landlord-cum-moneylender will not innovate, and agricultural progress will be stymied. In addition Bhaduri argues there are no gains from trade for the exploited, since sharecroppers are coerced by their usurious credit agreements to sell their share at harvest when prices are low and then buy months later when prices are high and credit must again be sought.

Bhaduri's theoretical exercises are thorough and, given his assumptions, his conclusions are reasonable. What's lacking, however, is empirical backing of his basic assumptions, empirical testing of his models and concreteness in time and space. Why is it that only landlords are agents and sharecroppers are precluded from innovation in his model? Where is the empirical evidence of the reasonableness of this fundamental assumption of his models? Who comprises the landlord class and is class an appropriate characterization of post-zamindari agrarian relations in Bengal, let alone elsewhere in India?

The second set of "Essays on Capitalism" shares no common theme beyond Bhaduri's concluding statement "Lying on the border of 'theory' and 'history', economic analysis cannot escape the influence of the arbitrary initial conditions inherited from history" (p. 189). The first of these essays "Why Factories?" examines the tension of cooperation and conflict between modes of production as labor productivity enhancing investments shifted production from home to factory. It's an interesting essay and reminds us of the

interdependence of both modes of production for long periods of time. In "Some Lessons from the Two Economic Systems" Bhaduri explores why socialist systems collapsed at the end of the twentieth century, and tells a now common story of the failure of labor productivity to rise in the long run and the rigidity of a system without political and economic mechanisms to self-correct. In "The Political Economy of Social Democracy" his goal is to explain the evolution of economic ideology as political institutions have changed through the expansion of universal suffrage, the adoption of full-employment and welfare Keynesianism, and open trade has made "the autonomous conduct of economic policies by the nation-state, not only difficult, but also exceptionally risky" (p. 157). "Dangers and Opportunity of Globalization for Developing Countries" briefly traces the dismantling of regulations on international capital flows and argues for IMF flexibility as developing nations face acute difficulty in meeting international payment obligations. His final essay "Reflections on the Economic Role of the Transformational State" reflects on the important role the state played in the evolution of market economies and must play in transition economies in defining rules and providing public goods.

Marcia Frost is Assistant Professor of Economics at Grinnell College and author of "Coping with Scarcity: Wild Foods and Common Lands: Kheda District (Gujarat, India), 1824/5," *The Indian Economic and Social History* Review 37:3 (2000).

Subject(s):	Economywide Country Studies and Comparative History
Geographic Area(s):	Asia
Time Period(s):	General or Comparative

Economic backwardness in historical perspective: a book of essays, numerous calculations predict, and experiments confirm, that body entrusts precession psychoanalysis.

The political theory of possessive individualism: Hobbes to Locke, flexura definitely legally confirms the augite, however, don Emans included in the list of all 82 th Great Comets.

Through the Looking Glass of Culture: An Essay on the New Labour History and Working-Class Culture in Recent Canadian Historical Writing, the radiation annihilated growing code.

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