Summaries of Doctoral Dissertations

The Human Cost of War: White Population in the United States, 1850–1880

Ten years ago Maris Vinovskis published an influential essay that signaled a profound shift in Civil War scholarship.[1] Although thousands of books and articles had been written about the military experiences of Civil War participants, Vinovskis contended that relatively little was known about their actual lives. Vinovskis called explicit attention to the demographic cost of the war and its continuing influence on the life course of the Civil War generation. An estimated 618,222 military deaths occurred during the war—roughly equal to the number of deaths suffered in all other American wars through the Korean War combined. The human cost of the Civil War becomes even more spectacular when one considers the rate of death. Nearly one in eight white men of military age died during the war, exceeding the rate of death in World War II by a factor of six, and the rate of death in the Vietnam War by a factor of 65.[2]

During the last decade historians have answered Vinovskis's call for a social history of the Civil War. We now have several excellent monographs on women's response to the war, numerous studies of the northern and southern "home fronts," dozens of community studies that illuminate how race, class, and gender shaped the wartime experiences of ordinary Americans, and a growing number of studies on the wartime and Reconstruction experiences of African Americans. Few studies, however, have looked beyond the immediate stress of the war to examine its long-term consequences, and virtually no research has built upon Vinovskis's preliminary demographic speculations to examine the war's impact on postwar population. The lack of long-term study is most regrettable in the South, which lost an estimated one in four white men of military age—three times the rate of death in the North.

In this dissertation I rely on new national samples of the 1850, 1860, 1870, 1880, 1900, and 1910 United States censuses to investigate short- and long-term trends in the white population and, where possible, to estimate the war's impact on marriage and fertility. These samples—part of the Integrated Public Use Microdata Series (IPUMS)—include data on more than 1.65 million individuals living in 357,000 households.[3] The dissertation also relies on a preliminary longitudinal sample of Union Army recruits currently being constructed at the University of Chicago as well as more conventional qualitative sources.[4]

Despite their seeming precision, estimates of the number of military deaths in the Civil War are crude approximations. Estimates of the number of noncombat deaths that occurred in the Confederate forces, whose medical records did not survive the war, are based on the crude assumption that disease had an equal impact on Confederate and Union troops.[5] There are a number of reasons to question this assumption, including the growing shortage of food, clothing, and medicine in the Confederate Army in the last few years of the war, and the lack of voluntary organizations in the South comparable to the U.S. Sanitary Commission and Western Sanitary, which helped improve the sanitary conditions of Union Army camps. Perhaps most importantly, the South's relatively lower urbanization and population

[1] This dissertation was completed in September 1999 in the Department of History at the University of Minnesota under the direction of Russell R. Menard and Steven Ruggles. Financial support was provided by a fellowship from the University of Minnesota Graduate School.

[2] Vinovskis, "Have Social Historians."

[3] Ruggles, Sobek, et al., *Integrated Public Use Microdata Series: Version 2.0*

[4] Fogel, "New Sources."

[5] Reasonably accurate estimates of the number of men killed in action have been made for both the Union and Confederate armies. Estimates of the number of noncombat deaths, especially for deaths that occurred in camp or in regimental hospitals, are also fairly reliable for the Union Army, although the figures do not reflect the number of men who died shortly after receiving a medical furlough. Livermore, *Number*.

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density increased the probability that a typical Confederate soldier entered the war without acquired immunities to infectious disease. Consequently, Confederate soldiers may have suffered disproportionately from disease mortality during the war.

Chapter 2 revisits estimates of Civil War disease mortality through a case study of its leading killers: diarrhea and dysentery. Kaplan-Meier survival analysis of data from the Union Army sample reveals that prior exposure to disease (as measured by the proxy of farm/nonfarm occupation before enlistment) proved to be a significant determinant of mortality from nonspecific diarrhea. Recruits with nonfarm occupations before the war enjoyed a 60 percent lower risk of death from "diarrhea" and "dysentery" than recruits with farm occupations over the course of a typical 36-month enlistment. Using these data and anecdotal evidence from other studies, the chapter suggests that estimates of disease mortality from nonspecific diarrhea in the Confederate Army are approximately 10 to 20 percent too low.[6]

Chapter 3 relies on the IPUMS census samples to examine the war's impact on trends and differentials in the timing and incidence of white marriage. It focuses on the potential existence of a "marriage squeeze" on a generation of white women after the war. Although social historians have hypothesized that the relative lack of marriageable men in the postbellum era led to a more competitive marriage market for women, especially in the South, the impact of the war on marriage has yet to be quantified. The nineteenth-century census samples pose some significant methodological problems for the study of nuptiality, however. Although the 1880 census was the first census to record individuals' martial status, it did not record the duration of their current marital status or the number of times they had been married. As a result, only the timing and incidence of individuals' first marriage at the time of the census can be estimated; short-term trends in first marriage and short- and long-term trends in remarriage are impossible to discern. More critically, the 1850-1870 censuses did not record individuals' marital status. Fortunately, the censuses contain enough information—surname, sex, age, and position in household—to reliably infer marital status.[7]

Estimates of the median age at first marriage by census year, census division, and sex for native-born whites suggest that the war's impact on marriage was moderate. Overall, the median age at first marriage for both men and women was somewhat lower in 1850 than in 1880, confirming indirect evidence of declining nuptiality in the nineteenth century. In the short term, the results for the southern census divisions seem to confirm the expected impact of the war: the median age at first marriage for southern white males fell 0.4 years between 1860 and 1870 while the median age for southern white females rose 0.5 years. Over the same period, there was little overall discernible change in the marriage age in the North. From a longer-term perspective, however, the evidence of a war-related marriage squeeze on southern white women is more ambiguous. The median age at first marriage for both southern white males and southern white females in 1850 is almost identical to the median age at marriage in 1870. In addition, large east/west differentials in the timing and incidence of marriage, the continuing increase in the median age at first marriage after 1870, and the long-term decline in the proportion of the population that eventually married

[6] Disease-mortality differentials between the two armies from acute infectious diseases such as smallpox and measles may have been even higher. Chulhee Lee found large differentials in acute infectious disease in his study of Ohio recruits in an earlier version of the Union Army recruit dataset. Lee, "Socioeconomic Background."

[7] I relied on imputed relationships included in the 1850–1870 public use samples to construct an "ever-married" variable. Men and women with imputed spouses or children present in the household were considered "ever-married" and those without apparent spouses or children present were considered "never-married." When applied to the 1880 census the imputation proved accurate 97 percent of the time. Although the inferred ever-married variable does not distinguish between married, divorced, widowed, or separated men and women—all are considered "ever married"—tabulating the proportion ever-married by single years of age allows the timing and incidence of first marriage to be calculated.

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suggest that economic factors, such as the differentials and trends in the price of farmland, played a more significant role in determining nuptiality than demographic factors.[8]

Chapter 4 indicates that the war had a dramatic, if short-term, impact on fertility. I construct new estimates of the white birth rate in the United States using back-projection techniques outlined by Ansley Coale and Melvin Zelnik and new estimates of nineteenth-century fertility recently published by Michael Haines.[9] Sectional estimates of white births indicates that the war resulted in a deficit of approximately 1.2 million white births between 1861 and 1870—approximately twice the number of men killed in the conflict. Although the fertility deficit was smaller in absolute numbers in the South than in the North, the percentage of the expected number of births was much larger in the South. During the years 1862–1866 the South suffered an average annual fertility deficit of 21.0 percent, almost twice the North's 10.9 percent deficit. The difference probably reflects the higher proportion of southern men who participated in the conflict than northern men, and the greater economic stresses and uncertainties in the Confederate South.

Chapter 4 also estimates underenumeration errors in the 1850, 1860, 1870, and 1880 censuses. These results indicate that net census underenumeration ranged between 6 and 9 percent in the four censuses, although some groups, such as infants and women over age 30, were far more likely to undercounted than other groups. Regional estimates of underenumeration confirm the long-held belief that the 1870 census undercounted parts of the U.S. South relative to other regions. The level of underenumeration, however, is far less than contemporaries believed. Indeed, the new estimates of underenumeration in the 1870 South suggests that previous adjustments used to "correct" for the perceived level of southern undercounts resulted in a larger error than the original count.

Chapters 5 and 6 shift the focus of the dissertation from the short-term impact of the Civil War on the white population to the study of long-term trends and determinants of fertility. Using own-child methods of fertility estimation, Chapter 5 estimates trends and differentials in total fertility by census division and various population subgroups. Marital fertility rates and various indices of marital fertility are also constructed to investigate the extent of conscious family limitation by census division and year. Despite the long-term decline in U.S. birth rates after 1800, I find no clear evidence that white couples in 1850 were able to effectively curtail childbearing after reaching a desired number of children. By 1860, however, couples in the New England and the Mid-Atlantic census divisions were practicing effective "stopping" behavior, and by 1870, couples in the East-North Central division were consciously limiting their pregnancies as well. There is no clear evidence that southern white couples between 1850 and 1880 practiced effective parity-dependent fertility control.

Finally, Chapter 6 investigates correlates of marital fertility among native-born women of native-parentage, the first group to effectively limit their births. Most researchers have stressed the economic determinants of fertility in the nineteenth century. Without denying the importance of economic factors, the chapter focuses on possible cultural determinants of fertility, specifically the relationship between religion and birth rates within marriage. Although no direct assessment is available of parents' religious affiliation or religiosity, indirect measures prove to be significantly correlated with the marital fertility. The proportion of own children with biblical names—believed to be a rough proxy of parental religiosity—is positively associated with marital fertility. County-level census data of church seating capacities also indicate that the presence of Congregationalists and Universalists is associated with lower marital fertility whereas the presence of Lutherans is associated with

[8] Although the war appeared to have only a modest impact on the timing and incidence of first marriage, it may have had a more dramatic impact on remarriage. The much higher rates of widowhood in the South evident in the 1880 IPUMS sample supports this conclusion, although it is impossible to make a conclusive test of this hypothesis without data on the number of times each individual had been married and the duration of their current marital status.

[9] Coale and Zelnik, *New Estimates*; and Haines, "Estimated Life Tables."

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higher marital fertility. These results are consistent with the hypothesis that "liberal" religious beliefs reduced cultural impediments to adopting family limitation strategies. The fact that early advocates of birth control in antebellum America were religious liberals or freethinkers further supports this conclusion.

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Re-Estimating Growth and Convergence for Developed Economies, 1870–1990

The development of real income series that are comparable both over time and across countries significantly expanded the possibilities for research in quantitative economic history and long-run economic growth.[1] This dissertation re-examines the long-run GDP estimates that span over 100 years (henceforth, long-span GDP estimates). While these estimates are used extensively in the literature, their reliability has received limited attention.[2] My findings are first, that there are persistent discrepancies between the long-span GDP estimates and alternative benchmark estimates of comparative GDP. Second, the long-span GDP estimates may result in the mismeasurement of economic performance for individual countries and the forces of catch-up and convergence across countries.

Two problems arise in the calculation of long-span GDP estimates. First, given that exchange rates do not accurately reflect relative purchasing power, GDP-level comparisons are possible only with the appropriate purchasing power parity converters.[3] This requires

[1] This dissertation was completed in 2000 in the Department of Economics at the University of Miami (FL) under the supervision of Professor John Devereux. Professor Luis Locay also contributed significantly to this work's progress.

[2] For example, these data are central to the large body of work on catch-up and convergence. The seminal papers in this area are Abramovitz, "Catching Up"; and Baumol, "Productivity Growth." Elsewhere in the growth literature they have been used to test competing growth theories in papers such as Kocherlakota and Yi, "Is There Endogenous Long Run Growth."

[3] The purchasing power parities (PPP) that are referred to are not those related to equilibrium exchange rates. Instead they have been developed only to allow price and volume comparisons of GDP. They refer to the entire range of goods that make up GDP, including goods that are not traded.

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an adjustment for price-level differences across countries. Second, although price benchmarks have been constructed since 1970 for this purpose, we do not have price benchmarks for the period before 1950. As a result, long-span GDP estimates are constructed using a single post-1970 price benchmark. Comparative GDP estimates in the benchmark year are combined with domestic growth rates to produce the long-span GDP series.

The single benchmark approach to long-span international comparisons is not new. Colin Clark was the first to use this method and many researchers have subsequently formed long-span estimates of this type. Angus Maddison's GDP data have, however, received the greatest attention.[4] In their most recent version, the Maddison data use a 1990 GDP benchmark from the International Comparison Project (ICP). GDP estimates for all other years in the series are formed when the GDP benchmarks are merged with growth rates from national GDP series.

There are several problems with the single-benchmark method. First, the use of a single benchmark implies that the long-span GDP series are based on only one direct price comparison. Second, the merger between the GDP benchmarks and domestic national accounts data is one that involves data sets that may, to a large extent, be based on different prices and goods. Any errors in national GDP series and domestic growth rates are also embedded in the long-span estimates. Furthermore, they are compounded for over 100 years. The alternative to this approach is to construct a series of benchmark GDP estimates; each based on a direct price comparison. The long-span GDP estimates can then be compared with the multiple benchmark estimates. This is the approach that I adopt in this study.

I form new GDP benchmarks based on direct price comparisons for 11 industrial economies over the period 1872 to 1990.[5] Each GDP benchmark is calculated relative to the United States, as the ratio of relative nominal GDP per capita to relative price levels within each benchmark year. The price benchmarks are consumer price benchmarks, formed using detailed price and expenditure data, many of which were previously unused for international comparisons. Each price benchmark is calculated as a Fisher Ideal index. I form GDP benchmarks for 1872, 1905, 1930, and 1950. I also form new long-span GDP estimates for the entire sample of countries from 1870 to 1990. Following the standard long-span approach, I form the long-span estimates using only one GDP benchmark. I merge the Fisher variant of the 1990 real GDP per capita benchmark from the ICP with growth rates from domestic national-accounts data.

Comparisons of the long-span and benchmark estimates of per capita GDP reveal substantial differences across the estimates. The discrepancies are largest in 1872. They are over 40 percent for five economies and over 20 percent for seven economies. For example, the difference is 54 percent for France,77 percent for Germany, 60 percent for the Netherlands, and 42 percent for the United Kingdom. In addition, with the exception of Canada, the long-span estimates show higher levels of per capita GDP relative to the United States than the benchmark estimates. In this sense, the long-span estimates may understate the relative position of the U.S. economy in 1872. Large discrepancies between the long-span and benchmark GDP estimates are also present in 1950. The differences are 59 percent for Italy, 29 percent for the Netherlands, and 38 percent for the United Kingdom.

These differences between the long-span and benchmark estimates of per capita GDP have important implications for measures of catch-up and convergence across the sample countries. Moses Abramovitz noted that the long-span estimates show convergence in the pre–World War I era but no evidence of catch-up, followed by rapid catch up and convergence in the post-1950 period.[6] The benchmark estimates, in contrast, show evidence of

[4] The various versions of the Maddison data vary in their country coverage and time span. The most recent version provides comparative GDP estimates for 56 countries from 1870 to 1994.

[5] The sample countries are Australia, Canada, Denmark, France, Germany, Italy, the Netherlands, Norway, Sweden, the United Kingdom, and the United States.

[6] Abramovitz, "Catching Up."

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significant convergence as well as catch-up in the pre-1913 period. Furthermore, they show stronger evidence of catch up and convergence in the post-1950 period than is suggested by the long-span estimates.

The differences between the long-span and benchmark GDP estimates also have implications for the economic performance of individual economies. The most notable example of this is seen by the relative performance of the United Kingdom and the United States. The data allow me to construct 12 per capita U.K./U.S. GDP benchmarks from 1872 to 1990 that I use for comparison with the long-span estimates. The long-span estimates describe an 80-year British decline, from 1870 to 1950, interrupted by two world wars and the great depression.

Although it is clear that the U.K. economy suffered a dramatic decline relative to that of the United States, the benchmark GDP estimates depart from the long-span GDP estimates in important respects. First, the benchmark estimates show that the United States already had a 16 percent income lead over the United Kingdom in 1872. According to the long-span estimates, the United Kingdom had an income lead of over 20 percent in 1872. The benchmark estimates also show that the United Kingdom kept pace with the United States from 1872 to 1930, and then declined precipitously from 1930 to 1950. There are two implications of this finding. First, contrary to the popular view, the Victorian economy did not fail. Second, the U.K. relative decline is concentrated in just 20 years, from 1930 to 1950. Finally, the benchmark estimates find the post-1950 resurgence of the U.K. economy to be substantially stronger than is estimated by the long-span estimates.

Which GDP estimates do we trust? I provide support for the benchmark estimates in two areas. First, I show that the benchmark GDP estimates are consistent with existing estimates of real wages.[7] Second, the differences between the long-span and benchmark GDP estimates can be explained by differences in their underlying relative price levels. I show that the relative price estimates that underlie the GDP benchmarks are more plausible than the relative prices that underlie the long-span GDP estimates.

Consider first the 1950 price benchmarks. These price benchmarks for most countries are from the OEEC study of Milton Gilbert and Irving Kravis.[8] The OEEC study was a large, carefully formulated price comparison that formed the basis for all subsequent international price comparisons. As such, these price estimates are clearly more accurate than the relative price levels that are implicit to the long-span GDP estimates.[9] The price benchmarks for 1905 and 1930 are taken in large part from well-known early-twentieth-century price comparisons. As a result, these direct price comparisons are also preferable to the relative price levels that are implicit to the long-span GDP estimates. The nineteenth-century price benchmarks, however, are based on new price data. They therefore require additional justification.

I find support for the nineteenth-century benchmarks in three areas. First, the data for the late nineteenth century appear to be of a very high quality. The majority of the data was collected specifically for the purposes of international comparisons by various departments of the U.S. government.[10] Furthermore, the commodity coverage of this data is in many cases wider than domestic CPIs during this period. For example, the 1872 price benchmarks are constructed using the prices of approximately 48 items covering food, rent, clothing, and fuel and light. The 1891 U.K./U.S. price benchmark includes the prices of over 110 items.

[7] Estimates of comparative real wages are provided in Williamson, "Evolution."

[8] Gilbert and Kravis, *Comparative National Products*.

[9] The relative price levels that are implicit to the long-span GDP estimates are derived as the ratio of relative nominal GDP per capita to relative real GDP per capita in each year.

[10] This was due to a great U.S. interest in the cost of living in foreign countries in the late nineteenth century, stimulated by U.S. commercial-policy debates and a fear that competition with Europe would reduce U.S. real wages.

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Second, the relative price changes implied by the price benchmarks between 1872 and 1905 are consistent with the well-documented falling transportation costs in the world economy in the late nineteenth century.[11] The price changes implied by the deflators implicit to the long-span estimates, however, are at odds with the transportation revolution. Third, the nineteenth century price estimates are consistent with consumer prices collected from alternative sources.[12]

In the final chapter I briefly address the comparative GDP estimates obtained through "shortcut" procedures which have been presented as another alternative to the long-span estimates. This procedure produces relative GDP estimates using relative price levels obtained from an estimated structural relationship between the price level and other important variables. I show that the shortcut procedure suffers from theoretical and empirical difficulties that render the resulting GDP estimates unreliable.

The contributions of my dissertation are threefold. First, I fill a gap in the data by providing new price benchmarks for the period 1872 to 1950. Second, I highlight discrepancies between the long-span and benchmark GDP estimates. This finding is important because the long-span estimates are widely used for research in economic history and long-run growth. I argue that the benchmark GDP estimates are preferable to the long-span estimates. Finally, I show that the long-span GDP estimates can result in the misreading of the historical record for individual countries and of growth experiences across countries.

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[11] O'Rourke and Williamson (*Globalization*) discuss this issue in detail.
[12] Some examples are Mulhall, *Dictionary*; and Allen, "Real Incomes."

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An Unsung Hero: The Farm Tractor's Contribution to Twentieth-Century United States Economic Growth

The United States has experienced tremendous improvements in its standard of living over the past hundred years, as have most nations in the developed world.[1] We now accept as necessities many things that our grandparents would have considered to be luxuries; we also enjoy countless goods, devices, and services completely unknown a century ago. Far fewer workers are required to produce the necessities of life that we now consume, and therefore an increasing number of us have time to devote to leisure activities, such as entertainment and recreation.

Improvements in agricultural technology have played a large part in helping to generate this improved standard of living. Our agricultural production function has seen three major modifications over the last 150 years, each of which has dramatically increased crop output per unit of input: the mechanization of the harvest via the reaper and steam thresher; replacement of horses and mules with tractors as a source of farm power; and the scientific revolution, which has brought genetic crop selection and chemical fertilizers, herbicides, and pesticides. This study explores the second of these three transformations, and measures the economic benefits brought by the introduction of the farm tractor.

As Joel Mokyr pointed out several years ago in *The Lever of Riches,* there are at least four different types of growth that contribute to increasing the standard of living in an economy.[2] The first of these, which Mokyr terms "Solovian," involves growth through capital accumulation, which is in turn fueled by redirecting some of current output away from consumption. This trade-off produces the economist's familiar "no free lunch." The other three sources of growth involve some type of free lunch, driven by nonconvexities that allow better economic outcomes than can be achieved with the optimal consumption and investment choices available in a fixed-technology, closed economy context. Mokyr describes commercial expansion growth through international trade and specialization as "Smithian" growth. Scale economies, arising from fixed costs and indivisibilities, can produce a third type of growth, one on which the twentieth-century command economies relied, perhaps too much.

Finally, increases in the stock of human knowledge, which include both technological change and institutional improvements, can drive a fourth type of growth, termed "Schumpeterian." It is this type of growth that is engaging the energies of so much of the economics profession today. Mokyr claimed that prior to about 200 years ago, population growth increased at about the same rate as technological change, and so humankind remained close to subsistence level. The sustained increases in the standard of living encountered since then are driven in large part by increases in the rate and magnitude of this "freest of all free lunches."

Despite a great deal of effort in recent years, economists and historians still have an inadequate understanding of the nature, causes, and consequences of this most important source of welfare improvement. The extensive literature in economics devoted to growth through technological change can be divided into two intellectual threads. The first discusses and models economies that exhibit the first three types of growth, using sophisticated mathematical techniques to explain optimal growth paths, rates of capital accumulation, and trade patterns, even in the presence of nonconvexities and spillovers. The current state-of-the-art is found in the work of Philippe Aghion and Peter Howitt and others, who explicitly include Schumpeter's "creative destruction" in their models.[3]

[1] This dissertation was completed at Ohio State University under the supervision of Richard Steckel.

[2] The following paragraphs rely on Mokyr, *Lever*, pp. 4–6.

[3] A few key works in this large and expanding literature include: Romer, "Are Nonconvexities"; Grossman and Helpman, *Innovation*; and Aghion and Howitt, "Model" and *Endogenous Growth Theory.*

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In order to allow these models to close, however, all of these economists must place restrictions on the nature of the technological change and nonconvexities considered. Indeed, if new technologies continually emerge and evolve such that factor inputs for a given output decrease irregularly over time, it becomes impossible to prove the existence of a general equilibrium.[4] The welfare properties and agent behaviors that emerge from such a dynamic situation cannot be successfully integrated into any microeconomic or macroeconomic models that require a stable equilibrium.

The second intellectual thread on growth and technological change, evidenced by the writing of Nathan Rosenberg and Joel Mokyr, does an excellent job of describing this dynamic instability over the course of recent economic history.[5] They discuss not only key technological changes that produced surges of growth, but also the impacts on the welfare of the affected populations. However, their work contains few formal models to describe this process, most likely because of the inappropriateness of the underlying assumptions. Douglass North, in discussing the use of neoclassical synthesis in understanding the development of institutions, made the following observation: "From the viewpoint of the economic historian, this neoclassical formulation appears to beg all of the interesting questions."[6]

This study, then, adopts the position that an empirical study of historical growth need not, and perhaps should not, start from a general-equilibrium framework. There have been many important partial-equilibrium analyses of the growth impacts of specific new technologies, including Robert Fogel's celebrated work on railroads and Zvi Griliches's analysis of hybrid corn.[7] Each of these seminal works has produced a number of derivative papers, which are considered in some detail in the body of the dissertation. The present thesis is an attempt, admittedly a modest one, to apply some structure and measurement tools to determine the welfare benefits of one of the most significant changes in the technology and the production function of our nation's agriculture: the replacement of draft animals with gasoline-powered tractors.

Economic and agricultural historians have debated the nature of this production-function change for a number of years, as well as the similar nineteenth-century mechanization via the reaper and steam thresher. In developing his threshold-model approach, Paul David argued that these changes can be thought of as affecting relative prices within an existing equilibrium.[8] Zvi Griliches, Theodore Schultz, and others believed that the new technologies brought about a disequilibrium, which might last for a number of years.[9] The approach to the present work implicitly adopts the disequilibrium point of view as regarding the economy as a whole. In doing so, the claim is not that market failures existed in product or factor markets, merely that slow, irregular, and unpredictable technological change made achievement of general equilibrium unattainable.

The large number of important new inventions and technologies introduced in the early years of the twentieth century and their steady rate of improvement led Simon Kuznets to conclude that no single invention or innovation was worth studying in isolation. Robert Fogel, in his successful attack on the "axiom of indispensability," demonstrated that the railroads made only a modest contribution to the growth of nineteenth-century income in the United States. In the views of both of these eminent scholars, technological change

[4] Existence theorems in general equilibrium were developed in a context of a pure exchange economy, and have been extended to include production only if firms' technologies exhibit constant or decreasing returns to scale. If technologies exhibit globally increasing returns to scale, there is no equilibrium. Likewise, the production technology is static in these proofs of existence of equilibria.

[5] Relevant works of these two authors include: Nathan Rosenberg, *Technology* and *Inside the Black Box*; and Mokyr, *Lever* and *Twenty-Five Centuries*.

[6] North, *Structure*, p. 5.

[7] Among the many publications which contain summaries of this work are: Fogel, *Railroads*; and Griliches, "Research Costs."

[8] David, "Mechanization."

[9] Schultz, *Economic Growth*, pp. 225–30.

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consisted of a series of uncountable small improvements, which together created impressive increases in output, income, and consumer welfare. Fogel warned against a "hero theory of history applied to things rather than to persons."[10]

The findings of the present work challenge Fogel's conclusion by presenting evidence that the farm tractor was one of the most important technological advances in the twentieth century—in fact, an unsung hero. Its universal adoption, by raising the productive efficiency of inputs to agriculture, reshaped the labor force, the face of rural America, and the structure of the American economy. Through the efficient use of farm mechanization, labor productivity was tripled and more than 24 million work animals were replaced. In this way, the tractor dramatically reduced total factor inputs to agriculture, freeing up resources for use in the rapidly growing manufacturing sector.

A counterfactual analysis is used to quantify the social savings from the tractor's adoption. Using 1954 as a base year, the analysis estimates the inputs which would have been required to grow and harvest that year's farm production if pretractor 1910 technology were the best available. An alternate specification calculates the hypothetical input requirements for producing the crop mix of 1910, adjusted for the larger population of 1954. In both cases, actual yields from 1954 are assumed to be achievable, even with the animal-powered technology.

These counterfactual input requirements, when compared with those actually experienced in 1954, give an estimate of the social savings generated. Even after adjusting for a large negative income effect, direct social savings are found to be in excess of 8 percent of 1954 GNP, more than double Fogel's highest estimate for railroads in 1890. When indirect benefits are added, such as the use of tractors to apply yield-increasing agricultural chemicals, their contribution to the growth of the economy in this century is even more significant. The most striking result from this analysis is that tractors eliminated the huge inefficiency created by one of the inputs (horses) consuming more than 20 percent of the output!

Even with the large social benefits of mechanization as a driver, the new technology was adopted rather slowly. The first fossil-fuel-powered tractor was demonstrated in 1892, only a few years after the commercialization of the internal combustion engine. Within 15 years a handful of manufacturers were offering units for sale, claiming that these modern marvels would eliminate the need for horses and mules in farm work. Reductions in weight and cost and improvements in performance created the first surge in sales after the First World War, but horses and tractors continued to co-exist until the 1960s. The slow, irregular diffusion of the tractor matched a gradual decline in the number of work stock on farms for more than half a century. Over the years, social scientists have attributed this delayed response to ignorance, irrationality, and most recently, to several types of market failure. Threshold models have been used extensively in the literature to illustrate the technology choices faced by the farmer.

This dissertation challenges the market-failure hypotheses. A newly developed production and quality database is used to construct a hedonic index of tractor prices, which shows that quality-adjusted prices fell continuously from 1918 until about 1940, then stabilized at half of their initial level as the technology matured. For example, a tractor selling for $2,250 in 1918 delivered power and other services available for $1,100 by 1955. Given this dynamic element, properly constructed threshold models show the pace and pattern of diffusion to be a rational market response to changing relative prices. As quality and features improved, and as cumulative production experience allowed manufacturers to reduce production costs, the tractor became a better replacement for animal power on all types and sizes of farms. The social benefits from this unsung hero then spread across the country, lowering food prices and freeing up labor for manufacturing and services.

By using a more accurate series of prices, much of the "slow diffusion" mystery disappears. Tractors were slow to diffuse initially because they were limited in capability and

[10] Fogel, *Railroads*, p. 236.

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their cost/performance ratio was little better than that of competing technology. Sustained product and cost improvement, along with a number of significant technical breakthroughs, made the tractor an increasingly better replacement for animal-powered agriculture. Within the diverse group of farmer customers, a greater proportion of them moved across the threshold for which replacement became optimal. By the late 1940s nearly every viable farm would conclude that a tractor was the best option.

The slow and unsteady rate of technological replacement seen with tractors is indicative of technological change in general, a third major theme of the dissertation. A series of product and process innovations, minor inventions, and cost reductions changed the tractor over time from a technology that was only superior for a small fraction of a heterogeneous group of farmers to one that completely replaced the horse and mule. Contrary to the notion of technology "shocks" employed in most if not all current macroeconomic growth models, the new technology was not born whole at an instant in time, but evolved over a period of almost 60 years.

To make matters worse, tractor-producing firms proved unable to accurately predict the future state of their production functions, and thus could not internalize scale economies or learning-curve effects and operate at Minimum Efficient Scale (MES). Qualitative evidence is presented that this, too is often the case in new technology areas; the over-expansion –shake-out phenomena depends on this type of knowledge failure.

The dissertation closes with a challenge to theorists of all stripes to work seriously to incorporate technological change, and the nonconvexities and disequilibria that it generates, into the theoretical structure of the discipline. It is expected that others in the future will find better ways to utilize formal modeling and mathematical tools to improve the elegance and persuasiveness of this type of argument.

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The Impact of Revolution: Business and Labor in the Mexican Textile Industry, Orizaba, Veracruz, 1900–1930

Social revolutions are a major historical feature of twentieth-century world history. For those countries in which they occurred, they became the axis on which their postrevolutionary histories revolved, acquiring a mythical character.[1] That these revolutions had created a better and more equitable world, that they had been a step towards progress, that without them, tyranny and injustice would have prevailed, became some sort of a religious faith difficult to question without being accused of heresy.

For Mexico, where the Revolution as myth or reality has been a source of national identity and pride, the Revolution and the way it is understood still occupies a central place in political debate. Used and abused, it is the starting point from which every conceived future for the country departs.

What were the effects of the Revolution on Mexico's economic development? Did workers' well-being actually improve as a consequence?

In order to answer these questions, this study focuses on Mexico's largest manufacturing industry, cotton textiles. Within the cotton textile industry it centers on Mexico's largest producers, the Compañía Industrial de Orizaba (CIDOSA) and the Compañía Industrial Veracruzana (CIVSA). At the beginning of the twentieth century these companies were among the most modern and best equipped. At the same time, their workers created the most powerful labor organization in the country, and gained tragic celebrity as a result of the military repression they suffered in 1907. During the revolutionary years and during the 1920s, they were among the most strongly unionized workers in the nation. Using their company archives, a detailed analysis of the nature of these firms and their work force is carried out.

Answering the question about the impact of the Revolution this work first provides readers with a detailed portrait of these firms prior to 1910 (when hostilities broke out). Then it follows the manufacturers through the years of conflict (1910–1920) and then through the postrevolutionary years (1921–1930), when the institutional changes produced by the Revolution were felt by the industry.

The cotton textile industry was established in Mexico relatively early compared to elsewhere in Latin America and other under-developed economies. However, textile manufacturing did not develop and prosper during the nineteenth century at the same pace as in industrialized nations because Mexico's institutional frailty, and the resulting slow growth of its economy as a whole, were obstacles too large for the industry to surmount. The industry did manage to continue growing and evolving technologically throughout that century, although at a slow pace and in a geographically dispersed way.

When political stability was finally achieved in the 1880s and railroads were built, banks created, and laws modernized, the textile industry experienced a revolution in distribution and production, Chandlerian in type but Mexican in style. Cloth distribution shifted from small family-run stores to huge department stores that controlled a substantial part of the wholesale cloth business. Its entrepreneurs combined the capital accumulated in these commercial enterprises to build new textile-manufacturing joint-stock companies. New textile mills were built, and old mills were modernized, incorporating world class, state-of-the-art technology, including hydroelectric power. CIVSA and CIDOSA are good examples of this process.

In spite of the considerable institutional transformation that Mexico experienced in the late nineteenth century, these changes were not large, broad, or fast enough to guarantee

[1] This dissertation was completed at Harvard University under the supervision of John Womack.

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certainty in legal contracts, create financial institutions that could provide investment capital to promising, well-backed projects irrespective of the name of the entrepreneur, or permit a general diffusion of education. In this context, it is easy to understand why specific ethnic groups, with capital, education, and strict moral codes that prevailed within them, such as the French Barcelonnettes, became so prominent in the Mexican economy, and in particular in the revolution in distribution and production that took place in the textile industry.

The new textile firms created in the late nineteenth century were in very good shape, as far as technology and productivity were concerned. Contrary to all expectations, the detailed study of CIVSA indicates that its products appear to have been fairly competitive when compared with English cloth (from which Mexico obtained most of its imports). While Porfirian Mexico followed a protectionist strategy to promote industry, the ad-valorem tariffs it granted cotton cloth were not higher than those the U.S. government established to protect the American textile industry during the same period.

These firms also appear to have been highly profitable in the decades previous to the Mexican Revolution as CIVSA's corporate accounts and CIVSA and CIDOSA stock-market reports indicate. Although they were able to survive the Revolution and even make good profits in some of the years during which the armed conflict took place, levels of profitability decreased and remained low in the 1920s.

Of all the changes that the Revolution brought about, the most enduring and pervasive one for the textile industry was the change in labor and capital relations that developed in the course of the 1910s and 1920s. Although workers' organizations and protests had ignited before the Revolution and, in spite of repression, not been completely suppressed, the Revolution provided plenty of fuel for the fire. In the course of the revolutionary decade, unions and confederations of unions were created and became powerful counterparts in dealing with employers. By the end of the decade, most textile workers in central Mexico had been unionized, hired through collective contracts and through the union's mediation.

To answer whether workers' purchasing power improved as a consequence of the Revolution, the evolution of real wages at CIVSA from 1900 to 1929 is studied using price indices constructed by the author for this purpose. Results indicate that during the Revolution workers were hardly able to keep their real wages constant because of rising inflation. From 1911 to 1913 workers recovered a 20 percent loss in their purchasing power they had suffered during the two last years of the Porfiriato. However, in 1913 when war assumed greater proportions, political chaos gave way to monetary anarchy, which in turn led to hyperinflation. Inflation eroded nominal wage increases from 1914 to 1916, causing a sharp decline in workers' purchasing power, which fell to its lowest point in May 1916, totaling a seventh of what it had been in 1912 in terms of gold pesos. By the end of 1916 continuous strikes forced companies to pay wages in gold and pre-inflationary real wages were recovered.

It was in the 1920s that CIVSA workers saw substantial improvements in their standards of living. Further union mobilization raised real wages by an impressive 131 percent in that decade. Moreover, workers obtained substantial improvements in other aspects of their living standards such as educational facilities and housing; and the workday was reduced. In the Orizaba valley major gains in living conditions were obtained directly by workers' organizations rather than by government policies. The experience of the Orizaba textile industry shows that the legal changes achieved as a result of the Revolution, including the labor chapter of the new Constitution itself, did not come out of nowhere, or spontaneously out of the minds of well-intentioned men. It means instead that somewhere in Mexico there were real demands or even established practices, and that the threat labor unions in those regions posed to political stability was large enough to matter nationally. It also means that legal changes only became real in those regions where there already were substantial popular organizations strong enough to enforce them, or to ensure that they were enforced.

Labor productivity did not decline in CIVSA as a consequence of the Revolution. In fact labor productivity per hour increased when working hours diminished, maintaining daily

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production per worker at the same levels as before. Yet, labor productivity did not increase further to keep pace with the changes experienced in textile exporting nations. This was partly the result of a reduction in investment generated by lower profit levels, which partly resulted, in their turn, from the increase in real wages mentioned previously. It was also the product of an institutional arrangement that had long-lasting consequences for the development of the industry, by which workers, supported by a sector of the industry, managed to block the importation of new technology that would displace them.

In 1912 a general wage-list meant to govern the textile industry in the whole country was adopted by industrialists, workers, and the government at a Convention organized in that year to stop frequent strikes. This wage-list, which rigidly set wages in terms of each particular input and machine, was shaped according to the prevailing wage-lists that governed capital and labor relations in the English textile industry of the time. Without any modifications of its technical specifications, the basic form of this wage-list was adopted again in 1927 now as an industry-wide, legally binding collective contract, and so on until 1951. Then it was gradually reformed, allowing for some modernization of the industry, but still maintained substantial obstacles to the adoption of new technology until 1994, when it was finally abolished.

The evidence reported in this dissertation shows that these wage-lists placed severe limits on the Mexican mills' capacity for modernization. That the wage-list was approved, again and again, without changes, was the result of a situation in which workers, industrialists, and the government gained in the short-run by making such decisions, but they left an increasingly severe problem for the future to solve. Unions did not want unemployment, industrialists—particularly those with little capital to invest—did not want to go bankrupt, and the government did not want social unrest.

In 1927 when the structure of the 1912 wage-list was first approved without any changes, the decision was marked by the first waves of the Great Depression that were beginning to wash over the textile industry. After that, path-dependency set in. It was always too difficult to choose another course, particularly because the government generously increased tariff protection as needed, keeping tariffs high for several decades to come.

This study of the Orizaba textile industry, from a period in which no unions existed to a time when they became powerful institutions, offers important insights on the role of unions and their impact on industrial development and workers' well being. It has shown how positive and constructive unions were in securing real improvements in workers living and working conditions. It has also shown how inconducive to industrial development they can become when set in nondemocratic institutional arrangements designed for the preservation of a political regime rather than for their more effective operation in favor of workers interests.

This dissertation has also evidenced the ever-present dilemma that exists between providing better living conditions for workers and building an industry capable of competing internationally, when productivity growth is left out of the equation. Because substantial improvements to workers' living conditions may be obtained in the short to medium run by disregarding international competitive levels, it becomes politically profitable to support this course of action. This study has shown how this strategy created a long lasting equilibrium with imbedded self-perpetuating forces. Unfortunately, taking this path generates lower national economic growth and therefore lower standards of living for the population in general. Moreover, it is a strategy destined to failure in the long run, with terrible consequences for those workers who have the misfortune to see it collapse.

Taking a broader perspective, the main conclusion found in this dissertation is that institutions matter. They are important in shaping business and industrial strategies, as well as in defining productivity levels and standards of living. One set of institutions that was found to be of crucial importance throughout this dissertation is that which shapes the relations between labor and capital, and particularly the relative power of workers to control

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the relationship between effort and pay. Another institution found crucial (necessary but not sufficient) to shaping the form capital-labor relations may take is tariff protection. It is essential to take these institutions into account in any analysis that compares input costs and productivity levels to understand the relative failure or success of nations in achieving industrial growth.

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Enforceability and Risk-Sharing in Financial Contracts: From the Sea Loan to the Commenda in Late Medieval Venice

My dissertation uses historical records and a context-specific mechanism-design model to investigate the institutional and contractual arrangements that enhanced mobilization of capital and risk-sharing in late-medieval Venice.[1] The funding of long-distance risky trade in late medieval Venice could potentially promote economic prosperity, but it required that merchants were able to commit ex-ante not to breach their financial contracts ex-post. Institutions for contract enforcement were thus required to mitigate this commitment problem and enable welfare-enhancing financial exchange.[2] Distinct institutional arrangements enforce different sets of contractual forms, among which particular ones can be chosen. The selection of various contracts, and their underlying institutional foundations, has significant efficiency effects. The dissertation thus integrates a historical institutional analysis of the emergence and transition of various contracts with the study of their efficiency attributes. This approach enables me to address the following questions: What institutions for contract enforcement enabled the Venetians to commit to the sea loan (a debt-like contract) and the commenda (an equity-like contract)? What caused the transition from the former to the latter? Did the Venetians attain an optimal allocation of risk?

The interest in this particular historical episode and these questions is three-fold. First, the funding of long-distance risky trade in Europe from the eleventh to the fourteenth centuries led to both economic growth and fundamental political, social and economic changes.[3] Venice, in particular, became the richest city of the time and the main European financial center for hundreds of years while enjoying political and social tranquility.[4] Yet, we know very little about the institutions underlying this economic and political success. Second, markets and political units co-emerged in Europe during this period. Knowledge of past interrelations between economic institutions and politics can be useful for the understanding of today's institutional transition of developing and ex-communist countries. Third, this period saw the intriguing replacement of the sea loan with the commenda. However, the present historical and economic literatures neither adequately explain this transition nor evaluate its efficiency implications.

[1] This dissertation was written at the Department of Economics at the European University Institute under the supervision of Professors Avner Greif and Ramon Marimon with support from the Social Science History Institute at Stanford University. I also wish to thank Andrea Drago, Gavin Wright, Jaime Reis, and Leandro Prados de la Escosura for their help in various ways.

[2] Institutions are constraints that enable merchants to commit. For a definition of institutions, see Greif, *Historical Institutional Analysis*; and North, *Institutions*.

[3] Lopez, *Commercial Revolution*; and North, *Institutions*.

[4] Lane, *Venice*.

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THESES

My dissertation advances three theses. First, it argues that in late medieval Venice, institutions for contract enforcement were based on the authority of the state, a third-party enforcer with more than just coercive power. Indeed, this state-based enforcement mechanism cannot be identified with the legal system, at least as traditionally described by economic historians and economists. Second, it explains that institutional arrangements that enhanced the state's ability to verify information enabled the transition from the sea loan to the commenda. This implied a better allocation of risk and, arguably, further mobilization of capital. Finally, it claims that the contracts used by the Venetians sustained the optimal allocation of risk given the institutions for contract enforcement.

The Venetian state-based mechanism for contract enforcement was not only supported by a legal system, but also by a set of institutional arrangements that, on the one hand, provided merchants with strong incentives to submit themselves to the coercive power of the state and, on the other hand, generated and transmitted the (verifiable) information required for the legal system to adjudicate disputes. These institutional arrangements enabled merchants to commit not to renege on their financial contracts, above and beyond the enforcement of the legal system.

A medieval court could not exercise its coercive power over a merchant who emigrated. Therefore, incentives to submit to the coercive power of the state were necessary to alleviate the merchants' temptation to embezzle capital and never return to Venice. Such incentives were generated by the state's controls over trade and a commercially oriented foreign policy. These institutional arrangements created economic rents to which only Venetians had access, hence rewarding merchants who kept their affiliation with Venice and generating effective barriers to exit. To sustain commercial rents during the thirteenth century, the Venetians established barriers to entry, which excluded foreigners from Venetian markets for overseas trade. In particular, immigrants needed to reside and pay taxes for no less than 25 years before acquiring full rights of citizenship. In addition, the state's ability to exclude merchants from joining the state's fleets and enjoying other commercial privileges rendered the expectation of being punished by the state effective without involving costly confiscation of property by the court.

On the other hand, the legal system would have been unable to enforce contingent contracts— essential for sharing risk—in the absence of verifiable information. To mitigate this problem, Venetian institutional arrangements from the end of the twelfth century onwards made trading a public affair. Overseas trade became carefully monitored by public mediators in Venice, scribes en-route, and state delegates in the Venetian colonies abroad. Hence, these public officials could and did provide verifiable information regarding commercial disputes.

METHODOLOGY

To understand the institutional and contractual arrangements that enabled the Venetians to mobilize capital and spread risk, I have developed a theoretical framework and confronted its assumptions and predictions with empirical evidence from almost 1,000 notarial acts (found in the *Archivio di Stato* of Venice and transcribed in full by Morozzo della Rocca and Lombardo, 1940 and 1953). These historical records are very rich in reflecting not only Venetian contracts, but also various aspects of the state-based enforcement mechanism, such as the operation of the courts, the generation and transmission of (verifiable) information, and the exclusion of non-Venetians from trade.

Based on this source, I have constructed an appropriate context-specific mechanism-design model, in which the financial relations between the merchant and his potential financier are represented as comprehensive contracting in the presence of hidden informa-

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tion. Technically, the model is inspired by the costly state-verification literature, but it differs in its main assumptions to capture the details of the historical episode.[5] In my model, all agents are risk-averse, each might be endowed with the resources required to finance the venture by his own, there are both a maritime and a commercial risk, and there is no costly verification of the venture's returns. Instead, the model considers two exogenous information structures, each of which reflects the state's ability to verify information at infinite or zero private cost. The model thus includes a parameter that measures verifiable information, which is, however, endogenously generated in the broader theoretical framework. To further capture the relevance of the previously mentioned institutional arrangements, the model assumes the existence of incentives for merchants to keep their affiliation with Venice.

The theoretical framework enables me to achieve two objectives. First, it generates various qualitative predictions under the assumption that financial relations were enforced by the state. Empirical confirmation of these predictions lends support to my first thesis: in late medieval Venice institutions for contract enforcement were actually based on the authority of the state. For example, this state-based enforcement mechanism is theoretically consistent with the operation of financial markets among Venetians and with the exclusion of non-Venetians. Indeed, the observed financial relations went beyond those that can (theoretically) be sustained by natural groups such as the family, or by reputation mechanisms. The theoretical model also predicts the observed capital structure. In particular, it accounts for the characteristics of the historically prevalent sea loan and commenda contracts. Furthermore, the model predicts the transition from the sea loan to the commenda as the flow of verifiable information increases. In reality, the commenda replaced the sea loan by the turn of the twelfth century in response to changes in the state's ability to verify information. This last empirically supported prediction also substantiates my second thesis: institutional changes led the transition to alternative contracts and facilitated a better allocation of risk.

Second, this context-specific model enables me to evaluate my third thesis, meaning the extent to which these contractual and institutional arrangements supported an optimal allocation of risk. Indeed, the model provides a full characterization of the optimal capital structure. The sea loan and the commenda emerge as the constrained optimal contracts in the model. Moreover, the enhanced ability of the state to verify information supported the first-best allocation of risk, which was attained by letting the merchant finance part of the venture and raising additional funds through commenda contracts.

CONCLUSIONS

The dissertation contributes to the historical institutional and contract theory literatures in two ways. First, it complements the former by providing an adequate theoretical framework to evaluate the nature, transition, and efficiency implications of various historically relevant contracts. At the same time, and in contrast with the standard static explanations of optimal contract design, this work links the emergence and transition of various contractual forms to their underlying institutional foundations. By doing so, this dissertation sheds new light on the dynamic process through which expanded exchange took place. Secondly, the dissertation finds that the Venetian institutions for contract enforcement evolved along a distinctive institutional path. Previous analyses of institutions that confronted various commitment problems during the Commercial Revolution from the eleventh to the four-

[5] For an excellent review of Optimal Security Design literature based on agency costs, including both models with costly state-verification and incomplete contracting, see Harris and Raviv, "Theory." The model is particularly influenced by the work of Townsend, "Optimal Contracts" and "Optimal Multiperiod Contracts."

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teenth centuries have emphasized the importance of reputation mechanisms.[6] In contrast, this historical institutional analysis finds that in late-medieval Venice the state functioned as an enforcement and information-transmission mechanism. However, the origin, nature, and role of this state-based mechanism differed substantially from that posited by economic historians and economists to premodern and modern legal systems.

This study suggests that the Venetian institutional and contractual arrangements not only reflected but also shaped the entire organization of the society, both politically and economically. Indeed, this state-based mechanism for contract enforcement enabled the Venetians, by and large, to mobilize capital through financial markets and, hence, resulted in a wide distribution of the gains from trading overseas among the Venetians. This generated the political support required to invest in the provision of public goods such as a court system and verifiable information, cooperate to ensure commercial rents; and establish barriers to entry and exit, which reinforced the Venetian closed social structure.

Despite the potential limitations to economic growth inherent in monopolistic practices and restrictions on labor mobility, the Venetian institutional and political equilibrium sustained broader exchange relations than those supported by reputation mechanisms in other historical episodes. For example, in late-medieval Genoa, overseas trade was governed by a bilateral reputation mechanism.[7] As a result, Genoa performed very differently in terms of the development of capital markets, risk-sharing, selection of alternative contracts, and business practices. Furthermore, Genoa's political organization encouraged factional rivalry which, arguably, led to economic decline and political instability.[8] The intriguing questions then are why these otherwise similar Italian city-states evolved along such different institutional and political trajectories? What dynamics characterized the process of equilibrium selection? Did distinctive historical experiences ultimately bring about distinct institutional equilibria in Venice and Genoa, and if so, what prevented the Genoese from developing similar institutions. These questions lead the way to a future comparative and historical institutional analysis that may facilitate our understanding of both past economic developments and the political impediments to economic growth in contemporary developing countries.

YADIRA GONZALEZ DE LARA, *Ente Einaudi*

[6] Greif, "Contract Enforceability, "Cultural Beliefs," and "On the Political Foundations"; Greif, Milgrom, and Weingast, "Coordination"; and Milgrom, North, and Weingast, "Role."

[7] Greif, "Cultural Beliefs."

[8] Greif, "On the Political Foundations."

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Producing a Peculiar Commodity: Jamaican Sugar Production, Slave Life, and Planter Profits on the Eve of Abolition, 1750–1807

This dissertation is a study of the Jamaican sugar economy and slave society on the eve of the abolition of the British slave trade.[1] Its most significant finding focuses on the "Decline Thesis," as laid out by Eric Williams more than 50 years ago in his classic work, *Capitalism and Slavery.* Slavery, according to Williams, was doomed to failure because it encouraged planter absenteeism and plantation mismanagement, resulting in the tendency to overproduce. Thus, planters were increasingly saddled with steadily rising input costs while facing falling revenue. Williams explained that the first cracks in the sugar economy appeared with the loss of the North American colonies. From this point on, the British West Indian planters saw a fall in their profits. This late-eighteenth-century decline of the slave mode of production culminated in Parliament abolishing the slave trade in an attempt to cut production levels and thereby revive West Indian fortunes.[2]

The quantitative studies produced since the publication of *Capitalism and Slavery* suggest that Williams's narrative regarding planter decline was fundamentally wrong. Most economic historians believe planters recovered quickly after the crisis of the American Revolution. Seymour Drescher has done the most to discredit Williams. Using aggregated trade data, he demonstrated that the abandonment of the slave trade occurred while the industry was the most vibrant. In fact, the decision was economic suicide, or "Econocide"; Parliament took a proactive step that damaged Britain's most important overseas trading partner.[3] Since the publication of Drescher's argument, historians have largely discounted Williams's link between the economic performance of the West Indies and the abolition of the slave trade.

[1] This dissertation was completed in the Department of History at the University of Minnesota (December 1999), under the direction of Professor Russell R. Menard. Published work based on this dissertation can be found in Ryden, "Does Decline" and "'One of the Fertilest Pleasentest Spotts.'"
[2] Williams, *Capitalism.*
[3] Drescher, *Econocide*.

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This dissertation questions the applicability of macroeconomic data in evaluating the economic health of the planter class. As an alternative to inferring the economic health from aggregate trade statistics alone, I have reconstructed the history of a large sector of the sugar economy based on microdata sources. By focusing on the important colony of Jamaica, a clearer understanding of sugar manufacturing and slavery emerges. This approach reveals more information about nuances of the economic change during this period while unearthing details about the lives and management of slaves.

This dissertation demonstrates that Williams's story of a *long-run* decline in sugar planting profits is inaccurate. The sugar industry was a competitive one and the efficiency of both input and output markets ensured prices were in constant adjustment, thereby ensuring a fair expected return to new investors. Significant increases in demand for sugar placed upward pressures on the value of plantations and their inputs; the market could bear these higher valuations because potential investors expected higher future revenue streams. Likewise, if demand for sugar slackened, the market worked in reverse; the value of the estate and capital equipment mirrored their earning potential. This relationship is illustrated by the correlation between the average revenue product of slaves and their prices. Thus the long-run adjustment between the price of sugar and the value of investment means that the notion of a long-run profit squeeze is untenable.

The fact that market pressures promoted, on average, a "normal" return for sugar planters does not mean the performance of the sugar economy had no affect on the decision makers who abolished the slave trade in 1807. I demonstrate that the European shortage of sugar following the Haitian Revolution led to irrational exuberance on the part of British planters, who more than doubled their output in order to cash-in on the high European prices. This massive expansion ushered forth a new period in the history of British sugar growing, during which British producers were, for the first time, re-exporting (from Britain to Europe) a significant share of their product. This international trade mandated a convergence of English sugar prices with European sugar prices, net of transportation and transaction costs. Thus, mercantilist policy could no longer effectively protect sugar growers from international competition. In the short run, when prices in Europe were extremely high, free trade to the Continent benefited British planters. However, the same high prices that stimulated the expansion of British sugar production also led to the growth of the infant Cuban sugar industry and encouraged the further expansion of the capacity of Guadeloupe and Martinique. These foreign planters had more productive land and they could turn to inexpensive North American merchants to transport their sugar, thereby effectively undercutting British producers. As output from these lower-cost producers increased and the price of sugar fell rapidly, overproduction became the common complaint among both critics and supporters of the planter class. This problem of overcapacity helps explain the timing of Abolition; the British Atlantic trade was halted once it was understood as a measure that would benefit both slave and master.

By approaching the sugar economy from the plantation level, I describe how the management of sugar estates changed during the half century before Abolition. I begin by arguing that during this period, the marginal cost curve for the typical plantation was shifting steadily to the right, given that the estimated average output per estate was increasing while farm-gate prices steadily declined. I test this hypothesis qualitatively by analyzing contemporary discussions regarding plantation efficiency and quantitatively by estimating crude labor productivity. Output per hand is calculated to have increased by roughly 55 percent between 1750 and 1807. This accounts for half of the increase in Jamaica's expanding output during this period. Planters achieved this efficiency gain through the implementation of a host of small improvements. New developments included the intermittent use of the plough, irrigation, modification of the planting cycle, innovations to mill design, and the introduction of a new species of sugar cane.

In describing the changes to the Jamaican sugar economy, this dissertation also includes a description of slave work and slave life. Based on surviving estate schedules, I outline

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how Jamaican slaves were organized into gangs based on sex and age. Children were sent to clean sprouting cane plants, carry guinea grass to cattle pens, and were assigned other light work around the estate. As they grew older, they graduated to cane-field gangs. It is estimated that slaves were most valuable, and most productive, when they reached their late teens and early twenties. Strength to withstand the rigors of sugar cane holing and cutting were the key qualities in determining a slave's market value. Most men continued doing the fieldwork until they reached their late thirties, while women continued this back-breaking task until they reached their forties. Furthermore, only men were given skilled occupations that actually enhanced their human capital, such as becoming a boiler, carpenter, or blacksmith.

Although planters used many positive incentives, such as occupational advancement, to engender diligent work, violence and the display of force were essential to the maintenance of control over the island's black majority. The constant threat of a slave rebellion or individual resistance to the system demanded a vigilant stance on the part of Jamaican planters. Furthermore, slave maintenance was neglected due to the planters' drive for profit. Data culled from probate inventories and estate schedules show that despite a near-equal sex ratio, Jamaica's slave-population could not reproduce itself during this period. Qualitative evidence points to unhygienic living conditions and poor diet as being responsible for high infant mortality from Tetanus, Whooping Cough, and dysenteric diseases. As the empire's largest importer of slaves, Jamaican planters found it more cost effective to rely on the slave trade than to promote healthy living and working conditions for their slaves.

The great paradox in the management of slaves during the late eighteenth century was the growth of the internal economy. The trade restrictions with America following the Revolution meant that the cost of provisioning a plantation increased significantly. Planters therefore found it increasingly economical to assign slaves to the task of producing their own food independent of the management of the estate overseer. Decisions regarding the type of crop, the style of production, and the management of planting strategy were handed to the slaves themselves. Planters hoped that in addition to cutting plantation costs, slave-managed provisions would instill a conservative attitude among bondsmen. By giving slaves a stake in the system, it was hoped that they would be less likely to resist their situation. Furthermore, as slave wealth accumulated at different rates, planters reaped the additional benefit of establishing competing interests among the slaves. The division of "rich" and "poor" re-enforced other divisions among the slave population.

Although independent provisioning was an economy for sugar growers, in the long run the planter class recognized the hazards embedded in the system. The ability of slaves to act in concert, independent of the planter direction, was a well-known concern following the initial successes of the slave rebellion in St. Domingue. The trading of goods and the ease of movement facilitated by provision marketing made the control of the slave population increasingly tenuous and displays of military force became essential to maintaining control. Furthermore, the acquisition of personal wealth by slaves engendered an independent or lofty spirit among the slaves so that when they felt affronted, they were inclined to respond in protest. These developments raised the costs of maintaining control of Jamaica's black majority and set the stage for the Creole-led Christmas Rebellion of 1832, which in turn served as the catalyst for Emancipation.

In short, this dissertation offers a multilayered analysis of the slave economy during the late eighteenth century. It addresses a major macroeconomic historical debate, while exposing significant details regarding slave life in Britain's most important American colony. Accompanying this analysis is the presentation of new data, including estimates of sugar and rum farm-gate prices, slave prices, and revised population estimates for Jamaica sugar-estates. This work is intended to buttress previous studies that highlight the importance of the West Indies within the eighteenth-century Atlantic World while offering a new interpretation of the performance of the sugar economy during this important period.

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Discussion of Hacker, Ward, and White

My assigned task is to discuss the three fine dissertations chosen as finalists for the Allan Nevins Prize. But, I want to start by mentioning the eight other dissertations that I read as part of the competition. I found it truly inspiring that so much good work is being done in American economic history. All 11 dissertations submitted were truly first rate and all of them deserved a chance to be recognized. So, to all who submitted, I want to say congratulations on a job well done. And, to the three who were chosen as finalists, I want you to realize just how special your dissertations really are. There were not only the best; they were the best of a very impressive group.

The other thing that I want to mention is taste. I tried very hard to be broad and eclectic in my preferences. But, I discovered as I was reading the submissions and choosing finalists that I am in many ways a prisoner of my training. I received my Ph.D. in economics in 1985, at a time when the "new economic history" really was not new any more. But, perhaps because Peter Temin gave me my first introduction to the field, some of the tenets of that revolution nevertheless found their way into my soul.

One thing that I learned is the importance of formulating a clear hypothesis and marshaling evidence to support or refute it. I find that I react very poorly to story-telling. I am constantly wondering, "What is the question being asked? What is the evidence?" I find it quite easy to accept a wide variety of kinds of evidence. Big data sets are lovely, but so are scattered numbers painstakingly gleaned from newspapers or archival records. And sometimes quotations from company or government records provide the clearest proof of what was going on. But I really want to see evidence logically assembled to answer a well-posed question.

Many of the dissertations submitted had this crucial characteristic. What distinguished the three finalists were the importance of the questions they ask and the quality of their evidence. All three address crucial issues in American economic history and change the way that we view the past. I will discuss them in alphabetical order.

James Hacker has written a monumental study in American demographic history. He was a participant in the Minnesota Historical Census Projects that created computerized samples of the U.S. Censuses of Population for the second half of the nineteenth century. His dissertation contains no fewer than five distinct studies of the demographic changes experienced by the white population between 1850 and 1880. He is particularly interested in the effects of the Civil War on the demography of the American South.

The Minnesota center is an example of a research framework we do not often see in economics. It involves many people doing cooperative research—each one tackles a small piece and the whole is greater than the sum of the pieces. Hacker's dissertation is a microcosm of the Census Project. No one of Hacker's studies makes you bang your fist on your

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head and exclaim, "I wish I had thought of that!" But, taken together the dissertation is an extraordinary achievement. It furthers our understanding of nineteenth-century American demographic history greatly and challenges many long-held beliefs.

Hacker's first study actually uses data from another of these cooperative data ventures —Robert Fogel's project on Union Army recruits. In this study Hacker attempts to estimate the number of Confederate soldiers who died of diarrhea and dysentery. While this is certainly not a glamorous topic, Hacker is thoroughly convincing that it is interesting and important. Indeed, I have to give Hacker credit for opening my eyes to the prevalence of disease-related deaths among soldiers on both sides. Ever since I read a biography of Florence Nightingale in fifth grade, I have known that camp conditions and health care were abysmal. But, until I read Hacker's dissertation I certainly never realized that the majority of deaths during the Civil War were due to disease, not to battle wounds.

Thanks to spectacular Union Army medical records, we have excellent data on the cause of death of Northern soldiers. These records show that roughly one-quarter of the Union soldiers who died of disease died of diarrhea and dysentery. Scholars have estimated the number of Southern deaths from these causes, for which the records burned in the Richmond fire of 1865, by applying the same proportion to Southern enlistments. What Hacker is able to do using the new data on Union Army recruits is to see what death from diarrhea and dysentery are correlated with. He finds that Union recruits from farms were substantially more likely to die from these enteric diseases than urban recruits. Since a larger fraction of Southern soldiers were farmers, it follows that more Confederate soldiers probably died of diarrhea and dysentery than previously believed.

Does this finding fundamentally change our world view? Surely not. But, it does give us a more accurate picture of both the human suffering of the Civil War and the disadvantages faced by the South. And, it is indeed ironic that many Southerners believed that Southern soldiers would be superior because they were healthy farm lads, not puny Northern factory workers. We discover from Hacker's dissertation that fetid urban factory conditions, by exposing future soldiers to disease, yielded some immunity to the diseases prevalent in Civil War camps.

Two of Hacker's studies look at the demographic consequences of the Civil War on the South. One asks whether the decimation of the male population led to a "marriage squeeze" for women; that is, did it make it hard to find a husband in the South after the war? The other asks what effect the war had on births in the South during and after the war. In both cases Hacker comes up with surprising conclusions. He finds that the horrendous rate of Southern war casualties actually had little effect on the age of Southern women at marriage or the likelihood that Southern women would never marry. The war did, however, have a large impact on births. Hacker finds that the shortfall of Southern births from their trend level (as a fraction of the expected number of births) was almost double the percentage shortfall in the North and larger than previously estimated.

Both of these studies are largely descriptive. That is, there is an air of Joe Friday about them—"just the facts, ma'am." Hacker often has tantalizing speculations about why various things occurred. For example, he hypothesizes that the female marriage age did not change after the war because age differentials between men and women may have grown or the remarriage rate for men may have risen. I have to confess that I would have liked to see him write fewer papers and pursue some of these hypotheses more seriously.

A by-product of Hacker's analysis of birthrates is that he is able to use survival analysis to estimate the degree of census under-enumeration in various years. For a century now scholars and census officials have believed that the 1870 census greatly underestimated the Southern population and so a correction factor of nearly 10 percent has been added. The microdata Hacker uses suggests that there was some under-enumeration in the South in 1870, but it was not nearly as large as previously thought. Indeed, he finds that the truth is probably closer to the original published count than to the adjusted version. This by-product

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of Hacker's study is probably more significant than his finding about births. For example, one can only wonder what impact these new population estimates may have on our estimates of per capita Southern output. They could have a fundamental effect on the whole debate about postbellum Southern stagnation and decline.

The last two chapters of Hacker's dissertation deal with the decline in fertility in the nineteenth-century United States. A well-known fact is that American fertility declined steadily from 1800 to 1900. This finding is confirmed by Hacker's analysis of microdata. But Hacker is able to plumb this change more deeply. Using own-child estimates of fertility, he finds little evidence of stopping behavior (parity-dependent birth control) before 1850. This finding raises more questions for me than it answers. For example, if it was not stopping behavior, what allowed the decline in fertility before 1850? What changed in 1850? Hacker has some tantalizing speculations that I would love to see him pursue.

In many ways Hacker saves his best work until last. The final chapter analyzes the role of culture in determining marital fertility. Economic historians often attribute the decline in marital fertility to economic determinants such as land availability and income. But, it is certainly possible that cultural factors, such as religion, may have been important. Hacker implements a clever test of whether religiosity might be key. He uses the proportion of children's names in a family that are biblical as a proxy for religiosity. In essence, he tests whether couples that name their children after biblical figures have higher fertility than those who do not. Hacker is well aware that his independent variable is imperfect. But he is very persuasive that people in the mid-nineteenth century were actively choosing names rather than following tradition. He marshals a plethora of primary sources to show that the use of biblical names did reflect the religious convictions of the parents.

Hacker finds that religiosity had a large impact on marital fertility. In regressions with controls for region, literacy, husband's occupation, and many other factors commonly thought to be correlated with fertility, the prevalence of biblical names had a positive, statistically significant, and quantitatively important impact on fertility. This finding surely moves our priors toward thinking that social and cultural changes, such as declining religiosity, may have been important forces behind the demographic transition of the nineteenth century.

Let me turn next to Marianne Ward's dissertation. Comparative estimates of the behavior of real output per capita over long periods play a central role in shaping our views of the process of economic growth. They provide critical evidence concerning whether followers tend to catch up to leaders; whether there is overall convergence in real incomes; what countries were the world leaders in real income in different eras; and when the United States became the world's leading economy and how its growth performance compares with that of other countries.

The motivation for Ward's dissertation is the observation that almost all comparative long-term estimates of real output rely on the same methodology. As Ward ably explains, this methodology involves two steps. The first is to obtain comparative estimates of real output per capita for a base year. This is done by taking standard estimates of GDP per capita in domestic currency units and then adjusting them for differences in the currencies' purchasing power. The base year is typically fairly recent. The second step is to project the estimates of per capita real output for each country backward using conventional estimates of the country's real growth; these estimates are typically those produced by the country's own statistical agency. This is the methodology underlying Angus Maddison's widely used comparative estimates.

Ward points out that this methodology has three major drawbacks. First, small errors in estimating growth rates, or small conceptual differences in the measurement of real output, will compound over long periods to produce large differences in the estimated levels of output per capita. Second, the base-year estimates are calculated using prevailing international prices in the base year, while the backward projections are performed using each

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country's prices. As a result, the estimates of real output per capita for different countries prior to the base year are not comparable: different weights are implicitly being used in different countries. And third, in cases where the domestic growth rates are not based on chain-weighting, the estimates implicitly put considerable weight on products, notably services, whose relative prices have risen over time.

Motivated by this observation, Ward sets out to construct more useful comparative long-term estimates of real output per capita. Her dissertation is a perfect illustration of the adage that genius is one part inspiration and 99 parts perspiration. The inspiration is the realization that comparative long-term estimates constructed using benchmark comparisons in multiple years are likely to be much more reliable and useful than estimates based on the traditional methodology—especially if some of the benchmark years are early in the sample. The perspiration begins with scouring the historical and modern literatures and the archives for data that can be used to construct estimates of currencies' relative purchasing power at different times. The range of sources Ward's hard work has turned up is remarkable: from the 1874 report of the Massachusetts Bureau of Statistics of Labor, to the 1914 South African Economic Commission Report, to work by the Ford Motor Company in 1930, to the modern International Comparison Project. She then carefully and painstakingly uses the raw data to construct estimates of overall purchasing power, and combines them with nominal GDP estimates, to create new comparative estimates of real GDP per capita at different times.

This detailed examination of the nitty-gritty of the data turns out to be crucially important. Even for a year as recent as 1950, Ward's approach yields estimates for several countries of real GDP per capita relative to the United States that differ by more than 20 percent from the standard estimates. When we go back to the beginning of the sample in 1872, in several cases the differences are more than 50 percent. And in the case of Italy, Ward's procedure leads to a revision in our estimate of its GDP per capita in 1872 relative to the United States of an astonishing factor of three. As Ward shows, these revisions significantly change the evidence about a wide range of important issues, including when the United States overtook the United Kingdom as the world leader in real income per capita and the overall performance of the United States relative to the major European economies.

Ward's analysis is so persuasive and her results are so important that I find it hard to understand why no one followed her approach sooner. But, I can only say that I am glad Ward finally did. I have no doubt that her estimates will become a crucial input to future studies. She has given economic historians a great gift—better data on a crucial topic.

Nevertheless, I would like to close with a complaint that is easy to make: I wish that Ward had done more. I mean this in two respects, one minor and one not-so-minor. The minor respect is that I wish she had gone further in investigating the implications of her new estimates. For example, her analysis of catch-up does not go much beyond a few summary statistics. It would not be terribly hard, and it would be very instructive, to run some simple statistical tests. The places to start would be a basic Baumol-style convergence regression and a slightly more complicated De Long-style procedure that accounts for the fact that even the improved GDP estimates still suffer from some error. I would have liked to know what results these procedures produced when applied to the new data, and how the results differed from those that are obtained from the standard estimates.

The not-so-minor respect is that I feel that Ward has stopped just short of putting the last nail in the coffin of the traditional methodology. Her approach is logically compelling; she marshals a convincing array of evidence that the price indexes underlying her estimates are reliable; and she shows persuasively that auxiliary evidence about other countries' economic performance relative to the United States is much more consistent with her estimates than with the traditional ones. But the nail that is missing is an explanation of how the traditional approach can have gone so far awry. An error of 20 percent in the estimate of a country's income relative to the United States 40 years before the base year requires a

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misestimate of half a percentage point per year of the country's growth rate relative to the United States. And an error of a factor of three in the estimate of Italy's income relative to the United States in 1872 requires an error of a full percentage point in the estimate of its average annual growth relative to the United States. Such errors are a great deal larger than I would have expected from the traditional approach. I hope that as she pursues her work further, Ward will delve into the gory details of the traditional estimates, at least for a few crucial countries such as the United Kingdom and Italy, and determine why her approach and the traditional one produce such different results. In the meantime, I would like to congratulate her for a major contribution to our understanding of the economic performance of the United States and other leading economies over the past century and a quarter.

Let me turn, finally, to William White's dissertation. White provides a superb analysis of the adoption and contribution of the farm tractor—in his words, the unsung hero of twentieth-century American economic growth. White's analysis draws heavily on two of the most significant works of the New Economic History: Robert Fogel on the social savings of railroads and Paul David on the diffusion of the reaper. Yet, White invigorates these well-known approaches with fresh insights and analysis, and reaches conclusions that are at the same time both believable and amazing.

The first half of White's dissertation is a social savings calculation of the impact of the tractor. He formulates the question this way: what would have happened if in 1954 the social planner had come and bought back all the tractors and erased the knowledge of how to make them? Using evidence on the stability of capital per acre, White argues that farmers would have had to use all the proceeds from this hypothetical sale to buy draft animals. Therefore, the question becomes what quantity of additional inputs would be needed to yield the level of food consumption we actually had in 1954. This will be an estimate of the social savings due to the tractor.

White's finding is that we would have needed many more workers and acres under cultivation to produce 1954 consumption with horses and mules rather than with tractors. There are two basic reasons for this. First, it takes a lot more workers to do a given amount of work using animals rather than tractors. Second, horses and mules eat a lot. Therefore, we would have had to produce dramatically more food to have the same human consumption. White calculates that the social savings of the tractor in 1954 were roughly 10 percent of GDP—more than twice the social savings Fogel calculated for the railroad in 1890.

White's analysis is extremely detailed and complicated. But he spells out his assumptions clearly and inspires great confidence. You just feel certain that anyone who has read every report ever published by the USDA on what horses eat did his accounting right. Even so, I would have liked to see some rough back of the envelope calculations. Something along the lines of, it takes x acres and the labor of y workers to feed a horse, so replacing three horses with one tractor will save z 1954 dollars. This would have given me both more understanding of where his results come from and even more confidence in them.

On the issue of explaining where his results come from, White does make what I found to be a profound point. How is it, he asks, that the lowly tractor was much more important than the mighty railroad? His answer is that water transport was a much closer substitute for rail transport (at least in the United States) than animal power was for tractor power. The clearest evidence of this is that many goods are still transported by water, but nobody in his right mind still uses a mule to pull a plow.

Another strength of White's work is his careful sensitivity analysis. He is persuasive that his analysis understates the importance of the tractor. For example, he assumes that mechanization had no impact on yields per acre. But tractors almost surely allowed fields to be planted and harvested more quickly and at more opportune times. Indeed, White is so convincing that he has understated the contribution of the tractor that I would have liked to see his best guess of the true social savings. Was it just a little more than his baseline 10 percent, or was it closer to 15 or 20 percent of GDP?

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The second half of White's dissertation deals with quality changes and the slow diffusion of the tractor. White does a masterful job of deriving a quality-adjusted price index for tractors for 1918–1955 using hedonic price regressions. Whereas the real price of tractors unadjusted for quality changes was roughly constant for the first 20 years after World War I, White finds that the quality-adjusted price plummeted. In this derivation he has again marshaled a marvelous variety of unusual sources, such as collectors' guides to antique tractors and the annual results of the University of Nebraska's tractor trials.

White then tests whether this falling price can rescue threshold models of the diffusion of the tractor. A recurrent finding in the literature is that threshold models suggest that tractors should have diffused much more quickly than they did. White finds that taking into account the falling quality-adjusted real price gives a slower decline in the threshold size. Thus, he argues his hedonic adjustment can reconcile actual diffusion and the threshold model.

I have to confess that I find this chapter of White's dissertation the least persuasive. The truth is all threshold models seem to me to put too much emphasis on farm size. Farm size is clearly an important variable—but other things surely matter. Some obvious examples are soil type and texture, the ability to share farm implements with neighbors, and the farmer's bodily strength and felicity with machines or animals. It seems to me that White limits the usefulness of his contribution here by tying it to a threshold model. Almost any model of diffusion will find diffusion rising when the real quality-adjusted cost of the technology falls. By showing us the dramatic change in the effective price of tractors, White builds a prima facie case that it is no surprise that diffusion was gradual rather than immediate.

The other thing that struck me was the fundamental similarity between White's finding and Olmstead's finding about the gradual diffusion of the reaper. White emphasizes how his results go against Olmstead's critique of threshold models of the reaper. But the thing I remember most from Olmstead's work was the tremendous changes he documents in the quality and usefulness of reapers. Well, that is much of what White finds about tractors—they improved greatly over their first couple of decades of availability. This fact, not the somewhat limited and artificial threshold calculation, is the crucial insight of White's analysis of diffusion.

Although I have tried hard to think of at least one piece of constructive advice for each of the finalists, I hope that my admiration for all three dissertations is obvious. Based on these fine dissertations and the other excellent submissions, I can honestly say that the field of American economic history is not merely alive and well, it is truly flourishing.

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Comments on Dissertations Selected as Finalists for the Gerschenkron Prize

I feel especially honored to be handling the Gerschenkron prize for these meetings on the American Century because I have concluded that the Gerschenkron prize is a quintessentially American beast. This may seem an odd statement; the prize is named for a man who was born in Russia, trained in Austria, and spoke more than a dozen languages. None of these traits sound quintessentially American. But the prize we have named for him reveals some profound national characteristics. To see my point you have to recall that a dissertation is eligible for the Gerschenkron prize only if it is not eligible for the Nevins prize. We often say that the Nevins prize is for North American economic history, but if you check the precise definition it is really for the economic history of the United States and Canada, thus illustrating our characteristically weak grasp of geography. North America, my encyclopedia tells me, includes all the lands north of the Isthmus of Panama. There is a very

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large country well to the north of Panama and just to the south of the United States, but for some reason dissertations on this country do not qualify for our prize in North American economic history. But the most revealingly American feature of the Gerschenkron prize is the way it is defined as "not American." The prize is not for European, or Latin American, or Asian economic history; it is for the rest of the world, or as one of my uncles would put it, "them foreigners over there."

As the person entrusted with reading the dissertations submitted for this year's Gerschenkron prize, I am grateful for the way these prizes have been defined. The "rest of the world" is a fascinating place, and it receives the attention of some first-rate scholars. I received nine submissions. Two were on Mexico as it is now defined. Another was on California while it was still part of Mexico. This raises another eligibility issue that I will leave to others to sort out. In addition to those two submissions and to the dissertations under discussion today, there was one each on Argentine public finance, on post-World War II British military expenditure, on French stock markets, and on the agricultural revolution in Britain. This diversity signals a welcome breadth and vitality in our field.

Yadira González De Lara's dissertation focuses on the shifting of contract types in Venice during the commercial revolution, here roughly the eleventh through the thirteenth centuries. Scholars have long noted that growth of trade in this period is surprising, not least because it took place in an environment of extreme political fragmentation, poor communication and transportation technology, risk from pirates and other sources, and a lack of international legal enforcement mechanisms. We know from the work of Avner Greif and others that one way in which traders overcame these problems was by developing private-order institutions that gave their members the ability to commit to honest conduct. But this was not always necessary, as Gonzalez shows for Venice. Venice in this period was a city-state with a substantial and growing empire of its own, plus the military and economic power to demand respect from other powerful entities. Gonzalez begins by showing that Venice's coercive power extended beyond its own boundaries, enabling parties to write contracts that they could expect Venice to enforce. (The enforcement organs were not just the state narrowly defined, as Gonzalez notes.) Traders under the protection of Venetian law could travel with large, well-protected fleets, and thus reduce the risks inherent in sea voyages. Venetians also enjoyed commercial privileges in other places and enjoyed some protection from capricious behavior by foreign rulers. Any trader who tried to evade Venetian sanctions would lose those privileges. Venetian protection and privileges, and the fear of losing them, made it possible for traders to commit to honest behavior.

So the constraints on contracting were not those implicit in a defective legal system, which is the case in Greif's work on the Maghribi or the merchant guild. The constraints in this dissertation are two of those that form the heart of the Optimal Security Design literature today: investors cannot verify the returns to a project at zero cost, nor can they always get back everything they invested if the project fails. Gonzalez follows the modern literature in assuming that both investors and entrepreneurs would like their contracts to incorporate an element of risk-sharing. Gonzalez's theoretical model asks a deceptively simple question: given these constraints, what would be the optimal way to finance a trading voyage from Venice? How much capital should be provided by the trader, and how much capital by investors? What kind of contract should it be: more like debt, or more like equity? The first part of the dissertation devises an abstract model that incorporates stylized versions of the constraints imposed by the historical situation. This model has several appealing features, one of which is the testable implication that a change in the information available to the state implies a switch from debt contracts (in this case, Sea Loans) to equity contracts (the *commenda*).

The second part of the dissertation then uses some 1,000 Venetian notarial documents from the eleventh through mid-thirteenth centuries. This empirical section has a two-fold purpose. Gonzalez uses example agreements to defend the way she has characterized the various contracts in her theoretical discussion. She then uses the notarial documents to test

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two implications of her model. First, as Venice became more organized and powerful, it was easier for its coercive organs to verify what actually happened on trading voyages. This amounts to a reduction in the costs of state verification in the model, and implies a shift from the Sea Loan to the *commenda*. This shift, in fact, took place. Second, Venetians who resided outside the city were less able to verify to Venetian authorities what happened to their ventures. The model predicts a greater and more prolonged use of the Sea Loan for these ventures, and Gonzalez's notarial data again confirm this implication of the theory.

One welcome feature of Gonzalez's dissertation is that she overcomes a failing in much of the current work on the economics of institutions. This literature is good at constructing a model that has as an equilibrium that looks something like the historical institution in question. This literature is not very good at using the model's testable implications to convince us that this is the *right* model, which to my mind is basic to empirical economics. I have done some work myself in this area, and I know that it is as hard to test these models as it is easy to criticize others for not doing so. Gonzalez counts as a forerunner for her successful transcendence of this limitation.

In revising this work, I would suggest greater attention to an issue that she considers only briefly. Her model focuses on the costs of state verification, and in so doing rules out the other two common information problems, adverse selection and moral hazard. She provides two justifications for this simplification. She notes that expanding her model to account for adverse selection and moral hazard would make it intractable. This may be right, but it is the sort of justification that means more to theorists than to economic historians. I suggest greater emphasis on the second justification she offers, which is empirical. She says that given the institutions and circumstances neither adverse selection nor moral hazard were serious issues. I am not entirely convinced on this point, but with additional evidence I could be.

Aurora Gómez-Galvarriato Freer's dissertation challenges received interpretations of the impact of the Revolution on industrial workers and the role of industrial workers in the Revolution. At the heart of her study is a very careful reconstruction of the histories of two textile firms (or conglomerates), the Compañia Industrial Veracruzana (CIVSA) and the Compañia Industrial de Orizaba (CIDOSA). Both of these companies were in the Orizaba valley, in the state of Veracruz. Many of CIDOSA's records were declared off-limits, but Gómez was able to use CIVSA's records to compile remarkably detailed and complete histories of employment (wages, working hours, and so on) and to examine the firm's balance sheets. Some of her most revealing material comes from memos and other narrative accounts that reveal how these two firms thought about the problems they faced from labor unrest and unionization, government policy, and revolutionary turmoil.

Gómez asks how workers at these two textile firms fared during and after the Revolution, and how the labor agreements struck during the Revolution affected the firms' later profitability and growth. To make sense of these questions requires a great deal of context, however, and along the way we learn much about the creation of the Mexican textile industry in the early nineteenth century and the way the various factions in the Revolutionary period dealt with management and labor. In the study's heart she estimates a new price index and then uses that price index to study the behavior of real wages over the period in question; looks at changes in nonwage aspects of economic well-being, such as public goods; examines the firms' balance and profit-and-loss sheets to see how the Revolution and labor agreements affected their profitability; and examines the productivity of these firms and the Mexican textile industry more generally to understand how the long-term impact of the Revolution shaped Mexican industry's ability to compete with other textile producers.

This dissertation is remarkably ambitious and complete. Many would be content to examine these issues from the viewpoint of the workers (thus writing a "labor history"). Others would restrict their attention to the firm and its managers (thus writing a "business history"). Gómez gives us both sides, and in so doing helps us to see the constraints affecting each side and the way agreements struck at one stage led to changes in the organization

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of work that in some cases undermined the union's original intentions. Few dissertations that deal with microlevel data of this scope and detail would also turn to newspapers and other contemporary sources to give us a richly detailed political and social history of the region and the Revolution. By doing so, Gómez not only identifies the features of the Orizaba region that were unusual, she places the struggles in these few textile factories in their larger political context.

The challenges for Gómez in continuing this project are implicit in the dissertation's scope and ambition. Although 740 pages long, there are instances where a more extensive discussion and defense would pay off. One example comes from her estimates of cost-of-living deflators, which are obviously crucial to her findings on real-wage growth. She shows that real wages did not improve for CIVSA's workers until the 1920s, but did increase by about 130 percent during that decade. She has two sets of consumption weights, one from 1914 and one from 1930. These two sets of weights imply enormous changes in consumption patterns in a period of 16 years. To some extent the changes are sensible (as the workers became wealthier they spent relatively less on food) but the changes are so large that they warrant a bit more attention. Another kind of example comes from her tantalizing discussion of international productivity differentials and the way the unionization of Mexican textile workers impeded adoption of new machinery and techniques in Mexico. Gómez tells us a bit about the labor agreements and how they constrained adjustments to, for example, capital-labor ratios, but much of this discussion is too cursory to forestall the objections and questions many readers will have. I know that her advisers would groan to hear someone tell her that 740 pages is not enough, but to develop the full promise of this outstanding research she will have to find ways to economize on some discussions and leave more room for others.

David Beck Ryden's dissertation marries an exploration of Jamaican plantation society in the second half of the eighteen century with an economic analysis of the sugar economy and the demise of slavery. Although not narrowly focused, the project's underlying concern is Eric Williams's "decline thesis," the claim that Britain's abolitionist movement succeeded because the economic foundations of slavery in Britain's colonies were, by the early nineteenth century, weak. The heart of Williams's claim is that slave holders were by nature conservative and opposed to technological change, making them unable to weather the storms of new techniques and markets. Ryden takes a "bottom up" approach, immersing himself in a wealth of archival materials on Jamaica and its slave economy. This, to my mind, is the best way to confront Williams, because only at the plantation level can we compare his image of the slave holder to the historical reality. Ryden's dissertation falls naturally into two parts. The first is a careful and rich description of plantation life, focusing on the experiences of the enslaved people and how changes in the larger economy altered their lives. The second part takes several approaches to addressing the Williams thesis. This is a nuanced and thoughtful account, and in the end Ryden concludes that, in the form Williams stated it, the thesis is wrong. Nothing in the Jamaican experience supports the notion that a slave-based sugar economy had long-run weaknesses that implied its eventual demise.

To focus on the Williams thesis is to risk doing this dissertation an injustice, because it is much more comprehensive than any single-minded pursuit of a by now well-worn hypothesis. Scholars of slave economies in any context will appreciate Ryden's accounts of plantation life, and his emphasis on problems of plantation management and the ingenuity of enslaved people in trying to enhance their position forms a useful corrective to earlier discussions. But it is in his pursuit of the Williams thesis that this dissertation really shines. He demonstrates that Jamaican slave-holders were not the conservative, hidebound aristocrats that Williams asserts. They were acutely aware of developments in other Caribbean sugar producers, and strove to import productivity-improving methods whenever possible. Concrete technical changes include new varieties of sugar, new plowing methods, increased use of irrigation, and dozens of smaller changes. If anything, Ryden does not adequately stress that his documentation of this deep concern with increasing

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productivity is, by itself, a refutation of the reasons Williams advanced for the putative decline of the slave economies.

Ryden also takes a more familiar approach, examining several economic indicators during the late eighteenth and early nineteenth centuries for signs of ill-health in the slave economy. Here he is on familiar ground, but his discussion of productivity, slave prices, slave emancipation, and other indicators of slavery's vitality is careful and makes excellent use of the sources available. One weakness in this discussion is reliance on crude measures of output per slave and similar indicators. Output rises and falls dramatically over the late eighteenth century. Surely this implies some changes in land/labor ratios, and with that changes in output per worker that have little to do with long-run changes in the economy's health. His discussion of new techniques also implies other forms of factor substitution. Given the detail he has for at least some plantations, it would seem possible to construct estimates of changes in total factor productivity. This is the right measure, after all, and even if its construction requires some assumptions it would be worth seeing how total factor productivity measures compared to the per capita output measures.

I have one more suggestion for these dissertation-writers. Each of these dissertations relies on a case study, and when using case-studies comparisons become critical. The best way to refine and strengthen your own findings is to understand how the issues you study were different elsewhere. To return briefly to Ryden's dissertation as an example, the Caribbean had other islands where sugar was worked by kidnaped Africans. Some explicit comparisons based on secondary sources would flesh out some of his own argument and help us distinguish between what was Jamaica and what were larger economic forces. I realize this requires more work, and I can hear Gómez's advisors groaning again, but I suspect that all three of these dissertations would be improved with a larger perspective.

In closing I would like to note that all three of these dissertations share an important virtue. From time to time there is an outbreak of worry about whether a single profession can adequately respect the distinctive concerns and contributions of those trained in history departments and those trained in economics departments. These three dissertations show that these worries are exaggerated. All three of these scholars display virtuosity with the defining methods and concerns of their original department, be it economics or history. But each has gone beyond this origin to acquire and use the wisdom of the other side. Gonzalez, an accomplished economic theorist, incorporates a thorough and careful use of notarial documents. Gómez, an historian, made a serious contribution to understanding changes in the well-being of workers, and did so relying on analytical tools that rarely appear in history journals. Ryden, who is equally well informed on his period in Jamaica's history, also turned to the tools of economic analysis to refine and test the basic idea of the Williams thesis. Any specific finding or accomplishment aside, it is impressive and reassuring that these three new Ph.D.s show no qualms about dealing with both the noun and the adjective in their new profession's title.

We all like to have students because the best of them teach us more than we teach them. These three young scholars are no longer students, but I am sure they taught their advisors as much as they have taught me. We are much richer for their choice of profession, and I would like to welcome them to our ranks.

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