Summaries of Doctoral Dissertations

Grain Markets, Food Supply Policies, and Living Standards in Colonial Mexico

This dissertation is a political economic history of access to food in central Mexico in its critical late colonial era (ca. 1700–1821). In this preindustrial, predominantly peasant society, with strong geographic fragmentation, access to food of different social groups significantly changed in connection with greater market integration and trade regulations. Over this period, the nutritional status of the urban population experienced gains relative to the rural population as trade became a more pervasive means to secure the access to food. Market integration allowed cities to leverage higher purchasing power and institutional resources. But also critical was the support of the colonial government, which deemed administrative centers and mining towns key to the survival of the colonial regime. In the end, grain markets mattered not only in terms of welfare, but also as a tool of empire.

The late colonial era in New Spain was characterized by population growth, urban expansion, and increased regional specialization. These trends, which gained greater force in the 1780s, led to a deepening commercialization of food production and acquisition, while subsistence agriculture receded, and the price of food and land soared. Many indicators point to increased inequality in income and in the access to land. Famine broke out in 1785 and 1809, causing death and a rethinking of food supply policies. These were also years of important administrative, economic, and fiscal reorganization, usually referred to as the Bourbon reforms, that sought to strengthen the metropolitan control of the colonies and make the extraction of revenue more efficient. As generalized rural insurrection broke out in the 1810 and the colonial government moved back and forth in the implementation of liberal reforms coming from Spain, fights intensified on peasant autonomy, land property, and the organization of economic and political life. Throughout this period, the balance of economic and political power between different social classes, as well as between cities and countryside was dramatically changing, and trade became an increasingly important way to distribute the allocation of the means of subsistence.

Late colonial Mexico is an ideal case to understand the importance of access to food and grain markets in preindustrial societies. Historians have debated about whether, before the arrival of fast and cheap transportation and improvements in agricultural yields, the climate was determinant of the amount of food production that was available to the population; others, instead, have pushed an approach that emphasizes the distributional and institutional factors in the acquisition of food. Late colonial Mexico, with its fractured topography, slow and costly transportation, and undercapitalized agriculture is a good test case to assess these different approaches to hunger and famine in history. Moreover, this case allows studying the boundaries of arguments that colonialism, by focusing on the extraction of colonial benefits to the metropolis, was not accountable to guarantee the access to food of the native population.

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Chapter one provides a long-run perspective on the trajectory of living conditions from the 1730s to the 1830s through the use of adult heights, an indicator related to conditions of nutrition, ecological factors, disease environment and child-care practices in the early years of life. In total, about 4,000 cases were used, comprising urban and rural dwellers, unskilled, semiskilled and literate workers, different complexions, and regions. Average height declined steeply around the mid-eighteenth century and then heights stagnated; as a result, height significantly declined one centimeter over the period, and there was no rebound after the rural insurrection in 1810. More importantly, height gaps between different sectors of the population changed. The height gap along socioeconomic lines expanded over the eighteenth century and continued into the postindependence era. Finally, rural heights declined while urban heights remained stable; while rural soldiers born in 1750 were significantly taller than urban soldiers, by the late eighteenth century and early nineteenth century, the difference became insignificant. The analysis of heights suggests that, although social inequality increased, nutritional status improved, in relative terms, in the cities by the end of the colonial period. I argue that this relative gain was related to increasing advantages in the access to food of the urban population. This thesis serves as the background for the analysis of grain markets and regulation policies in chapters two to four.

Chapter two argues that urban grain markets throughout central Mexico were more competitive than usually believed. Through an examination of legal and administrative conflicts, I find that, while large landowners (called the *hacendados*) held great power in local town politics, their position did not give them a total control of the local marketplace. Groups with conflicting interests, such as manufacturers, miners, and the highest authorities, were gaining power and privileged the ample supply of the cities. Moreover, mid- and small-scale traders competed in the official grain exchange and in street stalls and booths. The daily shipments of corn to San Luis Potosí in the 1770s further demonstrate the importance of the mid-scale merchants in supplying the city in times of high prices. These merchants, who predominantly came from distant markets, supplied more than half of all corn traded in the official market in years of shortages. Only when prices were low, the local *hacendados* and (even more) tithe collectors provided the majority of corn consumed in the city. I argue that this competitive nature of local grain markets stemmed primarily from technical limitations in the capacity to store corn, the staple in Mexican diet, as well as from grain trade regulations that discouraged investment in storage. These limits provided an incentive to market integration by relying on substitute products, such as wheat, and distant supplying areas.

In the third chapter, I study long-distance corn and wheat trade and assess the hypothesis that local grain markets were becoming more integrated. My argument stands in contrast with the prevailing view in the literature, that high transportation costs and geographic barriers made corn supply (and to some extent the whole grain supply) locally bound, and hindered the potential of market integration. The chapter tackles this issue in three steps. First, I deal with the key actors involved in regional grain trade: city commissioners, tithe collectors, large landowners, and mid-scale itinerant merchants. I show that the latter were major articulators of long-distance networks, buying at the large landowners' storehouses and small marketplaces and taking them to consumer markets. Second, I show that corn and wheat supply areas of select cities in central Mexico involved longer distances than usually believed (from 100 to 200 miles in the case of corn). The supply areas overlapped one with another,

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suggesting an articulated economic space. More importantly, the supply areas were expanding over the eighteenth century, the most important evidence coming from the mapping of shipments to Mexico City flour mills from the 1690s to the 1800s. Third, I analyze four city corn price series in central Mexico to gauge the degree of integration. Price dispersion of the four markets slightly declined over the period. Other measures give stronger support to the hypothesis of integration. The correlation of price differentials in the four markets improved after 1780, the coefficients being comparable to international cases of integrated markets analyzed in the economic history literature. Finally, the price series show a significant convergence toward equilibrium relative prices, and the adjustment became faster after 1780, again pointing toward a process of market integration in late colonial Mexico.

This emergent process of market integration in central Mexico entailed a redistribution of economic and political power between city and countryside. For a consumer area such as a city, well-oiled trade networks were critical to secure the food supply and keep prices checked. For a producer district, having the possibility to export the production raised the price and stimulated production, but it also affected the entitlement of impoverished communities who relied on the market to procure food. Market integration, in this sense, was not a mechanical process. It entailed a dispute over who had rights to harvests and stocks that depended on the political economic balance between different sectors. This type of disputes and policies on regional grain trade is the subject of chapter four.

Regional trade policy traditionally followed a local-control model: different administrative instances had jurisdiction to control trade in their territory. With the exception of the viceroyalty's capital and a few mining centers, the local magistrates had almost unchecked control to decide whether grain could be removed from the jurisdiction or not. As in the case of eighteenth-century England famously analyzed by E. P. Thompson, local authorities used this power in times of local or generalized dearth, generating conflicts with potential claimants of food from other jurisdictions. Trade restrictions elicited protests from local producers and buyers from other districts that reached the viceroy's office and provide a rich perspective on the moral economic conceptions of the time.

By the 1770s and 1780s, the policy shifted toward more central control and a pragmatic application of liberal ideas. Central authorities in Mexico City became more involved in the process of regulating regional trade and claimed the power to authorize trade bans at the local level. In some cases, the viceroy repealed trade bans and criticized measures of this type on grounds of modern political economic ideas. To some extent, this move toward centralization and free trade responded to legislative landmarks, but more important was the authorities' pragmatic reaction to the acute shortages of 1785 and 1809. By contrast to previous crises of similar magnitude, the viceroys reclaimed a central role by soliciting detailed reports, overseeing local conditions, ordering generalized bans on interjurisdiction trade, and actively participating in the relief measures. By the 1810s the policies shifted more pronouncedly toward the liberalization of grain markets in a time of pronounced political upheaval.

Chapter five traces public and official discourse on grain markets in the eighteenth and early nineteenth centuries, with a focus on the grain monopolist, a figure that reveals desirable or undesirable characteristics of economic organization. For most of the eighteenth century, popular misapprehension against monopolists was matched by

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indifference among the elites in the most important cities. The great famine of 1785 marked a noticeable shift in official and public discourse on grain markets, as viceroys, bishops, judges, and intellectuals as well as local authorities began using a discourse that blamed monopolists for the scarcity and altering prices. In contrast to previous interpretations that take this discourse as proof that grain merchants could effectively control grain supply, I argue that the emergent monopolist discourse has political motivations. For reformist bureaucrats, the denunciation of grain hoarding during the 1785 famine was a way of reasserting royal administrative power against powerful local actors. For the church authorities, the discourse against market abuses was a way to sustain the privilege of the church over tithed grain and its imperiled political standing in the colony. Rural insurrection and the advance of the Cádiz liberal project in the 1810s were also followed by a significant change in the discourse about grain markets and perceived abuses. While in government proceedings, market liberalization became a frequent theme, the most interesting aspect was the reinterpretation of liberal discourse among popular writers of pamphlets. Their attacks against monopolists were virulent and relied on a vivid imagery designed to appeal to various urban elements exasperated by the continual, extraordinary difficulties of their lives, especially in food supply, between 1809 and 1821. Ultimately, their denunciation of market abuses and the inaction of authorities delegitimized the colonial regime, by emphasizing the traditional commitment of government to secure the food supply of its urban constituents.

To conclude, this dissertation is an attempt to reclaim the centrality of access to food, grain markets, and their regulation as central elements in the well-being of the population and in empire building in the late phase of Spanish domination. This is the history of a dramatic shift in entitlements that benefited the cities, and the more affluent people in the cities. As the colonial regime reached its final stage, food supply became a highly political issue that pamphlet writers used to criticize the legitimacy of the regime, while for the viceroy it became a key to retain a hold of Spanish presence in Mexico.

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A Political Economy Approach to Bureaucracies

This dissertation aims to explain why the organization of some public administrations resembles that of private-sector *hierarchies*—where chief executives (e.g., politicians) are free to manage public employees—while others could be defined as *bureaucracies*—where politicians have delegated the powers to hire, fire, and promote public employees to autonomous bodies (e.g., *corps* of functionaries, Civil Service Commissions). Why organize public activity in bureaucracies (with civil service laws constraining politicians) rather than in hierarchies (where politicians are free to hire, fire, and promote employees and thus they are able to address the organizational problems of adverse selection and moral hazard)? If Ronald Coase asked the question *Why Do Hierarchies Exist,* given the fact that markets—according to neoclassical economy—should work efficiently, the question this dissertation addresses is *Why Do Bureaucracies Exist* if hierarchies—according to basic principal-agent theory—should work efficiently.[1] The dissertation consists of three parts: a political economy theoretical model, a statistical analysis of the hypotheses, and a qualitative analysis which provides the flesh-and-bone case histories designed to show the micro-foundations of the theory.

Theoretically, the paper explores what is the rationale behind the limits to the discretion of politicians for managing public employees. Following Murray J. Horn, it is difficult to believe that institutional arrangements that are so common and persistent, such as autonomous civil service systems, are a clearly inefficient way of addressing the problems faced by politicians.[2] It is also complicated to maintain that

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[1] "Nature of the Firm," pp. 386–405.

[2] *Political Economy of Public Administration*, p. 95.

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the reason for independent civil service systems lies in the type of activities governments undertake, since, firstly, not all administrations possess autonomous civil service systems, and, secondly, there are no government activities that have never been done by a private firm.[3] Even state core activities, such as tax collection and military defence, have been historically implemented by standard private-sector hierarchies in many countries. Therefore, there are reasons to think that there is something within the nature of political institutions that may explain the peculiar features of public bureaucracies. This dissertation explores those political factors that may lie behind the adoption of autonomous civil service systems. Similar to some political economy approaches (e.g., Howard Frant), this dissertation specifically analyzes the impact of the *forms of government* over the *administrative structure* of a polity.[4]

The study of bureaucracy has traditionally resisted theoretical progress because, first, it is a very complex subject with no natural focus for analysis and, second, the study of bureaucracies has been monopolized by organizational theories that—through emphasizing so much the inherent complexities of bureaucracies—resist building testable theoretical propositions.[5] As a result, studies of bureaucracy tend to be more descriptive than analytical and authors are prone to make more judgements than analysis. For example, the German autonomous civil service system is frequently explained as a "guarantor of the public good" and the French one by the "strong state tradition".[6]

In order to address these shortcomings in the literature, the dissertation proposes a testable political economy model. It is based on a double problem of trust in the relationships between politicians and public employees. First, there is a problem of *positive control*: politicians, like private-sector executives, must create incentives that inspire public employees to go beyond the required minimum levels of effort. Yet, in a standard hierarchy, public employees do not trust politicians when the latter are too decisive (i.e., powerful) and as a result they can easily renege on the promises given to public employees (e.g., economic rewards, promotions). Tying her hands through the establishment of an autonomous bureaucracy becomes a second-best solution for rulers who are too powerful for credibly promise rewards to employees that undertake costly asset-specific investments (e.g., studying civil engineering). Therefore, if concerned with this positive control, the more powerful a politician is, the more she will have to bureaucratize her administration—given that autonomous bureaucratic bodies offer more credible promises to employees.

Second, policy implementation involves as well a problem of monitoring or *negative control*. In this case, it is politicians who are the ones who do not trust public employees because the latter may shirk or pursue opposite interests to the former. As the principal-agent literature has pointed out, agents are abler to shirk when they are facing two or more principals, because they can play off one principal against the other; and, at the same time, the more principals, the longer it takes to undertake a sanctioning decision. That is, political systems in which politicians have more decisiveness are better equipped to prevent agent's shirking, because politicians can easily sanction agents' misbehaviour. What can non-decisive rulers do to prevent agents' shirking? It is contended here that bureaucracy represents again a second-best

[3] Wilson, *Bureaucracy*.

[4] Frant, "Rules and Governance," pp. 990–1007.

[5] Moe, "Positive Theory of Public Bureaucracy," p. 455.

[6] Goetz, "Development and Current Features of the German Civil Service System," p. 87; and Meininger, "Development and Current Features of the French Civil Service System," p. 189.

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solution. Bureaucracy is understood as a tool to achieve or "buy" public employees' loyalty. For a public servant, the costs of shirking against the incumbent within a bureaucratic administration (i.e., losing a secure tenure) are higher than without it (i.e., losing a temporary job). Here, the relationship between politicians' power and bureaucratization thus becomes the opposite: the less powerful a politician is, the more she will tend to "buy" public employees' loyalty through bureaucratizing the administration.

In sum, the level of concentration of powers in a polity has a dual effect on the degree of bureaucratization of a public administration. It has a positive impact when rulers' survival in office mostly depends on the positive control (e.g., on inducing employees to undertake the costly asset-specific investments required for providing public goods). On the contrary, one should expect a negative effect of the level of concentration of powers on bureaucratization when rulers' survival in office mostly depends on the negative control (e.g., on preventing employees from undertaking disloyal activities). The empirical tests of these hypotheses includes both *quantitative* as well as *qualitative* methods and the scope of analysis entails, on the one hand, *inter-* as well as *within-country* differences and, on the other, *contemporary*, as well as *historical*, variations.

Firstly, the dissertation provides a quantitative analysis with cross-country data on bureaucratization that contrasts these hypotheses for two data sets—one for developing countries and another for OECD ones. The analysis shows a general map of the situation. It demonstrates that there are statistical correlations among the variables and that they follow the theoretical predictions. For the countries where one may assume that rulers are mostly interested in providing public policies, there is a negative relation between the separation of powers of a polity and its level of bureaucratization. On the contrary, there is a positive relationship between the variables in those countries which have recently experienced civil wars—a variable used here as a proxy for rulers' concerns about employees' loyalty.

Within the same comparative section, the dissertation develops two observational implications of the theory. In the first place, keeping constant the level of concentration of powers (in particular, it focuses only on systems with very high concentration of powers such as authoritarian regimes), it asks what happens in terms of administrative design when autocrats have different survival strategies. For example, is the administration of a despot like Mobutu—who is mostly seen in the literature as interested in what here is defined as the negative control—different from the administration of despots such as seventeenth-century France's Louis XIV or twentieth-century Spain's Franco—who have been traditionally depicted as more concerned with the provision of public goods (i.e., the positive control)? Since Mobutu was primarily concerned about the negative control of his subordinates, his approach to the management of public employees was that of an extremely personalistic hierarchy. Quite the opposite, rulers who, like Louis XIV or the twentieth-century so-called developmentalist dictators like Korea's Park or Spain's Franco, were more interested in the positive control, tended to develop more autonomous bureaucracies.

In the second place, keeping constant the variable rulers' survival strategies, the goal is to see if changes in the degree of concentration of powers lead to variations in the level of bureaucratic autonomy. This section focuses on early modern European countries where, following the literature, one may assume that rulers were mostly interested in the provision of public policies.[7] *Ceteris paribus*, the European monarchs who had fewer constraints to their powers (e.g., France, Prussia, or Spain) were

[7] Bates, *Prosperity and Violence*.

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more inclined to create bureaucracies with autonomous (from rulers' interferences) administrative *corps* than the monarchs who were more severely constrained by parliamentary checks and balances (e.g., Britain or Sweden), who, quite the opposite, developed administrations closer to standard private-sector hierarchies.

In the second empirical section, the dissertation shifts its focus to the analysis of *cross-time* within-country differences in the OECD country that has experienced the highest variation in the independent and dependent variables of this study for the latest two centuries: Spain. The narratives of the Spanish case supply the causal mechanisms or *micro-foundations* that link the independent variables of the theory (survival strategy of rulers and separation of powers) with the level of bureaucratization. The dissertation focuses on ten main administrative reforms or periods of reform in the last two centuries of the Spanish history.

The analysis shows diametrically opposite administrative preferences of authoritarian and democratic rulers exposed to similar exogenous shocks. When the survival in office has depended on a larger extent in keeping employees loyal (e.g., periods of political instability, social riots, pre- and post-civil war), authoritarian rulers have relied on hierarchies where they (and their clique) enjoy a high degree of discretion—e.g., the nineteenth-century oligarchic system or the first periods of Primo (1923–1925) and Franco (1939–1951) dictatorships. On the contrary, when democratic rulers have faced the same kind of short-term threats their response has been exactly the opposite: the establishment (or enlargement) of extremely autonomous bureaucracies—e.g., the enactment of the 1918 Civil Service Act in the aftermath of the proto-revolutionary period 1917/18 in which many public employees were taking part.

When the survival in office has depended more on trying to induce employees to undertake costly asset-specific investments (e.g., large public infrastructure projects), authoritarian rulers have chosen an extremely autonomous bureaucratic structure —e.g., the "developmentalist" period of Franco's dictatorship (1951–1975), where administrative *corps* achieved one of the highest degrees of autonomy in the history of Western Europe. Again, the opposite is the case when we have democratic rulers. Elected incumbents, unlike the less-credible authoritarian ones, do not need to grant high levels of autonomy and they tend to move the administrative structure towards a more discretionary hierarchy that would allow them a more flexible policy implementation.

The dissertation provides a second within-country analysis—this time of the country with the highest *cross-sectional* variation: the United States. A large-N quantitative test with data for over 800 U.S. municipalities offers a unique setting for testing the theory. Firstly, proxies for the degree of bureaucratization can be built for U.S. local governments, because there are municipalities which have a Civil Service Commission (CSC) and municipalities which lack it. More importantly, there are municipalities where the CSC plays a minor role in the selection, firing and promotion of employees, and municipalities where staff policies are more extensively delegated (from the chief executives of the municipality) to CSCs. Secondly, the number of observations is larger than in cross-country comparisons, thus allowing a more comprehensive quantitative analysis. And thirdly, unlike cross-country comparisons, where the probability of omission of relevant variables is high, U.S. municipalities share a common denominator in cultural and historical terms.

The empirical contrast confirms the predicted relationship between concentration of powers and bureaucratic autonomy in a democratic setting, where rulers are mostly interested in the positive control of public employees. Municipal governments with more concentration of powers (mayor-council cities) exhibit higher levels of

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bureaucratic autonomy (more numerous and more active Civil Service Commissions) than municipalities with more separation of powers (council-manager cities). At the same time, this finding runs against the prevailing cultural view in the literature that contends that the establishment of administrations with Civil Service Commissions went *hand in hand* with the establishment of the Council-Manager form of government because both would be the result of the Progressive Movement at the beginnings of the twentieth century.

In sum, this dissertation complements transaction-cost economics (TCE) developments both theoretically and empirically. Traditional TCE assumes that "in the beginning, so to speak, there were markets".[8] From there, TCE basically tries to understand the shift from markets to hierarchies. This dissertation moves the initial assumption a step forward. It assumes that, in the beginning, states were organized as hierarchies. And it tries to understand why some states (or, more generally, why some polities) have remained closer to the hierarchical ideal while others have switched toward the bureaucratic one.

[8] Williamson, "Modern Corporation," p. 1547.

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The Yellow Non-Pareil: Industrialization and the Making of Modern Japan

The Meiji period (1868–1912) witnessed a remarkable transition for the Japanese economy, whose rapid development propelled the premodern agrarian nation to industrialized status. Under the banner of "rich country, strong military," the economy trebled in size between 1880 and 1913 while the military forces won unexpected victories against China (1895) and Russia (1905). Investment in roads, railways, harbors, and the telegraph system grew at 10 percent per year, and industrial output grew fivefold. Institutions developed apace with the establishment of a central bank in 1882, promulgation of a constitution along western traditions in 1889, and adoption of the gold standard in 1897. By 1899 Japan no longer had to recognize extraterritorial rights for foreign nationals, and the country regained full tariff autonomy in 1911. For most of the twentieth century, Japan was the only non-Western country to have industrialized, setting an example for its region and the developing world at large.

How did Japan overcome its late start? On the one hand, it has been argued that the government played a pivotal role in industrialization by seeding strategic sectors and providing both the infrastructure and institutions that would be the basis of subsequent development.[1] Consistent with theories of late development, leaders of the Meiji Restoration considered public intervention necessary to mobilize financial resources for broad-based industrialization and to mitigate the short-term risk of unfamiliar and costly investment projects. Some of the government's achievements include Japan's first modern manufacturing facility, the Tomioka Silk Filature; the first railway, between Tokyo and Yokohama; and the first domestic-built steamship in its Nagasaki shipyard.

Nevertheless, other scholarship suggests that despite the government's high profile accomplishments, its industrial impact is belied by the failure of many public enterprises and the withdrawal of direct investment in the early 1880s, predating Japan's first wave of industrial activity in the following decade.[2] Instead, some contend that credit belongs to the private sector, notably the large financial conglomerates known as *zaibatsu* that were thought to have led the introduction of foreign technology and industrialization throughout the economy due to their size, assets, private ownership, and diversified holdings.[3]

Does either narrative accurately describe the emergence of modern Japan? This is an open question because despite Meiji Japan's success as a late developer, little economic analysis has been done owing to a lack of detailed data. In the few instances where Meiji Japanese industrialization has been studied, the research relies on possibly unreliable anecdotes; national-level data that obscure regional and industrial variation; or case studies of specific firms and industries that may not be

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[1] See Lockwood, *Economic Development of Japan*; Morikawa, *Zaibatsu*; and Rosovsky, *Capital Formation in Japan*.

[2] Nakagawa, "Business Strategy." Ohkawa and Rosovsky, *Japanese Economic Growth*, p. 20, date Japan's initial period of industrialization between 1888 and 1897.

[3] Morikawa, *Zaibatsu*.

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representative of the economy. Little is known about smaller firms or less visible sectors that nevertheless contributed to industrialization, especially when compared to either the activities of the government or *zaibatsu*.

With this motivation, my dissertation comprises three studies about the nature of Japan's developing economy during the Meiji period, utilizing a newly created data set drawn from corporate genealogies. The first paper examines the role of *zaibatsu* in introducing foreign technology, comparing their performance relative to other private entrepreneurial firms. The second paper looks more generally at the relationship between financial intermediation and industrial startup to assess the possible influence of one on the other. The final paper considers the impact of the government's early industrial investments and whether there were fundamental differences between the sectors it pioneered and those started with private funding.

The research in all three papers has been made possible only with the development of a new data set of nineteenth-century Japanese firms. The primary data source is the *Shuyo Kigyo no Keifuzu*, a compilation of 1,089 firm genealogies edited by Japanese business historians, Shintaro Yagura and Yoshiro Ikushima.[4] While genealogies are typically overlooked for use in economic research, the modest amount of information they contain about firms is sufficient to analyze the emergence and expansion of new industries in Japan. Such data include a firm's date of establishment, industry, source of startup finance, and location, to which I add industrial classification codes and demographic and geographic information.[5] Taken together, these genealogies constitute the oldest evidence of Japanese firm activity across the industrial spectrum.

With this data set, I am able to analyze some long-held views regarding Japan's industrialization that thus far have relied on anecdote and case study. For example, it has been asserted that *zaibatsu* conglomerates, through their advantages of size, investment autonomy, and diversification, led the introduction of foreign technology and industry formation during the Meiji period.[6] Nevertheless, this claim has been unverifiable due to the absence of records from their smaller, contemporaneous rivals. Using genealogical data, I am able to test whether affiliation with one of these conglomerates increases the likelihood of being an industry pioneer. To motivate my analysis, I use a new game-theoretic model of firm entry based on an earlier model of technology adoption.[7] My model accounts for firm and industry differences in the firm's profit function, from which I derive a mixed-strategy Nash equilibrium that predicts a greater likelihood for conglomerates to enter riskier industries. I confirm this with results from a cross-sectional probit model, where I regress the indicator variable for being an industry pioneer on firm affiliation, ownership, diversification, and other variables. In particular, I find that *zaibatsu* leadership manifests strongly in capital-intensive sectors and owes largely to their diversification, which suggests the existence of capital market failure and investor risk aversion.

Zaibatsu leadership, however, progressively wanes and by the end of the period, they are no more likely to pioneer industries than other independent competitors. Some explanations for this include the administrative costs of managing many unrelated enterprises; firm inertia and the loss of entrepreneurial drive; and their preference for scale-oriented and monopolistic industries. Institutional changes such as

[4] Yagura and Ikushima, *Shuyo Kigyo*.

[5] Statistics Bureau of Japan, *Standard Industrial Classification of Japan* and "Chapter 1: Land and Climate"; Ohkawa, *Nihon Choki*; and Trewartha, *Japan*.

[6] Morikawa, *Zaibatsu*.

[7] Fudenberg and Tirole, "Preemption and Rent-Equalization."

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increased bank and equity finance as well as improved protection of property rights further diminish the ability of *zaibatsu* to maintain their technological advantage.

Given the findings of the first paper, it is natural to ask how scarce was private capital and if financial access influenced Japan's industrial development? I explore this issue in the second paper, looking at the relationship between financial intermediaries and firm startup among various industries during the Meiji period. There continues to be vigorous debate on whether financial intermediation is a precondition for industrial growth. On one extreme is the premise that banks and investment institutions are needed to mobilize investment funding for capital-intensive activities or projects requiring large fixed costs.[8] Additionally, in the absence of a managerial culture, these intermediaries may better allocate scarce capital by screening for profit potential and monitoring performance. On the other hand, some argue that the relationship between financial and industrial sector growth is coincidental or endogenous.[9] Meiji Japan, with its nascent financial and industrial sectors at the turn of the century, provides an appealing context to assess the merits of finance-led growth.[10]

In the second paper, I use a panel dataset of firm startups by prefecture and a system general method of moments (GMM) estimator to test whether a Granger-causal relationship runs from financial intermediation to modern industries. The main finding corroborates the position of finance-led growth proponents and is robust to different specifications and estimation methods. Furthermore, this Granger-causal effect is more pronounced among heavy industries as well as for the latter half of the period, as suggested by economic theory and earlier literature.

These two studies indicate that financial mobilization (via conglomerates and intermediaries) played a significant role in Japan's rapid industrialization during the Meiji period. Yet this narrative seems incomplete without a discussion of the government's early contribution to the development of industry. Despite the failures of numerous public enterprises and the government's subsequent withdrawal from direct investment, its leadership may nevertheless have catalyzed industry formation at the beginning of the Meiji period, before the emergence of *zaibatsu* or the maturation of the financial system.

In the third paper, I look at the industries chosen by the government for development and compare their characteristics with those that were pioneered by private entrepreneurs. While it is commonly known that the government seeded a number of industries at the beginning of the Meiji period, less well understood is how these industries evolved over time after these public enterprises were privatized[11] Were there distinguishing features of government-led industries relative to private-led ones, like greater capital intensity or geographic dispersion, given the government's resources and social objectives?

Using multiple statistical techniques, I find that indeed there were differences between public- and private-led industries and that these differences were consistent with the government's strategic goals at the time. That is, the government was more likely to invest in heavier sectors and in less densely populated areas, which

[8] For example, Schumpeter, *Theory of Economic Development*; and King and Levine, "Finance and Growth," pp. 717–37.

[9] For example, Robinson, "Generalization of the General Theory"; and Lucas, "Mechanics of Economic Development," pp. 3–42.

[10] Rousseau, "Finance," pp. 185–98, examines finance-led growth in Meiji Japan using national and industry aggregates.

[11] Rosovsky, *Capital Formation in Japan*.

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correspond with capital market immaturity and market fragmentation. At the same time, these industries saw greater entry than private-led ones because of the substantial growth of light industries like textiles that were targeted for export. Besides underscoring the broad-based nature of public-led investment, this result suggests that technological uncertainty may have been the main obstacle to entrepreneurs (*zaibatsu* excepted) in developing these sectors. Reducing this uncertainty through model factories and subsidized technology would thus attract a greater number of private firms.

These three papers provide multiple perspectives of how the Meiji Japanese economy entered the modern era. At first glance, the findings seem not to say much that is new, merely verifying with empirical analysis the received wisdom about Japanese industrialization. Nevertheless, qualifications to government and *zaibatsu* leadership, such as the significance of risk-taking and demonstration as well as the trade-off between scale and innovation, add nuance to our understanding about early economic development and the efficient use of public and private resources. They also inform our understanding of the subsequent modernization in South Korea and Taiwan, whose economies may in some degree have benefited from emulating Japan's policies and building on their colonial legacies. Other conclusions like the urban-orientation of industries and the progression from low to high value-added manufacturing show the feasibility of transferring foreign practices and technology to non-Western settings.

Yet perhaps the most salient contribution from this research lies not in any single result, but in that results could even be made. Through the use of historical data and modern statistical techniques, we not only raise the level of discourse on Japan's industrialization beyond anecdote, but also better appreciate the reality of its unprecedented achievement.

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Passports and Economic Development: An Antropometric History of the U.S. Elite in the Nineteenth Century

Estimates of the performance of the U.S. economy indicate that the first half of the nineteenth century saw the onset of modern economic growth. Real output per capita began growing at annual rates larger than 1 percent. At the same time, health conditions were on a downward trajectory: mortality rates increased and the average height of men decreased among birth cohorts after ca. 1830. This seemingly paradoxical divergence of indicators of human well-being has received considerable attention in the literature since the mid-1980s and is often referred to as an "antebellum puzzle." This dissertation seeks to contribute to the understanding of the phenomenon by investigating the anthropometric history of wealthy Americans.

The literature on physical stature as an indicator of a biological standard of living builds on the premise that the growth of the human body depends on the energy balance between nutritional intake and energy expenditure on other purposes, including basal metabolism, physical activity, and fighting off infectious diseases. At a given genetic potential, adult stature thus depends on the trajectory of a person's net-nutritional status in youth, including the fetal stage. While variation in the genetic potential plays a meaningful role for the variation of stature within a population, the genetic composition within a society is assumed to remain stable. Under this assumption, the genetic component virtually cancels out at the population level, so that

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changes in (sex-specific) average adult height over time reflect changes in net-nutritional conditions. Inasmuch as food represented a large fraction of the consumption bundle in past populations, economic prosperity and average physical stature should be positively associated, and there are attempts to use information on heights as proxy for missing income data for historical populations in their statistical dark ages. The case of the paradoxical height trend in the United States, however, warns us that the biological standard of living cannot at all times be equated with a conventionally conceived standard of living. A better understanding of this episode of divergence would thus be useful for other historical stature comparisons as well.

Several explanations for the American stature experience have been brought forward. John Komlos provides a classification of heavily cited factors according to their relationship to economic development.[1] "Endogenous" factors are those that are related directly to economic development including commercialization and market integration, changes in the price of food products relative to other goods, and changes in the distribution of income. In contrast, "exogenous" factors do not involve links between the market economy and human health. These include changes in the infectious disease environment and climatic conditions. While Komlos suggested very early on that endogenous factors contributed strongly, others prefer explanations based on exogenous factors.[2] For example, S. R. Johansson concludes that "[. . .] Increasing exposure to disease in childhood is probably the main reason why the average height of adult males declined in the food-abundant United States of America in the middle of the nineteenth century."[3] To be sure, the two groups of factors are not mutually exclusive, but their relative importance has remained subject of debate. A positive weight attached to endogenous factors points to a "pessimistic view" of industrialization's impact on the well-being of the common man—at least in the health dimension—whereas attaching all the weight to exogenous factors would suggest that one can "exonerate" the market economy from adverse health outcomes.

The dissertation contributes to this discussion by exploring a new set of evidence: the physical stature of American passport applicants. The sample, obtained from microfilms available at the National Archives and Records Administration, includes native-born men, women, and adolescents, covering the birth cohorts of the nineteenth century. Due to temporary restrictions regarding the use of occupation information in passport documents, occupations are only available for a subset of the sample. The composition of stated occupations among men and the occupational composition of heads of households in a subsample linked to the 1880 Census suggests that applicants represent the upper strata of the contemporary society. The assumption is that wealthy and poorer Americans faced similar epidemiologic environments in the early nineteenth century due to the state of medical knowledge and technology before the germ theory of diseases had gained acceptance. Research by Richard Steckel supports this view: in the mid-nineteenth century, wealth did not predict child survival rates.[4] The impact of endogenous factors on stature, however, should have differed by socioeconomic group. A combination of increasing relative prices of food products and a skewing of the income distribution should have resulted in a divergence in physical stature over time.

[1] Komlos "Shrinking in a Growing Economy," pp. 779–802.
[2] "Height and Weight of West Point Cadets," pp. 897–927.
[3] Johansson, "Fogel and the History of Mortality," p. 249.
[4] Steckel, "Health and Mortality of Women and Children," pp. 333–45.

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The physical stature of male passport applicants stagnated in the antebellum period. This result may be surprising inasmuch as even in this group, the onset of modern economic growth does not show up in terms of a rising biological standard of living. It nonetheless represents a deviation from the general pattern of shrinking stature of the common man. The gap between the heights of passport applicants and Richard Steckel's series of native-born white men is quite small around 1830, but it increases by 0.8 inches between ca. 1830 and ca. 1850, by another 0.4 inches between ca. 1850 and ca. 1870, and almost another whole inch between ca. 1870 and ca. 1890. In the second half of the century, when Steckel's series continues to shrink, passport applicants grew significantly taller than in the first half of the century. This increase is even more pronounced among female applicants, though a comparison with the general population is not possible for females, since no height series for white American females from the general population exists for this time period. Most likely the reason is not lack of interest but that the usual archival sources from the military or prisons include no or only few records on women. Stature differences were more pronounced among adolescents, which is consistent with the notion the part of the stunting experienced by children from poorer families could be compensated by catch-up growth at later ages. The bodily growth of the wealthy in America was not exceptional by international standards, though, inasmuch as the growth profile resembles the one documented for boys from the British gentry.

In addition to current place of residence, the applications feature information on place of birth, making it possible to assign regional characteristics to each applicant. Consistent with the literature on the biological standard of living of Americans in general, urbanization was negatively associated with physical stature among passport applicants as well. Furthermore, the local availability of nutrients, proxied by calculations based on the census of agriculture, had a positive impact on heights, similar to what M. R. Haines, L. A. Craig, and T. Weiss and T. Cuff documented for other parts of the population.[5] Using the information on place of birth, we linked a subsample of men born between 1860 and 1880 to the Census of 1880 to investigate further effects of family background. While applicants were native born, some had European-born parents. While the composition with respect to foreign-born parents in this subsample closely resembles the contemporary general population, there is a relatively large negative effect of having foreign-born parents—on the order of half an inch. To my knowledge, the size of this effect has not yet been discussed for the general population, but one could speculate that the adjustment to American stature levels took longer than one generation in the rest of the socioeconomic spectrum as well. Hence, some part of the continued stature decline among the general, native-born population might be attributable to the immigration waves of the nineteenth century and slow adjustment of stature levels. Yet, such a claim would require a detailed analysis with data on the full spectrum of the population. I furthermore find a pronounced effect of the season of birth on adult stature of male applicants, even after controlling for occupational composition. Birth season effects have previously been identified for stature in modern populations and are also discussed in the demographic literature on length of life in historical populations. These patterns are assumed to be related to seasonal variation in nutritional status in the very early stages of bodily development. In addition to the very long-run trends in physical stature and the cycles of a few years length, such a seasonal rhythm would constitute a third component of

[5] Haines, Craig, and Weiss, "Short and the Dead," pp. 382–413; and Cuff, *Hidden Cost of Economic Development*.

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height fluctuation over time.[6] Further research would be useful in this area, in particular on the question to what extent this rhythm declined with increasing possibilities of storing and transporting food products and supplementing vitamins to the diets.

The main results of this investigation add to a branch of the literature that identifies deviations from the general decline in the biological standard of living in antebellum America. Such deviations were hitherto documented for slaves and the inhabitants of economically isolated regions.[7] These deviations are consistent with economic incentives. The evidence from passport applications now suggests that even in the market-integrated urban northeast, a part of the population was virtually immune against the forces causing the decline of net-nutritional status experienced by the common man. The unfolding wedge between the biological well-being of wealthy and poorer Americans in the antebellum period underscores the notion that the general decline did not occur in spite of, but at least partially because of, rapid transformation of the economy and the resulting changes in economic conditions. The rising wealth inequality in the mid-nineteenth century was thus paralleled by a rising inequality in net-nutritional status. Recent research with modern data suggests that the height gap between the socioeconomic strata has declined in the meanwhile, but it still exists and is accompanied nowadays by a serious obesity problem. The association between human health and economic conditions and institutions will remain an intriguing field of research for different time periods, including our own time.

[6] Woitek, "Height Cycles," pp. 243–57.

[7] Komlos "Shrinking in a Growing Economy," pp. 779–802; and Cuff, *Hidden Cost of Economic Development*.

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The Private and Public Effects of School Reform

The American public education system underwent a dramatic transformation during the early decades of the twentieth century. Public grammar schools and high schools were introduced throughout the country, school attendance rates rose dramatically and the educational attainment levels of the population rose substantially to meet the needs of the modern economy. This transition to modern public schools has been well documented, both in terms of the educational gains of the population as a whole and the individual returns to education.[1] My dissertation explores the effects of the evolution of public schools not on individual outcomes or on the national economy as a whole but rather on the distribution of income at the community level, examining the impact of public school expansion on intergenerational mobility and human capital spillovers.

The focus of the dissertation is early-twentieth-century Iowa. The first decades of the twentieth century were a time when Iowa was replacing small common schools with modern grammar schools and high schools. This transition to a modern public school system offers a unique opportunity to study the effects of public school reform. Iowa was a leader in the high school movement, being one of the first states to introduce the primary and secondary public schools that characterize the modern public school system.[2] Its transition was particularly rapid and well documented. As a consequence, it is possible to collect micro-level data on individuals' educational backgrounds and labor market outcomes at a time when available schools ranged from small, rural common schools to large, modern high schools. The first chapters of my dissertation exploit this heterogeneity in school types and the wealth of data on Iowa's residents and schools to study the effects of public school expansion on mobility.

To study the impacts of school expansion on intergenerational mobility, it is necessary to have both detailed schooling data and intergenerational data on individuals' economic outcomes. The schooling data are drawn directly from annual school district records containing detailed information on the types of schools and budgets of each school district in the state.[3] Piecing together intergenerational data for individuals is a more involved task. It begins with the 1915 Iowa State Census which offers a wealth of data relevant to mobility including educational attainment and annual earnings.[4] The first chapter of the dissertation describes the process of

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[1] For a thorough discussion of the evolution of the public school system, the rise in attendance rates and educational attainment and the returns to education see Goldin and Katz, *Race Between Education*.

[2] For details on Iowa's schools at the beginning of the twentieth century, see Goldin and Katz, "Education and Income," pp. 782–818.

[3] The specific sources used are the annual reports of the county superintendents of schools. These records offer information on pupils, teachers, expenditures, revenues, school buildings and curriculum at the district level.

[4] The 1915 Iowa State Census is the only American census, state or federal, containing both income and educational attainment data prior to 1940.

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constructing an intergenerational data set from these census data. By matching individuals from the Iowa census to their parents' household in earlier federal censuses and then matching the fathers of these households back to the Iowa census, I construct an intergenerational data set of father-son pairs containing income, educational attainment, and occupation data for both the father and son. Linking these data to the school district records adds detailed information about the types and quality of schooling available to the son when he was a child. This intergenerational data set offers a unique opportunity to study intergenerational income mobility in a historical setting and examine the impacts of school reform on mobility.

In the second chapter, these intergenerational data are used to estimate income mobility rates for Iowa in 1915. With father's income and son's income, it is possible to estimate the intergenerational income elasticity, a measure of mobility common in the modern literature but something that has not been calculated for earlier periods due to a lack of data.[5] The estimates reveal that Iowa was a very mobile society at the turn of the century. The intergenerational income elasticity was substantially lower than modern U.S. elasticities.[6] A variety of other income mobility measures are estimated for both the Iowa data and modern data and confirm the same finding: income mobility was substantially higher in 1915 than in modern times.[7]

These mobility estimates suggest that a sharp decline in income mobility occurred over the twentieth century. Studies of modern American intergenerational mobility have shown that income mobility rates have been fairly constant over recent decades, implying that this sharp decline likely occurred over the first half of the twentieth century making it concurrent with the expansion of modern public schools.[8] The third chapter of the dissertation explores the relationship between schooling opportunities and income mobility to assess whether public school expansion may have contributed to the overall decline in mobility. To test the effects of school expansion on income mobility, I include measures of school quality and school access in the intergenerational income elasticity regressions.[9] The results of these regressions reveal that improvements in public schooling, particularly in terms of access to graded schools, decreased intergenerational income mobility. The introduction of better schools led to larger intergenerational income elasticities and greater persistence in the tails of the income distribution from one generation to the next.

In an effort to understand why better access to public schools led to less income mobility, the chapter includes a series of logit regressions to measure the contributions of occupational transitions, geographical mobility, educational attainment, and personal characteristics to the likelihood of moving into or persisting in different

[5] For intergenerational elasticity estimates using modern data, see Hertz, "Trends in Intergenerational Elasticity," pp. 22–50; and Solon, "Cross-Country Differences," pp. 59–66.

[6] All comparisons to modern intergenerational income mobility rates are done by comparing the Iowa estimates to equivalent estimates using the Panel Study of Income Dynamics.

[7] These findings are consistent with Ferrie's "History Lessons," pp. 199–215, study of occupational mobility rates over the past century. Ferrie finds that intergenerational occupational mobility was substantially higher at the turn of the century than in recent decades.

[8] See Hertz, "Trends in Intergenerational Elasticity," pp. 22–50, for a study of intergenerational income mobility trends in the United States over recent decades.

[9] A variety of measures are used. The school quality measures constructed include spending per student, student-teacher ratios and taxes per student. School access is measured by the number of classrooms per square mile in a school district, the number of graded classrooms per square mile in a district and a dummy variable indicating whether or not a district had graded schools.

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quantiles of the income distribution. The results reveal that the primary ways an individual could improve his economic standing relative to his father were increased educational attainment or an occupational transition (which often required formal schooling). Estimates of the returns to education conditional on parental income reveal that the returns were equally large for children from poor families and children from wealthy families. Given these findings and the fact that Iowa's schools were truly public with low financial barriers to entry, the expansion of public education seemed then to offer substantial benefits to children from all backgrounds and to promote mobility. To understand why mobility actually declined as public schooling became more egalitarian, it is necessary to focus not on the potential benefits from the new public schools but instead on who actually took advantage of those benefits. The chapter concludes with estimates of educational attainment as a function parental income and school district access and quality. These estimations reveal the ultimate source of the puzzling relationship between public school expansion and declining mobility. Wealthy families responded more strongly to improvements in school access than poor families. As school access improved, all families increased their educational investments but the gap between the investments of poor families and of wealthy families increased. The expansion of public schools offered benefits to individuals across the income distribution, but those benefits went disproportionately to children from wealthy families.

The fourth chapter of the dissertation explores a different and less disheartening dimension along which the improvements in public schools altered the distribution of income in communities. The chapter examines human capital spillovers in Iowa's agricultural sector at the time of the public school expansion. At the same time as public schools were expanding, the agricultural sector was witnessing a series of innovations that placed a premium on human capital.[10] Schools introduced farmers to the latest agricultural science and developed the skills for successful experimentation and adoption of new farming techniques. Consequently, formal schooling offered potentially large private returns for farmers. However, the ways in which a farmer could improve his yields and profitability were typically observable by his neighbors, leading to the possibility of human capital spillovers across farms.[11] To estimate these spillovers, I digitize maps of farm boundaries in 1915 and link the maps to data on the earnings and educational attainment of the farm owners from the 1915 Iowa State Census. This allows for identification of the characteristics of a farmer's neighbors and local community. By regressing earnings on a farmer's own education and the education of his neighbors, I can estimate both the private returns to education and the spillovers resulting from a neighbor's education.

The results of these earnings regressions reveal a significant role of public schools in improving agricultural productivity. Additional years of schooling and,

[10] For a discussion of Iowa agriculture at the turn of the century, see Iowa State University, *Century of Farming*. The work of Olmstead and Rhode, "Red Queen," pp. 929–66 and Induced Innovation," pp. 100–18, discusses agricultural innovation in this period for the nation as a whole.

[11] These types of spillovers have been observed in field studies in developing nations. See for example the work of Conley & Udry, "Social Learning," pp. 668–73; and Foster and Rosenzweig, "Learning by Doing," pp. 1176–1209.

[12] A farmer's annual earnings increased by 5 to 6 percent with an increase in his own high school attainment of one year and increased by 2 to 3 percent with an increase in the high school attainment of his neighbor of one year.

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in particular, additional years of high school led to substantial increases in a farmer's own income. Additional schooling also had a significant impact on the earnings of his neighbors.[12] These results demonstrate that there was a very public element to education at the time that modern primary and secondary schools were being introduced. The new schools were generating important positive externalities in the agricultural sector. These findings offer new insight into why the Midwest was a leader in public education in the early decades of the twentieth century: education was crucial to productive agriculture not only for the educated farmer himself but also those other farmers that faced similar environments.

These Iowa data further our understanding of the complex ways in which the expansion of public education altered local economies. While schools offered significant returns for individuals who attended, their impact went well beyond those private returns. Differential responses to school expansion led to children from wealthy families benefiting disproportionately from new public schools, leading to declines in intergenerational mobility. Offsetting this relative decline in economic status of those unable to attend the new schools were human capital spillovers; the benefits of schooling were not limited to the educated but extended to their neighbors as well. These complex effects of public schools on the local community suggest that the forces behind school expansion and the effects of further school reform are far more complicated than previously thought. Any future evaluation of why public schools expanded in the United States the way they did or what effects public school reform would have in modern developing countries requires serious consideration of these broader effects on mobility and the community stock of human capital.

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Her Real Sphere? Married Women's Labor Force Participation in the United States, 1860–1940

After suffrage, the growth in married women's employment was the most important change in American women's lives in the twentieth century. My dissertation advances a new interpretation of married women's choices about paid work between 1860 and 1940. I document how generational change unfolded, using new sources to explain changes identified by previous scholars. The dissertation has three key findings: (1) the late-nineteenth-century married women's property acts primarily affected women not yet married; (2) between 1920 and 1940 family decisions about whether wives worked changed dramatically. The added worker effect dropped substantially; and (3) public opinion in the 1920s and 1930s moved towards conditional acceptance of working wives.

My work advances on existing scholarship by using new sources.[1] The principal sources are samples of the American censuses from 1860 to 1940, family budget studies, and interviews with workers. The census and budget study data are microdata, letting me model labor force participation decisions. Unlike previous scholarship focusing on employed women, my dissertation looks at married women's work as a decision all families faced and includes women not pursuing paid work.[2]

In chapter one, I provide an overview of changes in married women's labor force participation between 1860 and the present, to provide context for the 1860–1940

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[1] Goldin, *Understanding the Gender Gap;* Kessler-Harris, *Out to Work*.

[2] Kessler-Harris, *Out to Work*.

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period. I use data from the IPUMS and the complete 1880 Census.[3] The complete count data have not been used to study married women's employment before, and allow unique insights into work by minority groups. Gradual changes in women's work before 1940 set the stage for rapid movement of married women into paid work after 1950.

The labor force participation concept is historical, presenting challenges for consistent measurement. By modern definition, used since the 1940 Census, an individual is in the labor force if they are: working for pay, working without pay on a family farm or business, temporarily absent from a job, or actively looking for paid work. Through 1930 the census asked instead about a person's regular occupation. This approach may undercount work by married women in several ways. It will miss women working regularly in family businesses, but not receiving separate paychecks. It will often miss women in seasonal full-time work, if the census is done in a different season. It may miss women doing some part-time work. Using budget studies, I adjust earlier census figures to make them compatible with modern censuses. Using consistent definitions, the overall level of married women's labor force participation changed little between 1860 and 1940, fluctuating between 14 and 17 percent.

Race strongly influenced whether wives worked. At every census from 1870 through 1940, 20–30 percent of black wives were working. By contrast, white wives' labor force participation rates in late-nineteenth-century censuses were less than 5 percent. The racial gap narrowed between the world wars: white women moved into wage work, while black women's participation remained static. But even in 1940, white wives' labor force participation (13 percent) was just under half that of black wives (28 percent).

By using the complete 1880 Census database and large samples of later censuses, I provide new findings about paid work by wives in ethnic minorities. Hispanic wives had participation rates similar to white women until 1920, but their rates remained constant through 1940, while white women's rates increased. In 1880, 10 percent of Native American wives worked, yet their participation hardly changed by 1940. Japanese wives had comparatively high participation rates, exceeding 30 percent in most years before 1940. Women from all these groups had fewer occupational choices than white women did, concentrated in agriculture and domestic service.

In chapter two, I show the long-term effects of the married women's property acts. Property acts were passed by nearly all American states between 1850 and 1900. Before these legal reforms, when a woman married, her assets typically became her husband's property. Any money she earned while married also came under her husband's control. There were two forms of property acts that had the potential to significantly impact women's lives. The first gave women stronger independent title to real property and financial assets. The second gave women independent title to anything they earned while married, from salary or wages, or from running businesses.

Because the property acts were passed in different states at different times, historians have focused on these laws in just one or two states at a time. My dissertation looks at the experience of women across the United States, using difference-in-difference methods to measure the impact of the laws. Overall, the property acts had little effect on whether married women worked in the late nineteenth century.

Instead, the significant effects of the property acts were on young girls and women, unmarried when the acts were passed. The property acts gave parents and young

[3] North Atlantic Population Project, *NAPP: Complete Count Microdata. Version NAPP 1.0*; Ruggles et al., *Integrated Public Use Microdata Series: Version 3.0*.

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women incentives to invest in education and keep working. After the passage of property laws between 1870 and 1900, girls were more likely to be enrolled in school. The effect of property laws was independent of whether states also passed school attendance or child labor laws. In 1880, 55 percent of all American children between the ages of 5 and 17 were in school. After the passage of property laws, girls were 7 percent more likely to attend school. The effect of passing property laws on girls' chances of being in school was greater than the effect of being in a two-parent family compared to a one-parent family.

The passage of property laws also led young women to delay marriage. In the 1880s and 1890s, after property laws were passed, a 20–30 year old woman's chances of being married declined 6 percent. This was equivalent to delaying marriage a year. An extra year before marriage gave a young woman 3–6 months more work experience. The generation of young women who entered marriage later with more schooling and work experience were then the first cohort of married women to enter wage labor in substantial numbers in the early twentieth century.

In chapter three, I examine family decision making about married women's work between 1890 and 1940, using cost of living surveys from 1889/90, 1917, and 1935, and the 1940 Census. I provide consistent models of married women's work decisions between 1890 and 1940. Because the census did not collect information on earnings until 1940, I use cost of living surveys for the earlier years. These surveys—limited primarily to families living in urban areas—are less representative than the census, but provide information on income and unemployment unavailable elsewhere. In addition to the large surveys collected by the federal government, I also use smaller surveys conducted by academics and social workers.

Using these surveys, I find that while the level of married women's labor force participation did not change significantly, the form of work shifted. The surveys show that until the 1930s, many wives supplemented family incomes by doing part-time work or taking in lodgers—activities not always recorded by the census. In 1920, for example, the census shows 5 percent of mothers in white families with a full-time job. Among a similar group in the 1917 Cost of Living Survey, the level of full-time work was the same, but 10 percent of wives also earned money from work at home such as sewing or taking in lodgers. Over the next two decades, the balance shifted towards full-time work outside the home. In 1935, 10 percent of mothers in white families reported full-time work, but only 7 percent earned money from work at home or lodgers. I also used the cost of living surveys to model changes in the added worker effect over 50 years. In 1890 a woman whose husband suffered a 10 percent wage cut was nearly twice as likely to work as a woman whose husband's wages were not reduced. In 1917 a 10 percent fall in a husband's wages made women 16 percent more likely to work. By 1940 the same 10 percent fall in wages made a woman only 5 percent more likely to work.

The changing added worker effect was not obvious to contemporaries. Because of the Great Depression, there were, by 1940, many women whose husbands had suffered large falls in earnings or had lost their job. Thus, many more families were faced with making the decision of whether wives would work to compensate for husbands' lost earnings. However, according to my analyses, within an individual family, the lost earnings of husbands weighed much less heavily on decisions in 1940 than they had prior to the depression.

Married women's economic role was vigorously debated during the Great Depression. In chapters four and five, I provide a new interpretation of public attitudes towards married women's work in the 1920s and 1930s. Married women were often

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blamed for mass unemployment in the depression.[4] Politicians and newspaper editors called for married women to quit work so all men might work. Companies fired married women to public acclaim.

This public hostility towards working wives has led historians to conclude there was widespread opposition to married women's work. I show that while public opposition was sometimes intense, the general public moved towards conditional acceptance of married women's work. I reach this conclusion based on a series of archived interviews with Chicago factory workers, conducted in the 1920s and 1930s. The interviews were done at the Hawthorne plant of the Western Electric Company, and part of a scientific study of workers' productivity.[5] Five hundred workers were interviewed three to four times from 1928–1932.

My dissertation advances on previous scholarship based on these sources.[6] The interview questions were open-ended and asked respondents to speak freely about any topic of concern to them in the workplace. Between the first and last interviews, working wives became topical at the Hawthorne plant, because some departments proposed furloughing married women for six months. Interview subjects did not have to talk about the issue, so the number of employees raising the topic serves as an index of concern about working wives. In addition, consistent sociodemographic information (such as marital status and earnings) was collected about these interview subjects. This allows me to relate attitudes about working wives to personal characteristics of the subjects.

The majority of workers did not mention, and likely did not care strongly about, married women's work. Just 25 percent of employees raised the issue in interviews carried out soon after departments proposed furloughing married women. The strongest opinions against working wives were expressed by single women. Many single women worked to support parents unable to work and felt acute pressure to keep working. They often blamed the depression for their own marriages being delayed. Because there was sexual segregation of occupations, single women competed against married women—rather than against men—for jobs. Single women, not men, had the most to gain from firing married women. By contrast, just two of 154 men argued wives should not work under any circumstances.

Most employees discussing the issue of working wives were conflicted about the question. Many respondents believed it was inequitable for some families to have both spouses working, while other families had neither spouse working. They thought that if companies furloughed working wives, family income would be distributed more equally. Yet they also recognized this idea clashed with individual rights to choose to work. Laying off productive married women employees violated norms of individual equity and due process rights. Most respondents felt unable to resolve this tension. While there were pockets of intense opposition to married women's employment during the depression, Americans were beginning to accept married women's right to work.

The slow acceptance of married women's work and the declining added worker effect set the stage for married women's rapid entry into the labor force during and after World War II. Although there was little change in the *level* of married women's labor force participation between 1860 and 1940, the *form* of work changed substantially. Married women—who had new legal rights to the fruits of their labor—made independent decisions about whether to work, went outside the home to work,

[4] Scharf, *To Work and to Wed.*

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and were increasingly accepted in the workplace. The social, cultural, and economic consequences of this revolution have transformed the family life and workplaces of all Americans.

[5] Gillespie, *Manufacturing Knowledge*.
[6] Kessler-Harris, *In Pursuit of Equity*.

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Comments on Challú, Giné, and Tang

It is a great honor for me to have been asked to comment on the finalists for this year's Alexander Gerschenkron Prize. All three dissertations are outstanding pieces of work. They ask big questions and bring an impressive range of empirical evidence to bear on these, which is the hallmark of our profession. Methodologically, they are very different, reflecting their origins in three separate disciplines: history, economics, and political science. However, they all reflect the enduring importance of the big themes raised by Alexander Gerschenkron, notably the role of government and its interaction with the market.

AMILCAR CHALLÚ

The political economy of grain markets was the subject of Gerschenkron's *Bread and Democracy in Germany*, and it is also the subject of Amílcar Eduardo Challú's extremely ambitious dissertation on late colonial Mexico. Challú is a historian, and this dissertation displays all the virtues of his profession, in particular an enormous amount of archival work in the field. This is work on the extensive margin of economic history, providing a wealth of new information on a developing economy in the eighteenth and early nineteenth centuries, and the first thing to say about such

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work, pushing back the empirical frontier, is that it is essential if our subject is to avoid intellectual diminishing returns. Another feature of this dissertation, reflecting its author's background, is that it is not afraid to analyze the world in all its complexity. This is not a Ph.D. thesis with one or two killer regressions whose coefficients "prove" a particular worldview to be correct. Rather, Challú has erected an institutionally detailed and complex argument, which is correspondingly difficult to summarize in an adequate fashion. Broadly speaking, the argument is that interregional Mexican grain markets became progressively more integrated over the course of the period, and that this contributed to a shift in the distribution of food away from rural producing areas and towards urban, consuming areas. In turn, this redistribution of calories between regions had real implications for people living in both the town and countryside, with the former gaining in stature relative to the latter, something which the author documents using new data. None of this happened because markets were imperfect, in the way that traditional scholarship has asserted. Rather, it happened because markets worked better than has been traditionally realized, and that this very fact implied that food would necessarily flow in the direction of higher incomes and privileged consumers. Along the way, Challú has fascinating things to say about the operation of grain markets during crises, about the political economy forces which led to grain market intervention being first centralized, and later liberalized, and about a great many other things besides.

A good example of how his historian's training affects the way in which Challú approaches an issue is his discussion of the interregional integration of Mexican grain markets. An economist would simply collect price data from different locations, as does Challú, and analyze them econometrically, probably using more sophisticated techniques than are used here—which would be a good thing. Having established that markets were integrated, or not as the case may be, that would be essentially that. Challú not only looks at a number of measures of market integration using prices, but also provides an account of who it was that was trading grain between regions, establishing (using original archival evidence) that different types of traders moved grain across different distances, and that some of the distances concerned were larger than has been traditionally acknowledged. He also shows that the different supplying hinterlands of various urban centers overlapped with each other geographically, suggesting that markets were even more integrated across Mexico than his city-specific evidence shows. I found that discussions such as these, as well as the detailed descriptions of the institutions regulating grain at the city level, gave me much more of a feel for what was actually going on in late colonial Mexican grain markets than any number of threshold error correction models; and this to me is also a good thing.

On the other hand, sometimes the clarity which economists bring to bear on questions concerning causality pays off. If reasonably well functioning markets led to food being redistributed towards urban centers in late colonial Mexico, this could have reflected two conceptually distinct forces which were, however, not mutually exclusive. It could have arisen simply as a result of widening income gaps between town and country, with purchasing power being shifted towards the former and away from the latter. In this case, the relative height trends documented here were ultimately due to relative trends in the demand for labor in urban and rural centers, and were unrelated to the way in which grain markets worked. If market integration, or the lack thereof, was to blame, then it was labor market integration that was relevant. Alternatively, the redistribution of food towards towns could have been a result of the process of market integration itself, as argued here. This should of course have raised rural grain prices relative to urban ones, and establishing this ought to be crucial to

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Challú's argument. In other words, the relative height trends identified by Challú ultimately reflected divergent real wage trends, and when trying to figure out the causes of this it matters conceptually whether these real wage trends were due to divergent trends in the numerator or in the denominator. Challú's argument, that stresses market integration, is obviously an argument about relative trends in the denominator, although in a couple of places he talks about rising inequality more generally—it would be nice to know if he thinks this mattered, and if so, by how much. If he is right, then market integration should have been reflected in relative rural-urban grain prices, with the former rising relative to the latter (or, equivalently, converging on the latter). In this perspective, Challú's conclusion (p. 204) that "there was no apparent long-running trend dominating the dispersion (across regions) of prices" is rather dismaying, as it seems to invalidate the central plank of his argument. On the other hand, the raw price data displayed earlier (p. 201) seemed to me to show a pretty clear convergence of prices, so we need a lot more clarity here about what was actually going on. Alternatively, it could be that Challú is making an argument, not about average price trends, but about prices in times of crisis: in years when harvests were bad, how efficiently was grain shipped to where it was needed, and was it shipped increasingly more efficiently to towns? In this case, the evidence that he presents regarding the speed of price adjustment, which was apparently increasing over time, might become relevant—but this is a different argument, and we would need to have the precise nature of the argument spelled out explicitly. So, does Challú have in mind a story about long-run trends in market integration, or about how markets responded to short-run fluctuations? Clearer and more explicit statements about causality, as well as a concluding chapter recapping the main points of the argument, would have been extremely useful for this reader.

I would also have liked a more comparative perspective on one or two occasions, to give me benchmarks by which to measure the Mexican experience. For example, how big were the height declines documented in chapter two? Are there other countries where urban heights exceeded rural ones during this period, and can we learn something from their experience? How impressive were the distances over which Mexican corn travelled during this period, when compared with the grain trade elsewhere? What was happening to domestic market integration in the late eighteenth century in other countries? And so on.

VICTOR GINÉ

Víctor Lapuente Giné, on the other hand, is a political scientist, and yet again his is a dissertation fully consistent with his intellectual background. In terms of subject matter, his dissertation is a logical progression from the two which I have already discussed. Challú is interested inter alia in the age-old question of why and how governments intervene in grain markets. Tang is interested in the nineteenth- and twentieth-century question of how governments may help backward regions overcome the obstacles to growth. Giné's dissertation has as its object of study government itself, and in particular, the way in which governments are organized. If governments matter for economic development, and if government organization matters for government performance, then such a question is certainly a relevant one for economic historians. Methodologically, Giné brings to his study a rational choice approach to understanding government decision making, a love of empirical evidence, and the political scientist's commitment to 2 x 2 matrices. Unlike Tang, there is no new data set here, sufficiently original, representative, and informative that an entire argument

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can be based around it. Rather, Giné is a magpie when it comes to empirical evidence, seizing whatever comes to hand when it seems to fit his purpose. There is no single conclusive regression here, which clinches his argument, but an abundance of very different types of evidence. None of these might on their own convince, but by wielding them all Giné in my view succeeds in finally bludgeoning readers into submission. When you reach the end of this dissertation, you find yourself putting your hands up and admitting that he's probably on to something.

There is another point that I would like to make about this dissertation, and that is the quality of the writing. This was by far the most enjoyable dissertation that I read this year. I'm fond of George Orwell's argument that good writing essentially reflects clear thinking, and this dissertation seems to me to illustrate the point perfectly. Giné is not a native English speaker, and it occasionally shows. However, he has a neat and logical mind, with the result that this dissertation is extremely clearly structured and easy to read—despite the odd grammatical lapse it is a really well-written piece of work.

The argument is easily summarized. When governments are interested in providing public goods, as Alexander Gerschenkron might have suggested was the case in some late developing countries, such as late-nineteenth-century Japan, then they have an interest in eliciting maximum effort from their employees. In principle, this should imply a very flexible system, with mobility between the private and public sectors, and strong individual incentives. However, where governments are too powerful (too decisive, in Giné's language), they will not be able to credibly commit to treating their employees fairly ex post. Under such circumstances, the next best solution is to have strong independent bureaucracies managing their own members' careers with no political interference.

On the other hand, sometimes governments are less interested in providing public goods than in avoiding political instability, and in those circumstances securing their employees' loyalty—or averting their disloyalty—is their primary concern. Under these circumstances, decisive governments hire and fire so as to secure a loyal civil service. This was certainly the choice the U.S. government made in Iraq, although one can certainly doubt whether it was the right one. Weak and divided governments, on the other hand, try to buy off their public servants, giving them widespread autonomy to manage their own affairs in return for political loyalty.

One of the merits of work such as this is that you find yourself applying the ideas in other contexts. For example, what explains the strong individual incentives and high mobility of the U.S. academic system, as opposed to the more traditional hierarchical European academic system, where individuals progress up ladders automatically over time, but compete with each other for promotion between ladders? According to Giné's theory, the U.S. equilibrium has probably arisen because university administrators are interested in eliciting maximum effort from their employees, and are able to credibly commit to rewarding individuals appropriately. If European universities work differently, is this because their administrators cannot credibly commit in this way, leaving open the possibility that hardworking teachers and researchers will find themselves passed over for pay rises or other rewards? It is tempting to pursue such lines of thought further, but I'd better not.

Probably the least convincing empirical evidence which Giné presents is the cross-country econometric evidence in chapter three. I mention this, since this is the sort of evidence which applied economists rely on all the time, and it is subject to all of the difficulties to which such research is prone: for example, the difficulty of finding adequate proxy variables which are available for all countries, or the danger that

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omitted country-specific forces are driving the results. For this reason, the historical part of the thesis, which consists of a very old-fashioned narrative, but also a theoretically well motivated one, and ultimately quite a convincing one, is particularly useful. This detailed discussion of trends in Spanish political organization over the past two centuries succeeds, in my view, in establishing the author's main point. It also serves as a good example of the usefulness of case studies, which can provide detailed evidence of the mechanisms which theory claims should be underlying cross-country correlations. To be sure, I would not want to advise a young economist seeking employment in an economics department to approach the job market with papers such as these, but once they have tenure we expect economic historians to write books, even those in economics departments, and this dissertation is a good example of the way in which more traditional scholarship can enable us to make arguments which deserve to be made, and could not otherwise be made.

JOHN TANG

John Tang's dissertation, by contrast, was written in an economics department, and is a terrific example of what cliometrics can bring to the study of economic history. It is also, of course, an explicitly Gerschenkronian piece of work in terms of its subject matter. While Challú is documenting the gradual relaxation of government intervention in the grain market, Tang is concerned with the ways in which late developers such as late-nineteenth-century Japan could overcome the obstacles which they faced in seeking to successfully industrialize. By and large, his conclusions would have pleased Alexander Gerschenkron. Government was more likely to invest in capital intensive industries than was the private sector, consistent with accounts in which capital market failures impeded successful industrialization. The importance of capital markets also comes through in his finding that industrial development was positively related to financial development, and probably in his finding that *zaibatsu* played a leading role in pioneering new capital intensive industries as well. The major contribution of this dissertation, however, is not in making such arguments, since they have been made before, but in the ingenious way in which Tang provides empirical support for them. He has developed a new and extremely useful data set, in an imaginative way, and has analysed this data set in an impeccably rigorous econometric fashion.

Since Tang is an economist, his dissertation comes in a standard three paper format. The first paper looks at the role of *zaibatsu* in establishing new industries in the context of the rapidly industrialising late-nineteenth-century Japanese economy. His basic finding is that *zaibatsu*-affiliated firms were more likely to be first entrants into new industries than other firms, while publicly listed firms were less likely. He finds a positive interaction effect between *zaibatsu* and a variable indicating whether the firm was diversified across sectors or not, but a negative interaction effect between *zaibatsu* and an "industry innovativeness" variable, suggesting a certain conservatism on the part of these conglomerates. The results on the face of it suggest an important role for *zaibatsu* in the Japanese industrialization process, but in fact Tang, in my view, has a lot more work to do in order to establish the case. As he himself recognizes, while being affiliated to a *zaibatsu* increased the likelihood that a firm would be a first entrant into a new industry, the absolute number of such *zaibatsu*-affiliated first entrants was small. Furthermore, this chapter (and the dissertation as a whole) relies on statistical significance to prove its points, meaning that we can't be sure how important the effects identified by his regressions were in practice. This is particularly

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problematic when the regressions are nonlinear, as here, and the coefficients are correspondingly difficult to interpret. I would urgently suggest to Tang that he provide readers with an indication of how big the effects are which he has. Quantifying the importance of *zaibatsu* to Japanese industrialization is the big missing piece in this chapter. Moreover, moving away from a rhetoric based on statistical significance would have the additional benefit that when particular coefficients important to the details of Tang's argument turn out to be statistically insignificant, as they sometimes are, readers would get less upset.

The second chapter in this dissertation establishes, using panel data, that there is a significant and positive relationship between financial sector development (that is, the number of financial firm start-ups) and industrial sector development (similarly defined). The paper exploits the fact that these firm start-ups can be allocated to particular Japanese prefectures. I have a couple of comments both about the underlying economics and about the empirical strategy. In terms of the economics, there seems to me to be an obvious implicit assumption here regarding interregional Japanese capital mobility—that is, capital is implicitly assumed not to be particularly mobile across regions. That may well be perfectly correct, but it would be nice to see some discussion of this, and if possible some supportive evidence. In terms of the econometrics, what Tang is looking for here is a correlation between annual financial firm start-ups in a given year and annual industrial firm start-ups. On the face of it, this seems rather odd —you would think that industrial development would depend on the stock of financial services available in a region, rather than on the annual increment to the stock. The specification used here seems to me to heighten the possibility that the positive correlation between these two flow variables is spurious, for example being driven by some prefecture-specific shocks which raised the number of start-ups in both the financial and industrial sectors.

Tang's third chapter is a simpler exercise. It seeks to identify the characteristics of industries in which government firms were the first to invest. The main finding of the paper, which the discussion emphasizes, is that government led entry was positively correlated with the industry's capital-labor ratio, which the author says "makes sense given the relative immaturity of the financial system and risk aversion of private capital for a large part of the period." However, the bivariate correlation between these two variables is strongly negative, and this negative relationship survives the introduction of several other variables in the author's probit regressions. The partial correlation between government-led entry and the capital-labor ratio only turns positive once the capital-labor ratio has been entered several times more into the regression, interacted with a variety of geographical variables. Some of these interaction terms are positive, and others negative. In my view, the direct coefficient on the capital-labor ratio on its own tells us relatively little in these circumstances. On the other hand, these results are potentially very interesting, in that they tell us under what geographical conditions capital intensive industries were government led, and under what conditions they were not—which in turn might help us think a little bit more rigorously about causality.

Despite these caveats, I was extremely impressed with Tang's thesis, with its combination of big questions, original data, and very competent econometrics. It was the most classically cliometric dissertation to have landed on my desk this spring.

In conclusion, let me once again say how much I enjoyed reading these dissertations, as well as the others submitted for this year's competition, and how much I learned from them. All three scholars should be congratulated on writing outstanding theses, and I look forward to running into them at many conferences to come.

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Comments on Parman, Roberts, and Sunder

A friend of mine objects to what he calls the "verticality of rankings" and I entirely agree. After all, the dissertations that did not make the final list also demonstrated laudable abilities and insights that at times rivalled those of the finalists. But this selection of a single exclusive winner from among a cadre of worthy contenders is still intriguing because it forces us to confront the complicated hedonics of quality. Ultimately, we do not need to be followers of Plato to identify quality: we can all recognize that it is embodied in the output of such eminent economic historians as Stanley Engerman, Robert Fogel, Claudia Goldin, Robert Margo, and Alan Olmstead . . . (don't hate me because I have not mentioned your name, I only have 15 minutes). To my mind, the quintessential model of excellence and quality in empirical economics is Kenneth Sokoloff's superb paper on the transition from the artisanal shop to the non-mechanized factory. Those of you who are placing bets on the outcome should note that this article forms a template for my choice among the three finalists for the award of the Nevins prize.

JOHN PARMAN

I cannot recall a dissertation session in recent memory where Northwestern University did not pop up in some guise or other. (As an aside, surely this monotonous trend might be a more compelling argument for path dependence than the QWERTY keyboard?) The Northwestern manufactory scorns anything but the large-scale mass production of data and (nonincendiary) matches. Parman starts with a sample of father-son linkages, located in the Iowa State Census of 1915 and the 1900 Federal Census. This procedure raises questions about the accuracy of matches made, and about the characteristics of the individuals who were not traced. He has a thoughtful discussion of the sensitivity of the results to incorrect matches. However, not enough attention is paid to the potential for selection bias in the matched sample. It would have been advisable to estimate the covariates of the probability of being matched, since the credibility of the findings hinge on the representativeness of those that survive the match.

One wonders, do students become like their thesis supervisors or is there some endogeneity to the pairing process? Like his major advisors, Parman's work is interesting and important, articulate, and meticulous. His primary objective is to investigate intergenerational mobility in terms of income, promising to make significant additions to the existing stock of knowledge regarding inequality and larger questions of democracy in America. The billing of the chapter promises "1915 to 2001" but the effective period of analysis is largely limited to Iowa in 1915. Thus, the point estimate of income in that one year must proxy for average lifetime income, creating the potential for measurement error in the mobility variable. The results that are gleaned from the 1915 data are then compared to published findings for 2001. This is the basis for Parman's main conclusion, that a major decline in income mobility occurred over the course of the twentieth century. Overall, the reader is likely to experience misgivings about such stark universal inferences being made about trends, merely on the basis of two single endpoints in time.

One also obviously wonders, to what extent is the nation merely Iowa writ large? Claudia Goldin and Lawrence F. Katz agree that this was a critical period in American education, with enrollment rates in secondary schools growing almost fourfold between 1910 and 1940, and that the prairie states formed an integral part of that

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trend.[1] Still, the work would benefit from more context about the relationship between income mobility, structure of employment and industry, and geographical mobility, in Iowa relative to the rest of the world. Don't mind me, I am perhaps overly bitter about the state of Iowa at the moment. I was served a $110 fine for speeding just outside a burg called, of all things, Montezuma, Iowa: the only speeding ticket I have ever received! But I am willing to be reassured that Montezuma is no different from Montana or Marblehead, and that we can safely draw general conclusions from Parman's results.

The mythology of American society, its democratic ethos, is in part based on perceptions of the leveling influence of universal access to education. Parman next investigates the relationship between educational institutions and income mobility, exploiting the heterogeneity in schools across the state of Iowa. The regressions surprisingly suggest that the expansion of public schooling might help to explain the posited decline in income mobility. Areas with better schools had lower mobility rates than those with worse schools, perhaps because the rich were more responsive to the new opportunities that educational inputs afforded. Rates of return to education were high for all students, but rich families made smarter investment decisions than poor families. The expansion of schooling thus advantaged the children of the rich to a greater extent than the poor. As the vice chancellor for Cambridge University recently declared, we should not expect educational institutions to function as engines for promoting social justice.

But wait, there is more! The final chapter bolsters our professional pride by finding a more sanguine perspective on the link between education and social welfare. Here, Parman contends that investments in human capital among farmers created positive spillovers. Education helped them to innovate and adopt new technologies that benefited their fellow farmers. More years of schooling, it is proposed, increased the earnings of the farmers as well as the earnings of their neighbors. The analysis was creative, but I found this part of the project to be the least persuasive, because of data and estimation problems. For instance, the sample includes only owners of farms, which clearly biases the results. Parman estimates the standard Mincer equation to explain these farmers' incomes, with land value as a proxy for variation in the quality of land, even though land value is likely endogenous to the farmer's earnings. Moreover, we have no information on the presence of the owner, nor controls for capital inputs and other specialized inputs such as managers. Despite these reservations, taken as a whole, this dissertation constitutes yet another finely crafted product such as we have come to expect from the Northwestern assembly line.

EVAN ROBERTS

If dissertations were valued by the pound, then Evan Roberts would be the clear and incontrovertible winner. His thesis includes almost 500 pages, a 39-page bibliography, and 117 figures and tables. The acknowledgements alone occupy three single-spaced pages, and made me suddenly aware of the relatively lackadaisical attitude of Sunder and Parman to those who have helped them along the way. Even before delving into this blockbuster, I realized that Roberts was careful about the details when he emailed to say that he hoped he had not put too much tape on the package. I of course responded that it was sealed with just the right amount of tape, although it was as well- embalmed as a mummy and forced me to resort to the use of my trusty Swiss Army knife.

[1] Goldin and Katz, "Education and Income," pp. 782–818.

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Suffragists in the nineteenth century argued that women were largely excluded from the benefits of American democracy, especially once they were married. The obstacles to economic independence that women confront still comprise an important part of the agenda to improve global living standards. Evan Roberts illuminates this important topic, with a thesis that addresses the course of married women's labor force participation from 1860 through 1940. Formal employment of married women actually did not vary much over this time, rising only about 10 percent over the entire century leading up to 1940. And, as the lengthy bibliography indicates, the topic has not been overlooked or ignored by other researchers, above all in Goldin's unparalleled contribution to "Understanding the Gender Gap." However, no other single monograph offers such a comprehensive data-intensive exploration of the experience of women in the labor force as Roberts now presents in his dissertation.

Once upon a time, not so long ago, major contributions in economic history could be made merely by laboriously hand-crafting a sample from a single census in one's artisanal shop. Economists felt that we had a relative advantage in analyzing these labor-intensive data sets and resolving issues of statistical inference. Enter technological change, altering comparative advantage. Roberts, the graduate of a history department, uses microdata from every extant federal census in the decades from 1860 through 1940, including the complete 1880 Census. That is just the first chapter. The second chapter then analyzes the effects of married women's property laws and finds little impact on women's labor force participation. But the results suggest, he argues, that such legal reforms did encourage girls to invest more in schooling and to defer the age at which they were married. Not satisfied with the treasure chest of data already hoarded, Roberts accumulates further information using cost of living surveys from 1890 through 1936. This funds his investigation of the relationship between own income, husband's income, and employment decisions. Finally, he considers attitudes towards women's work, and appeals to evidence from within the workplace to argue that such attitudes became more favorable as early as the interwar period.

Some of these conclusions warrant further consideration. Let me consider the issue of married women's property and earnings laws, with which I am somewhat familiar. Roberts finds that changes in these laws had no impact on married women's labor force participation. He justifies his results by speculating that, while they might have been affected by *specific* legal changes, women did not respond to *general* legal changes such as the married women's property laws. I suspect that he has the right result but the wrong conclusion. Women did respond to general changes in the law, but in terms of specific economic behaviour—hours worked, the extent of part-time employment, or type of occupation—responses that a simple dummy variable for general labor force participation necessarily fails to capture. My own research on female inventive activity indicated that these legal reforms significantly altered the types and intensity of women's economic pursuits.

MARCO SUNDER

Marco Sunder, our final finalist, adds to the already corpulent literature on human biology and the standard of living. His bibliography runs to 26 pages, fully 15 percent of the slim body of his entire dissertation. Sunder provides a magisterial and balanced summary of existing research that highlights the major issues that are still outstanding. Kenneth Sokoloff and Georgia Villaflor had previously found that white American men attained modern heights as early as the Revolutionary Era. Other scholars have

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discovered that such heights later declined, despite the advent of modern rates of economic growth and advances in material well-being, a pattern that was also prevalent in parts of Europe. What explains this so-called "antebellum puzzle?" The pessimists attribute the decline to the deleterious effects of industrialization and economic growth: the exchange of a few inches of stature for more consumption opportunities. Others propose explanations that include everything else, ranging from a colder climate to the common cold.

Attempts to resolve the "antebellum puzzle" bring to mind a jigsaw puzzle that has to be completed from several partially full boxes of pieces. The diverse data sets used by prior studies are drawn from scattered groups such as convicts and army recruits, limited localities, and over discordant periods. Sunder's sample, on the other hand, is based on passport applications over the years 1830–1924 so we get an unusually long and continuous time series for most of the major regions in the country. Since few women tend to be jailed or recruited, it is especially valuable that his sample includes over 8,000 women. His data set is also noteworthy because linkage to the population census allows him to separate out the effects of gender, age, family background, region, and occupation. As we might expect, he finds that people holding passports primarily belonged to the upper-middle class. This allows him to tackle the identification problem arising from multicollinearity between the key potential determinants of height. If the stature of higher income groups exhibited the same pattern as the overall population, then the causal factor was likely universal (owing, for instance, to what Rick Steckel terms "the democratic process of disease"). If the experience of the rich differed, then the corollaries of income inequality must have played a part in bringing about the negative effect on the heights of the lower classes.

The major result from this project is immediately evident from the simple untreated trend: rather than declining over the nineteenth century, the heights of the passport sample increased slightly. This is not to say that the statures of the wealthy were immune from external influences: Sunder finds that their heights were affected by urbanization, access to markets, and food availability within region of birth. However, these effects were attenuated by economic status. Thus, Sunder's work suggests that universal shocks such as disease were unlikely to have been at fault, and that income must be regarded as an important element of the explanation for the fall in heights.

Of course, in such an extensive study one is bound to entertain doubts about many of the assumptions and procedures. For instance, a major assumption is that susceptibility to disease was the same for the rich as for the lower classes. While I agree that both groups were equally ignorant about germ theory, it seems plausible that the rich (especially those with passports!) might have had the resources and sense to get out of town when it was hazardous for their health, as the number of summer houses in Maine attests. The quantitative work is competent but at times omits opportunities for completely satisfying the reader; for instance, more intriguing results about the impact of occupation might have surfaced if the job categories, especially those for female employment, were less ahistorical. And it is disappointing that the results from the sample of women are not particularly robust. But the nature of economic history is such that nirvana can never be attained, and enlightenment consists in recognizing what can be attempted and what can only be desired. Sunder consistently anticipates critical comments, and remedies those flaws that can be addressed, a characteristic of the most persuasive scholarship in any field. As such, he should be congratulated for this mature and well-executed contribution.

The prize I was delegated to award is offered in honor of Allan Nevins, an eminent and prolific historian of the twentieth century. Nevins, it is said, had over 100 doctoral

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students over the course of his distinguished career. He advised them that it was important to build their work "on a wealth of factual material, much of which included economic and social data. But he also valued the tradition of history as a form of literature and narrative. He believed that only through a story could one piece together the otherwise unassembled data of history".[7] In other words, to be effective and to excel in our profession, the economic historian must demonstrate the skills of both economist and historian. All three of these dissertations are notable for demonstrating just such abilities. Instead of disconnected essays on bijoux minor topics, the three finalists have conducted their extensive empirical analyses with similar literary charm and sensitivity to the opportunities and limitations inherent in careful historical research. These scholars have dissipated much of their youth in the pursuit of delinquent data and elusive truths and we should thank them for their sacrifice. They have enriched our understanding of the sources and consequences of long-run economic growth.

[2] Fetner, *Immersed in Great Affairs*, p. 46.

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