Summaries of Doctoral Dissertations

Essays on the Economic History of the American Frontier

A prevailing view among economic historians is that after 1850 inhabitants of the frontier enjoyed significant opportunity to accumulate wealth. For instance, in his path-breaking study of the Wisconsin frontier, Merle Curti found low levels of initial wealth but also high rates of wealth accumulation among the early settlers of Trempealeau County.[2] More recently, a younger generation of economic historians has identified similar patterns of wealth holding and wealth accumulation in frontier areas of Utah, Arkansas, Texas, Iowa, and Missouri between 1850 and 1870, contributing further to the perception of the frontier as a place of economic opportunity.[3]

While our understanding of wealth on the frontier is now considerable, less is known about who moved to the frontier. That is due in part to the effort required to trace households over time in the census manuscript schedules. The contributions of Donald Schaefer, Richard Steckel, and Joseph Ferrie notwithstanding, historians know less about the backgrounds of migrants, including their wealth, occupations, and places of residency before migration.[4] We do not know how many of the poorest Americans moved to the frontier, nor whether, as Frederick Jackson Turner surmised, households in urban and rural areas in the East availed themselves of the "free lands" of the West.[5] In short, the forces impelling households to the frontier are still unclear. Hence, our understanding of economic opportunity remains incomplete.

The first chapter adds to our understanding of economic and geographic mobility in U.S. history by studying the migration of households to Kansas, Nebraska, and South Dakota (then a part of the Dakota Territory) between 1860 and 1870. The chapter restricts attention to households that migrated to the frontier and established farms. Historians have dwelled extensively on the experiences of such households, debating the ability of poor households to farm on the frontier, the effects of population growth, and the dwindling supply of agricultural land in the East on the incentives to migrate to the frontier, and the barriers to the migration of unskilled workers in urban areas.

I consider these questions using two longitudinal data sets constructed from the manuscript schedules of the federal censuses of population. I constructed the first data set, a sample of 1571 household heads from the 1870 U.S. Census of Population of Kansas, Nebraska, and the Dakota Territory linked to the 1860 U.S. Census of Population. The second data set is a sample of household heads from the 1870 U.S. Census Public Use Micro-sample linked to the 1860 U.S. Census of Population. The second data set comprises the group of nonmigrants to which the group of migrants in the first data set is compared. As part of the analysis, I estimate a model of migration and wealth accumulation that accounts for the wealth maximizing behavior of households

[1] This dissertation was completed at Northwestern University under the supervision of Joseph P. Ferrie.

[2] Curti, *Making*.

[3] See Kearl, Pope, and Wimmer, "Household Wealth"; Schaefer, "Model of Migration"; Galenson and Pope, "Economic and Geographic Mobility"; and Gregson, "Wealth Accumulation."

[4] Schaefer, "Statistical Profile"; Richard Steckel, "Household Migration"; Ferrie, "Migration."

[5] Turner, "Contributions."

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and the endogeneity of the migration decision and wealth accumulation in frontier and nonfrontier areas.

The analysis of wealth indicates migrants to the frontier between 1860 and 1870 accumulated wealth at high rates. Rates of wealth accumulation in Kansas, Nebraska, and the Dakota Territory in 1870 were approximately equal to those in Iowa and slightly higher than those in several counties in Utah in 1870. The rates of wealth accumulation in Kansas, Nebraska, and Dakota in 1870 were significantly above those in agricultural areas of the rest of the northern United States in 1860.[6]

Who migrated to the frontier to take advantage of the opportunity to accumulate wealth? I find that many of the poorest households—those reporting less than $100 in total estate in 1860—moved to the frontier. Almost 30 percent of frontier migrants reported 1860 wealth below this threshold, while 30 percent of nonmigrants did the same. Overall, migrants were half as wealthy as nonmigrants.

Also, a majority of migrants, 60 percent, had been farmers in 1860. But they tended to originate in densely settled areas, where the opportunity to acquire land and to accumulate wealth was limited. For instance, a farmer in a county with 50 percent more prospective farmers than farms sites was 33 percent more likely to move to the frontier than an otherwise identical farmer in a county with a 50 percent excess supply of farm sites.

Finally, a large fraction of frontier migrants had been unskilled workers: 22 percent of migrants and nonmigrants alike had been in such positions. Nonetheless, only 18 percent of unskilled workers who migrated to the frontier originated in urban areas of 2500 or more inhabitants, and just 3.5 percent came from cities with more than 10,000 inhabitants. The regression analysis suggests, however, that the distance of most urban areas in the East from the frontier and not barriers specific to the movement of unskilled workers in urban areas was the reason for the small number of urban unskilled workers who migrated to the frontier.

The second chapter studies a phenomenon that has confounded historians of the frontier: the growth of farm unrest after the Civil War. Frontier farmers joined antimonopoly granges, alliances, and independent political parties in greater numbers than farmers elsewhere. Economic historians, however, have scrutinized farmers' claims about monopoly and found them to be inconsistent with the prices of credit, transportation, and agricultural goods. To explain farm unrest, economic historians have instead pointed to the risks and uncertainties of frontier agriculture.[7]

In this chapter, I expand upon the theme of risk and uncertainty in frontier agriculture. I argue that farm unrest is best understood from the perspective of information. It is well established that the availability of information can constrain optimal decision-making. In the late nineteenth century, imperfect information about frontier growing conditions led many Americans to migrate to the frontier under false beliefs about their prospects for gain. The result was that farmers were ill prepared for the decade-long drought that struck the region in the 1880s. I use qualitative evidence and data on land entries, rainfall, and agricultural output and prices to show that migrants were poorly informed about growing conditions at the time of their migration. I argue that drought not only radicalized farmers by spreading hardship and despair, but also strained farmers' relationships with creditors, who also had been misinformed about

[6] Atack and Bateman, *To Their Own Soil*.

[7] For examples, see Mayhew, "Reappraisal"; McGuire, "Economic Causes"; and Stock, "Real Estate Mortgages."

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the region's potential for drought. In the wake of the drought, creditors restricted access to credit, prompting accusations of monopoly on the part of farmers.

The third chapter of my dissertation studies the rule of law in the mining camps of the West. After the discovery of gold in California in 1848, miners settled remote regions of the western United States far beyond the reach of established centers of authority. Miners were without customary laws and institutions, but their need for social and economic controls was great. Moreover, mining itself raised legal conundrums that existing law was ill-equipped to handle. The result was that miners everywhere established rules of their own.

A comparison of the mining camps shows, however, that some were much more successful than others at securing property rights and preserving order. Many mining camps were, for the most part, lawful and orderly, while others experienced long periods of anarchy and violence.

In this chapter, I explain why the rule of law prevailed in some camps but not others. By the rule of law or, alternatively, law and order, I mean an environment in which individuals abide by and enforce agreed-upon rights and individuals expect those rights to be enforced in the future.

Just how societies elicit cooperative behavior is increasingly the preoccupation of institutional scholars.[8] The establishment of property rights in the gold fields follows directly from the postulates of rational choice theory, but how do societies enforce those rights? It is not enough, as Avner Greif has pointed out, to say that individuals obey the rules because otherwise they will be punished. Who does the punishing? If it is the officers of the mining camp, why do they bother to enforce the rules? Alternatively, if it is the whole population of miners responsible for enforcing the laws, we are back at the starting point. In other words, the creation of property rights and governing institutions, while solving one problem, produces another, more formidable one.

I take an evolutionary and equilibrium approach to the rule of law, using ideas about the evolution of institutions, conventions, and norms developed in the last two decades by economic theorists. The rule of law was a self-enforcing behavioral rule, because the rule of law occurred only when sufficiently many individuals decided to obey and uphold the law; and when most others behaved one way, it was optimal for other individuals to do the same. The rule of law was uncertain, however, because the power to uphold the law was dispersed among many individuals for whom exercising this power was costly, and miners who did not contribute to the public good could nonetheless enjoy its benefits. Thus, I model the rule of law as a collective action problem, in which players must divide their labor between mining and supporting institutions that promote law and order.

The evolutionary model shows that the cooperative outcome in which individuals make socially efficient contributions to the rule of law is but one of two asymptotically stable behavioral rules. But the rule of law, although a stable equilibrium, is not, by any means, guaranteed. Whether it emerges depends upon the ideology or cultural beliefs of miners, factors affecting the opportunity cost of upholding the rule of law, and the ability of miners to sanction one another.

To test the predictions of the model, I constructed a data set of the characteristics of 25 mining camps in California, Colorado, and Montana between 1848 and 1865 from published histories of the camps as well as primary sources from the Montana Historical Society. The data set indicates whether a camp experienced the rule of law, the

[8] See, for examples, Aoki, *Toward* and Greif, *Institutions*.

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population of the camp, whether foreign miners inhabited the camp, and the mining technologies employed. A camp is said to have experienced the rule of law if there existed evidence of miners working through the camp's collective institutions to enforce the laws.

The evidence largely affirms the predictions about the rule of law, providing support for the evolutionary approach to institutions and evidence about the sources of cooperative behavior. Statistical tests show that small mining camps and those with culturally homogenous populations were more likely to enjoy the rule of law. In addition, I discuss qualitative evidence that illuminates the roles of community size and cultural factors in cooperation.

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Firm Financing, Ownership Structure and Market Competition in United States Manufacturing during the Nineteenth Century

My dissertation aims at improving our understanding of what are the critical determinants for a choice of ownership structure of a firm and how the ownership structure affects a firm's performance. For this I examine manufacturing firms in the United States during the nineteenth century. The United States has excellent data for this research endeavor in the R.G. Dun & Co. Collection and the manuscript census of manufactures. The R.G. Dun & Co. Collection is a set of detailed credit reports recorded by a credit rating company, the R.G. Dun & Co. Starting in 1842 agents of the R.G. Dun & Co. gathered information on the firms in their various regions and regularly reported this information to the New York headquarters. The basic content of the record includes the owners of the firm and their status (age, family relation, personal aptitude as an entrepreneur, and wealth), firm performance (sales, profit), financial transaction (mortgages, failure, bankruptcy, etc.), and an evaluation of their creditworthiness. In the 1960s Dun & Bradstreet, the successor of the R.G. Dun & Co., made the firm's records collected from 1842 to 1890 available to the public at the Baker Library of the Harvard Business School. The collection consists of about 2,600 ledgers with 300–400 pages per volume with records on more than a million firms. The manuscript census of manufactures consists of records of firm-level information such as total product, capital, employment, and wages, on which published aggregate information in the census of manufactures was based. By combining the manuscript census of manufactures of 1850, 1860, 1870 and 1880 with the R.G. Dun & Co. Collection, it is possible to construct a firm-level panel data set covering the 1850 to 1880 period.

Due to large size of the two sources, sampling is necessary. I chose two leading industrial areas of the nineteenth century, Bristol and Essex County, Massachusetts, recorded every firm from these regions in the two data sources, and then linked them. Bristol County had three major industrial cities, Fall River, New Bedford, and Attleboro. Fall River and New Bedford had grown up as a center of the textile industry during the nineteenth century, and Attleboro was one of the national centers of the jewelry industry. Essex County was an important producer of textile, boots, and shoes. The boot and shoe industry of Essex County, led by Lynn and Haverhill, produced almost 50 percent of the total national output. These thriving and competitive industrial conditions are suitable for investigating determinants of firm ownership structures and their impact on firm performances.

I first explore the dynamics of this firm population by analyzing their exit behavior. Because the R.G. Dun & Co. Collection recorded whether a firm exited by closure, sales, or dissolution, etc, it is possible to comprehensively examine exit patterns and the relation between ownership structure and firm performance. In addition, this analysis can contribute to a better understanding of the business failure problem during the nineteenth century. Business failure was one of the most important social and political issues during the nineteenth century. Business journals frequently dealt with a business failure as their cover story, and the U.S. Congress had repeatedly enacted and repealed national bankruptcy laws until the introduction of a permanent one in 1898.

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Previous studies have investigated legal, social, or political aspects of default or bankruptcy during this period. However, my finding indicates that only 10 to 20 percent of the firms in my data set discontinued their production due to financial default. This implies that analysis of exit in general can shed new lights on this historical event.

For exploring why business failure was such a big social problem during the nineteenth century, I compare the exit rate for the nineteenth century with that of the post–Second World War period by Timothy Dunne, Mark J. Roberts, and Larry Samuelson.[1] The comparison shows that the exit rate for the nineteenth century was much higher than that of the post–Second World War period. This was mainly due to the fact that the average firm size in the nineteenth century was much smaller than that of the post–Second World War period. Out of various mechanisms generating the negative correlation between firm size and exit rate, I show how the indivisibility of entrepreneurial input increased the exit rate of small firms and consequently the overall exit rate for the nineteenth century. In addition, I show that the large firms of the post-Second-World-War period contributed to decrease in exit rate by internalizing introduction of new products, which were mainly performed by entry of new firms in the nineteenth century.

However, size-specific exit rates indicate that size differences alone do not completely explain the difference in the exit rates between the two periods. Medium size firms in the nineteenth century had higher exit rates than the average exit rate of the post–Second World War period. Although a case study of the boot and shoe industry implies that the high exit rate of medium size firms was partly due to rapid technological change, this pattern was evident in other industries as well. Why did these medium size firms have such a high exit rate? I study the ownership structure of the firm during the nineteenth century to discover the reason. Because only large firms were organized as corporations during this period and most of the medium size firms were either proprietorships or partnerships, I focus on the choice between proprietorship and partnership.

In the twentieth century, partnerships account for about 10 percent of all firms. However, this proportion was much higher in the nineteenth century, when it reached about 30 percent. One might believe that the importance of family relations in business during the nineteenth century drove the high share of partnerships. However, the data do not support this hypothesis. About 60 to 70 percent of the total partnership firms were nonfamily partnership firms. On the other hand, it is widely believed that partnerships are optimal for firms in special activities such as law and medicine.[2] If we generalize and apply this view, we can expect industrial variations of ownership structures, that is, some industries were dominated by proprietorships whereas others were occupied by partnerships. However, partnership firms were not concentrated in a few industries as this industrial-variation view predicts but occupied from 20 to 40 percent of each industry.

I show that entrepreneurs adopted partnerships in order to grow quickly in an environment with high transaction costs. First, the distribution of firm sizes shows that whereas proprietorships ranged from small to medium size firms, partnerships were mainly adopted for medium size production. Second, the geographical distribution of ownership structures suggests that the share of partnerships in local industries was 20 percent higher in large cities than in other areas. We can infer that firms tried to grow faster by organizing partnerships when they had more demand. Third, the relation be-

[1] Dunne, Roberts, and Samuelson, "Patterns."
[2] Representative is Alchian and Demsetz, "Production."

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tween the growth of the firm and adoption of co-partners is examined by estimating the correlation between the transition from proprietorship to partnership and the growth rate of the firm. Regression results show that adding a new co-partner accompanied an increase in employment and capital.

One question, therefore, is why did not all firms adopt a partnership structure for faster growth? The answer lies with three factors. First, a partnership was just one alternative to alleviating resource constraints that did not weight equally on all entrepreneurs. Many firms that did not suffer from resource constraints could operate their firms as proprietorships. Second, the cost of searching for good co-partners and the potential cost of unsuccessful relationships (e.g., costs due to forced reorganization or liquidation) could deter many entrepreneurs from organizing partnerships. In this case they would choose the slower incremental approach to growth through proprietorship. Lastly, duration of the partnership contract was short, which cancelled out increases in new partnerships. On the one hand, this short duration could reflect the weakness of partnership contracts. However, it could also be a result of voluntary dissolutions. Many partnerships might have been dissolved once members had built up their reputation and wealth, and could thus operate independently. These three factors help explain why all firms were not partnerships.

I also make a detailed case study on the ownership structure of the jewelry industry at the Providence/Attleboro area during the nineteenth century. Providence, Rhode Island and adjacent Attleboro, Massachusetts were renowned as a major center of jewelry production throughout the nineteenth century. *The Manufacturing Jeweler*, the trade journal of the jewelry manufacturers in the Providence/Attleboro area, asserted that "forming a business partnership is the next best thing to getting married."[3] Why did *the Manufacturing Jeweler* encourage entrepreneurs to organize partnerships? Did the jewelry manufacturers adopt partnership as *the Manufacturing Jeweler* recommended? If so, was it a general phenomenon of the jewelry industry or was it specific to the Providence/Attleboro jewelers? If partnership was popular, why did jewelers organize partnerships?

I show that the industrial conditions of the Providence/Attleboro area made its jewelry manufacturers adopt partnership during the nineteenth century. Due to the differentiated nature of the product, the jewelry manufacturers needed active promotion for sales. The promotion activities were made not directly to customers or retailers but to wholesalers located in large cities such as New York. There are two reasons why this industrial condition—the geographical separation from the wholesalers and the differentiated nature of product—made the Providence/Attleboro jewelry firms prefer partnership. First, partnership ameliorated the informational problem between producer and salesperson. Second, retaining a sales force for the promotion activities required a nontrivial cost. One way to optimize the cost given this sales force was to increase the total value of sales per transactions. This was possible when entrepreneurs had a greater variety of products through increasing production capacity. Some entrepreneurs might not have enough money or credit, and then they might organize partnerships to overcome the resource constraints and exploit economies of scale.

The data support this hypothesis. First, the share of partnerships for the Providence/Attleboro jewelry producers was about 60 percent, much higher than any other contemporary industries. Comparison with the producers in New York shows that this high ratio was not the characteristic of jewelry manufacturers in general but that of the Providence/Attleboro producers. Second, these partnership firms were more successful

[3] *The Manufacturing Jeweler,* 18 March 1897, p. 180.

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in terms of growth than proprietorship firms. Cross-sectional pattern shows that partnership firms were at least twice as large as proprietorship firms, and partnership firms had higher growth rates. Third, I collected about 200 biographies of Providence/Attleboro jewelry entrepreneurs from *the Manufacturing Jeweler* and analyze how they chose the ownership structure of their firms in their business life. I find that about 90 percent of these entrepreneurs organized partnerships when they first started their own business in their twenties or thirties for exploiting these advantages over proprietorship.

My dissertation highlights the importance of partnership as a firm ownership structure for understanding entrepreneurial activities in the United States during the nineteenth century. The share of partnerships started to decline from around 1900, and it was replaced by corporations. Changes in production technology, capital market conditions, or the introduction of corporate tax or bankruptcy laws, etc., could affect this transformation, which will be studied in the future research. In addition, I am currently extending the data set by connecting it with the manuscript population census. By analyzing this data, I will further investigate the relationship between individual wealth, financial market condition, and choice of ownership structures.

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The Manufacturing Jeweler.

The Impact of State Labor Regulations on Manufacturing Input Demand During the Progressive Era

Typical studies of regulatory impacts focus on how a narrowly defined regulation or class of regulations affects firm choice decisions. This approach yields results that inform about marginal impacts of adding an additional regulation, but can ignore interaction effects from already existing regulations. In this research, I use state labor laws to question how the regulatory burden represented by an entire class of laws affects firm choice decisions. To achieve this end, a comprehensive list of 135 labor regulations that existed in at least one state between 1899 and 1924 was collected. Temporal and regional trends in the passage of labor legislation are described. Significant inter-regional variability is found, with different parts of the nation favoring dif-

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ferent classes of labor legislation. Evidence for the presence of intra-regional homogeneity is found. Outlier states that do not fit the overall regional pattern are identified and discussed. Four summary variables are developed using principal components analysis. These variables represent first-wave labor laws, second-wave labor laws, mining laws, and anti-union laws. First-wave laws were common laws passed in most states by 1909. These laws were general in nature and probably not binding on employers. Most states enacted second-wave laws after 1909. The pattern of passage of second-wave laws was more uniform than that of first-wave laws, with most states adopting the more progressive second-wave laws by 1924. Anti-union laws experienced relatively little growth, but were most common in the Southeast. Three of the four summary variables were incorporated into a four-input translog cost function model of state manufacturing establishments over the 1899 to1919 period. In addition, a variable representing state appropriations for labor programs and enforcement was included in the translog model. Increases in labor legislation resulted in an increase in demand for materials and salaried labor, and a decrease in demand for wage labor. Increases in the number of first-wave laws led to an increase in salaried labor demand, possibly due to bureaucratic requirements associated with regulatory compliance. First-wave laws had no impact on wage labor, confirming the findings of many that these early laws codified existing practice. In contrast, second-wave laws significantly reduced wage labor demand, suggesting that these laws were binding.

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American Silver and the Decline of Spain

The rich mines of modern-day Mexico and Peru provided Spain and its Crown with a steady stream of silver between the mid-sixteenth and late seventeenth centuries, thrusting them to a position of hegemony in Europe and beyond. By the beginning of the seventeenth century, however, Spain entered a period of economic, political, and military decline, lagging behind the rest of Western Europe until well into the twentieth century, and never recovering the position of dominance it once had. The central contention of this dissertation is that the windfall acquisition of precious metals had a strong and persistent negative impact on the economic and institutional development of Spain. Two separate mechanisms, both anchored in the common root of exaggerated expectations about the magnitude and value of the silver remittances, worked towards such an outcome. The first channel was purely economic in nature: the perceived increase in permanent wealth set off a phenomenon known as the Dutch Disease, whereby resources were allocated away from traded sectors and into nontraded ones in a suboptimal manner, the competitiveness of domestic industries fell, and welfare was permanently reduced. The resource windfall, however, also had deep political economy implications. Relying on the hard currency base provided by the silver flows, the Crown was able to command unprecedented amounts of credit, which it used to finance its many ill-fated military campaigns; with costs soaring and revenue falling consistently short of expectations, the monarchy resorted to selling privileges, triggering a large increase in rent-seeking behavior. By the end of the seventeenth cen-

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tury Spain had lost most of its empire; it would still need over 200 years to rid itself of the perverse institutional legacy of American silver.

Standard Dutch Disease models revolve around static frameworks that, while exhibiting a shift in the composition of production from traded to nontraded industries in the presence of a resource boom, fail to explain the origin of long-term welfare losses that many resource-rich countries seem to experience.[1] In chapter 2 of the dissertation I build a dynamic general equilibrium model based on the work of Sergio Rebelo and Carols Végh to make up for this shortfall.[2] Such a framework preserves all the features of previous Dutch Disease models, but can generate welfare losses if agents fail to anticipate correctly the magnitude of the windfall. In the Spanish case, there is plentiful anecdotal evidence that both the Crown and private agents expected silver flows to be much higher than they actually were; these expectations might well have generated excessive consumption and insufficient savings in the short run. Once the true value of the resources was revealed, the Crown was repeatedly forced to declare bankruptcy, and individual income decreased markedly.

The model's main prediction, paralleling standard Dutch Disease results, is a discrete increase in the price of nontraded goods relative to that of traded goods (which is used as the numeraire) whenever the agents include a positive shock to permanent income in their expectations. Conversely, an expected reduction in permanent income would result in a discrete fall in the price of nontraded goods. Although national income data for the early modern period remain a chimera, prices for a wide range of goods and geographical regions are available in the works of Earl J. Hamilton.[3] The price history literature, however, has not produced a suitable set of weights to convert those prices into the indexes of traded and nontraded goods needed to test the predictions of the model. I therefore constructed two such sets of weights, the first in the literature to be entirely derived from primary sources. My weights reflect data collected from 14 different institutions, representing purchases for over 250 different goods. The baskets used in the actual calculations were compiled from the records of the household of Queen Joanna in 1546[4] and from the Hospital de las Cinco Llagas in Seville in 1587, because they had no gaps and contained the most representative bundles; results from the remaining sources were used to test the consistency of the baskets and to fill in gaps.[5]

The institutions whose records have survived to the present, namely hospitals, monasteries, and royal households, hardly resemble the Spanish representative consumer of the time. Royal households represent the extreme upper tail of the income distribution, whereas hospitals and monasteries exhibit consumption patterns that reflect their specific institutional goals. Hospitals are of particular interest; because they devoted most of their expenses to social assistance functions (such as feeding the poor), they provide a glimpse into the consumption of the lower classes, for which otherwise no records exist.

Armed with newly constructed price indices for traded and nontraded goods, I set out to identify whether their relative movements indicate the presence of Dutch Disease around the time of the silver discoveries. I use a Markov-switching regression model, which endogenously identifies the timing of the breaks and estimates their

[1] The traditional Dutch Disease references are Corden, *Exchange Rate*; and Corden and Neary, *Booming Sector*.

[2] Rebelo and Végh, *Real Effects*.

[3] See in particular Hamilton, *American Treasure*.

[4] Archivo General de Simancas, Casas y Sitios Reales, Legajos 12 (29) and 96 (751–839).

[5] Archivo de la Diputación Provincial de Sevilla, Hospitales 109.

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magnitude. For all three Spanish regions where price data are sufficiently complete, the model clearly identifies an increase in the price of nontraded goods at dates ranging between 1545 and 1550, precisely when the silver mines of Potosí entered full production. In all three regions, the model also identifies a discrete fall in the price of nontraded goods between 1575 and 1577, coinciding with the first Crown default on domestically held debt. Interpreted in context of the theoretical framework, a decline in the relative price of nontraded goods indicates a negative shock to permanent wealth; agents might well have interpreted the Crown's reneging as a signal that any future silver flows would have been used for servicing existing debt rather than for financing new consumption, as they effectively were.

The increase in the relative price of nontraded goods in the 1540s is estimated at between 5 percent and 15 percent, whereas its fall in 1575–1577 lies between 15 percent and 25 percent. A rough calibration of the model based on these values and parameters extracted from the data and the literature suggests that at the beginning of the silver boom agents expected its present value to be in the order of 50 percent to 100 percent of a year's GDP. The Crown and private Spaniards certainly spent much more than that in multiple military campaigns and luxury imports; in the face of adverse military events and declining silver returns, they were forced to decrease their permanent consumption by a substantial amount. It is worth noting that all my conclusions are obtained in the context of a simple, frictionless neoclassical model. Including considerations such as learning, rent seeking, and the allocation of human capital would predict even larger downturns, thus strengthening my results.[6]

Chapters 3 and 4 examine the decline of Spain in the light of rent-seeking theory. There is ample evidence that rent-seeking institutions such as venality, land entails, and ecclesiastical privileges gained a firm foothold in Spanish society starting in the second half of the sixteenth century, precisely when the silver windfall entered its culminating phase. Rent seeking did not occur directly over the ownership or distribution of the natural resources, however; although the bullion flows were substantial, they probably could not have been sustained for long without clear property rights defined over them. Rather, the Crown was able to leverage its share of the precious metals as collateral to engage in an unprecedented debt expansion, which it used to finance its multiple wars throughout Europe. As the silver stream fell short of the amount necessary to honor debt payments and sustain extremely expensive military campaigns, the Crown, rather than reduce some of its main expenditures, raised taxes while peddling all kinds of privileges and tax exemptions. The desperate effort to close the budget constraint from the revenue side encouraged a flight of skilled human capital to rent-seeking activities, as entrepreneurs abandoned productive trades to acquire nobility titles and the tax exemptions that came with them. The Catholic Church also received long-ranging privileges in exchange for a hand in raising revenue for the Crown, as did various guilds and trading organizations.

Rent seeking creates large welfare losses by diverting resources from productive uses into redistributive efforts, and discouraging additional wealth creation in the process. Its most damaging consequences, however, arise from situations where rent-seeking equilibria become traps from which economies are unable to escape without exogenous intervention. The model in chapter 4 uses a game-theoretical framework to

[6] Dutch Disease models where a learning curve leads to long-term loss of competitiveness are presented by Wijnbergeng, *Dutch Disease*; and Krugman, *Narrow Moving Band*; the effect of human capital allocation in the context of resource booms is studied by Asea and Lahiri, *Precious Bane*; and a treatment of the interaction between windfalls and rent seeking is provided by Baland and Francois, *Rent Seeking*.

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show how a fiscal expansion combined with the possibility of buying tax exemptions from the government can drive an economy to such an equilibrium. Once the economy falls into a state where rent seeking dominates over entrepreneurship, it is no longer possible to reverse course using the policy tools available to the government. The global stability of the rent-seeking equilibrium can help explain why Spain continued to languish long after its wars of conquest and its mountains of riches had quietly become romantic stories of a golden past.

Economic and social decline are complex phenomena; although modern economic theory has concentrated its focus on economic growth, we still know very little about processes of decline. This work does not attempt to capture all the factors at play in Spain's fall from grace in the sixteenth and seventeenth centuries; it rather seeks to draw attention to one overwhelming force which set Spain apart from her neighbors, giving her access to vast economic resources. In the wake of the silver discoveries, kings, merchants, and ordinary people alike faced new choices and possibilities. The economic and institutional environments shaped and constrained those choices, leading Spain down a path of backwardness while some of her neighbors, unable to count on vast natural resources, slowly but steadily developed their industries and institutions, making their way towards modern economic growth. Blinded by silver dreams, Spain missed on the ways of technical progress and sound institutions; it would take two centuries for her to finally awake, and much longer to make up the lost ground.

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Economy and Society in Rural Russia: The Serf Estate of Voshchazhnikovo, 1750–1860

The Russian peasantry is generally seen as a paradigm of "traditional society," characterized by social and geographical immobility, early and universal marriage, large multiple-family households, strong communal ties, little sense of private property, and an aversion to market transactions. Russian communal practices—in particular, the periodic redistribution of communally-held land among member households—are thought to have been more collectivist and egalitarian than those in other premodern societies. These features are often regarded as part of a deeply rooted underlying peasant culture, on which external constraints, such as serfdom, had little effect. This view is usually associated with the renowned agronomist Aleksandr Chayanov, to whom its classic formulation is attributed.[1] It has since been accepted by most social scientists as an accurate characterization of Russian rural society before the Emancipation Act of 1861. The implications of this view are immense. It is assumed in standard accounts of structural developments leading to the Russian Revolution, and it is regularly invoked to explain the economic and social divergence between eastern and western Europe, as well as the economic difficulties of modern "transition" economies. However, despite its enormous ramifications, this view has never been subjected to systematical empirical test.

The central aim of the project described here is to test this widely held view, using archival evidence for a particular locality, the Sheremetyev family estate of Voshchazhnikovo, in Yaroslavl' province (roughly 200 miles northwest of Moscow). Voshchazhnikovo was comprised of 30 villages, in which resided some 3,500 agricultural serfs. The estate archive contains a wide variety of documents for a reasonably long time period (1750–1860).[2] Household listings, soul revisions, parish registers, serf petitions, landlords' instructions, account books, communal meeting minutes, land transactions contracts, credit contracts, passport lists, inventories, and wills are used to construct a detailed picture of the workings of a serf society, and to examine the role of institutions such as serfdom and the peasant commune in a specific local context.

HOUSEHOLDS

It is generally assumed that the peasant household economy in Russia was entirely dependent on the inputs of family members, as peasants had no access to rural labor markets. On this view, all peasant households in Russia were large, extended, and headed by elder patriarchs.[3] Communal land tenure is thought to have provided an additional incentive for large households, as communally held arable was supposedly distributed among households in accordance with their size. Finally, the effects of landlords' policies on household structure are assumed to have been minimal; instead,

This dissertation was completed at the University of Cambridge (Downing College) under the supervision of Richard M. Smith. Research was financed by the Ellen McArthur Fund and an Earhart Fellowship.

[1] Chayanov, *Theory*.

[2] Documents for Voshchazhnikovo are located in the Russian State Archive for Old Documents (RGADA) in Moscow and in the State Archive of Yaroslavl' Province (GAYaO) in the city Yaroslavl'.

[3] This pattern was found for the Central Black Earth region by historians Peter Czap and Steven Hoch. See Czap, "Perennial Multiple-Family Household"; and Hoch, *Serfdom*.

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large, complex households are viewed as manifestations of an underlying Chayanovian peasant culture.

Interestingly, none of these assumptions is borne out in the empirical evidence for Voshchazhnikovo. On this estate mean household size—calculated from data in household listings and census-like "soul" revisions—was roughly five, and over half of all households were simple nuclear families. Mean age at first marriage was over 20 (as late as 26 for men), and marriage was not universal: up to 20 percent of women on this estate *never* married. Many of these headed their own independent households, with the result that between 15 and 25 percent of all serf households were headed *not* by aged patriarchs but by unmarried women. Households at Voshchazhnikovo, like those in other parts of Europe, also contained live-in servants and labourers. Communal land on this estate did *not* provide an incentive for large households; no correlation between size of household and size of communal allotment was found. Finally, the evidence indicates that, contrary to the widely held view, many of the demographic patterns observed at Voshchazhnikovo were the direct result of estate policies. For instance, taxes were levied on women who married outsiders, but not on men. As a result, many serf men married outsiders, and large numbers of women remained unmarried. And fines for unauthorized household divisions meant that poorer serfs were forced to continue living with their parents, whereas better off serfs could establish separate households.

COMMUNITIES

Russian communes are assumed to have been more cohesive than communities in other premodern societies, as specifically manifested in the communal ownership of local resources—in particular, land—and the distribution of these resources to member households in accordance with their consumption needs. Resources, in this view, were periodically reallocated in response to changes in household size and needs. This egalitarian distribution of wealth, it is claimed, ensured that all commune members were provided for and could meet their feudal and state obligations. Collectivist forms of resource allocation are supposed to have made it possible for Russian peasants to resist outside market forces more effectively. In addition, communal cohesion is thought to have facilitated the organization of collective action to protect commune members against excessive rent seeking by landlords and state officials.[4]

But in this case, too, we find that the conventional view does not stand up to empirical scrutiny. Communal resolutions, estate reports, and petitions to the landlord indicate that serf society on this estate was highly stratified and marked by significant intracommunal conflict. The commune was not economically self-sufficient; serfs, as we will shortly see, bought and sold land, hired labor, and grew cash crops. Nor were resources distributed equitably. Communally held land on this estate was feudal land, for which serfs were required to pay feudal dues. As a result, it was distributed not to those who needed it, but to those who could afford to pay. These better-off serfs often served as communal officials, and used their position to allocate resources to themselves and members of their social networks. Poor households were not only deprived of land, but often had their requests for monetary assistance rejected. Widows with young children, as well as the elderly and infirm, were repeatedly denied poor relief by the commune. Collective action was rarely mobilized against landlords and other outsiders; instead it was used by powerful members of the commune to coerce the

[4] As argued in Aleksandrov, *Sel'skaia Obshchina.*

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poor and marginalized. Petitions to the landlord—thousands of which have survived—indicate that few serfs trusted the commune in matters of justice. Across an enormous range of issues, from property rights and contract enforcement to taxation or inheritance, Voshchazhnikovo serfs chose to bypass the commune, taking their cases directly to the landlord.

THE SERF ECONOMY

Russian peasants, it is thought, "raised or made most of what they had."[5] According to Chayanov, peasants were incapable of calculating factor costs, because they did not regard land and labor as commodities. And because arable land was held mainly in communal tenure, a "weak understanding of private property" is attributed to rural Russians.[6]

It is difficult to square this view with the evidence from land transactions contracts, credit agreements, lists of servants and laborers, estate reports, land surveys, guild records, and serf petitions. These sources indicate that land, labor, and credit markets were fairly widespread around Voshchazhnikovo, and that serfs of all social strata participated in them. In addition to communally held land, serfs had private holdings, many of which were located in distant parts of the province, suggesting they were used mainly as a form of investment or enforceable loan collateral. Serfs clearly grasped the concept of property very well; some 30 percent of all petitions to the landlord concerned disputes over property. Roughly half of all Voshchazhnikovo households let their communal land to others or hired laborers to work their allotments so that they could engage full-time in wage work (in trade, service, or rural industry). Serfs lent and borrowed money, setting out the terms of their agreements in formal contracts with explicit references to interest and collateral. There was a Friday market on the estate, where serfs could buy (or sell) grain, meat, oils, wine, salt, and sugar, as well as tobacco, paper, ink, and candles. Serfs also traveled to markets in other parts of central Russia, including Rostov, Moscow, and Nizhnyi Novgorod. Wills and dowries include items such as French headscarves, expensive jewelry, silk stockings, and luxury house wares such as silver tea and coffee pots. Finally, the archival sources indicate that this estate was not unique. Contracts and court records show that Voshchazhnikovo serfs engaged in land, labor, and credit transactions with other Voshchazhnikovo serfs, serfs from other estates, free peasants, merchants, and other landlords, from neighboring districts to places as far away as Moscow, St. Petersburg, Odessa, Helsinki, and Riga.

INSTITUTIONS

In the conventional account of Russian history, serfdom is usually blamed for Russia's economic underdevelopment.[7] The evidence for Voshchazhnikovo supports this view, but indirectly. The link between serfdom and underdevelopment is not as straightforward as historians have imagined. For on this estate serfdom both enabled a surprising number of serfs to achieve a remarkable degree of wealth, and, at the same time, prevented this wealth from spilling over to the rural economy at large.

All the transactions described above were made possible through the quasi-formal legal-administrative framework developed by the Sheremetyev family to oversee their estates (Voshchazhnikovo was only one of many). This framework explicitly set out

[5] Hellie, *Economy*, p. 645.
[6] See Pipes, *Property*, esp. p. 204.
[7] Gerschenkron, *Economic Backwardness*, esp. p. 17.

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the rules and regulations of the estate and the consequences of violating them. Juridical procedures were clearly outlined and a system of formal contracts and recordkeeping left no doubt as to the terms agreed to by parties to disputes. Enforcement by the landlord could be counted on. This significantly reduced the risk to serfs of engaging in land, labor, and credit transactions. Property rights were largely upheld, contracts enforced, and serfs were given the right to take disputes directly to the landlord in St. Petersburg if they were not satisfied with the decision of local officials. As a result, many serfs—though not all—became considerably richer. And the landlord gained by levying new taxes on wealth.

Two things, however, prevented "serfdom" from contributing to wealth creation on any larger scale. First, not all landlords ran their estates as the Sheremetyev family did. Most Russian landlords did not have the resources to establish an administrative apparatus like that at Voshchazhnikovo. It is therefore unlikely that serfs on smaller estates enjoyed the benefits of an institutional framework like the one described above. Second, even a legal-administrative framework like the Sheremetyev's was not integrated into any larger body of law, issuing from a central government. Unlike serfs in medieval England who could appeal to the Royal Courts, or serfs in early modern Prussia who could appeal to the Emperor, Russian serfs had essentially no rights before the law. They were the property of their landlords and were thus at their lords' mercy. Moreover, this lack of a larger framework of society-wide institutions meant that serfs were at the mercy of other rent-seeking groups in the society. Although landlords had a certain degree of power, they could not always protect their serfs from other state-backed corporate groups, such as local and provincial officials, merchant guilds, and the church, who were always seeking to exploit serfs' tenuous legal status. The result was, at best, a reasonably well-functioning *informal* economy, limited to the jurisdiction of a particular lord. To do well in this system, a serf was forced to devote enormous resources to bribing officials, making contacts in important places, and getting around the numerous obstacles arbitrarily placed in his path. Thus even the most entrepreneurial serfs were unlikely to invest over longer time horizons in the sort of large-scale projects that made the agricultural and industrial revolutions possible in early modern England.

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Turnpike Trusts, Infrastructure Investment, and the Road Transportation Revolution in Eighteenth-Century England

The transportation revolution of the eighteenth and nineteenth centuries embodied dramatic reductions in passenger fares and freight charges as well as improvements in the quality of services. It also had far reaching effects on economic development by contributing to the growth of domestic and international trade.[1] In order to better understand this phenomenon, many scholars have examined the causes of the transport revolution. A wide variety of factors have been emphasized including technological improvements, changes in the organization of firms and markets, and increases in infrastructure investment.[2] My dissertation extends the literature by examining the contribution of turnpike trusts to changes in the road transport sector in eighteenth-century England.

Turnpike trusts were private organizations that financed road improvements by levying tolls. They were established by individual Acts of Parliament. The acts appointed local landowners and merchants to a body of trustees and gave them the authority to levy a maximum schedule of tolls and to issue mortgage debt secured upon the income from the tolls. Most acts also required that trustees could not earn direct profits by mandating that all toll revenues be devoted to road improvements and other expenses.

Turnpike trusts were established in cycles throughout the seventeenth, eighteenth, and nineteenth centuries. The first wave of adoption occurred during the 1720s, when a number of trusts were established along the major roads leading into London and other large cities. The second wave of adoption took place during the 1750s and 1760s, and was the most significant in terms of the number of trusts and the mileage covered. The second wave was also distinctive in that turnpike trusts were established along most intercity routes throughout the country. The last two waves of adoption took place during the 1790s and 1820s, and included the establishment of trusts along secondary roads near large cities such as London, Manchester, Leeds, and Newcastle. By the end of the adoption process in the 1830s there were around 900 turnpike trusts managing approximately 20,000 miles or 17 percent of the entire road network in England and Wales.

Turnpike trusts emerged as the dominant supplier of road maintenance and investment along major roadways, but they were not the first to have this responsibility. Nearly all turnpike trusts were established along existing roads that were previously maintained by local governments (i.e., parishes). Unlike turnpike trusts, parishes did not have access to external sources of funding, such as tolls or bonds. Instead, they financed road improvements by levying local property taxes and by claiming up to six days of labor per year from their residents. Parishes were also different because they were subject to the oversight of county magistrates, who had the discretionary authority to levy fines upon parishes if they failed to provide adequate road repair.

This dissertation was completed under the direction of Jean-Laurent Rosenthal in the Department of Economics at University of California, Los Angeles.

[1] See Williamson and O'Rourke, *Globalization and History*; and Estevadeordal, Frantz, and Taylor, "Rise."

[2] For example, see North, "Sources"; and Harley, "Ocean Freight Rates."

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The transition from parishes to turnpike trusts changed the way road improvements were organized and financed, but it is not clear that the transition increased road investment or that it generated benefits to road-users. Earlier scholars, such as Sidney Webb and Beatrice Webb, argued that turnpike trusts were necessary for greater road expenditure because parishes were unwilling to improve their roads. The Webbs claimed that parishes were restricted by the use of local labor and property taxes and that turnpike trusts eliminated this constraint by levying tolls.[3] Their positive assessment of turnpike trusts was seemingly confirmed by William Albert and Eric Pawson, who documented that freight charges and passenger travel times fell dramatically during the 1750s and 1760s, just after turnpike trusts were established along most of the major roads.[4]

An alternative view of turnpike trusts emerged in the work of John Ginarlis and Sidney Pollard, who provided the first estimates of total parish and turnpike trust road expenditure during the eighteenth century.[5] Their estimates indicate that parish road spending was already significant by 1750 and that total road expenditure did not increase in real terms between 1750 and 1800 because rising turnpike expenditures were offset by falling parish expenditures. These findings suggest that parishes were willing to finance greater road expenditures with their own tax revenues and that turnpike trusts may have been adopted because the tolls shifted the tax burden to external road-users. Dorian Gerhold also challenged the conventional view of turnpike trusts by studying their impact on the transport sector.[6] Like Albert and Pawson, Gerhold argued that turnpike trusts increased efficiency by improving road quality. However, he also argued that innovations in horse breeding and increases in the size of carriage firms had a greater impact on freight charges during the 1750s and 1760s.

My dissertation reassesses the impact of turnpike trusts and shows that they raised road expenditure and generated substantial benefits to road-users and the economy. In the first case, I use new information on total parish expenditures, excluding poor relief, to show that Ginarlis and Pollard substantially overestimated total parish road expenditure between 1750 and 1800. I also provide new estimates of total road expenditure by drawing upon a sample of account books drawn from archival sources. The new estimates show that turnpike trusts accounted for the majority of a four-fold increase in total road expenditure between 1730 and 1800. This finding is also corroborated by microdata, which show that road expenditure per-mile increased by more than a factor of ten after turnpike trusts replaced parishes along individual roads. Overall, the new evidence on road expenditure demonstrates that turnpike trusts spent substantially more than parishes and ultimately improved road quality.

The preceding results suggest that there may be a causal link between turnpike trusts and greater road expenditure, but they do not rule out the possibility that parishes would have increased their road spending had turnpike trusts been absent. This concern arises because turnpike trusts were generally adopted in areas that were developing, and therefore, the demand for road infrastructure was likely to be increasing. This issue is addressed by studying the behavior of parishes that managed roads where petitions to establish turnpike trusts failed in Parliament.[7] I begin by showing that turnpike petitions failed for reasons unrelated to the demand for road investment. Then I estimate the road expenditures of parishes where turnpike petitions failed. The ex-

[3] Webb and Webb, *Story*, p. 146.
[4] Albert, *Turnpike Road System*, pp. 168–87; and Pawson, *Transport*, pp. 287–99.
[5] Ginarlis and Pollard, "Roads," pp. 182–224.
[6] Gerhold, "Productivity Change."
[7] For more information on failed turnpike petitions see Hoppit, *Failed Legislation*.

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periment demonstrates that in some cases parishes increased their spending, particularly if they were able to coordinate with one another. However, in the majority of cases parishes did not change their behavior, which suggests that the adoption of turnpike trusts did indeed cause road expenditure to increase.

Besides looking at the effects on road investment, I also examined whether turnpike trusts generated benefits to road-users in terms of lower transport costs and higher quality services. The impact of turnpike trusts is controversial because the tolls may have offset the benefits associated with road improvements. In addition, there may have been other innovations that had a larger impact on the transport costs, such as improvements in horse breeding or organizational changes within carriage firms.[8] To address these issues, I tested whether turnpike trusts lowered freight charges by using data on land carriage rates. Land carriage rates were the maximum price that a carrier could charge for the shipment of a ton of goods between two cities. They stem from a law passed in the 1690s, which authorized local magistrates to set freight rates so that carriers could not raise prices through collusion.

Land carriage rates are particularly useful for this study because they represent a good proxy for actual freight charges. They are also available for a large number of city-pairs, both before and after turnpike trusts were adopted along their route. To exploit this source, I collected a sample of over 5,000 land carriage rates from 130 city-pairs. I then matched the yearly observations with information on the fraction of route mileage managed by turnpike trusts. Finally, I tested whether land carriage rates fell after turnpike trusts were adopted along the routes of individual city-pairs. The analysis yielded two key findings. First, land carriage rates declined by approximately 40 percent in real terms between 1750 and 1800. Second, land carriage rates tended to fall shortly after turnpike trusts were adopted, in some cases by as much as 30 percent. These findings show that turnpike trusts did not raise freight charges by introducing tolls. They also suggest that turnpike trusts made a significant contribution to lower freight charges by improving road quality.

The effects of turnpike trusts are also evident in the passenger sector, where travel times fell by around 60 percent between 1750 and 1820. As before, I matched the travel time data with information on turnpike trusts and show that travel times fell shortly after turnpike trusts were adopted. Therefore, the evidence suggests turnpike trusts made an additional contribution by widening roads and limiting congestion.

Turnpike trusts generated substantial benefits to road-users because they directly addressed a number of problems in the parish system. One problem was that parishes could not force external road-users to contribute to the costs of maintenance and investment. This problem was especially relevant along the major highways leading into London, where wagons and carriages often passed through dozens of parishes along their route. Turnpike trusts resolved the through-traffic problem because the tolls forced external road-users to contribute to the costs of improvement. The tolls also allowed trusts to price discrimination against different types of road-users and thereby generate the necessary revenues without incurring substantial deadweight losses.

Turnpike trusts also raised road investment by relieving borrowing constraints faced by parishes. Legal restrictions on issuing debt forced parishes to finance road improvements using current revenues from property taxes. Turnpike trusts eliminated this problem because they were able to issue debt at interest rates below 5 percent for most of the eighteenth century.[9] Interest rates were low because turnpike trusts could

[8] Gerhold, "Productivity Change."
[9] Albert, *Turnpike System*, pp. 120–31.

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use the toll revenues as collateral and because local investors expected to earn indirect returns through higher property values.

Turnpike trusts were also successful in raising road investment because they addressed coordination problems in the parish system. Coordination was relevant because the benefits associated with improving one road segment often depended on the investments in another segment. Turnpike trusts were able to address this issue by unifying control over an entire roadway or a network of roads. Some turnpikes also created inter-locking bodies of trustees, which aligned the interests of numerous trusts within a particular county.

Perhaps the important lesson from the history turnpike trusts is that organizations and institutions play a crucial role in increasing transportation investment. This is a particularly important lesson in today's environment, where the demands for infrastructure are growing even more rapidly than in the eighteenth and nineteenth centuries.

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Nevins Panel Discussion, 11 September 2004

We are gathered here to recognize three of the best economic history dissertations on North American topics completed over the last year. These three are competing for the "Nevins" Prize, named in honor of Allan Nevins. Given that Allan Nevins was born in 1890, many of us are too young to know who he was or what he did. Recently, I read for the first time a few of the 50 plus works of Allan Nevins listed in my li-

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brary's card catalogue. I thought it might be most proper and instructive here to take a moment and hear a few of his words.

On 16 January 1967 at age 76 Allan Nevins delivered an invited address to the Library of Congress, which was published under the title "The Old History and the New."[1] I will read some words from that address—shamelessly taken out of their thirty-plus-year-old context—that should still resonate with us today. Nevins said:

> History becomes a more formidable subject every year. Once its tools were simple, the most important being diligence and accuracy in research, judgment in interpretation, and force in style. . . . The modern historian . . . [must now] prize an understanding of the way in which the most expert numerical techniques, the mastery of statistics, and the ability to frame complex problems for computer solution can cast new light upon a hundred subjects from poverty in Calcutta to voting patterns in Chicago. . . . He believes that we move forward in our mastery of the past not alone by old-fashioned fact-finding and factual analysis, but by the use of quite new bodies of knowledge and new interpretations based mainly in the new social sciences. . . . As the new tools are applied, and as fresh conceptual analyses of old historical problems are developed out of constant advances in all the social disciplines, we catch at least glimmerings of a broader outlook upon history. [But] the new tools do not themselves guarantee breath. . . . What disturbs me most is the tendency of many workers in the social studies to arrogate too large a field to themselves, to overrate their expertness, and to forget the towering achievements of their elders and contemporaries in the humanities. . . . This attitude, if given full rein, would destroy history as art—for it is an art. It would eliminate all the color and drama we have associated with history as *story*—for it is fundamentally a dramatic story. . . . One reason why specialists in the social studies seek a proper definition of their place . . . lies in their realization that for vast areas of historical study their skills have little or no value. . . . The social scientists who count are for various reasons as humble-minded as the historians who count. They know that their ideas and methods must be pooled in history with other ideas and methods, and they cannot be too dogmatic. They know that much of their material is likely to be so dull to lay readers that active minds will balk at it.[1]

Our three panelists here, and hopefully all of us, have in some way tried to lived up to and I hope will continue to aspire to live up to Nevins's words.

I do not intend to give lengthy comments on each dissertation. The panelists have presented their key methods and results, and I leave the painful dissection of the minutiae to future journal and book referees and editors—of which I am sure these panelists will soon receive more than their fill. Instead, I will comment on the mysteries of the selection process, and so in a roundabout way highlight the superior qualities of the three dissertations before us.

Overall, ten dissertations were received and deemed eligible for this year's Nevins Prize—four from West Coast universities, five from Mid-West universities, and one from an East Coast university. None were submitted from Southern or non-U.S. universities. In total, they included 233 tables, 124 figures, and 1,670 references embedded within 2,760 written pages, and I read every single page—well ok, I looked at every single page. The sample of submissions varied from a low of 133 pages to a high of 687 pages, from a low of four tables to a high of 46 tables, and from a low of 53 references to a high of 550 references. Quality, however, does not necessarily correlate with high numerical values.

Most of the dissertations focused on the late nineteenth and early twentieth centuries. One dissertation dealt with the late twentieth century and one with the first half of

[1] Nevins, "Old History," pp. 1-19.

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the nineteenth century. Alas, no dissertations on colonial or pre-Columbian economic history were submitted. Nor were any dissertations on Canadian, Mexican, Caribbean, Central American, or Native American economic history submitted. Although field topics varied, there did seem to be one prominent theme, namely some type of interface with issues relating to institutions and law and how they affected social welfare.

Being in charge of giving the Nevins Prize is truly an edifying experience, and is recommended to all—though perhaps an experience you should only be blessed with once in a lifetime. It forces you to see, perhaps for the only time in your career, a full cross-section of what the new research in economic history on North America is. I personally learned many new things, such as the techniques of "Generalized Entropy" for measuring inequality, "Social Structure of Accumulation" Theory, and "Principle Components Analysis." I also learned that what I experienced first-hand during my college days is now considered fodder for historical study—a somewhat disconcerting revelation. And I learned that a dissertation should never have a "third" volume.

Reading the acknowledgments in a dissertation can also be curiously edifying. For example, one dissertation submitter acknowledged his ". . . great intellectual debt to Karl Marx who opened my eyes to the reality of exploitation." This made me conclude that the life of a graduate student is not what is used to be, if the reality of exploitation is only experienced when reading Marx. Another dissertation submitter acknowledged the blessings of "God." This in turn made me ponder the metaphysical source of standard errors and whether they were really necessary.

Because no dissertation on colonial America was submitted, there was no obvious winner, and so I had to invent some criteria, four in total, for ranking the submissions. First, I wanted to see some original data work—something more than just grabbing a canned data set, such as IPUMS or a data set from ICPSR or the BLS, and slicing it in yet another way, or more than simply reporting what was read in an archive like so much travelogue. I wanted to see either original source evidence sorted, coded, and built into a new quantitative data set from scratch, or two or more existing canned data sets merged and extended in an ingenious way not previously done, or original non-quantitative archival evidence not previously used brought to bear in some new and ingenious way.

Second, I wanted to see economic theory used as the driving force behind crafting the historical questions asked and techniques applied, not necessarily post-hoc math modeling or the latest econometric techniques, but hypothesis testing informed by the application of reasoning gleaned from core economic theory. Altough some dissertations were very good at applying the latest statistical and econometric techniques, this by itself is not economics. Thus even dissertations that applied the most current and sophisticated statistical techniques and math-modeling still needed to incorporate some strong hypothesis testing motivated by core economic theory in order to receive superior marks. I would hazard to say that as a profession we are doing a good job of teaching our students the current quantitative techniques and modeling methods, but we need to do a better job of teaching our students how to use basic economic theory, counterfactuals, and hypothesis testing to motivate their application of said techniques and methods. Economics as a useful science, as a research method, as an investigative tool is not just math. It is not just statistical estimation. It is not just models of post-hoc rationalizations. It is not just storytelling about the economy. Because grasping these notions and making use of them is probably the most difficult lesson to be learned in applied economics it received substantial weight among my criteria.

Third, I wanted to see a substantive tome—something with both breath and depth—that attacked its topic from many different angles and thoroughly explored the robust-

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ness of the techniques used and results found. Having only one substantive essay was not enough to receive superior marks on this criterion—even if that essay was good enough to be publishable in a top journal, as I believe some among the submissions clearly will be. In addition, I gave little weight to introductory essays that merely reviewed the literature uncritically and to concluding essays that just re-summarized the project's findings unless they were accompanied by more than one substantive essay sandwiched in between. This is not to say that I discounted the standard three-essay dissertation so popular in economics or treated them differently than the single large monograph dissertations, but it meant that each of the three essays in such dissertations needed to be substantive and the three needed a common theme linking them together into a coherent whole.

Fourth, the dissertation needed to make a significant new contribution to history—substantially advancing its sub-area of study. This criterion was harder to apply as I am not an expert in all fields and in all sub-areas. Here, I relied somewhat on the writers themselves to sell their own contributions as new and important in their introductory and concluding chapters. I think it is important for a writer to be convincing on this score and so it deserves some weight among the criteria.

Now, how I weighted the four criteria is hard to say. The weights I used are probably not time-consistent. If I evaluated the same lot five year ago or five years from now, I probably would generate a different rank ordering, at least among the upper half of the submissions, than I did this year. Maybe that is not a lot of consolation to those not selected or exceptional praise for those so selected, but such honesty should not be used to diminish the accomplishments of those before us now. For all three have brilliantly satisfied these criteria.

And now for a little summary and criticism of our panelists' dissertations—namely what do they most need to do to turn what is now an outstanding dissertation into a truly powerful contribution to the field. In alphabetical order, Rebecca Ann Holmes uses principle components analysis to condense into a manageable data set the seeming chaos of heterogeneity in the massive number of labor laws passed in the various states in the early twentieth century. This data set is then used to estimate the cumulative impact and evaluate the subcomponents of what was thought maybe to be unmeasurable, namely the impact of the totality of state labor laws on the country's overall labor demand and on the individual input demands for materials, salaried workers, and wage workers. Although thorough and extremely detailed, the dissertation could be made far more compelling with a livelier narrative and some illustrative storytelling about its subject. Its potential to reach the broader audience that it deserves to reach will be lost without such additions.

Duol Kim, using case studies and new detailed micro-level data, provides a significant advance in our understanding of the selection of firm ownership structures, namely partnerships versus proprietorships, and how these structures performed and survived conditional on various factors. I particularly liked the application of not only standard estimation techniques but also of ingeniously simple techniques, such as separating samples into partnerships where the partners were only family members versus partnerships where the partners were not family members, and then modeling, estimating, and interpreting the different outcomes. While the dissertation is fascinating reading and while I learned a lot, at the end of the day I am still not sure what it all means and how much it matters, i.e., what the true opportunity costs were. The dissertation will have far greater impact if such things can be addressed more adequately.

James Ireland Stewart constructs new merged or linked household samples thereby making important contributions to our understanding and ability to directly measure

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the extent and nature of self-selection in frontier migration to the Great Plains. These new data allowed Stewart to then examine the role that individual experience, knowledge, and information played in coping with regional climate variations, namely drought, and how that in turn affected the credit markets and feed the populist politics of the times. This three-essay dissertation, however, seems to lose steam in the third essay, which applies evolutionary games theory to mining camps in the far west. How this third essay coheres with the first two essays on frontier farming on the Great Plains is not well articulated. In addition, the model used in this third essay does not seem to lead to a clear set of well-executed or executable tests. The dissertation as a whole would be improved if the third essay on mining camps could be integrated more with the first two essays on frontier farming, and if the tests executed in the third essay were as neat as those executed in the first two essays.

In conclusion, we should all look forward to seeing the important contributions made by our three panelists to our discipline in print, hopefully, in the near future. Please welcome them into the profession.

FARLEY GRUBB, *University of Delaware*

REFERENCE

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Gerschenkron Panel Discussion, 11 September 2004

The prize for the best dissertation on topics outside of North America is named after the economic historian Alexander Gerschenkron, who was professor of economics at Harvard University for the latter half of his life. Professor Gerschenkron was a Russian born, Austrian trained scholar, who applied his wide-ranging knowledge to studying the problems of modern economic development. He served as mentor to many of the most distinguished members of our association and was a godfather to the Cliometrics movement. His lively interest in European economic history was said to have been especially appropriate to a man who spoke half a dozen languages, and often claimed to speak half a dozen more.

He would no doubt be extremely pleased to hear the work of the three finalists for this year's thesis prize named in his honor. Although non-North American theses are usually less numerous than those devoted to the study of the New World, this year's competition was exceptional and saw 13 submissions, with one having been partially written in French. Although it is clichéd to say so, this year's competition was unusually stiff and any one of the three finalists would have been a worthy recipient of the prize for best thesis. Indeed, I dare say that all three would have been serious contenders for the prize in any recent competition.

One of the more remarkable changes has been the way in which the choice of topic has evolved. In the 1960s and early 70s, when the Cliometric movement was at its height, the state-of-the-art research was focused on tackling the problem of historical development with the tools of modern economics and statistics that had only just be-

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gun to enter the analytical historian's toolbox. Conceptually, the early Cliometricians struggled to place their studies within the leading paradigms of economic development derived from the neoclassical synthesis of Keynesian macroeconomics and neoclassical microtheory that characterized the growth theories of Samuelson and Solow, and that received their expression within economic theory in the writings of Walt Rostow and Gerschenkron himself.

As the neoclassical synthesis has fallen out of favor, and as we have turned away from narrowly economistic explanations of economic change, the emphasis on grand macroeconomic aggregates has declined. In its place has come a greater interest in institutions and the role of the state in promoting or retarding economic performance. Directly or indirectly inspired by the work of Coase, North, Olson, and scholars in the public choice tradition, today's economic historians are more likely to focus on issues of institutional design, the interplay between regulation and performance, the role of rent-seeking, and institutional selection within a broader long run bounded by changes in demographics and technology. It is now commonplace, and fairly uncontroversial in our field, to assume that all actors behave rationally, or nearly so, and that poor outcomes are more likely the product of maximizing responses to inefficient or perverse constraints.

All three of the finalists for this year's thesis prize have produced work that exemplifies this newer thinking, and have shown how well sophisticated theory and technique can be deployed to examine profound historical questions of lasting scientific interest.

Daniel Bogart focused his attention on the turnpike trusts in eighteenth-century England as a means of understanding what role the state plays in enhancing economic performance through timely institutional innovation. He wishes to confront both the descriptive question of how turnpike trusts were promoted and operated as well as the counterfactual question of their importance in bringing about the transportation revolution that is often said to have been a critical element in the Industrial Revolution. Bogart's accomplishment is the creation of quantitative series on aggregate road expenditure by village based on material from highway taxes, village account books, and total village expenditures in the eighteenth and early nineteenth centuries. He also showed through an examination of roads that remained under the traditional village system, that the turnpike trusts did much to encourage road investment throughout the eighteenth century. Bogart also did much to examine the theoretical significance of the trusts and to suggest why they were a superior investment-promoting institution than the earlier arrangements. Where his dissertation is weakest is in arguing against the notion that road expenditure over the long run was not simply driven by general economic demand and changing technology and in showing quite clearly that without the trusts, the transportation revolution would have faltered in an economically significant fashion.

Tracey Dennison's thesis is another example of how a detailed examination of a micro-historical case can shed light on broad issues of economic development. Her work is a demographic study of the serf estate of Voschchaznikovo in Russia from 1750–1860 to test the dominant view in Russian historiography that serfdom represented a particular type of traditional society, with serfs being indifferent or hostile to private property and market transactions, while suffering from social and geographic immobility that hindered the modernization and growth of Russian agriculture. This debate mirrors a larger debate in the social science literature about the existence of a traditional, "moral" economy prior to the advent of industrialization. Where the economic historical literature has tended to come down on the side of the "rational peasant" the

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historiography of Russia is still dominated by the writings of scholars such as Chayanov, who emphasized the peculiarities of peasant culture.

Denison's work flatly contradicts this view. Peasant behavior in Russia was not very different from what we have observed in the rest of Europe. They participated in the land and labor markets at multiple levels, they were geographically mobile, and did not necessarily live in a stereotypical multigenerational household. Many of the more "collective" aspects of peasant life were actually imposed on the communes by the rules of the state regarding taxation and the like.

If there is a greater failing to this work, it is the fact that Denison does not do much to tie her findings to the great debates in economic history. What role did the 1861 reforms really have in promoting agricultural productivity? How did the evolution of peasant behavior match the changes in property rights throughout the late nineteenth century? What should our views be of the importance of the Stolypin reforms of 1905 on the overall modernization of Russian agriculture?

Mauricio Drelichman takes us back to a longstanding, but still unsettled question in economic history: Why did imperial Spain fail? And what role did her conquest and management of Latin America play in creating institutions that affected the abysmal showing of the Spanish economy from the seventeenth to the mid-twentieth century?

Drelichman's is really two very different theses held together by a desire to grapple with a large and important topic. Drelichman's is the most technically sophisticated of the three thesis finalists and the first part of his dissertation is a skillful and rigorous demonstration that the discovery of American silver produced a shock that undermined the comparative advantage of Spain, a negative outcome of a positive surprise that is known in the theoretical literature as the Dutch Disease. Using a Markov-switching regression model, Drelichman finds evidence of a rise in the price of nontradeables that is consistent with the Dutch Disease story. He then goes on to push a claim that over-reliance on this silver contributed to Spain's bankruptcies and subsequently troublesome fiscal straits.

The second part of the thesis is a very different enterprise with Drelichman trying to create a model of rent-seeking in Spain that would account for the long-run underperformance of the Spanish economy. Although no one will be shocked to hear an argument that perverse institutions had an important role in the problems of Spanish economic development, Drelichman undercuts his fine work somewhat by insisting on tying this section to the Dutch Disease story. Ultimately he tries to lay much of the blame for the long-run failure of Spain on the Dutch Disease effects and related consequences that he worked so hard to demonstrate in the earlier section. Although not impossible, the connection seems tenuous at best. The kinds of mechanisms that produce Dutch Disease effects are really quite different from the institutional feedback mechanisms that Drelichman and other scholars have postulated as resulting in the negative effects of colonialization on the mother country. If the link really is there, the case will have to be made with stronger logic and evidence in future work.

Notwithstanding the criticisms I may have leveled at the three authors, the theses I have examined were clearly the work of mature and innovative scholars. There is no doubt that all three are likely to have productive careers ahead of them. We have rarely been blessed with such an evenly matched set of finalists for the thesis award. Their work will bear close scrutiny in the future, and we should all welcome them into the community of scholars.

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