doi: 10.1017/S0022050725100739

Summaries of Doctoral Dissertations

The Dissertations of Katherine Hauck, Carlo Medici, and Laura Weiwu: 2024 Allan Nevins Prize Competition of the Economic History Association

I was honored to convene this year's Nevins dissertation prize session. In a departure from previous years, the dissertations were judged by a committee rather than a single convener. The combined committees for both the Nevins and the Gerschenkron Prizes tackled the full set of submissions to whittle them down to shortlists for each prize. Each committee then settled into the incredibly difficult work of weighing the relative merits of all the impressive research on display in those shortlisted dissertations. While there are a range of sound logistical reasons motivating the new format, a delightful practical outcome was the chance to read a fascinating broader range of dissertations and engage in spirited debates about which rose to the top three.

These debates were not about separating good from bad or interesting from dull. They focused on sorting through the comparative highlights of several uniquely excellent dissertations. Without revealing the details of our smoke-filled-Zoom-room deliberations, I can say that our three finalists stood out for exemplifying some of the best features of economic history: big questions, herculean data collection efforts coupled with careful econometrics, and constant awareness that history matters. Allow me to highlight just a few aspects of what made our three finalist dissertations special.

KATHERINE HAUCK (UNIVERSITY OF ARIZONA)

Katherine Hauck's dissertation, titled "The Impact of the Homestead Act on Land Acquisition Decisions and Productivity in the American Midwest," sheds new light on the expansion of agriculture so critical to the economic development of the United States. Her focus is on the farmers themselves and the decisions they faced regarding both how to acquire land and then how to farm it. Hauck's dissertation offers a fascinating story about just how intertwined these two decisions were.

Countless resources help modern homebuyers navigate the choices surrounding mortgage down payments, interest rates, and term lengths. The would-be farmer in the nineteenth century faced a more daunting set of options with no slick sliding-bar-calculator websites to help. Land could be acquired through homesteading, direct purchase, military warrants, railroad grants, pre-emption, and resale. Each of these had advantages and disadvantages that varied with farmers' skills, assets, and land quality, leading different farmers to choose different land acquisition and improvement approaches with differing outcomes.

Most historical agricultural statistics come to us at best aggregated to the county, masking all this fascinating farm-to-farm and farmer-to-farmer variation. Hauck sheds light on this critical variation by building a new dataset of Kansas farms and farmers with an unprecedented amount of detail. It is an impressive feat of digitizing new data sources and linking them to more familiar historical census records. Beyond the familiar (at least to economic historians) data on farmers and farms from the Census of Population and Census of Agriculture, Hauck brings in a wide range of new historical data. While the full range of data does indeed require a dissertation to discuss, I want to highlight two key pieces particularly relevant to economists. Central to her topic, Hauck digitizes information from the Bureau of Land Management Tract Books on both successful and *unsuccessful* homesteads. All too often we economic historians are confined to the surviving records of those whose businesses flourished, letting us study why they succeeded. Equally if not more interesting is what we can learn as economists from failure. Hauck's new data on unsuccessful homesteads gives us an intriguing new lens for studying the impacts of the Homestead Act. The other new piece of data that I want to highlight are resale deeds digitized by Hauck. Homesteading and the settling of the frontier was not a process of planting your flag and establishing a family farm for the generations, there was substantial churn and consolidation in terms of ownership. Hauck brings in exciting new data on this aspect of farming, once again giving us a much more complete picture of the economics of success *and* failure in the development of the agricultural sector.

New, amazing data is nice but the question is always what you and future researchers can do with it. Hauck puts these new data to work to show the consequential, and heterogeneous, decision-making taking place at the farmer level. First, she provides descriptive statistics documenting that homesteaders did indeed differ from other farmers along several dimensions both in terms of farmer characteristics and farm operations. These new stylized facts raise a new empirical problem: If there is selection into homesteading, how can we tease out the impact of homesteading, or the Homestead Act, on agricultural output? Is it the farmer or the nature of homesteading impacting outcomes?

Hauck tackles this question with an elegantly simple instrumental variable approach. Homesteaders and purchasers were both required to get their land from the land office, but homesteaders had to go twice. So, the farther you are from a land office, the greater the difference in costs between purchasing and homesteading. Hauck uses this to demonstrate that homesteaders and purchasers differed in their investments, with purchasers more likely to initially invest in durable improvements, but crucially shows these differences were not driven by the type of land acquisition itself but rather by the type of farmer.

These results suggest that differences in underlying farmer ability are central to understanding farm operations and outcomes. The final chapter of Hauck's dissertation uses her rich data to estimate a model of farmers with varying abilities and differing beliefs about their abilities who update their information over time. This lets Hauck evaluate counterfactuals of a world without the Homestead Act or with fully informed farmers. She finds that westward expansion would have been reduced by 37.5 percent in the absence of the Homestead Act. This gives us a new appreciation for the impacts of the Homestead Act, but one that is complicated by all of Hauck's other results. It raises important new questions about the efficiency of nudging different types of individuals into agriculture in the nineteenth century and the impact of those nudges on their individual welfare and that of subsequent generations.

CARLO MEDICI (NORTHWESTERN UNIVERSITY)

Carlo Medici's dissertation tackles very different types of migration and their economic impacts on the evolution of the American economy. While the Homestead

Act was intended to encourage migration, Medici examines institutions designed to limit immigration or mute its impacts on local workers. His dissertation considers three different settings: union formation in the early twentieth century, the Chinese Exclusion Act, and upward migration through the ranks of the federal judiciary. Each of these different cases offers a new perspective on the tangled relationship between migration, institutions, and the American economy.

Medici's first chapter investigates whether union formation in the early twentieth century was in part an effort to protect native workers from immigrant competition. Given the scale of migration during the Age of Mass Migration, it is certainly a plausible thesis but one that is surprisingly hard to test. More accurately, it is surprising to those who have not tried to work on unions. For an institution so intertwined with the evolution of the twentieth-century economy and politics, it is remarkably hard to find detailed union data. This is where Medici reminds us as economic historians to always keep searching through those archives. Medici turns to the convention proceedings of the state federations of labor. One might stumble upon these and give up on them as they record delegates from local branches but not branch membership. However, Medici cleverly invokes the organizations' constitutional rules which specified a local union's representation be proportional to its membership. With his new county-level data on union membership, Medici employs a shift-share instrument focusing on county-level exposure to European migration flows to demonstrate that counties with greater exposure to immigrant inflows were more likely to see unions form and their membership rise. It is an important result that highlights a way in which the Age of Mass Migration would leave its mark on labor market institutions long after the Quota Act.

His second chapter focuses on the one major restriction on immigration during that Age of Mass Migration, the Chinese Exclusion Act. While the Act was motivated in part to protect native workers from Chinese competition, Medici notes that those protections could be outweighed by general reductions in overall economic output from losing Chinese workers and consumers. Comparing the evolution over time of counties with high Chinese populations prior to the Act to those with low Chinese populations prior to the Act, Medici and coauthors show that counties with initially large Chinese populations suffered economic losses following the Act, a consequence that lingered through 1940, a full half-century after the passage of the Act. Here Medici provides us with another case study of how the institutions surrounding immigration and labor at the turn of the twentieth century would have lasting consequences for the economy.

Medici's final chapter focuses on a very different form of migration, moving up through the ranks of the federal judiciary. However, it tells a very similar story: the institutions that influence that migration have major consequences for efficiency. The basic question is what happens after the Senator who championed a particular judge for promotion leaves office. Does a judge increase effort to overcome the loss of this political connection to still advance? Or does a judge pack it in, becoming what we academics might call "deadwood"? After putting together data on the performance of federal judges from 1789 to the present, Medici finds the answer is depressingly the latter. Once their champion exits Congress, a judge experiences a 48 percent drop in the yearly probability of promotion. That might not be inefficient, who's to say Senators know a good judge when they see one? Unfortunately, Medici and his coauthor also show these judges write fewer opinions, those opinions are of shorter length and they receive fewer citations; deadwood indeed.

LAURA WEIWU (MASSACHUSETTS INSTITUTE OF TECHNOLOGY)

We journeyed westward in Hauck's dissertation, across the Atlantic and through the judiciary in Medici's, we now turn to migration from city to suburb in Laura Weiwu's dissertation, "Essays on Inequality in Cities." Weiwu focuses on the construction of the interstate highway system's impact on residential sorting and racial gaps in socioeconomic outcomes across multiple generations. While the relationship between race, infrastructure, and economic opportunity is well studied, Weiwu brings a fresh perspective by marshaling new, granular data on individuals and roads in conjunction with a rich spatial model. Either aspect of the dissertation would be a substantial contribution by itself, but the combination of this data and theory is what allows Weiwu to make big pushes forward in our understanding of the evolution of racial gaps in the United States.

There is a deceptively simple narrative around all of this. The Great Migration brought black families to Northern cities. White families responded by moving themselves and their economic resources to the suburbs. Income gaps and institutional barriers prevented black families from following, increasing racial inequality. Weiwu's dissertation does not set out to refute this story. What it does do is use incredibly detailed data and rich modeling to give us a much better understanding of how each piece of this story, preferences, infrastructure, and institutions, played its part.

As with the dissertation as a whole, Weiwu's theoretical model tells a familiar story with impressive detail. It incorporates preferences for neighborhood amenities and racial composition along with institutional barriers that create race-specific frictions to movement to model the endogenous evolution of housing prices, racial composition, and wages across neighborhoods. Estimation of the key parameters of the model comes from some excellent economic history data work, including creating estimates of commute times from newly-digitized Shell Atlases, constructing instruments for highway placement from engineering maps for 100 cities, and combining all of this with the data sources created by the economic historians who came before her.

The data construction, details of the model, and the empirics that tie everything together are complex. However, in Nevins fashion, Weiwu clearly conveys the intuition behind each component in simple terms. The big payoff from this complexity is Weiwu's ability to provide decomposed estimates of the varying contributors to the evolution of black-white gaps. Interstate construction lowered Black household welfare by 1 percent while raising it for white households by nearly 3 percent. Critical to these disparate impacts were institutional restrictions on black movement. Estimating a counterfactual with these restrictions completely removed leads to a world where black household welfare increased from Interstate highway construction and the racial gap in welfare closed by 54 percent.

This evidence of the residential sorting and racial gaps in welfare generated by the interstate highway system naturally leads to the question behind Weiwu's final chapter: What were the intergenerational consequences of these effects? Given my assertion that the question follows naturally, I want to skip to the data Weiwu uses to answer it. In particular, she constructs novel parent-child linkages for the near universe of children born between 1964 and 1979 using historical IRS tax data. In the context of this dissertation, these cohorts are important as they grow up as the interstate highway impacts take hold. But at a broader level, these cohorts represent a bridge between the modern mobility literature using panel studies or linked IRS data and the historical mobility literature using links between public complete count census data. Weiwu uses these data to estimate the impact of highways on mobility through two channels: the role of improved commute access and the role of residential relocation changing peer composition. These are important questions but on the horizon are even bigger questions that this cohort of linked children, growing up during many of the biggest changes to government assistance programs, will help answer.

A WARM WELCOME

In preparation for these comments, I reviewed several past conveners' remarks both to avoid reinventing the wheel and to acknowledge that those prior conveners are far better wheelwrights than I am. One thing that stood out was the familiarity of the past finalists despite my hazy recollections of their dissertations. This familiarity was rooted in the continued presence of those finalists on panels at the Economic History Association meetings, in the pages of the *Journal of Economic History*, and in countless coffee break discussions and email exchanges about data sources and projects. While these three finalists have all produced excellent dissertations, the truly exciting thing about reading through those dissertations is knowing what they signal about the research agendas ahead and the many future contributions these scholars will make to economic history. I hope these remarks serve as not just a justified celebration of their achievements but even more so as an enthusiastic welcome to the economic history community.

John M. Parman, *William & Mary and NBER*

The Impact of the Homestead Act on Land Acquisition Decisions and Productivity in the American Midwest

Shortly before the Civil War, Senator Henry Clay said in a speech to Congress, "No subject which has presented itself to the present, or perhaps any preceding congress, is of greater magnitude than that of the public lands." This quote speaks to the importance that Americans of the time placed on land allocation. In the second half of the nineteenth century, individuals had multiple ways to acquire land from the federal government, including pre-emption, cash purchase, military service, and railroad or university land grants. However, the method that has captured the most attention by economic historians is the Homestead Act of 1862.

Globally, the late nineteenth century was a period of rapid colonization. In the United States, the farmers who acquired land via the Homestead Act represented the turning point from impermanence to permanence of the American presence in the West. Permanent American occupation of the western half of the country was by no means inevitable, both because of the people already living there and because of the difficulties

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of starting a farm. Therefore, the decisions of individual farmers in the West collectively determined the mechanism behind how American presence in the West was solidified.

My dissertation analyzes the mechanisms behind the decision to acquire land in Kansas in the last half of the nineteenth century and the causal impacts of that decision. I focus on learning under uncertainty and how homesteading interacted with the land resale market. The Homestead Act granted over 10 percent of the land area in the United States to individuals for the express purpose of farming. Further, the administrative requirements set up by the Homestead Act encouraged farmers to learn about other methods of land acquisition by having a two-step process. First, individuals paid a $10 fee to apply for the homestead. After filing the initial homestead application, a farmer had three choices: prove the homestead by farming it for 5 years, commute it by paying the full purchase price after 6 months, or abandon it. This two-step process made homesteading an explicitly dynamic decision that incorporated a farmer's uncertainty about future events. It allowed farmers to learn more information before committing to the homestead. Further, the resale market for land inherently introduced a major source of uncertainty, but it has been less well studied than the initial market for land from the federal government.

The first chapter of my dissertation describes how land acquisition from the federal government and resale to other farmers was influenced by demographic and land characteristics at the individual level. The second chapter addresses how selection into homesteading impacted subsequent farm-level production and investment decisions related to resale using an instrumental variable approach. The last chapter builds counterfactuals from a dynamic discrete choice model to answer how western expansion would have changed without farmers learning new information via the Homestead Act about reselling land.

The three chapters deepen our understanding of how farming ability, land acquisition, and land resale interacted in the Midwest in the late nineteenth century. Each chapter develops a different aspect of how land acquisition is related to (1) farming ability and (2) land resale, and the chapters interact with each other to provide a more complete picture of land acquisition. The model created in Chapter III allows individuals to resell their farms after learning about their value. Chapter I provides an individual-level description of the land resale market to further understand the context under which farmers resold their land. Chapters I and III find that purchasers resold their farms much more quickly than homesteaders. In particular, about 30 percent of purchasers resold their land within the first year after gaining the title, as compared to less than 10 percent of homesteaders. This resale pattern means that investments in fences and buildings would have been more valuable to purchasers than to homesteaders. This result is corroborated in Chapter II. Results from that chapter show that purchasers were more likely than homesteaders to invest in durable improvements to their farm, like fences and buildings, which were resold with the farm for greater revenue. The results in Chapter II provide preliminary evidence that purchasers bought land rather than homesteaded it in order to resell it more quickly. Together, the results from Chapters I, II, and III create a broad picture of the land resale market and how the decision to resell land interacted with other land and farming decisions in Kansas in the late 1800s.

I focus on Kansas because it was a state heavily impacted by the Homestead Act. In the decades under study, it received the third most in-migration of any state. By using individual-level data to focus on several counties in one state, this dissertation provides an in-depth examination of many aspects of acquiring public land that could not be addressed by past work that used county-level data to analyze land acquisition in the whole country. Using micro-data in one state complements existing works that addresses the whole country with more aggregate data.

The dataset created by this project combines several unusual sources of individual-level data. These data include samples from the 1860, 1870, and 1880 Kansas Agricultural Censuses; the Bureau of Land Management tract books; the 1860, 1870, and 1880 restricted-access United States Population Censuses; historical Kansas resale deeds; and land quality characteristics. The Kansas Agricultural Censuses provide farm-level data about the inputs and outputs of each small farm, such as the number of bushels of corn grown. The Bureau of Land Management Tract books provide plot-level data about how the farm was acquired from the federal government. The United States Population Censuses provide demographic characteristics of the farmer. The resale deeds describe how and when each individual farm was resold by the owner. Finally, the historical land characteristics, all at the individual farm level, include historical water access, soil quality and soil type, gradient and slope, and historical railroad and town access.

All of these data sources are matched together. This matching allows me to define farm outcomes as a function of settler characteristics at the individual level and land characteristics at the plot level to identify differing farm outcome effects based on settler and land heterogeneity. Matching these datasets together allows for a deeper understanding of how the land market operated and how farming decisions were made.

CHAPTER I

The first chapter examines the relationship between (1) farmers' choices of how to acquire and resell land and (2) the land characteristics at the farm level and demographic characteristics at the individual level. This chapter uses multiple individual-level data sources to lay the groundwork for understanding who acquired land through homesteading, direct purchase, military warrants, railroad grants, pre-emption, and resale; what type of land each chose; and how they farmed it.

I begin by showing that the resale market for land in eastern Kansas in the late 1800's comprised more than three-quarters of the total land market by number of transactions. Despite this fact, most literature on the historical land market has focused on the initial acquisition of land from the federal government. I show that reselling land was sometimes highly profitable for farmers, with an average price of approximately $8 per acre. This figure represents a return of about 7 times because land was initially purchased for $1.25 per acre. However, about 20 percent of farmers lost money by reselling their farms. The widespread distribution of the profits from reselling land introduced a significant element of uncertainty for farmers that have not been fully addressed in the literature. Further, the distribution of how quickly farms were resold varied significantly by how the land was initially acquired from the federal government, even after controlling for land quality, indicating that different administrative requirements of land laws created different incentives and farm types. I explore the implications of this result in Chapter II.

CHAPTER II

The second chapter examines selection into homesteading at the farmer level to determine through what mechanism and to what extent the Homestead Act changed agricultural production decisions and land resale. This chapter explores the causal effect of (1) farmer selection into different methods of acquiring land and (2) the potential for future resale on farm productivity at the individual level in the 1860s, 1870s, and 1880s in Kansas. A complication in determining the causal impact of the Homestead Act on any subsequent outcomes is that individuals who selected to homestead may have been different than individuals who selected to purchase land, and such differences may be endogenous to future outcomes. Individuals who purchased land were plausibly wealthier than individuals who homesteaded, and this wealth may have allowed them to invest more capital into their farms, leading them to be more successful in the future. Therefore, I exploit an unusual feature of the Homestead Act to develop an instrument based on travel costs created by the differing administrative requirements of purchasing and homesteading. I use the distance to the nearest land office active at the time as an instrument for the decision to purchase or homestead because homesteaders were required to go to the land office twice while purchasers only had to go once.

I then create and test a model of agricultural production that predicts the differences in investment decisions. The results show that purchasers and homesteaders used different farming strategies: homesteaders initially invested in crops and livestock, which were not sold with the farm, while purchasers initially invested in durable improvements like fences, which were sold with the farm. Further, I show that even when purchasers and homesteaders produced the same type of agricultural goods, they did so using heterogeneous methods. These results demonstrate how different administrative requirements of land laws interacted with the resale market and how uncertainty about resale impacted farm investment decisions and agricultural production. In particular, the future resale potential of the land impacted which farmers selected which methods of land acquisition initially. This problem is dynamic and involves uncertainty and beliefs, which I explore in the third chapter of my dissertation.

CHAPTER III

The third chapter explores how homesteaders learned about farming in Kansas. Because homesteaders were required to farm the land for several years, they had the opportunity to learn more about farming. Specifically, this chapter addresses (1) what percent of farmers would still have abandoned their land if they had started with more information and (2) how the rate of western expansion would have changed if farmers had been better informed. This chapter models the farmer's learning process by combining a dynamic discrete choice model with a Bayesian learning model that allows for (1) dynamic optimization and (2) some individuals to be better at farming than others. Farmers had an initial belief about the value of farming in Kansas, and then they started farming and learned more information. They used this new information to update their belief and make a more informed decision about whether to sell their farm, abandon their farm, or continue farming over time. Using this model, counterfactuals indicate that if the United States government had not offered homesteading, which encouraged farmer learning, the rate of western expansion would have slowed by about 37 percent. Further, the rate of learning for homesteaders was high: if homesteaders had begun with the information they had learned after 10 years, about 38 percent fewer of them would have abandoned their farm.

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Essays in Labor Economics, Political Economy, and Economic History

This dissertation studies topics in American economic history that intersect with labor economics and political economy. The three chapters focus on understanding how shifts in labor supply, immigration policies, and political connections shape labor market dynamics and economic outcomes.

CLOSING RANKS: ORGANIZED LABOR AND IMMIGRATION

Labor unions have long been central institutions in the labor markets of advanced economies. Throughout the twentieth century, they played a critical role in reducing inequality (Farber et al. 2021), improving working conditions (Rosenfeld 2019), shaping policy (Ahlquist 2017), and influencing political systems (Acemoglu and Robinson 2013; Kaplan and Naidu 2024). Despite fluctuations in membership, unions remain pivotal in today’s economy (Jäger, Naidu, and Schoefer 2024). Yet, considering

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their sustained importance, there is surprisingly little evidence on the drivers of unions' emergence and growth.

This chapter aims to fill this gap with systematic empirical evidence by studying the effect of a large and protracted increase in labor supply on the formation and expansion of labor unions, leveraging the episodes of mass immigration to the United States of the early twentieth century (Medici 2024). The effect is *ex ante* ambiguous, as it influences both workers' incentives to organize and employers' ability to undermine organized labor. On the one hand, increased job competition can motivate workers to unionize in response to economic threats to their employment and wages. On the other hand, a larger labor supply reduces the cost for business owners to replace uncooperative workers and break strikes. Thus, how an increased labor supply impacts unionization is ultimately an empirical question.

This study addresses two key challenges in examining the relationship between immigration and unionization. The first is the need for disaggregated data on the presence and membership of labor unions. The second is establishing causal effects. To measure unionization, I hand-collect and digitize archival records on unions affiliated with the American Federation of Labor (representing over 80 percent of union members nationwide) for the period 1900–1920, to measure the location, quantity, and membership of labor union branches across the United States. These data constitute the first comprehensive dataset measuring historical union presence and density at the local level in the United States. To estimate the causal effect of immigration, I construct a shift-share instrumental variable (Card 2001) to exploit plausibly exogenous variation in the flow of immigrants across counties in each decade. The instrument interacts the 1890 share of immigrants living in a given U.S. county and born in different European countries with the aggregate immigration flows from each country to the United States between 1890 and 1920.

The main results show that immigration fostered the emergence of organized labor. Counties that received more immigrants as a fraction of the population experienced an increase in union presence, the number of union branches, the share of unionized workers, and the number of union members per branch. This finding empirically documents a novel driver of unionization and highlights an unexplored effect of immigration in the labor market. Immigration spurred unionization both at the intensive and extensive margins, as counties with an existing labor movement experienced a growth in union size, while new counties saw the establishment of labor unions. A back-of-the-envelope calculation reveals that, in the absence of immigration, the average union density between 1900 and 1920 would have been 22 percent lower.

The second part of the chapter examines the mechanisms behind the expansion of organized labor. First, immigration strengthened labor unions primarily among skilled workers, especially in counties with higher exposure to labor competition from immigrants. In contrast, it had no statistically significant effect on the unionization of low-skilled workers, whose bargaining power was weakened by the increased availability of inexpensive labor. Second, unionization grew more prominently following an influx of culturally distant immigrants and in areas with less favorable attitudes toward immigration, as indicated by historical vote shares for the Know Nothing Party or baseline levels of residential segregation between U.S.-born individuals and European immigrants. Notably, the results suggest that this effect is unlikely to be explained by immigrants disproportionately joining or forming unions.

In summary, these findings show that immigration substantially contributed to the emergence and expansion of organized labor in the early twentieth-century United

States. The evidence is consistent with existing workers unionizing in response to immigration, driven by both economic and social motivations.

THE IMPACT OF THE CHINESE EXCLUSION ACT ON THE ECONOMIC DEVELOPMENT OF THE WESTERN UNITED STATES

In 1882, the U.S. government introduced the Chinese Exclusion Act, which banned laborers born in China from entering the United States and China-born individuals already residing in the United States from obtaining citizenship or re-entering the country. A central motivation was economic, as proponents argued that Chinese workers reduced economic opportunities for white workers. At the same time, many business owners opposed Chinese Exclusion, worrying that highly productive Chinese labor could not be easily replaced and that a wide-sweeping ban would lead to significant economic losses. There is little empirical evidence on whether these concerns were valid and on the consequences of this policy on white workers and economic activity.

This chapter aims to fill this gap and provide novel and rigorous empirical evidence on the economic effects of the Chinese Exclusion Act (Long et al. 2024). Such effects are ambiguous *ex ante*. On the one hand, reducing the number of Chinese workers can reduce competition for jobs, which can increase wages and employment for other workers (Borjas 2003). On the other hand, it can decrease the demand for other workers or lower their wages, if there are economies of scale or if Chinese workers complement them in production (Ottaviano and Peri 2012). The net effects and their evolution over time are empirical questions.

The analysis examines a county-level panel for the period 1850–1940 for the eight western states where almost all Chinese immigrants resided, using a difference-in-differences strategy that exploits time variation from the introduction of the Act and cross-sectional variation in treatment intensity across counties. The Chinese Exclusion Act should have had little direct effect on counties with few Chinese residents and larger effects on those with many Chinese residents at the time of its enactment.

The results indicate that, as expected, the Act drastically reduced the Chinese labor supply. Contrary to the expectations of its proponents, however, the Chinese Exclusion Act also reduced the labor supply and occupational income scores of white workers, who were the intended beneficiaries of the policy. Moreover, the Act negatively impacted the manufacturing industry, a key and fast-growing sector for the economy of the U.S. West during this period and one where Chinese workers were highly concentrated. Chinese Exclusion reduced manufacturing output, the number of establishments, and labor productivity. Since the western United States grew rapidly during this period, these estimates should be interpreted as a slowdown in growth and not as a decline in levels.

There are two main concerns when interpreting the results. The first one is that, even in the absence of the Chinese Exclusion Act, counties with a larger Chinese population share would have experienced an economic decline. To address this, we use the eastern United States as a "placebo" sample and compare labor force and economic outcomes in counties that, based on their 1880 characteristics, would have had many Chinese immigrants to those that would have had few in the hypothetical scenario that Chinese arrived from the Atlantic. Reassuringly, we find that counties with high hypothetical Chinese shares grew more—and not less—than those with low hypothetical Chinese shares after 1880. The second concern is that the Act might have caused labor and economic activities to move from counties that had a high Chinese share in 1880 to others within the

West. The results indicate no spillovers to nearby areas, implying that reallocation does not drive the results.

The last section of the chapter sheds light on the mechanisms underlying the main results. Consistent with travel costs and information frictions making it hard for employers to replace the "missing" Chinese workers, the results are larger in counties that were less connected to the rest of the country. Moreover, the negative effects on labor supply are driven by white men born outside the western states. These findings suggest that the Act discouraged prospective white migrants from moving to the West and support the interpretation that the Chinese Exclusion Act reduced the aggregate economic development of the West. Second, consistent with the presence of complementarities between Chinese and white workers, we find that places that lost more skilled Chinese workers also experienced a larger decline in skilled white workers and manufacturing output.

Although the magnitudes of the estimates are specific to this context, the insights that the loss of productive immigrant labor can have adverse economic effects on the remaining workers are generalizable. In particular, the findings indicate that this can occur in settings where immigrant workers are concentrated in key economic sectors (as the Chinese were in the mining, railroad, and manufacturing industries) or when they are not easily replaceable by other workers or technology.

POLITICAL CONNECTIONS, CAREERS, AND PERFORMANCE IN THE CIVIL SERVICE: EVIDENCE FROM U.S. FEDERAL JUDGES

Political appointments are the primary method for selecting public sector workers worldwide (Lim and Snyder Jr 2021). Judicial appointments follow a similar pattern: by 2021, 70 percent of the world's nations filled court positions through presidential appointment (CIA 2021). Despite this, there is surprisingly little evidence of the consequences of politicians' influence in judicial nominations.

In this chapter, we leverage the appointment process of U.S. federal judges to provide the first within-judge estimates of how political connections influence judicial performance (Medici and Pulejo 2024). Federal judges in the United States are nominated based on recommendations from home-state senators who share the same party affiliation as the president. Using individual-level data on judges and senators from 1789 to 2019, we link each judge to their recommending senators. We then use a difference-in-differences design to analyze how the departure of these senators from Congress impacts judges' productivity and careers.

The main results indicate that district court judges produce fewer judicial opinions—a well-established measure of judges' output—after their recommending senators exit office. A reduction in the number of opinions issued could indicate decreased effort—such as judges taking longer to work on a case, resulting in fewer cases closed per year—or increased care in crafting decisions. The results show no significant improvement in the quality of judicial opinions following recommenders' exit, as measured by the opinions' length, number of citations included, and number of citations received. Event-study estimates confirm that judges' output begins to decline only after their recommenders leave the Senate, supporting the parallel trends assumption underlying our identification strategy.

To ensure that the negative effect on judicial productivity is driven by the loss of connections with home-state senators, we present two additional results. First, we document that the treatment effect remains negative and significant when a judge's recommender is replaced by a co-partisan. Second, we conduct a falsification test on judges who, at the time of their nomination, had no home-state senators from the president's party and show that they do not experience any change in output once such senators leave Congress.

Next, we explore the mechanisms linking the loss of senatorial connections to the observed decline in judges' productivity. We find a large negative effect of recommenders' exit on judges' probability of being promoted to an upper-level court, implying that the loss of political connections essentially shuts the door to judges' advancement within the U.S. federal judiciary. These results suggest that the reduction in effort is likely driven by an erosion of career incentives. In accordance with the rules for federal judicial nominations, this impact is more prominent in years when judges share partisanship with the sitting president and thus can benefit from the support of their senatorial connections.

In summary, these findings highlight how political appointments can incentivize civil servants through career concerns but also show that these incentives are closely tied to the tenure of their political patrons.

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Essays on Inequality in Cities: Evidence from the Interstate Highway System

This dissertation investigates the impact of the largest infrastructure project in American history—the Interstate highway system—on inequality by race and class and its lasting intergenerational consequences on children’s outcomes. It is organized into two core projects. First, I examine the contemporaneous effects of highway infrastructure where I follow a quantitative approach to elucidate the key forces behind the unequal effects and highlight the role of institutions for disparities by race. As a result of exclusionary institutions that limited access to areas outside of the urban core for Black families, they benefit far less from highway development than in the absence of these spatial barriers. Second, I consider how the permanent transformation of neighborhoods brought about by the Interstate system altered the geography of opportunity for long-run individual outcomes. Building on these results, I provide a general framework for measuring the aggregate impacts of place-based policies on intergenerational mobility.

Both projects draw on several rich historical datasets I have constructed from restricted Census micro-surveys, digitized archival maps, and historical tax forms covering the entire country for the mid-twentieth century. To build these new data for a tremendously large number of observations, I employ several computationally intensive geospatial and machine-learning methods. I describe my findings and methodological approach in more detail next.

UNEQUAL ACCESS: RACIAL SEGREGATION AND THE DISTRIBUTIONAL IMPACTS OF INTERSTATE HIGHWAYS IN CITIES

The Interstate highway system is a defining infrastructure project of American history and was characterized by two main transformative impacts. Its central aim was to facilitate commuting between locations, and these benefits motivated federal policymakers to invest monumental sums of funding toward its construction. On the other hand, highways also came with substantial costs such as local pollution, noise, and the splitting of existing communities (Mohl 2004; Currie and Walker 2011).

In my primary dissertation project (Weiwu 2024a), I investigate the distributional consequences of the Interstate system, which historical narratives have previously suggested increased inequality in cities, for example, Caro (1974), Jackson (1985),

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and Rose (1990). During construction in the 1960s, segregation in urban areas reached its peak as pervasive legal and extralegal institutions excluded Black Americans from homogeneous White neighborhoods (Massey and Denton 1993; Cutler, Glaeser, and Vigdor 1999; Rothstein 2017). This segregation concentrated Black families in the city center, where Interstate roads were designed to intersect. Commuting improvements further appeared largely in suburban areas that prohibited the entry of minority families.

Yet, several challenges have stood in the way of a systematic assessment, which this paper overcomes by undertaking the collection of spatially granular commuting statistics for the entire United States. I put forth a mechanism for why, in particular, racial disparities emerged: Black households were constrained by *exclusionary institutions* from leaving central areas, which interacted with the Interstate system to create stark inequalities in impacts. To quantify the strength of this mechanism, I address the challenge of distinguishing the institutional forces of segregation from other competing *economic* forces of housing affordability and *social* forces of desire to self-segregate with same-race neighbors.

I leverage restricted microdata from the 1960 and 1970 Decennial Censuses, which includes the previously under-studied Journey to Work survey, to construct the first historical measures of commute flows in 25 cities. As large-scale commuting surveys were never administered for this period, I generated commute time matrices at high spatial resolution across 49 million bilateral pairs using road maps I digitized from Shell Atlases for 71 cities.

With these data, I document declines in population, rental prices, and racial composition (percentage White) by highways, which are informative of the local costs and equilibrium responses for neighborhoods. The population shifts are driven by White migration, and importantly, I find there is essentially no Black migration, suggestive of their limited ability to relocate. Using a multi-fold empirical strategy where I designate historical roads as control groups and instrument with planned maps I digitized for 100 cities and a Euclidean ray network, I find these declines can be interpreted as causal.

I next show that the population grew in suburban areas where connectivity increased as evidence for the commute benefits of highways. Because peripheral growth did not originate solely from Interstate construction, I control for distance from the central business district and exploit variation within the suburbs relative to the comparison roads. Consistent with the response to highway costs, the response to the benefits is near zero for Black families.

Why do Black households not respond to either effect of highways? Spatial frictions from institutions may be key. I draw on redlining maps from the Home Owners' Loan Corporation that evaluated neighborhood credit risk and proxied for practices by private actors, real estate agents, and government officials to preserve the homogeneity of White neighborhoods (Nelson et al. 2020). Including redlining fixed effects in the specification and measuring Black responsiveness within redlined areas, population responses are no longer zero and are in fact significantly positive, indicating barriers inhibit free movement across types of neighborhoods.

Going beyond the empirical facts and disentangling feedback between prices, sorting, and residential choice, in the second part of the paper, I develop a quantitative model of neighborhoods with a novel focus on how institutions shape heterogeneous impacts. Institutional barriers are present via neighborhood-specific wedges that differentially affect Black households. Employing the model, I find that Interstate construction lowers Black welfare by -1.04 percent and raises White welfare by 2.9 percent. Class gaps are far smaller with gains for all education groups. Much of the racial gap is due to the lower reallocation of Black households, who remained in central areas where costs outweigh benefits, while White households migrated to the suburbs and away from the harms of highways.

I then show that institutional segregation is a primary mechanism behind racial disparities in highway impacts. Racial composition sharply shifts over borders of redlining maps, but the discontinuity cannot all be attributed to institutions. Housing prices vary and sorting from racial preferences can reinforce any differences. In a border discontinuity design and using estimated parameters for price sensitivity and preferences for racial composition, I find that 65 percent of the massive rise in population (140 log point increase) for Black households crossing into redlined neighborhoods is unaccounted for by prices, racial preferences, or socioeconomic variables. The remaining component represents residual fundamental characteristics containing exclusionary institutions.

Simulating the removal of barriers, constructing Interstate highways leads to less unequal impacts: Black households are more spatially dispersed and consequently harmed less by Interstate development. Removing all race-specific wedges in neighborhood choice, I provide Black households the same access as White households, a hypothetical upper bound on how far exclusionary barriers can be eliminated. Under this arrangement, the Black population gains by 1 percent from Interstate highways so that all groups benefit from highway development, and the racial gap in welfare impacts closes by a striking 54 percent.

In conclusion, this paper goes beyond the existing literature on aggregate effects on interstate highways (see Baum-Snow 2007; Michaels 2008; Brinkman and Lin 2022) to quantify the *distributional* impacts, made possible by the novel disaggregated data and granular archival maps constructed for this dissertation. Moreover, I extend the existing theoretical frameworks in the quantitative spatial economics literature summarized in Redding and Rossi-Hansberg (2017) to incorporate institutions and demonstrate their central role in inequality across space and groups. I find that discriminatory institutions entail welfare costs, and importantly, I go one step further—I examine how residential exclusion *interacts* with highway infrastructure. This interplay between institutions and policy extends to other neighborhood interventions beyond transportation infrastructure and animates why there exist profoundly disparate impacts by race.

OPPORTUNITY IN MOTION: EQUILIBRIUM EFFECTS OF A PLACE-BASED POLICY ON ECONOMIC MOBILITY

In my secondary dissertation project (Weiwu 2024b), I consider the intergenerational consequences of this prominent place-based policy. The United States exhibits vast disparities in economic opportunity across its cities and persistent racial gaps in the long-run outcomes of children, even conditional on parental income (Chetty et al. 2020). Place is commonly considered a leading determinant of intergenerational inequality (Wilson 1987; Reardon and Bischoff 2011). A natural question to ask is: Can policies that target places alter them for the better and influence these gaps? The answer depends on which neighborhood characteristics contribute to positive outcomes and to what extent place-based policies enhance or diminish these characteristics to transform economic opportunity across places.

Promoting one location can come at the expense of others through the shuffling of peer quality among locations, given that high-status peers are in fixed supply. In this paper, I exploit the construction of the interstate system to investigate the impact of policy for intergenerational mobility through *two channels*. First, I find highways dramatically increased access to workplaces for suburban neighborhoods and consequently raised incomes to boost economic opportunity. Second, given the policy's massive scale, households responded by migrating toward areas with greater commuting cost reductions. This response is heightened for more-educated, higher-occupational status, and White families. Places left behind—in this case, central city neighborhoods—became populated by fewer advantaged peers and subsequently faced a loss in peer externalities. The migration responses, which in turn affect peer composition, are "general equilibrium" indirect impacts that additionally influence the level of economic opportunity.

Place-based policies often generate spillovers that create local gains but losses elsewhere (Glaeser and Gottlieb 2008; Kline and Moretti 2014). Rather than studying spillovers through agglomeration, the focus of Duranton and Puga (2004) and Greenstone, Hornbeck, and Moretti (2010), this paper advances an alternative local externality that likely plays a larger role for neighborhoods: spatial sorting or segregation, which has been documented to be a key determinant of inequality in productivity, human capital, and long-run outcomes (Sharkey 2008; Chetty and Hendren 2018; Fajgelbaum and Gaubert 2020).

I develop a flexible theoretical framework to examine how equilibrium sorting of large-scale policies affects long-run consequences. The innovation is to measure the economic mobility of children, rather than productivity or welfare, as the outcome of recent quantitative economic geography models (Redding and Rossi-Hansberg 2017). Since the policy creates winners and losers, the framework enables assessing whether in *aggregate*, children's outcomes were improved—or rather, if gains in some locations were offset by losses in others—and whether the gains were shared equally across races.

To measure the long-run outcomes of children during interstate construction, I employ novel parent-child linkages for the 57 million children born between 1964–1979, constructed at the Census Bureau using historical IRS tax data. Described in Stinson and Weiwu (2023), we apply name-matching techniques that incorporate machine learning methods and restricted names from the Social Security Administration. We attain a high match rate of 67 percent. These newly linked cohorts fill a crucial gap for large-scale measures of intergenerational mobility as this period spans the Civil Rights movement, the War on Poverty, and the creation of Medicaid. Using these linkages, I find upward mobility for Black children was strikingly low. Black children from the top quintile of parental income were more likely to fall to the bottom quintile than remain in the top. Conditional on family income, Black children reach adult income ranks 17 ranks lower than White children.

This project makes a few key contributions. A vast literature exists on the measurement and implications of intergenerational mobility such as Solon (1992), Abramitzky, Boustan, and Eriksson (2012), and Collins and Wanamaker (2014). I provide the first large-scale measures of intergenerational mobility for the 1960s and 1970s, a critical period in U.S. history. Most closely related is recent work by Chetty and Hendren (2018) and Chetty et al. (2020) using IRS administrative data to study the geography of opportunity. In this paper, I provide evidence of how large-scale policies alter places and neighborhood segregation to show that economic opportunity is not fixed over time. The key implication is that instead of moving families to better neighborhoods, policymakers can influence the levels of opportunity across places through both direct targeting as well as secondary equilibrium spatial spillovers between locations.

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The Dissertations of Mallory Hope, Felix Schaff, and Colin Sharpe: 2024 Alexander Gerschenkron Prize Competition

Each year at the Economic History Association meetings, we come together for a unique celebration of the efforts made by new scholars where we highlight the very best work produced. This unique celebration creates a sense of community and collegiality that acts as a potent offset. It is thus a great honor for me to present the candidates for the Gerschenkron Prize.

We received many submissions—all of which were fine works. It was, and this is no sop, a very difficult task to arrive at a shortlist. All the submissions were strong works of economic history. Picking out which stood out most from what was a great set made us appreciate how much work went into these efforts. Before I proceed, I want to encourage all the scholars who submitted their dissertations for our consideration to submit their work to journals such as the *Journal of Economic History*, *Explorations in Economic History*, *Cliometrica*, *European Review of Economic History*, *Economic History Review*, *Essays in Economic and Business History*, *Scandinavian Economic History Review*, *Economic History of Developing Regions*, and *Asia-Pacific Economic History Review*. All of these would be fine homes for these good works. Also, I should point out the wise words of James Buchanan to his Ph.D. students—send your work out, never let it sit.

The three deserving finalists have each produced work that makes significant contributions not only to global economic history. Each one made serious efforts to deal with "big" questions while mixing novel methods and data to do so. Each one also opens new venues for research on, which many of us can (and should) build. I do not believe I can fully do justice to their work here, but I want to explain each briefly to provide an enticement to spend a few enjoyable (and instructive) hours reading them in more detail.

MALLORY HOPE

As Ronald Coase once quipped "the provision of markets is an entrepreneurial activity and has a long history." Since institutions are chosen to maximize wealth net of transaction costs, entrepreneurs who create markets are those who can find lower transaction costs. The work of Mallory Hope on French maritime insurance can be interpreted as providing a strong business history study of how entrepreneurs in insurance created a market that, while it had existed before, was far more sophisticated than anything observed (or believed). In the process, she opens the door to multiple scholars in other fields of economic history.

Hope's starting point is a market with few companies or corporations specializing in insuring vessels and where most underwriters were "individual merchants or small partnerships for whom insurance was a sideline." The question is how was it possible for these merchants to "translate all of dangers that ships and cargoes were exposed to at sea into just one number, an insurance premium" at a time when people had to rely on proto-probabilistic reasoning given the lack of knowledge of empirical data to connect with formal probability theory.

Previously ignored, a few thousand maritime insurance contracts from French port cities from the 1680s to 1820s are used to document the emergence of marine insurance markets outside of the typically well-considered "Anglo-Saxon" cases. The use of these contracts—previously ignored—is novel. They provide a wealth of information about marine insurance that can be complemented with other qualitative sources. For that alone, Hope's work is worth consulting as a guide to a potent empirical tool.

Her findings are numerous and so I am going to name only a few. First, she finds that this was a highly competitive market. French insurance providers were numerous in the ports she studied but they were also competing with providers in other ports in both France and Northern Europe. Insurance quotes were solicited from multiple ports by shipowners via letter. If more favorable deals in a more distant port were found, coverage could be secured through an agent. Prices were competitive with those in other markets.

Second, French underwriters were able to price different sea lanes differently—suggesting an understanding of risk levels—and were shown to also be able to design different contractual terms that minimized usual problems tied to insurance (e.g., moral hazard). Alone this is a fascinating finding as it suggests that competition was generating a great deal of proto-probabilistic insights about how to properly price services. It was not formalized or scientific, but it worked as if it was. She also finds that what caused the greatest complications for insurers were not the natural causes of shipwrecks. Rather, wars and political uncertainty were the main headaches for insurers who nevertheless found ways to price these human-made risks into the premiums. She also finds that premium rates were falling over time (suggesting productivity gains in insurance provision).

Third, Hope observes that while the provision of insurance varied across the ports she studied, there were no significant differences in performance. The key factor was the creation of a stable framework for exchange. Additionally, competition in rule-making offered the flexibility to tailor rules to specific contexts and incentivized institutional improvements. Moreover, the informal governance of private institutions was enhanced by the crucial involvement of state-based institutions, marking a stark contrast between France and Britain in this regard. This point alone invites Hope's work into broader debates regarding differences in development between Britain and the rest of Europe.

Before proceeding to the next dissertation, it is worth noting that this work exemplifies a remarkable symbiosis between historical research and economic theory, deserving recognition for its interdisciplinary qualities.

FELIX SCHAFF

Few topics generate as much interest as inequality—its current evolution, causes, and influence on historical institutions and economic development. Felix Schaff's research takes a serious look at the latter aspect, specifically focusing on German-speaking areas of Europe, where less is known.

The first part of Schaff's dissertation is the first step of any good work of economic history that tries to move into the political economy consequences of inequality: creating data about the underlying trends. He does so by expanding the work of Alfani et al. through the addition of tax data from multiple new areas to the existing data. This radically increases the quality of any estimates produced. He finds that in Germany from the fifteenth century to the early nineteenth century, there was an increase in wealth inequality (with ups and downs in some periods, such as the Black Death). By 1600, inequality had reached a historical peak, with economic elites holding the largest share of wealth, while the poor classes held the smallest. The poorest half of the population experienced its most prosperous period from the early to mid-eighteenth century. The *general* trend in living standards over the period also suggests that it is quite likely that there was a worsening of living standards at the bottom end of the ladder. This is an increase in inequality before modern economic growth—something that would puzzle a 1950s Simon Kuznets.

The second part concerns a test of Walter Scheidel's point that big drops in inequality tend to follow wars and other large catastrophes. Contrary to Scheidel, Schaff finds that wars required the adoption of new taxes to finance resistance to predation by other polities. The choice of taxation tools tended to be more regressive—thus reinforcing inequality. The last chapter of Schaff's dissertation connects itself quite well to this by pointing out that the expansion of state capacity during the period was used by certain elites to better extract rents—something that further increased inequality. This is an interesting point that should allow Schaff's work to be embedded within the literature on state capacity and its growth.

The third part concerns how the Reformation expanded social welfare but in a way that privileged certain groups over others. This is somewhat reminiscent of the work of Ira Katznelson and Richard Rothstein who suggested that major programs of the New Deal and Fair Deal in the 1930s and 1940s were built on deeply discriminatory foundations that excluded certain groups of workers in ways that prevented the racial economic gap in America as much as it could have been closed. This is an interesting parallel of how political economy settings can affect the shape of redistribution that is enacted in ways that make redistribution regressive at certain ends of the income distribution.

COLIN SHARPE

Lastly, we have Colin Sharpe, who revisits the familiar topic of Britain's rise as a leader in living standards. However, this is far from a minor addition to an already vast body of work. Instead, it represents a contribution at the margin, a distinction that sets it apart. After all, there's a significant difference between making marginal contributions and contributing at the margin.

He does not attempt to diminish existing explanations such as those revolving around the high-wage economy hypothesis or the importance of institutional reforms. He identifies a gap in the literature where some divergence between Britain and the rest of Europe had already occurred well before the Industrial Revolution. He thus attempts to shift to an earlier institution that could have explained the earlier source of lead. What is that earlier institution? The English common law—a topic dear to classical economists and jurists who emphasized its importance.

The first chapter uses the English-Welsh (i.e., the Marches of Wales) border to observe the causal importance of the common law. The March of Wales offers a unique case to examine how access to common law courts affected the British economy. Established in the late eleventh century as a military buffer between Norman England and independent Wales, the March granted nobles significant privileges, including military powers and judicial independence, preventing appeals in royal courts. These were initially similar to the rest of England. However, as common law developed in the twelfth century, access to royal justice became easier elsewhere but remained limited in the March of Wales, which retained manorial law. This legal distinction persisted until the laws were unified in the 1530s, despite Wales' incorporation into England in the 1280s. Common law areas used more modern, impersonal, and lower-(transaction)-cost arrangements. Small landowners invested more due to improved access to credit, while large landowners reduced investment, possibly due to increased regulation. Over time, common law areas were more likely to develop rural markets and a larger middle class of farmers. These results support the view that England's distinct legal institutions made medieval factor markets more efficient, fostering the growth of a middle class engaged in commercial agriculture. The third chapter of the dissertation is logically connected to this one in that Sharpe explores why jurists of common law courts gradually became a recurrent adversary of royal absolutism in England. This chapter was, for me, the most fascinating for reasons that still elude me to some degree. It speaks to how certain *de facto* checks on royal absolutism were created—a result that is clearly relevant to economic historians.

The second chapter extends the same effort of understanding Britain's early lead by looking at the differences in guild power across Europe. He uses a new panel of city-level data on various aspects of power of textile guilds in Europe from 1200 to 1850 with the going (confirmed) hypothesis that guild power was greater where the state had greater financial needs due to recurring conflicts. He then finds a connection between guild power and the level of "market development" (for lack of a better term). This latter connection, which explains why guilds were weaker in Britain, is motivated by the idea that weak guilds are a byproduct of competitive external pressures. The chapter presents a nice, simple, and elegant model tying these together.

VINCENT GELOSO, *George Mason University*

Underwriting Risk: War, Trade, Insurance, and Legal Institutions in Eighteenth-Century France and Its Empire

In 1781, the Académie des Sciences announced a prize to elicit scholarly interest in France's insurance industry. The Marquis de Condorcet—a mathematician and philosopher best known for his role in the French Revolution—presided over the call for the best original contribution to "the theory of maritime insurance."[1] However, his call attracted a paltry number of entries. Originally slated to announce a winner in 1783, the Académie had to postpone its decision twice.[2] In the 1780s, few mathematicians showed themselves willing to theorize insurance transactions. The business of evaluating and underwriting risk was practically the exclusive province of merchants and shipowners, a worldly class of traders that the aristocrats and military engineers who usually competed for the Académie's prizes rarely associated with.

The disappointing response to the prize competition prompted the Marquis de Condorcet to offer candidates a few hints and to publish his basic theoretical approach to the problem of insuring commercial vessels against shipwreck and capture. Unfortunately, his model was of little use to insurers, as it required knowing the probability of loss or damage during a sea voyage, when foreseeing and quantifying risk was precisely the complex skill that theory was supposed to explain or formalize. Condorcet recognized the shortcomings of his first attempt but believed that the risks of navigation could eventually be estimated empirically "based on experience, that is to say based on the knowledge of past events in the same line of trade or similar lines of trade."[3]

Without Condorcet's knowledge, insurance underwriters and industry regulators had come to very similar conclusions by the 1780s, around the time France sealed an alliance with the Thirteen Colonies, pitting itself against Britain in yet another imperial war. My thesis, *Underwriting Risk: Trade, War, Insurance, and Legal Institutions in Eighteenth-Century France and Its Empire,* tries to understand this convergence of ideas. I also follow the evolution of marine insurance markets in France from the 1680s to the 1820s and show how practitioners, who depended for their livelihoods on placing good bets in insurance markets, estimated risk without the benefit of actuarial science and in the absence of data about the historical frequency of shipwrecks and pirate and privateer raids.

French insurers, like the famous "Names" who underwrote voyages at Lloyd's Coffee House in London during the eighteenth century, were not specialized firms but

[1] Charles-François Bicquilley, *Théorie élémentaire du commerce*, ed. Pierre Crépel (Lyon: Aléas, 1995, p. 211).

[2] Jean-Antoine-Nicolas de Caritat Condorcet, *Arithmétique politique*, ed. Bernard Bru and Pierre Crépel (Paris: Presses universitaires de France, 1994, p. 466); Bicquilley 1995, p. 211.

[3] "…d'après l'expérience c'est-à-dire d'après les connaissances des événemens passés du même commerce ou d'un commerce semblable." Condorcet 1994, p. 468.

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individual traders or small partnerships who purchased insurance themselves whenever they outfitted a ship while continuing to underwrite the voyages of others. Through the archive that is the cornerstone of my thesis—a collection of over 500 registers created by notaries in France's major Mediterranean port of Marseille—I study how underwriters rated the dangers of individual sea voyages and which features or contract terms increased the risks they felt they were exposed to. Marseille's notarial registers record an estimated 150,000 insurance transactions spanning the 1680s to the 1820s. Out of this abundance of material, I constructed a representative sample of policies negotiated through this market as well as a second dataset that offers more granular detail on market activity during the Seven Years' War, a major turning point in Britain and France's eighteenth-century rivalry sometimes termed the "Second Hundred Years' War." Marseille's notaries produced an exceptional historical record because they set down every word of the contracts they brokered. In each entry, notaries quoted premium rates alongside the policy's detailed terms, which allowed me to perceive patterns in how the cost and the extent of coverage were related. Most researchers who have worked on the history of insurance before 1870 have used sources such as underwriters' and insurance brokers' account books that do not allow us to place premium rates in the context of the terms of the insurance coverage.

Marine insurance underwriters in the eighteenth century did not keep statistics on the number of accidents that took place at sea, nor did they consciously calculate the probability of an accident before they offered to insure a vessel at a certain rate. Although many wrecks and ship captures were reported in the British newspaper *Lloyd's List* and in the registers of insurance claims maintained by the Chamber of Commerce in Marseille, the number of ships and of cargoes voyaging along a given sea lane that were insured each period was not recorded in any public register. To further complicate matters, insurers in important ports like Marseille underwrote many vessels and shipments belonging to foreign merchants, so to formally calculate the historic frequency of claims, they would have needed information not only about local port traffic, but also for commerce on a European, Atlantic, even a global scale.

Underwriters did not bemoan the lack of statistics on shipping volumes or historical claims because they had at hand a reliable set of heuristics that allowed them to make split-second decisions about whether or not to insure a voyage at a premium they were offered on the exchange floor or in a notary's office. Insurers seem to have had a baseline rate in mind for vessels traveling along a major sea lane—perhaps 2 percent for a voyage from Marseille to a Levantine port, or 6 percent from a Mediterranean harbor, through the Strait of Gibraltar, to Saint-Domingue, although these basic rates would not remain fixed for all time. In Marseille, average insurance rates decreased between 1680 and 1820 as ship construction and safety at sea improved and as underwriters updated their risk assessments. The baseline rate could be nimbly adjusted up or down based on the unique characteristics of the risk—whether the insurance purchaser was asking for protection only in the case of total loss or wanted coverage for partial loss as well, whether the asset being insured was something imperishable, like specie—and undoubtedly based on the underwriter's private opinion of the skill and sobriety of the captain. Since it was common practice in the eighteenth century for merchants to purchase insurance coverage in a distant port and across state borders, competition kept insurance rates in check. Peacetime premiums came close to approximating the actual frequency of accidents at sea.

Although early modern underwriters were able to estimate and put a price on the risks of navigation according to stable and reasonable principles, they did not conceive of their expertise as the application of an objective formula or a simple projection of the historical frequency of insurance losses onto a portfolio of risks. Balthazard-Marie Émérigon, a practicing lawyer in Marseille and expert in insurance law, described the determination of insurance premiums as:

> a kind of game which demands great prudence on the part of those who apply themselves to it. It is necessary to analyze the dangers, and to know the science of calculating probabilities; to foresee the perils [literally, reefs] of the sea and those of bad faith; to not lose sight of bizarre and extraordinary cases; to combine everything, to compare it with the rate of premiums, and to judge what will be the overall result. Such speculations are a work of genius.[4]

"Prudence," long experience, and agile adjustments to recent news were the centerpiece of insurers' skills because marine insurance in the eighteenth century did not only protect ships and cargoes from predictable risks that were primarily seasonal and geographic. It also functioned as insurance against political uncertainty.

In the eighteenth century, every time France came into conflict with another maritime power (and sometimes when French diplomats made peace) insurance markets were thrown into a state of turbulence. Premium rates in the wake of an outbreak of war could easily skyrocket to 25 percent, 35 percent, or even 60 percent of the value of the vessel or cargo being insured. High prices reflected the new wartime risks French commercial ships faced from British naval squadrons and privateers.

In eighteenth-century France, the press was censored, and the crown's diplomatic agenda was a secret of the state. At the beginning and often at the dénouement of conflicts, many underwriters and shipowners who had purchased insurance policies for their vessels failed to prepare in advance for the shock. Either they underwrote a fleet of merchant ships at close to peacetime rates, not anticipating that a third of the vessels in their portfolio might be gobbled up by British privateers, or (in the merchant-shipowner's case) they agreed to pay wartime insurance rates before realizing that lines of trade would soon be safe again, the markets their ships would target would be flooded with trade goods, and their sales would not cover the inflated insurance premiums they owed. Throughout the eighteenth century, inevitable failures of foresight like these drove insurance underwriters and merchants to court. Since they hinged upon matters like the exclusive prerogative of the crown to declare a state of war, insurance lawsuits frequently moved up the ladder of appeals and were heard by the *Conseil d'État* (Council of State), the supreme court in France.

At the beginning of the eighteenth century, the *Conseil d'État* responded to these wartime lawsuits by holding merchants and underwriters to the letter of their previous contracts, enjoining policyholders to pay the premiums they had promised, even if

[4] "…une espèce de jeu qui exige beaucoup de prudence de la part de ceux qui s'y adonnent. Il faut faire l'analyse des hasards, et posséder la science du calcul des probabilités, prévoir les écueils de la mer, et ceux de la mauvaise foi, ne pas perde de vue les cas insolites et extraordinaires, combiner le tout, le comparer avec le taux des Primes, et juger quel sera le résultat de l'ensemble. Pareilles spéculations sont l'ouvrage du génie." Balthazard-Marie Émérigon, *Traité des assurances et des contrats à la grosse,* vol. 1 (Marseille: 1783, p. 14).

those prices ended up being out of the proportion to the perils their ships were actually exposed to. As the central state continued to see a rash of insurance lawsuits during later wars, however, the *Conseil d'État* became convinced that the slow percolation of news of sea battles and privateer attacks was a source of market failure. The state redefined its role in wartime insurance lawsuits as compelling policyholders to pay "equitable" premiums for wartime coverage, even if that meant overruling the terms of their original agreements. The preferred ammunition in these legal battles over wartime premiums was statistics: What percentage of French ships crossing the Atlantic were actually lost in a given period of conflict, and based on that data, what price should policyholders have to pay insurers for running their risks?

In successive periods of conflict and insurance market crises, institutions at different levels of the France state were involved in determining the "equitable" premiums that should be imposed. A solution adopted in many major French ports by the American War of Independence enjoyed the greatest popular support among the merchant community: city-level chambers of commerce elected special commissions to study the records of active underwriters and determine historical trends in insurance rates and in ship captures during the first months of a conflict. Based on this research (which was very similar to the work Condorcet was calling for), French Chambers of Commerce issued schedules of adjusted insurance rates that should be applied to ships venturing out at the beginning of a conflict.

The findings in *Underwriting Risk* make three main contributions to European economic history. First, in this dissertation, I bring to scholars' attention an unexplored archive, notaries' insurance registers in Marseille. These entries offer an unbiased cross-section of a preindustrial financial market, recording daily dealings in that market across more than 140 years. Second, my work challenges a frequently repeated historical narrative that early modern insurance markets emerge out of advances in the mathematics of probability. In fact, merchants' letters and their market behavior show that making decisions about insurance in the preindustrial maritime economy involved little formal calculation. It was only in the context of wartime legal battles that French insurers began to articulate a theory that insurance premiums ought to be in line with the historical frequency of claims and with expected losses. Finally, my dissertation questions many historians' dismissal of France's economic policies. The Old Regime usually comes in for criticism for trying to exert too much control over the economy and stifling private enterprise, but the state's regulation of marine insurance markets hardly deserves this critique. In 1681, during Colbert's term as Finance Minister, the central state established laws to govern insurance contracts nationally as well as a special set of courts that would have exclusive jurisdiction over insurance lawsuits. These institutions helped insurance markets to flourish in French port cities over the course of the eighteenth century. Insurance was an industry that required special tutelage from the state, particularly in the second half of the eighteenth century as the scale of imperial wars grew and British sea power became more pronounced. Although the French state intervened boldly and creatively to adjust insurance rates during wartime crises, these interventions always came at the instigation of private merchants who were embroiled in legal battles with their insurers, and they relied on local institutions like chambers of commerce for their enforcement.

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The Political Economy of Inequality in Preindustrial Europe

Why was economic inequality already high when industrialisation and modern economic growth began? Following Kuznets (1955), growing income or wealth inequality has long been considered a phenomenon attributable to the Industrial Revolution. This view is likely outdated. First, in several regions of preindustrial Europe, economic inequality rose considerably at least from the end of the Middle Ages, and was already relatively high at the onset of industrialisation. When inequality reached its historical maximum in the early twentieth century (Piketty 2014), it had grown for about four centuries (Alfani 2021). If we want to understand where high inequality in the twentieth century came from, we must understand what happened during the early modern period. Second, many factors typically thought of as driving inequality in the industrial era, such as modern economic growth, unfettered capital markets or industrialisation, barely existed in preindustrial times. We need explanations for rising inequality that are different from Kuznets' mechanical relationship between growth and inequality.

Many different theories exist about the determinants of economic inequality in the preindustrial period; proposed potential causes include economic expansion, political institutions, cultural factors, and demographic growth (see van Zanden 1995; Piketty 2020; Alfani 2021). However, in terms of systematic empirical evidence "we have at best some guesses about the forces that might explain changes in [preindustrial] inequality" (Milanovic 2018). In this thesis, I do two things: I construct novel, long-run inequality statistics for German-speaking Central Europe; I then test the impact of three central facets of the political economy of early modern Europe on inequality: warfare, oligarchic city governments, and the Protestant Reformation. I focus on German-speaking Central Europe because it has exceptionally rich and hitherto underutilised archival records, making it possible to study inequality at the "national" macro-level, the town- and village-level, and the individual level. Central Europe was also economically, culturally, and politically very diverse, with rich and poor regions; it was a cockpit of preindustrial warfare; Europe's main cultural dividing line—Protestantism vs. Catholicism—ran right through it; and state capacity was highly fragmented. Its data richness and diversity make it the ideal laboratory for testing theories about the causes of preindustrial inequality.

NEW INEQUALITY ESTIMATES FOR GERMAN-SPEAKING CENTRAL EUROPE

In the first part of the project (Chapter 2), I collect (with Guido Alfani and Victoria Gierok) new data from archival documents and secondary sources to estimate the extent of wealth inequality in a panel of towns and villages, and at a hypothetical "national" level. These sources are registers of town- and village-level property taxes, similar to modern wealth taxes, levied at the household level. Taxes had to be paid by the entirety of civilian households, such as craftsmen, peasants, and merchants, as well as very poor and very rich households. Overall, we collect about 430,000 household wealth

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observations in 89 rural and urban communities, making it possible to construct almost any inequality indicator (e.g., Gini, wealth percentiles).

This new evidence shows German-speaking Central Europe followed a secular trend of inequality growth between the fourteenth and nineteenth centuries, interrupted by two idiosyncratic shocks, the Black Death epidemic (1348) and the Thirty Years' War (1618–-48). This second shock was nothing less than the most destructive war in European history, killing about 40 percent of the German population. The drop in inequality caused by the war makes the region stand out from all other areas for which comparable data are available (see Alfani, Gierok, and Schaff 2022, 2025).

ARE WARS GREAT EQUALISERS?

It is a major stylized fact in the inequality literature that wars reduce inequality through the destruction of capital, demographic decline, the confiscation of wealth of the rich, plundering, state collapse, or decline of trade and commerce (see van Zanden 1995; Piketty 2014; Scheidel 2017; Alfani, Gierok, and Schaff 2022). Warfare was omnipresent in preindustrial Europe (Voigtländer and Voth 2013), raising the question of why there was only one substantial drop in inequality associated with warfare in Central Europe. In this chapter (Schaff 2023), I argue the "wars are great equalisers" hypothesis is incomplete for the preindustrial period, a consequence of most studies focusing on major wars. In actuality, wars had two countervailing effects; destruction could reduce inequality (Scheidel 2017), but fiscal extraction could increase inequality, because it usually happened through regressive taxation (Alfani 2021), the result of political authorities being induced by the threat of war to extract economic resources. The extractive effect often outweighed the geographically limited destruction of ordinary military conflicts, so most wars in the preindustrial era led to higher economic inequality. Only in major wars—such as the Thirty Years' War, or the world wars in the modern era—could destruction outweigh extraction and reduce inequality.

To test this theory, I combine my disaggregated data on inequality in towns and villages with geographic information on more than 700 sieges and battles in Central Europe, and with data about the local presence of military garrisons and construction. The data show—contrary to the widely held view that wars were "levellers" in history—the frequent military conflicts happening in preindustrial times constantly reinforced inequality. Moreover, I find the setting up of garrisons, central to communities' defence infrastructure, was associated with higher inequality, as was the construction of buildings with a military purpose, suggesting that costly wars induced authorities to inequality-promoting extraction. The positive conflict-inequality relationship found during ordinary wars was significantly different from what was found during the Thirty Years' War. This war was the exception and not the rule, because it was exceptionally destructive and lasted for 30 years.

The findings suggest we need to consider the indirect effects of wars, especially the many ordinary ones, because they generate a negative externality in the form of increasing economic inequality.

BEYOND WEBERIAN GROWTH: PROTESTANTISM'S IMPACT ON INEQUALITY AND POVERTY

Many of the wars happening in early-modern Europe were about whether Protestantism or Catholicism was the "right" religious confession. In Chapter 3, I study the effect of the Protestant Reformation, beginning in 1517, on inequality in the area (see Schaff 2025). The Reformation was a revolutionary ideological and institutional change and has motivated scholarly debate for a considerable time, especially regarding its effect on long-run economic growth (see Weber 1930; Becker and Woessmann 2009). Yet, its impact on inequality has only been studied marginally from a historical perspective.

I combine my disaggregated data on wealth distribution in towns and villages between 1400 and 1800 with information about whether a community adopted Protestantism. Somewhat surprisingly, I find no evidence of the Reformation affecting top wealth shares. This is contrary to what one would expect if the Reformation impacted inequality through "Weberian" factors, like economic growth, capital accumulation, or higher upper-tail human capital, as these tend to increase inequality from the top of the distribution. Similarly, I do not find evidence for changes to the wealth shares of middling classes. Still, the data reveal the Reformation substantially increased inequality, by making poor people poorer relative to the rest of the population. The bottom fifth of the population lost about 39 percent of its pre-Reformation wealth share in eventually Protestant communities. Interestingly, I find aggregate economic growth was not large enough to compensate Protestant poor strata for their substantial relative losses, making the poor worse off in both relative and absolute terms. So it is not the case that a much larger Protestant economic "pie" made up for the poor having a marginally smaller slice of it.

My analysis of "church ordinances" and monastery closures then suggests that poor people became poorer because Protestantism introduced restrictions on the provision of social welfare: local community outsiders (e.g., non-residents, strangers) and "undeserving" (e.g., able-bodied but non-working) poor people were excluded from support. Only community residents and "deserving" poor (i.e., strictly unable to work) were eligible to receive social welfare, an approach resulting directly from Martin Luther's idea that good Christians should work. This left behind the bottom of the poor in Protestant society, making some poor poorer, and increasing inequality. My study focuses on Central Europe, but discrimination between "deserving" and "undeserving" poor, and between natives or strangers in the provision of social welfare exists in many societies imprinted by Protestantism; poor houses in Victorian England or SNAP work requirements in America today are just two examples.

WHY CITIES ARE SO UNEQUAL: ECONOMIC VS. POLITICAL STRUCTURE

The final chapter (see Schaff 2024) investigates the impact of urban political structure on inequality. Cities have always been places of higher wealth concentration compared to rural areas (Alfani, Gierok, and Schaff 2022). From a Kuznetsian perspective, this urban inequality is simply the result of cities' more vibrant economies compared to the agricultural sector (van Zanden 1995). However, cities had not only different economic structures, they also had different political structures. Preindustrial European cities usually had oligarchic (or closed) governments where a small elite held almost absolute power, without being held accountable through elections. In Germany, around 1800, 80 percent of cities had no form of election for the city government. In a standard political economy logic the lack of accountability would induce political elites to engage in rent-seeking, increasing their personal wealth and driving up local inequality (Ogilvie 2007; Acemoglu 2008; Alfani 2021). There also exists a more romantic historical view of what pre-modern urban political elites were: benevolent oligarchs. These highly civic-minded individuals, it is often argued, would act for the common good instead of personal interest; holding a political office would not increase their personal wealth or lead to higher local inequality (see Friedrichs 2000). Which of these two opposing views can be supported quantitatively?

As a first analytical step, I study the broad association between urban political structure and aggregate wealth inequality in a panel of early modern Central-European cities. I find more closed political institutions were related to substantially higher inequality, controlling for economic development and other factors. Cities without council elections had a 5 percentage points higher top 1 percent wealth share.

To identify the mechanisms behind that macro-relationship, we need individual-level micro data, which are extremely difficult to obtain for the preindustrial period. Exceptionally orderly archival records allow me to construct an individual-level panel dataset, containing c.27,000 observations on personal wealth and political office-holding in the Central-European city-state of Nördlingen from 1585 to 1700. I show political elites increased their personal wealth substantially after entering office, by 44–55 percent. Individuals with the greatest political power in the city—mayors—increased their personal wealth even more, by another 30–34 percent. This increase in the personal wealth of political elites contributed to higher wealth inequality in the city-state. For example, government members were 7–10 percent more likely to be part of the top 5 percent of the wealth distribution. Historical evidence suggests the sources of government members' personal enrichment were a combination of substantially increasing salaries (over which government members decided themselves), patronage, and the embezzlement of public money. Interestingly, government members enriched themselves the most during the Thirty Years' War, which in Nördlingen coincided with a plague wave. This suggests moments of severe socio-economic crisis, such as warfare and epidemics, were not always good for the economy (see Voigtländer and Voth 2013). Instead, they could be a veil for political elites' personal enrichment at the expense of the wider population, contributing to higher inequality and likely inflicting deadweight losses on the city economy. The results are hard to square with the "civic-mindedness" narrative of urban political elites.

In sum, the findings of my thesis suggest it is very unlikely preindustrial inequality was the mechanical result of economic growth alone (see Kuznets 1955). Instead, a number of elite-made policies and institutions seem crucial to understanding the roots of inequality, namely the decision to wage war, tax- and welfare policies, governmental institutions, and the imposition of a new religious confession. The unequal institutional structure of preindustrial society is mirrored in its economic inequality.

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Three Essays on Legal Institutions and Growth in Medieval and Early Modern Europe

This dissertation explores Britain's unique legal institutions as an explanation for the later differences in economic development between it and the rest of Europe. Between 1066 and 1600, England diverged from other states in its political and legal framework. The English state was more centralized and developed a system of common law that differed substantially from the decentralized, feudal laws of its contemporaries. This

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centralized legal system fulfilled many of the roles played by local institutions elsewhere, augmenting the patchwork of manors, towns, and guilds that regulated trade and production throughout Europe.

Medieval legal change has several attractive features as an explanation for future economic divergence. First, prior research has found that laws and institutions matter in the real economy. Second, the timing of medieval legal change fits with evidence that wages (Humphries and Weisdorf 2019) and productivity (Bouscasse, Nakamura, and Steinsson 2021) began to rise in Britain in the early 1600s, more than a century prior to the Industrial Revolution. Despite this, legal institutions have been comparatively understudied in the context of British development, because of limited availability of data and the challenges conducting causal inference. The aim of my dissertation is to study the origins and effects of legal change in medieval and early modern Britain, emphasizing the use of new data and leveraging quasi-experimental variation to identify causal relationships between legal and economic change. I focus on the process of state building, particularly with respect to legal institutions, to understand how the early divergence in the structure of the law between England and the rest of Europe contributed to the later divergence in economic growth. The dissertation is organized into three essays, which build on this common theme but are otherwise separate with respect to subject and methodology.

The first chapter focuses on the effects of common law along the medieval English-Welsh border. This region, known as the March of Wales, provides a unique opportunity to observe how access to common law courts changed the structure of the British economy. The March of Wales was formed in the late eleventh century on the border between Norman England and then-independent Wales. The purpose of the March was to create a military buffer zone, in which the local nobility was charged with defending the border in exchange for privileges. These privileges included judicial independence, meaning subjects in the March could not appeal a judgment of their local lord to the King. In the eleventh century, this was a heightened version of lordship as it appeared outside of the March, but the differences were limited since elsewhere appealing to the king was prohibitively difficult. However, as common law developed in the twelfth century, access to royal justice became cheaper outside of the March, while within the March common law courts had no jurisdiction. This difference in legal institutions persisted after Wales was incorporated into the English state, and lasted until the laws were homogenized in the 1530s.

The March is a reasonable comparison with neighboring regions of England for two reasons. First, the difference in legal systems is precisely access to the common law system that distinguished medieval England from its peers. Second, other institutions, cultural elements, and economic variables are similar across the border, limiting the risk of confounding that characterizes studies of modern common law. A contribution of this chapter is the use of newly digitized data drawn from the Inquisitions Post Mortem, a set of property surveys taken on both sides of the border. To locate estates, and provide controls for pre-common law characteristics, I link estates in the Inquisitions Post Mortem to the Domesday Book, a 1086 tax assessment. Linking between these datasets allows me to confirm that estates within and outside common law jurisdiction are balanced in observable characteristics a century before the development of common law, and to control for these characteristics and geography in the empirical analysis.

I find that common law caused a shift in land contracts away from costly feudal forms of ownership to more modern contracts, and that small landowners invested more under common law, reflecting an increased availability of credit. In the long run, common law areas were more likely to have markets and middle-class farmers. The results support the narrative that England's distinctive legal institutions made medieval factor markets more efficient, which allowed for the development of a larger middle class engaged in commercial agriculture.

The second chapter focuses on a different medieval institution—the craft guild. Craft guilds were associations of artisans that organized their members for a mixture of social, political, and economic purposes throughout medieval and early modern European cities. Guilds are important to study in the context of pre-industrial legal institutions because they are creatures of the law. Recent scholarship by Ogilvie (2019) has documented the reliance of guilds on public support to enforce their regulations. From an economic perspective, guilds are associated with some benefits, mitigating problems of trust (Greif 1993) and information asymmetry (Epstein 1998; de la Croix, Doepke, and Mokyr 2018), but come at the cost of exercising market power for their own gain. Previous work suggests that politically weak guilds tend to be more beneficial than guilds that wield significant political power. However, it is an open question why some areas of Europe developed stronger guilds than others.

This question touches on the larger theme of English exceptionalism, because guilds in England were particularly weak. The literature on the Industrial Enlightenment has noted that the flexible apprenticeship system in Britain allowed apprentices to flow into growing industries, whereas a stronger guild system might have barred these new entrants. Theoretical work suggests that state institutions like courts might be substitutes for guilds in resolving market failures, which would suggest a role for the unusually centralized state and legal system in Britain in determining guild power. To explore the role of state institutions in determining guild power, I turn to the state capacity literature, which has noted that the ability of the state to enact policy has often developed in response to conflict (Besley and Persson 2011; Dincecco 2015). I developed a model linking conflict to guild power that emphasizes the partnership between local governments, which used guilds to raise resources, and the guilds themselves, which relied on government enforcement for their status. The prediction of the model is that guilds represent an alternative to stronger state institutions for raising revenue, and that in weak states conflict leads to stronger guilds.

To test this prediction, I construct a dataset of textile guild characteristics from a mix of primary and secondary sources. I find that conflict exposure strengthened guilds, while exposure to trade weakened guilds. With respect to the central questions of this dissertation, the results suggest that England was less exposed to external threats and had less reason to rely on short-term revenue raised from guilds, instead building up state capacity consistently over the Middle Ages.

The final chapter returns to a focus on English common law, examining the correlates of ideological change within the judiciary between 1500 and 1750. Motivating this question is the fact that in the early 1600s, the lawyers and judges of the common law courts tended to side with Parliament in the series of conflicts with the King that culminated in the Civil War (1642–49). The opposition to royal absolutism within the judiciary is striking because there were no formal guarantees of independence for judges, who served at the pleasure of the King. It is a puzzle in the legal history of Great Britain, and one with important implications for understanding British institutional development in the critical centuries leading to the Industrial Revolution.

To narrow the scope of the investigation to something feasible, I focus on treason cases. Treason cases create a situation where judges must rule in favor either of the King in advancing a case against an accused traitor, or against him in limiting or obstructing a case, making it possible to proxy for the extent of support for the monarch. I measure the outcomes of treason cases using the English Reports, a digitized collection of common law cases covering the time period that has been used in recent economic history research (Grajzl and Murrell 2021a, 2021b). To explain shifts in the degree of support for the crown, I use information on judges drawn from the Oxford Dictionary of National Biography. I link treason cases from the English reports to the judges that decided those cases and examine the background of those judges for correlates of opposition to the monarchy. I find that class background matters, with judges drawn from noble families being significantly more likely to support the King. In addition, it appears that professional norms and social dynamics within the judiciary have an effect. Support for the monarch is clustered within each Inn of Court, the institutions in London that trained lawyers and served as professional and social clubs for established attorneys and judges, suggesting an important role for peer influences and norms arising from social interactions.

Colin Sharpe, *Cornerstone Research*

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