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Summaries of Doctoral Dissertations

The Dissertations of Allison Green, Elijah Locke, and Pablo Andrés Valenzuela-Casasempere: 2025 Allan Nevins Prize Competition of the Economic History Association

I had great fun last summer reading economic history dissertations. Less fun was having to choose the three finalists for the Nevins prize from among the 16 excellent dissertations submitted. Thankfully, I did not have to make these decisions alone. All decisions were made jointly with John Parman, who ran this session last year and generously shared his accumulated knowledge with me.

We liked many things about these finalists' dissertations. All use remarkable data, cutting-edge empirical methods, and present results clearly. But what we found most attractive about these three dissertations is their interest in big questions and the seriousness with which they approach history. All three authors pay careful attention to historical context in ways that make their work more convincing.

Perhaps indicative of the future of the field, all the dissertations were, in some fashion, "big data" projects, by which I mean they used datasets with tens of thousands or millions of individual-level observations, along with methods of data collection or empirics that would have been impossible 20 or 30 years ago. Such individual-level data was not a requirement. I have learned much in my own work from running 48-observation cross-state regressions, and there will always be questions in economic history for which more aggregate data are most informative. Still, the Nevins Prize finalists show how much can be learned from large individual-level datasets, especially when datasets are linked across time, as these dissertations all do.

ALLISON GREEN (PRINCETON UNIVERSITY)

Allison Green's starting point for the first chapter of her dissertation is that 25 or 30-year-old white men were more likely to leave their birth state around 1950 than at any other time in the twentieth century. An obvious hypothesis is that serving in the military might have had something to do with the willingness of young men to migrate after WWII. And Green comes up with a clever way to test this. Conditional on demographic variables such as state of enlistment, assignment to ships in the Navy was random. As a result, young men were (conditionally) randomly assigned a social network. Green can then look at the causal effect of one's social network on postwar migration.

That may sound straightforward. And like so much of good economic history, the question and basic method are straightforward. But the implementation is not. The data collection requirements were large, including scanning 6.5 million Navy personnel records. To measure migration in the short run and long run, Green then links these personnel records to the 1950 Census and to death records.

Green's first chapter could have been two since it has two key results. First, social networks have a large effect on migration. Those serving on a ship with few people from

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their home state were more likely to leave their home state after the war. The resulting migration was directed toward the home states of their shipmates, especially if their shipmates came from high-opportunity areas such as California. Exposure to new social networks during military service likely explains roughly 14 percent of California's population increase between 1945 and 1950.

The second result is that migration had a large, positive effect on wages. Migration induced by exogenous variation in Navy shipmates is associated with roughly 60 percent higher income.

I tend to think that the most exciting part of the chapter is the history. But it also addresses the general question of how social networks influence migration. That said, I am somewhat skeptical of the external validity of the results. WWII was a unique context, not least because people serving on a ship had already left home.

Green's dissertation is an economic history dissertation plus another paper. The other paper, her second chapter, is not history. Green, along with her co-authors Victor Couture, Jonathan Dingel, and Jessie Handbury, uses smartphone data to document people's preferences for eating at restaurants with those of the same income or race. Thus, while Green's first chapter looks at the effect of randomness in the formation of social networks, the second chapter looks at one way in which people choose their own social networks. The chapter does have implications for economic history; that people have a strong preference for restaurants and other commercial establishments frequented by higher-income patrons and people of the same race or ethnicity has obvious implications for thinking about historical migration patterns and residential segregation.

In her third chapter (co-authored with Kaan Cankat), Green returns to economic history to look at the effect of municipal annexations in the Sunbelt. States in the Northeast and Midwest generally forbade cities from annexing other municipalities unless the other municipality wanted to be annexed. Thus, many cities in the Northeast and Midwest have geographic boundaries identical to those in 1940. In fact, the city of Philadelphia has the same boundaries today that it did in 1854. In the Sunbelt, by contrast, cities were often able to annex surrounding areas without the support of the newly annexed population. The result was the rapid geographic expansion of Sunbelt cities after WWII and an urban political geography different from that in the North.

Green uses a synthetic control approach to study the impact of annexations on municipal finances. Here, she runs into some limits of a small sample and the resulting wide confidence intervals. Still, the results are intriguing, suggesting that annexation led to some economies of scale in service provision.

ELIJAH LOCKE (BOSTON UNIVERSITY)

Locke's first chapter investigates the causes of ethnic occupational niches during the age of mass migration. Why, in 1900, for instance, were Jewish immigrants overrepresented among tailors and Italian immigrants among stonemasons?

To answer this question, Locke has to solve several conceptual and data problems. One such problem is how to quantitatively define an ethnic-occupational niche. He does so with a definition from sociology, in which an ethnic-occupational niche is defined as an occupation in which an immigrant group is overrepresented by 50 percent or more in the occupation relative to the immigrant group's share of overall employment.

Locke's first contribution is to document the existence and quantitative importance of ethnic-occupational niches such as Jewish tailors and Italian stonemasons.

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Ethnic-occupational niches are not small-scale historical curiosities; they characterized the labor market experience for close to half of all immigrants. Locke investigates two possible causes of such niches: pre-existing skills and ethnic-occupational networks. To study the effect of pre-existing skills, Locke digitized new data from the *Reports of the Commissioner of Immigration*. He finds that both skills and social networks mattered, but that social networks mattered more. Here, social networks are proxied by the share of immigrants in a county working in an occupation. In other words, Locke finds that the reason Italian immigrants were disproportionately represented among stonemasons is that, upon arrival, they found many prior Italian immigrants were stonemasons.

There is a circularity here, with new immigrants becoming stonemasons because previous ones did. That's not to say it isn't true; the argument is intuitive, and Locke's empirical work is convincing. But it does raise a question for further work: if networks are so important, then what determines the beginning of ethnic-occupational niches? Is that determined by pre-existing skills, which then snowball over the years as social networks develop? Or is there an element of randomness, with it being perfectly possible that Germans rather than Italians would have become stonemasons if some (small) number of German immigrants had selected into the occupation earlier in the nineteenth century?

Locke's second chapter is titled "The Gender of Names" and is co-authored with Sam Bazzi, Martin Fiszbein, and Katarina Fedorov. They start with the insight that names contain information; linguists have documented that some sounds are perceived as more masculine and some as more feminine. While Mark and Timothy are both boys' names, English speakers will often perceive Mark to be a more masculine name than Timothy. The authors' hypothesis is that the gender connotations of names may be correlated with broader societal indicators of gender equality. In a large dataset of names collected from the Social Security Administration and the Census, they find that this is indeed the case for boys' names. In years and places where boys' names are less masculine, indicators of gender progressivity are higher.

Locke's third chapter, co-authored with Sara Benetti and Ross Mattheis, considers how to measure who is Jewish in the Census with the goal of studying the effect of the arrival of Jews from the Russian Empire on the assimilation of Jews from an earlier wave of immigration from Germany. The Census did not ask about religion, so identifying who is Jewish is not straightforward. Previous work has used whether Yiddish was spoken as a proxy for whether a household was Jewish. But that has the obvious problem for Locke that it will miss Jews from Germany who spoke German. So he uses names from graves that are identified as Jewish. Locke's research will allow us to examine the effect of Russian Jews' arrival on the assimilation of German Jews.

PABLO ANDRÉS VALENZUELA-CASASEMPERE
(UNIVERSITY OF BRITISH COLUMBIA)

Often, economists and economic historians focus on the benefits of infrastructure, the ways roads and railroads have facilitated trade. But infrastructure also has costs, particularly in cities where it displaces people and produces noise and pollution. That is the focus of the first two chapters of Valenzuela-Casasempere's dissertation.

The first chapter is a detailed look at outcomes for people displaced by interstate highway construction. Understanding the mortality and wealth effect of displacement requires some cleverness and a lot of hard work. No existing dataset reports who was displaced when an interstate highway was built. Prior to Valenzuela-Casasempere's

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work, we had no quantitative estimates of the effect of highway construction on displaced residents. To obtain such estimates, Valenzuela-Casasempere geocodes the 1940 Census and looks at outcomes for people living where an interstate highway was built in the 1950s. He matches individuals from the 1940 Census to mortality records 60 years later.

The first contribution of this chapter is to document the number and characteristics of the people displaced by highway construction. Valenzuela-Casasempere finds that interstate highway construction displaced roughly 150,000 people from 1956–1960. Consistent with qualitative accounts, he finds that highways were more likely to be built through poorer neighborhoods with larger black population shares.

An obvious concern is that some people living in 1940, where a highway was built in the late 1950s, may not have actually been displaced by the highway; they might have moved before the highway was built. In what is an example of his careful empirical work, Valenzuela-Casasempere provides a quantitative bound on the resulting possible measurement error. Still, I wondered whether one could now do the analysis with the full-count 1950 Census, reducing this source of error.

In any case, Valenzuela-Casasempere finds that individuals displaced by interstate highway construction die younger and end up living in more impoverished neighborhoods. Perhaps surprisingly, he finds no effects on individuals living more than 100 meters from where a highway was built. I say surprisingly, since I would have expected negative mortality effects from pollution to extend farther from a highway. The result is an example of how valuable Valenzuela-Casasempere's empirical work is. One might think that the negative effects of highway construction were obvious, but exactly who is affected and how is nuanced.

The second chapter continues the investigation of the costs of interstate highway construction, focusing on neighborhoods rather than individuals. Valenzuela-Casasempere looks at what happens to an urban census tract when an interstate highway is built through it. He finds that the population decreases. Perhaps most interesting are the results about how the effects of highway construction vary with census tract characteristics. His results are consistent with a story in which highways encouraged white flight to the suburbs; when a highway was built through a neighborhood near a city center, the white population declined. But when a highway was built through a neighborhood farther from a city center, the white population grew. One might dig into this result further, with a finer-grained measure of distance to the city center scaled by metro area size.

I spent a month last summer in Japan, and I thought about Valenzuela-Casasempere's dissertation while there. I stayed in an apartment 100 yards from the Mukojima Line expressway. Yet, the highway seemed to have no ill effect on the neighborhood. The combination of its design and slow traffic speeds meant it produced little noise. Moreover, the highway was elevated such that pedestrians and traffic could easily move under it. So, I wonder whether Valenzuela-Casasempere's results reflect the effects of highways in general, or whether the results are specific to the way urban highways were built in the United States in the 1950s and 1960s. Of course, such questions about external validity are inevitable and are in no way a criticism of Valenzuela-Casasempere's work.

In his third chapter, Valenzuela-Casasempere turns to a different urban history question: How has the relationship between city size and the college wage premium changed over time, and how are these changes related to worker age? The college wage premium

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tends to be larger in larger cities. Valenzuela-Casasempere shows that this agglomeration effect has increased over time, particularly for younger workers. The results suggest that understanding the evolution of wage inequality between college and high school workers requires an understanding of the role of cities in driving demand for college-educated workers, particularly young college-educated workers.

JOSHUA K. HAUSMAN, *University of Michigan and NBER*

The Good Place: How Networks, Preferences, and Public Policy Determine the Value of Where We Live

My dissertation examines how networks, demographic preferences, and public policy shape geographic mobility and its economic consequences in the United States. The two chapters, which are historical in focus, show how large-scale transformations in the mid-twentieth century have shaped the geography of economic activity today.

The first chapter uses one of the largest natural experiments in network formation in U.S. history—military service during WWII—to show that expanding the geographic diversity of personal networks causally increases migration and improves economic outcomes. The second chapter, co-authored with Victor Couture, Jonathan Dingel, and Jessie Handbury, uses trips to chain restaurants as a laboratory for measuring preferences over the demographics of others in shared spaces. The third chapter, co-authored with Kaan Cankat, examines the expansion of municipal boundaries in Sun Belt cities in the decades after WWII, investigating how this expansion during mass suburbanization affected municipal finance and public good provision.

NETWORKS AND GEOGRAPHIC MOBILITY: EVIDENCE FROM WWII NAVY SHIPS

Geographic mobility in the United States is low and continues to fall, despite evidence of substantial positive returns to migration. Cross-state migration among young adults rose sharply in the first half of the twentieth century, peaking for those born in the 1920s before declining steadily since. One potential explanation for limited geographic mobility is the prohibitive cost of moving without an established network (Munshi 2020; Blumenstock, Chi, and Tan 2023). Many Americans today lack out-of-state connections, with geographically broad networks typically concentrated among higher-income and more educated individuals (Chetty et al. 2022). The striking correspondence between geographic mobility and military service rates in the twentieth century suggests that wartime service may have expanded networks and facilitated migration. This chapter uses Navy service during WWII as a large, natural experiment to assess whether expanding the geographic scope of networks can increase migration and expand economic opportunities.

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WWII exposed a substantial portion of young American men to individuals from outside their immediate communities, often for the first time. Nearly 80 percent of white men born between 1920 and 1926 served in the war in some capacity. I measure the impact of wartime networks on the migration decisions of rank-and-file sailors serving on Navy ships during WWII. This population was young, and mostly native-born, and without post-secondary education. In part due to low migration during the Great Depression, the vast majority were living close to where they were born prior to entering naval service. These characteristics make rank-and-file Navy sailors well-suited for studying the potential benefits of expanded geographic diversity in personal networks, as pre-existing research has shown migration gains are typically largest for young movers (Chetty, Hendren, and Katz 2016; Nakamura, Sigurdsson, and Steinsson 2022).

To estimate the impact of ship networks on migration, I use quasi-random assignment to Navy ships. According to contemporaneous institutional records, assignments to ships were largely random, though men were more likely to serve with others from similar geographic origins due to operational constraints. The empirical strategy addresses this endogeneity by comparing migration outcomes for individuals from the same state who were exposed to different sets of peers during their service. I construct a novel dataset by processing 6.5 million scans of Navy personnel documents, creating detailed information on over 1.7 million sailors across more than 6,000 ships. I then link individuals to migration outcomes five years after the war (1950 Census) and at the time of their death.

I find that shipmates significantly influence both whether a Navy sailor moves and where they move. By 1950, a one standard deviation increase in exposure to out-of-state shipmates raises the likelihood of moving out-of-state by 4.2 percent. The effect is larger for long-distance moves: a one standard deviation increase in exposure to out-of-region shipmates increases the likelihood of moving out-of-region by 6.5 percent. The impact on directed migration is even more pronounced: exposure to shipmates from fast-growing Pacific states increased migration to those areas by over 15 percent by 1950. Using a discrete choice migration model, I show that exposure to people from distant parts of the country—places where someone is unlikely to have met anyone from before—has the strongest impact on migration decisions. A back-of-the-envelope calculation suggests that over 14 percent of migration to California between 1945 and 1950 can be attributed to exposure through wartime networks.

In the final section, I examine whether moves facilitated by Navy networks led to greater economic opportunity. Using random variation in networks as an instrument for migration, I find economically large impacts of network-induced moves on lifetime earnings: individuals induced to move out-of-region by their Navy networks reside in zip codes with over 70 percent higher income by the time of their death.

DEMOGRAPHIC PREFERENCES AND INCOME EXPOSURE

Americans are segregated by race and income not only where they live, but also where they shop, dine, and socialize. Segregation in shared commercial spaces matters because contact with high-income individuals shapes people's opportunities and economic outcomes. Some venue segregation may simply reflect people visiting places near their homes, but it may also arise because people choose where to go based on the race or income of others they expect to encounter. In this chapter, we estimate preferences for being around co-patrons of certain races or income levels and assess how much these demographic preferences explain observed segregation.

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To measure exposure to high-income co-patrons, we use data on the movements of millions of smartphones in the United States during 2018 and 2019. Joining these movement data with building-level residential demographics, we measure the socioeconomic composition of each venue's patrons and characterize exposure across eight demographic groups comprising four racial/ethnic categories (Asian, Black, Hispanic, and White) interacted with two income categories (above and below median income). We document substantial income segregation in shared spaces and large differences in exposure to high-income co-patrons across both racial and income groups. For example, high-income Black individuals experience roughly the same high-income exposure as low-income White individuals.

We estimate demographic preferences by modeling individuals' travel choices among venues within the same business chain, allowing us to distinguish the role of demographic preferences from proximity to venues and preferences for product attributes. Our estimates reveal notable regularities: high- and low-income individuals exhibit similar levels of racial homophily, while only high-income individuals exhibit monotone preferences over high-income co-patron shares. Low-income individuals prefer establishments with an integrated mix of income levels. These preferences are economically large: individuals are willing to travel two to three additional kilometers to visit a venue in the 95th percentile of either the same-race or high-income distribution rather than one at the 5th percentile.

Using our estimated model, we show that residential segregation plays a major role in exposure differences, but demographic preferences alone would generate substantial exposure gaps even in the absence of any travel costs to venues. Black individuals experience lower high-income exposure because majority-Black venues tend to have lower-income co-patrons, creating a trade-off between visiting high-income venues and same-race venues. Finally, we investigate whether demographic preferences directly impact residential segregation by studying movers between metropolitan areas. The estimated preferences are consistent with people sorting into neighborhoods based on their preferences for co-patron demographics, with preferences strengthening after moves consistent with intergroup contact theory.

ANNEXATION AND THE MAKING OF THE SUN BELT: MUNICIPAL BOUNDARY EXPANSIONS AND LOCAL PUBLIC FINANCE

Between 1945 and 2000, the average Sun Belt city grew in land area by nearly a factor of six, while the average city elsewhere grew by only a factor of two. This growth was not merely relative catch-up—by the 1950s, the average Sun Belt city had surpassed the average non-Sun Belt city in area, and by 2000, all ten of America's largest cities by land area were in the Sun Belt. This divergence stemmed from state annexation laws that allowed cities in Sun Belt states to expand into unincorporated areas, while municipalities in Northern states had limited legal options to grow. City planners believed that the spatial growth of municipal boundaries was consequential for local governance: proponents argued annexations would lower costs, increase revenue, and prevent suburbanites from free-riding on central city public goods. As post-war suburbanization threatened the financial stability of many cities, the ability of Sun Belt cities to expand and retain their tax bases may have allowed them to remain attractive environments in terms of municipal services and taxes.

In this chapter, we examine large annexation shocks to investigate how changes in municipality size affect local public finance and whether differences in city growth

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contributed to the rise of the Sun Belt. To estimate the effects of boundary expansions, we digitize annual annexation data published in the Municipal Yearbooks between 1948 and 1968 and employ a staggered difference-in-differences design around large annexation events. The data come from annual surveys on the square mileage and population of annexed areas provided directly by municipal governments. We supplement the annexation data with information from the Annual Survey of Governments and construct an annual panel of city boundary changes, as well as municipal revenues, expenditures, and employment.

We find that the average large annexation in our sample increases a city's population by 79,000 people—a 42 percent increase. Our findings suggest that while these large population increases raise total municipal budgets, they allow costs to be dispersed across the larger population base. Our estimates indicate that per capita revenues and expenditures both decline by 23 percent, with effects that persist for ten years after the annexation. These reductions in per capita expenditures may reflect economies of scale, where similar or better levels of public goods are provided more efficiently. Consistent with this interpretation, we find no evidence of declines in public service provision.

Annexation also shifts what cities spend on, with much larger per capita declines in current expenditures (27 percent) compared to capital outlays (6 percent), suggesting a focus on fixed cost investments. Similarly, we observe 26 percent and 23 percent per capita declines in fire and policing expenditures, while highway spending shows no decline and sanitation spending increases slightly. These patterns suggest that annexation prompts cities to reallocate resources toward infrastructure investments that may be more valuable to suburbanizing populations. Finally, results for total debt issued—a 24 percent increase—suggest that city planners may have used these expanded tax bases to increase access to credit. In contrast to recent empirical evidence on inter-municipal cooperation, which finds mixed returns to scale, we find that increases in municipal size generate efficiency gains and are associated with broader economic spillovers: employment is approximately 3 percent higher ten years after annexation in counties with large expansions.

Allison Green, *Princeton University*

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Labor Markets, Social Networks, and Economic History

My dissertation contains three projects covering how the social and cultural aspects of certain groups shape their experiences in labor markets in U.S. history. The first chapter concerns "ethnic-occupational niches": particular ethnic or immigrant groups being vastly over-represented in specific occupations. I show that niches were highly prominent between 1850 and 1940 and were primarily driven by social networks. The second chapter (joint with Sam Bazzi, Martin Fiszbein, and Katarina Fedorov) uses a novel name-based measure to analyze changing gender norms in the U.S. economy over the last two centuries. Finally, in the third chapter (joint with Sara Benetti and Ross Mattheis), we study whether frictions between successive waves of co-ethnic migrants shape the assimilation choices of their previously arrived countrymen.

ETHNIC-OCCUPATIONAL NICHES: EVIDENCE FROM THE AGE OF MASS MIGRATION

In 2023, individuals of Mexican ethnicity made up over 37 percent of all workers in construction in the United States, yet accounted for just 11 percent of U.S. employment (Bureau of Labor Statistics 2023). Nearly half of all motels in the United States are owned by individuals of Indian descent, despite them being only 1 percent of the U.S. population (Dhingra 2020). These are examples of an *ethnic-occupational niche*: a given ethnic, immigrant, or minority group that is (statistically) overrepresented within a certain occupation, relative to that group's presence in the overall population (Wilson 2003).

Ethnic-occupational niches are present across the globe and are undoubtedly relevant to a contentious political issue today: the impact of immigration on the economy. Nowhere are niches more salient right now than in the United States; niches appear in important industries like agriculture, construction, healthcare, information technology, and throughout the service sector, and are found among a diverse range of minority and immigrant groups (Eckstein and Peri 2018). Yet, despite their importance, the economic mechanisms that underlie these niches are not well understood.

This paper seeks to provide an empirically driven explanation of what gives rise to ethnic-occupational niches. I propose that niches occur due to two forces: the pre-migration skill background brought by immigrants from a particular country of origin to their destination, and the social networks of fellow countrymen in the niche who help draw newly arrived immigrants into that occupation. Knowing the precise channels that lead groups to form niches can help us better understand their consequences for the welfare of minorities and immigrants and ultimately shape policy.

The United States during the Age of Mass Migration (1850–1924) is a natural context to explore the drivers of niches, given both the rich data as well as the large and varied immigration flows during this period (Abramitzky and Boustan 2022). Niches

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are identified as cases in which an immigrant group is over-represented by more than 50 percent in a given occupation, relative to that group's overall employment share (Waldinger and Bozorgmehr 1996). Applying this to the full-count censuses between 1850 and 1940 (Ruggles et al. 2024), nearly half of immigrants—46 percent, or almost 18 million—were in a niche, with large variation across time and nationality. Immigrants were most severely overrepresented in semi-skilled, urban occupations. Altogether, these facts provide novel empirical evidence that niches were a pervasive feature of the U.S. economy during this time.

New data on immigrant occupations prior to arrival in the United States are digitized from tables in the historical Reports of the Commissioner of Immigration (RCI). Before boarding ships to come to the United States, immigrants were asked about the current occupations they held in their home countries. These data exist at the nationality-occupation-year level between 1875 and 1932. Merging these data on immigrants' prior occupations with the Census (Lafortune and Tessada 2016) provides direct information on immigrant group-level skill background.

Immigrant ethnic-occupational networks in the United States are derived from Census data by computing the share of one's incumbent co-ethnics in a given occupation in a county-year. In an ordinary least squares (OLS) regression with granular fixed effects, both skill background and ethnic-occupational networks generate positive, statistically significant, and economically meaningful increases in the likelihood that a newcomer immigrant chooses to enter the niche. While both channels matter, in a horse race exercise, ethnic-occupational networks are shown to be more salient. In particular, a 1 percentage point increase in one's ethnic-occupational network leads to a 6.4 percent increase in the probability that a newcomer chooses this occupation.

Finally, a novel IV for ethnic-occupational networks is constructed to address any remaining endogeneity concerns. More specifically, the method from Burchardi, Chaney, and Hassan (2019)—which, using broad leave-outs, generates a plausibly exogenous estimate of the number of immigrants from a certain origin in a given U.S. county—is combined with the national-level data on occupation prior to arrival. The IV coefficient on ethnic-occupational networks is only slightly smaller than that of OLS, demonstrating a small upward bias in the baseline estimates. Taken together, this suggests that OLS is only slightly upward-biased relative to IV. A thorough discussion is provided of the assumptions necessary for exclusion, supported by robustness checks and substantial historical evidence.

The paper closes by discussing how the insights derived in this project can help us understand contemporary ethnic-occupational niches in new light. Almost all studies of modern ethnic-occupational niches in the United States find that, qualitatively, social networks play an instrumental role in niche development (Eckstein and Peri 2018). How has their role been so persistent over the past 150 years? Moreover, are these networks and the niches they fuel an engine of intergenerational mobility, or do they lock in generations of minority groups into the same occupations across time, stymieing upward movement instead? Altogether, this research motivates important questions regarding the broader consequences of ethnic-occupational niches for the global economy today.

THE GENDER OF NAMES

Gender norms shape critical facets of the economy: labor force participation, access to education, representation in political and domestic spheres, marriage markets, and

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much more (Goldin 1995; Fernández, Fogli, and Olivetti 2004; Fernández and Fogli 2009; Blau, Kahn, and Papps 2011; Bertrand 2020; Kleven 2022). It is essential to understand how gender norms have changed over time and altered these parts (and others) of the economy in order to determine how and why increased opportunities for women have arisen and highlight where barriers persist today.

However, gender norms are challenging to measure. This is especially true when trying to track changes over time. In the United States, for example, survey data covering views on women's roles in the household and workplace are virtually nonexistent before 1950. Even when available, there are often other concerns, such as small sample size, lack of information like geographic identifiers, or inconsistent wording of survey questions over time. Motivated by these issues, we propose measuring gender norms using first names.

A literature in linguistics and psychology finds that, on average, names commonly given to girls tend to have more syllables, are less likely to have the primary accent on the first syllable, and more often end in soft or pleasing sounds, such as vowels. These phonetic characteristics create a gentler auditory quality, which, according to the theory of sound symbolism, aligns with traditional conceptions of femininity, where softness and pleasantness are closely associated with female names.

To encode such attributes, we turn to the methodology of Barry and Harper (1995). Their *BH score* generates a discrete measure capturing how masculine- or feminine-sounding a name is. Armed with this metric, we turn to modern computational tools to, for the first time, calculate BH scores at scale. Combining the Social Security Administration names data and the full-count historical censuses (Ruggles et al. 2024) covers the first names and sex at birth of nearly the universe of births from every U.S. birth cohort between 1790 and 2022. We demonstrate that names commonly given to girls are far more likely to have high BH scores; the opposite is true for boys, and BH scores vary greatly within sex.

From 1790 to 1930, names for both sexes became less feminine-sounding. Since then, the names of both boys and girls have become substantially more feminine-sounding; however, the difference in the average score between genders has remained largely stable over the past 200 years. Using a separate, popularity-based measure, we also find that the androgyny of names—the extent to which both boys and girls are given a particular name—has been increasing over the last century.

We then leverage survey evidence to evaluate the validity of these name-based metrics as a proxy for gender norms. In the second half of the twentieth century, in states where BH scores for boys were higher, the public held more favorable attitudes toward women. In 1940, higher BH scores are associated with a greater likelihood of women's labor force participation, a commonly used contemporaneous indicator of gender progressivity. Overall, our findings suggest that names are valuable indicators of gender attitudes. Male BH scores consistently correlate with progressive gender attitudes.

OUR CROWD: INTRA-ETHNIC FRICTIONS IN IMMIGRANT ASSIMILATION

Understanding the assimilation of immigrant groups at their destination is key to evaluating the effects of migration on economic outcomes, which can inform immigration policy. Most scholarship on assimilation implicitly assumes that each group is a

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homogeneous monolith that carries specific traits from its origin; within-group heterogeneity is ignored (Kazal 1995; Massey 1981). In reality, immigrants from the same group usually come in different waves, and cultural and economic characteristics vary with each wave. Despite evidence pointing to the salience of within-group tensions in shaping assimilation, this force has received little focus (Fouka, Mazumder, and Tabellini 2022).

Are incumbent migrants more or less likely to assimilate following new waves of co-ethnic immigration? We answer this by focusing on historical Jewish migration to the United States, which occurred in two relatively distinct waves. A first wave from Western Europe in the mid-1800s was followed by a much more religiously and culturally conservative wave from the Russian Empire near the turn of the century. These two waves both belonged to the same ethno-religious group, but tensions between them were pervasive: they differed in language, dress, politics, and more (Cohen and Fein 1985; Waxman 1983). These strong intra-group dynamics make this an ideal context for our question.

Because the census has never directly asked about religion, we develop a new method to classify Jewish individuals by using Bayes' rule to identify distinctively Jewish surnames. To do this, we generate novel data by web-scraping grave records from Jewish genealogical sites and combine them with census data on Yiddish speakers (Abramitzky, Boustan, and Connor 2024). Altogether, we identify more than two million likely Jews in the United States between 1900–1920 and are able to bound the resulting rate of false positives. Finally, we turn to the American Jewish Year Books to digitize, for the first time, state- and city-level historical estimates of the Jewish population to validate our measure.

With a sample of Jewish individuals as identified by distinctive names in hand, we analyze the effects of the arrival of the second wave on the assimilation practices of the first wave. Quasi-random allocation of Jewish immigrants from the second wave across the United States is leveraged to identify how the incumbent immigrants reacted to the newcomers arriving (Aaronson, Davis, and Schulze 2020; Abramitzky, Boustan, and Connor 2024). In addition to assimilation outcomes from the Census, we digitize and construct lists of lists of historical Jewish institutions, which can be geolocated. Preliminary results suggest that in locations that received individuals from the second wave, the first-wave incumbents moved out and did not build more congregations or charities.

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Essays in Displacement, Infrastructure, and Spatial Inequality

My dissertation examines two features of twentieth-century U.S. economic history: the construction of the Interstate Highway System (IHS) and the rise of spatial wage inequality. The first two chapters study how a large infrastructure program reshaped the lives of individuals and the evolution of neighborhoods. The third chapter analyzes why wage gaps between college and high school workers grew more in larger cities than in smaller cities and how those gaps vary across the work lifecycle.

1. DISPLACEMENT AND INFRASTRUCTURE PROVISION: EVIDENCE FROM THE INTERSTATE HIGHWAY SYSTEM

Infrastructure provision is a key driver of economic growth and development, but construction often requires demolitions that displace residents. Tens of millions are displaced each decade by major projects globally (Cernea and Maldonado 2018), yet systematic evidence on the long-run consequences for the displaced is scarce. This chapter studies the individual-level displacement effects caused by urban interstate construction and estimates the impacts on mortality, residential mobility, and wealth accumulation (Valenzuela-Casasempere 2024a).

This study addresses two key challenges to measuring the long-run effects of displacement. The first challenge is identifying who was displaced, since historical relocation records are incomplete or missing. I construct an individual-level displacement measure by geocoding address information for all urban residents in the full-count 1940 U.S. census and overlaying those addresses with the footprint of highways built afterward. This approach identifies which 1940 homes were subsequently razed for interstate construction and, therefore, which residents were forced to move. Then, I link these individuals to 1995–2005 administrative mortality records to observe long-run outcomes (Goldstein et al. 2023). These links allow me to track survival, location at death, and the characteristics of the residences and neighborhoods in which individuals ultimately lived.

The second challenge is the estimation of credible causal effects, as highways were not placed randomly. I implement three complementary strategies. First, I compare displaced residents to their unaffected near-neighbors who lived 100–200 meters from

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the newly built highway and therefore shared the same pre-construction neighborhood but did not lose their homes. Second, I match displaced individuals to controls living more than 2,000 meters from any highway on a rich set of individual, household, and neighborhood characteristics. Third, I digitize federal plan maps to construct a counterfactual "planned but not built" control: individuals who would have been displaced if the plan had been constructed. Across specifications, I also treat residents living within 100 meters of a highway as a separate treatment group since I show that construction impacted residents beyond those directly displaced.

The main results show that highway construction negatively affects displaced individuals and those living adjacent to newly constructed highways. Relative to their unaffected neighbors, displaced individuals die approximately three months earlier and are significantly less likely to live to the age of 70, conditional on reaching 65. In contrast, the effect on mortality for individuals living within 100 meters of the highway is smaller and only marginally significant. Additionally, nearly all displaced individuals had moved to different neighborhoods by the time of their deaths, and those living within 100 meters were also more likely to relocate. However, despite these moves, displaced and nearby individuals generally remained within the same city at the time of their deaths.

The second part of this chapter examines whether and how displacement affects economic outcomes. Using last known addresses, I link individuals to a neighborhood quality index and to municipal tax assessment records. Displaced residents, and to a lesser extent those living very near the highway, end up in neighborhoods with lower measured quality and in homes with lower assessed values per square meter. Together, the mortality and wealth results indicate that the costs of interstate construction were not only borne at the time of demolition but persisted for decades.

In summary, these findings highlight how displacement caused by infrastructure provision can negatively affect those displaced and the residents of the affected communities.

2. NEIGHBORHOOD EVOLUTION AND INFRASTRUCTURE PROVISION

The second chapter shifts from individuals to neighborhoods to study how highways reshaped urban structure over the short, medium, and long run (Valenzuela-Casasempere 2024c). I construct a panel of neighborhood characteristics with consistent 2010 census tract boundaries from 1930 to 2020 for 59 metropolitan areas with detailed historical spatial data. The selection of metropolitan areas is based on the availability of spatial information from 1950 (Manson et al. 2023). I create neighborhood-level data for 1930 and 1940 by geocoding address-level information from historical census files and aggregating it to match census tract boundaries. For later decades, I harmonize spatially to the 2010 census tract boundary definition.

To estimate causal effects, I use a matched difference-in-differences design that pairs neighborhoods where highways were built with observationally similar neighborhoods outside the metropolitan area that never received a highway. Event-study estimates show no differential pre-trends across a range of outcomes, supporting the credibility of the research design. I then study the dynamic impacts of highway openings on population, racial composition, homeownership, and rents.

The results show that highway construction significantly alters the demographic composition of neighborhoods. Two decades after construction, treated neighborhoods experience a 10.4 percent decline in population relative to their matched controls. The

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decline is driven by an 18.3 percent reduction in the Black population, while effects on the white population are negligible. Additionally, highway construction impacts economic activity in affected communities, as homeownership rates decrease by 5.7 percent in the long run, while rents remain unaffected.

Highway construction occurred during peak levels of urban segregation, shaped by white suburbanization (Boustan 2010) and institutional barriers limiting Black residential choice (Cutler, Glaeser, and Vigdor 1999). I test how these forces mediated highways' neighborhood impacts by comparing areas above versus below the median pre-construction Black share and by examining outcomes across distance-to-Central Business District (CBD) quartiles. In high-Black share neighborhoods, total population losses are driven by white outmigration, with no detectable decline in the Black population and a resulting increase in the Black share. In low-Black share neighborhoods, the Black population falls while the white population rises, yielding little net change in total population. Homeownership declines only in high-Black share areas, while rents remain flat in both cases. These patterns align with accounts that highway construction helped create "second ghettos."

By distance to the CBD, neighborhoods near the center lose residents while outer neighborhoods gain, indicating strong suburbanization effects (Baum-Snow 2007). Black and white populations decline near the CBD and increase in the suburbs, and homeownership falls downtown while rising at the periphery. Overall, the Interstate Highway System accelerated suburbanization across racial lines while reinforcing central-city segregation dynamics.

3. THE EVOLUTION OF THE SPATIAL WAGE INEQUALITY ACROSS THE WORK-CYCLE IN THE U.S.

In the last 40 years, the U.S. labor market has been characterized by a significant increase in the average wage gap between high school and college graduates. This gap is not population-neutral. College and high school workers face larger differences between their hourly wages in more populous cities. This relationship is referred to as the college bias of agglomeration externalities and has existed during the last 40 years. Previous work studying the college bias of agglomeration externalities, however, falls short of providing a satisfactory analysis of the effect agglomeration externalities have on the work-cycle. The literature has assumed that workers of different ages are perfect substitutes and, consequently, are impacted homogeneously by agglomeration externalities. Nonetheless, this assumption doesn't hold in the data.

In this chapter, I relax the assumption of perfect substitution across workers of different ages (Valenzuela-Casasempere 2024b). I show that the shifting structure of the returns to college education in larger U.S. cities is a reflection of an increase in the age-group specific college bias of agglomeration externalities, which attracts young, college-educated workers to larger cities. As a result, the relative supply of college-educated workers in more populous cities has increased with each passing decade. I find that a larger age-specific relative supply of college workers is associated with a decrease in the college wage gap. However, this decrease is smaller in magnitude than the increase caused by the rise in the college bias of agglomeration externalities. Thus, explaining why the college wage gap has increased in more populous cities.

I propose a theoretical framework to quantify the factors that impact the college wage across cities of different populations. In particular, I employ a model of city aggregate

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production with age-group-specific labor supplies that incorporates imperfect substitution between workers of different ages and educational attainment. The model allows for a heterogeneous effect of agglomeration externalities across the work-cycle and between different educational levels. The model predicts that the college wage gap in a given city will increase: (i) if the age-specific relative supply of college workers decreases, (ii) if the overall relative supply decreases, (iii) if there is a technological shock that benefits college over high school workers, or (iv) if agglomeration externalities are skill-biased.

Next, I estimate the predictions of the theoretical model using five decades of U.S. census data. The results indicate that changes in the college bias of agglomeration externalities have been central to the rise of wage inequality across local labor markets. I find that for all workers, the college bias of agglomeration externalities has increased over time. However, this effect has been particularly large for workers under 40 years old. The decadal increase in the college wage gap for young workers located in the city with the largest population is 3.7 percent and 7.1 percent larger than the increase in the median city in 1990 and 2019, respectively. For old workers, the same effects are 2.4 percent and 3.4 percent, respectively. The heterogeneous evolution in the college bias of agglomeration externalities is not caused by sorting on unobserved characteristics, by outliers in the wage distribution, or by compositional effects.

The empirical strategy used allows me to recover the elasticity of substitution between workers of different ages and between workers with different educational attainments. I find an elasticity of substitution between age groups of 4.36, which supports the assumption of imperfect substitution between workers of different ages. Although my goal is to measure the college bias of agglomeration externalities, my estimates of the elasticity of substitution between age groups are comparable in magnitude to those in Card and Lemieux (2001). I also estimate an elasticity of substitution between workers with different educational attainment of 3.6, in line with previous estimates (Baum-Snow, Freedman, and Pavan 2018).

The shift in the occupational structure across the work-cycle and cities explains the increase in the college bias of agglomeration externalities. Using Autor and Dorn's (2013) routinary task index, I find that older workers have moved away from highly routinary jobs regardless of their educational attainment or the city they live in, while the same is not true for younger workers. The share of young workers with a high school diploma performing highly routinary jobs in 2020 has the same level and slope with respect to city size that it had in 1980. Conversely, with each passing decade, young workers with a college degree have reduced their share and slope with respect to city population in highly routinary jobs while simultaneously increasing their share in high-paying low routinary jobs. Thus, the heterogeneous change in the occupational structure explains the increase in the college bias of agglomeration externalities.

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The Dissertations of Hillary Vipond, Qiyi Zhao, and Diego Castañeda Garza: 2025 Alexander Gerschenkron Prize Competition

It was an honor and absolute pleasure to convene the Gerschenkron Prize session this year, and I would like to congratulate the three finalists for rising to the top of what was an excellent field. We received 20 submissions that showcased the rich variety of economic history research, ranging across regions, historical periods, and methodological approaches. Reading and assessing them has truly been one of the more inspiring tasks of my career and has given me great confidence in the future of our field.

What made these three stand out was the combination of ambition and creativity in the design and implementation of the research. Each dissertation addresses big and important problems that we still face today—the impact of wars, technological changes, and religious upheavals. Naturally, these are all questions that have received a lot of attention before. But these three dissertations tackle them in ways that are innovative across a variety of dimensions, whether by unearthing new evidence in contexts where people said it didn't exist or by revisiting old sources using new methods.

HILLARY VIPOND (LONDON SCHOOL OF ECONOMICS)

Hillary Vipond's thesis asks a question that has recently been on the minds of computer science majors who thought they would be earning large incomes through coding jobs right now: What happens when a machine can do your job better or cheaper than you? Technological advances can benefit us all collectively, but they often have uneven impacts on people who have invested in specific skills. The impact of these changes on the lives and livelihoods of those people contributes to the social upheaval that often accompanies major technological advances.

This impact is not always straightforward to measure. Technology can create new jobs while also destroying old ones. The same technology might also have different impacts depending on the type of job. Transitions to new technology may also occur at different speeds, allowing incumbents more or less time to react.

Standard measures of occupational structure fail to capture such nuances. This leads me to the main contribution of Hillary's work, what she calls the "tasks based approach." She focuses on the boot making industry in England from 1851 to 1911. This was an important economic sector, employing nearly a quarter of a million people. At the beginning of the period, most of these were employed as artisanal boot makers. Over the course of the period, some 150,000 of these artisan jobs disappeared. At the same time, 144,000 new jobs in mechanized boot making were created. As she shows, the geography of the industry shifted from being fairly broadly distributed to being concentrated in specific counties—particularly Leicestershire and Northamptonshire.

How were these estimates created? The approach used is really the heart of the dissertation's contribution to the field, as it offers substantial scope for extension into other areas. The main data source is the British census, already widely used to document changes in occupational structure. But this dissertation uses these data in a new way—and this is the part I really like—by resurrecting data that other people had dismissed as noise.

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As she describes in the thesis, the original occupational descriptions given by census enumerators were highly varied, ranging from single words to detailed descriptions that were meant to capture key aspects of the work. People employed in boot making might have originally described what they did as "Cordwainer," "riveter," or "master employing 4 men, 4 women and 4 apprentices." When the enumerators books were returned to the General Register Office, these descriptions were streamlined and standardized—transforming more than 60,000 unique descriptions into a narrower taxonomy. People have continued to use this more aggregated data to understand occupational change up to the present.

Instead of relying on these standardized industrial categories, Hillary uses the more nuanced descriptions in the original census returns—newly digitized by the Integrated Census Microdata Project—to examine how the tasks undertaken by boot makers changed over time. She does this using machine learning methods to create a sub-industry "task" classification scheme to understand how jobs evolved over time. She then pairs this with linked census data to understand not just how jobs evolved, but how the jobs of specific individuals changed. What she finds is that the 150,000 lost artisan jobs generally came not from existing workers leaving the profession but mainly from younger men deciding not to enter. New jobs—machinists and foremen—emerged and displaced old ones—binders and closers.

Beyond the scope for future research, there are important policy implications from these findings. As Hillary points out in the dissertation, the details of who is impacted by technological change are important in understanding how we should respond to minimize the social upheaval that follows. Do we need to invest in retraining people who have lost their jobs, or should we focus instead on the next generation of workers? For boot making, Hillary's findings suggest that the latter was more important than the former. But this is partly contextual—technological change was relatively slow, allowing people time to adapt. In industries where the transition was faster, the answers may be different. I hope—and anticipate—that the methods pioneered here will be used to build a better understanding of these changes in other industries.

QIYI ZHAO (STANFORD UNIVERSITY)

Our second finalist is Qiyi Zhao. This dissertation offers three innovative papers on the economic history of two major sources of economic change in early modern Europe: the Reformation and the printing press. If an aspiring Ph.D. student came to me saying she wanted to work on these topics, I might gently suggest that it would be easier to find something new to say about a topic that has not been so widely covered. The extent of the literature is amply demonstrated by the very helpful review at the beginning of this dissertation.

Fortunately, Qiyi's advisors did not give her this advice—or, if they did, we as economic historians are fortunate that she did not listen. Through a combination of newly unearthed archival sources and innovative approaches, the three papers offer a highly original take on events that have been the focus of major research efforts across both history and economics for several decades—no small feat for someone at the beginning of her career.

The first paper casts a critical eye on a widely used technique in the economics literature on the impact of the reformation. Distance from Wittenberg may have become the subject of running jokes about the pursuit of causal explanations for economic change,

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but its use has underpinned many important arguments about the unfolding of one of the defining eras of social and economic change in European history. It relies on the assumption that, in an era when information could not travel any faster than humans, and humans could not go any faster than horses, distance was a binding constraint on the pace at which ideas could move.

This sounds sensible, but is it actually true? Qiyi's work tackles this question head on, looking at the dates of publication of pamphlets to assess how quickly ideas could indeed travel. She finds that ideas could spread much faster than previously assumed and that there was no clear relationship between physical distance and their adoption, thus calling into question a fundamental assumption of this approach to research. The key thing that stuck with me from this chapter was the need to interrogate our assumptions about the past, and use what knowledge we can to understand how things really worked—not how we think they did.

The second paper tackles another central topic in research on the Reformation, namely the adoption of Protestantism. As she points out, existing work has focused on the decisions of rulers to adopt the new faith or stick with the old—and what this meant for later economic developments. But little of this work considered, at an individual level, who opted to adopt Protestantism and who did not. Qiyi argues that faith was a "conscious choice" (p. 87) for individuals, and rulers' decisions were not only personal to them but reflected careful strategic considerations. Structural features of cities are not enough to explain individual choices within them. In this chapter, insight into this question comes from the archives.

Qiyi zooms in on several cities that conducted referenda on whether to adhere to Catholicism or Protestantism. The city of Memmingen offers sufficient data to link individual voting records to tax rolls and other information, producing a detailed picture of who voted for what. This is amazing stuff for the early modern period. It shows that what you might call the "old guard"—people with higher political status, who lived in more prestigious neighborhoods, or belonged to guilds with more capital-intensive production were less likely to embrace Protestantism than others.

The third paper turns to the impact of printing. Economists and economic historians have credited the diffusion of movable type with a whole range of impacts on European economic history, from rising innovation to growing human capital. This chapter dissects the business of printing to understand these impacts in more detail. While the focus of much research on the introduction of printing is on the reduction of cost, a more granular take on the operations of printers shows that the impact was more nuanced. Put simply, movable type favored a different kind of publication—shorter, simpler, and more likely to enjoy broad-based demand. At the same time, hand-copying of manuscripts continued for a long period after the introduction of the printing press. This argument is supported with detailed data on the lengths of different kinds of publications.

Together, these three chapters represent what can be achieved by bringing together methodological innovation and a detailed engagement with historical sources.

DIEGO CASTAÑEDA GARZA (UPPSALA UNIVERSITY)

Our third finalist is Diego Castañeda Garza. The key phrase in his dissertation is "censored century." This is an ambitious work, which in seven substantive chapters, offers new interpretations of Mexican economic history from independence in 1810

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until the middle of the twentieth century. Perhaps the biggest contribution is new data on the nineteenth century—which has been, as he puts it, "censored" due to a lack of available data. This was an exciting period in Mexican history, which included not only the end of the Spanish empire and the transition to independence but also the Mexican-American War, the dictatorship of Porfirio Diaz, and the Mexican Revolution.

Here, Diego challenges the notion of the "censored century" through deeply impressive research in an astonishing number of archives across Mexico and the United States. I had initially intended to read out the list of archives presented in the dissertation but realized that (a) it would take too long, and (b) my Spanish pronunciation would probably be painful to anyone who actually speaks it. Still, you can take my word for it that this represents a major effort. Diego collected more than 180,000 documents—primarily wills and testaments—from these archives. Of these, some 5,000 were sufficiently detailed for him to create a pioneering new dataset on wealth inequality in Mexico across the eventful century from 1810 to 1910.

It shows, perhaps not surprisingly, that inequality was high—Gini coefficients presented range from over 0.7 to 0.9. While valuable, these figures don't capture the richness of the dataset. This database not only allows for a picture of how wealth inequality changed over time, it also—and this is where the dissertation goes beyond existing work on the topic—offers a story about subnational variation. Other chapters focus not on the national picture but on trends in inequality in particular regions—for example, Sonora, for which there is a detailed account of growing inequality in landholding.

This dataset represents a major contribution to the economic history of an important but perhaps neglected country, which will undoubtedly pave the way for future research by Diego and other scholars. It will allow Mexico to be included in comparative research on global economic history in new ways. For that alone, we should congratulate him, and I look forward to seeing the research that emerges from his efforts.

But there is a wider contribution here to questions relevant beyond the economic history of Mexico. The focus on inequality and the construction of social tables is apt, given the history of the period during which historians have claimed that the distribution of wealth played an important role in the political and economic turmoil of the period. How did inequality contribute to war and vice versa? The data presented here show that war had a mixed impact on inequality. Mexico faced several periods of conflict during the nineteenth century and not all had the same impact on the distribution of resources. The War of Independence destroyed wealth, while others provided opportunities for elites to consolidate their positions.

Having used these data to develop a detailed account of the "censored century," which perhaps would have been enough of a contribution on its own—the dissertation goes on to examine how the earlier period shaped Mexico's economic policies during the first six decades of the twentieth century. The final two chapters examine, respectively, income inequality from 1895 to 1940 and the origins and impact of ISI policy from the 1930s to the 1960s.

We have become used to the three-paper format in economic history dissertations and to the notion of the job market paper, which is stronger than the others. What is truly impressive about Diego's dissertation is not only the construction of the dataset on wealth inequality but also the fact that all seven chapters are substantive and make a real contribution to the economic history of the region. This dissertation was astonishing in the breadth of topics covered, ranging from land tenure to ISI. Every chapter had a unique question and an empirical strategy to match.

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Much like the first two finalists, the relevance of this research goes beyond the historical. As Diego writes, its findings speak "directly to ongoing debates around the developing world"—and, we might add, the developed world—about the implications of high levels of inequality and the struggles of political institutions to manage them.

LEIGH GARDNER, *London School of Economics*

Technological Unemployment in Victorian Britain: A Tasks-Based Analysis

To what extent did technological unemployment sweep across England in the wake of the British Industrial Revolution? We do not know. The extent to which new machines replaced workers, leaving them temporarily unemployed, has never been quantified. Some scholars have argued that technological unemployment caused substantial harm to workers (Landes 2003; Frey 2019), while others doubt the scale of the impact (Mokyr, Vickers, and Ziebarth 2015). This disagreement reflects the paucity of quantitative data available on labor displacement and reinstatement in historical context.

Two decades of research on skills-biased technological change have shown that the impact of new technologies is best viewed at the task level of occupation (Acemoglu and Autor 2011). I therefore construct a new, finely grained occupational dataset covering 180 million individual-level English census records (ICeM).[1] This enables the first systematic quantification of job loss and job creation during Britain's Second Industrial Revolution.

With the fourth industrial revolution on the horizon, long-standing anxieties about the impacts of new technologies on the future of work have resurfaced. The historical record provides a rich archive of how individuals and societies adapted to previous periods of rapid structural transformation, and long-run data permits insight into how margins of adjustment differed between generations and cohorts.[2]

A TASKS-BASED ANALYSIS

In my first paper, I propose an approach that makes it possible to quantify the number of jobs lost to, and generated by, creative destruction in the nineteenth century. I create new, sub-industry, task level data on occupation for England between 1851–1911. This more granular information is extracted from text descriptions of occupation in the 180 million individual level English census observations in the Integrated Census Microdata project (ICeM).

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[1] The data is parsed from the recently digitized full count decennial British Historical Census Data (Schurer and Higgs 2025).

[2] The literature review and reflections on lessons from history section of the dissertation is heavily based on my co-authored paper with Benjamin Schneider: The Past and Future of Work (Schneider and Vipond 2023).

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Every British Census from 1841 to 1921 asked individuals to declare their occupation. Which they did, sometimes parsimoniously, with 2 or 3 words, and sometimes with 10 or 15. The result is millions of unique character strings describing occupation. In 1851, when the General Register Office received thousands of enumeration books, officials quickly realized that they could not tabulate occupations at the level of detail that had been reported. They therefore devised a taxonomy of occupations at the industry level and employed clerks to assign individuals to the appropriate categories. When the historical census records were digitized, the original occupational strings were preserved in digital form.

My approach dynamically clusters occupational words by combining machine learning with interactive user input. Occupations are compared to existing clusters, and when confidence is low, the user decides whether to join an existing cluster or create a new one. Over time, these decisions train a classifier that learns to assign future words automatically, allowing the dictionary to grow adaptively across industries.

To pilot my approach, I focus on the mechanization of the English bootmaking industry. This was an extremely large industry, employing nearly a quarter of a million people. In 1851, it ranked as the fourth-largest sector in the country, only surpassed by agriculture, domestic services, and general labor. As the largest craftsmanship industry of its time, it supported the livelihoods of nearly a million people. Mechanization was precipitated by the introduction of the bootmaking sewing machine in 1857, during a period that coincides with the availability of excellent, individual level data on occupation, preserved in the historical census records. This makes the bootmaking industry a particularly straightforward case study for examining the long-run impacts of labor-displacing technological change.

The newly constructed "task-level" micro-occupational data reveal sharp structural changes that had been entirely obscured at the industry level of analysis. At the more granular level of observation, I find a pronounced decline in older, artisanal forms of bootmaking work following mechanization. Approximately 152,000 artisanal bootmaking jobs disappeared as traditional skills became obsolete. At the same time, 144,000 new jobs emerged. These were more specialized, mechanized, and adapted to a new mode of production.

At the national level, the countervailing forces of job loss and job creation netted out. This was not the case at the more regional level. Following mechanization, artisanal bootmaking jobs declined in every county in Britain. However, the emergence of the new occupations was heavily concentrated in only two counties, the counties which mechanized—Northamptonshire and Leicestershire. Using the British Business Census of Entrepreneurs (Van Lieshout, Bennett, and Smith 2021) data on firm size, I show a strong correlation between the location of large industries and the emergence of new jobs.

THE YOUNG BEAR THE BURDEN: EVIDENCE ON TECHNOLOGICAL UNEMPLOYMENT FROM NINETEENTH-CENTURY BRITAIN

With the new data having revealed previously invisible structural shifts, I explore the impacts on workers and their families. I construct panel datasets from cross-sections of full count, individual level, decennial census returns (1851–1911) to track incumbent workers over time. Male incumbents, who constituted 82 percent of the industry, largely avoided substantial labor displacement or technological unemployment. In contrast,

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female incumbents experienced significant displacement. Given the compositional effects, however, the overall rate of labor displacement for incumbents was minimal. The loss of 152,000 jobs in traditional bootmaking occupations was predominantly a matter of declining entry. Young men stopped taking up work in these occupations.

I ask whether the decline in entry that followed mechanization was the result of the new labor-saving technology pushing young men out of artisanal bootmaking work or whether they were simply moving to better opportunities in other industries. The identification strategy exploits the fact that the sons of bootmakers had always been disproportionately likely to become bootmakers themselves, and this behavior did not change following mechanization. In sharp contrast to other young men, the sons of bootmakers continued to enter the industry at similar rates both before and after mechanization. This effectively assigned them quasi-randomly to declining and flourishing local industries. I compare outcomes for sons who became bootmakers in declining versus nondeclining areas, capturing the differential effects of entering a shrinking industry. To isolate these effects from unobserved family or regional characteristics, I implement a within-household difference-in-differences design, comparing treated sons to their brothers who did not become bootmakers. This design controls for shared household background, county-level shocks, and other confounding factors.

I find that sons who entered bootmaking in declining counties experienced significantly worse outcomes than their peers in flourishing areas, and worse than their own brothers who pursued other occupations. While the bootmaker sons of bootmakers in prospering regions maintained relatively stable trajectories, those in declining areas were less likely to marry, head their own households, or remain in their home counties. In contrast, brothers who did not become bootmakers show similar outcomes, regardless of local bootmaking industry conditions.

The finding that the labor-saving technological shock displaced entrants rather than incumbents challenges conventional narratives. I propose to draw a distinction between two kinds of labor-displacing technologies: those that are abrupt and displace all incumbents immediately, and those in which diffusion is slow, allowing incumbents to remain in place. In the former, the impact of the labor-displacing technology falls on incumbents. In the latter, it primarily impacts young people, who face fewer employment opportunities in the impacted industry.

CONFIDENCE IN THE ABSENCE OF GROUND TRUTH: A CENSUS LINKING QUALITY TEST

In my third paper, I check that my findings are not driven by the choice of census linking specification. Linking historical census datasets faces significant challenges. A first-order issue is the risk of false positives, which can distort key downstream coefficients of interest, particularly for migration and occupational change (Ó Gráda et al. 2024). These errors then result in incorrect conclusions being drawn. In this paper, I propose an approach that makes it possible to assess the share of false positives in panel datasets generated by any given linking algorithm. It does not require ground-truth data. Instead, it exploits systematic biases induced by false positives in outcomes of interest to construct a litmus-testing process. Using this framework, I take a multi-model comparison approach to explore the impacts of model specification choices on the linked panel datasets. I propose a generalizable falsification test derived from this methodology, which can serve as a data quality check for any census linking algorithm.

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I show that British census records, leveraging granular data on the parish of birth, are much less exposed to false positives, and that my substantive findings are robust to a range of census linking specifications.

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Technology and Institutional Change in Sixteenth-Century Europe

This dissertation examines the economic dynamics of two transformations in Early Modern Europe, with a focus on the Holy Roman Empire. I study the advent of movable-type printing around 1450, and the Reformation starting in 1517. The three chapters, which are intended as separate journal articles on a shared theme, each explore one aspect of these changes. Drawing on economic theory and modern statistical methods, I use granular primary-source data to challenge some of the influential conclusions in the literature and offer new perspectives on these two transformations' economic roots and legacies.

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RETHINKING "DISTANCE FROM": THE EXAMPLES OF WITTENBERG AND MAINZ

This chapter reappraises an influential methodology in current research, the use of distance-based instruments. I take two widely used such instruments—distance from Wittenberg for the diffusion of the Reformation (pioneered by Becker and Woessmann 2009), and distance from Mainz for the diffusion of printing (developed by Dittmar 2011)—as case studies. The findings have general implications.

Distance instruments function on two premises. First, distance influenced adoption because the low speed of transmission and high cost of human travel made historical events diffuse concentrically around their origins. Second, the exclusion restriction requires that since origin places such as Wittenberg or Mainz were otherwise unremarkable in history, distance from these places is not otherwise correlated with long-term economic development.

My chapter provides historical and statistical evidence that challenges these assumptions. First, I investigate how the Reformation and printing technology diffused and find two patterns that challenge the existing view. First, even in pre-modern times, information and people traveled much faster than conventional wisdom implies. Therefore, their distance should not have constrained the adoption of available ideas. For instance, Martin Luther himself noted that his 95 Theses "almost raced through all of Germany in fourteen days" (Moeller 1972, p. 24). In contrast, the first German territory to adopt Protestantism took about eight years to reach that decision. Second, there is no evidence of concentric diffusion around an innovation's birthplace (e.g., Wittenberg or Mainz) beyond its immediate surroundings. I collected data on the publication of Luther's writings as a conservative estimate for the speed and scope of information diffusion. Specifically, I track how the same pamphlet—essentially a packet of information—got published around the Empire. Regression analysis shows no evidence that places closer to Wittenberg published the same pamphlet earlier than cities further away. At times, it even produces significant results to the contrary. In addition, a pamphlet edition took at most a few months to traverse the Empire, a small lag compared to the timeframe from adoption decisions. City-level data on the diffusion of printing technology show a similar pattern of rapid and non-concentric spread.

If physical distance did not impede adoption, then why do prominent studies such as Becker and Woessmann (2009) and Cantoni (2012) find significant first-stage IV and OLS results? I conduct a series of placebo exercises, replacing "distance from Wittenberg" with distances from about 25 other locations, including some very Catholic locales such as Rome or Vienna. Wittenberg turns out to be far from unique. Most alternative locations yield similarly significant, or even stronger, results. Of course, the placebo locations being significant in predicting adoption does not mean Wittenberg was insignificant, or that the alternative locations should serve as instruments instead. It shows, however, that statistical significance is not a reliable indicator of distance as a predictor of adoption.

The phenomenon noted previously extends beyond the Reformation case and German geography. Monte Carlo simulations reveal a general statistical pattern: distance-based regressors frequently produce falsely significant correlations. Even when the outcome reflects pure spatial noise, distance variables—distance from one place or from the nearest of multiple places—produce falsely significant correlations up to 95 percent of the time. The false significance rate varies with the degree of spatial autocorrelation on either side of the regression equation, and it only disappears when one side exhibits no spatial

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structure at all, an unrealistic requirement for most historical variables. Conventional adjustments, such as Conley standard errors (Conley 1999), do not solve this problem.

These findings suggest that in OLS settings, distance-based regressors will often produce misleading results. In IV regressions, spurious correlations imply a violation of the exclusion restriction, in addition to questionable instrument relevance. These drawbacks undermine these instruments' validity and, consequently, the conclusions of research studies that rely on them for identification. My paper concludes with caution regarding the use of distance instruments in historical settings.

VOTING ON THE REFORMATION

This chapter asks who embraced Protestantism in the sixteenth century and why, investigating the extent and direction of self-selection into this institutional change. A large literature aims to measure the Reformation's long-term consequences, usually focusing on the exogenous aspects of adoption (e.g., Becker and Woessmann 2009) or framing the Reformation as a natural experiment or external shock (e.g., Cantoni 2015, p. 562). Limited research has examined the endogenous aspects of adoption decisions or acknowledges that faith represented a conscious choice for many individuals rather than an accident imposed by rulers.[1]

Choice was indeed available to many ordinary people in the sixteenth century, and archival data permit systematic analysis. Many free imperial cities decided their own religion by conducting polls or referenda among adult citizens. I can identify ten cities that engaged in this practice, four of which left detailed evidence that identifies the participants by name. For some cities, I can match these individuals to other documents from the same period, which reveal their social and economic standings. This allows me to formally test for predictors of supporting the Reformation.

I focus on one city as a case study: the imperial city of Memmingen in southern Germany today. Memmingen had the most comprehensive records regarding its referendum, which happened in November 1530. I manually link it to two other sources: the city's tax register from 1521 and a continuous roster of political office holders between 1517 and 1530.

Regression analysis finds significant selection along several dimensions. Patricians, the hereditary urban elite, were much more likely to support Catholicism than commoners. Holders of key political offices—such as city council members and guild masters—supported Catholicism more than lower-level civil servants and ordinary people. In addition, residents of the Old Town (*Primo Stat*) area, which was home to some of the longest-standing families, as opposed to the newly arrived and socially mobile urban middle class, also supported Catholicism more than any other neighborhood. While the amount of tax one paid was not a consistent predictor, the *nature* of one's income and occupation mattered: guilds whose livelihoods depended on capital, such as patrician rentiers and wholesale merchants, supported the old faith much more than guilds that relied on labor, namely craftsmen. In all, the elite, however defined, embraced the Reformation much more reluctantly than ordinary people.

Qualitative sources suggest some possible explanations. First, the elite enjoyed certain privileges that had long been a source of contention. Reform theology, as it was understood at the time, promised to abolish them. Second, resourceful families often

[1] A list of existing papers can be found in Becker, Pfaff, and Rubin (2016).

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had business connections with the Empire's Catholic elite, and even the Habsburg court itself (Eirich 1971). Accepting Protestantism would harm one's business interests.

These findings shape our understanding of the Reformation and its impact in at least two ways. First, meaningful self-selection seems to have existed. While existing research attributes long-term differences between Protestant and Catholic economic outcomes to Protestantism's "treatment effect" in fostering pro-economic behavior and attitudes, the people and communities who chose to become Protestant might be fundamentally very different from those who remained Catholic. Second, existing scholarship tends to see the Reformation as an unexpected shock triggered by a charismatic monk (Becker et al. 2020), and Reformation theology (such as Gospel-reading or worldly asceticism) as unintentionally fostering economic prosperity (Becker and Woessmann 2009; Weber 1930). But perhaps the movement owed its success to addressing longstanding tensions, and the reform theology might have already reflected ordinary people's socioeconomic demands and aspirations.

TECHNOLOGICAL CHANGE AND THE MARKET FOR BOOKS, 1450–1550

This chapter examines how printing technology's economic features shaped the market structure of this industry. Conventional perspectives consider movable-type printing to be a cost-reducing technology, which shifted the supply curve of books outward, lowering the prices and raising the quantity of books and knowledge in circulation (e.g., Dittmar 2011; Mokyr 1990, p. 49). This view, however, does not explain many market phenomena. For example, printing's predecessor, manuscript production (copying books by hand), did not quickly disappear but continued for centuries. Moreover, despite an already lively medieval book market, the printing industry was initially unprofitable, and many firms failed. Drawing on extensive product-level data and the business records of sixteenth-century firms, I document widespread market patterns that are currently overlooked or misunderstood. I then use economic theory to explain them, with new implications for printing's contribution to European economic development.

Firm operation records reveal several kinds of new incentives and constraints brought forth by this technology. First, printing changed the cost structure of book production from constant returns to scale to increasing returns to scale, with lower but still substantial marginal costs. Second, manuscripts were a bespoke product made one copy at a time. Typically, printing produced about 1,250 copies simultaneously (Voet 1972). A book edition was printed one sheet of text (for all copies) at a time; no complete copy existed before the end of a print run. This necessitated large upfront investments, which many firms found challenging. It also entailed substantial uncertainty because firms were making the entire print run before knowing if the product would sell. Lastly, rather than bringing total improvements over hand-copying, printing entailed some comparative disadvantages, such as printing in color and customization.

I show that these incentives and constraints translated into concrete changes in the book market. Printing's financing requirements, uncertainty, and comparative disadvantages led firms to shift toward shorter, smaller, and less aesthetic products because doing so lowered the cost of each copy and thus the overall investment in each edition, limiting potential losses and improving cash flow. My statistical analysis shows that firms that adapted to these incentives enjoyed longer survival in this industry. In addition to making shorter and smaller editions, this also meant diversifying one's product portfolio, given limited total financing capacity, as a form of hedging against uncertainty.

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In between-firm interactions, printing's increasing returns to scale encouraged firms to differentiate their products rather than competing on price or quantity on the same product. I built a simple model of entry and competition whose implications are consistent with some interesting features of the emerging market. For example, the rise of book fairs and book catalogues provided a signaling and coordination mechanism to discourage product overlap. The economic incentives against overlap were so strong that legal deterrence mechanisms, namely privileges and copyright restrictions, were seldom invoked.

As a corollary to product differentiation, the inter-regional market saw extensive long-distance trade, rather than localized re-printing and consumption. This challenges the current research that studies the impact of printing, which assumes localized consumption and compares cities with a press against cities without a press (Dittmar 2011; Rubin 2014; Boerner, Rubin, and Severgnini 2021; Dittmar and Seabold forthcoming).

My findings suggest new perspectives on printing's historical impact. Rather than making the same medieval volumes cheaper and more abundant, printing gave rise to smaller and simpler products that presumably appealed to a less-educated audience and fostered variety through portfolio diversification and product differentiation. While conventional scholarship sees printing as a vehicle for the diffusion of useful information or a conduit to Protestantism, book content in this period was largely religious with limited direct economic relevance, and both Protestant and Catholic regions participated actively in this industry. Printing's core contribution to European economic development might not lie in the diffusion of *content*, but in the aforementioned changes in *form*, which gave rise to demand for literacy—an economically useful skill that fulfilled a precondition for industrialization and modern democracy.

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Wealth, War, and Modernization

Historians have traditionally referred to Mexico's nineteenth century as the "Censored Century" due to the lack of available data, especially economic data. This dissertation challenges this view by constructing wealth and income data that provide a lens through which to observe the political economy of wars, political reforms, and structural change.

Although the dissertation seeks to address several research inquiries, three interconnected questions structure the project from its inception to its conclusion. Firstly, can we quantify the nineteenth century's wealth distribution? The answer is affirmative; by compiling a substantial corpus of testament or will inventories, triangulating multiple estimating methodologies, and disciplining the resulting series against political chronology, I am able to reconstruct wealth distributions for 1810 to 1910. Secondly, do wars exhibit a consistent distributive pattern? I find this is not the case. Wars impact the distribution of income and wealth through financial means (i.e., who bears the financial burden) and destruction (i.e., which capital is consumed or undergoes a change in ownership). Extractive wars—characterized by regressive finance and privatized gains—increase inequality; destructive wars can mitigate it; and a third type of war, which I refer to as depredative,[1] shares the attributes of the preceding two. Examples of such wars are the War of the Reform (1858–1861) and the Second French Intervention (1861–1867), both of which led to greater inequality. Lastly, what impact did modernization have on inequality? During the Porfiriato, the dictatorship of Porfirio

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[1] Figure 2 in the dissertation introduction exemplifies the typology.

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Díaz (1876–1911), modernization progressed under the influence of elite capture, and inequality increased. Following the revolutionary leveling (1910–1940), the Industrial Search for Investment, which expanded an urban middle class while neglecting agriculture, exacerbated overall inequality through structural dualism.

Following these organizing questions, the dissertation is structured into two distinct sections: Part 1: "From Insurrection to Revolution" and Part 2: "From the Country of One Man to the Country of Institutions and Laws."

FROM INSURRECTION TO REVOLUTION

Measuring the censored century necessitates pragmatism. In a world characterized by sparse wage series and scarce fiscal microdata, wills and notarial inventories serve as repositories for individuals' assets. I construct a novel, meticulously curated database (Castañeda 2023) and estimate wealth distributions spanning the period from 1810 to 1910 employing five complementary strategies (Castañeda 2024): (1) direct tabulations on the unbalanced sample; (2) wealth "social tables" aligned with known social structures; and (3–5) parametric fits (lognormal, weighted-lognormal, and generalized beta of the second kind (GB2) on balanced decadal samples to assess robustness. Credibility rests on two fundamental pillars: triangulation (concordance across estimators) and historical alignment (co-movement with political events). I explicitly address the selection process, acknowledging that will-makers are not a representative sample. These limitations are the reason why I turn to the study of income once sources become available and reconstructing the income distribution becomes feasible.

The wealth distribution was non-linear and experienced significant fluctuations during the period from 1810 to 1910. The action was concentrated in the top 1 percent and the middle 40 percent of the population, while the top 10 percent and the bottom 50 percent maintained a fairly constant share of the wealth. Wealth inequality fluctuated in response to various political events, including episodes of war finance, debt crises, and institutional reforms. These events mapped onto the oscillatory pattern of wealth inequality. Notably, the Porfiriato period broke this earlier oscillatory pattern. With the elite capturing a consolidated state and relative peace that facilitated development, inequality trended upward, creating conditions conducive to revolutionary pressures.

The new data refutes a simplistic assessment of war. Two primary channels—finance and destruction—determine the effect of war on inequality. Destructive wars occur when concentrated capital is physically destroyed, and these losses are not offset by regressive finance. Extractive wars exacerbate inequality by socializing costs through narrow bond markets, regressive taxation, or indemnities, while rents or compensation remain privatized. Predatory civil wars combine seizure, destruction, and extraction, accompanied by localized levelling where devastation is severe.

This categorization of wars is not a matter of wordplay; it is a measurement guide. I use the Mexican-American War (1846–48) as a quasi-experiment; weak fiscal capacity and debt financing were associated with increases in wealth inequality—the extractive channel—even while some localized destruction temporarily diminished it. The identification strategies employed are an event study and an instrumental variables approach that precisely identify these dynamics and situate the argument within broader debates on leveling (e.g., Scheidel 2017; Alfani and Di Tulio 2019; Scheve and Stasavage 2010). The results show that the Mexican-American War, indeed produced some leveling,

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but it was short lived. About three years after the war ended, the political status quo returned, and with that, inequality was back to its pre-war levels.

If war "made the state" (Tilly 1990), Mexico illustrates how it can simultaneously fail to develop capacity (Centeno 1997). The post-1848 border was shorter and more manageable; frontier pacification and customs enforcement became easier, allowing for state formation. However, Porfirian policy produced a captured state: land laws that privatized communal holdings through surveying companies; coerced frontier campaigns (e.g., against Yaquis and Mayos); and property-rights engineering that assembled the asset base for an elite-led modernization. The dissertation's middle chapters distinguish state formation from state capacity and argue that their misalignment (Mazzuca 2021) is an important explanation behind the high levels of inequality. In that interpretation, the Revolution serves as a distributional correction to a modernization project spearheaded and sustained by an oligarchic regime.

FROM THE COUNTRY OF ONE MAN TO A NATION OF INSTITUTIONS AND LAWS

As sources improve, I am able to use different research methods. I construct income social tables for the years 1895, 1910, 1930, and 1940 at an 18-group resolution that represents the most important broadly defined occupations. This resolution strikes a balance between preserving economic structure and enabling long-term comparability between the smaller 1895 and 1910 social tables and the more than 100 groups from the 1930 and 1940 tables. I source population shares from censuses, and I assemble group incomes from historical yearbooks, payrolls, and administrative records. Special attention is given to the top incomes, including large landowners, industrialists, and merchant-financiers. These figures are anchored in archival and scholarly evidence, such as Haber's (1989) work on investment returns and Rosenzweig's (1965) analysis of historical income series.

A key contribution underscores the frequently overlooked aspect of mishandling the highest incomes while employing social tables. This mistake, not taking into account top incomes, can generate a mechanically egalitarian impression, disregarding the underlying economic disparities. The analysis reveals that, despite the Revolution's initial potential to engender a more egalitarian society and some early leveling, the confluence of institutional capture, cronyism, and structural transformations led to heightened inequality even during a progressive era.

The concluding chapter capitalizes on the previously established social tables and renders them dynamic. By annually re-constructing the social tables (1895–1960), I employ group-specific income indices and population shares to generate dynamic Ginis, top shares, and sectoral series. This approach facilitates the identification of causal relationships during the Import Substitution Industrialization (ISI) period (1935–1963). The ISI was characterized by the introduction of trade barriers and protections for national industries to promote industrialization of the country, gradually moving from consumer to intermediate to capital goods. I use a generalized two-way fixed effects and a staggered difference-in-differences design that links policy instruments and development bank lending to sectoral incomes and the overall income distribution. Notably, during the ISI period, industrial policy resulted in convergence within contemporary urban sectors, while agriculture experienced stagnation. Consequently, the overall outcome was a mesocratic distribution characterized by growth accompanied by wider structural disparities.

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THE CONTRIBUTIONS: THEORETICAL AND EMPIRICAL

I demonstrate that in Mexico in the long nineteenth century, inequality was not a linear progression but an oscillating, politically mediated sequence that fluctuated in response to political events and steadily increased under Porfirian elite capture. This provides a distributional explanation for revolutionary fervor that avoids determinism, emphasizing the political economy and the disparities between economic winners and losers in the country's transformation. Traditional neo-institutional approaches to underdevelopment tend to be static or impervious to the dynamic nature of political economy. My dissertation specifically addresses the shortcomings of existing generalized interpretations of Latin American inequity.[2]

I posit a conditional bellicist logic, wherein wars escalate or reduce inequality through financial means and destruction. I distinguish state formation from economic capacity, with the income and wealth distributions indicating the disparity. Furthermore, I identify a coherent mid-century regime—mesocratic industrialization—that harmonizes rapid growth with increasing inequality, drawing parallels with Graciarena (1979), Vellinga (1988), and Thorp (1992).

Two implications become evident. Firstly, war and crisis finance leave a distributional mark. When large, urgent state action relies on narrow debt markets and regressive taxation, it will result in wealth concentration unless countered by broader taxation or explicit redistributive design. Mexico's 1840s serve as a historical laboratory for a macro-fiscal problem with distinct political implications. Secondly, industrial policy is inherently distributional. The ISI illustrates how a policy bundle that boosts productivity and formal-sector wages can still exacerbate structural disparities if it redirects resources to the organized urban sector while neglecting agriculture and informality as secondary concerns. The lesson is not to abandon industrial policy but to collaborate on inclusive design—finance, standards, skills, and supply-chain integration that transcend the urban-rural and formal-informal dichotomy.

From an empirical perspective, my use of pioneering methods of digitalization for constructing large datasets offers the first comprehensive long-term reconstruction of the distribution of wealth. I also clarify certain contentious debates surrounding comparability and standardization in the ongoing social table literature. Furthermore, I present a novel perspective on the formation of the Mexican state from an empirical standpoint, challenging existing interpretations of the nineteenth century. In summary, my dissertation provides a fresh perspective on Mexican economic history and serves as a series of historical examples pertinent to contemporary development debates.

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[2] See the discussion about the historical inequality in Latin American and the neo-institutionalist positions of North, Summerhill, and Weingast, Engerman and Sokoloff, and Acemoglu, Johnson, and Robinson in the introduction, pages 54 to 56.

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