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Essays on Political Economy and Economic Geography

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by

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ABSTRACT OF THE DISSERTATION

Essays on Political Economy and Economic Geography

by

Sebastian Ottinger
Doctor of Philosophy in Management
University of California, Los Angeles, 2021
Professor Nico Voigtländer, Chair

My dissertation consists of three essays. In the first, I systematically document the importance of chance to a fundamental question of economic geography: How did locations develop their specializations in specific manufacturing industries? I show that European immigration to the United States affected the initial location of industries in the late nineteenth century, creating a spatial pattern that remained relatively stable. Immigrants' exposure to specialized manufacturing knowledge and skills depends on their origin. The comparative advantage that came to US counties "embodied" in immigrants predicts employment in disaggregated manufacturing industries in subsequent decades. The early establishment of firms in novel industries gave locations first-mover advantage and shaped local manufacturing specialization. Agglomeration forces locked in industries until the present. I address endogeneity issues by exploiting arguably random variation in early immigration enclaves due to the interaction of the aggregate arrivals from European countries, and the movement of the frontier of settlement across U.S. counties.

The remaining two chapters consider the importance of individual actors in the realm of politics. My second chapter, co-authored with Max Winkler, studies the incentives for local political leaders when facing an unforeseen threat to their incumbency. The chapter examines the case of the unexpected and short-lived electoral success of the pro-redistribution Populist Party in the 1892 presidential elections. The Populists sought support among poor farmers, regardless of race. This biracial alliance threatened the Democratic establishment in the South, providing it with an incentive to fan racial fears to split the newly formed coalition. Newspapers affiliated with the Democrats spread propaganda of attacks by Blacks on the White community, often involving allegations of sexual assault. Using novel newspaper data, we identify these hate stories and show that they become more prevalent in the years following the 1892 presidential election in counties where the Populists were active. The effect is large and found in newspapers affiliated with the Democrats only. The evidence suggests that the propaganda "worked": where newspapers spread more propaganda, the Democrats see stronger gains in presidential elections in the following decades, long after the Populists left the political arena.

The third and last chapter, co-authored with Nico Voigtländer, considers the importance of national political leaders for the performance of the states they govern. We create a novel reign-level dataset for European monarchs, covering all major European states from the 10th century until World War I. We first document a strong positive relationship between rulers' intellectual ability and state-level outcomes. To address endogeneity issues, we exploit the facts that i) rulers were appointed according to primogeniture, independent of their ability, and ii) the wide-spread inbreeding among the ruling dynasties of Europe led to quasi-random variation in ruler ability. We code the degree of blood relationship between the parents of rulers. The 'coefficient of inbreeding' is a strong predictor of ruler ability, and the corresponding instrumental variable results imply that ruler ability had a sizeable effect on the performance of states and their borders. This supports the view that 'leaders made history,' shaping the European map until its consolidation into nation states. We also show that rulers mattered only where their power was largely unconstrained. In reigns where parliaments checked the power of monarchs, ruler ability no longer affected their state's performance. Thus, the strengthening of parliaments in Northern European states (where kin marriage of dynasties was particularly wide-spread) may have shielded them from the detrimental effects of inbreeding.

The dissertation of Sebastian Ottinger is approved.

Romain Wacziarg

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Nico Voigtländer, Committee Chair

University of California, Los Angeles
2021

To My Family and Friends

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UCLA Anderson School of Management and the Economics Department of UCLA offered an intellectually exciting and thriving research environment in economic history, political economics, cultural economics, and international economics to me. I want to extend my special thanks to Michela Giorcelli, Dora Costa, Ariel Burstein, Pablo Fajgelbaum, and Jonathan Vogel for the opportunity to present each quarter, and to them and all participants at the Economic History and International Trade proseminars for the thoughtful feedback.

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All remaining errors are mine. The second chapter is a version of a working paper with the same title co-authored with Max Winkler (Harvard University). The third chapter is a version of a working paper with the same title co-authored with Nico Voigtländer (UCLA). For both co-authored chapters, Sebastian and other co-authors were co-PIs.

This dissertation would not have been possible without the support of my family and friends, to whom I dedicate this dissertation.

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CHAPTER 1

Immigrants, Industries, and Path Dependence

1.1 Introduction

Employment in most manufacturing industries is strongly geographically concentrated (Ellison and Glaeser, 1997; Holmes and Stevens, 2004). To explain why locations specialize in particular manufacturing industries, two approaches have been advanced and tested. The first shows that natural advantages render locations suitable to specific industries (Kim, 1998; Ellison and Glaeser, 1999). Second, a location's market potential –that is, its access to producers of inputs and to consumers– could induce local specialization (Harris, 1954; Krugman, 1991a).[1] However, many locations share comparable endowments in natural resources and market potential. What determined which of these became centers of a specific industry?

Case studies often emphasize *chance*. Krugman (1991b), for instance, asserts that "the whole process of industrialization in the United States was marked by [..] stories of small accidents leading to the establishment of one or two persistent centers of production."[2] Arthur (1990) shows that such accidents can, theoret-

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[1] Both of these accounts have empirical content for explaining local specialization during the emergence of the U.S. manufacturing belt. Klein and Crafts (2011) show that initially, natural advantages were decisive for cities' manufacturing specialization, but their market potential became increasingly important as a determinant.

[2] The importance of such accidents has been noticed for at least a century. For instance, reviewing 15 US industries that were highly localized by 1900, the US Census Bureau (1907) remarks that market potential and natural resource endowment "in almost all cases account for localization only in its broader sense. They prescribe an industry's possible area, but they fail to explain the most marked form of localization that within a single city or town." The Census report highlights chance locations of individuals as a driving force in rendering a few locations

ically, shape a location's industrial specialization over the long-run. They help in selecting among multiple equilibria and, once an industry is established in a location, agglomeration and co-agglomeration of related industries "lock-in" the industry there (Krugman, 1991a; Ellison et al., 2010). One of these accidents is the choice by an individual pioneer in an industry's early stage of one of a set of locations sharing comparable endowment in natural resources and market potential. Empirical evidence on the consequences of such chance location and the resulting path dependence at the location-industry level is scant.[3]

In this paper, I propose and validate a determinant of such chance locations and document their effect on employment patterns at the location-industry level. I exploit the historical context of the emergence of the US manufacturing belt, which coincided with the Age of Mass Migration from 1850 to 1920. Throughout this period, European immigrants, trained in particular manufacturing industries, brought their knowledge, skills, and entrepreneurial spirit to counties all across the United States. The Midwest, settled in the nineteenth century became one of the world's manufacturing powerhouses, with highly pronounced regional patterns of specialization. Immigrants contributed to local industrial development in various ways. They provided their skilled labor and (often) tacit knowledge to existing establishments, induced others to set up shop there, or became entrepreneurs themselves.[4] I proxy for the *potential* embodied in a county's early immigrants to affect local employment patterns – either themselves or by attracting later immigrants who shape the local industry. My proxy combines two data sources.

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centers of these industries.

[3] Bleakley and Lin (2012) and Allen and Donaldson (2018) study path dependence of economic activity at the *location* level. Davis and Weinstein (2008) show that even after the destruction of Japanese cities during World War II, their industrial specialization remained unaffected. While this points to the possibility of a unique equilibrium for county-industry location, it is not definite evidence of it, as the shock might have been too small to make an established, already locked-in and coordinated, system of location-industries switch between equilibria.

[4] With the recent availability of historical patenting data, research has confirmed the role of immigrants in shifting aggregate and local innovative activity in the US during and beyond the Age of Mass Migration (Akcigit et al., 2017; Peters et al., 2018).

For each of 13 European origin countries,[5] I multiply their population in US counties settled in 1850 by their origin country's comparative advantage in 49 manufacturing industries.[6] Summing these up at the county-industry level yields my measure of "immigrant specialization."

My main finding is that immigrant specialization is a strong predictor of county-industry employment across manufacturing industries and US counties in 1910. I control for possible confounders by introducing fixed effects at the county and industry levels, and control for the initial level of county-industry employment in 1850. In the preferred specification, doubling immigrant specialization increases county-industry employment in 1910 by four percent.[7] The baseline association is stronger in faster-growing industries; in particular, in the then-novel industries associated with the First and Second Industrial Revolutions. In these industries in their early stages, European immigrants had valuable skills, such that one would expect their effect on county-industry employment patterns to be greater. The baseline association is particularly strong for German immigrants, because Germany industrialized in the second half of the nineteenth century – also the period of its unification –, and became an international manufacturing powerhouse in then-novel industries, such as chemical and electrical manufacturing.

Immigrant specialization is empirically distinct from other determinants of county-industry employment, such as counties' market access or inherent natu-

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[5] These 13 countries are Ireland, Germany, the United Kingdom, France, Sweden and Norway, Switzerland, the Netherlands, Italy, Spain, Portugal, Austria-Hungary, Belgium, and Russia. Immigrants from these countries account for 91% of all immigrants to the US in 1850 and 72% in 1910.

[6] I calculate the comparative advantage revealed in exports to the United States in 1909. This approach draws on the insight of Balassa (1965) that a country's comparative advantage reveals itself in its specialization in trade. The data comes from Treasury reports listing imports into the US by trade partner and traded good. In robustness checks, I use US import data from 1851 and 1881 and data on global exports by country and industry to calculate the manufacturing specialization of European origins.

[7] The size of the coefficient is comparable to that of market potential. In specifications excluding county fixed effects, the elasticity of market potential in 1850 (varying at the county level only) is 0.14, similar to that of immigrant specialization in the specification excluding county fixed effects.

ral advantage for specific industries. By including market potential (interacted with industry fixed effects) and industry characteristics (interacted with county fixed effects), the regression flexibly controls for the two dominant approaches to explaining specialization at the location-industry level. The baseline association is robust to alternative measures of immigrant specialization, such as using county-origin immigrant populations in later decades, or using origin-industry comparative advantage calculated from 1851 and 1881 trade data, or from global trade patterns. Alternative measures of county-industry employment similarly confirm the positive and significant association of immigrant specialization with local industrial employment. I show that immigrant specialization is also positively associated with a dummy indicating whether there is any employment in county-industries, and with various other measures of local employment specialization advanced in the literature.

I provide evidence that immigrants did not selectively settle in counties where "their" industries were already established, or had inherent potential. On the "frontier of settlement" (Turner, 1893), agriculture dominated and many areas only became industrialized and specialized in particular manufacturing industries after the Civil War. Following Bazzi et al. (2020), I identify the counties on this frontier in each decade. The immigrants living in counties on this frontier in the decades before 1850 had migrated to counties that by 1850 were *less* specialized than other counties in "their" industries. Only after 1850 did the specialization embodied in the early immigrants translate into employment patterns. By 1910, the specialization of counties in the earlier frontier regions – as well as in the rest of the US – is strongly predicted by immigrant specialization.

An instrumental variable strategy further addresses the concern of immigrants foresightedly migrating.[8] The approach is inspired by the historical narrative

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[8] I complement this with heterogeneity results speaking to the concern of selective migration to industry potential. The baseline effect is particularly strong in the auto and electrical manufacturing industries. These industries did not exist or see commercial application until well

emphasizing that immigrants coming to the US in search of land to farm migrated to land available on the frontier. Specifically, I use only the variation in county-origin populations induced by the interaction of (i) a dummy indicating whether a county was on the "frontier" in each of the years 1820, 1830, and 1840, and (ii) the aggregate arrivals of immigrants from each country of origin in the subsequent decade. Complementing the "pull" attraction of the frontier to immigrants, I extract the component of emigration "pushed" out of its European origins by climatic shocks. With this variation in county-origin populations, I construct instruments for immigrant specialization. The first stage is strong and the second stage shows no evidence of selective positive migration to counties based on their potential. The instrument confirms that on the frontier, immigrants initially tended to settle in regions no more likely than others to subsequently have more employment in their industries. By 1880, this reverses, and the specialization embodied implicitly in the immigrants 30 years earlier translates into employment patterns.

How, then, did immigrants affect employment in their destination counties' industries? I show that immigrant specialization predicts the early entry of pioneer firms in county-industries. I proxy for the initial entry of firms into a county-industry by identifying whether any owners or managers in the full-count US census were active in a county-industry in a given year. I show that this entry of pioneer firms is systemically predicted by immigrant specialization – that is, the early immigrants origins and the comparative advantage of those origins across manufacturing industries. On the frontier of settlement, counties with higher immigrant specialization are initially *less* likely to have individuals working as owners in industries in which the immigrants have a comparative advantage. Again, this pattern reverses with entry in county-industries in accordance with the specialization embodied in the counties' initial immigrants. I show not only that immigrant

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after 1850. Immigrants could hardly have moved to counties because of their suitability for still unknown industries.

specialization leads to the earlier entry of firms per se, but also that these pioneer firms are owned by immigrants from countries with higher county-industry immigrant specialization. [9]

The presence of immigrants from particular countries in 1850 gave US counties a comparative advantage once these industries developed in those countries, producing skilled and entrepreneurial workers who later immigrated. Once county-industries were locked in as the manufacturing belt matured, county-industry employment tended to remain high in its initial centers. I show that immigrant specialization predicts county-industry employment in 2000 in places where it was an important determinant initially: in then-novel industries, and particularly in the counties that before 1850 were on the frontier.

The paper proceeds as follows. Section 1.2 discusses the related literature and my paper's contribution. In Section 2.2, I provide information on the historical background and highlight two case studies of immigrant entrepreneurs shaping their destinations' industrial future for decades. The measure of immigrant specialization is introduced in Section 1.4, in which I also describe the measurement of county-industry employment based on full-count US censuses. Section 1.5 presents the baseline result and discusses its robustness and heterogeneity. In Section 1.6, I provide evidence that selective migration likely does not account for the baseline result. Mechanisms are discussed in Section 1.7. Section 1.8 shows that immigrant specialization has an effect on county-industry employment lasting down to the present. Section 2.6 concludes.

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[9] I also show, using data from the Census of Manufactures, that immigrant specialization increases the size and productivity of existing establishments at the county-industry level as early as 1860. This suggests that there are multiple channels by which immigrant specialization affected employment patterns. Immigrant specialization induced pioneer entrepreneurship, by natives and immigrants, yet it also enabled existing establishments in county-industries to draw on a foreign pool of skilled labor.

1.2 Related Literature

This paper speaks to several streams of literature in economics. Research in urban economics has established the importance of multiple equilibria in determining aggregate economic activity across locations (Davis and Weinstein, 2002; Redding et al., 2011; Bleakley and Lin, 2012; Jedwab et al., 2017; Allen and Donaldson, 2018). While both historical narrative and economic theory emphasize the importance of multiple equilibria at the location-industry level (US Census Bureau, 1907; Arthur, 1990; Krugman, 1991b), providing systematic empirical evidence for historical events locking-in industries in particular locations has proven difficult.[10] I provide such evidence for the historical setting of the emergence of specialization in the US manufacturing belt. The presence of immigrants from particular origins only became a comparative advantage over time.[11] Naturally, then, the paper speaks to a literature concerned with the determinants of the spatial distribution of industry-specific economic activity. A county's early immigrants' origins and the trade specializations of those countries of origin constitute a significant determinant of county-industry employment patterns. This baseline result is robust to controlling for determinants of location choice emphasized by the literature both in general (Rosenthal and Strange, 2001; Ellison and Glaeser, 1999; Midelfart et al., 2000), and specifically for the emergence of the American manufacturing belt (Klein and Crafts, 2011; Kim and Margo, 2004).[12] The evidence presented in this paper suggests that "accidents of history" – in this paper's case, the initial location decisions of earlier immigrants that were unrelated to the potential those

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[10] Note that once industries are locked-in across locations, the resulting pattern seems robust even to large shocks (Davis and Weinstein, 2008). Rauch (1993) analyses how – even in the presence of agglomeration forces – transition between equilibria can take place.

[11] Note the similarity to, and difference from, Bleakley and Lin (2012). Portage was a source of locational comparative advantage lost over time, while immigrant specialization only becomes a comparative advantage at the location-industry "activated" decades later.

[12] An earlier literature on patterns in US manufacturing has emphasized the importance of natural advantages and market access (Perloff, 1960; Niemi, 1974). US Census Bureau (1907) provides an early indication of the role of immigrant entrepreneurs and skilled immigrant labor for selected industries. See footnote 1 and the discussion in Section 2.2.

chosen locations had for *manufacturing* industries – can lock in industries across locations.

This paper also furthers our understanding of skilled immigrants' contribution to the United States' rise to manufacturing power (Rosenberg, 1977; Sequeira et al., 2019).[13] Using granular census and patent data, Akcigit et al. (2017) show that immigrant origins affected aggregate innovation patterns in the long-run, and Peters et al. (2018) provide evidence for local short term effects. My paper shows that immigration from specific origins at the technological frontier influenced employment patterns over the short and long runs. Crucially, my measure of immigrant specialization is based on trade data and thus captures both productivity codified in patents, and the tacit skills embodied in individuals. Patenting rates in the late nineteenth century varied starkly across industries (Moser, 2012). Skilled craftsmen and engineers often provided tacit skills received in their training in Europe to American manufacturers or founded their own manufacturing companies based on trade secrets. The spillover effects described in the mechanisms section of this paper, onto natives and immigrants from other countries may underlie the positive relationship of diversity to economic outcomes in this setting (Alesina et al., 2016; Fiszbein, 2017). The main result speaks to the effects of individual origins on economic outcomes (Fulford et al., 2020).

My findings also contribute to broader studies of the short- and long-term effects of immigration on host locations. Although the origin-specific effect of international migration on economic outcomes across regions within a country has received some attention (Hornung, 2014; Moser et al., 2014; Borjas and Doran, 2012; Fourie and Fintel, 2014), this paper is the first to analyze, across many origins, the long-term effect of origin-specific differences on industry-specific out-

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[13] I review the empirical evidence and extensive literature in economic history on the nexus between skilled migration and U.S. manufacturing during the Age of Mass Migration in more detail in Section 1.3.1. Abramitzky and Boustan (2017) provide an excellent overview of migration in US history and of recent advances in the literature based on newly available Census data.

comes in the destination locations. The findings indicate that the sign, size and significance of such an effect for the historical episode under consideration vary strongly across origins and industries. Immigrants arriving from origins at the technological frontier enabled the diffusion of novel industries. This implies that the industry transfer through immigration is conditioned by factors beyond the institutional and economic environment of the destination country. The one-time transfer of skills and knowledge during the Age of Mass Migration and its effects have been studied for a wide range of single-origin country-industry pairs. For instance, Danish immigrants brought knowledge and skills in the dairy industry (Boberg-Fazlic and Sharp, 2018) and British immigrants did so for shipbuilding in Newport News (Hanlon, 2018). Other examples include Belgian window glass workers (Fones-Wolf, 2004) and Welsh tinplate workers in Pittsburgh (Jones and Lewis, 2007). This paper, however, focuses on immigration from many origin countries and on many disaggregated manufacturing industries in a systematic assessment, which allows me to analyze the determinants of knowledge transfer across origin, destination, and industry characteristics.

In the last decade, a parallel literature highlighting that immigrants can facilitate the diffusion of industries has emerged at the cross-country level. There is increasing evidence that immigrants influenced their destination countries' industry and export structure since the late twentieth century. Bahar and Rapoport (2018) document that receiving immigrants from a country that already exports a specific good, increases the probability that the host country will start exporting this good within the next decade.[14] In this paper, I use within-nation variation in destination locations. All immigrants to the US arrived in the same destination country with an integrating national market and emerging federal regulatory in-

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[14] They survey the accumulating evidence on international migration flows and knowledge transfers in the first paragraph of Bahar et al. (2018). See Lissoni (2018) for an excellent review of the evidence. We have ample reason to believe that the diffusion of productivity through immigration was even stronger in earlier times, when knowledge was more tacit and less codified (Meisenzahl and Mokyr, 2011; Moser, 2012).

stitutions. Thus, different industrial policies or related pull-factors at the national level are not potential confounders of immigrants' effect on economic outcomes.[15]

Lastly, the results in this paper speak to a literature in international trade concerned with the origins of comparative advantage. Ever since the seminal contribution of Eaton and Kortum (2002), Ricardian theories of trade have seen an unexpected revival (Costinot and Vogel, 2015). The origins of productivity differences at the core of these models, as well as the identification of these as truly Ricardian forces have been subject to some debate (Costinot and Donaldson, 2012; Morrow, 2010), as have the dynamics of relative productivities (Hanson et al., 2015; Levchenko and Zhang, 2016). Pellegrina et al. (2019) study the role of internal immigration in shaping specialization in agriculture in Brazil. In this paper, I provide evidence on how accidents of history can lock-in local specialization for extended periods. Particularly in manufacturing, for which natural advantages are less important, and agglomeration forces are stronger than they are for agriculture, path-dependency – and therefore the role of historical accidents – is likely to be a more important determinant of local specialization.

1.3 Context: US Manufacturing and European Immigration

In this section, I first briefly discuss the coincidence of the Age of Mass Migration and the emergence of the Manufacturing Belt. The fact that the US Manufacturing

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[15] Note, however, that industrial policies at the local level might pose a problem for the interpretation of the results. "Boosterism" – the practice of promoting a city to immigrants and entrepreneurs – was fairly common during the nineteenth century in the US (Abbott et al., 1981; Gold, 1994; Wrobel, 2002). In several cases, an entire industry was attracted by such boosting. Attracting Goodrich's rubber company in 1869 with a cash payment laid the groundwork for Akron, Ohio, to become the "rubber capital of the world." The extent to which boosters searched for entrepreneurs in *particular* industries instead of entrepreneurs per se is not clear from the historical literature. Further complicating matters, divergent views among different segments of the local elite might often have made searching for entrepreneurs in particular industries difficult (Haeger, 1982).

Belt was only emerging then, combined with the sizable skilled immigration from European countries at industry-specific technological frontiers provides an optimal setting for immigrants to affect local manufacturing employment patterns. Then, I provide two examples of immigrant entrepreneurs from Europe that shaped the industrial development in their destination counties.

1.3.1 Skilled Migration and the US Manufacturing Belt

From 1850 to 1930, during the "Age of Mass Migration", 30 million European immigrants arrived in the United States (Hatton and Williamson, 1998). At the same time, the United States emerged as the world's manufacturing powerhouse. This section briefly presents selected stylized facts of both these developments, and traces the scholarly inquiry into their relationship.[16]

Immigrants came from all of Europe and settled widely across US counties. The leading sending nations varied substantially over time, as the left panel of Figure 1.1 illustrates. Initially, the largest share of immigrants to the United States came from the United Kingdom, Ireland, and the German states. Later into the second half of the nineteenth century, the share of immigrants hailing from Italy, Russia, and Austria-Hungary increased. These countries became the main origins of immigrants to the United States by 1900 and remained so until the Age of Mass Migration ended after WWI. Immigrants settled on the open farming land on the nation's Western frontier and its principal and growing cities. The right panel of Figure 1.1, depicting the share of individuals of foreign birth by US county in 1850, documents wide variation across counties in their share of foreign-born individuals. Notably, the South attracted little immigration, while the Midwest and Northeast boasted sizable foreign-born populations. In some counties and cities, their population share exceeded 50 percent.

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[16] An excellent survey of immigration in US history is provided by Abramitzky and Boustan (2017). Niemi (1974) and Kim and Margo (2004) document the emergence of the American Manufacturing Belt.

Almost simultaneously, the United States underwent a structural transformation from agriculture to manufacturing. Out of this, it emerged as the world's major industrial power by around 1900. Yet, manufacturing employment was not equally distributed across space in the United States. Figure 1.2 shows the number of manufacturing workers across US counties in 1850 and 1910. From there, the spatial dimension of this change is apparent. While by 1850, the New England region dominates manufacturing employment in the US, by 1910, the "manufacturing belt", extending from there to the Midwest, had formed (Krugman, 1991a).

What is the nature of the nexus between European immigration and US manufacturing from 1850 to 1920? The early literature on this subject has mainly emphasized *domestic* economic forces, such as the integration of the United States' national market through canals and railroads, and the implementation of tariffs in manufactured goods (Wright, 1990; Rothbarth, 1946; Hounshell, 1985).[17] Habakkuk (1962), for instance, argues that high wages in the US and the resulting incentives for labor-saving technological progress were crucial in leapfrogging the United Kingdom. This view therefore considers immigrants mainly as a cheap source of low-skilled labor in US factories. Recently, with the availability of full-count census data and historical patent data at highly disaggregated spatial units, the relationship between the US's rise to manufacturing might and skilled migration has seen renewed interest.[18] This literature highlights that the United States profited from skilled immigration from countries at the technological frontier through their innovative capabilities and diversity (Alesina et al., 2016; Fiszbein, 2017). Using granular census and patent data, it has been shown that immigrant origins affected innovation patterns in the aggregate in the long (Akcigit et al., 2017) and locally in the short-run (Peters et al., 2018).

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[17] See Rosenberg (1977) for a contemporaneous, yet, contrasting account emphasizing the importance of foreign imported technology and Lafortune et al. (2015) for a recent inquiry emphasizing domestic factors.

[18] So has the study into its domestic determinants, see for instance Perlman et al. (2016) and Hornbeck and Rotemberg (2019).

Indeed, while many immigrants were farmers of low-skilled laborers, at the same time, many immigrants were skilled and often worked in skilled or semi-skilled positions in US manufacturing after arrival. Figure 1.3 shows the skill composition of US manufacturing workers by country of birth in 1850 and 1910. In both years, about half of the immigrants working in manufacturing and born in the United Kingdom and Germany are working in semi-skilled or skilled occupations, and so are sizable shares of immigrants from other origin countries.[19]

1.3.2 Case Studies of Immigrant Entrepreneurs

Who were those immigrants working in manufacturing, and how did they affect the location of industries in the United States? Many skilled artisans, exposed to industries and trained in Europe, likely gave a county-industry an edge over its competition. Apart from select case studies, we know little about their long-run effect. To the contrary, historical records of individual entrepreneurs abound. They allow me to illustrate the effect of immigrant entrepreneurs on local manufacturing employment decades later. In the following, I briefly introduce two of such entrepreneurs, and describe their effects on the locations they resided in. These case studies focus on a Welsh immigrant entrepreneur in shoe-making who settled in Lynn, Massachusetts, and the son of a Westphalian (German) immigrant in St. Louis, Missouri.

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[19] Wegge (2002), in her study of German immigrants from Hesse-Cassel, and Long and Ferrie (2013), in their study of British immigrants, find evidence of skilled migration. Abramitzky et al. (2014) provide systematic evidence that immigrants from several European origin countries arrived with more occupation-based skills than native workers. Sequeira et al. (2019) note that the nature of selection is line with the prediction of the standard Roy model. Immigrants from Western European countries were neutrally selected, while immigrants from the European periphery were negatively selected. I will return to these differences in the heterogeneity analysis (section 1.5.2 of the paper).

John Dagyr and Shoe-making in Lynn, Massachusetts

The first case study considers the Wales-born entrepreneur John Adams Dagyr and the rise of Lynn, Massachusetts, to one of the "shoe-making centers of the United States" (US Census Bureau, 1907). That census report on the "Localization of industries" (chapter 12) puts the consequences of his location decision well:

> *"In the early colonial days this settlement had its quota of cobblers, who made as well as repaired the shoes: for the region thereabout, but did not attempt a broader market. In 1750, however, John Adams Dagyr, a Welshman, and a skilled shoemaker, settled in Lynn, and began to teach his apprentices the art of fine shoe-making. It soon became known that shoes were being made in Lynn nearly as good as the best made abroad, and as early as 1764 Dagyr was spoken of in a Boston newspaper as "the celebrated shoemaker of Essex." Had this man settled in Roxbury, Mass., rather than Lynn, the bias toward shoe manufacturing might have become established in that quarter, and Roxbury instead of Lynn might to-day be one of the three great shoe centers of the United States."*

One immigrant, therefore, shaped the employment history of Lynn, Massachusetts. It is noteworthy that this is not a singular case, as many individuals decisions left equally important marks on manufacturing employment in a particular industry:

> *"The nature of many a city's industry has been shaped in just this way in the early days of its history by the decision of one man. Instances of this [extend to] the localization of collars and cuffs, hosiery and knit goods, jewelry, gloves, and fur hats."*[20]

Often, European entrepreneurs seemingly set up industries in towns in appar-

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[20] Another example is the (then-illegal) "import" of Arkwright's British technology by Samuel Slater that led to city's localization in textile mills. He became known as "the father of the American Industrial Revolution" in the US, and as "Slater the traitor" in his native England.

ent ignorance of their suitability for a particular industry:

> *"Several of the above selected industries have been established in their respective localities by the emigration from Europe of individual skilled workmen or groups of skilled workmen. The town where such a man chances to settle is taken for a location of the industry in most cases without much questioning whether or not it is better adapted for it than any other town."*

This example focused on a British immigrant entrepreneur bringing industries of the First Industrial Revolution from the United Kingdom to the United States. The next example is that of a German immigrant and an industry commonly associated with the Second Industrial Revolution.

Emil Mallinckrodt and Drug Manufacturing in St. Louis, Missouri

Emil Mallinckrodt, born to a Westphalian aristocratic lineage, emigrated to the United States in 1832 from "political disillusionment and economic deprivation" (Mercelis, 2015). There, he successfully operated a farm close to St. Louis, Missouri. Two of his sons went on to study chemistry in Germany in 1864, studying under Carl R. Fresenius in Wiesbaden. One of them worked as a trainee at " E. de Haen Chemische Fabrik List GmbH", a company founded by one of Fresenius' former students. Returning to the US, and St. Louis in particular, they founded a chemical manufacturing company on their father's farm. This company became one of the primary drug and photographic equipment producers in the following decades. The company they founded, G. Mallinckrodt & Co. was one of the "earliest chemical manufacturing firms west of Philadelphia" (Mercelis, 2015).

Instances of such a knowledge and skill transfer, its entrepreneurial consequences at the individual level, and their short and run-long run effect in their destination location are difficult to predict. Neither of the Mallinckrodt's worked in chemical manufacturing in 1850 in St. Louis. The company received patents later on, but the initial immigrant, Emil Mallinckrodt, did not. He was not lured to St. Louis by city or county institutions, for he came to the United States without education or experience in that industry. The very industry his sons helped establish in the United States was experiencing unforeseen and dramatic technological progress in Germany only *after* he emigrated.

Both examples highlight the very element of chance in the location decision of immigrants, and, therefore, the problem of measuring the potentials for such accidents. The next section sets out to approach this measurement problem.

1.4 Data and Measurement

In this section, I first describe the construction of the immigrant specialization measure from county-origin populations and origin-industry specialization. Then I describe the measurement of county-industry employment from 1850 to 1930 based on the US full-count censuses.

1.4.1 Immigrant Specialization

The main dependent variable used in the analysis is immigrant specialization. It is a proxy for the industry-specific skills, experience, exposure, and knowledge *potentially* embodied in the immigrants of each US county depending on their origin. To measure immigrant specialization at the county-industry level, I combine and aggregate two sources of variation: the population born in European countries residing in US counties in 1850, and their origin's specialization in manufacturing industries revealed in US import data in 1909.

Immigrant Population By Origin Across US Counties In 1850

First, from the full-count US decennial census of 1850 (Ruggles et al., 2019), I calculate the total number of immigrants born in a particular origin country o residing in US destination county d in 1850 ($IMMIG_{o,d}^{1850}$).[21] For instance, about 26,400 individuals residing in St. Louis County, and recorded by census takers in 1850, were born in polities now part of Germany (30% of the total county population). The maps in Figure 1.5 show the spatial distribution of the total county population and the population born in Germany across US counties in 1850, focusing on the Eastern Seaboard, the Midwest and the South.[22]

Table 1.10 in the Appendix lists the main immigrant origin countries reported in the 1850 census.[23] It lists the total number of immigrants by origin, their population share across US counties, and the number of counties settled by at least one person born in that origin. Irish, Germans, and British amount to the largest groups of immigrants in this census year. Yet, immigrants from many origin countries form non-negligible county enclaves. For instance, immigrants from Sweden and Norway, account for more than 15% percent of the total population in one county.

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[21] To ensure that counties are comparable across time, I employ the crosswalk of Eckert et al. (2020b) and re-aggregate all historical variables at the level of 2010 counties. This leads to some individuals being split across modern counties depending on their area belonging to historical counties. See Eckert et al. (2020b) for detail. In robustness I instead also used 1850 counties as the level of analysis, aggregating and splitting later values to this level based on the approach of Eckert et al. (2020b), yielding highly comparable baseline results (unreported).

[22] Figure 1.12 in the Appendix show this for the entire US Except for California, there are few settlements and manufacturing employment outside of the area displayed in these maps. For the remainder, all maps will retain this focus.

[23] IPUMS classified the various origin countries into modern countries. To ensure comparability with the historical trade data below, I aggregate immigrants born in Sweden and Norway to “Sweden and Norway”, immigrants born in Wales, Scotland and England into the “United Kingdom”, and those born in Austria, Hungary or Czechoslovakia into “Austria-Hungary”.

Comparative Advantage Of European Origin Countries Across Manufacturing Industries In 1909

Secondly, I use information revealed in US import data on origin countries' comparative advantage in traded manufacturing industries. Harmonized data on bilateral trade in disaggregated manufacturing industries is available only from 1962 onward (Feenstra et al., 2005). To overcome this data limitation, I digitize imports into the US by trading partner and traded good from reports on the "Foreign Commerce and Navigation of the United States" in the years 1851, 1881 and 1909 (Treasury, 1851, 1881, 1909). I crosswalk the traded good categories to the industry classification of the Census reported in IPUMS. Appendix 1.10.3 provides details on the sources, correspondences between industries and traded goods, as well as stylized facts on specialization across European origins in manufacturing industries from 1851 to 1909. I calculate the specialization of origins in manufacturing industries revealed in exports to the United States. Balassa (1965)'s seminal insight that countries' comparative advantage is revealed in their export specialization underlies this. His proposed, and commonly used, measure is based on the value of a country o's exports in a particular industry, i, $X_{o,i}$.

$$RCA_{o,i} = \frac{X_{o,i,}}{\sum_i X_{o,i}} / \frac{\sum_o X_{o,i}}{\sum_i \sum_o X_{o,i}}$$

It effectively is the share of industry i in country o's exports relative to the total share of that industry in global exports. Values larger than one indicate that country o has a comparative advantage in industry i. In contrast, values below one indicate a comparative disadvantage.[24]

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[24] Vollrath (1991) and French (2017) provide further measures related to the baseline Balassa measure used in the literature and a thorough discussion of these. Costinot et al. (2011) have recently provided a theoretical foundation in the Eaton and Kortum (2002) model of Ricardian trade for measures of revealed comparative advantage identified from exporter-industry fixed effects. Hanson et al. (2015), in their analysis of the dynamics of comparative advantage, show that their measure yields comparable results to the standard Balassa measure used in here.

I calculate specialization in trade in manufactured goods vis-a-vis the US in 1851, 1881 and 1909.[25] In 1851, 38 industries had some of their goods traded, and by 1909 this number increases to 49. For the baseline measure, I, therefore, use the origin's comparative advantage calculated using 1909 import data.[26] Consider for instance the case of manufacturing of "drugs and medicines".[27] As Table 1.11 in the Appendix shows, there is a sizable variation across countries in their export prowess in this industry. Of the three major origins of immigrants in the US by 1850, Germany has a comparative advantage in this industry, while the United Kingdom and Ireland do not.

Immigrant Specialization

To measure immigrant specialization at the county-industry level, I combine these two sources of variation and aggregate them for European origin countries. Specifically, I first multiply the county-level number of individuals hailing from each origin in 1850 with their origin's comparative advantage in 49 manufacturing industries. Intuitively, the measure weights every individual according to the comparative advantage their origin country exhibited in trade with the US. Returning to "drugs and medicines" manufacturing, one immigrant born in Germany and re-

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[25] A limitation is that I only observe exports to the US and therefore can only calculate export specialization vis-a-vis the US. The export specialization of European origins towards the United States might be systematically different than that towards other countries. I use trade data from 1899 by Tyszynski (1951) available for 16 aggregated manufacturing industries and fewer origins for global exports to show robustness to such a concern.

[26] I provide robustness using 1851 and 1881 trade data. Apart from the increased number of industries, a further advantage of 1909 import data is related to measurement. Some origin countries, such as Italy and Germany, only became unified in the 1860s, and therefore necessarily, the reporting of origins in the 1851 report is unclear. Through Trieste and the Hanse cities of northern Germany, listed as trade partners in 1851, exports from various countries other than Austria or Germany were shipped. The main threat from using 1909 import data is reverse causality: Immigrants might have brought US industries back to their European origins, which then, by 1909, developed a comparative advantage because of the return migrants or information transmission through these. Cf. **?** and **?** for evidence on such channels, and Dunlevy and Hutchinson (1999) for evidence of the general pro-trade effect of immigrants in this setting.

[27] IND1950 industry code 467. Appendix 1.10.3 provides detail on the traded goods corresponding to this industry and other industries, and describes the procedure of constructing a correspondence from traded goods to manufacturing industries in detail.

siding in a US county in 1850 will contribute as much to immigrant specialization as two immigrants hailing from the U.K. and residing in the same county would, or as much as 63 immigrants from Ireland would.

Next, I sum these weighted individuals hailing from 13 European origins up to arrive at a measure at the county-industry level:

$$ImmigrantSpecialization_{d,i} = \sum_{o} \left(IMMIG_{o,d}^{1850} \times RCA_{o,i}^{1909} \right)$$

Table 1.12 in the Appendix provides summary statistics for this measure across 113,239 county-industries.[28] To illustrate, Figure 1.4 shows the geographic variation in immigrant specialization for "drugs and medicine" manufacturing in 1850. Note its close correspondence with the distribution of German immigrants in 1850 (evident from Figure 1.5).[29]

Discussion of Measurement

Immigrant specialization is a necessarily crude measure of industry-specific skills, training, and knowledge that immigrants or their offspring, relatives and future immigrants from their origin might have been exposed to. By construction, it presumes that every immigrant in 1850, and those arriving later, hailing from a particular origin embodies the entire industry specialization of its origin. The measure is thus agnostic to selective migration of immigrants to counties where specific industries are already set up or have future growth potential.

Further features of the measure are noteworthy: First, it ignores the native-

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[28] The data set covers 2,311 settled US counties and 49 manufacturing industries. The origins - listed in decreasing order of their immigrant population in 1850 - are Ireland, Germany, the United Kingdom, France, Sweden and Norway, Switzerland, Netherlands, Italy, Spain, Denmark, Belgium, Portugal, and Austria-Hungary. Immigrants from these origins account for 90.6% of all immigrants residing in the US in 1850 and 99.8% of all European immigrants.

[29] In the empirical analysis, I control for the origin-specific population or include county fixed effects. It is the measures' variation at the county-*industry* level, rather than that of immigrant enclaves at the county level, driving the main results.

born population and their comparative advantages across industries. By using county fixed effects in the analysis, I control for natives' population size in a county, but still do not allow for them to have industry-varying comparative advantages. In a robustness check, I include the native-born with the comparative advantage of the U.K.

Second, using population shares in 1850 might appear early, particularly since this date only marks the beginning of the Age of Mass Migration, with only a negligible number of immigrants from Italy and Russia present. These two countries became the primary source of immigrants into the US at the end of the nineteenth century. By data limitations, 1850 is the earliest date at which the census recorded national origins and the earliest possible start date. As will become evident in the heterogeneity analysis, immigrants from Germany which strongly affected local patterns of manufacturing employment in the US were already present in sizable numbers then.[30] Third, note that origin countries are often geographically large and diverse in their manufacturing employment and specialization in specific sectors within these countries. Most European origins varied strongly in their internal differences in industrialization, especially in their local differences in employment in specific manufacturing industries. Many immigrants from some origins certainly were not exposed in any way to particular industries that their origin exported to the US. Such inaccuracies in measurement should only increase classical measurement error and attenuate coefficients.

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[30] A stronger concern is that measuring immigrant's county-origin population in 1850 is too late. Immigrants might have already sorted to counties according to their comparative advantage and the location's natural advantage of prior employment in particular industries. In a separate section of the paper, I use the fact that the frontier of settlement was only settled very recently and manufacturing, and specialization in particular manufacturing industries, had not developed there in earnest until after 1850.

1.4.2 Manufacturing Employment in US Counties

I measure the employment in manufacturing industries for all US counties from 1850 to 1930 using the full-count decennial census available from IPUMS USA (Ruggles et al., 2019). Their industry classification (IND1950) forms the baseline classification for all the analyses in the remainder. The US Census Bureau only asked its census takers to record industries from 1910 onward. Before that, individuals reported their occupation, albeit they often included their industry when answering this question. IPUMS has imputed industries from this additional information and the direct link between certain occupations and industries. These imputed industries allow me to calculate the number of individuals employed in each US county and industry from 1850 to 1930. Throughout, I focus on individuals of working age (14-70 years). To ensure that counties are comparable across time, I employ the crosswalk of Eckert et al. (2020b) and re-aggregate all historical variables at the level of 2010 counties.

The maps in Figure 1.6 illustrate this by showing employment in "drugs and medicine manufacturing," one of 50 manufacturing industries, in 1850 (left panel) and 1910 (right panel) across US counties. They document the spread of this industry from the New England region (in particular, from a cluster around Boston) to all over the Midwest, with concentrations around St. Louis, Chicago, Indianapolis, and Louisville.

Two facts are evident from both these maps. First, at least for this industry, there is strong evidence of localization. That is, industry-specific manufacturing activity is concentrated in particular locations. This a well-documented fact and extends to most manufacturing industries.[31] Second, note both the consolidation of initial clusters, as well as the emergence of new centers of employment

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[31] Ellison and Glaeser (1997) show that the size distribution of individual plants across industries does not solely drive such clustering. Crafts and Klein (2017) provide evidence that the spatial concentration in US manufacturing decreased from the early 20th century onward, and confirm that most manufacturing industries were spatially concentrated then.

in "drugs and medicine" from 1850 to 1910. Understanding the determinants of these changes for all manufacturing industries, which took place as the US manufacturing belt formed and local specialization within it deepened, is the aim of the following sections.

1.5 Immigrant Specialization And Industry Employment

This section documents a strong association between immigrant specialization in 1850 and county-industry employment in 1910. In heterogeneity analysis, I document that this association is more substantial in newer industries, and for immigrants from Germany. I show that accounting for natural advantage and market access as determinants of county-industry employment does not alter the size or significance of the baseline association, and summarize the robustness analysis conducted in Appendix 1.10.5.

1.5.1 Baseline Results

In the baseline specification, I regress the industry specialization of immigrants in county d and industry i in 1850 on the employment in this county-industry in 1910, controlling for initial employment in this county-industry in 1850:

$$\begin{aligned} log(1 + Employment_{d,i,1910}) &= \beta log(1 + Imm.Spec._{d,i}) \\ &+ \gamma log(1 + Employment_{d,i,1850}) + \mu_i + \mu_d + \varepsilon_{d,i} \end{aligned}$$

Both employment variables enter this specification as the logarithm of one plus the total employment in a county-industry. This enables an interpretation of coefficients as elasticities and including both margins of employment in one specification.[32] Starting with various controls at the county level, I successively intro-

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[32] In the Appendix, I document that the baseline association is driven by both margins, and provide PPML estimation results of the baseline association, as well as additional robustness to the measurement of dependent and independent variable, and the assumptions on the error

duce fixed effects at the industry and county level to account for unobservables. Standard errors are clustered at the county level.

Table 1.1 presents the results of estimating this specification.[33] Column 1 shows the raw association between employment in 1910 and immigrant specialization across 113,239 county-industries. The coefficient implies that one percent higher immigrant specialization in 1850 is associated with employment in that county-industry in 1910 being higher by 0.2%. Column 2 includes county-industry employment in 1850, such that β identifies the association of immigrant specialization with changes in county-industry employment across manufacturing industries and counties. County-industry employment in 1850 is a sizable and significant determinant of county-industry employment patterns 60 years later. So is the primary variable of interest: Immigrant specialization is strongly associated with county-industry employment in 1910, even after accounting for county-industry employment in 1850. Introducing industry fixed effects, column 3 documents that this association is stronger when only considering differences across counties within the same manufacturing industries. Column 4 adds a set of county-level controls, including similar logarithmic transformations of total county population, total county employment in manufacturing, total foreign-born population and, an (untransformed) measure of birthplace diversity, as well population shares of the foreign-born population of each of the origins used in the measure of immigrant specialization. Jointly, these variables decrease the coefficient of interest by a third compared to column 3 and hint at the importance of confounders at the county-level. Specifically, the decrease solely due to the local population shares from European origin countries (from 0.155 to 0.128, unreported) signifies the importance of origin-specific immigrant populations in 1850, and is potentially indicative of immigrants hailing from particular origins selectively migrating to

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terms.

[33] The robustness section below discusses the results in Appendix 1.10.5 on robustness to clustering, fixed effects, measurement of immigrant specialization and using various transformation of the outcome variable.

counties where "their" industries had the potential for growth.

Column 5 is the preferred specification. Including county fixed effects additional to industry fixed effects, the association between county-industry employment in 1910 and immigrant specialization is only identified off variation within industries and counties. The association is sizable. A one percent increase in the number of immigrants in 1850, weighted by their origin's comparative advantage in manufacturing industries, increases employment in 1910 in this county-industry by 0.044%, controlling for county-industry employment in 1850 and only exploiting variation within counties and industries.[34] This amounts to more than a tenth of the effect size of increasing county-industry employment in 1850 by 1%.

The strong positive association of the origin's specialization of a county's immigrants in 1850 and the change in employment patterns in manufacturing in the following decades is the main empirical finding of this paper. In the remainder of the section, I explore the heterogeneity and robustness of this association.

1.5.2 Heterogeneity

Here I show that the association of immigrant specialization with employment patterns is increasing over time – that is, with later county-industry employment as outcome from 1860 to 1930 – and larger in then-novel industries, and for German immigrants.[35]

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[34] Figure 1.8 shows the binned scatter of the residual variation underlying the estimation of β in this preferred specification.

[35] I provide further heterogeneity across county and industry characteristics in Appendix 1.10.4. There I document that the baseline association is stronger in counties with more inhabitants in 1850 and in those with a higher share of foreign born population in 1850. The baseline association is further stronger in initially less agglomerated industries and in those with higher national employment growth between 1850 and 1910. Further, across broader geographic regions – US Census Divisions – the baseline is positive everywhere, but larger in the focal regions of immigration.

Importance of Immigrant Specialization Increases from 1860 to 1930

The outcome variable in the baseline specification is county-industry employment in 1910. Table 1.2 repeats the baseline specification (column 5 of Table 1.1, including county and industry fixed and controlling for initial county-industry employment in 1850) using county-industry employment for the available years between 1850 and 1910 and beyond 1910 as outcome variables instead.[36] The coefficient of interest, β, increases over time, particularly between 1880 and 1910. The decrease in the amount of within variation explained is entirely driven by county-industry employment in 1850 losing its predictive power over time. Excluding it from the regressions reveals an increase in the within-R^2 from 0.07% to 0.17% from 1860 to 1930.[37]

Stronger in Industries of the Industrial Revolutions

During the nineteenth century, two major revolutions redefined manufacturing and entire economies. The "First Industrial Revolution" started in England in the late 18th century and then spread to continental Europe and the US. Industries related to iron, coal, and textile manufacturing recorded particularly sizable technological progress. During the "Second Industrial Revolution", usually dated from 1870 to 1914, Germany and the US gained technological leadership over Great Britain (Mokyr, 1998). While advances in communication, transport, business organization, and the emergence of firm-based R&D departments are seen as crucial components of this Second Industrial Revolution, there also exists a clear notion of newly emerging or dramatically changing flagship industries for the Second Industrial Revolution. These include chemical manufacturing, electri-

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[36] The full-count census of 1890 is not available, as the original documents were destroyed by fire in 1921. For 1900 and 1940, the geographic identifiers used to transform historical counties to modern ones (NHGIS county codes) are not available yet.

[37] The negligible share of variation explained by immigrant specialization reflects its crude nature. See the discussion in Section 1.4.

cal manufacturing, engines, machine tools, and the automobile.

Immigrants likely left a stronger mark on local employment patterns for these novel industries, especially compared to industries that have seen little change during or before the Age of Mass Migration. Novel industries often crucially depend on human capital. Thus, the tacit technical knowledge embodied in high-skilled individuals from European origins likely was a more important locational determinant in these industries. Further, for these industries the spatial equilibrium was only emerging or subject to rapid change. In these circumstances, the potential for the comparatively earlier emergence of an industry in a location to lock it in there is higher (Arthur, 1990).

I operationalize the notion that certain industries were inherently novel (such as electrical manufacturing) or rapidly changing (such as machinery) in the nineteenth century using a simple dichotomy. I classify manufacturing industries into those commonly associated with either Industrial Revolution, labeled "Industrial Revolution industries" henceforth, and those with are not, labeled "Other industries".[38]

Table 1.14 in the Appendix shows results of interacting the baseline association with a dummy for whether the industry was novel then. The coefficient for those "Industrial Revolution industries" is significantly larger than that of the other industries. This difference grows. By 1880, the baseline coefficient for novel industries is not statistically different from zero. In 1910, the baseline coefficient is twice as large for novel industries, and highly significant. Finally, by 1930, the coefficient for novel industries is three times as large than that for other industries. This result confirms that a counties' immigrants specialization in industries had larger effects for industries in which European immigrants potentially had novel

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[38] See Table 1.13 in the Appendix for details. These industries, apart from those mentioned already, further include blast furnaces, fabricated metals, machinery, and textiles, among others. Whenever not clear, I chose to assign an industry to not being an "Industrial Revolution industry".

knowledge and skills, and which at the same time were only emerging in the US

Baseline Association Stronger for German Immigrants

As the baseline effect is stronger in faster-growing industries, a natural question is whether different origins contributed differently at various times. While the United Kingdom was the technological leader throughout the First Industrial Revolution and the industries associated with it, the German Empire, unified in 1871, rapidly caught up in these industries and then became a powerhouse of the Second Industrial Revolution. Separating the measure by the contribution of individual origins allows analyzing which countries immigrants were associated with employment growth at different points in time.

I focus specifically on the separate contribution of German immigrants compared to the contribution of the remaining European origins.[39] Table 1.3 shows the baseline coefficient for immigrant specialization of all 13 European origins in column 1. Column 2 then uses only the immigrant specialization of German immigrants as the independent variable. Specifically, I estimate

$$\begin{aligned} log(1 + Employment_{d,i,1910}) = \beta log(1 + Imm.Spec.(Germany)_{d,i}) \\ + \gamma log(1 + Employment_{d,i,1850}) \\ + \mu_i + \mu_d + \varepsilon_{d,i} \end{aligned}$$

where $Imm.Spec.(Germany)_{d,i}$ is the multiplication of German immigrants residing in county d in 1850 and the comparative advantage of Germany in industry i in 1909:

$$Imm.Spec.(Germany)_{d,i} = IMMIG^{1850}_{d,Germany} \times RCA^{1909}_{Germany,i}$$

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[39] In Appendix 1.10.4.3, I jointly estimate the separate contribution of all European origins. I find systematic differences across origins, both in sign and significance, which I discuss in more detail there.

The coefficient for Germany is more than twice as large than the baseline coefficient comprising the contribution of all European origins. Yet, the baseline association is not merely driven by immigrants from Germany. Column 3 uses only the contribution of all origins except for German immigrants as independent variable. The coefficient is smaller (about 30% of the baseline coefficient) but remains highly significant. Columns 4 to 6 repeat this analysis for industries of the "Industrial Revolution" only. For these, the coefficient of German immigrant's specialization is three times larger than the baseline coefficient, while that of the other origins is about 30% of it.

1.5.3 Alternative Explanations: Market Potential and Natural Advantages

The main result could be driven by initial immigrants accidentally or purposefully moving to counties with higher market access for their industries, or to counties with an inherent natural advantage in these. I account for this possibility by including interactions of county (industry) fixed effects with industry (county) characteristics following the approach of Klein and Crafts (2011).[40] Table 1.4 presents results. I start with the baseline in column 1. Column 2 adds interactions of a county's market potential with industry fixed effects. This allows market size to have a varying effect by industry. The main coefficient of interest is slightly larger than in column 1.[41] Column 3 includes further county charac-

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[40] Since the contribution of Midelfart et al. (2000), economic historians have increasingly used their theory-based empirical framework that allows for simultaneously analyzing the importance of New Economic Geography (NEG) and Heckscher-Ohlin (HO) sources of local specialization (e.g. Wolf (2007)). Klein and Crafts (2011) use this approach in a setting closely related to this paper. They find that both NEG and HO forces were important sources of the specialization of US cities from 1880 to 1920. Ronsse and Rayp (2018) critically review this literature.

[41] I calculate market size (which enters in logarithmic transformation in the specification) using the definition of Harris (1954): $MS_d = \sum_k POP_d/d_{dk}$ where d_{dk} is the geodesic distance between two counties d and k. The exponent on the distance is assumed to be (minus) one. I have experimented using 5 and other values, with little difference in results (unreported). Further I have included the change in market size for each county between 1850 and 1910, which technically is a bad control (Angrist and Pischke, 2008): Immigrant specialization is associated with

teristics interacted with industry fixed effects in the baseline specification. The county characteristics used are dummies for the presence of a railroad, a river or a canal, coal fields, and oil fields, and the share of manufacturing workers in a county that are skilled or semi-skilled in 1850.[42] While accounting for such characteristics of locations increased the amount of variation explained, it leaves the main coefficient of interest sizable and significant. Lastly, in column 4, I include interactions of industry characteristics with county fixed effects in the baseline specification. These industry characteristics are intermediate input use, sales to industry, agricultural input use, mineral resources use, and the share of skilled and semi-skilled workers in an industry.[43] Column 4 shows that flexibly accounting for all such other determinants of industry-specific employment does not alter the coefficient of interest by much.

In sum, immigrant specialization is a sizable and significant determinant of county-industry employment patterns, and distinct from other major determinants of specialization in manufacturing in the US during this period.

1.5.4 Robustness

In Appendix 1.10.5, I conduct various further robustness checks concerned with measurement, specification, and the level of analysis. I briefly summarize the

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employment growth in particular industries, which in turn likely induces employment growth in related (manufacturing or not) industries in the county itself or nearby counties, and thus ultimately affects population and market potential. Controlling for the change in market potential decreases the main coefficient of interest only slightly to its baseline value (unreported). Therefore, the baseline association is not driven by immigrants moving to counties with a larger anticipated market potential.

[42] Data on railroads, rivers, and canals comes from Atack (2013) (accessible from `https://my.vanderbilt.edu/jeremyatack/data-downloads/`. Data on oil (shale plays) come from `https://www.eia.gov/maps/layer_info-m.php` and for coal fields from `https://hifld-geoplatform.opendata.arcgis.com/datasets/us-coal-fields`. I create a dummy for the presence of each of these in a county. Further, I calculate the share of skilled or semi-skilled manufacturing workers present in a county in 1850 from the full-count census (occupation codes 0-99, 200-599).

[43] The first four characteristics come from Klein and Crafts (2011), which I crosswalk from SIC codes to census industries (see Table 1.13). The last industry characteristic, the share of skilled and semi-skilled workers in an industry, is calculated from the census (see above footnote).

findings in the following.

First, I consider robustness to the definition of immigrant specialization (Appendix section 1.10.5.1). The baseline is larger when using origin country specialization calculated from US import data in 1881 in the immigrant specialization measure, and smaller when using that of 1851, yet significant and sizable for both. Using merely dummies indicating a comparative advantage of an origin country in an industry vis-a-vis the US shows a robust positive association. Measures of comparative advantage vis-a-vis the rest of the world (in a more aggregated set of industries) yield a positive association for British, French, and German immigrants. I also explore robustness to the second component of immigrant specialization, immigrant population by origin country in each county (1.10.5.2). Using later years of immigrant populations by origin country in each county yields smaller coefficients, hinting at the importance of the number of early immigrants. Accounting for the – thus far ignored – comparative advantage of native-born individuals by assigning them that of the United Kingdom similarly yields a smaller coefficient .

Second, I provide robustness to specification choices and assumption on the standard errors. Considering both margins of county-industry employment separately, as well as using non-transformed count-industry employment (1.10.5.3), and various transformations of county-industry employment, such as local employment shares and localization measures (1.10.5.4) confirms the baseline result. In Appendix 1.10.5.5 I document robustness to alternative assumptions on the standard errors, paying particular attention to spatial correlation.

Third, in Appendix 1.10.5.6, I document that the baseline is highly comparable when states are used as the spatial units of analysis instead of counties. States with more immigrants in 1850 from origins that had or developed a comparative advantage, on average also saw more employment growth in those industries, relative to other industries and states.

1.6 Selective Migration

The primary threat to a causal interpretation of the baseline association is that immigrants from particular origins selectively migrated to counties where "their" industries already existed or had future growth potential. In this section, I show that in the only recently settled counties on the "frontier" of settlement, early immigrants – mainly farmers– moved to counties that by 1850 instead were *less* likely to be centers of "their" industries. Until 1910, however, these counties developed specialization in the industries embodied in the origins of their immigrants of 1850. Thus, for this geographic subset I show that immigrants did not move to counties where their industries already existed. To address the possibility that immigrants moved to counties where their industries had potential, I advance an instrumental variable strategy based on the movement of the frontier over counties and the aggregate arrivals of immigrants pushed out by climatic shocks from their origin. Results support the view that immigrants induced industrial development in specific manufacturing industries in counties in which they did not selectively migrate to for manufacturing purposes.[44]

1.6.1 Reversal of Employment Patterns on "Frontier" of Settlement

Could immigrants from origin countries that already boasted a competitive manufacturing industry have moved to counties in the US where said industry already existed or had inherent potential? The historical record is clear on this and implies an affirmative answer. Immigrants could know which skills were scarce in the US, where industries requiring those skills where located, and could learn for themselves which locations might potentially be suitable for "their" industries. Early immigrants passed on such information in letters home to relatives inter-

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[44] In Appendix section 1.10.6 I document that the baseline effect is larger in counties settled after 1850 and in industries only emerging after then – auto and electrical manufacturing – further supporting the assessment that immigrants did not selectively migrate until 1850 to counties where their industries had future growth potential.

ested in emigration. Further, migrants could learn it from books circulating in Europe,[45] during their travel to the United States (Battiston, 2018) and finally after arrival directly from local sources. Additionally, firms relied on skilled foreign labor and actively hired European immigrants.[46] Note that directly controlling for county-industry employment in 1850 in the baseline specification only partially addresses the issue of selective migration to existing county-industries. Immigrant specialization might be a better proxy for county-industry characteristics relevant in 1850 and beyond, such as inherent productivity or natural resources, as immigrants skilled in particular manufacturing industries might be able to identify suitable locations for their industries even if the industries did not exist there yet.

Column 1 of Table 1.5 shows evidence for such selective migration.[47] Immigrant specialization in 1850 is significantly associated with county-industry employment in 1850 already. Column 2 of that table repeats the baseline regression with 1910 county-industry employment as outcome. The following two columns repeat the former two using a geographic subsample – counties on the frontier of settlement between 1820 and 1850. The historical narrative of the settlement of the US emphasizes that early immigrants, many of them farmers, settled on land available at their arrival. Land was mostly available in the West of the Alleghenies. This "frontier" of settlement slowly moved further West, until by 1890, the profitably arable land was settled, and the frontier closed (Turner, 1893). Following the approach of Bazzi et al. (2020), I identify the counties on the frontier in the decades from 1820 to 1840.[48] For these, immigrants hardly could migrate to long-established centers of manufacturing employment. Instead, the vast majority of early settlers were farmers. Column 3 of Table 1.5 show that there, immigrant specialization in 1850 is negatively associated with county-industry employment

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[45] For instance, Wander (1852) is one of many such guide books for immigrants.

[46] In terms of magnitude, international recruiting, however, likely was of minor importance (Rosenbloom, 2002).

[47] Similarly, the decrease in the main coefficient of interest after accounting for county-origin population shares in the baseline specification indicates that selection is a concern.

[48] Appendix 1.10.7 provides detail.

in 1850.[49] Immigrants arriving until 1850 located in counties with comparatively less employment in industries in which their origin countries had a comparative advantage. However, in the following decades, employment in these industries grew comparatively more where immigrant specialization is high – as in the rest of the country – such that by 1910 the employment pattern in counties on the frontier in until 1850 resembles the specialization of their 1850 immigrants' origin countries. Still, immigrants to the frontier could have settled in counties with inherent potential for industries, even if these industries did not exist there until after 1850.

1.6.2 Instrumental Variable Approach

To address the concern that some immigrants by 1850 might still have selectively settled to counties where their industries were undeveloped or not existent, I leverage variation in county-origin population arguably unrelated to inherent potential for county-industry employment in both 1850 and 1910. Using this quasi-exogenous component of the county-origin population in the immigrant specialization measure facilitates the construction of an instrumental variable addressing this concern directly.

Logic and Implementation of Instrumental Variable Strategy

The instrumentation strategy employed is as follows. Immigrant specialization is measured as the summed-up multiplication of county-origin population (say, the number of French-born individuals in St. Louis) in 1850 and their origin's comparative advantage revealed in US import data in 1909 (say, France's in "electrical manufacturing"):

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[49] Figure 1.9 depicts the coefficients of these regression illustrating the reversal of specialization on the frontier.

$$ImmigrantSpecialization_{d,i} = \sum_{o} \left(IMMIG^{1850}_{o,d} \times RCA^{1909}_{o,i}\right)$$

I provide quasi-exogenous variation in its first component, $IMMIG_{o,d}$, drawing on the "pull" exerted by the frontier on immigrants "pushed" to the US out of their European origins due to climatic shocks there. Specifically, the instrument strategy extracts the component in county-origin population induced by (i) the movement of the frontier across counties in the West, and, (ii) aggregate arrivals of immigrants from European origin pushed out due to large deviations from seasonal temperature and precipitation. For the exclusion restriction to hold, this variation, interacted with the origin's comparative advantage, must be unrelated to a location's inherent potential for success in specific manufacturing industries. This requires that, as the frontier covered counties suitable to particular industries, immigrants from origins which were - or became - specialized in these industries, did not migrate in larger numbers to the United States because of climatic shocks in their origins.

For the first component, I trace the frontier and identify all counties that were on the frontier in any of the years 1820, 1830, and 1840 following Bazzi et al. (2020). The second component is the total immigration into the US from particular origin countries (Office of Immigration Statistics, 2013) in the following decade. To focus on agricultural immigrants, I use the component of immigrant flows by origins predicted by yearly deviations from seasonal precipitation and temperature means larger than one standard deviation and aggregate these at the decade-origin level (Sequeira et al., 2019).[50] From the interaction of these, I predict log population in 1850 across county-origins in a cross-sectional "zero-stage" regression:

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[50] Appendix Section 1.10.7 provides maps, information on the construction of the frontier indicator and on the calculation of immigrant inflows by origin based on climatic shocks.

$$log(IMMIG_{o,d}) = \sum_{t=1820,1830,1840} \beta_t \left(1(frontier_{d,t}) \times IMMIG_{o,t}\right) + \mu_d + \mu_o + \varepsilon_{o,d}$$

where $1(frontier_{d,t})$ is a dummy indicating whether a county was at the frontier at the beginning of a decade. $IMMIG_{o,t}$ is the total number of immigrants arriving from an origin in the rest of that decade predicted from climatic shocks in their origins. County (μ_d) and origin fixed effects (μ_o) account for features rendering locations attractive to all immigrants alike and differences across origins in their settlement patterns across counties.

I extract the population by origin and county predicted only by the interactions, $\widehat{POP}_{o,d} = exp(log(\widehat{IMMIG}_{o,d}))$, and use it in lieu of the actual county-origin population to construct an instrument for immigrant specialization:

$$Immigrant\widehat{Specialization}_{d,i} = \sum_o \widehat{POP}^{1850}_{o,d} RCA^{1909}_{o,i}$$

The first stage to predict the component of immigrant specialization due to the "push" exerted by arrivals due to climatic shocks in their origin and the "pull" of the open and available land of the frontier is then as follows:

$$\begin{aligned} log(1 + ImmigrantSpecialization_{d,i}) = \beta log(1 + Immigrant\widehat{Specialization}_{d,i}) \\ + \gamma log(1 + Employment_{d,i,1850}) \\ + \mu_i + \mu_d + \varepsilon_{d,i} \end{aligned}$$

IV Results

In Appendix 1.10.7, I provide the results of the zero-stage estimation. The movement of counties along the frontier and aggregate inflows predicted by climatic shocks strongly predicts county-origin populations in 1850. Table 1.6 presents the results of the instrumental variable analysis. The regression sample is restricted to all counties on the frontier at least once between 1820 and 1840. These counties cover most of the Midwest, albeit in varying duration.[51] Column 1 shows the first stage results. The constructed instrument is a significant predictor of immigrant specialization in 1850. Column 2 replicates the baseline analysis for the reduced sample. The reduced form regression in column 3 shows a smaller and less significant relationship between county-industry employment in 1910 and immigrant specialization. Column 4 finally shows the IV results. The IV coefficient is by order of magnitude larger than the comparable OLS coefficient of column three, and strongly significant. This does not necessarily imply negative selection by the immigrants according to counties' industry potential. IV results reflect the local average treatment effect of the compliers. In this setting, compliers are county-industries where immigrants arrived because the county was at the frontier when many immigrants from origins with a comparative advantage in these industries arrived. Always-takers in this setting are county-industries that would have seen employment growth independent of immigrants arriving due to the frontier. These potentially are locations with inherent natural advantages pertaining to these industries. Immigrants' embodied specialization probably was a more important determinant for the complier counties which would not have developed industries were it not for the immigrants arriving early on. Therefore, we expect OLS estimates to be smaller than the corresponding IV estimates.[52]

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[51] See the Map in Figure 1.17 for details.

[52] Another reason why we would expect larger IV estimates lies in the migration of more individualistic migrants to the frontier, as documented by Bazzi et al. (2020). These likely were more open to new industries and more entrepreneurial in their traits, resulting in larger effects for the compliers with the frontier instrument.

While being larger than the OLS coefficient, the size of the IV coefficients is economically meaningful. The IV results suggest that increasing immigrant specialization in 1850 by 1% increases county-industry employment in 1910 by 0.11%, accounting for county-industry employment in 1850 and including industry and county fixed effects. Note that the first stage F-statistic is 41.58, indicating a sufficiently strong first stage.

The IV results confirm the "reversal" in specialization on the frontier. Columns 5 to 7 of Table 1.6 repeat the IV analysis using county-industry employment in 1850 as the dependent variable. For counties on the frontier between 1820 and 1840, the OLS relationship between county-industry employment in 1850 and immigrant specialization is significantly *negative*. Immigrants on the frontier did settle in counties where "their" industries had less employment by 1850. The IV results similarly show a larger negative estimate, albeit marginally not significant. The immigrants pushed out of their origins by climatic shocks did mostly not settle in counties to start manufacturing industries, especially as most of the affected regions only saw the rise of the relevant transport infrastructure and access to markets decades later after the Civil War. In sum, for the counties on the frontier, the early immigrants appear not to have selectively migrated to counties with inherent potential for "their" industries. Instead, their settlement was largely guided by the availability of unsettled land and aggregate arrivals from their origins. Only after 1850 did the counties on the frontier become specialized in manufacturing industries according to their immigrant's specialization by 1850.

1.7 Mechanism

Why is it that counties whose 1850 immigrants had a comparative advantage in specific industries specialized in these industries in the following decades, even though they did not settle to locations with inherent potential? Arthur (1990) showed theoretically that the "historical order of firm entry" matters. In this section, I provide evidence along this line. Immigrant specialization increases the probability that county-industries see the earlier entry of firms.

1.7.1 Increased Entry of Pioneer Firms in County-Industries

Here I document that county-industries with higher immigrant specialization have an increased likelihood of entry of "pioneer" firms from 1860 to 1930. Pioneer firms refers to the first firm entering in a particular industry and county. I proxy for the existence of at least one firm in a county-industry with the existence of at least one "owner/manager" (OCC1950 code 290) recorded in the full-count census. I define a dummy, $1(Owner^{t}_{d,i})$ indicating that at least one owner/manager of firm was present in county d, industry i, and year t and estimate variants of the following linear probability model:

$$1(Owner^{t}_{d,i}) = \beta ImmigrantSpecialization_{i,d} + \gamma[1(Owner^{1850}_{d,i})] + \mu_d + \mu_i + \varepsilon_{i,d}$$

where, whenever the outcome year is not 1850, I control for the presence of owners in a county-industry in 1850, and – in the baseline specification – include county and industry fixed effects and cluster standard errors on the county level.

County-industries with higher immigrant specialization in 1850 are more likely to witness the entry of pioneer firms from 1860 to 1930. Figure 1.10 depicts β for each decade, for both the full sample and the frontier sample. Pioneer firms are less present on the frontier in 1850 where immigrant specialization is high, and no more or less likely in the full sample. In 1860 and 1870, entry by pioneer firms is taking place in county-industries with lower immigrant specialization. Only from 1880 onward does immigrant specialization in 1850 induce the entry of pioneer firms there. This effect continues well into the twentieth century. Table 1.7 provides regression results for 1850 and 1880, as well as IV estimates supporting the conclusions herein.

Appendix section 1.10.9 provides further evidence on this mechanism. I show that immigrant specialization predicts the entry of pioneer firms owned or operated by both immigrants and natives. Immigrants arriving after 1850 are driving the effect on the frontier, but both natives born to immigrant and native parents are as well. This speaks to the importance of spillovers of immigrant specialization to natives. I turn to the county-industry-*origin* level to investigate further. There, I first document that immigrant specialization at this level increases pioneering activity by immigrants from these origins, compared to other immigrants and natives in the same county-industry. This level of analysis also enables me to inquire into the effects of aggregate immigrant specialization on other origins and natives. In 1850, immigrant specialization from a particular origin decreases the probability of pioneering activity by natives and immigrants from other origins. Later, positive spillovers on other immigrants and natives prevail.

1.7.2 Existing Establishments Larger and More Productive

Immigrant specialization also increases the size and productivity of companies recorded in the Census of Manufactures as early as 1860. This indicates the presence of a further channel distinct to that of county-industry pioneers. Apart from immigrants becoming entrepreneurs or inducing the entry of entrepreneurs by their presence, immigrants also provided a skilled, foreign-trained workforce benefiting already existing local firms. The data comes from Hornbeck and Rotemberg (2019), who digitize census tabulations for 1860, 1870 and 1880.[53] I calculate measures of establishment size and productivity at the county-industry level. I regress logarithms of these measures on immigrant specialization in the three decades separately, including county and industry fixed effects and clustering standard errors

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[53] I crosswalk their detailed disaggregated industries to the 49 manufacturing IPUMS industries used in this paper. Appendix section 1.10.8 provides detail on the crosswalk, the construction of the variables, and summary statistics of the data set.

at the county level. The regressions only include counties reporting at least one establishment in any industry.

Table 1.8 shows results. Columns 1 to 3 show the effect of immigrant specialization on the logarithm of workers divided by the number of establishments. In 1860, when sample coverage is largest, establishments are significantly larger where immigrant specialization is higher. The effect is sizable. A 1% increase in immigrant specialization increases firm size by 0.07%. In the following two decades the data set is comparatively smaller, as it is limited to by the increased minimum establishments cutoff (see Appendix for details). Only in 1880 is the association significant and positive again.[54] Columns 4 to 6 focus on productivity, measured as value added per worker. County-industries with higher immigrant specialization feature more productive establishments as early as 1860. Again the association is sizable and significant in the reduced sample of 1880, but not in that of 1870.

In sum, immigrant specialization in 1850 induces employment specialization in the following decades because new county-industries entered, and existing ones thrived.

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[54] Note that the reduced sample size in all three years results in a too small first stage F-statistic of the instrument employed before.

1.8 Persistence

The preceding sections showed that immigrant specialization increased county-industry employment in the early decades of the twentieth century. While since then manufacturing as a whole dispersed and tended to move to the sunbelt states, the region now known as the "rust belt" remains a center of US manufacturing.[55] Kim (1998) shows that specialization in manufacturing industries across states was lower in 1987 than ever before. Yet, manufacturing industries remain highly localized (Ellison and Glaeser, 1997; Holmes and Stevens, 2004). Anecdotal evidence has it that they often remain in the same region.[56] Take the auto industry. While new plant openings all over the country have dispersed the industry, Michigan still is the state with the highest employment in it. Figure 1.11 shows that county-industry employment in 1910 is strongly associated with county-industry persistence in 1910. In this section, I show that immigrant specialization in 1850 predicts employment patterns in then-novel industries until the present.

I use data on US county-industry employment in 2000 from Eckert et al. (2020a). They impute the non-disclosed cells by exploiting adding-up constraints in the released County Business Patterns data set of the Census bureau.[57] Column 1 of Table 1.9 shows that immigrant specialization in 1850 is not significantly associated with county-industry employment in 2000 across all industries. Column 2 shows that immigrant specialization only is a significant predictor of modern county-industry employment patterns for the industries that were novel as the

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[55] As Krugman (1991a) puts it, the "manufacturing belt took shape in the second half of the nineteenth century, and proved remarkably persistent. Harvey Perloff (1960) estimated that as late as 1957, the manufacturing belt still contained 64 percent of US manufacturing employment-only slightly reduced from its 74 percent share at the turn of the century." Holmes and Stevens (2004) show that by 1999, the manufacturing belt still boasted almost 50% of the nation's manufacturing employment.

[56] In the words of Marshall (1890): "When an industry has chosen a locality for itself it is likely to stay there for long"

[57] I crosswalk the NAICS 1997 industry classification used in there from NAICS to IPUMS' IND2000, using the information provided in `https://usa.ipums.org/usa/volii/indcross.shtml` and then from IND2000 to IND1950, relying on `https://usa.ipums.org/usa/volii/occ_ind.shtml`. Details are provided in Appendix 1.10.10.

immigrants arrived. For these, the association is sizable and highly significant. Column 3 restricts the sample to frontier counties only. There the association is particularly sizable: it is almost half of the coefficient on 1850 county-industry employment. With the initial location of novel industries determined, agglomeration and co-agglomeration forces set in, and locked county-industry specialization for decades. The potential for immigrants to lock-in county-industries was far greater on counties on the frontier which only industrialized decades after their arrival, and in industries in which the spatial distribution of economic activity was not yet determined. Importantly, on the frontier the early immigrants did not settle in counties with a inherent comparative advantage for these industries. [58]

1.9 Conclusion

I show that immigrants who arrived in the United States during the Age of Mass Migration from 1850 to 1920 influenced their destination counties' employment patterns in manufacturing. My results build on a novel measure based on immigrant populations at the US county level in 1850 and the origin countries' specialization in manufacturing industries engaged in trade. This measure captures the industry-specific skills, experience, exposure, and knowledge embodied in the European immigrants of each US county (both those present in 1850 and those arriving later), depending on their country of origin. It predicts county-industry

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[58] Note that this analysis does not allow me to speak to whether immigrant specialization remained a source of comparative advantage even in 2000. Burchardi et al. (2019) for instance show that US counties ancestry causally affect their foreign direct investment flows. Thus, immigrant origins likely affect current employment patterns after the initial equilibrium-selecting effect emphasized in this paper. Until World War I and the subsequent changes to US immigration policy, such an effect likely was a combination of a migration (embodied knowledge transfer) and an information channel. After that, migration from European origins was strongly reduced, such that the information channel emphasized in Burchardi et al. (2019) likely was more important. Further, the United States emerged as a technological leader in most manufacturing industries by then, all with established spatial centers in the country, leaving less possibility for European skills and knowledge to affect industries' location.

employment patterns in the subsequent decades and is not driven by the initial selective migration of immigrants. I provide evidence for the various channels at work. Most importantly, in counties receiving more immigrants whose countries of origin were specialized in a particular industry, immigrants set up shop early on.

These findings are in line with the theoretical work of Arthur (1990), and the wealth of case studies and observations emphasizing the importance of an accidental early start in making a particular location a center of employment for a particular industry.

The findings presented in this paper are evidence of only one of many potential determinants of such accidental early starts. Other settings might uncover many other determinants. The long-term ramifications of such accidental early starts for individual cities, regions, and countries make an understanding of them in different contexts, periods, and regions essential to local and national industrial policy. This paper's empirical nature does not enable me to consider the local or aggregate welfare gains resulting from those chance decisions, or to suggest policies that would enable locations to reap such gains. Gauging these in a theoretical framework, that allows for the interaction of different industries across space, is left to future research.

FIGURES

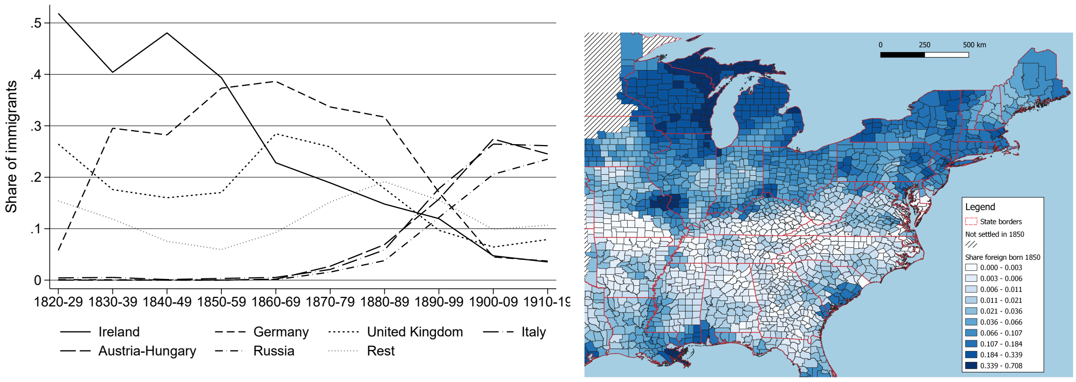


Figure 1.1: Immigrant Origins (left) and their Spatial Distribution in 1850 (rights)

Note: The left panel shows the share of immigrants by their origin country for each decade from 1820 to 1919. The data comes from Office of Immigration Statistics (2013), and "Rest" is the share of immigrants from all other origins. The right panel shows the share of individuals of foreign birth across in US counties in 1850. The population data comes from the IPUMS full-count decennial censuses (Ruggles et al., 2019). The spatial units used are 2010 counties, and the historical variables have been re-aggregated to this level using the procedure of Eckert et al. (2020b).

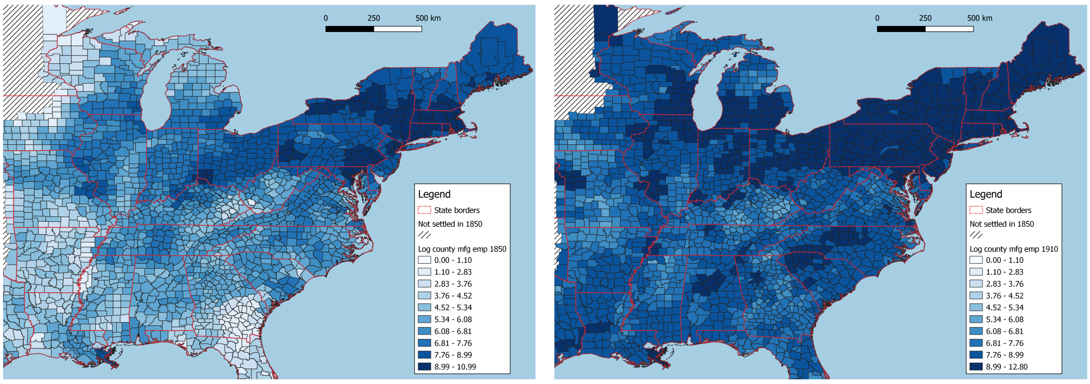


Figure 1.2: Individuals Employed in Manufacturing in 1850 (left) and 1910 (right)

Note: These Maps show the distribution of manufacturing workers across US counties in 1850 (left) and 1910 (right). The data comes from the IPUMS full-count decennial censuses Ruggles et al. (2019), particularly from the individuals recorded as working in manufacturing (IND1950 industries 300 to 500). The variable displayed is the logarithm of the individuals recorded as working in these industries. The spatial units used are 2010 counties, and the historical variables have been re-aggregated to this level using the procedure of Eckert et al. (2020b).

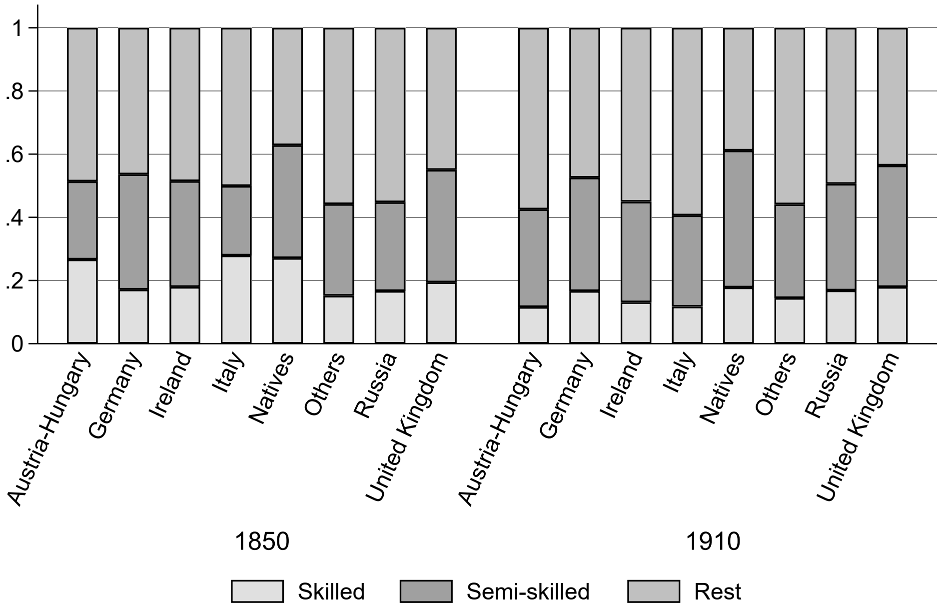


Figure 1.3: Manufacturing Workers by Skill Group and Country of Birth

Note: This Figure is based on the 1850 and 1910 full-count census. I identify all manufacturing workers (IND1950 codes 300 to 500) and define as "skilled" those working in professional and technical occupations (OCC1950 codes 1-99) and managerial ones (200-290). Semi-skilled contain clerical occupations, salesworkers and craftsmen (300-595). All others, including operatives are included in the "Rest" category.

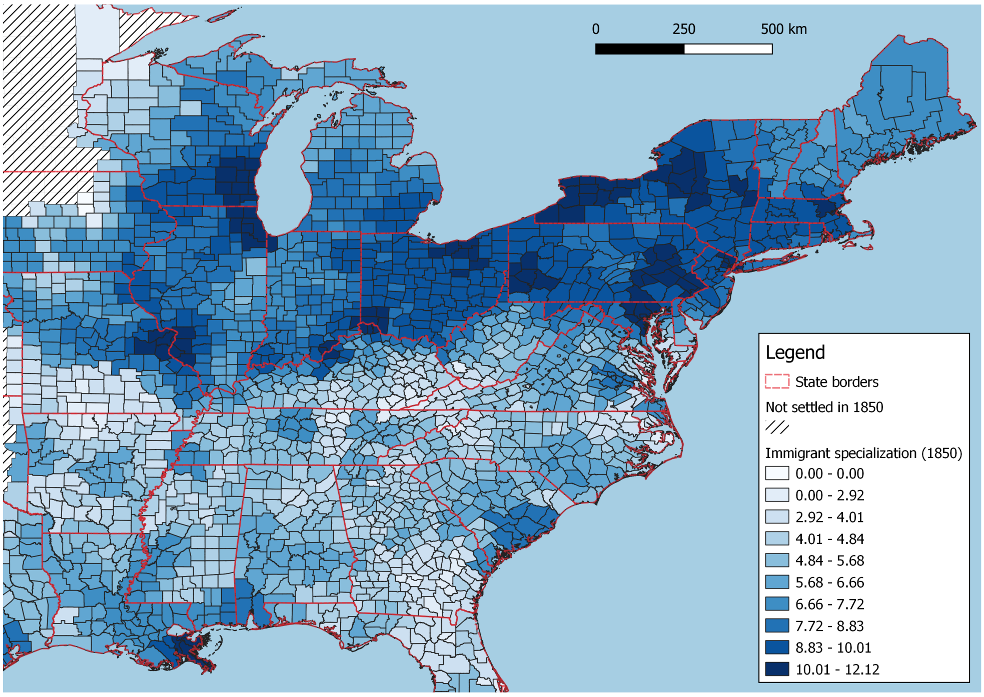


Figure 1.4: Spatial Variation of Immigrant Specialization in ‘Drugs and Medicines” across US counties

Note: This Map shows the distribution of immigrant specialization in “drugs and medicine” manufacturing (IND1950 code 467) across US counties in 1850. This measure, its construction and underlying data sources are described in the text. The spatial units used are 2010 counties, and the historical variables have been re-aggregated to this level using the procedure of Eckert et al. (2020b).

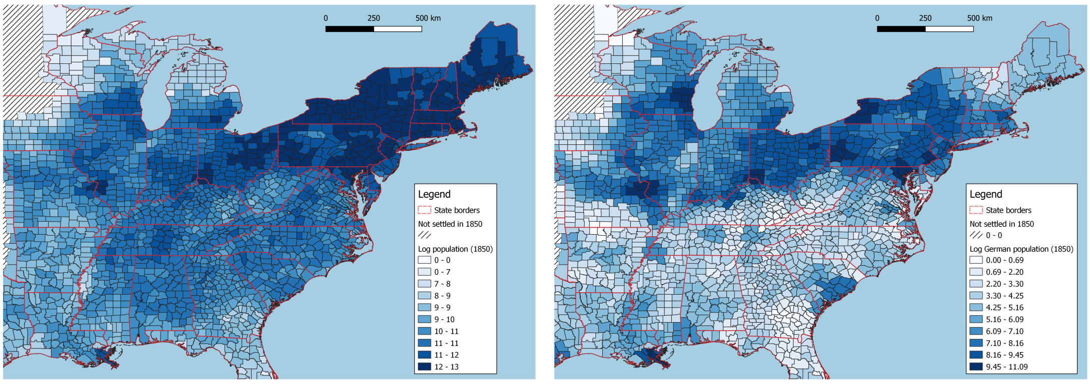


Figure 1.5: Total Population (left) and German-born Population (right) across US Counties

Note: These Maps show the distribution of total population and German-born population across US counties in 1850. The population data comes from the IPUMS full-count decennial censuses Ruggles et al. (2019). The spatial units used are 2010 counties, and the historical variables have been re-aggregated to this level using the procedure of Eckert et al. (2020b).

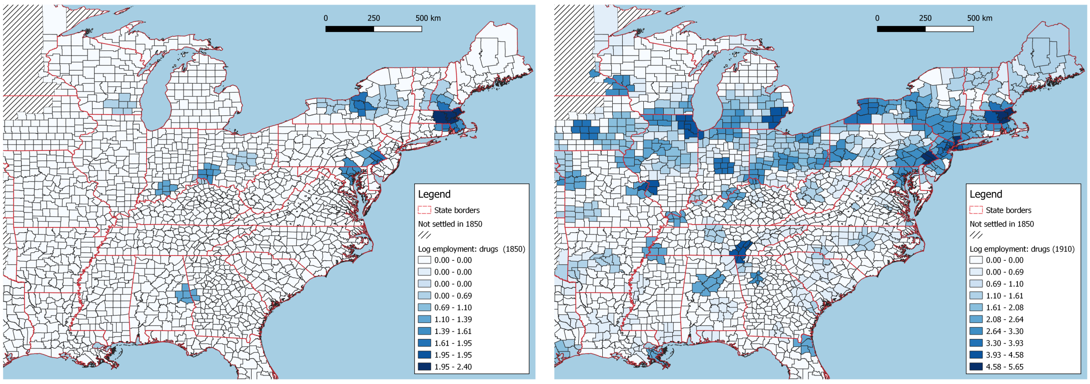


Figure 1.6: County-level Employment in "Drug and Medicine" Manufacturing, 1850 to 1910

Note: These Maps show the distribution of employment in "drugs and medicine" manufacturing (IND1950 code 467) across US counties in 1850 and 1910. The data comes from the full-count of US decennial census (Ruggles et al., 2019). This measure, its construction and underlying data sources are described in the text. The spatial units used are 2010 counties, and the historical variables have been re-aggregated to this level using the procedure of Eckert et al. (2020b).

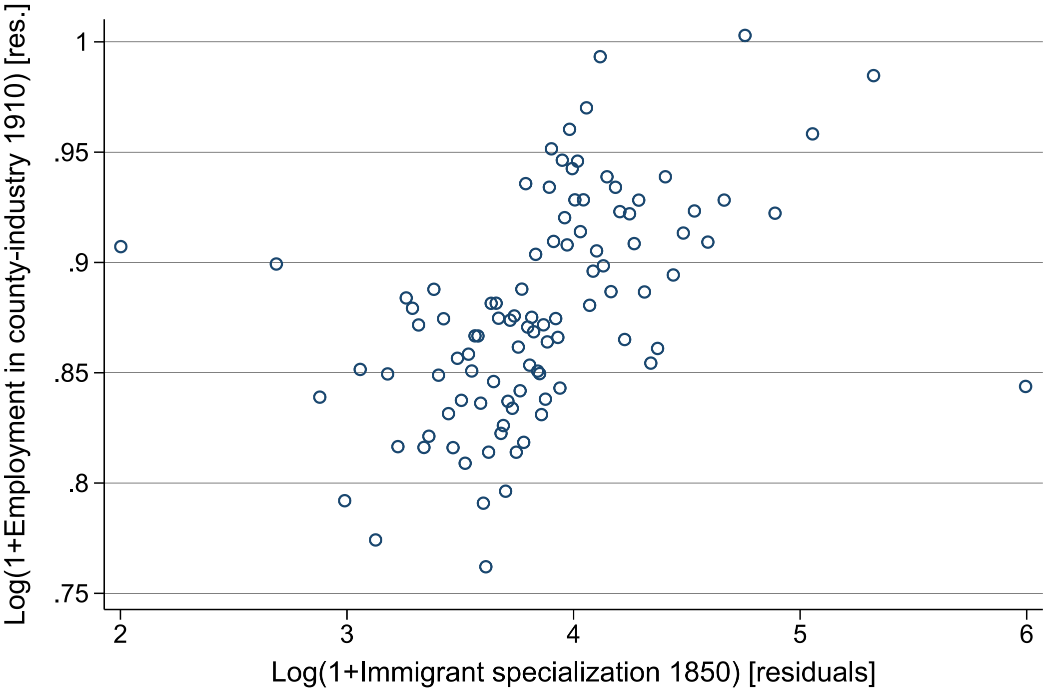


Figure 1.7: Immigrant Specialization and County-industry Employment in 1910

Note: This bin scatter shows the association between the immigrant specialization and county-industry employment in 1910. Both of these measures are transformed with the logarithm of one plus the depicted variables. Immigrant specialization is the sum of immigrants residing in each US county in 1850 weighted by their origins comparative advantage revealed in US import data in 1909. The binscatter shows the association after controlling for county-industry employment in 1850 and including county and industry fixed effects.

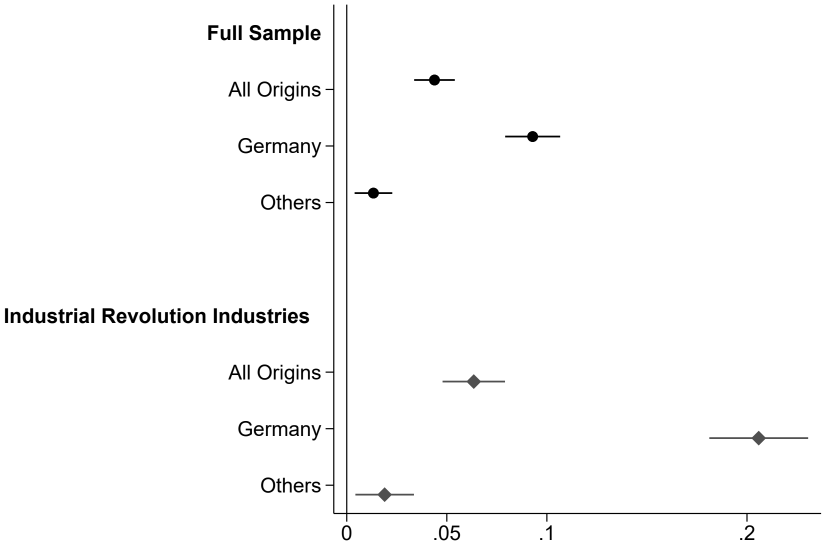


Figure 1.8: Baseline Association Stronger in Novel Industries, for German immigrants

Note: This figure plots coefficients of regressions of immigrant specialization on county-industry employment in 1910, controlling for 1850 county-industry employment in 1850, and including county and industry fixed effects. All variables enter estimation as the logarithm of plus the variable. Immigrant Specialization (Germany) is the number of German immigrants in 1850 in each county, multiplied by Germany's comparative advantage in specific industries. "Industrial Revolution industries" are those manufacturing industries associated with either the First or Second Industrial Revolution and listed in table 1.13.

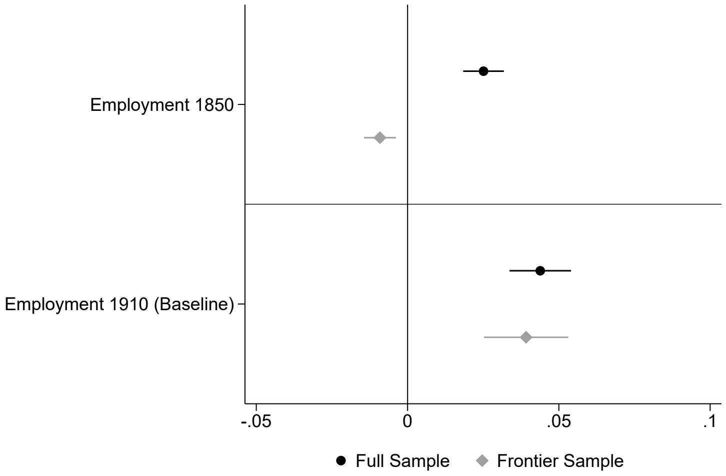


Figure 1.9: Reversal of Employment Pattern in the Frontier of Settlement

Note: This figure shows coefficients of regressions of immigrant specialization on county-industry employment in 1850 and 1910. Black markers indicate coefficients from the full sample, grey ones are coefficients from the sample of counties settled between 1820 and 1850 (Bazzi et al., 2020). All regressions include county and industry fixed effects, and the baseline regression with county-industry employment in 1910 as the outcome further controls for county-industry employment in 1850.

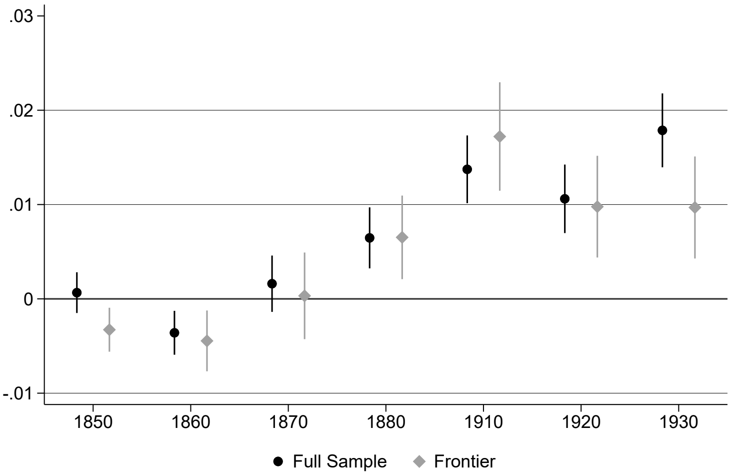


Figure 1.10: Entry of Pioneer Firms in County-Industries

Note: This figure shows coefficients of regressions of immigrant specialization on county-industry employment in 1850 and 1910. Black markers indicate coefficients from the full sample, grey ones are coefficients from the sample of counties settled between 1820 and 1850. All regressions include county and industry fixed effects, and the baseline regression with county-industry employment in 1910 as the outcome further controls for county-industry employment in 1850.

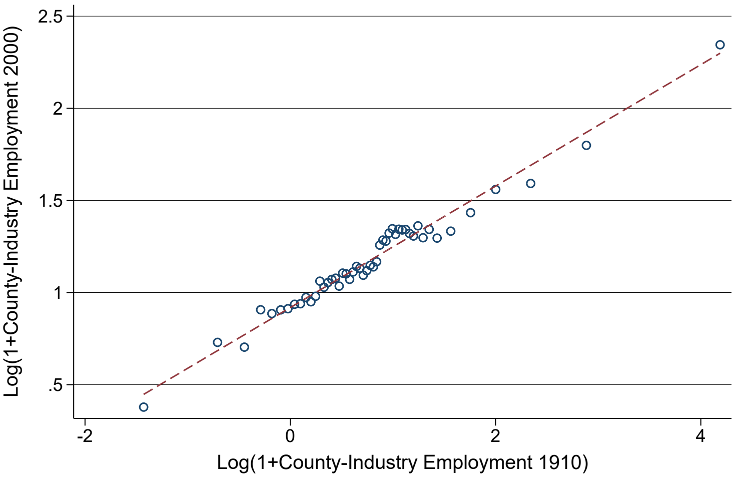


Figure 1.11: County-industry Employment in 2000 and 1910

Note: This bin scatter shows the association between county-industry employment in 1910 and 2000. Both of these measures are transformed with the logarithm of one plus the depicted variables. The bin scatter shows the association after controlling for county-industry employment in 1850 and including county and industry fixed effects.

TABLES

Table 1.1: Baseline Results: Immigrant Specialization and County-industry Employment

Dep. Var.: Log(1+Employment$^{1910}_{d,i}$)

	(1)	(2)	(3)	(4)	(5)
Note:			Industry FE	County-level controls†	County FE
Log(1+Immigrant Specialization$^{1850}_{d,i}$)	0.211***	0.101***	0.155***	0.100***	0.044***
	(0.009)	(0.006)	(0.007)	(0.011)	(0.005)
Log(1+Employment$^{1850}_{d,i}$)		0.714***	0.584***	0.489***	0.374***
		(0.011)	(0.016)	(0.011)	(0.008)
Industry FE			✓	✓	✓
County FE					✓
R^2	0.118	0.283	0.465	0.487	0.630
R^2 (within)	0.118	0.283	0.301	0.330	0.061
Observations	113239	113239	113239	113239	113239

Note: This table shows that immigrant specialization in 1850 is positively associated with employment patterns across US counties and manufacturing industries. All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods. Columns from 3 onward include industry fixed effects, and column 5 additionally includes county fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

† Controls for (log transformations of) total population, total county employment in manufacturing industries,and total foreign-born population, as well as a Hirsch-Herfindal measure of birthplace diversity in 1850 and the county population shares of all immigrant origins used in the of the measure of immigrant specialization .

Table 1.2: Baseline Association Increases with Later Outcomes

Dep. Var.: Log(1+Employment$^{t}_{d,i}$), Year t as Indicated in Table Header

	(1)	(2)	(3)	(4)	(5)	(6)
Year:	1860	1870	1880	1910	1920	1930
Log(1+Immigrant Specialization$^{1850}_{d,i}$)	0.010***	0.001	0.011***	0.044***	0.059***	0.073***
	(0.002)	(0.003)	(0.004)	(0.005)	(0.006)	(0.006)
Log(1+Employment$^{1850}_{d,i}$)	0.754***	0.727***	0.684***	0.374***	0.317***	0.253***
	(0.006)	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)
R^2	0.813	0.759	0.739	0.630	0.605	0.631
R^2 (within)	0.540	0.398	0.297	0.061	0.039	0.026
Observations	113239	113239	113239	113239	113239	113239

Note: The table shows that the importance of immigrant specialization in 1850 for county-industry employment patterns increased over time. All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods. All variables are transformed using the logarithm of one plus the variable. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.4: Robustness – Accounting for Market Access and Natural Advantage

Dep. Var.: Log(1+Employment$^{1910}_{d,i}$)

	(1)	(2)	(3)	(4)
Log(1+Immigrant Specialization$^{1850}_{d,i}$)	0.044***	0.050***	0.042***	0.049***
	(0.005)	(0.005)	(0.005)	(0.006)
Log(1+Employment$^{1850}_{d,i}$)	0.374***	0.340***	0.297***	0.320***
	(0.008)	(0.010)	(0.009)	(0.011)
Industry FE × Market Size 1850†		✓	✓	✓
Industry FE × County Characteristics‡			✓	✓
County FE × Industry Characteristics §				✓
R^2	0.630	0.648	0.668	0.708
Observations	113239	113239	113092	106168

Note: The table documents that the baseline association is robust to accounting for market access and natural advantages as alternative determinants of county-industry employment patterns. All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.
† Market potential is calculated using 1850 county population data and geodesic distances using the approach pioneered by Harris (1954). Its logarithm is used.
‡ County characteristics include a dummy for the presence of railroad, a river or canal, oil or coal reserves, and the share of manufacturing workers that were skilled or semi-skilled.
§ Industry characteristics (at the SIC industry level) come from Klein and Crafts (2011) and include intermediate input use, sales to industry, agricultural input use, and mineral resources use. Further, I calculate the share of skilled or semi-skilled workers from the full-count census.

Table 1.3: Heterogeneity – Stronger in Novel Industries and for German Immigrants

Dep. Var.: Log(1+Employment$_{d,i}$)^{1910}

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Full Sample			Industrial Revolution Industries		
Log(1+Imm. Spec.$^{1850}_{d,i}$)	0.044***			0.063***		
	(0.005)			(0.008)		
Log(1+Imm. Spec. (Germany)$^{1850}_{d,i}$)		0.093***			0.206***	
		(0.007)			(0.013)	
Log(1+Imm. Spec. (Others)$^{1850}_{d,i}$)			0.013***			0.019**
			(0.005)			(0.007)
Log(1+Employment$^{1850}_{d,i}$)	0.374***	0.373***	0.374***	0.480***	0.480***	0.482***
	(0.008)	(0.008)	(0.008)	(0.012)	(0.012)	(0.012)
R^2	0.630	0.631	0.630	0.627	0.630	0.626
R^2 (within)	0.061	0.062	0.060	0.090	0.097	0.088
Observations	113239	113239	113239	48531	48531	48531

Note: This table shows heterogeneity of the baseline result. Immigrant specialization of German immigrants is particularly important, and especially so in then-novel industries. All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods. Columns 4 to 6 further restrict the sample to the industries associated with the industrial revolution. All variables are transformed using the logarithm of one plus the variable. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.5: Selective Migration – Reversal on Frontier

Dep. Var.: Log(1+Employment$^{t}_{d,i}$), Year t as Indicated in Table Header

	(1)	(2)	(3)	(4)
	Full Sample		*Frontier Sample*	
Dep. Var. Year t .	1850	1910	1850	1910
Log(1+Immigrant Specialization$^{1850}_{d,i}$)	0.025***	0.044***	-0.009***	0.039***
	(0.003)	(0.005)	(0.003)	(0.007)
Log(1+Employment$^{1850}_{d,i}$)		0.374***		0.270***
		(0.008)		(0.012)
R^2	0.563	0.630	0.535	0.604
Observations	113239	113239	51205	51205

Note: This table documents that on the frontier of settlement, immigrant settled in counties that by 1850 were less specialized in “their” industries. This pattern reverses over the course of the following decades. All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 (columns 1-2) and the counties which where at the frontier at least once between 1820 and 1840 (columns 3-4) , as well as all industries with corresponding traded goods in 1909. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.6: Selective Migration – IV Results

Dependent Variable as Indicated in Table Header

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.	Log(1+Imm.Spec.$_{d,i}^{1850}$)	Log(1+Employment$_{d,i}^{1910}$)			Log(1+Employment$_{d,i}^{1850}$)		
Stage	First	OLS	RF	IV	OLS	RF	IV
Log(1+$\widehat{\text{Imm.Spec.}}_{d,i}^{1910}$)	0.425^{***} (0.066)		0.047^{**} (0.024)			-0.020 (0.013)	
Log(1+Imm. Spec.$_{d,i}^{1850}$)		0.035^{***} (0.006)		0.112^{*} (0.057)	-0.009^{***} (0.003)		-0.048 (0.031)
Log(1+Employment$_{d,i}^{1850}$)	-0.010^{***} (0.003)	0.279^{***} (0.012)	0.278^{***} (0.012)	0.279^{***} (0.012)			
First Stage F-statistic				41.58			41.60
R^2	0.949	0.602	0.602		0.533	0.533	
Observations	52250	52250	52250	52250	52250	52250	52250

Note: This table shows that the baseline association is not driven by selective migration to inherent industry potential. The instrument is constructed with county-origin populations based on aggregate arrivals by European origin and the movement of the "frontier" of settlement across counties from 1820 to 1840. All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 which where at the frontier at least once between 1820 and 1840 and all industries with corresponding traded goods in 1909. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.7: Mechanism – Early Entry of County-Industries

Dep. Var.: Dummy for at least one owner/manager in County-industry, Year indicated in Header

	(1)	(2)	(3)	(4)	(5)	(6)
Year outcome	1850	1910	1850	1910	1850	1910
Sample	Full		Frontier			
Estimation	OLS				IV	
Log(1+Immigrant Specialization$_{d,i}^{1850}$)	-0.002 (0.002)	0.011^{***} (0.002)	-0.009^{***} (0.002)	0.016^{***} (0.003)	-0.040 (0.031)	0.238^{***} (0.048)
1(Owner$_{d,i}^{1850}$)		0.636^{***} (0.009)		0.709^{***} (0.013)		0.715^{***} (0.013)
R^2	0.262	0.432	0.254	0.451		
Observations	115550	115550	52250	52250	52250	52250

Note: This table shows that immigrant specialization induces the entry of companies in county-industries after 1850. All regressions are run at the county-industry level. The dependent variables is a dummy indicating whether at least one owner/manager in a county-industry resided in a county (IPUMS occupation code 290). The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods in 1909. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.8: Mechanism – Existing County-industries And Immigrant Specialization

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.:	Log(Workers/Establishment)			Log(Value Added/Worker)		
Year Dep. Var.:	1860	1870	1880	1860	1870	1880
Log(1+Immigrant Specialization$^{1850}_{d,i}$)	0.071***	-0.029	0.107***	0.083***	-0.206	0.442***
	(0.009)	(0.023)	(0.035)	(0.029)	(0.127)	(0.163)
R^2	0.368	0.426	0.485	0.554	0.672	0.715
R^2 (within)	0.002	0.000	0.003	0.001	0.001	0.004
Observations	33167	8366	3938	15539	4570	2376

Note: This table shows that as early as 1860 establishments are larger and more productive where immigrant specialization is higher. All regressions are run at the county-industry level. The dependent variables are proxies for establishment size and productivity in county-industries in 1860, 1870 and 1880, digitized from the Census of Manufactures by Hornbeck and Rotemberg (2019). The sample includes all counties with non-zero population in 1850, all industries with corresponding traded goods, and all county-industries with at least one establishment recorded in the Census of Manufactures in the corresponding year. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.9: Persistence — Immigrant Specialization And County-industry Employment in 2000

Dep. Var.: Log(1+Employment$^{2000}_{d,i}$)

	(1)	(2)	(3)
Sample	Full		Frontier
Log(1+Immigrant Specialization$^{1850}_{d,i}$)	0.006	-0.004	-0.002
	(0.010)	(0.010)	(0.014)
Industrial Revolution Industry × Log(1+Immigrant Specialization$^{1850}_{d,i}$)		0.017***	0.027***
		(0.004)	(0.007)
Log(1+Employment$^{1850}_{d,i}$)	0.083***	0.085***	0.057***
	(0.009)	(0.009)	(0.016)
R^2	0.484	0.484	0.451
Observations	113239	113239	51205

Note: All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850, and all industries with corresponding traded goods in 1909. In column 3, the sample consists only of those counties on the frontier of settlement from 1820 to 1840. All columns include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

1.10 Appendix to Chapter 1

1.10.1 Additional Tables

Table 1.10: Immigrant origins in the 1850 U.S. Decennial Census

Birthplace	Population	County population share				Counties present:
		Mean	Median	Minimum	Maximum	
United States	119,000,000.00	0.901	0.964	0.292	0.999	2311
Ireland*	5,468,662.00	0.023	0.010	0.000	0.253	2298
Germany*	3,836,062.00	0.028	0.008	0.000	0.394	2290
United Kingdom*	2,358,209.00	0.014	0.005	0.000	0.381	2305
Canada	910,319.00	0.009	0.001	0.000	0.342	2175
France*	342,506.00	0.004	0.001	0.000	0.079	2126
Mexico	259,402.00	0.028	0.000	0.000	0.320	1211
Sweden and Norway*	114,856.00	0.003	0.000	0.000	0.152	1498
Switzerland*	94,164.00	0.001	0.000	0.000	0.042	1660
Netherlands*	71,819.00	0.002	0.000	0.000	0.125	1461
West Indies	31,207.00	0.002	0.000	0.000	0.281	1486
Italy*	22,809.00	0.001	0.000	0.000	0.008	1330
Spain*	15,377.00	0.001	0.000	0.000	0.012	997
Poland	14,770.00	0.000	0.000	0.000	0.005	1439
South America	11,776.00	0.001	0.000	0.000	0.027	585
Abroad (unknown) or at sea	11,386.00	0.000	0.000	0.000	0.002	1280
Denmark*	10,871.00	0.000	0.000	0.000	0.007	1290
Belgium*	9,102.00	0.000	0.000	0.000	0.007	904
Portugal*	8,714.00	0.000	0.000	0.000	0.007	820
Austria-Hungary*	8,016.00	0.000	0.000	0.000	0.009	1098
Europe (not specified)	6,402.00	0.000	0.000	0.000	0.003	970
Russia	5,564.00	0.000	0.000	0.000	0.002	757
Cuba	5,379.00	0.000	0.000	0.000	0.003	552
China	4,791.00	0.001	0.000	0.000	0.024	178
Africa	2,824.00	0.000	0.000	0.000	0.002	583

Note: This Table shows summary statistics on the population by origin in the 1850 U.S. Decennial census (Ruggles et al., 2019). Column one shows total population by origin, columns three to five show mean, median, minimum and maximum of population by origin across counties. Column six shows the number of counties in individuals from a particular origin were residing in 1850. Origins with a * are the European origins listed in U.S. import data in 1909.

Table 1.11: Trade data and revealed comparative advantage in "Drugs and Medicines"

Trade partner	US Imports (1909), in thousand $		RCA
	Drugs and Medicines	Total	
Austria-Hungary	1226.353	15433.96	2.50
Germany	8690.558	143497.4	1.90
Netherlands	792.879	26083.52	0.956
United Kingdom	5682.615	191480.6	0.93
France	2886.705	108375.4	0.84
Spain	324.536	14076.99	0.72
Belgium	580.882	27392.84	0.67
Italy	918.501	49287.95	0.59
Switzerland	320.64	23814.12	0.42
Sweden and Norway	69.651	9032.308	0.24
Denmark	11.631	1625.396	0.22
Ireland	20.822	16881.22	0.04
Portugal	6.043	6240.562	0.03

Note: This Table shows U.S. imports by trade partner in 1909. Data comes from Treasury (1909). Column 1 shows the total imports in in "drugs and medicine manufacturing", column 2 shows the total imports from each origin. Column 3 shows the revealed comparative advantage calculated from the preceding columns and total U.S. imports.

Table 1.12: Summary statistics

Variable	Obs	Mean	Std. Dev.
Immigrant Specialization$_{d,i}^{1850}$	113239	1486.083	29344.4
Log(1+Immigrant Specialization$_{d,i}^{1850}$)	113239	3.854	2.467
Log(1+Employment$_{d,i}^{1850}$)	113239	.393	.94
Log(1+Employment$_{d,i}^{1880}$)	113239	.679	1.278
Log(1+Employment$_{d,i}^{1910}$)	113239	.878	1.513
1(Owners$_{d,i}^{1850}$)	113239	.077	.266
1(Owners$_{d,i}^{1910}$)	113239	.356	.479

Note: This Table shows summary statistics of the main dependent and independent variables used in the analysis. The level of observation is county-industries. The data set covers 49 manufacturing industries and 2,311 counties.

Table 1.13: Manufacturing industries in IPUMS

IND1950 Code	Industry Name	SIC Code	Ind. Rev.	Traded in 1909	1851
306	Logging				
307	Sawmills, planing mills, and mill work	24		X	X
308	Miscellaneous wood products	24		X	X
309	Furniture and fixtures	25		X	X
316	Glass and glass products	32		X	X
317	Cement, concrete, gypsum and plaster products	32		X	X
318	Structural clay products	32			
319	Pottery and related products	32		X	
326	Miscellaneous nonmetallic mineral and stone products	32		X	X
336	Blast furnaces, steel works, and rolling mills	33	X	X	X
337	Other primary iron and steel industries	33	X	X	X

Table 1.13: Manufacturing industries in IPUMS

IND1950	Industry Name	SIC Code	Ind. Rev.	Traded in	
Code				1909	1851
338	Primary nonferrous industries	33	X	X	X
346	Fabricated steel products	34	X	X	X
347	Fabricated nonferrous metal products	34	X	X	X
348	Not specified metal industries	34	X	X	
356	Agricultural machinery and tractors	35	X		
357	Office and store machines and devices	38			
358	Miscellaneous machinery	35	X	X	
367	Electrical machinery, equipment, and supplies		X	X	
376	Motor vehicles and motor vehicle equipment	37	X	X	
377	Aircraft and parts	37	X		
378	Ship and boat building and repairing	37	X	X	X
379	Railroad and miscellaneous transportation equipment	37	X	X	
386	Professional equipment and supplies	28		X	
387	Photographic equipment and supplies		X	X	
388	Watches, clocks, and clockwork-operated devices	38		X	X
399	Miscellaneous manufacturing industries	39		X	X
406	Meat products	20		X	X
407	Dairy products	20		X	X

Table 1.13: Manufacturing industries in IPUMS

IND1950 Code	Industry Name	SIC Code	Ind. Rev.	Traded in	
				1909	1851
408	Canning and preserving fruits, vegetables, and seafoods	20		X	X
409	Grain-mill products	20		X	X
416	Bakery products	20		X	
417	Confectionery and related products	20		X	X
418	Beverage industries	20		X	X
419	Miscellaneous food preparations and kindred products	20		X	X
426	Not specified food industries	20			
429	Tobacco manufactures	21		X	X
436	Knitting mills	22		X	X
437	Dyeing and finishing textiles, except knit goods	22		X	X
438	Carpets, rugs, and other floor coverings	22		X	X
439	Yarn, thread, and fabric mills	22	X	X	X
446	Miscellaneous textile mill products	22	X	X	X
448	Apparel and accessories	23	X	X	X
449	Miscellaneous fabricated textile products	23	X	X	
456	Pulp, paper, and paperboard mills	26		X	X
457	Paperboard containers and boxes	26			X
458	Miscellaneous paper and pulp products	26		X	X

Table 1.13: Manufacturing industries in IPUMS

IND1950 Code	Industry Name	SIC Code	Ind. Rev.	Traded in 1909	1851
459	Printing, publishing, and allied industries	27		X	X
466	Synthetic fibers		X		
467	Drugs and medicines	28	X	X	X
468	Paints, varnishes, and related products	28		X	X
469	Miscellaneous chemicals and allied products	28	X	X	X
476	Petroleum refining	29	X	X	
477	Miscellaneous petroleum and coal products	29	X	X	
478	Rubber products	30	X	X	
487	Leather: tanned, curried, and finished	31		X	X
488	Footwear, except rubber	31			X
489	Leather products, except footwear	31		X	X
499	Not specified manufacturing industries	39			

Note: This Table lists the industries used in the analysis. Column 1 shows their IND1950 code, column 2 their name. Column 3 is the corresponding SIC industry where available (Klein and Crafts, 2011). Column 4 indicates whether an industry is considered to have been affected by the First or Second or Industrial Revolution. Column 5 and 6 indicate industries for which corresponding goods are imported into the U.S. in 1909 and 1851.

Table 1.14: Baseline Heterogeneity by Industrial Revolution industry

Dep. Var.: Log(1+Employment$^{t}_{d,i}$), Year t indicated in Table Header

Year:	1880	1910		1930	
	(1)	(2)	(3)	(4)	(5)
Other industries × Log(1+Immigrant Specialization$^{1850}_{d,i}$)	0.020***	0.025***	0.014***	0.036***	0.027***
	(0.004)	(0.005)	(0.005)	(0.007)	(0.006)
Industrial Revolution industries × Log(1+Immigrant Specialization$^{1850}_{d,i}$)	0.005	0.056***	0.053***	0.098***	0.095***
	(0.004)	(0.006)	(0.005)	(0.007)	(0.007)
Log(1+Employment$^{1850}_{d,i}$)	0.682***	0.377***	0.002	0.259***	-0.029***
	(0.007)	(0.008)	(0.007)	(0.008)	(0.008)
Log(1+Employment$^{1880}_{d,i}$)			0.549***		0.422***
			(0.007)		(0.008)
R^2	0.739	0.631	0.687	0.633	0.661
Observations	113239	113239	113239	113239	113239

Note: All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods. All variables are transformed using the logarithm of one plus the variable. All regressions include county and industry fixed effects. "Industrial Revolution industries", and "Other industries" are industry dummies indicating whether an industry is associated with Industrial Revolutions or not. Details on this correspondence are provided in Table 1.13 and the text. Standard errors clustered at the county-level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

1.10.2 Additional Figures

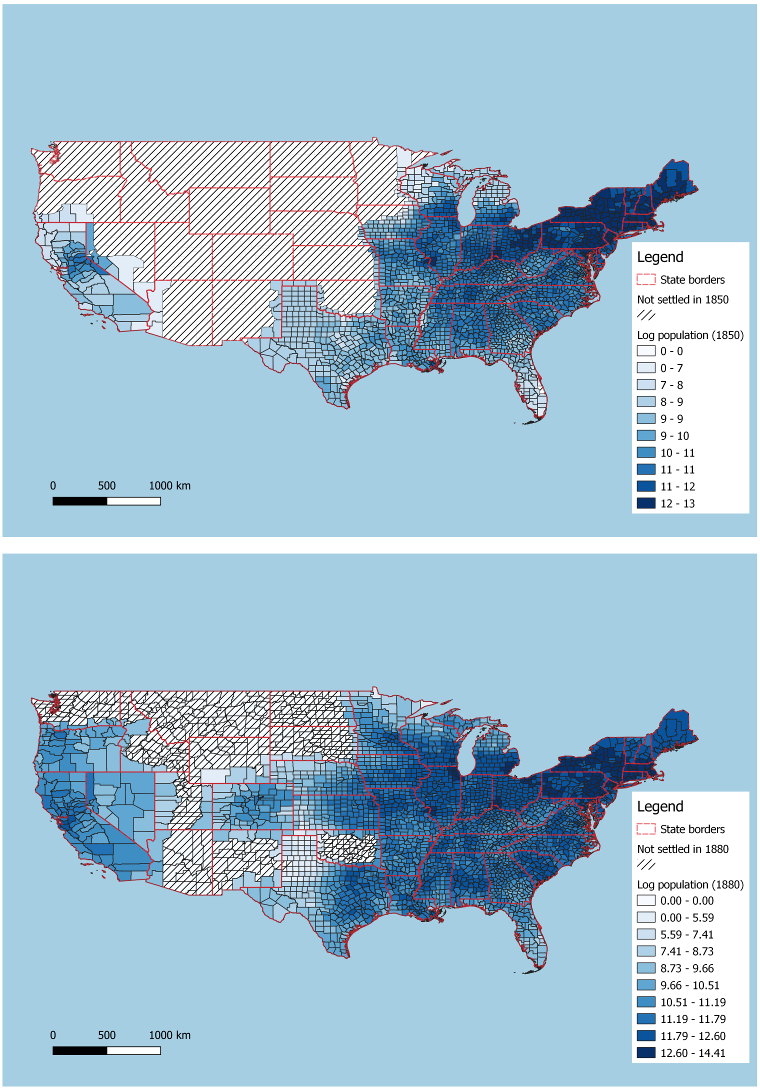


Figure 1.12: Total population in 1850 (upper) and 1880 (lower) of U.S. counties

Note: These maps show the distribution of population across U.S. counties in 1850 and 1880. The population data comes from the IPUMS full count decennial censuses Ruggles et al. (2019). The spatial units used are 2010 counties, and the historical variables have been re-aggregated to this level using the procedure of Eckert et al. (2020b).

1.10.3 Details on US Import Data

In Section 1.4.1, I describe the construction of comparative advantage based on U.S. import data. Here I detail the underlying data sources, describe how I aggregate the data in these to the origin country - manufacturing industry level, and finally provide some stylized facts of specialization in manufacturing across European origin countries and over time.

1.10.3.1 Data Sources

I digitize imports to the U.S. by traded good category and exporter. This data comes from in reports of the Bureau of Statistics of the U.S. Treasury Department in the years 1851 (Treasury, 1851), 1881 (Treasury, 1881) and 1909 (Treasury, 1909). Table 1.15 shows the digitized page ranges for all three reports. Figure 1.13 shows an example of a digitized page.

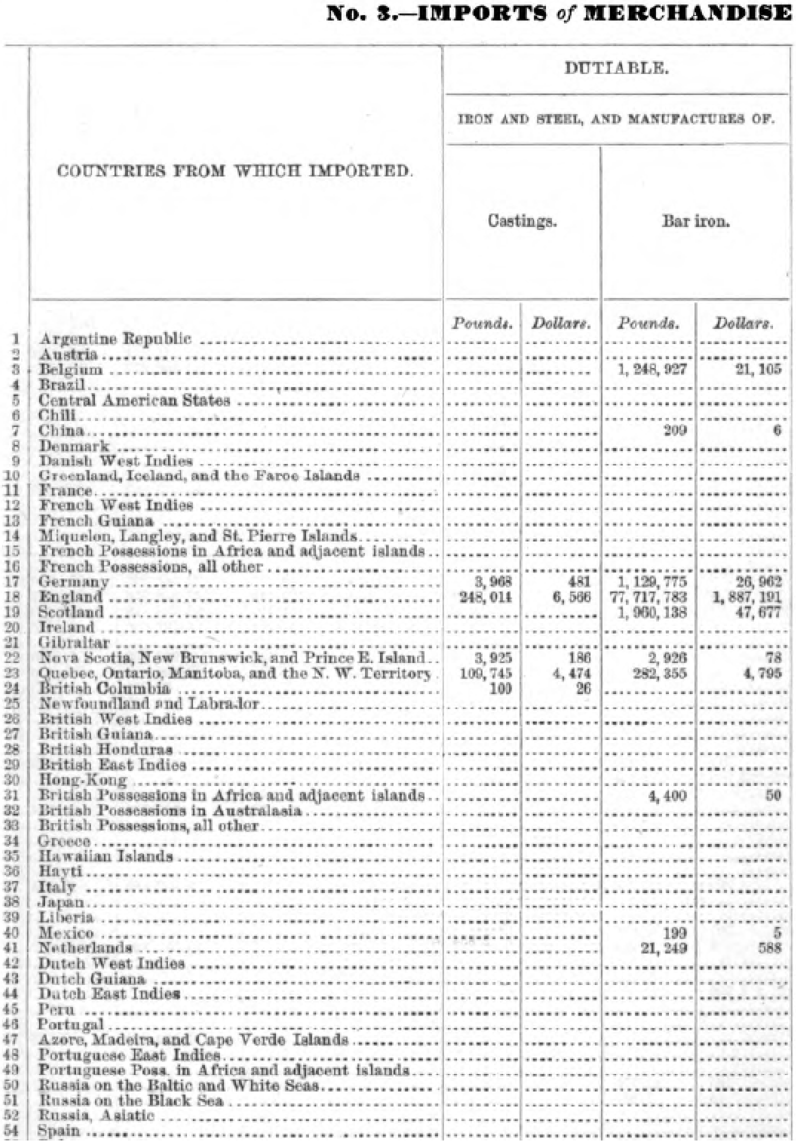
Table 1.15: Digitized pages from Treasury reports

Report	Pages
Foreign commerce and navigation of the United States 1851	158-292
Foreign commerce and navigation of the United States 1881	118-168
Foreign commerce and navigation of the United States 1909	170-373

1.10.3.2 Cross-walking Traded Goods To Manufacturing Industries

As is evident from the example snapshot of the treasury reports (Figure 1.13), both relevant dimensions of the data, trade partners and traded goods, require cross-walking to the immigrant origins and manufacturing industries listed in the full count census (Ruggles et al., 2019).

Correspondences between exporting countries and immigrant origins are fairly straightforward. I always aggregate data in the import data for the United Kingdom when listed separately (England, Wales, Scotland) and for Sweden and Norway, which are combined into one entity. Further, whenever importers are separated by ocean or region (as in



No. 3.—IMPORTS *of* MERCHANDISE

	COUNTRIES FROM WHICH IMPORTED.	DUTIABLE.			
		IRON AND STEEL, AND MANUFACTURES OF.			
		Castings.		Bar iron.	
		Pounds.	*Dollars.*	*Pounds.*	*Dollars.*
1	Argentine Republic				
2	Austria				
3	Belgium			1,248,927	21,105
4	Brazil				
5	Central American States				
6	Chili				
7	China			209	6
8	Denmark				
9	Danish West Indies				
10	Greenland, Iceland, and the Faroe Islands				
11	France				
12	French West Indies				
13	French Guiana				
14	Miquelon, Langley, and St. Pierre Islands				
15	French Possessions in Africa and adjacent islands				
16	French Possessions, all other				
17	Germany	3,968	481	1,129,775	26,962
18	England	248,014	6,566	77,717,783	1,887,191
19	Scotland			1,960,138	47,677
20	Ireland				
21	Gibraltar				
22	Nova Scotia, New Brunswick, and Prince E. Island	3,925	186	2,926	78
23	Quebec, Ontario, Manitoba, and the N. W. Territory	109,745	4,474	282,355	4,795
24	British Columbia	100	26		
25	Newfoundland and Labrador				
26	British West Indies				
27	British Guiana				
28	British Honduras				
29	British East Indies				
30	Hong-Kong				
31	British Possessions in Africa and adjacent islands			4,400	50
32	British Possessions in Australasia				
33	British Possessions, all other				
34	Greece				
35	Hawaiian Islands				
36	Hayti				
37	Italy				
38	Japan				
39	Liberia				
40	Mexico			199	5
41	Netherlands			21,249	588
42	Dutch West Indies				
43	Dutch Guiana				
44	Dutch East Indies				
45	Peru				
46	Portugal				
47	Azore, Madeira, and Cape Verde Islands				
48	Portuguese East Indies				
49	Portuguese Poss. in Africa and adjacent islands				
50	Russia on the Baltic and White Seas				
51	Russia on the Black Sea				
52	Russia, Asiatic				
54	Spain				

Figure 1.13: Treasury Import data: Example

Note: This Figure shows a snapshot of p.145 of Treasury (1881). It lists the value and amount of imports into the U.S. by exporter of iron and steel castings and bar iron in 1881.

"France on the Atlantic" and "France on the Mediterranean"), I aggregate these at the country level ("France"). In 1851, neither Germany nor Italy existed as unified political entities. I aggregate the values for "Hanse Towns" and "Prussia" to the former, and combine "Sardinia" and "Sicily" to "Italy". I further add "Trieste and other Austrian ports" to "Austria-Hungary". I do not combine colonial possessions with their metropole in any case, as I aim to measure the goods exported from the metropole where Europeans migrated from. Table 1.16 documents in column 3 that the number of trading partners (after applying the aforementioned adjustments) increases from 55 in 1851 to 107 in 1909. Correspondences between traded goods and manufacturing industries as listed in Ruggles et al. (2019) are less straightforward. Column 2 and 3 of Table 1.16 indicates that the number of traded goods far exceeds the number of manufacturing industries. I crosswalk the traded goods listed in the Treasury reports to the manufacturing industries of Ruggles et al. (2019) (IND1950 codes) using a combination of an automated and manual procedure. First, I identify for all manufacturing industries of Ruggles et al. (2019) the underlying occupations in the 1880 full count census. I drop filler words and generic terms. This results in a list of terms associated with each IND1950 industry. Then, for each of of the traded goods listed in the Treasury reports, I identify for each IND1950 industry the number of terms that appears in the underlying occupation strings and the traded goods description. This results in a candidate list of manufacturing industries for each traded good. From there I manually identify the closest match.

Table 1.16: Overview on Trade Data in Treasury Reports

Year	Number of		Exporters	Total value of imports
	traded goods	industries	to U.S.	(billion $)
1851	322	38	55	0.30
1881	201	43	66	0.65
1909	509	49	107	1.24

Table 1.17 illustrates this for the 1909 import data. Column 2 provides, for each manufacturing industry, the value of goods imported into the US per the described correspondence. Column 3 indicates the number of traded good categories corresponding to each manufacturing industries. On average, ten traded goods are cross-walked to one manufacturing industry. This number varies widely across industries, from 65 (Misc. manufacturing industries) to nine industries only cross-walked to a single traded good. This reflects varying reporting standards across industries in the Treasury reports, as well as differing width of industries in the census. For instance,“furniture manufacturing” almost exclusively corresponds to the traded good “furniture”. In an (unreported)

robustness check, I estimate the baseline regression only using industries with at least ten corresponding traded goods. The coefficient for these is twice the one reported in column 6 of Table 1.1 and strongly significant. Column 4 of Table 1.17 reports the main traded good category associated with each manufacturing industry by total value traded. For instance, for the manufacturing industry "Apparel and accessories", 36% of the value of imports in this industry comes from the corresponding traded good "Woolen Clothing", as evident from column 5. Column 6 shows the main exporter to the U.S. in each manufacturing industry and column seven shows that importers share of total imports into the U.S. in this industry. For instance, 21% of U.S. imports in "drugs and medicines" hails from Germany in 1909.

Table 1.17: Correspondence of traded industries and manufacturing industries

Manufacturing industry	Imports	# goods	Main traded good		Main exporter	
	million $	traded	Name	Share	Name	Share
Apparel and accessories	82.60	13	Woolen Clothing	0.36	United Kingdom	0.25
Bakery products	0.03	1	Wafers, Unmedicated	1.00	Germany	0.65
Beverage industries	27.41	18	Champagne and Other Sparkling Wines	0.25	France	0.43
Blast furnaces, steel works, and rolling mills	7.17	4	Pig Iron	0.49	United Kingdom	0.42
Canning and preserving fruits, vegetables, and seafoods	18.30	20	Beans and Dried Pease	0.27	Canada	0.25
Carpets, rugs, and other floor coverings	20.15	6	Woolen Carpet	0.55	United Kingdom	0.22
Cement, concrete, gypsum and plaster products	1.30	7	Roman, Portland, and Other Hydraulic Cement	0.55	Canada	0.37
Confectionery and related products	15.22	2	Crude Cacao	0.98	British West Indies	0.26
Dairy products	6.03	3	Cheese	0.97	Italy	0.44

Drugs and medicines	41.72	30	All Other Chemicals, Drugs, and Dyes	0.17	Germany	0.21
Dyeing and finishing textiles, except knit goods	9.90	1	Cotton Cloths: Bleached, Dyed, Colored, Stained, Painted, or Printed	1.00	United Kingdom	0.77
Electrical machinery, equipment, and supplies	0.31	2	Carbons for Electric Lighting	0.91	Germany	0.86
Fabricated nonferrous metal products	3.87	3	Tin Plates, Terne Plates, and Taggers Tin	0.83	United Kingdom	0.95
Fabricated steel products	9.55	14	Nickel Ore and Nickel Matte	0.27	United Kingdom	0.31
Furniture and fixtures	0.71	1	Cabinet Ware or House Furniture	1.00	France	0.31
Glass and glass products	5.27	10	All Other Glass and Glassware	0.49	Germany	0.29
Grain-mill products	19.30	16	Macaroni, Vermicelli, and All Similar Preparations	0.19	Italy	0.20
Knitting mills	0.06	1	Woolen Knit Fabrics	1.00	United Kingdom	0.56
Leather products, except footwear	7.47	1	Gloves, of Kid or other Leather (Dutiable)	1.00	France	0.40
Leather: tanned, curried, and finished	87.17	17	Goatskins	0.30	British India	0.12

Meat products	3.75	8	Sausage Casings	0.60	United Kingdom	0.53
Miscellaneous chemicals and allied products	64.71	63	Nitrate of Soda	0.19	Germany	0.32
Miscellaneous fabricated textile products	7.21	2	Silk Laces and Embroideries	0.78	France	0.71
Miscellaneous food preparations and kindred products	202.80	22	Cane Sugar	0.46	Cuba	0.34
Miscellaneous machinery	4.29	2	Iron and Steel Machinery	0.99	United Kingdom	0.45
Miscellaneous manufacturing industries	48.29	65	Undressed Furs and Fur Skins	0.24	Germany	0.28
Miscellaneous nonmetallic mineral and stone products	31.88	31	Diamonds, Cut, But not Set	0.61	Netherlands	0.28
Miscellaneous paper and pulp products	6.30	2	All Other Paper, and Manufactures of	1.00	Germany	0.50
Miscellaneous petroleum and coal products	4.30	4	Bituminous Coal	0.81	Canada	0.80
Miscellaneous textile mill products	78.96	11	Cotton Laces and Other Similar Tamboured Articles	0.44	British India	0.23
Miscellaneous wood products	5.42	4	Unmanufactured Cork Wood or Cork Bark	0.37	Portugal	0.22

Motor vehicles and motor vehicle equipment	3.68	2	Automobiles	0.79	France	0.64
Not specified metal industries	6.81	1	Metals, Metal Compositions, and Manufactures of, not elsewhere specified	1.00	Germany	0.48
Other primary iron and steel industries	3.46	4	All Other Manufactures of Iron and Steel	0.95	United Kingdom	0.40
Paints, varnishes, and related products	1.75	3	Paints, Pigments, and Colors	0.97	Germany	0.44
Petroleum refining	2.99	9	Tar, Preparations of, Not Colors or Dyes and Not Medicinal, n. e. s.	0.25	Germany	0.45
Photographic equipment and supplies	1.29	2	Collodion, and Manufactures of	0.89	France	0.74
Pottery and related products	9.65	3	Decorated or Ornamented China, Porcelain, Parian, and Bisque	0.87	Germany	0.37
Primary nonferrous industries	74.76	24	Copper, and Manufactures of-Pigs, Ingots, Bars, Plates, and Old	0.39	United Kingdom	0.27

Printing, publishing, and allied industries	16.05	10	Lithographic Labels and Prints	0.28	Germany	0.41
Professional equipment and supplies	0.48	1	Philosophical and Scientific Apparatus, etc.	1.00	Germany	0.77
Pulp, paper, and paperboard mills	10.75	7	Wood Pulp, Chemical, Unbleached	0.42	Canada	0.33
Railroad and miscellaneous transportation equipment	0.05	2	Bars, Railway, of Iron or Steel, or in Part of Steel	0.67	Germany	0.63
Rubber products	66.25	10	Unmanufactured India Rubber	0.93	Brazil	0.52
Sawmills, planing mills, and mill work	30.95	8	Boards, Planks, Deals, and Other Sawed Lumber	0.52	Canada	0.86
Ship and boat building and repairing	0.03	1	Adhesive felt, for Sheathing Vessels	1.00	Ireland	0.68
Tobacco manufactures	29.09	5	Tobacco Leafs	0.69	Cuba	0.66
Watches, clocks, and clockwork-operated devices	2.56	2	Watches, and Parts of	0.82	Switzerland	0.67
Yarn, thread, and fabric mills	157.35	30	Raw Silk	0.50	Japan	0.29

1.10.3.3 Manufacturing Specialization of European countries, 1851-1909

Combining the data sources introduced earlier with the crosswalk of traded good categories to manufacturing industries results in data set of comparative advantages of thirteen European countries revealed in their U.S. imports in 1851, 1881 and 1909. Here I provide summary statistics and describe stylized facts about the cross-country specialization and its change over the course of the "First Globalization".

Specialization in manufacturing increased throughout this period, particularly from 1881 to 1909. Table 1.18 shows summary statistics of the calculated comparative advantages of 13 European countries. The number of industries represented in U.S. imports continuously increases. Switzerland only enters the data set in 1909. Thus, the number of observations at the country-industry level increases from 468 in 1851 to 595 in 1909. Specialization in manufacturing industries increases from 1851 to 1909. The decrease in 1881 is largely owed to the entry of new traded industries. Focusing only on the industries for which comparative advantages are available in 1851 reveals a slight decrease in the mean comparative advantage from 1.345 to 1.335. Specialization in manufacturing more than doubles from 1881 to 1909. Also, the variance in these increased from 1881 to 1909. The last row of Table 1.18 shows that specialization was large and varied substantially across country-industries, even when considering only European exporters to the U.S.

Table 1.18: Summary Statistics of Comparative Advantage

Variable	Obs	Mean	Std. Dev.
RCA 1851	468	1.345	5.147
RCA 1881	504	1.2	3.849
RCA 1909	587	1.659	4.891
RCA 1909 (European origins)	587	1.145	3.695

Yet, comparative advantage is persistent, as documented in modern trade data by Hanson et al. (2015) and Levchenko and Zhang (2016). Table 1.19 shows suggestive correlations that this also was the case in the period under consideration. Column 1 shows that comparative advantage in a county-industry in 1881 predicts comparative advantage in 1909. Yet, the identified slope is below one, indicating, on average, a regression to the mean in comparative advantages across countries and industries. This, as well, is consistent with the dynamics of comparative advantages in modern trade data (Hanson et al., 2015; Levchenko and Zhang, 2016). In line with this, column 2 shows that this relationship weakens over time. Column 4 indicates less persistence from 1851 to 1881. This is not resulting from the inclusion of additional industries compared to column 1. Restricting the regression samples to those already traded in 1851 barely affects the coefficient of column 1. Instead, it potentially indicates more change in country-industry specialization during this period. For all of these results, an important caveat is the change in the underlying traded goods in the industries over time, as the measurement of origins improved particularly after 1851. Column 4 lastly shows that considering comparative advantages in manufacturing vis-a-vis all exporters to the U.S. tends to amplify comparative advantages across the thirteen European origins. Figure 1.14, showing a scatter plot of country-industry comparative advantages in 1881 and 1909, illustrates the two facts of persistence and change. Certain countries retained a comparative (dis) advantage in specific manufacturing industries, gained it, or lost it, during this period.

Table 1.19: Regression Results: Persistence and Change in Comparative Advantage

Dep. Var.: Comparative Advantage, Year As Indicated in Table Header

	(1)	(2)	(3)	(4)
Year Dep. Var.:		1909		1881
RCA (1881)	0.504***			
	(0.150)			
RCA (1851)		0.090		0.276**
		(0.057)		(0.101)
RCA (1909) – Europe			1.168***	
			(0.229)	
R^2	0.205	0.009	0.815	0.127
Observations	463	422	587	420

Note: All regressions are run at the country-industry level. The sample covers 13 countries and 50 manufacturing industries. Standard errors, clustered at the country and industry level, are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

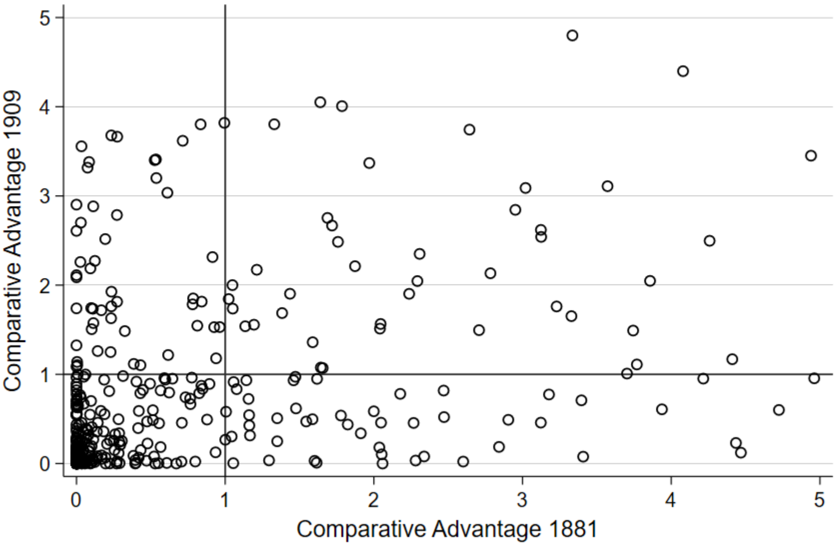


Figure 1.14: Scatter plot of Country-Industry RCA in 1881 and 1909

1.10.4 Additional Baseline Heterogeneity

In this appendix, I provide additional heterogeneity results. I show that the baseline association is stronger in more populous countries and those with a higher share of foreign-born population, as well as in initially less spatially concentrated and faster-growing industries (1.10.4.1). Consequently, the baseline is stronger in the focal regions of immigration (1.10.4.2). While German immigrants account for most of the baseline association, interesting patterns for other origins exist (1.10.4.3).

1.10.4.1 Heterogeneity By County And Industry Characteristics

How do industry and county characteristics affect the association between immigrant specialization and county-industry employment in 1910? Table 1.20 shows results from estimating the baseline specification with immigrant specialization interacted with dummies. These dummies indicate an above-median value of several county and industry characteristics. Columns 1 and 2 show that the baseline association is stronger in counties with a larger population in 1850 and more pronounced in counties with a higher share of foreign-born in 1850. Column 3 turns to industry characteristics, showing that the baseline association is stronger in initially less agglomerated industries.[59] From column 4, it becomes clear the baseline association is stronger for industries that experienced above-median national employment growth from 1850 to 1910.

1.10.4.2 Heterogeneity by Geographic Region

The baseline analysis included all counties settled by 1850, that is, those with a nonzero population recorded in the 1850 decennial census. Here I ask how the baseline association varies across broad geographic regions. I use U.S. Census Divisions and interact dummies for these with immigrant specialization. Table 1.21 shows that throughout, effects are large and significant for all census divisions. Effect sizes are systematically

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[59] Agglomeration is calculated using the following Gini index $G_i = \sum_d (s_{id} - x_d)^2$, where d is destination county in the U.S., s_{id} is a counties' share of total employment in industry i, and x_d is a counties share in aggregate manufacturing employment. Higher values indicate more agglomeration of economic activity in an industry.

Table 1.20: Baseline Heterogeneity by County and Industry Characteristics

Dep. Var.: Log(1+Employment$_{d,i}^{1910}$)

	(1)	(2)	(3)	(4)
Level of Interaction Variable	County		Industry	
Interaction Variable:	Population (1850)	Foreign-born share (1850)	Concentration (1850)	Employment Growth (1850-1910)
Above Median × Log(1+Immigrant Specialization$_{d,i}^{1850}$)	0.076***	0.035***	-0.027***	0.059***
	(0.015)	(0.005)	(0.005)	(0.003)
Log(1+Immigrant Specialization$_{d,i}^{1850}$)	0.038***	0.026***	0.070***	0.031***
	(0.005)	(0.005)	(0.007)	(0.005)
Log(1+Employment$_{d,i}^{1850}$)	0.371***	0.373***	0.360***	0.409***
	(0.008)	(0.008)	(0.008)	(0.009)
R^2	0.630	0.630	0.633	0.632
R^2 (within)	0.061	0.061	0.060	0.065
Observations	113239	113239	108617	113239

Note: This table document heterogeneity of the baseline association at the county and industry level. All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods. All variables are transformed using the logarithm of one plus the variable. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

larger in the focal regions of immigration. Throughout, they are particularly strong in New England and the Middle Atlantic division (including the State of New York) and become larger over time in the census regions containing the Midwest (East North Central and West North Central). Figure 1.15 plots the coefficients of columns 2 and 4 in Table 1.21. Note that the "Mountain" division only includes 16 counties in Arizona, Nevada, and New Mexico.

1.10.4.3 Heterogeneity by Origin

Table 1.22 shows results of including the immigrant specialization of all origins separately in the baseline specification. Column 1 shows the association between each country's immigrant specialization at the county-industry level and county-industry employment in U.S. counties by 1850.[60] Immigrant specialization of the United Kingdom, Spain, and Ireland has the largest coefficients. By 1910 then, German immigrant specialization has the largest effect on county-industry employment (column 5) and its change (column 6), while that of the U.K. has diminished. U.K. immigrant specialization has a significantly negative effect on county-industry employment in 1910.

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[60] Figure 1.16 depicts the coefficient for 1880 (column 3) and 1910 (column 5).

Table 1.21: Baseline Heterogeneity by Census Division

Dep. Var.: County-Industry Employment

	(1) 1850	(2) 1880	(3) 1910	(4) 1930
Year	1850	1880	1910	1930
East North Central × Immigrant Specialization	-0.008^{**}	-0.012^{**}	0.039^{***}	0.106^{***}
	(0.004)	(0.006)	(0.008)	(0.009)
East South Central × Immigrant Specialization	-0.003	-0.007	0.007	-0.006
	(0.003)	(0.005)	(0.007)	(0.008)
Middle Atlantic × Immigrant Specialization	0.130^{***}	-0.008	0.075^{***}	0.132^{***}
	(0.012)	(0.012)	(0.015)	(0.015)
Mountain × Immigrant Specialization	0.075^{***}	0.783^{***}	0.971^{***}	0.268^{**}
	(0.016)	(0.079)	(0.100)	(0.133)
New England × Immigrant Specialization	0.182^{***}	0.092^{***}	0.098^{***}	0.171^{***}
	(0.023)	(0.014)	(0.020)	(0.025)
Pacific × Immigrant Specialization	0.006	0.117^{***}	0.093^{***}	0.166^{***}
	(0.009)	(0.019)	(0.027)	(0.028)
South Atlantic × Immigrant Specialization	0.008^{**}	-0.005	0.034^{***}	0.034^{***}
	(0.004)	(0.005)	(0.007)	(0.009)
West North Central × Immigrant Specialization	-0.010^{**}	0.016^{*}	0.045^{***}	0.085^{***}
	(0.004)	(0.008)	(0.009)	(0.010)
West South Central × Immigrant Specialization	-0.006^{**}	0.051^{***}	0.056^{***}	0.078^{***}
	(0.003)	(0.005)	(0.008)	(0.010)
County-industry Employment (1850)		0.682^{***}	0.372^{***}	0.249^{***}
		(0.007)	(0.008)	(0.008)
R^2	0.566	0.739	0.631	0.632
R^2 (within)	0.007	0.299	0.061	0.028
Observations	113239	113239	113239	113239

Note: All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods. The coefficients are from interactions of a dummy for each U.S. census region and immigrant specialization. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Consider a county home to many immigrants from the U.K. in 1850. The negative coefficient implies that employment in industries in which the U.K. had a comparative advantage in 1909 is higher there in 1850, relative to other industries in this county, but employment growth until 1910 is significantly less in these industries. Moreover, in 1850, the coefficient of Portugal, Sweden & Norway, Switzerland, and Denmark is significantly negative (column 1).

Many potential explanations for these negative coefficients are possible.[61] The small number of origin countries prevents systematic econometric inquiry. Instead, I offer a conceptual account of these explanations. For one, if an origin is successful in export-

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[61] Note that these coefficients are not reflective of all industries. Denmark for instance, exhibits a strongly negative coefficient in the baseline analysis. Boberg-Fazlic and Sharp (2018) convincingly demonstrate that immigrant communities settled prior to 1880 become increasingly specialized in dairying after the development of that industry in the Denmark. Indeed, the comparative advantage of Denmark vis-a-vis the U.S. in dairying increases from 0 in 1851 to 17 in 1909 in the raw data, such that the measure of immigrant specialization reflects this.

Table 1.22: Baseline Heterogeneity by Origin Country

Dep. Var.: County-Industry Employment

	(1)	(2)	(3)	(4)	(5)
Year	1850	1880		1910	
Belgium	0.001	0.015	0.014	0.033**	0.033**
	(0.011)	(0.011)	(0.009)	(0.015)	(0.015)
Denmark	-0.103***	-0.110***	-0.040***	-0.131***	-0.092***
	(0.007)	(0.009)	(0.007)	(0.012)	(0.010)
France	0.062***	0.027***	-0.015***	0.023***	-0.000
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Germany	0.026***	0.049***	0.032***	0.110***	0.101***
	(0.004)	(0.005)	(0.005)	(0.007)	(0.007)
Ireland	0.102***	0.092***	0.023***	0.045***	0.007*
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Italy	-0.018**	-0.051***	-0.039***	-0.012	-0.005
	(0.008)	(0.010)	(0.009)	(0.011)	(0.010)
Austria-Hungary	0.005	0.002	-0.002	-0.014	-0.016*
	(0.008)	(0.009)	(0.008)	(0.010)	(0.009)
Netherlands	0.020***	0.086***	0.072***	0.068***	0.060***
	(0.005)	(0.008)	(0.007)	(0.008)	(0.008)
Portugal	-0.051***	-0.009	0.026***	0.021*	0.040***
	(0.008)	(0.008)	(0.009)	(0.011)	(0.011)
Spain	0.092***	0.106***	0.044***	0.082***	0.048***
	(0.012)	(0.012)	(0.009)	(0.013)	(0.012)
Sweden & Norway	-0.006*	0.021***	0.025***	0.046***	0.048***
	(0.003)	(0.004)	(0.004)	(0.006)	(0.006)
United Kingdom	0.082***	0.006	-0.050***	-0.062***	-0.092***
	(0.004)	(0.006)	(0.006)	(0.008)	(0.008)
Switzerland	-0.057***	-0.035***	0.004	-0.027***	-0.006
	(0.003)	(0.005)	(0.005)	(0.005)	(0.005)
Log(1+$\text{Employment}_{d,i}^{1850}$)			0.432***		0.370 ***
			(0.020)		(0.008)
R^2	0.579	0.636	0.741	0.610	0.633
R^2 (within)	0.036	0.020	0.302	0.010	0.066
Observations	113239	113239	113239	113239	113239

Note: All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods. The coefficients are from origin-specific immigrant specialization as described in the text. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

ing goods of a particular industry, immigrants might be selected from those working in *other* industries, in which the country is not successful and hence does not offer opportunities or compensation.[62] Second, even if skilled artisans migrate who are skilled in industries where in which origin country has a comparative advantage, high return migration rates might prohibit the long-term investment required from these immigrants to influence local county-industry employment. Similarly, low expectations about these artisans' persistent availability in a county might hinder domestic entrepreneurs from setting up or expanding shop there. In the second half of the 19th century, with the

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[62] Abramitzky et al. (2014) however document the higher occupational standing of immigrants from Austria-Hungary and Italy compared to natives after 1900. Note that a negative coefficient can even result from high-skilled migration, as long as the immigrants are highly skilled in industries in which their origin has no comparative advantage.

steamship's introduction, return migration rates are generally presumed to be high. As immigrants from the U.K. already spoke English and were close to steamship routes to the U.S., one might assume high return migration rates for these. Indeed, Bandiera et al. (2013) find that from 1900 to 1910, out-migration rates were relatively high for Great Britain (0.67), particularly when compared to other origins whose immigrants were relatively more skilled than natives (Abramitzky et al., 2014), such as Germans and Austrians (0.52 and 0.55).[63] Lastly, as is evident from the effect being particularly strong for fast-growing industries, we would expect immigrant specialization to induce country-industry employment in the early phases of industries or as technology shifts quickly. As industries age, employment tends to disperse across space (Desmet and Rossi-Hansberg, 2009). This happened to one of the foremost industries of the First Industrial Revolution, textile manufacturing. By 1900, the textile industry had moved from its prior center in New England to Georgia, North, and South Carolina, which then became sizable centers of production and employment (Niemi, 1974). This move was largely owed to the maturing of technology in this industry, which was then less reliant on skilled artisans and more on unskilled labor. The Southern states - already being close to the primary input, cotton - boasted cheap low-skilled labor and became increasingly connected to national and international markets with the expansion of the railroad network. This "older" industry, therefore, dispersed away from its initial centers - which immigrant specialization does predict well - to other locations, as it was less dependent on skilled, often immigrant, workers.[64]

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[63] Out-migration rates are also high for Italians (0.72), Irish (0.74) and Spaniards (0.84) - the latter two have a positive effect of immigrant specialization on county-industry employment however.

[64] However, Carlson (1981) argues that for the location of industries *within* these Southern states, the initially available skilled labor force played a crucial role.

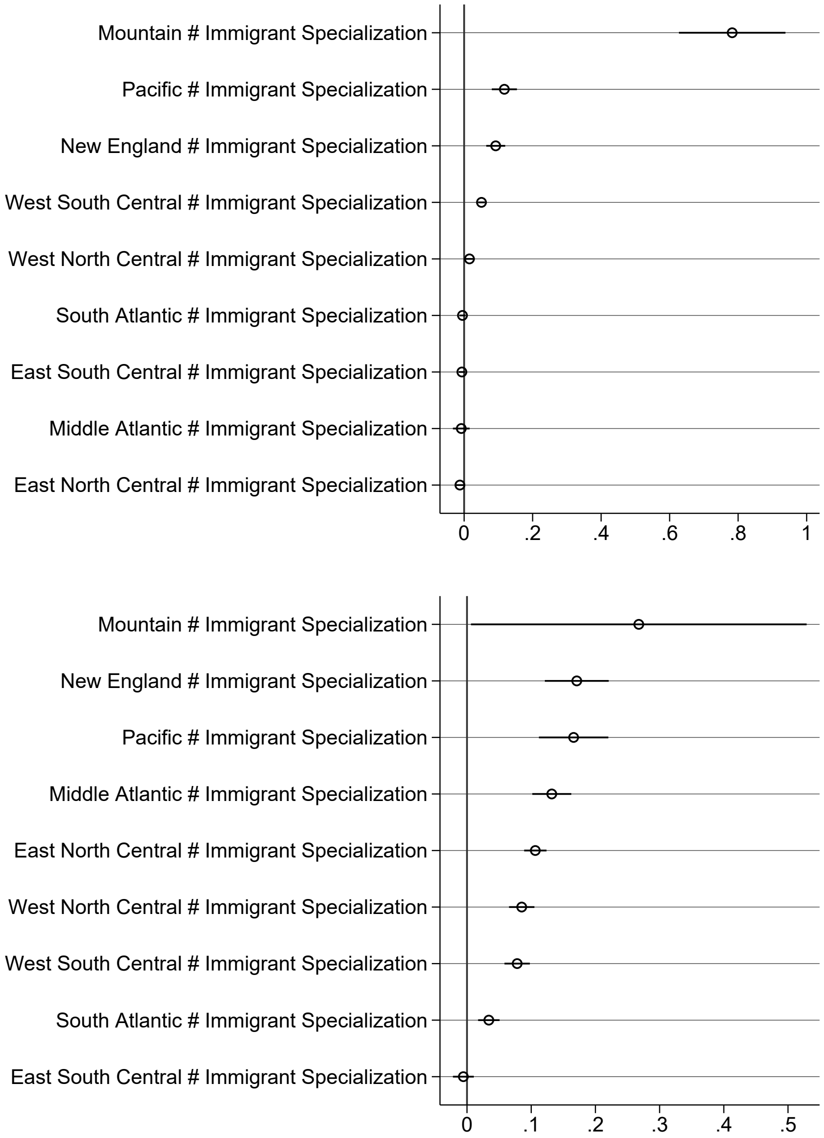

Figure 1.15: 1880 (upper panel) and 1930 (lower panel)

Note: This figures shows the coefficients of interactions of immigrant specialization and dummies for each census division on county-industry employment in various 1880 (upper panel) and 1930 (lower panel). Both of these measures are transformed with the logarithm of one plus the depicted variables. Immigrant specialization is the sum of immigrants residing in each US county in 1850 weighted by their origins comparative advantage revealed in US import data in 1909.

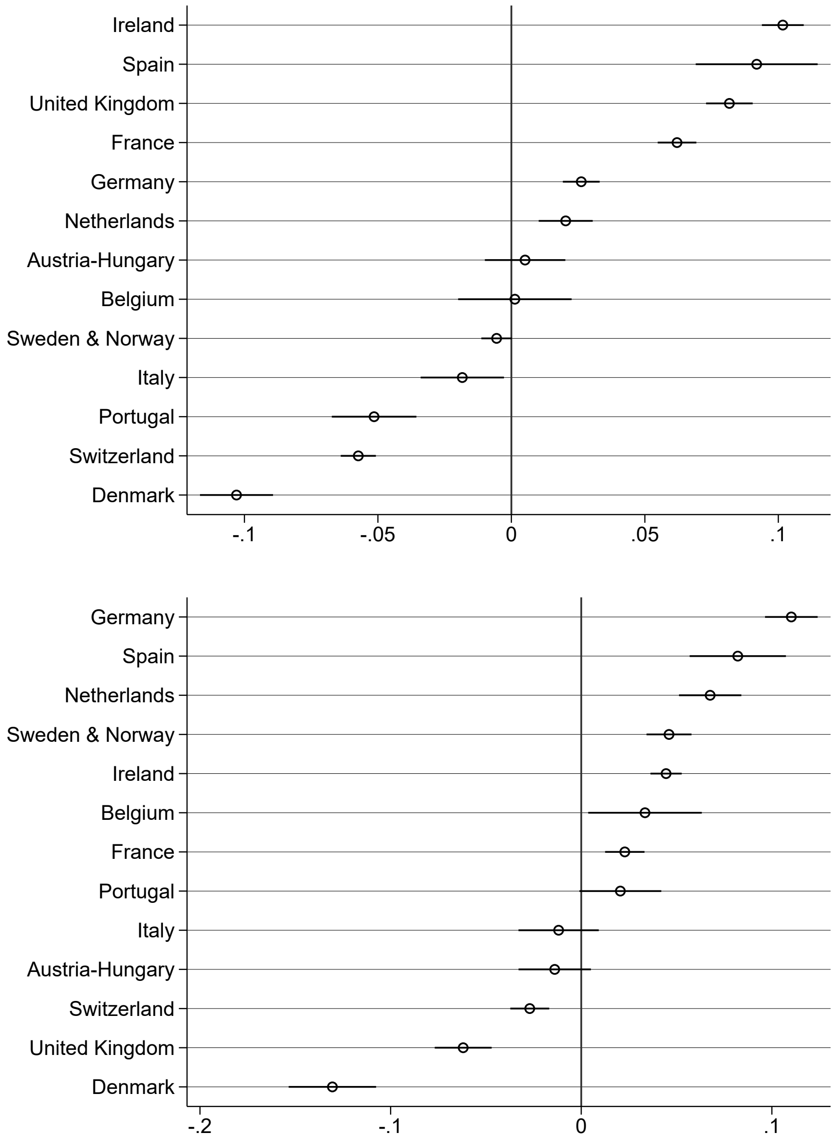

Figure 1.16: Baseline Effect by Immigrant Origin: 1850 (upper panel) and 1910 (lower panel)

Note: This figures shows the coefficients of regressions of immigrant specialization by origin country on county industry employment in 1850 (upper panel) and 1910 (lower panel). All of these measures are transformed with the logarithm of one plus the depicted variables. Immigrant specialization for each origin is the number of immigrants residing in each US county in 1850 from that origin, weighted by their origins comparative advantage revealed in US import data in 1909. All regressions included county and industry fixed effects and controlled for the county-industry employment in 1850 (transformed as the logarithm of one plus county-industry employment in 1850). Standard errors clustered at the county level.

1.10.5 Baseline Robustness

In this Appendix, I provide detail on the robustness of the baseline specification as summarized in Section 1.5.4 of the paper.

1.10.5.1 Immigrant Specialization: Comparative Advantage - Timing and Measurement

The baseline measure of immigrant specialization was constructed using imports to the United States by origin country and industry in 1909 (Treasury, 1909) to proxy for the immigrant's origin comparative advantage. In the definition of the measure, this amounts to a baseline choice of $t = 1909$.

$$ImmigrantSpecialization_{d,i} = \sum_{o} IMMIG_{o,d}^{1850} RCA_{o,i}^{t}$$

Here I use information recovered from U.S. import data for $t = 1851$ and $t = 1881$ instead (Treasury, 1851, 1881). Note that this earlier data comes with shortcomings, including information on less industries and issues of mapping trade partners to immigrant origin countries (see discussion in Section 1.4 and Appendix 1.10.3). Column 1 of Table 1.23 shows the baseline using 1851 comparative advantage in the construction of immigrant specialization. The size of the main coefficient of interest is reduced. The origins of this reduction are likely to be found in increased measurement error and reduced coverage, particularly of industries that were not traded by 1851 - before the steamship and the increasing manufacturing specialization within Europe. Column 2 repeats the baseline using 1881 trade data in the construction of origins comparative advantage. The coefficient of interest is larger than the baseline and significant.

Does specialization in trade with non-European countries and colonies drive the baseline association? Column 3 replicates the baseline specification (column 5 of Table 1.1). Column 4 ignores all non-European trade partners of the U.S. and calculates comparative advantage only of the European origins among each other. The coefficient of interest is increased by this, reassuring that it truly is differences in comparative advantage between European origins driving the baseline.

Is measurement error in comparative advantage a concern? In Column 6, instead of using the continuous value of comparative advantage, I rely on dummies indicating whether a particular origin exhibited a comparative advantage in an industry in 1909 ($1(RCA^{1909}_{o,i} > 1)$):

$$ImmigrantSpecialization_{d,i} = \sum_{o} IMMIG^{1850}_{o,d} 1(RCA^{1909}_{o,i} > 1)$$

With this, the measure reduces to a count of immigrants in a location, whose origins exhibited a comparative advantage in a manufacturing industry. Yet, as the measure's variation still is at the county-industry industry level, the baseline specification including county fixed effects can still be run. The coefficient again is larger than in the baseline, speaking to the limited importance of extreme comparative advantages (as for Swiss watch manufacturing, for instance).

Table 1.23: Robustness: Timing and Measurement of Comparative Advantage of Origins

Dep. Var.: Log(1+Employment$^{1910}_{c,i}$)

	(1)	(2)	(3)	(4)	(5)
Log(1+Immigrant Specialization$^{1850}_{d,i}$)[CA: 1851]	0.016***				
	(0.005)				
Log(1+Immigrant Specialization$^{1850}_{d,i}$)[CA: 1881]		0.061***			
		(0.005)			
Log(1+Immigrant Specialization$^{1850}_{d,i}$) [CA: 1909]			0.044***		
			(0.005)		
Log(1+Immigrant Specialization$^{1850}_{d,i}$) [CA: 1909, Europe]				0.052***	
				(0.006)	
Log(1+Immigrant Specialization$^{1850}_{d,i}$) [CA: 1909, Dummy]					0.059***
					(0.004)
Log(1+Employment$^{1850}_{d,i}$)	0.335***	0.383***	0.374***	0.374***	0.370***
	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)
R^2	0.622	0.636	0.630	0.630	0.631
R^2 (within)	0.051	0.063	0.061	0.061	0.063
Observations	87818	94751	113239	113239	113239

Note: All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods, which results in different sample sizes in 1851 and 1881. All variables are transformed using the logarithm of one plus the variable. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

A related concern is the usage of comparative advantage measures based solely on imports to the United States. While the U.S. import data allows me to map traded goods at a highly dis-aggregated level to 49 manufacturing industries, it comes at a disadvantage, as features of the bilateral relationship of origins with the U.S. might affect their export specialization vis-a-vis the U.S. but not other trade partners.

To overcome this limitation, I require the *total* exports of a country to all its trading partners. For this, I rely on data from Tyszynski (1951) for 17 broad manufacturing industries and many major trading nations in 1899. I bring the industries of the census data to that level (available upon request) and re-run the baseline analysis. Table 1.24 provides results. Column 1 replicates the baseline analysis of the paper at the origin level, with 13 European origins and 49 manufacturing industries based on US imports. In column 2, I turn to the broader industry classification used in Tyszynski (1951). This means that I aggregate county-industry employment at this level, as well as traded goods for the calculation of the origins' comparative advantage. Changing to this broad industry level significantly affects the sign of some origins, speaking to the highly dis-aggregated nature of the country-level effects. While the Dutch immigrants for instance tended to settle in counties in which their dis-aggregated manufacturing industries saw stronger employment increases until 1910, they appeared to also settle in counties where their more aggregated manufacturing industries saw less employment until then, compared to other industries in the counties. The opposite holds true for the United Kingdom.[65] Column 3 then restricts attention to the effect of immigrant specialization from all those origins that also appear in Tyszynski (1951). Again, the coefficient of some origins changes sign. The missing immigrant specialization of other nations now constitutes an omitted variable in this and the following estimation. Column 4 lastly uses immigrant specialization based on comparative advantage calculated in Tyszynski (1951). It shows broadly comparable results to those of column 3. The coefficient of immigrant specialization based on global comparative advantage is larger for Germany and for the United Kingdom, compared to that based on imports into the U.S. only. This hints at the possibility that at this higher aggregation level of industries, the comparative advantage of these two industrialized nations was more pronounced vis-a-vis

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[65] Note that while the sign and size of the effect at the origin-level varies at the industry-level of Tyszynski (1951), the combined effect of immigrant specialization from all origins is sizable, positive, and highly significant (elasticity of 0.14, and t-statistic of <0.01).

the rest of the World than solely the United States.

Table 1.24: Robustness: Global Comparative Advantage of Origins

Dep. Var.: Log(1+Employment$^{1910}_{d,i}$)

	(1)	(2)	(3)	(4)
Industries	49 (Census)	17 (Tyszynski, 1951)		
Data	US Imports (1909)			World Exports (1899) (Tyszynski, 1951)
Belgium	0.033**	0.004	-0.049*	-0.112***
	(0.015)	(0.028)	(0.027)	(0.029)
Denmark	-0.092***	-0.195***		
	(0.010)	(0.025)		
France	-0.000	0.159***	0.154***	0.041*
	(0.005)	(0.015)	(0.014)	(0.024)
Germany	0.101***	0.017	0.014	0.183***
	(0.007)	(0.010)	(0.011)	(0.014)
Ireland	0.007*	0.070***		
	(0.004)	(0.007)		
Italy	-0.005	-0.021	-0.056***	0.016
	(0.010)	(0.019)	(0.016)	(0.022)
Austria-Hungary	-0.016*	0.041		
	(0.009)	(0.031)		
Netherlands	0.060***	-0.061***		
	(0.008)	(0.016)		
Portugal	0.040***	-0.055**		
	(0.011)	(0.024)		
Spain	0.048***	0.011		
	(0.012)	(0.017)		
Sweden & Norway	0.048***	-0.001	-0.034***	-0.125***
	(0.006)	(0.013)	(0.012)	(0.017)
United Kingdom	-0.092***	0.146***	0.172***	0.473***
	(0.008)	(0.013)	(0.013)	(0.042)
Switzerland	-0.006	-0.089***	-0.073***	-0.150***
	(0.005)	(0.009)	(0.009)	(0.015)
Log(1+Employment$^{1850}_{d,i}$)	0.370***	0.282***	0.302***	0.318***
	(0.008)	(0.010)	(0.010)	(0.010)
R^2	0.633	0.739	0.738	0.737
Observations	113239	39287	39287	39287

Note: All regressions are run at the county-industry level. In column 1, industries are 49 manufacturing industries from IPUMS, and in columns 2,3, and 4, these are 17 manufacturing industries of Tyszynski (1951). The origins comparative advantage is calculated using imports in 1909 to the US in columns 1 to 3, and from total exports in 1899 ((Tyszynski, 1951)). The sample includes all counties with non-zero population in 1850. All variables are transformed using the logarithm of one plus the variable. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

1.10.5.2 Immigrant Specialization: County-Origin Population - Measurement and Timing

Here I focus on the second component of immigrant specialization. In the baseline analysis, I employ county-origin immigrant populations in 1850 and ignore the comparative advantage of natives. Column 1 of Table 1.25 addresses the latter issue. As

I only have import data of the US at the disaggregated industry level of the baseline, I cannot deduce the comparative advantage of the US itself. I assume that instead all US native-born individuals have the same comparative advantage as their former metropole, the UK does. Column 1 shows that said assumption reduces the coefficient of interest compared to the baseline (shown in column 2). The remaining columns vary the census year from which county-origin immigrant population is used in the measure. Column 3 shows the coefficient of interest when using county-origin population in 1880. The lower coefficient when using later immigrant populations in confirmed in Column 5 with 1910 county-origin immigrant populations.

Table 1.25: Robustness: Timing and Measurement of County-Origin Populations

Dep. Var.: Log(1+Employment$^{1910}_{c,i}$)

	(1)	(2)	(3)	(4)
Log(1+Immigrant Specialization$^{1850}_{d,i}$) [US CA = UK CA]	0.012***			
	(0.004)			
Log(1+Immigrant Specialization$^{1850}_{d,i}$) [Pop: 1850]		0.044***		
		(0.005)		
Log(1+Immigrant Specialization$^{1850}_{d,i}$) [Pop: 1880]			0.036***	
			(0.006)	
Log(1+Immigrant Specialization$^{1850}_{d,i}$) [Pop: 1910]				0.030***
				(0.005)
Log(1+Employment$^{1850}_{d,i}$)	0.374***	0.374***	0.374***	0.374***
	(0.008)	(0.008)	(0.008)	(0.008)
R^2	0.630	0.630	0.630	0.630
R^2 (within)	0.060	0.061	0.060	0.060
Observations	113239	113239	113239	113239

Note: All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods. All variables are transformed using the logarithm of one plus the variable. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

1.10.5.3 Zeros in County-industry Employment

The baseline analysis focuses on a log transformation of county-industry employment as the outcome variable. This transformation enables me to analyse both margins of county-industry employment in one regression and accounts for the skewed distribution of county-industry employment. Yet, many counties do not report employment in particular industries. Indeed, in 1850, 48.3% county-industry observations are zeros. By 1910, this is reduced to 22.1%. Column 1 of Table 1.26 shows regressions of immigrant specialization on a dummy indicating the presence of at least one individual working in a county-industry, controlling for a similar dummy in 1850. The results indicate that immigrant specialization is positively associated with the emergence of employment in a county-industry. Column 2 instead looks only at the intensive margin using untransformed county-industry employment. Increasing immigrant specialization by 1% increases employment in county-industries that already existed by 1850 by about 21 workers. Column 3 extends the sample again to include also county-industries with zero employment in 1850. Column 4 instead relies on the inverse hyperbolic sine to deal with zeros in the baseline, instead of adding one to the variable and taking the logarithm (Bellemare and Wichman, 2020). The coefficient is very similar to the baseline one. Column uses measures of revealed comparative advantage for county-industries based on employment industries. Column 6 estimates the baseline using Pseudo-poisson maximum likelihood (PPML). This approach is used in the trade literature to estimate gravity equations featuring many zeros (Silva and Tenreyro, 2006).

Table 1.26: Robustness: Measurement of Dependent Variable

Dep. Var.: $\text{Employment}_{d,i}$ (1910) - Measurement as Indicated in Table Header

	(1)	(2)	(3)	(4)	(5)	(6)
Measurement Employment:	Dummy (non-zero employment)	Employment	Employment	IHS(Employment)	RCA(Employment)	Employment
Estimation:	OLS					PPML
$\text{Log}(1+\text{Immigrant Specialization}_{d,i}^{1850})$	0.015***	20.651***	9.533***	0.044***	0.219***	0.448***
	(0.002)	(6.586)	(2.462)	(0.006)	(0.064)	(0.049)
$\text{Employment}_{d,i}^{1850}$	0.059***	3.754***	3.774***	0.339***	0.002	0.381***
	(0.004)	(0.986)	(0.961)	(0.007)	(0.002)	(0.035)
R^2	0.503	0.253	0.237	0.633	0.056	
Observations	113239	47274	113239	113239	108382	113141

Note: All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods in 1909. All regressions include county and industry fixed effects. The main independent variable is the logarithm of one plus the measure of immigrant specialization. Measurement of county-industry employment and estimation differs by column and are indicated in the Table header. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

1.10.5.4 Alternative Measures of Specialization

By the inclusion of county and industry fixed effects, as well as 1850 county-industry employment, the coefficient of interest in the baseline was interpreted as the change of employment within a county and industry from 1850 to 1910. As this was compared to other industries within the same county and other counties in the same industry, this measure it closely related to specialization in a county-industry. Various measures of specialization exist in the literature. Here I test the baseline with measures of specialization commonly used in the literature. Column 1 of Table 1.27 uses the share of employment in a county-industry of total employment in that county to measure specialization (both for the outcome variable in 1910 and the control variable in 1850). Doubling immigrant specialization is associated with a 0.7% increase in an industry's local employment share. The coefficients for a county-industries share in nation-wide total manufacturing employment (column 3) and its share in total nation-wide employment are smaller, but significant.

Table 1.27: Robustness: Specialization Measures as Independent Variable

Dep. Var.: Specialization (1910) - Measurement as Indicated in Table Header

	(1)	(2)	(3)
$x_{i,d}$: Employment$^{1910}_{d,i}$	$\frac{x_{i,d}}{\sum_i x_{i,d}}$	$\frac{x_{i,d}}{\sum_{i,d} x_{i,d}}$	$\frac{x_{i,d}}{\sum_d x_d}$
		$\times 1,000$	$\times 1,000$
Log(1+Immigrant Specialization$^{1850}_{d,i}$)	0.006***	0.002***	3.338***
	(0.000)	(0.001)	(0.785)
Employment$^{1850}_{d,i}$	0.067***	0.538***	2.773***
	(0.010)	(0.137)	(0.743)
R^2	0.350	0.237	0.158
Observations	112994	113239	108617

Note: All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods in 1909. All regressions include county and industry fixed effects. Measurement of county-industry employment and immigrant specialization differ by column and are indicated in the Table header, where i is industry and d county. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.
Column 1 measures specialization as the local employment share in a manufacturing industry, column 2 as the share in total employment, and column 3 as the share in total industry employment.

1.10.5.5 Clustering

This section discusses the assumption on the error terms in the baseline regression. What correlation across county-industry errors should we assume, after applying county and industry fixed effects? In the baseline specification, I allowed for these to be correlated within the same county. Column 1 in Table 1.28 uses robust errors instead. Clustering at the state level – as in Column 2 – results in larger standard errors. Extending the geographic units, to the four regions as in Column 3, shows comparable standard errors. Column 4 clusters standard errors at the industry level instead. Allowing for arbitrarily correlated correlation of unobservables at this level reduces significance below the 10% level of significance. The number of clusters in this case is only 49. Allowing for such correlations across industries only within regions - thereby increasing the number of clusters, but making arbitrarily assumptions of correlations particularly around these region's borders – in Column 5 instead increases precision again. Column 6 imposes more structure on the nature of spatial correlation, using Conley standard errors with a distance of 1650 kilometers, corresponding to the geodesic distance from New York City to Minneapolis.

Table 1.28: Robustness: Clustering of Standard Errors

Dep. Var.: Log(1+Employment$_{c,i}$, 1910)

	(1)	(2)	(3)	(4)	(5)	(6)
Standard errors:	Robust	State	Region	Industry	Industry × Region	Conley: 1,650 km
Log(1+Immigrant Specialization$^{1850}_{d,i}$)	0.044^{***}	0.044^{***}	0.044^{**}	0.044	0.044^{**}	0.044^{***}
	(0.005)	(0.013)	(0.011)	(0.028)	(0.019)	(0.005)
Log(1+Employment$^{1850}_{d,i}$) (1850)	0.374^{***}	0.374^{***}	0.374^{***}	0.374^{***}	0.374^{***}	0.374^{***}
	(0.006)	(0.022)	(0.060)	(0.048)	(0.034)	(0.010)
R^2	0.630	0.630	0.630	0.630	0.630	0.630
Observations	113239	113239	113239	113239	113239	113239

Note: All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods in 1909. County and industry fixed effects are included in all regressions. Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

1.10.5.6 Analysis at State Level

This sections repeats the baseline analysis at the state-industry level. Specifically, I construct my measure of immigrant specialization using immigrant populations by origin in entire U.S. states in 1850 rather than at U.S. counties. In the resulting data set, covering 41 states with non-zero population in 1850 and the 49 manufacturing industries traded in 1909. The results presented in Table 1.29 document that the association between immigrant specialization and employment patterns across US states and industries also holds at the state level. Due to the small number of clusters, robust standard errors are used. The coefficient size is highly comparable to the baseline analysis at the county-industry level, except for the last specification including state fixed effects.

Table 1.29: Robustness: Analysis at State level

Dep. Var.: Log(1+Employment$_{c,i}$, 1910)

	(1)	(2)	(3)	(4)
log(1+Immigrant Specialization 1850)	0.302*** (0.014)	0.109*** (0.016)	0.164*** (0.015)	0.175*** (0.059)
Log(1+County-industry Employment 1850)		0.555*** (0.020)	0.436*** (0.030)	0.295*** (0.025)
Industry FE			✓	✓
State FE				✓
R^2	0.208	0.390	0.630	0.825
R^2 (within)	0.207	0.390	0.419	0.085
Observations	2009	2009	2009	2009

Note: All regressions are run at the state-industry level. The sample includes all 41 states with non-zero population in 1850 and all industries with corresponding traded goods in 1909. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

1.10.6 Selective Migration: Heterogeneity Analysis

In this Appendix I provide additional evidence that immigrants did not selectively migrate to counties with inherent potential in their origins' industries.

In column 1 of Table 1.30, I show the baseline regression using all counties settled until 1880 and immigrant specialization calculated with county-origin populations in that year.[66] Column 2 then interacts immigrant specialization with a dummy indicating counties not settled by 1850. For those counties, only settled in the 30 years in between, the association of immigrant specialization with county-industry employment in 1910 is higher and significant. This speaks against the possibility that the baseline association is purely driven by the recruitment or selective migration of European skilled workers to counties with a long history of employment in a particular industry. Still, it leaves open the possibility that news traveled fast, and emerging companies attracted skilled immigrants quickly.

The next three columns use a different angle: Here, I exploit the fact that specific industries were not existing by 1850. Immigrants arriving before then could hardly have known about where these industries would find fertile soil. Column 3 first repeats the baseline specification of the preceding section. Column 4 then again interacts immigrant specialization with a dummy indicating whether an industry was imported into the U.S. in 1909 but not in 1851. We observe a slightly and insignificantly larger association for these. The fact that these 13 industries' goods were not traded in 1851 but 1909 might also reflect decreases in transatlantic transportation times (as for "bakery products") or reporting in the underlying data source. The dummy used in column 5 finally restricts attention to two industries coming into existence after 1851 only (auto and electrical manufacturing) to address this concern directly. For these, the baseline association is particularly strong. Could immigrants arriving until 1850 have foreseen a counties' potential for said industries and settled there? Potentially for the car, with carriage manufacturing as a predecessor industry. Arguably less so for electrical manufacturing, where most of the relevant scientific knowledge emerged after 1851 only.

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[66] Note that the thereby enlarged sample addresses the concern of the sample thus far consisting of those counties settled in 1850 only.

Table 1.30: Selective Migration: Heterogeneity Analysis

Dep. Var.: Log(1+Employment$_{d,i}^{1910}$)

	(1)	(2)	(3)	(4)	(5)
Immigrant Specialization Year:	1880		1850		
Dummy	Counties settled after 1850		Industries traded after 1850		
				All	Auto and Electricity
Log(1+Immigrant Specialization$_{d,i}^{t}$)	0.030^{***}	0.023^{***}	0.044^{***}	0.042^{***}	0.033^{***}
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Dummy × Log(1+Immigrant Specialization$_{d,i}^{t}$)		0.036^{***}		0.004	0.106^{***}
		(0.006)		(0.003)	(0.007)
Log(1+Employment$_{d,i}^{1850}$)			0.374^{***}	0.374^{***}	0.384^{***}
			(0.008)	(0.008)	(0.008)
Log(1+Employment$_{d,i}^{1880}$)	0.543^{***}	0.543^{***}			
	(0.006)	(0.006)			
R^2	0.680	0.680	0.630	0.630	0.631
R^2 (within)	0.200	0.200	0.061	0.061	0.063
Observations	134242	134242	113239	113239	113239

Note: All regressions are run at the county-industry level. The sample includes all counties with non-zero population in 1850 (except for column 1 and 2, where the sample consists of all counties with non-zero population in 1880) and all industries with corresponding traded goods in 1909. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Column 2 interacts immigrant specialization (calculated using 1880 county-origin population) with a dummy indicating whether a county - settled by 1880 - recorded zero population in the 1850 census. Column 4 (calculated using 1880 county-origin population) interacts immigrant specialization (calculated using 1850 county-origin population) with a dummy indicating whether an industry was imported into the U.S. in 1909 but not in 1851. In column 5 the dummy is one if the industry is either motor vehicle or electrical manufacturing.

1.10.7 Frontier Instrument

In this Appendix, I describe the construction of the quasi-exogenous variation in county-origin populations in 1850 and show corresponding zero-stage results.

1.10.7.1 Construction of the Instrument

Frontier Counties, 1820-1840 I trace the frontier line following Bazzi et al. (2020). Starting with the NHGIS county shape files for 1820, 1830, 1840 and linked county total population data, I calculate population density per square mile, and create a contour line at a population density of 2 inhabitants per square mile (Turner, 1893; Bazzi et al., 2020). Next, I select all the settled counties with a population density of less than 6 within 100 miles of the frontier line. Counties in this area are called "frontier counties" in a census year. Figure 1.17 shows the number of decades each county was at the frontier. Note that Texas only became a part of the United States after 1840 and hence is not part of the frontier sample.

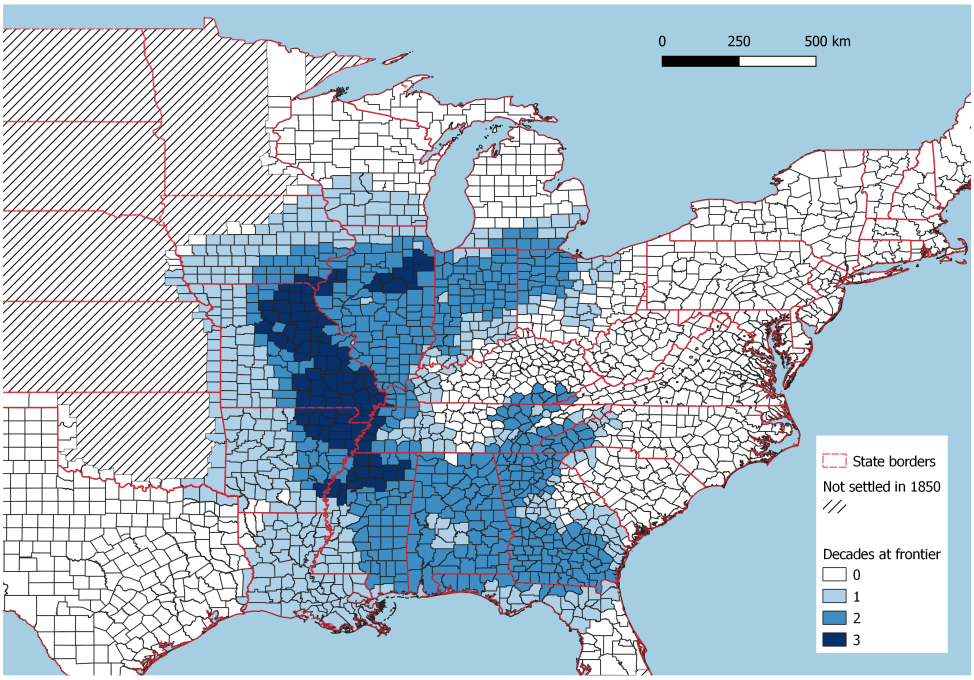

Figure 1.17: Counties at the frontier, 1820-1840

Note: This Map shows which countries have been at the frontier for how many decades between 1820 and 1840. The frontier area is constructed as detailed in the text, closely following Bazzi et al. (2020).

Immigrant Flows By Origin, 1820-1849 I combine this county-level variation with the aggregate inflows of immigrants from a particular origin in the following decade from Office of Immigration Statistics (2013). Figure 1.18 shows the inflows by decade for all European countries used in the analysis.

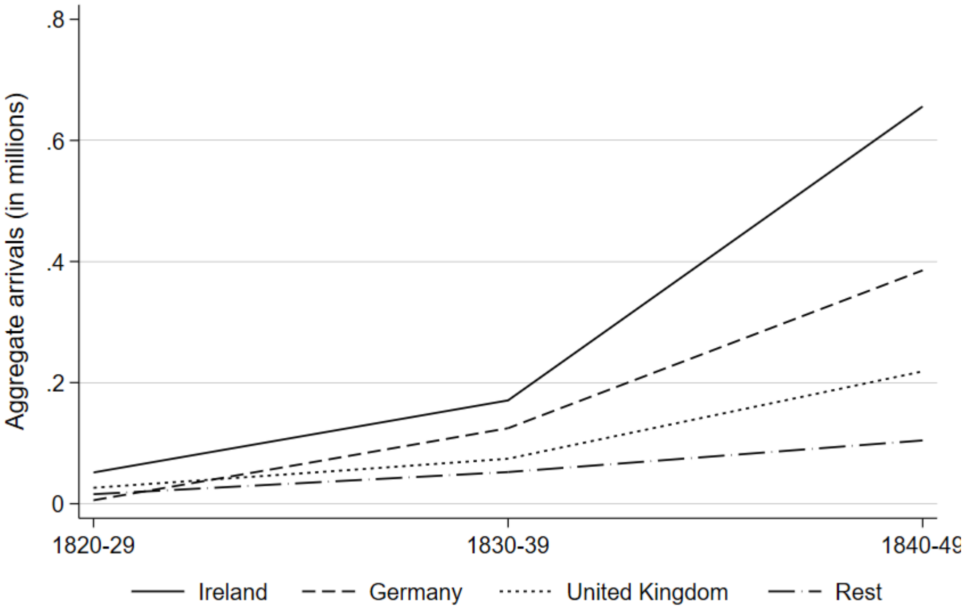

Figure 1.18: Aggregate Immigration from European origins, 1820-1849

A remaining threat to identification is that manufacturing entrepreneurs migrated from an European origin because the frontier area covered regions suitable to industries, in which a particular origin country had or developed a comparative advantage. To isolate agricultural immigrants, I rely on a factor "pushing" farmers out of their European origins. In the vein of Sequeira et al. (2019), I calculate the temperature and precipitation in regions not entirely unsuitable to either pasture or crop agriculture across European origins for each of the years y 1820 to 1849.[67]. With these and yearly immigrant inflows by origin from Willcox (1932), I estimate the following regression for each origin separately

$$log(Immig_{o,y+1}) = \sum_{s\in S}\sum_{k\in K}\beta_{s,o}I_{o,y}^{Temp,s,} + \sum_{s}\sum_{k}\gamma_{s,o}I_{o,y}^{Precip,s} + \varepsilon_{o,t}$$

where $I_{o,t}^{Temp,s}$ is a dummy variable indicating if the average temperature in origin country c was one standard deviation above or below its mean from 1820 to 1849 season

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[67] Data on cell suitability for agriculture comes from Ramankutty et al. (2008), precipitation data comes from Pauling et al. (2006), and temperature data from Luterbacher et al. (2004) and Xoplaki et al. (2005).

s in year t. The seasons naturally are spring, summer, autumn, and winter. $I_{o,y}^{Precip,s}$ is a similar set of indicators for precipitation. Figure 1.19 shows a scatter plot of the yearly immigrant flows predicted by climatic shocks.

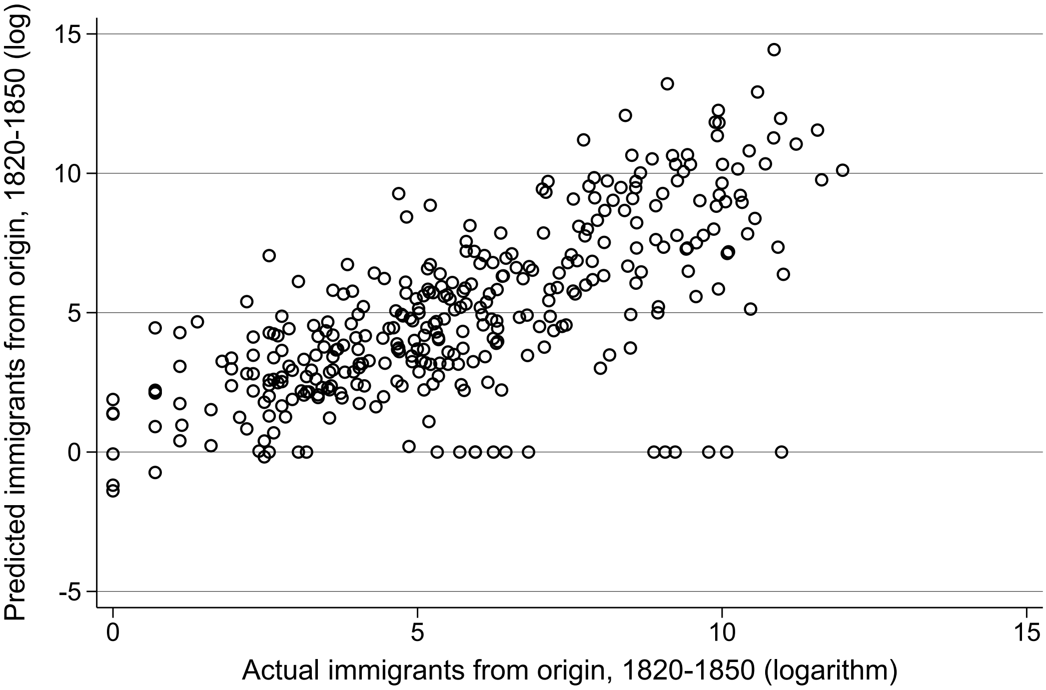

Figure 1.19: Yearly Immigration Induced by Climatic Shocks

Note: This scatter plot shows the association between the yearly immigration from European origins from 1820 to 1849 and the origin-year immigration predicted by climatic shocks only.

I use the component of yearly immigrant flows predicted by these climatic indicators and sum the actual numbers ($exp(Immig_{o,y})$) up at the decade (t)-origin level $IMMIG_{o,t} = \sum_{y \in t} exp(Immig_{o,y})$.

1.10.7.2 Zero Stage Results

Table 1.31 shows results of the zero stage regression, run at the county-origin level for all counties that were on the frontier at least once between 1820 and 1840:

$$log(IMMIG_{o,d}) = \sum_{t=1820,1830,1840} \beta_t \left(1(frontier_{d,t}) \times IMMIG_{o,t}\right) + \mu_d + \mu_o + \varepsilon_{o,d}$$

where $1(frontier_{d,t})$ is a dummy indicating whether a county was at the frontier at the beginning of a decade. $IMMIG_{o,t}$ is the total number of immigrants arriving from an origin in the rest of that decade predicted from climatic shocks in their origins. County (μ_d) and origin fixed effects (μ_o) account for features rendering locations attractive to all immigrants alike and differences across origins in their settlement patterns across counties.

I then use the predicted values to construct instruments as detailed in the paper. Figure 1.20 depicts the association between predicted and actual county-origin populations. The predicted county-origin populations are smaller than the actual values. This is particularly pronounced in counties hosting large and growing cities, as the instrument does not attempt to account for these.

Table 1.31: Zero-stage regression

	Dep. Var.: County-Origin Population 1850
	(1)
$IMMIG_o^{1820-29} \times 1(frontier_d^{1820})$	0.004**
	(0.002)
$IMMIG_o^{1830-39} \times 1(frontier_d^{1830})$	0.001***
	(0.000)
$IMMIG_o^{1840-49} \times 1(frontier_d^{1840})$	-0.000
	(0.000)
R^2	0.635
Observations	7934

Note: Regression run at county-origin level. Dependent variable is the county-origin population in 1850 and the independent variables are interactions of dummies indicating whether a county was on the frontier at the beginning of a decade and the aggregate inflow from an origin in the entire following decade pushed to emigration by climatic shocks. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

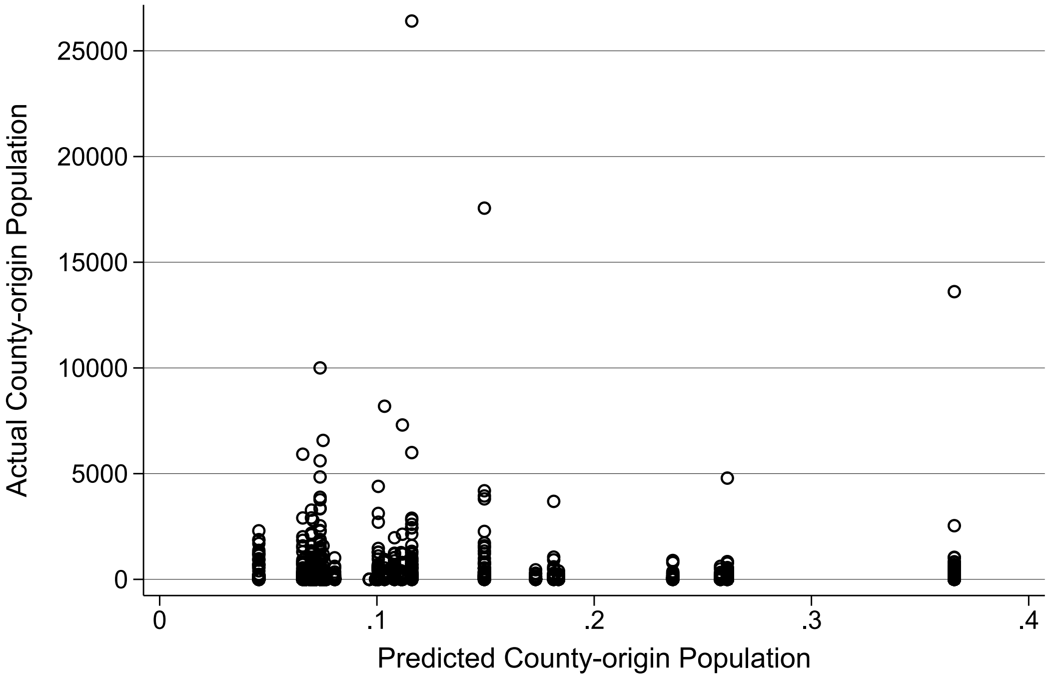

Figure 1.20: Zero stage: Predicted vs. actual County-origin Population

Note: This scatter plot shows the association between the population from European origins in US counties on the frontier and the population predicted by the movement of the frontier and the aggregate inflows by origin, pushed out by climatic shocks 1820-1840.

1.10.8 Details on Census of Manufactures, 1860-1880

The Census of Manufactures data comes from Hornbeck and Rotemberg (2019). They digitized the published county-industry data available in from published tables. In this Appendix I provide details on the data set used and the industry correspondence constructed.

1.10.8.1 Industry Correspondence

Hornbeck and Rotemberg (2019) provide concordances of the original industries used in the Census of Manufactures into a consistent set of 159 "granular industries". Based on the information in the underlying original industry names and the information in the occupations linked to IND1950 in Ruggles et al. (2019), I crosswalk these 159 industries to IND1950 industries. In most cases, the correspondence is directly evident from these. Sometimes, a one-to-one link is possible, as for "tobacco manufactures". Often, IND1950 industries map to more than one of Hornbeck and Rotemberg (2019)'s "granular industries". For instance, "Fabricated steel products" corresponds to, among others, "saws", "scales and balances", and "screws". In a few instances a granular industry potentially comprised more than one IND1950 industry (as for instance, in "yarn and cloth, other"). In these cases, I inquired into which underlying original industries dominates the granular industry and opted for the IND1950 industry corresponding to it (in this case, "Yarn, thread, and fabric mills", rather than "Apparel and accessories"). In rare instances, I link predecessor industries in the Census of Manufactures to the corresponding IND1950 industry, as in "carriages" corresponding to 'Motor vehicles and motor vehicle equipment". Further details and the full correspondence are available from the authors upon request.

1.10.8.2 Summary Statistics and Data Set

The coverage of this data is incomplete, with the degree of in completion varying across years. In 1870, the census only published information on large establishments (county-industries with output larger than 10k $, while in 1880, the published sample was further restricted to county-industries with output larger than 20k \$, and counties with output larger than 100k\$. Only in 1860 are all county-industries with at least one establishment included. See Hornbeck and Rotemberg (2019) for further detail. Table 1.32 provides summary statistics for the major variables used in the analysis. I combine "Hands employed", which are separated by gender in 1860 and 1880. "Establishments" is a count variable indicating how many establishments were recorded in a county-industry. "Worker Per Firmer" is the plain division of the former by the latter and used as a proxy for firm size at the county-industry level. "Value Added per Worker" is the division of "Value Added" by "Hands employed", whereby the former is $VA = Revenues - LaborCost - MaterialCost$.

Table 1.32: Summary statistics Census of Manufactures 1860-1880

Variable	Obs	Mean	Std. Dev.
		1860	
Log(Workers)	33204	1.24	1.63
Log(Establishments)	33204	0.71	0.98
Log(Worker per Firm)	33204	1.33	1.02
Log(Value Added per Worker)	15691	8.94	2.31
		1870	
Log(Workers)	8734	0.70	1.34
Log(Establishments)	8734	0.27	0.61
Log(Worker per Firm)	8734	2.23	1.03
Log(Value Added per Worker)	5061	3.73	5.63
		1880	
Log(Workers)	4422	0.65	1.43
Log(Establishments)	4422	0.22	0.55
Log(Worker per Firm)	4418	2.72	1.21
Log(Value Added per Worker)	2935	3.13	5.37

Note: This Table shows summary statistics of the main variables from Census of Manufacture used in the analysis. Data comes from Hornbeck and Rotemberg (2019).

1.10.9 Further Evidence on Pioneer Firms

This section provides further evidence on the main mechanism highlighted in the paper. Immigrant specialization in 1850 induces the entry of pioneer firms in the following decades. Here I show that these pioneer firms were owned or operated by both immigrants and natives (1.10.9.1). In 1910, it appears that immigrants arriving after 1850, as well as natives born to immigrant and native parents are driving the effect on the frontier, speaking to various channels at work (1.10.9.2).

I turn to the to the county-industry-*origin* level to investigate further in section 1.10.9.3. There, I first document that immigrant specialization at this level increases pioneering activity by immigrants from these origins, compared to immigrants from other origins, and to natives, in the same county-industry. This level of analysis also enables me to inquire into the effects of aggregate immigrant specialization on other origins and natives (section 1.10.9.4). In 1850, immigrant specialization from a particular origin decreases the probability of pioneering activity by natives and immigrants from other origin countries. Later, positive spillovers on other immigrants and natives prevail.

1.10.9.1 Immigrant and Native are Pioneer Entrepreneurs

Were the county-industry pioneers – those owners or managers of the first company in a particular industry to enter a location – immigrants or native-born individuals? I identify from the census if a owner is born in the United States or elsewhere and define dummies indicating the presence of at least one of them in a county-industry ($1(Owner(type)_{d,i}^{t}$) where type={native-born, immigrant}). Then I repeat the regressions of the main body of the paper with this outcome:

$$1(Owner(type)_{d,i}^{t}) = \beta ImmigrantSpecialization_{i,d} + \gamma[1(Owner_{d,i}^{1850})] + \mu_d + \mu_i + \varepsilon_{i,d}$$

where $[1(Owner_{d,i}^{1850})]$ controls for the presence of any either immigrant or native born owner in 1850 for all regressions except the one with $y = 1850$ as outcome year.

Table 1.33 shows results using 1910 as the year of the outcome. Columns 1 show significant positive effects of immigrant specialization on entry of native entrepreneurs. Column 2 shows that an insignificant and small positive effect on the entry of foreign-born pioneers. Columns 3 and 4 confirm these and find slightly larger coefficient for the frontier sample. Columns 5 and 6 present IV results. The variation in county-origin population in 1850 induced by early farmers moving to where land was available results in a strong first stage and a positive and sizable second stage relationship. Again, if anything, I find no evidence of selective migration until 1850 to places with inherent potential for later entry of manufacturing establishments. Figure 1.21 shows coefficients for the frontier sample and all years available from 1850 to 1930. The left panel shows OLS results and indicates a negative association of immigrant specialization with entry by immigrants until 1880 and by natives until 1870.

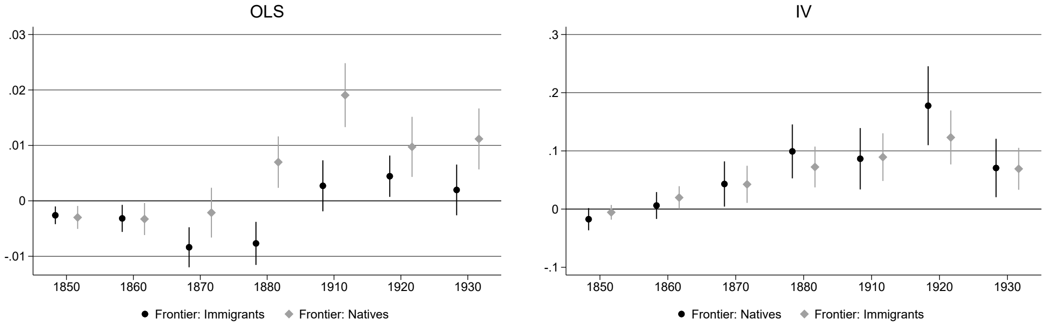

Figure 1.21: Entry of Native and Immigrant Owned Pioneer Firms in County-Industries

Note: This figure shows coefficients of regressions of immigrant specialization on the presence of owners in a county-industry in the frontier sample. Black markers indicate coefficients for immigrant (foreign-born) owners, grey ones are coefficients for native-born owners. All regressions include county and industry fixed effects,and 95% confidence intervals based on standard error errors clustered at the county level are shown.

Table 1.33: Mechanism: Both Immigrant and Native-born Owners are Pioneers

Dep. Var.: Dummy for at least one (native born/immigrant) owner/manager in County-industry, 1910

	(1)	(2)	(3)	(4)	(5)	(6)
Pioneer Birthplace	Native	Immigrant	Native	Immigrant	Native	Immigrant
Sample	Full		Frontier			
Estimation	OLS				IV	
Log(1+Immigrant Specialization$_{d,i}$ 1850)	0.014***	0.001	0.019***	0.003	0.123***	0.094***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.031)	(0.024)
1(Owner$_{d,i}$ 1850)	0.038***	0.075***	-0.018*	0.060***	-0.015	0.063***
	(0.006)	(0.006)	(0.010)	(0.012)	(0.010)	(0.011)
R^2	0.483	0.387	0.482	0.357		
Observations	113239	113239	51205	51205	51205	51205

Note: All regressions are run at the county-industry level. The dependent variables is a dummy indicating whether at least one owner/manager in a county-industry resided in a county (IPUMS occupation code 290) who was of foreign or native birth. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods in 1909, and in columns 3 onward further only counties that where on the frontier of settlement between 1820 and 1840. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

1.10.9.2 Immigrant and Native are Pioneer Entrepreneurs - Heterogeneity

Immigrant specialization in 1850 could induce pioneers to open up establishments through many potential channels.

First, immigrants arriving before 1850 could have set up shop early on. From the existing results it appears that this likely was not the case. If anything, the places where immigrants settled until then had less employment and were less likely to have an establishment in industries in which the immigrants potentially had a comparative advantage in. However, these early immigrants could have profited from early access to information available in their origin countries and started establishments after 1850. After 1880, the digitized census of IPUMS contains the year of immigration of each immigrant. I use this to identify whether an immigrant pioneer arrived before or after 1850. Columns 1-4 of Table 1.34 show OLS and IV results for those immigrant entrepreneurs arriving after or before 1850 separately. The positive baseline association is in its entirety driven by immigrants arriving after 1850, speaking to the importance of immigrant specialization in attracting future entrepreneurs from the European origins rather than the 1850 themselves being or becoming the manufacturing pioneers. Note however, that the late (yet earliest possible) outcome year of 1910 is a concern for such a conclusion. Earlier immigrants might well have founded such establishments (after 1850 however, by the earlier results), but passed them on to their (immigrant or native-born) sons or other individuals before 1910.

Another channel is that immigrant specialization might have enabled native born individuals to profit from immigrant-provided skilled labor or knowledge links to technological developments or business partners in the origin countries transmitted horizontally. The latter links were likely stronger for second-generation Americans, the children of immigrants from these origins. To this end, I rely on the census variable containing the national origin of the owner's father. Columns 5 to 8 of table 1.34 document that both channels appear at work. The presence of native-born owners, both that with immigrants and native-born fathers, is higher in county-industries with higher immigrant specialization in 1850.

Table 1.34: Mechanism: Early Entry of County-Industries - Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dep.Var.: Dummy indicating presence of ...							
	immigrant owner arriving...				native born owner with ...			
	before 1850	after 1850	before 1850	after 1850	immigrant father	native father	immigrant father	native father
Sample	Frontier							
Estimation	OLS		IV		OLS		IV	
Log(1+Immigrant Specialization, 1850)	-0.000	0.003	-0.023***	0.099***	0.008***	0.019***	0.052**	0.122***
	(0.001)	(0.002)	(0.006)	(0.024)	(0.002)	(0.003)	(0.022)	(0.035)
1(Owners, 1850)	0.013***	0.059***	0.012***	0.062***	0.047***	-0.012	0.048***	-0.009
	(0.005)	(0.011)	(0.005)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
R^2	0.075	0.355			0.402	0.454		
Observations	51205	51205	51205	51205	51205	51205	51205	51205

Note: All regressions are run at the county-industry level. The dependent variables is a dummy indicating whether at least one owner/manager in a county-industry resided in a county (IPUMS occupation code 290) who had the characteristics detailed in the header. The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods in 1909. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

1.10.9.3 Pioneering Entrepreneurs from Specific Origin Countries

Did immigrant specialization increase the probability that industrial pioneers from specific origin countries entered in a county? Thus far the analysis was at the county-industry level. To answer this question, I turn to the county-industry-origin level. Thus, the measure of immigrant specialization now is - for origin o, in industry i, and destination county d - simply the multiplication of the number of individuals born in o and residing in d in 1850 with the origin country's comparative advantage in industry i:

$$ImmigrantSpecialization_{d,i,o} = IMMIG^{1850}_{o,d} \times RCA^{1909}_{o,i}$$

I estimate linear probability models of the following type:

$$\begin{aligned} 1(Owner^{t}_{d,i,o}) = \beta log(1 + ImmigrantSpecialization_{d,i,o}1850) + \gamma 1(Owner^{1850}_{d,i,o}) \\ + \mu_d \times \mu_i + \mu_o \times \mu_d + \mu_o \times \mu_i \\ + \varepsilon_{d,i,o} \end{aligned} \tag{1.1}$$

where $1(Owner^{t}_{d,i,o})$ is a dummy indicating the presence of at least one owner/manager in industry i present in county d in year t and born in European origin o. A highly restrictive set of fixed effects is employed. The main coefficient of interest, β is identified only off variation within county-industries (holding fixed differences in natural advantage and market access pertaining to specific industries, but to immigrants from all origin countries alike), origin-counties (e.g. the size of immigrant communities), and origin-industries (e.g. accounting for particular origins having a comparative advantage equally in all counties).

Table 1.35 shows OLS results for the frontier sample in columns 1 to 3. I document an initially negative and then increasingly positive effect of immigrant specialization at the county-industry-origin level on the probability that an immigrant born in a specific origin country owns or operates a manufacturing establishment in that county-industry. The left panel of figure 1.22 shows coefficients for all available years; both for the frontier and the full sample. The positive baseline association between immigrant specialization and entry forms later on the frontier, and is not present in 1850 already. Columns 4 to 6 of table 1.35 show IV results for 1850, 1880, and 1910, pointing to an earlier entry based on the instrument and a significantly lower probability of entry on the frontier in 1850. The right panel of figure 1.22 confirms this. Note that - at the individual origin level and after accounting for this rich set of fixed effects - the instrument is not particularly strong.

In sum, there is some evidence that immigrant specialization induced the entry of pioneers in county-industries by immigrants from these origins. In line with the earlier results, it appears that this is driven by immigrants becoming manufacturing pioneers after 1850 only.

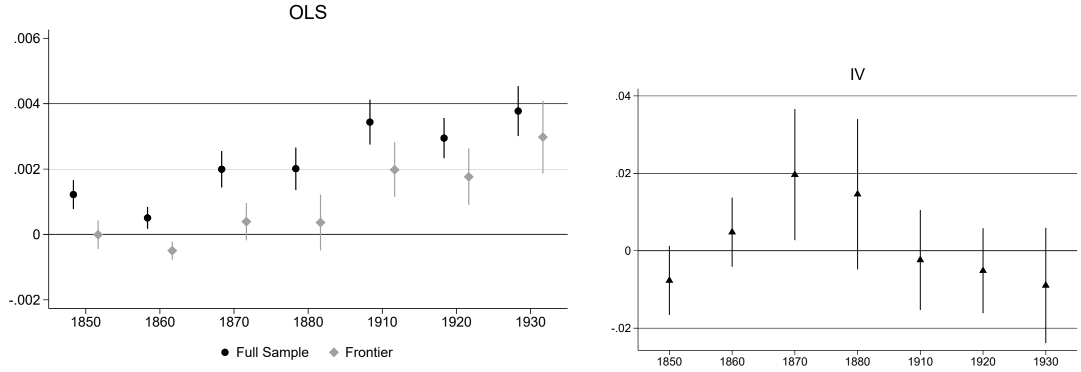

Figure 1.22: Entry of Owners From Specific Origin

Note: This figure shows coefficients of regressions of immigrant specialization (at the county-industry-origin level) on the presence of owners born in a origin active in a county-industry. Black markers indicate coefficients for the full sample in the left panel, while grey ones indicate the frontier sample. The right panel shows IV estimates. Confidence intervals (95%) based on standard error errors clustered at the county level are shown.

Table 1.35: Mechanism: Early Entry of County-Industries By Origin

Dep.Var.: Dummy indicating presence of owner/manager born at origin-industry-county level

	(1)	(2)	(3)	(4)	(5)	(6)
Year of Dep. Var.	1850	1880	1910	1850	1880	1910
Sample	Frontier					
Estimation	OLS			IV		
Log(1+ Immigrant Specialization$_{d,i,o}$ 1850)	-0.000	0.000	0.002***	-0.009*	0.015	-0.005
	(0.000)	(0.000)	(0.000)	(0.005)	(0.009)	(0.006)
1(Owner$_{d,i,o}$ 1850)		0.082***	0.063***		0.082***	0.063***
		(0.014)	(0.011)		(0.014)	(0.011)
R^2	0.228	0.350	0.334			
First Stage F-Statistic				10.4	10.4	10.4
Observations	665,665	665,665	665,665	665,665	665,665	665,665

Note: All regressions are run at the county-industry-origin level. The dependent variables is a dummy indicating whether at least one owner/manager in a county-industry resided in a county (IPUMS occupation code 290) and was born in one 13 European origins origin. The sample includes all counties with non-zero population in 1850 and on the frontier at least once between 1820 and 1840, all industries with corresponding traded goods in 1909, and 13 European origins with trade data available. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

1.10.9.4 Spillovers to Natives and others

The evidence on native-born pioneers born to native-born fathers speaks to spillovers of immigrant specialization on other locals. These might have profited from the skilled labor force attracted or provided by early immigrants, or by informal or cultural information networks being accessible due to their presence. Here, I inquire into these spillovers at the county-industry-origin level.

First, I consider spillovers of aggregate immigrant specialization in a county-industry on European immigrants. To do so I focus on the 13 European origins for which comparative advantage data is available and estimate linear probability models of the following type:

$$\begin{aligned} 1(Owner^{t}_{d,i,o}) = \beta_1 log(1 + ImmigrantSpecialization_{d,i,o}1850) \\ +\beta_2 log(1 + \sum_{o} ImmigrantSpecialization_{d,i,o}1850) \\ +\gamma_1 log(1 + Employment_{d,i,o}1850) \\ +\gamma_2 log(1 + \sum_{o} Employment_{d,i,o}1850) \\ \mu_o \times \mu_d + \mu_o \times \mu_i \\ +\varepsilon_{d,i,o} \end{aligned} \tag{1.2}$$

where β_1 is the coefficient of origin-level immigrant specialization in a county-industry, and β_2 is the coefficient of aggregate county-industry level immigrant specialization. I also control for employment in 1850 at the county-industry-origin and county-industry level. Standard errors are clustered at the county level and I exclude all earlier fixed effect interactions except that at the level of aggregate county-industry immigrant specialization. Column 1 of Table 1.36 shows evidence of negative spillovers of aggregate immigrant specialization at the county-industry level on the probability that an individual born in one of the 13 European origins is active as an owner or manager in that county-industry. Column 2 documents that such negative spillovers prevail until 1910.

Second, I analyze spillovers on native born individuals and immigrants from other origins (grouped together into one origin group o). For these I estimate above models excluding the unavailable specialization of these origins:

$$
\begin{aligned}
1(Owner^t_{d,i,o}) = \beta log(1 + \sum_o ImmigrantSpecialization_{d,i,o}1850) \\
+\gamma_1 log(1 + Employment_{d,i,o}1850) \\
+\gamma_2 log(1 + \sum_o Employment_{d,i,o}1850) \\
\mu_o \times \mu_d + \mu_o \times \mu_i \\
+\varepsilon_{d,i,o}
\end{aligned}
\tag{1.3}
$$

Column 3 shows that there is no significant evidence for spillovers in 1850. In county-industries with higher immigrant specialization by 1850, native-borns and immigrants from other origins are no more or less likely to operate or own firms. Column 4 shows that by 1910, positive spillovers exist. Native born and immigrants from other origins are significantly more likely to operate or own companies in county-industries with higher 1850 immigration specialization.

In sum, this evidence cautiously supports the view that for immigrants from other European origins negative spillovers prevailed – they are significantly less likely to become pioneers when aggregate immigrant specialization is high – throughout, while native-born and other immigrants benefited from positive spillovers.

Table 1.36: Mechanism: Entrepreneurship Spillovers on Other Immigrants and Natives

Dep.Var.: Dummy indicating presence of owner/manager born at origin-industry-county level

	(1)	(2)	(3)	(4)
Year of Dep. Var.	1850	1910	1850	1910
Sample	European Immigrants		Natives & Other Immigrants	
log(1+Immigrant Specialization$^{1850}_{d,i,o}$)	0.002***	0.003***		
	(0.000)	(0.000)		
log(1+$\sum_o$Immigrant Specialization$^{1850}_{d,i,o}$)	-0.000*	-0.001***	-0.001	0.005***
	(0.000)	(0.000)	(0.001)	(0.001)
log(1+Employment$^{1850}_{d,i,o}$)		0.062***		0.009***
		(0.003)		(0.003)
log(1+ $\sum_o$ Employment$^{1850}_{d,i,o}$)	0.005***	0.008***	0.040***	0.030***
	(0.000)	(0.000)	(0.001)	(0.002)
R^2	0.186	0.244	0.377	0.517
R^2 (within)	0.004	0.009	0.028	0.006
Observations	1472107	1472107	226478	226478

Note: All regressions are run at the county-industry level. The dependent variables is a dummy indicating whether at least one owner/manager in a county-industry resided in a county (IPUMS occupation code 290). The sample includes all counties with non-zero population in 1850 and all industries with corresponding traded goods in 1909. All regressions include county and industry fixed effects. Standard errors clustered at the county-level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

1.10.10 Industry Concordances County Business Patterns 2000

Data for U.S. county-industry employment in 2000 comes from Eckert et al. (2020a). They impute the non-disclosed cells by exploiting adding-up constraints in the released County Business Patterns data set of the Census bureau.

Industries are reported in the NAICS 1997 classification in the CBP data. The main regression data set uses IND1950 manufacturing industries coming directly from the full count census (Ruggles et al., 2019). I am not aware of any direct correspondence available between IND1950 and NAICS 19997 industries and hence construct a correspondence for these.

I start by crosswalking IND1950 to IND2000 industries. For this I rely on the information provided by IPUMS.[68] For instance, IND1950 industry 467, "Drugs and medicine" directly corresponds to IND2000 industry 219. Other IND1950 industries correspond to more than one IND2000 industry, as for instance IND1950 307, "Sawmills, planing mills, and mill work", which corresponds to IND2000 industries 377, 378, and 387.

Next, I crosswalk IND2000 industries to NAICS1997 industries relying again on information provided by IPUMS.[69] The three IND2000 corresponding to IND1950 307, "Sawmills, planing mills, and mill work" are comprised by the NAICS1997 industries 3211, 3212, 32191, 32192, and 321999. Note that these industries now vary at their aggregation level. Combined with the IND1950 to IND2000 correspondence, this results in a crosswalk from NAICS1997 industries at different levels of aggregation to IND1950 industries

I therefore proceed successively from lower to higher levels of aggregation in the CBP data. I first assign the corresponding IND1950 industry to six-digit NAICS1997 county-industry employment observations, and proceed until the three-digit level. Note that the crosswalk is designed such that the mapping from NAICS1997 to IND1950 is completely exclusive. This means that if, say, NAICS 31182 links to an IND1950,

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[68] `https://usa.ipums.org/usa/volii/occ_ind.shtml`

[69] `https://usa.ipums.org/usa/volii/indcross.shtml`

NAICS 3118 has no corresponding IND1950. From this I aggregate the CBP data at the county-industry level with IND1950 being the industry classification.

CHAPTER 2

Political Threat and Racial Propaganda: Evidence from the U.S. South

2.1 Introduction

Inflammatory propaganda in mass media can play an important role in political outcomes and violence. This is particularly the case when autocratic elites resort to hate creating stories that target certain ethnic or religious minorities. For example, hate stories broadcast by government-backed radio stations persuaded Hutu individuals to join the killings of Tutsis in the Rwandan genocide and stirred anti-Jewish violence in Nazi Germany (Yanagizawa-Drott, 2014; Adena et al., 2015). While mounting evidence suggests that propaganda can have serious consequences, we know less about its determinants. Natural drivers of propaganda may be past or current crimes committed by members of the targeted group or an evil ideology among members of the elite in power. In this paper, we take a different perspective. We investigate the possibility that propaganda may be the result of deliberate strategy responding to political threat.

Political threat refers to the fear among members of a dominant group of giving up political control and resources to a minority group. The work of Blalock (1967) on minority-group relations suggests that when two groups coexist with unequal access to political resources, the dominant group will engage in a wide variety of methods, including propaganda, to secure its privileged access to those resources. Building on this insight, models of the 'supply of hatred' formalize the conditions under which political threat may be an important driver of hate creating propaganda. According to Glaeser (2005), if a minority group fully supports one of two rival parties, then the other party may resort to propaganda that stirs resentment against the minority group to prevent the majority of the electorate from voting for the opponent. This logic suggests that hateful propaganda is part of the toolkit of political actors who seek to divide diverse coalitions.[1]

The best evidence in support of this hypothesis comes from studies examining the dynamics of anti-Black propaganda in the U.S. South. Woodward (1955) describes a rise in anti-Black antagonism after the Civil War, which was "was furthered by a sensational press that played up and headlined current stories of Negro crime, charges of rape and attempted rape, and alleged instances of arrogance." Glaeser (2005) shows that the frequency of anti-Black articles in the *Atlanta Constitution* increased between 1870 and the early 1900s and fell afterward until after World War I. He also observes that these trends coincided with changes in the political landscape and, in particular, with the rise and fall of the People's Party, also known as the Populist Party. The Populists were the first American party committed to redistribution from rich to poor. They sought support among poor farmers, regardless of race, and advocated redistributionist policies that would have disproportionately benefited the poor, including African Americans. Their alliance with black voters was crucial for the success in the 1892 elections and threatened the dominant position of the Democratic elites in the South.

In this paper, we test whether the relationship between political threat due to the emergence of the Populist Party and the use of anti-Black propaganda in the media is causal. Direct evidence has proved elusive for two reasons. First, a systematic empirical analysis requires measurement of propaganda in the media, and thus detailed information on media content. Such data sets have been unavailable until recently. Second, credible estimates of the effect of political threat require an estimation strategy that deals with the multitude of unobserved factors that may affect both political threat and propaganda. To make progress, we collect new fine-grained measures of anti-Black propaganda by accessing the full text of several hundred Southern newspapers over many decades, ranging from rural weeklies to big-city dailies. We measure propaganda by counting the frequency of the word "rape" in co-occurrence with the word "negro" on

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[1] Models of social identities such as Shayo (2009) also generate this hypothesis.

the same page relative to the total number of newspaper pages.[2] Since newspapers were the dominant mass media at the time and highly local in their readership, they are the ideal source to measure variation in the supply of propaganda at the local level and over time.

To identify the effect of political threat on the spread of anti-Black propaganda in newspapers, we exploit variation in the Populists' unexpected success in the 1892 presidential elections in a difference-in-differences setting. Specifically, the Populists' success varied from state to state and even between counties within states. Where they gained votes, the Populists posed a more salient political threat to the local Democratic elites, providing them with an incentive to turn poor white farmers against blacks by fanning racial fears and spreading hatred (Du Bois, 1935; Woodward, 1955). In our baseline analysis, we define that local Democrats perceived political threat if the Populists gained a non-zero vote share in their county in the presidential elections of 1892. We then compare newspapers from counties where Democrats experienced threat to newspapers from counties where they were not (first difference), before and after 1892 (second difference). Importantly, our strategy allows us to include newspaper fixed effects, which remove time-invariant newspaper traits, including newspaper ideology. We find that newspapers in counties under political threat see a statistically and economically significant increase in propaganda relative to newspapers in other counties after 1892. Importantly, we find this effect only in newspapers that endorsed the Democrats in the presidential elections, but not in newspapers affiliated with the Republicans, the Populists, or independent newspapers. The spread of propaganda remains significantly higher until the early 1900s and abates afterward. This decline likely reflects the collapse of the Populist Party in the years after the 1896 election, which reduced the political incentive for Democrats to stir racial hatred.

Identification in our difference-in-differences specification rests on the assumption of parallel trends: absent political threat due to the rise of the Populist Party, newspapers

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[2] The keyword selection is guided by Glaeser (2005) who uses a similar combination of keywords in his analysis of anti-Black articles in the *Atlanta Constitution.* Anti-Black propaganda was often propagated through stories of attacks by Blacks on the White community, often involving allegations of rape.

in counties where the Populists gained votes would not have experienced a differential increase in propaganda. To assess the plausibility of this assumption in our context, we estimate a dynamic difference-in-differences specification. We find no evidence for a pre-trend. Before the 1892 elections, anti-Black propaganda did not change differentially in counties exposed to vs. counties not exposed to political threat.

Another obvious concern with our result is that local Populist vote shares are not random. Determinants of anti-Black propaganda that also correlate with the local presence of the Populist Party may violate the parallel trends assumption of the difference-in-difference strategy. In particular, the Populists were more successful in counties that suffered from the economic downturn in the 1880s and 1890s (Eichengreen et al., 2019). It is conceivable that this economic distress gave rise to differential dynamics in anti-Black sentiment. To address this concern, we flexibly control for the effects of differences in local economic conditions. Specifically, we include a broad set of socio-economic county characteristics, interacted with year dummies, as control variables. The result corroborates our finding: the Populist political threat increases the prevalence of propaganda in newspapers affiliated with the Southern Democratic elite.

We also provide evidence in support of the interpretation that the effect is driven by the supply of propaganda. First, our evidence suggests that it is unlikely that local demand for racist stories drives the effect. Newspaper fixed effects remove time-invariant differences in demand across newspapers. Moreover, we control for the county-level Democrat vote shares in 1892, interacted with year dummies. The vote share serves as a proxy for local demand for anti-Black propaganda, and the interaction with year dummies flexibly removes demand effects that vary over time. Again, the result is very similar, supporting the interpretation that the supply of propaganda plays an important role in our setting. The finding is also consistent with the evidence in Gentzkow et al. (2015), who demonstrate that the Reconstruction South was the only place and period in American history during which state-level politics significantly affected newspaper circulation and political affiliation. It is precisely this political control of newspapers that we build our analysis on, and that makes a supply-side interpretation plausible. Second, an increase in real rape crimes is unlikely to account for the effect. We replicate the analysis using the extent to which newspapers report about rapes unrelated to African Americans as outcome variable. The coefficient of this placebo test is statistically indistinguishable from zero and, if anything, points in the opposite direction.

In light of the theory, we expect that political threat was felt more strongly in places where the Southern Democratic elite had more to lose from the redistributionist policies that the Populists advocated. We probe into the heterogeneity of the effect of political threat on anti-Black propaganda using the average size of farms in counties as a proxy for white wealth. The result suggests that the effect is stronger in counties with larger farms, and the magnitude of the heterogeneity is large: a one standard deviation increase in farm size increases the effect by 31.5%. Moreover, the effect is stronger in counties with a larger population share of African Americans, where the threat may have been more salient. As a placebo test, we also examine the effect of the Populist Party on anti-Black propaganda in newspapers outside the Southern states, where few African Americans lived. The Populists thus competed without relying on the support of black voters. Using the same empirical specification, we find no evidence that Populism per se affected anti-Black propaganda in newspapers.

Finally, we document that the propaganda wave in Democratic newspapers is associated with electoral gains for the Democratic party in subsequent elections. In particular, anti-Black propaganda in the years 1892 - 1894 had a lasting impact on future voting outcomes, while we find no evidence for an association before and after this period. This finding suggests that the propaganda "worked".

In sum, our results suggest that Southern Democratic elites responded to the emerging Populist threat by spreading anti-Black propaganda in local newspapers and that the propaganda was politically successful: counties with a larger increase in propaganda see stronger gains for the Democrats by 1900.

Our findings contribute to several strands of literature. A series of theoretical papers have formalized the idea, which goes back to Machiavelli, that elites may find it optimal to use a divide-and-rule strategy to remain in power against challenges. In Acemoglu et al. (2004), kleptocratic elites bribe pivotal groups to undermine competing alliances;

in Padró i Miquel (2007), the fear of being ruled by elites who favor a different group drives voters to accept rent-extracting policies by their elites, even as those reduce their welfare; and in Shayo (2009), rich elites may appeal to voters nationalist identity to implement less redistributive policies. Closest to our setting is Glaeser (2005), who studies the supply of hate stories by politicians and voters' demand for such stories. We provide robust empirical evidence that the Southern Democratic elites circulated hate stories in the primary mass media of the time to divide an alliance of black and white voters.

We further contribute to an empirical literature that studies the economics of persuasion (reviewed in DellaVigna and Gentzkow, 2010) and the short- and long-run consequences of propaganda (DellaVigna et al., 2014; Yanagizawa-Drott, 2014; Cantoni et al., 2017; Durante et al., 2019; Bursztyn et al., 2019). We exploit a historical setting where newspapers were the single dominant source of information, and demand effects are less powerful because newspaper markets were often under tight political control (Gentzkow et al., 2015). Our study shows that political threat can be an important determinant of propaganda.

Our study also adds to papers and books on the economic history of the Postbellum South, including racism and political repression of African Americans during the Reconstruction period (e.g., Du Bois, 1935; Woodward, 1955; Margo, 1982; Foner, 1997; Cook et al., 2018). Our findings suggest that the rise and fall of hatred of Blacks in the late 19th and early 20th century was a political response to the redistributionist Populist movement of the 1890s. By measuring anti-Black propaganda from newly digitized historical newspapers, we provide a new large-scale data source that, we hope, will be helpful to many researchers in the field.

Finally, we contribute to a vast body of work spanning the fields of sociology, psychology, political science, and economics that investigates the historical origins of racism in the United States. While recent contributions highlight the importance of deep factors in shaping local racist attitudes today (Acharya et al., 2018; Williams, 2019), our findings suggest that even a short-lived spike in politically motivated propaganda affected racial attitudes for decades to come.

The rest of the paper proceeds as follows: Section 2.2 briefly describes the historical background, the features of the rise and fall of the Populist Party, its political platform, and how it created political threat for the incumbent Democratic Party in the South. Section 2.3 describes our newspaper data set and how we measure anti-black propaganda and political threat at the local level. Section 2.4 lays out the empirical strategy, discusses the identification assumptions, and presents the main results and robustness checks. Section 2.5 investigates the extent to which the increase in propaganda affected electoral outcomes in the following decades. Section 2.6 concludes.

2.2 Historical Background

Three key features of the rise and fall of the Populist Party in the late 19th century make it an ideal context to the effect of political threat on the spread of propaganda in mass media. First, the Populists' success in the 1892 election was unexpected and varied at the local level, providing us with a natural experiment. Second, the Populists initially sought support among poor farmers, regardless of race, and advocated redistributionist policies that would have disproportionately benefited African Americans. This diverse coalition and the Populists' redistributionist policy demands map precisely into the conditions under which political threat may become an important driver of hate creating propaganda. Third, the historical account widely agrees that the incumbent Democrats perceived the Populists as a serious political threat to their dominant position in the U.S. South. This perception provided the Democratic political elites with an incentive to turn poor white farmers against blacks by fanning racial fears and spreading hatred. We now describe these points in detail.[3]

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[3] We purposefully restrict the scope of this section to the historical features that are key to our research question and the empirical analysis. Hicks (1931) and Goodwyn (1978) provide excellent histories of the Populist Party. Du Bois (1935), Woodward (1955), and Hahn (2003) trace the history of the African American political struggle in the U.S. Abramowitz (1953), Meier (1956), Shapiro (1969), and Saunders (1969) are excellent examples from a large literature discussing the political role of African Americans during the time of the the Populist party.

2.2.1 The Rise of the Populist Party

The rise of the Populist Party as a significant political force in the South was unexpected. The depression of the 1880s gave rise to several grass-root organizations of dissatisfied farmers that blamed deflationary monetary policies and the monopoly power of railroad companies for the dire economic situation of many farmers. Across the country, numerous local self-help groups sprang up. These groups met at national and regional conventions to discuss ways to influence policy by co-opting the major political parties. Yet, the formation of a new party was not the goal until the early 1890s as many Southern participants at these conventions opposed the idea.

> Led by Leonidas F. Livingston of Georgia, a number of southern delegates made it perfectly plain that they would never consent to any program that would threaten the unity of the white vote in the South and they promised to bolt the convention should such action be taken. To avoid disruption, therefore, the third party decision was waived and the convention devoted itself to the business of drawing up a satisfactory list of demands. (Hicks (1928))

Overcoming this opposition, the Farmers' Alliance established a full-fledged party before the 1892 election, where the Populist candidate James Weaver won 8.5% of the national vote and garnered much support in the South. Figure 2.5 displays the county vote shares in the South.

2.2.2 The Populists' Political Platform

The Populists advocated redistributionist policies. Their 1892 party program highlighted inequality as a major concern:

> The fruits of the toil of millions are boldly stolen to build up colossal fortunes for a few, unprecedented in the history of mankind; and the possessors of those, in turn, despise the republic and endanger liberty. From the same

> prolific womb of governmental injustice we breed the two great classes - tramps and millionaires.

Their demands included a graduated income tax, nationalization of the railroads, telegraphs, and postal system, and an eight-hour workday. To alleviate the debt burden of poor farmers, the Populists also called for reforms to monetary policies, including the free coinage of silver.

> The national power to create money is appropriated to enrich bondholders; a vast public debt payable in legal tender currency has been funded into gold-bearing bonds, thereby adding millions to the burdens of the people.

Moreover, particularly in their early years, the Populists catered to African Americans in the South. In many counties, African Americans even served as local candidates and were given a voice in the party organization. This catering to the African Americans was in part political arithmetic, in part reflection of an egalitarian conviction, and often both:

> I am in favor of giving the colored man full representation. (...) He is a citizen just as much as we are, and the party that acts on that fact will gain the colored vote of the South. (President of the Texas Populists, cited in Woodward (1981))

According to Du Bois (1935), the potential gains from building an alliance of white and black labor in the South were clear:

> White labor in the South began to realize that they had lost a great opportunity, that when they united to disfranchise the black laborer, they had cut the voting power of the laboring class in two. White labor in the Populist movement of the eighties tried to realign the economic warfare in the South and bring workers of all colors into united opposition to the employer.

However, Populist support for African Americans faded over time. Some Populists dropped their attempts to recruit Black voters and endorsed both anti-Black policies and racial hatred after 1900. Thomas E. Watson, the Populist nominee for vice-president in the 1896 Presidential election, is a case in point. He turned from an outspoken supporter of black enfranchisement in the 1890s into a white supremacist after 1900. However, these changes typically occurred after the 1890s, the period of our empirical analysis.

2.2.3 The Populist Threat

Southern Democrats perceived the alliance between Populists and Black as a critical threat to their dominant position in the South. According to the historical account, many Democrats responded by fanning racial hatred, often in the form of newspaper stories of attacks of Blacks on the White community. Their goal was to prevent African Americans from voting and scare poor whites of negro domination if the Populists were to take control.

> Alarmed by the success that the Populists were enjoying with their appeal to the Negro voter, the conservatives themselves raised the cry of 'Negro domination', and white supremacy, and enlisted the Negrophobe elements. (Woodward (1955))

In several states in the South, Democratic governments also enacted laws that effectively disenfranchised African Americans and poor Whites, the Populists' most important supporters.

At the national level, the Democrats managed to co-opt the Populist party by taking over some of their policy platforms. While this co-option led to the fall of the Populist party in national politics after the 1896 election, several local Populist organizations continued to be active into the early 1900s. For example, the Populists of North Carolina made it into government by forming a coalition with the Republicans and stayed in office until after 1900.

2.3 Data and Measurement

Our difference-in-differences empirical strategy compares the prevalence of anti-Black propaganda in newspapers from counties where the Populists were politically active in the 1892 presidential election to counties where the Populists did not receive any votes. This empirical strategy requires the measurement of propaganda in newspapers over time and county-level data on electoral outcomes. This section describes our data source for newspaper content, the method to measure anti-Black propaganda, spatial and temporal patterns in this measure, and the definition of political threat at the county level.

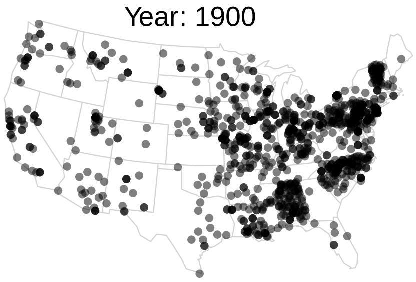
2.3.1 Newspaper Data

To investigate the occurrence of anti-Black propaganda across newspapers and over time, we draw on text data from *newspapers.com*, an extensive digital archive of historical and current newspapers. The provider scans newspapers and generates text using optical character recognition (OCR). The database is one of the most comprehensive digital newspaper archives currently available: it contains more than 550 million pages from over 17,000 newspapers – ranging from big-city dailies to rural weeklies.

We have developed an automated script that accesses the database via a personal subscription and downloads keyword frequencies. Specifically, we obtain the number of pages that a specified keyword appears on in a given newspaper and year. The script also allows us to search for co-occurrences of several keywords on the same page. We link the keyword frequencies to meta information of newspapers, including the place of publication for each newspaper recorded by *newspapers.com* and its longitude and latitude. Based on this information, we match each paper to a state and county.

Figure 2.1a displays the geographic distribution of newspaper locations in the data set over time. More than 1,300 U.S. counties have at least one newspaper at some point in the database. Importantly, the circulation of these newspapers was often highly local, typically limited to a single county. Thus, we interpret newspaper location as a proxy for newspaper coverage.

(a) Location of publication of newspapers in full database



(b) Southern counties with any newspaper between 1885 and 1903

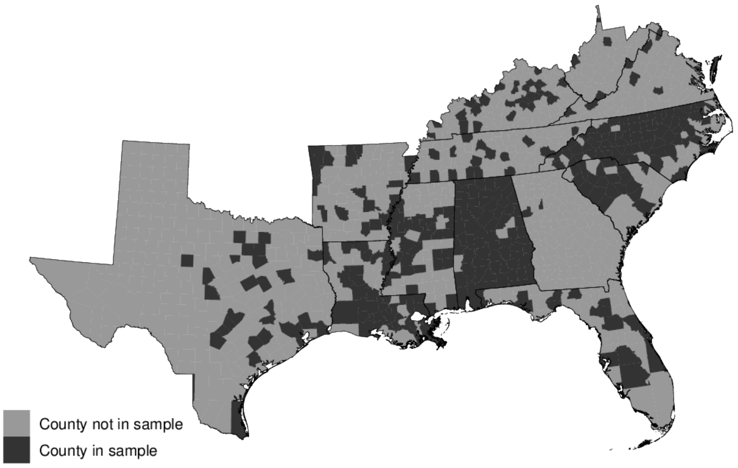


Figure 2.1: Geographic coverage of newspaper data set

Notes: Top panel: The map shows the locations of newspapers available from *newspapers.com* for 1900 and 1970. We exclude newspapers from Kansas because they are massively overrepresented in the database. Bottom panel: The map shows counties in the U.S. South for which we have newspaper data. Counties in dark (light) grey (do not) have newspapers at least once at any time between 1885 and 1903 and are (not) part of the analysis. Our coverage represents 42% of the population in Southern states.

The database comes with two shortcomings. First, it does not contain the universe of U.S. newspapers. When comparing the characteristics of counties with and without newspapers in the database, we find that counties with newspapers are more likely to be urban, have a higher share of African Americans, and more manufacturing output per capita (unreported). Moreover, not all titles have a complete run of issues digitized. Some titles only have one issue, while others have thousands. This unbalancedness may cause problems for our estimation strategy if selective entry or attrition of newspapers is systematically related to our outcome and both differences. We will address these concerns in the analysis by assessing our estimates' sensitivity to different sample definitions.

Second, the database does not allow a search for keywords within specific types of newspaper content, such as editorials or letters to the editor. While it is impossible to read all of the content carefully, we verified the content of a random sample of 100 pages. Appendix Figure 2.9 presents two examples.

For our empirical analysis, we restrict the database to newspapers published in the U.S. South between 1885 and 1903, the years around the presidential election of 1892. We are left with a sample of 764 newspapers in 329 counties, representing 42% of the population in the Southern states. Figure 2.1b displays the geographic coverage of the resulting data set. We obtain particularly good geographic coverage for the states of Alabama, Louisiana, and North and South Carolina.

2.3.2 Measurement of Anti-Black Propaganda

We measure anti-Black propaganda by implementing a word count exercise. For each year and newspaper, we count frequencies of the keywords "rape AND negro". In other words, we measure the presence of the word "rape" co-occurring with the word "negro" on the same page.[4]

To control for changes in the size of newspapers and coverage of the database, we also measure frequencies of the terms "monday OR tuesday OR wednesday OR thursday OR friday OR saturday OR sunday". We compute our measure of anti-Black propaganda

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[4] The keyword selection is guided by Glaeser (2005) who uses the same combination of keywords in one of his analyses of anti-Black propaganda in the Atlanta Constitution.

as

$$Anti\text{-}Black\ Propaganda_{i,t} = \frac{\sum_{n=1}^{N} n_{i,t} \times \mathbb{1}(rape\ AND\ negro)}{\sum_{n=1}^{N} n_{i,t} \times \mathbb{1}(weekdays)} * 100 \tag{2.1}$$

where n is the number of pages containing the keywords in newspaper i and year t. We multiply the resulting numbers by 100 to interpret *Anti-Black Propaganda* as the fraction of newspaper pages containing anti-Black propaganda in a specific newspaper and year.

Two issues with the measure are worth pointing out. First, the method of counting keyword frequencies on a page is dictated by our data source. The database structure prevents us from using more sophisticated methods to measure anti-Black propaganda in the newspapers.

Second, the resulting measure is a combination of reporting of (local and distant) rapes that occurred, their amplification by the local press, op-eds, letters to the editors, and fabrications. Building on the historical accounts, we argue that the bulk of the variation in the measure reflects *differential* reporting about local rapes, coverage of national headlines, op-eds, letters, and fabrications. We will come back to this point in the analysis. Specifically, we will provide evidence that the measure does not merely reflect the reporting of local rapes.

2.3.3 Descriptive Analysis

What are the spatial and temporal patterns of anti-Black propaganda in US local newspapers? Figure 2.2 shows the geography of anti-Black propaganda, averaged from 1870 to 1965, using counties as the unit of observation. It shows the cross-county distribution of deviations from yearly averages, recovered as the residuals from regressing our propaganda measure on year fixed effects. Darker red colors indicate above-average anti-Black propaganda in a particular county, while darker blues indicate below-average values of anti-Black propaganda in newspapers of a county. No data are available for counties in grey.

The map reveals two striking features. First, there are pronounced differences across regions. Anti-Black propaganda is more common in the South as compared to the rest of the country. Particularly states within the South Atlantic census division, such as North and South Carolina, exhibit above-average values of propaganda. Still, it also holds for states in the East South Central census division, such as in Alabama and Tennessee. Second, the map shows that sizeable differences in anti-Black propaganda also exist within states, even among neighboring counties.

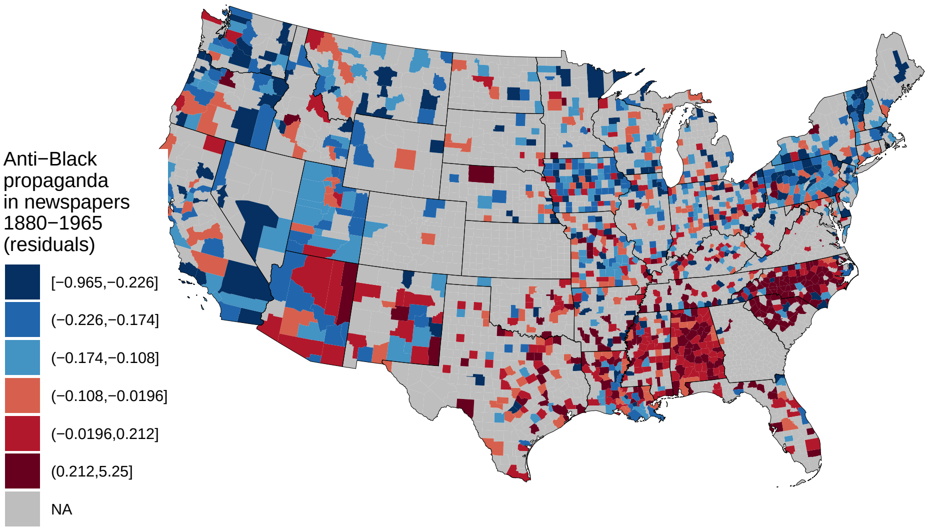

Figure 2.2: The geography of anti-Black propaganda in U.S. local newspapers.

Notes: This map shows the cross-county distribution of the residuals from regressing anti-Black propaganda from 1880 to 1965 on year fixed effects, as described in the text. Darker red colors indicate above-average anti-Black propaganda in a particular county, while darker blues indicate below-average values of anti-Black propaganda in newspapers of a county. No data are available for counties in grey.

Next, we investigate how regional differences change over time. Figure 2.3a shows the time variation by geographic region in the number of anti-Black propaganda in newspapers from 1880 to 1965. We document several interesting patterns. First, anti-Black propaganda markedly declined across all regions from 1880 to 1940; second, the

South deviated from this long-term trend between 1880 and the early 1900s, which is the period of our analysis. Third, we see an increase in anti-Black propaganda after 1930 in the South and the Northeast, which becomes most pronounced in the South after 1940. Fourth, anti-Black propaganda was always most frequent in newspapers in the South, especially in the first 40 years of our sample period, and particularly so from 1880 to 1900. After 1900 a decade-long convergence to the lower levels of the Northeast, Midwest, and West sets in. Figure 2.3b depicts variation over time in anti-Black propaganda in local newspapers for rural and urban counties.[5] It shows that rural counties primarily drove the increase in anti-Black propaganda in the late 19th century, where Populists were particularly active.

To summarize, the raw data offers some preliminary evidence in support of the hypothesis. Deviating from a general decrease in anti-Black propaganda in US local newspapers, Southern and rural counties saw a short-lived spike in anti-Black propaganda between 1890 and 1900. Variation across Southern counties in this short spike will be the focus of our analysis.

2.3.4 Political Threat

The second key empirical challenge is to measure political threat due to the rise of the Populist Party at the local level. To this end, we collect data on electoral outcomes in the 1892 presidential election. For each county, we record the vote share of the Populist Party, provided by ICPSR (Clubb et al., 2006).[6] Appendix Figure 2.5 depicts the Populist vote share in the 1892 Presidential election across counties in the US. It demonstrates that the Populists' electoral success varied from state to state and even between counties within states.

To operationalize political threat at the county level, we assume that where the Populists gained votes, they posed a political threat to the local Democratic elites.

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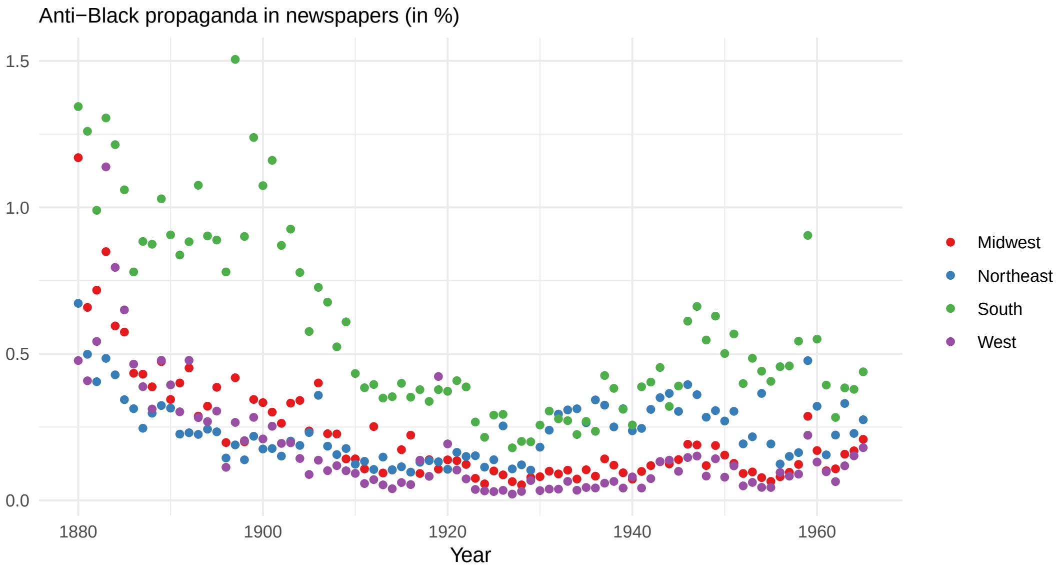
[5] We define rural counties as those with less than 200 persons per square mile in a given year. Yearly population density is calculated by linearly interpolating population from decennial censuses from 1880 to 1970.

[6] Populist vote shares for counties in Alabama are zero or missing in this data set. We draw on online sources to supplement these data.

(a) Anti-Black propaganda by census region



(b) Anti-Black propaganda in urban and rural counties

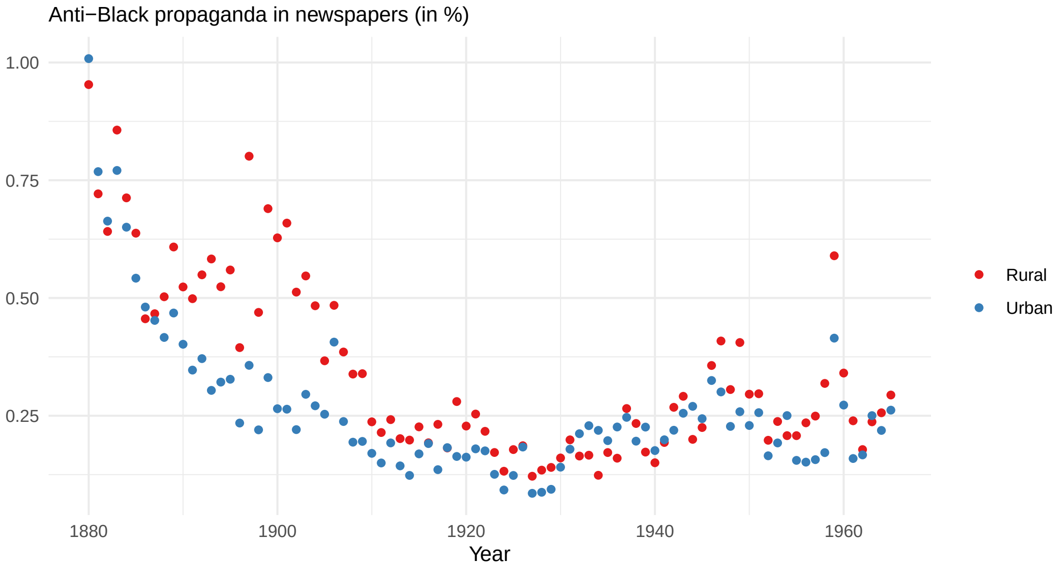


Figure 2.3: The evolution of anti-Black propaganda in U.S. local newspapers

Notes: This figure shows the time variation in the share of newspaper pages with anti-Black propaganda. In Panel (a), each dot corresponds to the population-weighted average level of anti-Black propaganda in a particular year in one of four broad geographic regions of the U.S. Panel (b) shows the population-weighted averages for rural and urban counties. We define rural as counties with less than 200 persons per square mile in a given year.

This assumption is motivated by the notion that what mattered to the local Democrat elites in their decision to "enlist the Negrophobe elements" (Woodward, 1955) was the perceived political threat resulting from the arrival of the Populists on the political stage, rather than their ability to attract a sizable vote share. We define an indicator for political threat, $\mathbb{1}(\textit{political threat}_c)$, equal to one if the Populist Party received any votes in the 1892 elections. Appendix Figure 2.6 shows that this was the case in roughly 90% of counties, and Appendix Figure 2.7 illustrates the counties presumed to be under threat for which we have newspaper data. Almost all states have at least one non-threatened county; however, most non-threatened counties are in Louisiana. Thus, we will assess the sensitivity of our results to excluding Louisiana from the sample as a robustness check.

We also test for the sensitivity of our results to alternative definitions of political threat. We explore whether our findings replicate in regressions that i) define the threat indicator equal to one if the Populist Party received a vote share higher than 10% in the 1892 elections, which results in an approx. even split into threatened and non-threatened counties (see Appendix Figure 2.8); ii) use vote share quintiles as main independent variable; and iii) include both the vote share and the baseline indicator as independent variables.

2.3.5 Other Data Sources

Newspapers at the time were often highly partisan and openly endorsed a particular party. Gentzkow et al. (2011) and Gentzkow et al. (2014) digitized newspaper directories that provide information about newspapers' political affiliations in presidential elections. We link this information to our newspaper data set to distinguish between newspapers that supported the Democratic Party and those that endorsed other parties or were independent. Running the analysis separately for Democrat and non-Democrat affiliated newspapers enables us to test whether all newspapers report more about rapes committed by Blacks after the Populist threat appeared in 1892 or whether this effect is limited to newspapers affiliated with the Democrats. We link endorsement in the 1892 election when such information is available. For newspapers that we cannot locate in 1892, we link the endorsement in the closest available year, i.e., in years 1896, 1888, 1900, 1884, 1904, and 1880 – in this order. Finally, we access county-level socioeconomic characteristics from the 1890 population census provided by Haines and Inter-University Consortium For Political And Social Research (2010) and the residential segregation index computed by Logan and Parman (2017)

2.4 Results

We now turn to the empirical analysis. In this section, we lay out the empirical strategy and present the results.

2.4.1 Empirical Strategy

We employ a difference-in-differences strategy with the first difference comparing the prevalence of anti-Black propaganda in newspapers from counties where the Populists gained votes in the 1892 presidential election to counties where the Populists did not receive any votes. The second difference compares changes in propaganda over time, in particular before and after the Populists arrived on the political stage in 1892. To this end, we define a dummy $\mathbb{1}(Post_t)$ that equals one from 1893 onward. We then investigate whether political threat is associated with an increase in anti-Black propaganda in newspapers by estimating the following regression:

$$\textit{Anti-Black Propaganda}_{i(cr),t} = \alpha_i + \alpha_{rt} + \beta \; \mathbb{1}(\textit{ political threat}_c \times \mathbb{1}(Post_t) + \epsilon_{i(cr),t}. \tag{2.2}$$

The dependent variable *Anti-Black Propaganda*$_{i(cr),t}$, defined in the previous section, is the measure of anti-Black propaganda in newspaper i in county c, census region r and year t. β is the coefficient of interest. If political threat increases the spread of propaganda, we expect that $\beta > 0$. Estimating regression (2.2) at the newspaper level allows us to control for time-invariant newspaper characteristics by including newspaper fixed effects α_i. This implies that the identifying variation comes from changes *within* newspapers over time. We control for year ×census region fixed effects α_{rt} to remove variation that is year-specific across newspapers in the same census region (South Atlantic, East South Central Division, or West South Central Division). Standard errors $\epsilon_{i(cr),t}$ are clustered at the county-level, allowing for correlations of unobserved variation across newspapers in the same county and over time. Appendix Table 2.5 provides summary statistics for all variables used in the analysis.

The central identifying assumption in our difference-in-differences framework is that of parallel trends in propaganda absent of treatment. In other words, absent political threat due to the rise of the Populist Party, newspapers in counties where the Populists won votes would not have experienced a differential increase in the spread of propaganda published therein. To inquire into differential trends, we implement a dynamic difference-in-differences estimation by estimating coefficients for each year separately. We then visually inspect yearly coefficients and test for the existence of a pre-trend in anti-Black hatred in newspapers across groups.

2.4.2 Main Result

Table 2.1 reports the results of the estimation of equation (2.2). We find a large and statistically significant relationship between political threat and the spread of anti-Black propaganda. The result in column 1 suggests that, after 1892, newspapers spread more anti-Black propaganda in counties where the Populist Party received a positive vote share in the 1892 presidential election. Since we include fixed effects for newspapers and year × census region, we identify the effect holding fixed newspapers' time-invariant racial bias and newspaper-invariant national and regional news affecting all newspapers in any given year and region. The effect size is large: relative to newspapers in counties with no political threat, newspapers in counties under threat spread on average roughly 0.5 pages more anti-Black propaganda per year after 1892. This corresponds to ca. a 50% increase in response to the Populists.

Table 2.1: Effect of political threat on anti-Black propaganda.

	Anti-black propaganda				
	All	Non Dem.	Democratic Newspapers		
	(1)	(2)	(3)	(4)	(5)
Political threat × Post 1892	0.448***	0.026	0.499***	0.578***	0.474***
	(0.119)	(0.248)	(0.128)	(0.137)	(0.152)
No. of newspapers	764	110	654	654	654
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes
Economic conditions × year FE	No	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	No	Yes
Observations	5,399	666	4,733	4,733	4,733
R^2	0.502	0.602	0.496	0.525	0.530

Notes: This table shows that political threat due to the rise of the Populist Party increased the frequency of anti-Black propaganda in newspapers. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. The main independent variable is an indicator equal to one if the Populist Party gained votes in the presidential election of 1892 in the newspaper's county (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year × census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 restricts the sample to newspapers that do not endorse the Democratic Party Column 3 to 5 focus on newspapers that endorse the Democratic Party for which this information is not available. Column 4 adds county-level economic controls, measured in 1890, and interacted with year dummies. These controls include log population, black population share, residential segregation (in the year 1880), log per capita output in manufacturing and agriculture, the average farm size, log railway miles per square mile, the average indebtedness of farms (= mortgage value of farm/value of farms); the average interest rate on farms mortgages, the share of share-cropping farms, and the share of cotton acreage to total farm acreage. Column 5 adds controls for the vote shares for the Democratic Party in the 1892 presidential election, interacted with year dummies. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

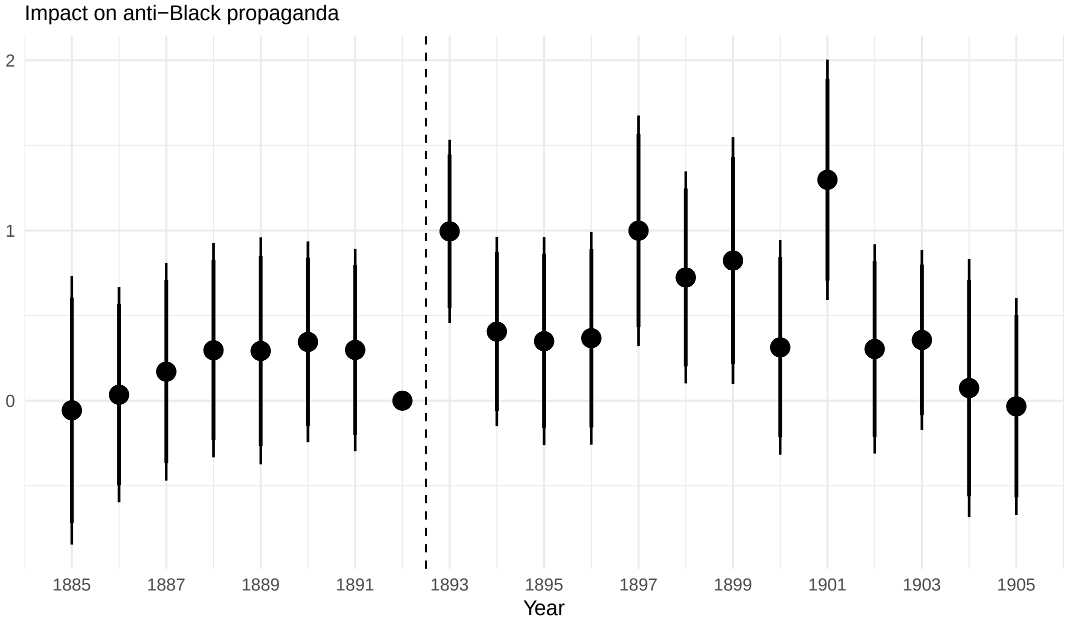
Next, we split the sample into newspapers that did not endorse the Democrats and those that did. Columns 2 and 3 report the results. Among 110 newspapers that were independent or affiliated with parties other than the Democrats, we find no increase in anti-Black propaganda after 1892. Instead, Democrat-affiliated newspapers drive the effect. These results provide strong evidence supporting the narrative in Woodward (1955) that Democratic elites spread anti-Black propaganda in newspapers to discredit the Populists in the eyes of poor white voters. Democrat-affiliated newspapers in counties with political threat increased the spread of anti-Black propaganda by about 0.5 pages per year after 1892 compared to Democrat-affiliated newspapers in counties without political threat.

Ruling out pre-trends. Our result could reflect differential trends in anti-Black propaganda that newspapers in counties with political threat followed already before the election in 1892. If so, the election result of 1892 could itself be an outcome of these differential trends, and the parallel trends assumption would be violated, which would invalidate our identification strategy. We conduct a dynamic difference-in-differences analysis to check for pre-existing trends in anti-Black propaganda. Figure 2.4 shows the coefficients of the regression of anti-Black propaganda on the political threat indicator interacted with year dummies, using the set of Democratic newspapers and controlling for newspaper fixed effects and year × census region fixed effects as in Column 3 of Table 2.1.

We fail to detect a visible or statistically discernible pre-tend in anti-Black propaganda. The F statistic for all coefficients before 1892 is 0.69 ($p = 0.68$). Moreover, the graph shows that the effect vanishes after the Populist Party collapsed in the years after the 1896 Presidential election. This finding is in line with our interpretation of short-lived incentives to Democratic politicians to spread anti-Black propaganda in newspapers in places where the Populists threatened the Southern Democratic elites.

Differences in economic conditions do not drive the result. An obvious concern with our result is that the Populists' vote shares in the 1892 presidential elec-

Figure 2.4: Dynamic differences-in-differences analysis



Notes: This figure shows differences in anti-Black propaganda between newspapers in Democratic counties with versus without political threat in 1892, based on the specification of Column 3 in Table 2.1. It shows confidence intervals at the 95% (thin lines) and 90% (thick lines) level. Standard errors are clustered at the county-level. The F statistic for all coefficients before 1892 is 0.69 ($p = 0.68$).

tions are not random. Determinants of the local presence of the Populist Party that also correlate with anti-Black propaganda may violate the parallel trends assumption of the difference-in-difference strategy. For example, Eichengreen et al. (2019) show that the Populists were more successful in counties that suffered from the economic downturn in the 1880s and 1890s. It is conceivable that this economic distress gave rise to differential dynamics in anti-Black sentiment. In column 4, we address this concern by flexibly controlling for the effects of differences in local economic conditions in the years before 1890. Specifically, we include a large set of county economic characteristics, interacted with year dummies, as control variables: the log county population share, the county population share of African Americans, residential segregation, log per capita manufacturing and agricultural output, average farm size, log miles of railways per square mile, average indebtedness of farms, average interest rates paid on farm debt, the share of cotton acreage to total farm acreage, and the share of sharecropping farms of the total number of farms. The latter two are motivated by the possibility that white plantation owners had an incentive to fan racial hatred to prevent black and white sharecroppers from joining forces and fight for higher wages. Column 4 shows that flexibly controlling for differences in these characteristics increases the coefficient to 0.578 while leaving the standard error almost unchanged. The result corroborates our finding: the Populist political threat increases the prevalence of propaganda in newspapers affiliated with Southern Democratic elites.

Controlling for differential increase in demand for propaganda. Based on historical accounts (Woodward, 1955) and the weak competitive forces in Southern media markets (Gentzkow et al., 2015), we argue that newspapers supply propaganda; that is, political actors such as newspaper editors, owners, and local officeholders were in the position to spread anti-Black propaganda in their newspapers. A competing view is that newspaper content is largely driven by readers' demand.[7] This view raises the concern that any increase in anti-Black propaganda after 1892 may be due to local demand for such content. While our newspaper fixed effects remove time-invariant differences in newspaper ideology and local demand, it may still be the case that differential shifts in demand over time may confound the result. To address this concern, we also control for the county-level Democrat vote share in the 1892 presidential election, interacted with year fixed effects. The vote share proxies local demand for anti-Black propaganda, and interaction with year dummies allows the demand effect to vary flexibly over time. Column 5 in Table 2.1 reports the result. The effect remains positive and highly statistically significant but loses roughly 18% of its effect size. Thus, the finding is in line with local preferences driving some of the demand for newspapers, but local demand plays a lesser role in our setting. Even with such a demanding specification, β is precisely estimated and sizable.

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[7] Gentzkow and Shapiro (2010) show that for the U.S. from 1972 to 1998, demand for media slant, as revealed in local political vote shares, are a more important determinant of newspaper slant than is the identity of the ownership group.

No differential increase in reporting of rapes unrelated to African Americans. In the previous section, we pointed out that our dependent variable reflects a combination of reporting of actual rapes (local and distant), their amplification by the local press, and op-eds, letters to the editors, and fabrications. This measurement raises the question of how to interpret the results; in particular, if the actual incidence of rapes increased in counties where the Populists entered local politics, our estimate of β could reflect accurate reporting. The best solution to this problem would be to control for the actual occurrences of rapes by using yearly crime statistics from primary sources with information on the type of crime and the race of the offender. Unfortunately, such data are not readily available, and would potentially still reflect biases in the local judicial system.

As an alternative solution, we conduct a placebo test, where we estimate the effect of political threat on the extent to which newspapers report about rapes unrelated to African Americans. To do so, we estimate such rapes' reporting by counting the occurrence of the keyword "rape" and subtract the number of times "rape" co-occurs with "negro" in local newspapers. We aggregate the frequencies to the newspaper-year level, normalize it by the measure for overall text length. Then, we replicate the previous regressions using the new outcome. Appendix Table 2.11 shows that the coefficients of this placebo test are statistically indistinguishable from zero and, if anything, point in the opposite direction.

No effect outside the South, where political incentives to spread propaganda were absent. Finally, we examine the effect of the Populist Party on anti-Black propaganda outside the Southern states, where few African Americans lived. The Populists thus competed without relying on the support of black voters. In the Midwest, for example, the Populists' were hugely successful in the 1892 election – they carried entire states such as Kansas or Colorado – but their position on race was less salient. Thus, we expect that the Populist Party's presence did not create an incentive for white elites to spread anti-Black propaganda because there was no diverse coalition to split.

Table 2.2 reports the result of this placebo test. Using the same specifications as before, we fail to detect an effect outside the South. The coefficients are small and change signs between specifications. We therefore conclude that the Populist Party's presence did not affect the spread of anti-Black propaganda in non-Southern states. This finding provides another piece of evidence in support of the hypothesis.

In sum, our results suggest that political threat due to the rise of the Populist Party increased the spread of anti-Black propaganda in Democratic newspapers in the South. The effect is unlikely to be driven by shifts in factors related to economic characteristics or in demand for racist content, nor do we find evidence that real occurrences of crimes differentially increased. Lastly, we fail to detect an effect in places where the political incentives to spread anti-Black propaganda were generally absent.

Table 2.2: Placebo: Non-Southern states.

	Anti-black propaganda					
	Northeast	Midwest	West	All regions		
	(1)	(2)	(3)	(4)	(5)	(6)
Political threat × Post 1892	0.035	−0.060	−0.060	−0.008	0.032	0.026
	(0.035)	(0.051)	(0.051)	(0.033)	(0.035)	(0.037)
No. of newspapers	394	494	77	965	965	965
Newspaper FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes	Yes
Economic conditions × year FE	No	No	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	No	No	Yes
Observations	4,017	4,251	4,251	9,073	9,073	9,073
R^2	0.334	0.375	0.375	0.364	0.387	0.389

Notes: The table shows that the Populist Party's presence did not increase the frequency of anti-Black propaganda in non-Southern states. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. The main independent variable is an indicator equal to one if the Populist Party gained votes in the newspaper's county in the presidential election of 1892 (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year fixed effects. Columns 1, 2, and 3 restrict the sample to states in the Northeast, Midwest, and West, respectively. Column 5 adds controls for county-level economic conditions in 1890, interacted with year dummies. These controls are described in Table 2. Column 6 adds controls for the Democratic Party's vote shares in the presidential elections of 1892, interacted with year dummies. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

2.4.3 Robustness

We now turn to the examination of the sensitivity and robustness of our baseline estimates.

Alternative definitions of political threat. So far, we assumed that a vote share greater than zero for the Populists created political threat for the Democratic elites. We now show that our findings replicate if we replicate our analysis using three alternative definitions of political threat. First, we define the political threat indicator equal to one if the Populists received a vote share higher than 10% in the 1892 presidential elections. According to this definition, the elites ca. half of the counties in our sample are assumed to perceive threat. Appendix Table 2.6 shows that all the conclusions of the analysis go through. The effect sizes become slightly smaller, possibly because elites perceived threat even when the Populists gained a vote share of less than 10%. Second, we add the vote share for the Populist Party to the regression. Table 2.7 shows that while the coefficient on Populist vote share is positive across specifications, the effect mainly comes from the political threat indicator. Third, we use quintiles of the Populist vote share as main independent variables. This definition allows us to examine whether a higher Populist vote share has a stronger effect on propaganda, arguably because it created a more salient political threat. Table 2.8 reports positive and statistically significant coefficients for higher quintiles of Populist vote shares but not for lower quantiles. In sum, our main finding is robust to different definitions of political threat at the county level.

Balanced panel. The newspaper database is highly unbalanced. While some newspapers are available over many years, most newspapers are available for short periods only. An unbalanced panel may cause problems for our estimation strategy if the entry and attrition of newspapers are systematically related to our outcome and both differences. To deal with this concern, Appendix Table 2.9 focuses only on the balanced panel of 60 newspapers from 1885 to 1903. With the substantially smaller sample, we obtain larger and highly statistically significant coefficients. This result lends empirical support to the assumption underlying our main result. Neither newspapers' selective entry or exit, nor their inclusion into the digitized sample drive the results. If anything, these factors work against us.

Dropping Louisiana. Table 2.10 drops all observations from Louisiana, where no county voted for the Populist presidential candidate in the 1892 election, as Democrats and Populists ran on a combined electoral ticket in 1892 (White, 1918). This fusion constrains our ability to identify political threat at the county level in our election data. Nevertheless, the same issues that drove poor white and black voters to the Populists elsewhere were also at work in Louisiana.[8] Therefore, we are concerned that including Louisiana and implicitly assuming that the Populists received zero vote share across counties introduces a downward bias, as we expect that Democrat elites in Louisiana also resorted to propaganda to respond to the Populist threat. Reassuringly, Table 2.10 shows that our estimates barely change when we drop Louisiana from the sample.

2.4.4 Heterogeneous Effects

We now probe into the heterogeneity of the effect. First, we explore whether the effect size varies based on pre-existing wealth differences among Whites. In light of the theory, we expect that white elites felt more threatened when they had more to lose from the redistributionist policies that the Populists advocated. We proxy white wealth by the average sizes of farms in counties. Column 1 in Table 2.3 reports the result. We find a positive and statistically (marginally) significant coefficient on the interaction term.

Next, we examine whether the effect was stronger in rural than in urban counties. The Populists sought support among poor farmers. We, therefore, expect that elites in rural counties perceived more threat than in urban counties. Column 2 reports a neg-

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[8] According to (White, 1918): "By entering into the fusion agreement, it was asserted, the people's party was merely fighting the democrats with their own methods. In concluding, an appeal was made to the voters to have the manhood to assert their rights, not to let the scarecrow of negro domination longer drive them to the democratic wigwam, and to rally to the standard of the people's party and elect the fusion ticket as a re buke to 'the Democrats in their strength, and the Republican party in its weakness. May Louisiana break the 'solid south' and greet our great toiling brethren of the North and West with the cheering hope of industrial reform in the near future."

ative coefficient on the interaction between political threat and log county population; however, the coefficient is not statistically significant at conventional levels.

We also assess whether the effect is stronger in places with a larger share of African American residents or in more segregated counties. Columns 3 and 4 show a positive coefficient on the interaction with the county population share of African Americans and a negative coefficient on the interaction with residential segregation. This result suggests that the effect of political threat on propaganda is more substantial in places with more African Americans and where African Americans were more likely to live next to Whites. These findings are consistent with the possibility that the perceived threat was more serious when African Americans were more salient to white residents.

2.5 Did the Progaganda Affect Voting?

Our findings provide insights into a so-far untested determinant of propaganda. Since previous studies have found that propaganda can affect behavior, the question arises whether, in our context, the propaganda "worked". Did it sway people to vote for the Democrats? To investigate this question, we examine whether anti-Black propaganda during the 1890s is associated with electoral outcomes in subsequent elections. Specifically, we test if counties see stronger electoral gains for the Democrats in the early 20th century if Democratic newspapers spread more anti-Black propaganda during the 1890s. We estimate the following equation,

$$\begin{aligned} Dem.\ Vote\ Share_{c,\ 1900,\ ...,\ 1916} = \sum_{t=1885}^{1900} \beta_t\ propaganda_{i(c)t} \times D_t \\ + Dem.\ Vote\ Share_{c,\ 1892} + X_c'\gamma + \varepsilon_{i(c)t} \end{aligned}$$

where $Dem.\ Vote\ Share_{c,\ 1900,\ ...,\ 1916}$ and $Dem.\ Vote\ Share_{c,\ 1892}$ denote the county-level vote shares for the Democratic Party in years 1900, 1904, 1908, 1916 and 1892, respectively; $propaganda_{i(c)t}$ captures the prevalence of anti-Black propaganda in local newspapers; D_t is an indicator variable for each year; X_c' denotes a vector of region

Table 2.3: Heterogeneity in the effect of political threat on anti-Black propaganda.

	Anti-black propaganda Democratic Newspapers				
	(1)	(2)	(3)	(4)	(5)
Political threat × Post 1892	0.515***	0.514***	0.507***	0.497***	0.539***
	(0.126)	(0.128)	(0.130)	(0.126)	(0.130)
× Avg. farm size [std.]	0.163*				0.174*
	(0.091)				(0.096)
× Log population [std.]		−0.068			−0.027
		(0.080)			(0.078)
× Share black pop. [std.]			0.053		0.139*
			(0.057)		(0.083)
× Residential segregation [std.]				−0.044	−0.113
				(0.062)	(0.077)
No. of newspapers	654	654	654	654	654
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes
Observations	4,733	4,733	4,733	4,733	4,733
R^2	0.497	0.496	0.496	0.496	0.498

Notes: This table shows that the effect of political threat on anti-Black propaganda is stronger in places with larger farms and a larger population share of African Americans. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. All regressions include newspaper and year-census division fixed effects. The sample is restricted to that endorse the Democratic Party and for which this information is not available. All interacted variables are standardized to z-scores. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

fixed effects and the same county-level socioeconomic characteristics that we use and describe in the main analysis (Section 2.4.2); and ε_c is the error term. Standard errors are clustered on counties. The coefficients of interest are β_t, in particular for the years after 1892, when the Populists threatened Southern Democrats.

Table 2.4 reports the results. In most years after 1892, it shows a positive association between anti-Black propaganda in Democrat-affiliated newspapers and the Democratic vote share in future elections. Moreover, the relationship is highly statistically significant for propaganda in 1893, when Southern Democrats perceived the Populist threat for the first time. Thus, we find suggestive evidence that the propaganda was politically successful: counties with a larger increase in propaganda see stronger gains for the Democrats in presidential elections by 1900.

2.6 Conclusion

This study provides empirical evidence for the hypothesis that autocratic political elites resort to hate creating propaganda when a diverse coalition threatens their dominant position. We exploit the rise of the redistributionist Populist party in the presidential 1892 election and the threat they posed to Southern Democratic politicians by aligning the interests of white and black poor farmers. We find that newspapers fanned racial hatred aimed at preventing poor Whites from voting for the Populists.

The empirical analysis makes use of a novel measure of anti-Black propaganda based on text data from an extensive corpus of newspapers. In a difference-in-differences framework, we show that newspapers in counties where the Populists received votes in the 1892 presidential election spread more anti-Black propaganda in the following years compared to newspapers in counties where the Populists did not pose a threat. Our results are identified from within-newspaper variation driven by newspapers affiliated with the Democrats, lending support to our interpretation that the effect is due to the supply of propaganda. The evidence also suggests that the effect is not present outside the South, where the political incentive to spread anti-Black propaganda was absent. Moreover, the effect is neither due to an increase in demand for such content nor due

Table 2.4: Effect of anti-Black propaganda on future Democratic vote share.

	1900	1904	1908	1912	1916
			Democratic vote share in year		
	(1)	(2)	(3)	(4)	(5)
Anti-black propaganda × Year 1886	0.638	−0.072	0.127	0.082	−0.252
	(0.769)	(0.622)	(0.546)	(0.587)	(0.853)
Anti-black propaganda × Year 1887	0.931**	−0.069	−0.067	0.113	0.144
	(0.400)	(0.352)	(0.376)	(0.376)	(0.352)
Anti-black propaganda × Year 1888	1.166***	0.361	0.501	0.655*	0.289
	(0.442)	(0.381)	(0.374)	(0.397)	(0.418)
Anti-black propaganda × Year 1889	0.990***	0.184	0.377	0.491	0.280
	(0.364)	(0.335)	(0.326)	(0.366)	(0.361)
Anti-black propaganda × Year 1890	0.550	−0.015	−0.107	0.003	−0.030
	(0.471)	(0.321)	(0.367)	(0.365)	(0.455)
Anti-black propaganda × Year 1891	0.494	0.147	0.307	0.256	0.325
	(0.452)	(0.447)	(0.498)	(0.492)	(0.585)
Anti-black propaganda × Year 1892	1.147*	0.711	0.614	0.787	1.197**
	(0.656)	(0.615)	(0.675)	(0.586)	(0.519)
Anti-black propaganda × Year 1893	1.363***	0.790**	1.136***	1.176***	0.968***
	(0.448)	(0.342)	(0.342)	(0.345)	(0.336)
Anti-black propaganda × Year 1894	1.485**	0.847*	0.839	1.105**	0.774
	(0.653)	(0.513)	(0.565)	(0.543)	(0.535)
Anti-black propaganda × Year 1895	0.754	0.574	0.315	0.191	0.042
	(0.461)	(0.406)	(0.400)	(0.477)	(0.506)
Anti-black propaganda × Year 1896	0.612	0.179	0.366	0.319	0.227
	(0.556)	(0.529)	(0.476)	(0.563)	(0.504)
Anti-black propaganda × Year 1897	0.206	−0.026	−0.057	−0.058	−0.264
	(0.261)	(0.248)	(0.236)	(0.284)	(0.286)
Anti-black propaganda × Year 1898	0.731*	−0.054	−0.091	0.129	−0.142
	(0.390)	(0.329)	(0.343)	(0.348)	(0.323)
Anti-black propaganda × Year 1899	0.705**	0.247	0.314	0.310	0.123
	(0.293)	(0.236)	(0.238)	(0.262)	(0.248)
Anti-black propaganda × Year 1900	0.425	0.234	0.211	0.191	−0.028
	(0.400)	(0.281)	(0.287)	(0.298)	(0.282)
No. of counties	276	276	276	276	276
Region FE	Yes	Yes	Yes	Yes	Yes
Economic conditions	Yes	Yes	Yes	Yes	Yes
Dem. vote share 1892	Yes	Yes	Yes	Yes	Yes
Observations	3,578	3,587	3,587	3,587	3,587
R^2	0.674	0.716	0.700	0.628	0.636

Notes: This table shows that anti-Black propaganda after 1892 affected the Democrat vote share in subsequent presidential elections. An observation is a newspaper-year from 1886 to 1900. The sample includes newspapers that endorse the Democratic Party and for which this information is not available. All regressions include census region fixed effects, county-level economic controls, which are described in Table 1, and the vote share for the Democratic Party in 1892. The outcome in each column is the vote share for the Democratic Party in presidential elections in the years 1900 - 1916. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

to differences in economic conditions. Finally, we find evidence suggesting that a rather short period of anti-Black propaganda shaped political outcomes for decades to come.

2.7 Appendix to Chapter 2

2.7.1 Additional Tables and Figures

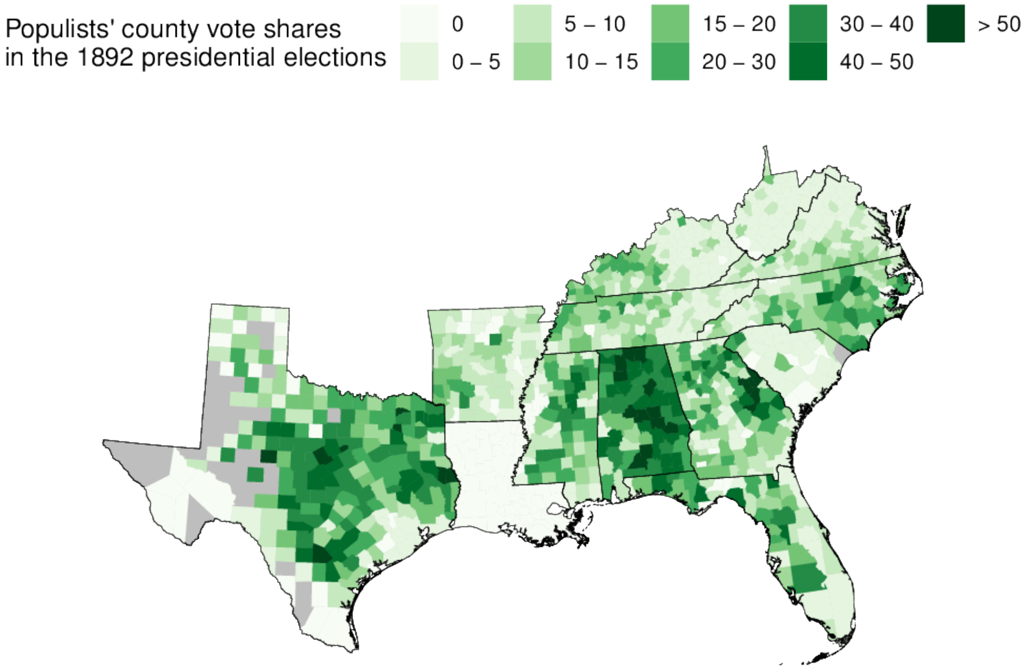


Figure 2.5: Populist Party's vote shares across counties in the 1892 presidential election

Notes: This map shows the county-level vote share for the Populist Party in the 1892 presidential election in the U.S. South. Darker greens indicate higher vote shares.

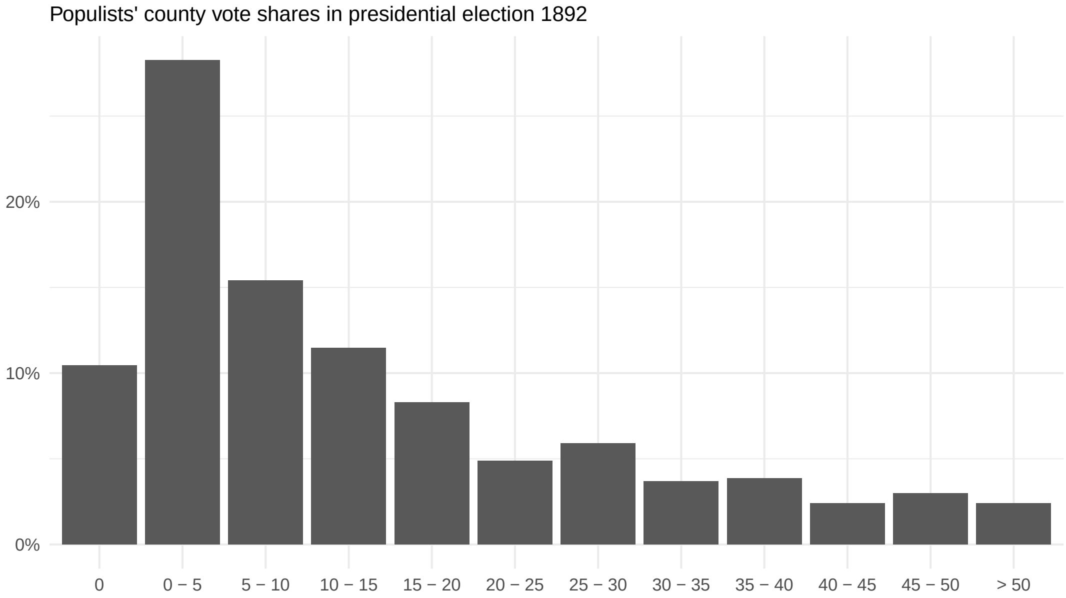


Figure 2.6: Distribution of Populist Party's vote share in the 1892 presidential election

Notes: The graph shows the distribution of the Populist Party's vote share in the presidential election of 1892 in the U.S. South.

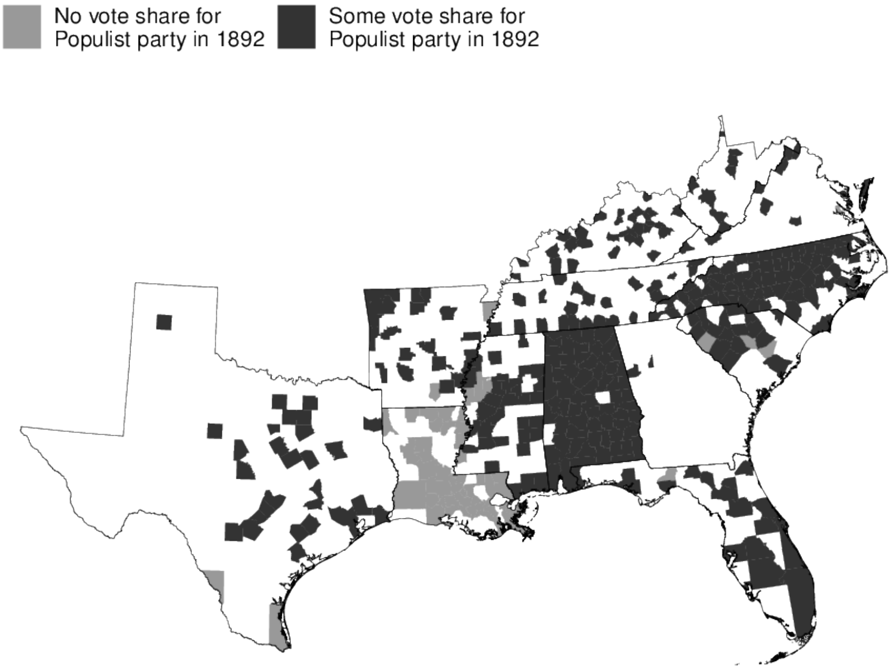


Figure 2.7: Distribution of the political threat dummy

Notes: The graph shows the Southern United States in the borders of 1890. Counties in dark or light grey have newspapers in the database and are part of the analysis. Dark (light) grey indicates that the Populist party won some (no) vote share in the 1892 presidential elections.

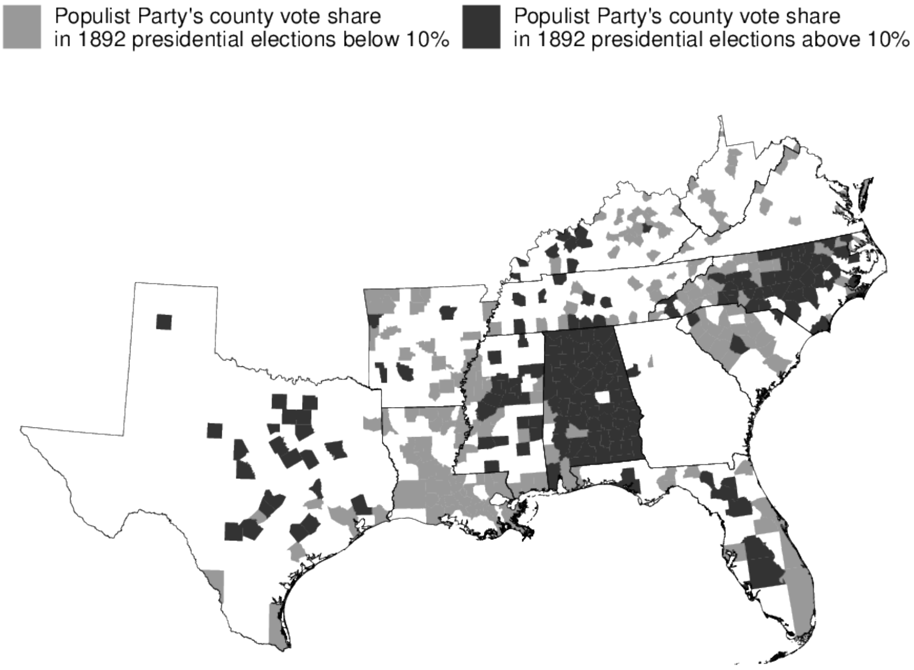
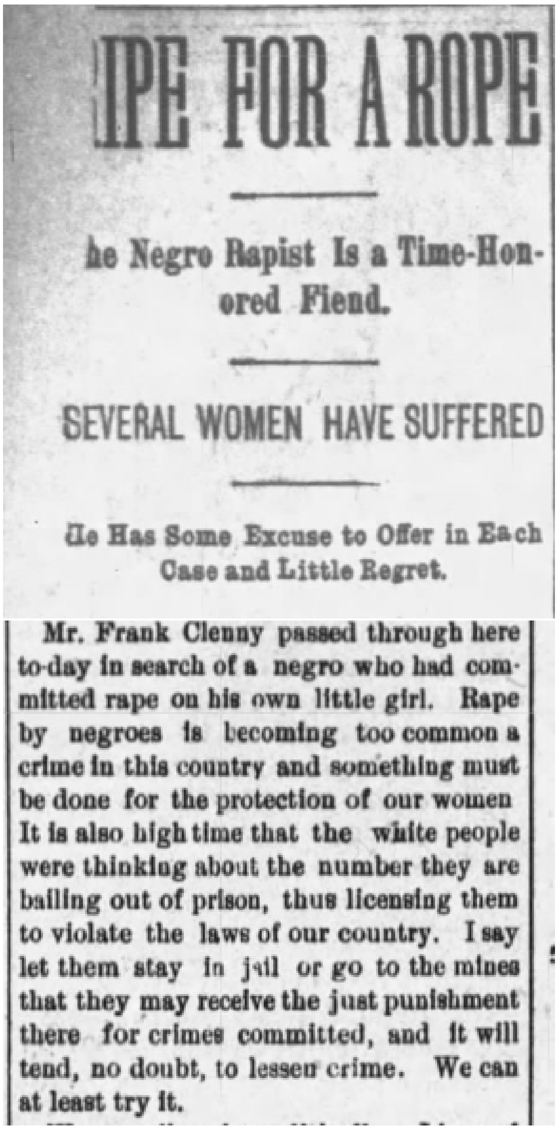


Figure 2.8: Distribution of the alternative political threat dummy

Notes: The graph shows the Southern United States in the borders of 1890. Counties in dark or light grey have newspapers in the database and are part of the analysis. Dark (light) grey indicates that the Populist party won a vote share higher (smaller) than 10% in the 1892 presidential elections.



IPE FOR A ROPE

he Negro Rapist Is a Time-Honored Fiend.

SEVERAL WOMEN HAVE SUFFERED

He Has Some Excuse to Offer in Each Case and Little Regret.

Mr. Frank Clenny passed through here to-day in search of a negro who had committed rape on his own little girl. Rape by negroes is becoming too common a crime in this country and something must be done for the protection of our women It is also high time that the white people were thinking about the number they are bailing out of prison, thus licensing them to violate the laws of our country. I say let them stay in jail or go to the mines that they may receive the just punishment there for crimes committed, and it will tend, no doubt, to lessen crime. We can at least try it.

Figure 2.9: Illustration of newspaper articles associating African Americans with rapes

Notes: Top panel: Public Ledger, Memphis. Bottom panel: Eufaula Daily Times, 1893

Table 2.5: Summary statistics

Statistic	N	Mean	St. Dev.	Min	Max
Anti-Black propaganda	8,086	1.002	1.437	0	22
Political threat indicator	8,086	0.832	0.374	0	1
Post 1892 indicator	8,086	0.597	0.491	0	1
Log population	7,947	10.726	0.595	7.437	13.090
Share black population	7,947	0.397	0.231	0.002	0.934
Residentail segregation	7,790	0.342	0.121	−0.002	0.708
Avg. log p.c. manuf. output	7,782	3.505	1.401	0.000	6.447
Avg. log p.c. farm output	7,947	4.225	0.672	1.074	5.609
Avg. farm size	7,947	144.390	505.320	38	25,576
Rail miles / county sq. miles	7,947	0.526	0.323	0.000	1.877
Avg. indebtedness of farms	7,839	0.447	0.136	0.100	1.000
Avg. interest rate on farm mortgages	7,839	3.650	1.299	0.778	8.221
Share cotton acerage	7,936	0.095	0.094	0.000	0.459
Share share-cropping farms	7,947	0.241	0.131	0.000	0.795
Democrat vote share 1892	8,086	59.563	17.081	21.100	100.000
Democrat newspaper indicator	6,051	0.830	0.376	0.000	1.000

Table 2.6: Robustness: Political threat if Populist vote share is great than 10%

	Anti-black propaganda				
	All	Non Dem.	Democratic Newspapers		
	(1)	(2)	(3)	(4)	(5)
Political threat × Post 1892	0.377^{***}	-0.022	0.402^{***}	0.393^{***}	0.306^{**}
	(0.115)	(0.289)	(0.125)	(0.119)	(0.127)
No. of newspapers	764	110	654	654	654
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes
Economic conditions × year FE	No	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	No	Yes
Observations	5,399	666	4,733	4,733	4,733
R^2	0.503	0.602	0.497	0.525	0.530

Notes: The table shows that the main result replicates if we define political threat as an indicator equal to one if the Populist Party gained more than 10% of the vote share in the newspaper's county in the presidential election of 1892. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. All regressions include newspaper and year × census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 restricts the sample to newspapers that do not endorse the Democratic Party. Column 3 to 5 restricts the sample to newspapers that endorse the Democratic Party or for which this information is not available. Column 4 adds controls for county-level economic conditions in 1890, interacted with year dummies. These controls are described in Table 1. Column 5 adds controls for the vote shares for the Democratic Party in the 1892 presidential election, interacted with year dummies. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

Table 2.7: Robustness: Political threat dummy and Populist vote share.

	Anti-black propaganda				
	All	Non Dem.	Democratic Newspapers		
	(1)	(2)	(3)	(4)	(5)
Political threat × Post 1892	0.365***	−0.016	0.149***	0.180***	0.165***
	(0.135)	(0.107)	(0.050)	(0.052)	(0.054)
Populist vote share × Post 1892	0.006	0.054	0.058	0.057	0.014
	(0.004)	(0.135)	(0.051)	(0.051)	(0.055)
No. of newspapers	764	110	654	654	654
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes
Economic conditions × year FE	No	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	No	Yes
Observations	5,399	666	4,733	4,733	4,733
R^2	0.502	0.602	0.496	0.526	0.530

Notes: The table shows that the main result replicates if we add the vote share for the Populist Party in the presidential election of 1892 to the regression. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. The main independent variable is an indicator equal to one if the Populist Party gained votes in the presidential election of 1892 in the newspaper's county (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year-census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 restricts the sample to newspapers that do not endorse the Democratic Party Column 3 to 5 focus on newspapers that endorse the Democratic Party of for which this information is not available. Column 4 adds county-level economic controls, measured in 1890, and interacted with year dummies. These controls are described in Table 1. Column 5 adds controls for the vote shares for the Democratic Party in the 1892 presidential election, interacted with year dummies. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

Table 2.8: Robustness: Quintiles of Populist vote share

	Anti-black propaganda				
	All	Non Dem.	Democratic Newspapers		
	(1)	(2)	(3)	(4)	(5)
2nd quintile Political threat × Post 1892	0.068	−0.351	0.099	0.206	0.179
	(0.220)	(0.263)	(0.162)	(0.151)	(0.150)
3rd quintile Political threat × Post 1892	0.499***	0.156	0.381***	0.441***	0.413***
	(0.143)	(0.260)	(0.103)	(0.112)	(0.118)
4th quintile Political threat × Post 1892	0.621***	0.031	0.487***	0.514***	0.463***
	(0.135)	(0.185)	(0.103)	(0.103)	(0.114)
5th quintile Political threat × Post 1892	0.507***	−0.035	0.405***	0.476***	0.388***
	(0.158)	(0.199)	(0.123)	(0.134)	(0.149)
No. of newspapers	764	110	654	654	654
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes
Economic conditions × year FE	No	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	No	Yes
Observations	5,399	666	4,733	4,733	4,733
R^2	0.504	0.604	0.499	0.527	0.531

Notes: The table shows that the main result replicates for the upper quintiles of the Populist Party vote share in the presidential election of 1892. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. The main independent variables are indicators equal to one for the 2nd, 3rd, 4th, and 5th quintile of the vote share for Populist Party in the newspaper's county in the presidential election of 1892 (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year × census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 restricts the sample to newspapers that do not endorse the Democratic Party. Column 3 to 5 restricts the sample to newspapers that endorse the Democratic Party or for which this information is not available. Column 4 adds controls for county-level economic conditions in 1890, interacted with year dummies. These controls are described in Table 1. Column 5 adds controls for the vote shares for the Democratic Party in the presidential elections of 1892, interacted with year dummies. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

Table 2.9: Robustness: Balanced panel.

	Anti-black propaganda			
	All	Democratic Newspapers		
	(1)	(2)	(3)	(4)
Political threat × Post 1892	0.216^{***}	0.233^{***}	0.310^{***}	0.271^{***}
	(0.067)	(0.071)	(0.089)	(0.088)
No. of newspapers	60	54	54	54
Newspaper FE	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes
Economic conditions × year FE	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	Yes
Observations	1,140	1,026	1,026	1,026
R^2	0.513	0.529	0.621	0.637

Notes: The table shows that the main result replicates if we restrict the sample to the balanced panel. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda. The main independent variable is an indicator equal to one if the Populist Party gained votes in the newspaper's county in the presidential election of 1892 (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year × census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 to 4 focus on newspapers that endorse the Democratic Party of for which this information is not available. Column 3 adds controls for county-level economic conditions in 1890, interacted with year dummies. These controls are described in Table 1. Column 4 adds controls for the vote shares for the Democratic Party in the presidential elections of 1892, interacted with year dummies. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

Table 2.10: Robustness: Dropping newspapersin Louisiana

	Anti-black propaganda				
	All	Non Dem.	Democratic Newspapers		
	(1)	(2)	(3)	(4)	(5)
Political threat × Post 1892	0.468***	0.104	0.530***	0.478***	0.412**
	(0.137)	(0.299)	(0.156)	(0.155)	(0.172)
No. of newspapers	710	104	606	606	606
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year-region FE	Yes	Yes	Yes	Yes	Yes
Economic conditions × year FE	No	No	No	Yes	Yes
Dem. vote share × year FE	No	No	No	No	Yes
Observations	4,914	615	4,299	4,299	4,299
R^2	0.508	0.613	0.501	0.533	0.537

Notes: The table shows that the main result replicates if we drop newspapers from Louisiana. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is anti-Black propaganda in newspapers. The main independent variable is an indicator equal to one if the Populist Party gained votes in the presidential election of 1892 in the newspaper's county (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year × census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 restricts the sample to newspapers that do not endorse the Democratic Party Column 3 to 5 focus on newspapers that endorse the Democratic Party of for which this information is not available. Column 4 adds county-level economic controls, measured in 1890, and interacted with year dummies. These controls are described in Table 1. Column 5 adds controls for the vote shares for the Democratic Party in the 1892 presidential election, interacted with year dummies. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

Table 2.11: Placebo test: political threat and rape unrelated to African Americans

	White rape terminology				
	All	Non Dem.	Democratic Newspapers		
	(1)	(2)	(3)	(4)	(5)
Political threat × Post 1892	−0.095	−0.126	−0.074	−0.029	−0.139
	(0.149)	(0.608)	(0.158)	(0.161)	(0.185)
No. of newspapers	764	110	654	654	654
Newspaper FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	No
Region-year FE	No	No	No	Yes	Yes
Economic conditions × year FE	No	No	No	No	Yes
Dem. vote share × year FE	No	No	No	No	No
Observations	5,387	666	4,721	4,721	4,721
R^2	0.316	0.379	0.315	0.352	0.357

Notes: The table shows that political threat due to the rise of the Populist Party does not affect the frequency of rape terminology unrelated to African Americans. An observation is a newspaper-year from 1885 to 1903. The outcome in each column is the frequency of rape terminology in newspapers, net of anti-Black propaganda in newspapers. The main independent variable is an indicator equal to one if the Populist Party gained votes in the newspaper's county in the presidential election of 1892 (first difference) interacted with an indicator equal to one for years greater than 1892 (second difference). All regressions include newspaper and year × census region fixed effects. Column 1 shows the estimate for the full sample. Column 2 restricts the sample to newspapers that do not endorse the Democratic Party. Column 3 to 5 restricts the sample to newspapers that endorse the Democratic Party. Column 4 adds controls for county-level economic conditions in 1890, interacted with year dummies. These controls are described in Table 1. Column 5 adds controls for the vote shares for the Democratic Party in the 1892 presidential election, interacted with year dummies. The standard errors are clustered on counties and reported in parentheses. ***, **, and * indicate significance at 1, 5, and 10 % levels.

CHAPTER 3

History's Masters: The Effect of European Monarchs on State Performance

> "It was a time ... 'when the destinies of nations were tied to bloodlines'."
>
> – Robert Bartlett ("Blood Royal: Dynastic Politics in Medieval Europe," 2020, p.432)

3.1 Introduction

A growing literature points to the importance of leaders for the performance of their firms or organizations (c.f. Bertrand and Schoar, 2003; Malmendier and Tate, 2005; Dippel and Heblich, 2021). Likewise, characteristics of local leaders in developing countries have substantial effects on public goods provision and conflict in the region or community under their control (c.f. Chattopadhyay and Duflo, 2004; Do et al., 2020; Eslava, 2020). However, identifying such effects at the national level is difficult. The question whether national leaders can shape their countries' fortunes has been widely debated in social sciences over the past two centuries. Early advocates proposed the strong view that the "history of the world is but the biography of great men" (Carlyle, 1840, p. 47). Subsequent qualitative analyses of biographies and comparative studies have lent support to an important role played by individual leaders.[1] On the other hand, a literature in the Marxist tradition has argued that underlying structural demographic and economic forces determine both a state's performance and the endogenous emergence of

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[1] Pointing to the historical consequences of individual leaders' failures or success is common, particularly among military historians – see for example Kennedy (1989) and Gueniffey (2020). A literature in political psychology has also underlined the importance of leaders' intellectual capabilities (c.f. Simonton, 2006). Horowitz et al. (2015, p. 11) conclude that "leaders do matter in systematic ways that we can understand."

its leaders. Scholars in this strand view leaders has "history's slaves" (Tolstoy, 2007, p. 605); in the words of (Braudel and Reynolds, 1992, p. 679): "Men do not make history, rather it is history above all that makes men."[2]

Economists have brought identification to this debate. Jones and Olken (2005) show that random leadership transitions due to natural death or accidents are followed by changes in economic growth over the post-WWII period, providing convincing evidence that leaders do indeed matter. Besley et al. (2011) expand the underlying data to 1875-2004, documenting that random departures of educated leaders cause particularly strong reductions in growth. While these results are an important step forward in identifying a causal effect of leader capability on state performance, some open issues remain: The actual "quality" of leaders is unobserved; it is estimated as the fixed effect in economic growth during a leader's rule and therefore captures a plethora of other factors. The estimated effects of leader quality are ultimately based on the variance in growth during transition periods. This approach has attracted criticism: Easterly and Pennings (2020) argue that the "average growth rate during tenure is largely useless" due to its high volatility. In addition, while the timing of the transition is exogenously determined by death, the initial appointment of the deceased leader and the time in office of the subsequent one are endogenous. To address this, the analysis must be restricted to a subset of both rulers' reigns – typically 5 years before and after the transition. Consequently, the results do not shed light on long-term effects of leadership. To make progress on these fronts, the ideal experiment would feature a sequence of randomly appointed leaders with varying, observed capabilities who govern over a long horizon. To the best of our knowledge, no such setting exists. However, Europe's monarchies

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[2] In his opus magnum 'War and Piece,' Russian writer Lev Tolstoy attested to leaders that "every act of theirs...is...predestined from eternity" (Tolstoy, 2007, p. 605). Karl Marx himself had stated: "Men make their own history, but they do not make it as they please; they do not make it under self-selected circumstances, but under circumstances existing already, given and transmitted from the past. The tradition of all dead generations weighs like a nightmare on the brains of the living" (Marx, 1852). Friedrich Engels elaborated: "But that in default of a Napoleon, another would have filled his place, that is established by the fact that whenever a man was necessary he has always been found: Caesar, Augustus, Cromwell, etc." (Engels, 1968). This alternative view, cautioning the interpretation of history through the biography of individuals, is well alive in the modern debate as well. March and Weil (2009, p. 97) assert that "it is not at all clear ... that major differences in the success of organizations reflect differences in the capabilities of their leaders, or that history is the product of leaders' actions."

over the late medieval and early modern period provide a context that gets relatively close. While this period has been most intensely discussed in the debate about the role of leaders in history, it has thus far not been examined empirically.

We study European monarchs over the period 990-1914, assembling a novel dataset on ruler ability and state performance at the reign level. To identify a causal effect of ruler ability, we exploit two imminent features of ruling dynasties: First, primogeniture – the pre-determined appointment of rulers by birth order, independent of their ability. Second, variation in ruler ability due to the widespread inbreeding of dynasties. Importantly, the negative effects of inbreeding were not understood until the 20th century; if anything, rulers believed that inbreeding helped to preserve 'superior' royal traits. In addition, the full degree of consanguinity (genetic similarity) was unknown due to complex, interrelated family trees over generations. Together, these features deliver quasi-random variation, where ruler ability is unrelated to the performance or potential of their states at the beginning of their reign.

We collect data on the ability of 428 monarchs from 15 states, building on and extending the work by historian Woods (1906), who coded rulers' intellectual capability and character traits based on hundreds of biographies. To instrument for ruler ability, we collect the coefficient of inbreeding for all rulers with the necessary information on family lineages from the rich genealogical database http://roglo.eu/. This variable is a strong and robust predictor of ruler ability. We use two measures of state performance. First, Woods (1913) assessed state performance drawing on the work by numerous historians. While Woods explicitly aimed to assess ruler's intellectual capability and character traits independent of the performance of their state, this coding is prone to endogeneity issues. Our instrument addresses these under the exclusion restriction that inbreeding affected ruler ability but is not related to state performance via other channels. A threat to this condition arises if inbreeding affected the assessment of state performance by historians – for example, if they hypothesized negative effects of inbreeding on rulers, and in turn of bad rulers on states. This is unlikely because our main source proposed the opposite hypothesis.[3] Nevertheless, we address this issue by using a second, objective, proxy for

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[3] Woods was a proponent of 'Social Darwinism,' viewing history as a process of natural

state performance: changes in area during each ruler's reign. We derive this variable from Abramson (2017), who provides European state borders at 5-year intervals over the period 1100-1795.

We find that ruler ability is strongly associated with both measures of state performance, and our IV results suggest that this relationship is causal. These findings hold using country and century fixed effects. A one standard deviation (std) increase in ruler ability leads to about a one std higher state performance, and to an expansion in territory by about 17 percent. We also study the institutional circumstances under which individual rulers mattered particularly strongly, similar to Jones and Olken (2005) and Besley et al. (2011). To shed light on this question, we construct a novel country-year specific measure of historical constraints on rulers, combining definitions of the modern Polity IV score with historical sources on factors such as the power of parliaments. To bypass endogeneity issues, we use constraints on rulers from the period just before they were appointed. We find that the ability of unconstrained leaders mattered particularly strongly, while the capability of constrained rulers made almost no difference for their countries' performance.

We run a battery of checks to confirm the robustness of our results. Our findings are unaffected when we exclude episodes of governments by regents (for example, when rulers were minor at the time of their appointment), when excluding episodes of foreign rule, or those when the same monarch governed more than one state.[4] We also verify and extend Woods' (1906) and (1913) coding of ruler ability and state performance, showing

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selection. Woods' (1913) hypothesis was that moral and intellectual ability is inheritable, so that kin marriage among successful dynasties would produce better rulers. This introduces a bias against our findings. In addition, the negative effects of inbreeding on fitness were not accepted in biology until the second half of the 20th century. Conclusions such as the following were common: "Inbreeding as such does not cause degeneration; the testimony of biologists is conclusive on this point" (White, 1948, p. 417). See Wolf (2005) for detail on this debate. Correct measures of inbreeding were first developed by Wright (1921). When these measures eventually became available, Asdell (1948) showed that Woods' hypothesis was wrong, using Woods' (1906) own coding of ruler ability.

[4] Foreign rule refers to instances when monarchs of one country temporarily ruled over another country. For instance, Philipp II of Spain ruled Spain and Portugal from 1580 to 1598. When excluding episodes of foreign rule, we drop this observation for Portugal. When excluding monarchs who governed in more than one country, we drop both observations, his reign in Spain and that in Portugal.

that our results are robust even to a conservative coding that specifies ambiguous cases so that they work against a positive association between state performance and ruler ability. Our findings are robust to numerous alternative specifications such as using dummies for different levels of ruler ability, using ordered Probit, as well as clustering at the country, dynasty, and century level. By controlling for country fixed effects, our baseline results compare rulers within states while capturing long-run time trends via century fixed effects. We confirm the robustness of our results in alternative pair-level regressions that compare concurrent rulers across countries within shorter time periods.[5]

Our IV results, in particular, are robust to excluding cases of unusually high inbreeding coefficients, and to restricting the sample to those rulers for whom historical sources explicitly confirm that they rose to power via primogeniture; for example, these specifications exclude all cases where a ruler from a new dynasty came to power. We also discuss potential threats to the exclusion restriction. For instance, such a threat would arise if royals tended to marry their kin when state performance was low, and if, in addition, low state performance during parents' reign led to low performance during the reign of the offspring. We show that this is not the case – past state performance predicts neither current state performance nor ruler ability. Similarly, our IV results are robust to controlling for lags in the coefficient of inbreeding. Another threat to our identification would arise if monarchs made strategic decisions on kin marriage for reasons that are correlated with the prospects for future state performance.[6] We address this possibility by exploiting only the hidden component of inbreeding that was due to kin marriage over previous generations – and which could only be assessed with methods in genetics that emerged in the early 20th century.[7] We confirm our IV results based on

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[5] We identify for each monarch all rulers from other countries that had at least a one-year overlap in their reigns. We then run pair-level regressions in differences, also controlling for reign-specific fixed effects. We find that differences in inbreeding across concurrently ruling monarchs are a strong predictor of differences in their ability, which in turn drives differences in country performance.

[6] Note that in this case, controlling for past state performance would not necessarily address the endogeneity issue because the potential for future performance may be uncorrelated with current performance.

[7] Becker et al. (2020) similarly exploit variation in the pedigree of nobility that was not a direct choice of the nobles themselves (changes of individual positions in the nobility network), and use it as an instrument for conflict in cities of the German lands.

this restrictive measure of inbreeding.

We make novel contributions both in terms of data collection and empirical results. We are the first to track the performance of European states at the reign level over a horizon of several centuries, allowing for fluid changes in their borders. In contrast, previous papers have typically used today's country borders as their unit of analysis, and they have relied on (half-) century level outcomes such as GDP per capita or urbanization. Our dataset thus opens a new dimension to study Europe's economic history. Using this novel dataset, we contribute to a large literature that has debated the role of rulers for nationwide outcomes. We analyze a period that has been at the center of this debate since its beginning in the 19th century.[8] Our paper is the first to provide causal identification of the importance of European rulers over the late medieval and early modern period. State performance during this period had long-lasting consequences, as the foundations for the modern nation states were laid across Europe. Our findings suggest that the territorial organization of Europe as we know it is at least in part the result of chance, embodied in the ability of individual rulers. We also contribute to a strand of the literature that has underlined the importance of individual characteristics of leaders in both managerial and political settings.[9] In the managerial literature, Clark et al. (2014) have documented that CEOs matter less when they are constrained by a well-defined governance structure, echoing the findings on constrained politicians by Jones and Olken (2005) and Besley et al. (2011).[10] Our interaction results show that institutional constraints also mattered in checking the power of European monarchs. This is particularly interesting because the detrimental effects of inbreeding became more severe in the 18th and 19th century, after centuries

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[8] For proponents of the "rulers matter" view see for example Carlyle (1840), Weber (1921), William (1880), and Spencer (1896). For the opposite view that "history makes men" see Marx (1852), Engels (1968), Braudel and Reynolds (1992). More recent contributions to this debate include March and Weil (2009), Logan (2018), Simonton (2006), and Xuetong (2019).

[9] C.f. Bertrand and Schoar (2003), Malmendier and Tate (2005), Bloom and Van Reenen (2007), and Becker and Hvide (2013) for the importance of managerial traits; and Ferreira and Gyourko (2014), Yao and Zhang (2015), Logan (2018), and Dippel and Heblich (2021) for results on traits of political leaders.

[10] Besley and Reynal-Querol (2017) document higher economic growth under hereditary leaders when constraints on them were weak, using data from 1875 onwards.

of inbreeding.[11] By that time, parliaments across Northern Europe had expanded their power (Van Zanden et al., 2012). Thus, our results suggest that parliaments protected (some) European states from the adverse effects of their ruling dynasties' inbreeding.[12]

The paper is organized as follows. Section 3.2 introduces the historical background of European monarchs and Section 3.3 discusses our data sources and coding. Section 3.4 shows our main empirical results and discusses our identification strategy. Section 3.5 examines heterogeneity by institutional constraints on rulers. Section 3.6 concludes.

3.2 Historical Background: European Monarchs, 1000-1800

This section briefly reviews the historical background of European monarchs in the late medieval and early modern period. We pay particular attention to those features that render the setting a rich testing ground for identifying the causal effect of national leaders on state performance. First, national leaders mattered back then: Europe was ruled by monarchs whose actions shaped their countries' performance. Second, they – by and large – ascended to power because they happened to be the oldest surviving offspring of the previous monarch. Third, intermarriage among ruling dynasties was common. These latter two features of the context enable our identification strategy.

3.2.1 Rulers and Country Performance

Historiography and political science is full of examples linking the fate of countries to certain ruler's actions and their ability.[13] Many observers have noted the series of able

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[11] The average coefficient of inbreeding increased by 80% between the 15th and the 18th century. In Northern Europe (comprising the countries of England, Scotland, the Netherlands, Denmark, and Sweden), this increase was particularly pronounced, with 180% as compared to 42% in the remaining countries.

[12] Our results also relate to a literature that studies political dynasties in modern democracies, where some prominent families repeatedly have members elected to important offices (c.f. Dal Bó et al., 2009; George and Ponattu, 2018). In contrast, in our setting, succession was guaranteed by law, and dynasties were the central governing bodies over the course of centuries.

[13] Biographies published by historians consistently emphasize the importance of certain individuals and their leadership qualities in shaping the nations they ruled – e.g. for the U.K. see Roberts (2018) and MacCulloch (2018) for the effects of Churchill's and Cromwell's actions and convictions upon their native England.

rulers accompanying Prussia's rise from small polity to great power.[14] Among many of such examples, Kennedy (1989) notes that one of the factors aiding Sweden's "swift growth from unpromising foundations" was "a series of reforms instituted by Gustavus Adolphus and his aides" increasing the efficiency of administration and allowing Sweden under Gustavus to play an outsized role in the Thirty Years Wars, which, "militarily and economically, (..) was a mere pigmy" when he ascended to the throne. Furthermore, the shortcomings of individual monarchs have been linked to political failures, such as in the case of John I of England, whose personal incapability in military matters resulted in Britain losing most of its continental possessions.[15] Similarly, the rising militarism in Germany, the naval buildup aiming to contest British dominance at sea, the break-up of the intricate system of alliances designed by Chancellor Bismarck are all linked to individual decisions of Emperor Wilhelm II of Germany. Röhl (1996) emphasizes emphasizes Wilhelm's character's role and these decisions in paving the way to World War I.

A Tale of two Carloses

In the empirical analysis, we compare rulers of the same country. In what follows, we provide an illustrative example of such a comparison. Carlos II was king of Spain from 1665 to 1700. Hailing from a line of successive marriages of relatives from the Spanish and Austrian Habsburgs, he was highly incestuous and commonly described an unable ruler with little effective power. While his parents technically were 'merely' uncle and niece, the build-up of consanguinity over previous generations due to marriage among relatives resulted in Carlos' parents sharing as many genes as siblings would. As the

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[14] In particular, Frederick William I. (the "Soldier King," who reigned 1713-1740) and his son, Frederick II. (the "Great," 1740-1786), facilitated the rise of Prussia into the rank of a Great Power of Europe with their administrative reforms and military decisiveness. And albeit – by his fathers achievements – "Frederick the Great came into a rich inheritance, (...) the favorable circumstances do not in the least explain his great success." (Woods, 1913, p. 159). The often idiosyncratic decisions of earlier rulers shaped the polity similarly, as for instance that of Elector John Sigismund to convert to Calvinism in 1613 (Clark, 2007, p. 115).

[15] "John was little, if at all, lagging behind Philip [his adversary] in wealth and resources. The explanation of the defeat [which lead to losing England's continental possessions] does not reside in economics. It rests between John's fault as a commander and his faults as a man." (Bradbury, p. 349)

pedigree in Appendix Figure 3.1 shows, all of Carlos II's grandparents descended from Joanna and Philip I of Castile (Alvarez et al., 2009). The degree of inbreeding was of no concern when Carlos II's parents married in 1649.[16]

The "inbreeding depression" resulting from intermarriage over generations left Carlos II hostage to physical and mental fragility.[17] Carlos II only started talking at age 4, and walking at age 8. Alvarez et al. (2009) describe him as "physically disabled, mentally retarded and disfigured." As Carlos II became king of Spain when he was 4 years old, his mother Mariana became regent and initially influenced his policies. The resulting power struggles between factious rivals to influence Carlos II did not aid in solving the domestic and foreign challenges Spain faced (Mitchell, 2013).[18]

The power struggles that followed Carlos II's death brought a new dynasty to the Spanish throne – the Spanish Bourbons. The ranks of the Bourbon dynasty first led to two relatively undistinguished monarchs.[19] Thereafter, the highly capable Carlos III came to inherit the throne in 1759 via the rules of primogeniture from his brother, who had left no heirs. Spain flourished under his reign, and contemporaries and historians hold him in high regards: He "was probably the most successful European ruler of his generation. He had provided firm, consistent, intelligent leadership (...) [and] had chosen capable ministers" (Payne, 1973, p. 371). Consequently, Carlos III's reign saw the "continued improvement in financial and commercial conditions, including agriculture

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[16] As we discuss below, restrictions on cousin marriage were not enforced among the European nobility, and knowledge about the adverse effects of inbreeding only emerged in the early 20th century and was not widely accepted even in academic circles until the second half of the 20th century. In addition, the 'hidden' degree of inbreeding in Carlos II's pedigree was, if anything, interpreted as a positive feature, signaling a 'clean' royal bloodline (Van Den Berghe and Mesher, 1980; Scheidel, 1995) .

[17] While population biology strongly suggests that inbreeding was further responsible for Carlos II's mental fragility, such assertions cannot be proven for historical cases, because genetic information is not available.

[18] "Diseased in mind and body from infancy, and constantly preoccupied with his health and eternal salvation, Charles II was incapable not only of governing personally but of either selecting his ministers or maintaining them in power. From the assumption of the regency by the Queen Mother, Mary Anne of Austria (...) to the death of Charles II not one of the many individuals who rose to power displayed genuine ability" (Hamilton, 1938)

[19] Philipp V (ruled from 1700 to 1745) and Ferdinand IV (1745-1759) "both were undistinguished rulers frequently incapacitated by near lunacy (Philip v dined at 5 a.m. and went to bed at 8 a.m., refusing to change his clothes)"(Carr, 1991). In both reigns, Spain's economic fortune improved moderately, starting off from the low levels left behind by Carlos II.

and the useful arts" (Woods, 1913, p. 331). Importantly for our instrumental variable strategy, Carlos III's parents were cousins of third degree, and the hidden component of inbreeding was about that of first cousins, both substantially smaller than that of Carlos II a century earlier.

3.2.2 Dynastic Rule and Primogeniture

The vast majority of European monarchs came to power according to fixed rules of accession. While these rules differed across countries and time, primogeniture became increasingly common. Primogeniture determines that the eldest living offspring of the current ruler becomes the country's next ruler. This practice was common on the Iberian peninsula early on, from where it spread to other countries quickly (to England in 1066, and France in 1222). It gradually replaced the two other common forms of successions, that by siblings and other relatives of the current ruler, as well as election of rulers by feudal elites.[20] In most cases, agnatic primogeniture was practiced, implying that he eldest living male offspring was heir apparent. In the absence of a heir, for instance due to premature death of the current ruler dies, the reign passed on to close relatives.[21] Due of primogeniture, monarchs of the same country often were related by blood across many generations. For example, until the French revolution, all kings of France were direct ancestors of Hugh Capet, who had ruled eight centuries earlier, from 987 to 996 and founded the "Capetian dynasty." Not all of the French kings after Capet are counted as member of this dynasty, as a few kings died heirless, and the direct line of succession from father to son broke twice until Napoleon. This happened first in 1328 (triggering a succession crisis that resulted in the Hundred Years War), when the Valois dynasty

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[20] Tullock (1987) describes theoretically that both current monarchs and elites favor primogeniture over other forms of succession, as it delivers political stability. Kokkonen and Sundell (2014) provide empirical evidence for this in our sample period. Often, kings crowned their sons while they were still alive to ensure a stable succession (Bartlett, 2020, p. 93).

[21] In general, the reign passed on to those individuals with the closest genealogical distance to the last male monarch. Whether this includes female lines of succession and the exact definition of genealogical distance differs by ruling dynasty according to their "house law". In some cases, such laws of ascension where incomplete and left multiple potential claimants to the throne. As in the case of the heirless death of Carlos II, such cases often resulted in succession crisis, sparked conflicts, and, later, amendments to succession laws to prevent such.

came to power, and again in 1589 with the rise of the Bourbon dynasty.[22] In general, only slightly more than half of the reigns for which we could unearth such information are clear cut cases of ascension to throne by primogeniture. More than three quarters of successors in Austria, Prussia, and France ascended through explicit primogeniture, but less than half did in England, Denmark, Russia, and Sweden.

3.2.3 Intermarriage Among Dynasties

Also *across* the countries of Europe, monarchs often were related by blood. The leaders of the Spanish and Austrian Habsburgs, for instance, practiced cousin marriages over multiple generations in the 16th century, culminating in Carlos II, as described above. Alvarez et al. (2009) argue that the frequent dynastic marriages ultimately resulted in the extinction of the Spanish Habsburgs. While the Catholic Church had formal restrictions on cousin marriage, these were rarely enforced for European monarchs.[23] In fact, intermarriage among royal dynasties increased throughout the early modern period (Benzell and Cooke, 2018), relatively unfettered by the Catholic Church's ban (which was eased from 7th to 4th degree cousins in 1215), and aided by Protestantism lifting the ban entirely. Even for Catholic rulers, however, the pope could – and usually did – grant "dispensations" (exceptions) from the ban.

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[22] The ascension of the Valois dynasty in 1328 illustrates the complexities of these successions. The sons of Philip IV all died heirless after relatively short reigns, so that multiple candidates were considered for the succession. Primarily the decision was between Philip VI, the son of Charles of Valois, brother of Philip IV, and Edward, offspring of a daughter of Philip IV and Edward II, King of England. The French estates and legal experts then excluded female lines from succession, such that Philip IV became the first king of France of the Valois dynasty. Note that, while starting a new dynasty, he was directly related to the former one, as Philip IV was his uncle. Uncertainties regarding the legal procedure in his ascension to the throne became a stepping stone into the Hundred Years War and led to British kings claiming heritage to the French throne for centuries.

[23] Restrictions on cousin marriage had been put in place starting from the 8th century to inhibit the formation of closed kinship networks through repeated intermarriage (Schulz, 2016). These restrictions increased the likelihood that bequests would fall to the Church (Goody, 1983), and would weaken the political power of kinship networks (Ausenda, 1999).

3.2.4 The Negative Effects of Inbreeding on Capability

Offspring of repeated dynastic marriages were less likely to be capable monarchs. Inbreeding reduces genetic diversity and evolutionary fitness (Robert et al., 2009; Ceballos and Álvarez, 2013; Royuela-Rico, 2020). It also systematically increases the risk of genetic disorders, affecting physical and mental capability. Children of first cousins have a five times higher risk of intellectual disability (Morton, 1978) and their intelligence is reduced by 10% (Afzal et al., 1993). Inbreeding further results in lower height and weight (Fareed and Afzal, 2014b), and it decreases fertility while raising child mortality (Fareed et al., 2017), thus lowering the probability of successfully producing heirs for the dynasty (Alvarez et al., 2009). Indeed, Asdell (1948) shows that more inbred rulers were assessed by Woods (1906, 1913) as systematically less capable – despite the fact that Woods had the opposite hypothesis (see footnote 3).

In sum, our setting features rulers who ascended to power by pre-defined rules, independent of their inherent ability for office. At the same time, the frequent intermarriage and negative effects of incest yield quasi-exogenous variation in these monarchs' ability.

3.3 Data

In this section we describe our dataset of ruler ability and country performance at the reign-level.

3.3.1 Ruler Ability

Our measure of ruler ability comes from the work of Frederick Adam Woods. A lecturer in biology at MIT at the beginning of the 19th century, he took an interest in heredity and, ultimately, history. To understand the heredity of moral and mental status between generations, Woods turned his attention to the royal families of Europe.[24] In his 1906

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[24] The appeal of this group of people to study heredity was manifold to Woods: The pedigrees of royal families were (and are) comparably well documented over multiple generations. Further, for most of these individuals, their life, character, and achievement was documented from letters, court biographies, or other written sources.

publication on “Mental and Moral Heredity in Royality” (Woods, 1906), he “graded” more than 600 individual members of royal families on their mental and moral qualities. This grading (on a 0–10 scale) was based on adjectives used in written sources that describe these individuals. Woods concluded that mental and moral status is heritable.[25] In his endeavor to test for the heredity of mental and moral status, Woods ventured beyond the realm of biology to the “great men” debate in history (Carlyle, 1840). Woods noticed a correlation between able rulers and favorable political and economic conditions in the country they ruled. In his chapter on Portugal, he provided a tabular statement of graded ruler abilities and a brief textual description of Portugal’s material conditions.

Our main data source is Woods’ (1913) publication “The Influence of Monarchs.” In this work, he extended his 1906 tabulation of the ability of rulers and their states’ performance from Portugal to 13 states, ranging from their foundation until the French Revolution.[26] Woods coded the ability of rulers for more than 300 European reigns by classifying the rulers into three distinct classes, namely into those exhibiting superior ability, inferior ability, and those in between.[27] Similar to his earlier 1906 work, this grading is largely based on the assessment of historians and contemporaries, as distilled by Woods from reference works and country-specific histories. Woods assigned a ”+” to able rulers, a ”-” to incapable ones and ”±” to those not clearly capable or incapable. We transform these into ”1”, ”-1”, and ”0” respectively.[28] Out of 331 reigns for which we have information on both the monarchs ability and the performance of the country,

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[25] Woods was part of a (then active) research agenda in biology on heredity sparked by the publication of Darwin’s “Origin of Species” in 1859 and Galton’s “Hereditary Genius” in 1869. Social Darwinism, foremost that of Grant (1919), had an influence on the eugenics crusade in the United States and the Immigration legislation after World War I (Saini, 2019). After World War II and the horrors of the Holocaust, Social Darwinism was largely discredited, as was the concept of heritability of traits such as mental or moral qualities (at the level of societies). While heritability of intelligence at the individual level is sizable (Neisser et al., 1996; Devlin et al., 1997), differences between population groups are resulting from other environmental differences (Lewontin, 1970).

[26] In the appendix we show the covered states in selected years (Figure 3.8) and a timeline of coverage for each state (Figure 3.9). The states covered are Castile, Aragon (Spain), Portugal, France, Austria, England, Scotland, Holland, Denmark, Sweden, Prussia, Russia, and Turkey. In contrast, most of the analysis in Woods (1906) did not extend before the 16th century.

[27] “Out of large groups, some few stand out as distinctly superior, some few as distinctly deficient, and between the two a mediocre class” (Woods, 1913)

[28] Cases where Woods expressed a doubt by, say, ”+ or ±” are averaged, in this case to 0.5. In a robustness check, we recode all these cases conservatively so as to work against our baseline.

124 are rated as clearly incapable, 120 as clearly capable, and 87 as neither.[29] For each of the reigns, Woods provided a brief summary underlying his assessment and references. For instance, Carlos III is described as “enlightened, efficient, just, and sincere. Not brilliant, but had a very well-balanced mind”. Woods is brief on Carlos II, whom he summarized as an “imbecile” with negative virtues.

Anticipating concerns of data quality, Woods appealed to the coarse nature of the measurement and challenged that few revisions would be necessary by other individuals attaining similar assessments of monarch ability.[30] Thorndike (1936) had his student, Dr. Edith E. Osburn, “grade” the morality and intellect on a scale from zero to ten of more than 300 individuals rated earlier in Woods (1906).[31] At the same time, Thorndike had five more research assistants independently do the same coding. This data quality assessment resulted in correlations of the intellectual grade across different graders (including Woods) ranging from 0.73 to 0.82. We similarly asked a research assistant to assess the capability of individual rulers on the three-point scale of Woods (1913) based on articles in online encyclopedias. This exercise also largely confirmed Woods’ coding (see Appendix 3.7.1.3).

We extend Woods’ sample until 1914 and to Hungary and Poland. Similarly to our validation of Wood’s original data, we asked a research assistant to assess the capability of rulers from all of the states covered by Woods reigning after Napoleon until World War I, and for Poland and Hungary. From Woods’ original sources and modern encyclopedias, the research assistant assessed the capability of rulers on the three-point scale ofWoods (1913) for in total 95 reigns.

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[29] Woods collected information for 368 reigns in total. Especially for early and short reigns, Woods did not provide an assessment. In instances of co-reign, as for Ferdinand and Isabella of Castile from 1479 to 1504, we generally take the assessment of one individual if it is only available for one of two rulers. When both are available, we use the assessment of the individual working against our hypothesis.

[30] “As there are only three grades, and the doubtful cases are allotted in a way to give the benefit of the doubt to an opponent of the conclusions, my position in the matter is a very safe one, and the assignment of grades becomes very easy. (...) I am ready to ask – who will challenge more than a very small per cent of these assignments ?” (Woods, 1913, p. 6).

[31] “[She] read what was printed about each of about four hundred of the persons studied by Woods, in each of the six biographical dictionaries used by him. This occupied her about forty hours a week for about eight weeks. She then read through the entire set of references again.” (Thorndike, 1936, p. 322).

Another concern in measurement is bias in Wood's assessment or in the consulted historiography itself. Monarchs that happened to reign in a fortuitous period might have been assessed well because the country did well, not because of their capability. Such a bias is directly addressed by our instrumental variable strategy.

3.3.2 State Performance

We collect two measures of state performance at the reign-level. Our first measure again comes from Woods (1913). Corresponding to ruler ability assessments, he provides assessments of the political and economic ("material") condition during each reign. In particular, he aimed at covering the following affairs: "finances, army, navy, commerce, agriculture, manufacture, public building, territorial changes, condition of law and order, general condition of the people as a whole, growth and decline of political liberty, and the diplomatic position of the nation, or its prestige when viewed internationally," while purposefully excluding "literary, educational, scientific, or artistic activities " (Woods, 1913, p. 10). As with the assessment of ruler ability, Woods coded a three-valued variable summarizing the political and economic performance of the country during each reign as "+","±", and "-", which we transform to 1,0,-1.[32] As with the measure of ruler ability, we extend the sample to include almost a hundred reigns until World War I and in Hungary and Poland, leading to a total sample of 428 reigns for which assessments of ruler capability and state performance are available. Consider again the two Carloses of Spain. The incapable Hapsburg king Carlos II reigned over a country characterized by "misery, poverty, hunger, disorders, decline, especially in agriculture, finances and strength of the army" while a century later, Carlos III reign saw "continued improvement in financial and commercial conditions, including agriculture and the useful arts". As an additional example, consider Maria Theresa, who reigned over Austria from 1740 to 1780, and was judged by Woods as "able and very industrious". Under her reign, "the various portions of the kingdom [were] unified and centralized" and "Austria gained slightly in territory and greatly in prestige", while "industry, commerce, and agriculture

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[32] We asked a research assistant to verify Woods' coding based on online encyclopedias, largely confirming his assessment.

improved." Many of the components of this composite measure are of a rather subjective nature, and furthermore individually assessed and then combined by Frederick Adam Woods, the very same person already assessing the ability of monarchs.

Our second measure of country performance therefore focuses on a continuously and objectively measurable aspect. We calculate the change in area ruled during the reign of each monarch. Abramson (2017) provides borders and the area of the independent polities of Europe at five year intervals from 1100 to 1795. We link these to the beginning and end of each reign and calculate the percentage change in area ruled during reign, $\Delta log(Area)$.[33] Figure 3.11 shows an example of this measure of state performance. Austria during the reign of Maria Theresa lost some areas from 1740 to 1780 (in red) and gained some areas (in green), such that in net terms, Austria increased its area by 7%.

3.3.3 Coefficient of Inbreeding for European Monarchs

Our instrument for ruler ability is the coefficient of inbreeding – the risk of genetic disorders of rulers, resulting from their parents' consanguinity. The first correct measure of the degree of similarity in the genes of offspring due to common ancestors was developed by Wright (1921). This "coefficient of inbreeding" is the probability that both gene copies at any locus in an individual are identical by descent, i.e., from a common ancestor. Offspring of siblings or of parent-offspring couples have $F = 25$, while offspring of first cousins or of uncle-nice couples have $F = 12.5$.[34]

A rich literature in biology documents that higher levels of F result in "inbreeding depression," i.e., lower evolutionary fitness due to the higher average homozygosity, that is, having more gene copies at any locus that are identical by descent (Robert

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[33] We link end dates of reigns to the closest larger five-year observation and start dates to the closest smaller five-year observation.

[34] The coefficient of inbreeding ranges from 0 to 100 (%). Humans inherit one allele at each locus from each parent. Because humans carry two alleles at each locus (humans are "diploid"), the probability that to pass on a particular allele to a particular offspring is 0.5. Hence, the offspring of self-fertilization would have $F = 50$, as there is a one in a half chance for each locus that the entire pair of alleles was passed on. Hypothetically, with repeated self-fertilization, F would approach 100. Offspring of completely unrelated parents have $F = 0$. We provide more detail on the details of the calculation in Appendix 3.7.5.

et al., 2009; Ceballos and Álvarez, 2013). In particular, the effects on many individual physical and psychological traits associated with successful leadership are large and negative.[35] European royal families did not defy the laws of biology. Asdell (1948, p. 175), using Woods' data and the coefficient of inbreeding, documents "a progressive decline in intelligence as inbreeding increases."

We collect F for 312 monarchs from http://roglo.eu/, a crowd-sourced online data source of the genealogy of European noble families. For the 312 monarchs with available information, we first identify the parents. For these, in turn, http://roglo.eu/ calculates the coefficient of inbreeding for their offspring, relying on rich data on relationships between their ancestors.[36] Figure 3.4 shows a histogram of the coefficient of inbreeding for all monarchs in our dataset. The figures also provides two illustrative examples. Carlos II is the individual with the highest coefficient of inbreeding. With $F = 25.36$, he is more inbred than offspring of siblings would be. Yet, his parents were "merely" uncle and niece (which in itself would imply $F = 12.5$). This points to an important feature of our setting: A sizable amount of the observed inbreeding is not the result of just one generation of consanguineous mating, but rather driven by a "build up" of inbreeding over previous generations.[37] We will use this "hidden" component of inbreeding explicitly in robustness analyses later (see appendix 3.7.6.3).

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[35] The literature on leadership traits has emphasized the importance of cognitive capabilities for leadership, see e.g. Judge et al. (2004). Yet empirical evidence causally linking specific traits to leadership success is scant. Adams et al. (2018) show that cognitive and non-cognitive ability, measured during military tests in Sweden, are strongly positive associated with individuals assuming leadership roles – becoming CEO's – later in life. If the allocation of talent works in Sweden, and individuals with leadership abilities do assume leadership roles, these traits are important for leaders. Their evidence on family compared to non-family run business further supports this. Importantly for our argument, there is a large literature documenting that inbreeding negatively affects all of these (Afzal et al., 1993; McQuillan et al., 2012; Fareed and Afzal, 2014b,a).

[36] We cross-checked and validated the coefficients we obtained from http://roglo.eu/ extensively with other publications, among them Asdell (1948) and Alvarez et al. (2009). Turkey is not covered by this source and is thus not included in our IV results.

[37] Consider again the pedigree of Carlos II (Figure 3.1). While Philipp IV, the father of Carlos II, married his niece, past consanguineous marriage weighted heavy in opening up many pathways for the common ancestors Joanna, "The Mad" and her husband Philip generations earlier.

3.3.4 Constraints on Ruler Power

We collect data on the legal and de facto constraints on the power of monarchs from a variety of sources. Our baseline variable refines and extends the measure "constraints on the executive" from Acemoglu et al. (2005), which is available between 1000 CE and 1850 (first at the century level and after 1700 CE in fifty-year intervals). Acemoglu et al.'s measure was coded following the approach of the Polity IV project (Marshall et al., 2017) at the level of today's countries. Using the same coding approach, we refine the coding of "constraints on the executive" on a year-by-year basis at the historical state level, guided by the Polity IV rating and the same primary sources used by Acemoglu et al. (2005). After 1800, we use the year-by-year measure of constraints on the executive from Marshall et al. (2017). Appendix 3.7.10 explains our methodology in detail. Figure 3.7 illustrates the improved measure using England in the turbulent seventeenth century. The black solid line depicts the fact that, according to Acemoglu et al. (2005), England exhibited "Substantial Limitations on Executive Authority" from 1600 to 1700 CE. Our measure, depicted as the dashed green line in the figure, shows the variability in constraints on the monarch in that century. Consider 1629, when parliament was dissolved and "Charles [I] governed without a parliament, raising money by hand-to-mouth expedients, reviving old taxes and old feudal privileges of the crown and selling mentarians contrary to the spirit of the constitution (..)"(Stearns and Langer, 2001). This results in sharp drop of our measure from substantial limitations on the monarchs authority ("5") to unlimited authority ("1"). Constraints became stronger during the "Long Parliament" from 1640-1660, as a consequence of the "Triennial Act [of 1641], requiring the summoning of parliament every three years without an initiative of the crown. (...) [This was] followed by (...) [a] bill to prevent the dissolution or proroguing of the present parliament without its own consent (...)".

Based on our year-reign specific measure for constraints on the executive, we define a ruler to be constrained if the constraints on the executive during the five years prior to the beginning of the reign were (on average) above a specific cutoff. As our baseline, we use "Substantial Limitations on Executive Authority" (the fifth out of seven categories) as this cutoff, but we document robustness to different cutoffs in Appendix 3.7.10.[38]

Second, we use the original measure by Acemoglu et al. (2005).[39] To map the century-frequency to our reign-level data in coherence with our main measure, we assign the average "constraints on the executive" over the five years before a ruler came to power. For example, a ruler ascending to power in 1500 receives the value of the 15th century in the country, and a ruler ascending to power after 1505 the value of the 16th century.

Finally, we use parliamentary activity as a proxy for constraints on the executive. Van Zanden et al. (2012) compile the frequency of parliamentary meetings across European countries from the twelfth to the eighteenth century. Based on this, we calculate the average of parliamentary activity five years before the reign, and identify rulers as constrained if parliamentary activity was above the 95th percentile of the entire sample.[40]

3.4 Main Empirical Results

In this section we document a strong association between the capability of European monarchs and the performance of their countries during their reign. We show that this association is robust to measurement, specification and in different samples. We then proceed by presenting our identification strategy based on inbreeding of dynasties, followed by our IV results.

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[38] This baseline cutoff is defined as follows: "The executive has more effective authority than any accountability group but is subject to substantial constraints by them." In our sample, this applies to 40 monarchs, of which 18 are rulers of England.

[39] The matching from their countries to the states in our sample is mostly straightforward. Following their sources, we assign their measure for Spain to Castile, and do not assign values to Aragon when using their measure.

[40] Van Zanden et al. (2012) collect the information on the relative frequency of meetings of parliaments from a variety of sources. For all countries except Turkey, we can link this to our data set. We link Prussia to the "Brandenburg Diet" and the "Generallandtag" of Austria to the Habsburgs. All other matches are straightforward, as the data is separately available for Scotland and England, Castile (and Leon) which we match to Castile, and Aragon. All other matches are straightforward.

3.4.1 Baseline OLS Results

Our baseline regressions are at the country-reign level:

$$y_{r,s} = \beta RulerAbility_{r,s} + \delta_s + \gamma X_{r,s} + \varepsilon_{r,s} \ , \tag{3.1}$$

where $y_{r,s}$ is either the performance of state s in reign r, assessed by historian Woods (1906, 1913) (and extended and validated by us) or the state's percentage change in land area during reign r. $RulerAbility_{r,s}$ is the assessment of the ability of the monarch of reign r. For a straightforward interpretation of coefficients, we standardize the assessments of state performance and of ruler ability so that both variables have mean zero and standard deviation one.[41] We include state fixed effects δ_s, so that we effectively compare rulers of the same state over time. $X_{r,s}$ are additional control variables, such as fixed effects for time periods or dynasties that rulers belonged to. Throughout, we report standard errors clustered at the state level.

Table 3.1 shows that ruler ability is strongly associated with country performance. Column 1 reports the raw correlation. The coefficient of interest, β, is highly significant and sizable: A one standard deviation increase in ruler ability is associated with a 0.54 standard deviation increase in state performance. [42] Column 2 shows that this association is unchanged when we add state fixed effects, thus comparing only monarchs who ruled the same state (henceforth our baseline specification). The results are also stable when we additionally include century fixed effects.[43] Columns 4-6 use the reign-

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[41] Note that while both are categorical variables, we treat them as continuous variables for ease of estimation throughout the paper. We provide a robustness check using ordered probit below.

[42] Woods (1913) himself had also manually computed the (not standardized) correlation coefficient of 0.6 in his raw data. He asserted a causal direction from monarch ability to country performance: "Only very rarely has a nation progressed in its political and economic aspects, save under the leadership of a strong sovereign." While Woods was well aware of reverse causality concerns, he provided descriptive evidence in favor of this conclusion. We go beyond Woods' findings by exploring richer specifications and, in particular, by providing an identification strategy.

[43] Note that century fixed effects require us to assign reigns to centuries. In column 3 we use the century corresponding to the first year of the reign. Other means of assigning reigns to centuries, e.g., based on the century wherein most of the reign lies, yield quantitatively very similar estimates. Below we present an alternative, more flexible method to filter out time effects: regressions at the ruler pair level, comparing monarchs in different states who ruled

specific percentage change in state area as outcome variable. Since this as on 'objective' measure, it is not subject to concerns about biased coding that potentially affects Woods' state performance measure. For this continuous outcome variable, we again document a significant and sizable association with ruler ability. Again, these results are stable when we include state or century fixed effects. A one standard deviation increase in ruler ability in the same state and century is associated with land area expanding by about 10%.

3.4.2 Robustness

Next, we examine the robustness of our baseline OLS results. Table 3.2 successively reduces the sample. In column 2, we focus on reigns in which the ruler was linked to a dynasty. Thereby we exclude cases of interregna, regencies in which non-royal individuals exerted power, and instances of non-monarchical governance (as in the Netherlands).[44] The coefficient increases slightly and remains highly significant. Column 3 excludes regencies, independent of whether the regent was a dynasty member or not. The coefficient again increases slightly. Note that the variation explained (R^2) actually increases in columns 2 and 3, indicating that indeed monarchs hailing from dynasties are crucial to the relationship between ruler ability and state performance. Column 4 excludes the few instances of foreign rule, e.g., Scotland during the reign of James VI of England. Column 5 excludes all individuals who appeared as rulers in more than a single reign. These are either monarchs that repeatedly came to power in the same country, or who ruled in more than one country contemporaneously. In both columns 4 and 5, the coefficient remains significant and comparable in size to the baseline. Finally, column 6 applies all restrictions of the preceding columns simultaneously. With only about 75% of the initial sample left, the coefficient remains almost unchanged, and the variation in

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contemporaneously.

[44] Interregna are periods between the rule of two monarchs when no monarch is present. Regencies are periods of government by others (regents) in lieu of the designated ruler. Usually, these are close relatives such as the mother of an underage monarch, but sometimes these can be officials or members of the elite. In column 2, we exclude all rulers during whose reign regents from outside their dynasty governed. We still include cases of rule by relatives of the designated heir until the heir assumed office. For example, Mariana was regent for Carlos II of Spain until he reached adulthood, and then tried to regain regency by arguing that he was unfit for office.

country performance explained by ruler ability is actually higher than in the baseline.

We document further robustness checks in Appendix 3.7.2. Table 3.9 documents robustness to measurement. We show that results also hold when we use our own coding of state performance and ruler ability, based on internet encyclopedias, which in turn draw on historical sources. In addition, we find that our results based on Woods' (1913) original coding are highly robust when we exclude cases that Woods coded with intermediate values for state performance or ruler ability, indicating that he felt a clear judgment was not warranted by the underlying information. Finally, results are even robust when we recode all those middling values to work against a positive association between ruler ability and country performance. Table 3.10 shows robustness to different specifications, such as using dummies for different values of state and ruler performance, ordered probit, as well as clustering at the state, century, and dynasty levels.

3.4.3 Heterogeneity

How does the association between state performance and ruler capability vary across time, space, and personal characteristics of rulers? In Table 3.3, we include interaction terms between ruler ability and numerous characteristics.[45] For column 1, we define a dummy indicating whether a monarch was female, which was the case for 39 of the 403 reigns to which a gender was assignable.[46] We find no evidence that the association between ruler ability and country performance varied significantly by the ruler's gender. In column 2, we similarly interact our baseline regression with a variable indicating whether a monarch ascended to the throne before the median age of ascension (28 years). The positive (albeit insignificant) interaction term suggests that the relationship between ruler capability and state performance was somewhat stronger when rulers ascended to the throne early in their lives. Column 3 uses a dummy indicating whether a ruler was raised as designated heir. This means that a ruler was raised as monarch, rather than ascending to the throne because another designated heir unexpectedly died

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[45] We collect the variables used in this section from encyclopedias and biographies, as explained in Appendix 3.7.1.2

[46] For 25 reigns we cannot assign a gender. As explained in Appendix 3.7.1.2, these other instances are reigns were councils are in power or multiple regents are.

earlier, or ascending to the throne by other means.[47] Again, we document a positive yet imprecisely estimated interaction effect. Finally, in column 4, we find a small positive interaction term for the 198 individuals who ascended to power by the rules of primogeniture (overall 356 rulers, for whom this information is unambiguous). We also note that of the dummies in Table 3.3 only one is itself statistically significant. That is, age at ascension, being raised as a designated hair, or ascension due to primogeniture, are by themselves not associated with state performance. The state performance of female rulers was assessed somewhat worse.

Did the relationship between monarchs' ability and state performance change over time? Figure 3.2 depicts the corresponding coefficients by broad time periods, showing a statistically highly significant correlation throughout.[48] After 1500, the coefficient size decreases somewhat. This period also coincides with the rise of parliaments in Western Europe (Van Zanden et al., 2012). Below, we examine whether this trend may have affected the role of ruler ability in their states' performance. Figure 3.3 provides further preliminary evidence on the role of Parliaments: It shows the correlation between ruler ability and state performance for all states in our sample.[49] The association between ruler capability and state performance is relatively similar across states, and it is statistically highly significant for all states except for Denmark.[50] The coefficient is strongest for Prussia, implying that this state fared particularly well under good rulers and/or suffered particularly strongly under bad ones. Prussia institutional setting featured few if any constraints on the monarchs executive power. The other extreme is England, where the association between ruler ability and country performance is less pronounced. This is particularly true after 1600, when the English Parliament gained

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[47] Note that we were only able to assess whether monarchs were raised for particular roles for 199 observations, of which 149 where raised as monarchs.

[48] As before, reigns are allocated to time periods according to the start year of each reign. Table 3.13 in the appendix provides point estimates for these broad periods and also in a more disaggregate fashion, by century.

[49] Table 3.12 provides the corresponding regression estimates.

[50] A possible explanation is that Danish crown had not fully transitioned to a hereditary monarchy. Danish kings were de jure elected by the nobility. However, de facto the oldest son of ruler was usually elected as his successor (Bartlett, 2020, p. 398). Therefore, Danish monarchs actually might been impeded by relatively strong constraints on their executive power, and the absence of an effect could be in line with our interpretation in section 3.5.

power vis-à-vis the Crown. For this period, we observe no more relationship between ruler ability and the country's performance.

3.4.4 IV Results

In what follows we provide evidence for a causal relationship between ruler ability and country performance. We first discuss our identification strategy based on primogeniture and inbreeding. Then we introduce our instrument – the coefficient of inbreeding – and document that it is a strong predictor of ruler ability. The corresponding IV results reveal a positive causal effect of ruler ability on country performance in the second stage.

Identification

An interpretation of our OLS coefficients as a causal estimate is subject to numerous concerns. Omitted variables could influence both the performance of a country and the ability of the ruler in power, or the fact which ruler ascended to power. Similarly, reverse causality, by which country performance drives ruler ability is a concern: Positive expectations about a countries' economic and political future might lead to the selection or emergence of capable rulers.

Our identification strategy enables us to address both of these concerns. It relies on two crucial features. First, primogeniture results in a pre-determined appointment independent of ruler ability. By primogeniture, the next ruler is automatically set to be the individual with the closest genealogical distance to the last male monarch. Second, drawing on a literature in population biology and rich genealogical data, we leverage quasi-random variation in ruler capability. Centuries of intermarriage within and between the ruling dynasties of Europe resulted in a fairly sizable degree of 'hidden' genetic closeness between the potential marriage partners of Europe's monarchs. Unbeknownst to the royal families, Woods, or the historians on which Woods based his assessments on, such genetic closeness between partners carries an increased risk of genetic disorders for their offspring. These disorders in turn likely render rulers incapable to effectively fulfill the duties of their offices.

First Stage

Our first stage shows that monarchs with a higher coefficient of inbreeding are significantly less capable rulers. Formally, our first stage is:

$$RulerAbility_{r,s} = \delta F_{r,s} + \delta_s + \varepsilon_{r,s} \ , \tag{3.2}$$

where $F_{r,s}$ is the coefficient of inbreeding of the ruler of state s in reign r (as described in Section 3.3.3), $RulerAbility_{r,s}$ is the capability of said ruler, and δ_s are state fixed effects. Again, we cluster standard errors at the state level.

Column 1 of table 3.4 documents a strongly negative raw relationship between a rulers coefficient of inbreeding and her or his capability. Column 2 adds country fixed effects and is our preferred first stage estimate. The effect is sizable and highly significant, even when comparing only rulers within the same country: Hypothetically increasing the coefficient of inbreeding of a ruler by one standard deviation decreases ruler ability by 0.3 standard deviations (standardized beta, unreported). Figure 3.5 shows a binned scatter plot of the variation underlying column 2.

The following two columns of table 3.4 show that the first stage is not driven by individuals with extremely high coefficients of inbreeding. Excluding individuals whose parents were as related as siblings are (column 3) or uncle-niece are (column 4) if anything increases the estimate, which retains its significance. In column 5, we focus on cases of documented primogeniture, that is those cases for which we could explicitly confirm from historical sources that the ruler ascended to power according to the laws of primogeniture as the closest relative of the former ruler.

Second Stage Results

Table 3.5 presents second stage results. In column 1 we again display the baseline OLS result. Column 2 limits the sample to those reigns for which F could be collected.[51] Column 3 then shows IV results. Note first the instrument is strongly relevant (F-statistic of 44). The IV coefficient is positive and strongly significant. The ability of monarchs had a strong positive effect on the performance of the countries they reigned.

The following three columns again direct attention to smaller samples, excluding offspring of highly consanguineous relationships in columns 4 and 5. Column 6 focuses on the 120 cases for which we could confirm ascension by primogeniture. The IV coefficients tend to be larger than the corresponding OLS coefficients, if not significantly different from them. For instance, in column 6, the corresponding OLS coefficient is 0.70 (unreported), and the IV coefficients is 0.89, 27% larger. These larger coefficients could result from Woods' coding and IV addressing this measurement problem. Woods had a bias in favor of rulers hailing from old dynasties, and thus assigned better grades to inbred rulers, and correspondingly, worse grades for less inbred rulers. The instrument then corrects this biased measurement and uncovers larger effects. Indeed, Woods' hypothesis was that intermarriage among what he considered the superior stock of royal families lead to more capable rulers works against us.[52] Further, the correct measurement of inbreeding was unknown when Woods was writing in 1913, and the fact that inbreeding has negative consequences was not widely accepted in academic circles decades later.[53] Therefore, the timing of scientific progress on inbreeding renders breaking the exclusion restriction purely by Woods' or historians' assessment of monarchs unlikely.

Our IV strategy addresses reverse causality and some, but not all, omitted variable

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[51] This mainly excludes interregna (periods without rulers), regencies where the regent was not of dynastic descent, phases of republican government, as in the Netherlands in some periods, and cases where our research assistant could not confidently assign ruler ability or state performance.

[52] "The very formation of royal families was thus a question of selection of the most of able in government and war. From their intermarriage with their own kind, in connection with the force of heredity, we find an explanation in their relative superiority"(Woods, 1906, p. 302). This turned out to be wrong later, as Asdell (1948) showed that opposite is true due to inbreeding.

[53] In 1927, Bronislaw Malinowski, one of the "founding father[s] of social anthropology" (Young, 2004), stated that "biologists are in agreement that there is no detrimental effect produced upon the species by incestuous unions" (Malinowski, 1927). See Wolf (2005).

biases. In particular, if Woods first observed the ability of rulers and this assessment biased his coding of countries, then our IV does not address this. To speak to that concern, columns 7 and 8 turn to our second outcome variable, the change in land area during the tenure of each monarch. Again, both OLS and IV coefficients point to a positive, large, and significant effect of ruler ability on country performance.

Threats to Identification

We discuss several threats to our identification strategy in separate appendices. First, we show that monarchs anticipating the negative effects of marriage between technically close relatives – such as between uncles and nieces – does not drive our results. Using only the "hidden" component of the coefficient of inbreeding – that coming from distant common ancestors rather than recent marriages among kin – yields larger IV coefficients (Appendix section 3.7.6.3). The exclusion restriction would further be violated if royals marry kin when state performance is low, *and* past bad state performance lowers performance during the reign of their offspring. We show that past state performance does not drive current state performance or lead to differently able leaders. Therefore it does not affect our IV results (Appendix section 3.7.6.1).[54] Neither does conflict, as we document in Appendix section 3.7.7.2. Further, if monarchs were to select the most able leader among their sons as successor, this works against our first stage. Brothers share the same coefficient of inbreeding, and we would be less likely to observe the worst realizations of the potential for genetic disorders.

Lastly, note that the monotonicity assumption required for IV is likely fulfilled. In our setting, this assumption requires that the instrument does not trigger "defiers," i.e., that inbreeding does not (by accident) lead to ingenious leaders. The literature in genetics documents that "inbreeding depression" only has negative effects on fitness (c.f. Robert et al., 2009; Ceballos and Álvarez, 2013), and therefore in all likelihood on leader ability. It is also not the case that inbreeding increases variance in ability, i.e., it is essentially impossible that inbreeding leads to "genius" by accident.

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[54] Relatedly, we show that our results are similarly not driven by strategic marriages *outside* of the kin network in Appendix section 3.7.6.2

3.5 Constraints on Ruler Power

Were incapable rulers always bad for the country they reigned? The modern literature in political economy and management suggests that leaders matter particularly when they act in institutionally unconstrained environments. Clark et al. (2014) show that "leaders matter most when ownership and governance structures correspond with a weak or ambiguous institutional logic". Jones and Olken (2005) finds particularly strong effects for autocratic leaders. While in our setting most leaders are autocrats, there remain important differences in how their actions were legally and de-facto constrained over time and space. Besley and Reynal-Querol (2017) document higher economic growth under hereditary leaders when constraints on them are weak using modern data from 1875 onwards. In this section, we systematically measure these constraints for our data set and document that institutions mitigated the effects of rulers in our data set. We first describe a motivating example – monarchs in England only mattered before a strong parliament emerged – and then present our results. A separate appendix provides more detail on measurement and additional robustness.

3.5.1 Example: Constraints on England's Monarchs in the 17th Century

Consider the cross-country variation of our baseline OLS association documented before. The coefficient of England was rather small, especially when compared to other Western European monarchies of Europe, such as France and Spain. In figure 3.6, we split England into two separate observations, one containing the reigns before the turbulent seventeenth century and one after (including it). In the seventeenth century, civil conflict and the Glorious Revolution lead to increased constraints on the monarch in power. As evident from the figure, England before then was a rather standard Western European country in terms of the effect we document. After 1600, we fail to document an effect of the ability of monarchs on country performance for England.

3.5.2 Results: Constrained Monarchs Matter Less

To assess whether constrained monarchs matter less we estimate interaction models:

$$y_{r,s} = \beta_1 RulerAbility_{r,s} \times Constr_{r,s} + \beta_2 RulerAbility_{r,s} + \beta_3 Constr_{r,s} + \delta_s + \varepsilon_{r,s} \quad (3.3)$$

where, $y_{r,s}$ is either of our proxies of state performance of state s in reign r, $RulerAbility_{r,s}$ is the assessed capability of the individual monarch, and $Constr_{r,s}$ is a dummy variable indicating whether a ruler was constrained in her or his actions, according to the various measures described in Section 3.3.4. Finally, δ_s denotes state fixed effects.

In the first three columns of Table 3.6 we present results of OLS estimations using the three data sources as a base for the dummies indicating constrained monarchs. In column 1, "Constrained Ruler" indicates whether, five years prior to the start of a reign, constraints on the executive were substantial, based on our own compiled measure at the year level.[55] As is evident, there is a significant and sizable difference in whether the ability of monarchs matters for state performance according to the institutional set-up in place *before* the monarchs ascended to power. On the other hand, this institutional feature by itself only has a small and statistically insignificant effect on country performance. In column 2, we define constrained rulers based on the data of Acemoglu et al. (2005), available at century and then 50 year intervals. Constrained rulers' ability still matter less, but constraints by themselves now exhibit a significant and sizable effect on state performance. Column 3 repeats the analysis, using the activity of parliaments measure of Van Zanden et al. (2012) to construct the dummy indicating constrained rulers. This different indicator confirms the baseline result from both earlier columns: Throughout, capable rulers are associated with better state performance, and less capable rulers with worse performance, while this association is weakened for constrained rulers. Column 4 documents that this similarly is the case when using the coefficient of inbreeding as an instrument for ruler ability. Columns 5 and 6 show comparable results when using the change in area ruled as our proxy for country performance.

In Appendix section 3.7.10, we provide further detail on the measure of constraints

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[55] In Appendix 3.7.10 we provide detail and an example of this measure.

on executive and an example of its year-by-year variation. Further, we provide robustness to different cutoffs and using only monarchs ascending to power according to primogeniture.

In sum, our baseline association is weaker for constrained monarchs. Whether they were capable or not was of less importance when and where their actions were partially bound by strong parliaments. In our setting parliaments, and therefore the constraints on monarchs, only gradually became stronger and especially pronounced in Northern Europe (Acemoglu et al., 2005). At the same time, the dynasties ruling Europe increasingly drew on an ever smaller pool of potentially suitable royal marriage partners, reflecting the success of preceding generations in consolidating the map of Europe. In turn, this increased the coefficient of inbreeding throughout, and particularly so Northern Europe. Therefore, the earlier emergence of strong parliaments there may thus have shielded states there from the likely negative capabilities of ever more inbred royal elites.

3.6 Conclusion

The importance of individual leaders for the course of history has been subject to continued debate since the times of Napoleon. The Emperor of the French also illustrates a central identification problem: rather than 'great men' shaping history, historical opportunities may give rise to 'great men,' who find their way into office even when born to a modest family on a far off Mediterranean island. In other words, it is hard to disentangle a causal effect of leaders on their country's performance from unobserved factors or even reverse causality. We explore the period that has been most prominently debated in this context: Europe between the 10th and 20th century.

This paper is the first to provide systematic causal evidence that more capable European rulers boosted outcomes for the states they governed. To identify these effects, we explored the fact that European monarchs ascended to power by primogeniture, independent of their ability. In addition, ruler ability varied because of century-long inbreeding within dynasties. The detrimental effects of inbreeding were unknown until the 20th century; in fact, a popular belief among European dynasties was that kin marriage helped to preserve royal virtues. In addition, a significant part of consanguinity (the degree of genetic similarity) was 'hidden' in the history of kin marriage during previous generations. In combination, these features yield quasi-random variation in ruler ability, allowing us to identify the effects of ruler ability on state performance. We find sizeable coefficients, with a one standard deviation increase in leader ability lifting a country's performance by about one standard deviation, corresponding to a 10 percent expansion in its land area. Our results thus suggest that European rulers did 'make history,' with their actions shaping the European map during the period that laid the foundations for modern nation states.

We also find that institutional constraints checked the power of European monarchs, showing that the relationship between ruler ability and state performance is muted in states with strong parliaments. Parliaments, in turn, were most active in Northern Europe, where inbreeding of dynasties surged between the 15th and 18th century. Our results suggest that parliaments shielded Northern Europe's states from the adverse effects of inbreeding within their ruling dynasties.

The basis of our empirical analysis is a novel reign level dataset for European states over a horizon of several centuries. We are in the process of adding more states, as well as disaggregated measures of state performance during the various reigns, such as administrative efficiency and the implementation of reforms. Our dataset will allow researchers to study Europe's economic history through a novel, sharper lens.

FIGURES

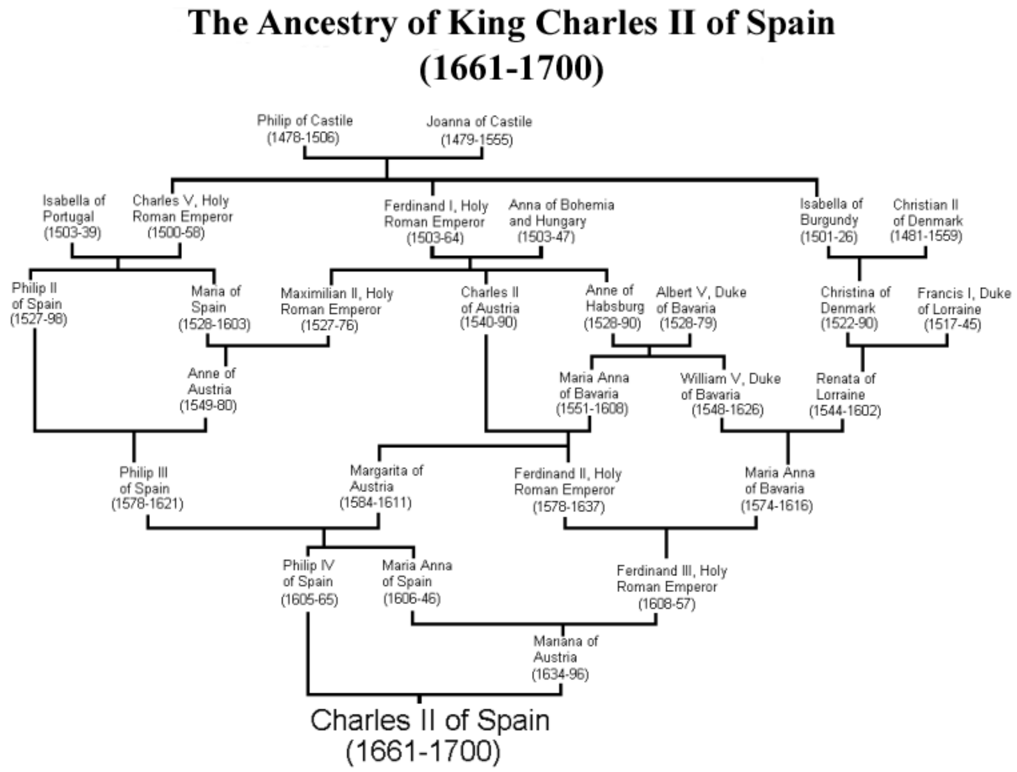


Figure 3.1: Pedigree of Carlos II. of Spain

Note: The figure shows the pedigree of Carlos II., King of Spain from 1665 to 1700. Note the intricate links to common ancestors of both his parents, stretching back over multiple generations.

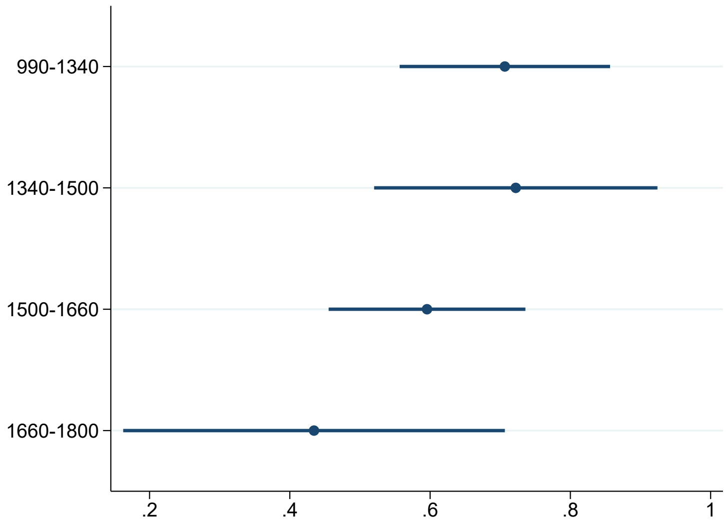


Figure 3.2: Association of Monarch Ability and Country Performance by Time Period
Note: The figure shows coefficient of broad time periods and 90% confidence intervals from a joint OLS estimation including country fixed effects and using standard errors clustered at the state level.

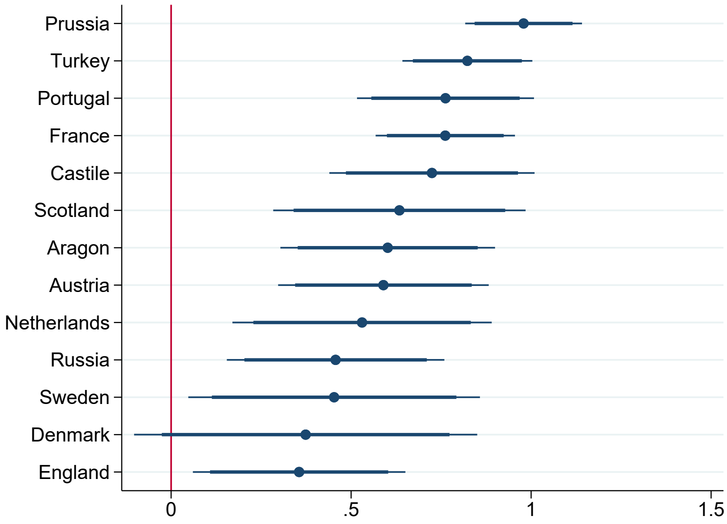


Figure 3.3: Association of Monarch Ability and Country Performance by Country
Note: The figure shows coefficient of each country and 90% confidence intervals from a joint OLS estimation including country fixed effects and using standard errors clustered at the state level.

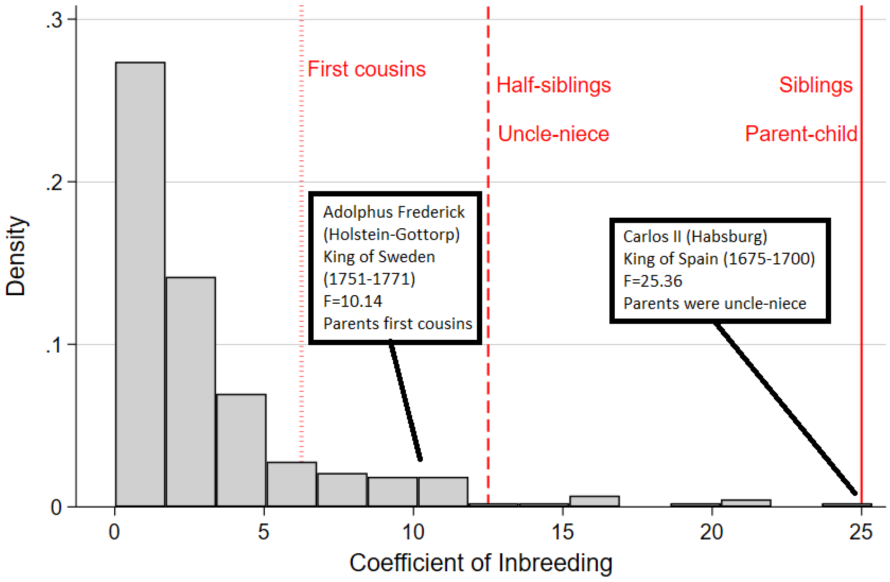


Figure 3.4: Histogram: Coefficient of inbreeding of Monarchs

Note: The figure shows the "coefficient of inbreeding"(F) – the instrument for ruler ability in our analysis – for the 246 European Monarchs. F=0 indicates no relation among the parents of a monarch, F=50 would theoretically result from self-fertilization.

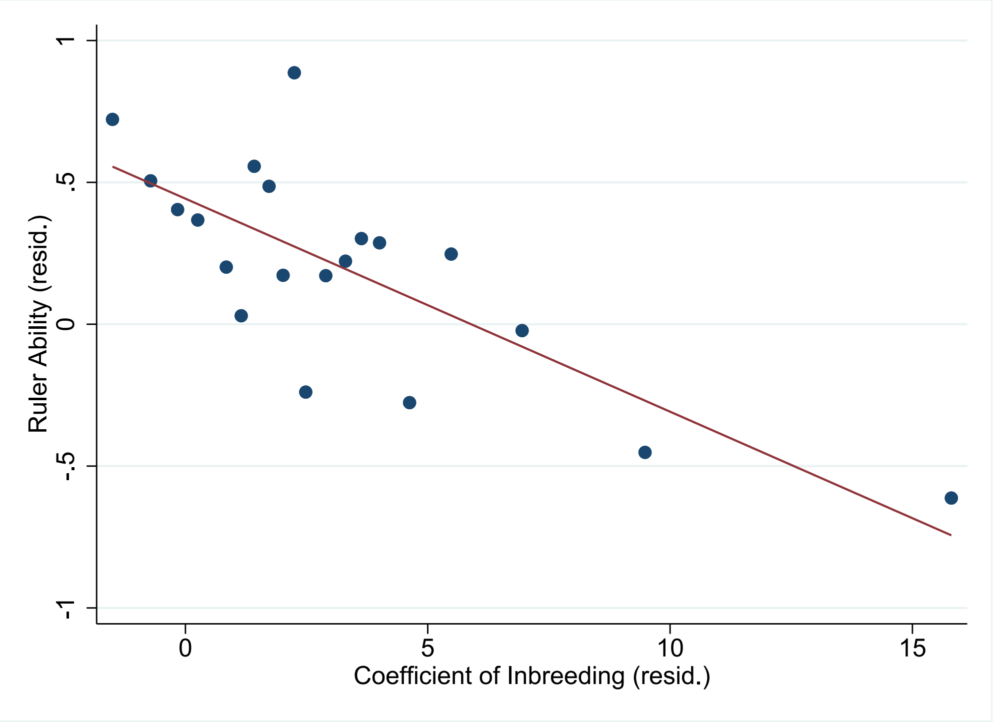


Figure 3.5: First Stage: Binscatter with Country Fixed Effects

Note: The figure shows a binned scatter plot of the first stage between a monarch's coefficient of inbreeding and the ability of the monarch. The first stage includes country fixed effects.

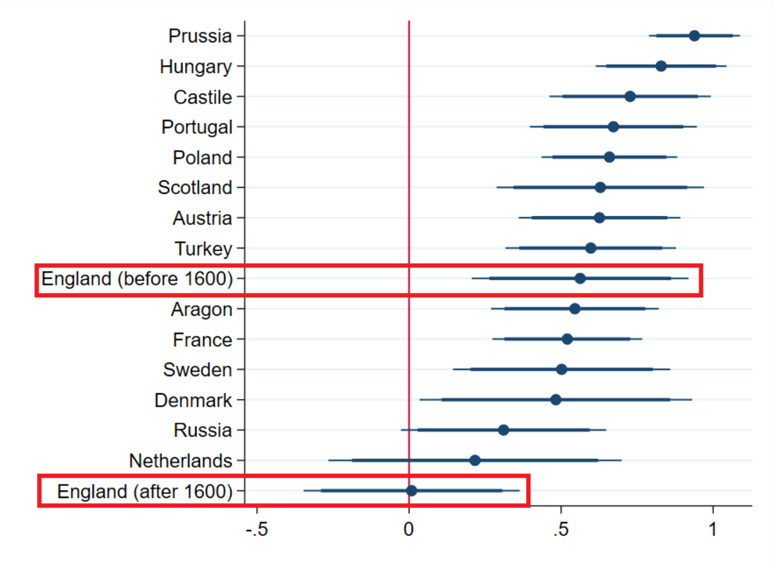


Figure 3.6: Association of Monarch Ability and Country Performance by Country - England Before and After 1600
Note: The figure shows coefficient of each country and 90% confidence intervals from a joint OLS estimation including country fixed effects and using standard errors clustered at the state level. England is split into two separate observations, one including all reigns before 1600, and a second one including all those after 1600.

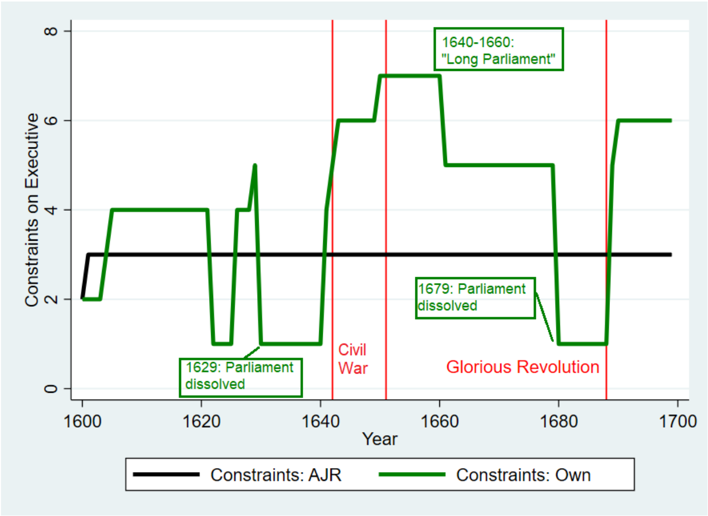


Figure 3.7: Constraints on Executive: Year-by-year, 17th Century England
Note: The figure shows changes in constraints on the executive for England in the 17th century.The black solid lines depicts the data in Acemoglu et al. (2005), while the green dashed line depicts our yearly measure.

TABLES

Table 3.1: Monarchs and State Performance – OLS Results

Dependent Variable as Indicated in Table Header

Dep. Var.	Performance of State			$\Delta log(Area)$		
	(1)	(2)	(3)	(4)	(5)	(6)
Ruler Ability	0.621^{***}	0.620^{***}	0.621^{***}	0.117^{***}	0.114^{***}	0.101^{***}
	(0.051)	(0.050)	(0.041)	(0.032)	(0.035)	(0.029)
State FE		✓	✓		✓	✓
Century FE			✓			✓
R^2	0.39	0.41	0.43	0.07	0.11	0.14
Observations	334	334	334	298	298	298

Note: This tables documents a strong relationship between ruler ability and state performance. The latter is based on the coding by Woods (1913) in columns 1-3; in columns 4-6, it is the change in a state's land area during a monarch's reign. All regressions are run at the reign level. Standard errors clustered at the state level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.2: Robustness: Different Samples

Dep. Var.: Performance of State During Reign of Monarch

	(1)	(2)	(3)	(4)	(5)	(6)
Note:	Baseline	Only Dynasty Members	Exclude Regencies	Exclude Foreign Rule	Exclude Same Rulers	All Restrictions
Ruler Ability	0.620^{***}	0.656^{***}	0.686^{***}	0.630^{***}	0.615^{***}	0.669^{***}
	(0.050)	(0.058)	(0.069)	(0.051)	(0.053)	(0.070)
State FE	✓	✓	✓	✓	✓	✓
R^2	0.41	0.46	0.50	0.43	0.42	0.50
Observations	334	288	262	324	316	235

Note: This table documents the robustness of our baseline regression (col 2 in Table 3.1) to using different samples. See Section 3.4.2 for a detailed description of the sample restrictions. All regressions are run at the reign level. Standard errors clustered at the state level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.3: Baseline Heterogeneity

Dep. Var.: Performance of State During Reign of Monarch

	(1)	(2)	(3)	(4)	(5)
Dummy for:	Female	Young Ascension	Designated Heir	Regicide (lagged)	Primogeniture
Dummy × Ruler Ability	0.013	0.092	0.039	-0.080	0.191
	(0.130)	(0.107)	(0.190)	(0.182)	(0.117)
Dummy	-0.210*	-0.039	-0.221	-0.204	0.053
	(0.108)	(0.071)	(0.150)	(0.217)	(0.085)
Ruler Ability	0.619***	0.565***	0.569***	0.633***	0.485***
	(0.065)	(0.101)	(0.184)	(0.066)	(0.097)
State FE	✓	✓	✓	✓	✓
R^2	0.42	0.41	0.46	0.40	0.40
Observations	308	301	140	187	272

Note: This table shows results of interacting the baseline regression with reign. In column 2, the interaction variable is a dummy for rulers ascending to the throne below median age of 28 years. In column 3, it indicate rulers who were being raised as designated heir, while in column 4 it indicates rulers ascending to power according to rules of primogeniture. All regressions are run at the reign level. Standard errors clustered at the state level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.4: Inbreeding and Monarch Ability - First Stage Results

Dependent Variable: Ruler Ability

	(1)	(2)	(3)	(4)	(5)
Note:			F < 25	F < 12.5	Documented PG†
Coefficient of Inbreeding	-0.068***	-0.077***	-0.077***	-0.084***	-0.057***
	(0.012)	(0.012)	(0.014)	(0.017)	(0.012)
State FE		✓	✓	✓	✓
R^2	0.09	0.15	0.15	0.12	0.15
Observations	234	234	233	226	136

Note: This table shows results of first stage regressions of each monarch's coefficient of inbreeding on ability as assessed by Woods (1913) and us. The coefficient of inbreeding measures the degree of similarity in the genes of offspring due to common ancestors, and the thus increased risk of genetic disorders resulting from the consanguinity of the monarch's parents. Column 3 excludes Carlos II. of Spain, whose parents shared as many genes as offspring of siblings do. Column 4 excludes all Monarchs whose parents share as many genes as offspring of half-siblings do. All regressions are run at the reign level. Standard errors clustered at the state level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

† Subsample includes only documented cases where rulers ascended to power due to primogeniture.

Table 3.5: Monarchs and Country Performance - IV Results

Dependent Variable as Indicated in Table Header

Dep. Var.	Performance of State						$\Delta log(Area)$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimation:	OLS	OLS	IV	IV	IV	IV	OLS	IV
Note:	Baseline	Only rulers with F		F < 25	F < 12.5	Primogeniture[†]	Only rulers with F	
Ruler Ability	0.620^{***}	0.628^{***}	0.794^{***}	0.762^{***}	0.647^{***}	0.912^{***}	0.113^{**}	0.177^{***}
	(0.050)	(0.055)	(0.099)	(0.119)	(0.183)	(0.169)	(0.037)	(0.051)
State FE	✓	✓	✓	✓	✓	✓	✓	✓
First Stage F-statistic			42.15	31.47	24.30	22.21		339.06
R^2	0.41	0.41					0.14	
Observations	334	234	234	233	226	136	204	204

Note: This table shows the results of IV regressions of ruler ability on country performance. The instrument, the coefficient of inbreeding, measures the increased risk of genetic disorders resulting from the consanguinity of the monarch's parents. Columns 1-6 use the assessment of political and economic conditions during each monarch's reign from Woods (1913) as measure of country performance (extended by us for post-Napoleon period, as well as Hungary and Poland), and column 7-8 use the change in land area during each monarchs reign, calculated from Abramson (2017). Except for column 1, all columns exclude rulers for which no coefficient of inbreeding is available. Column 4 excludes Carlos II. of Spain, whose parents shared as many genes as offspring of siblings do. Column 5 excludes all Monarchs whose parents share as many genes as offspring of half-siblings do. All regressions are run at the reign level. Standard errors clustered at the state level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

[†] Subsample includes only documented cases where rulers ascended to power due to primogeniture.

Table 3.6: Constrained Monarchs Matter Less

Dependent Variable: Change in Land Area during Reigh $\Delta Log(Area)$

	(1)	(2)	(3)	(4)	(5)	(6)
Constraints coding:	Author's Coding		AJR (century level)		Van Zanden et al.	
Estimation:	OLS	IV	OLS	IV	OLS	IV
Ruler Ability	0.106^{***}	0.156^{***}	0.115^{**}	0.205	0.098^{***}	0.183^{***}
	(0.033)	(0.045)	(0.039)	(0.123)	(0.029)	(0.059)
Constrained Ruler	0.128^{***}	0.056	-0.020	0.069	-0.020	-0.111
	(0.009)	(0.034)	(0.079)	(0.183)	(0.026)	(0.077)
Constrained Ruler× Ruler Ability	-0.129^{***}	-0.107^{**}	-0.103^{**}	-0.061	-0.090^{**}	-0.185^{*}
	(0.034)	(0.037)	(0.039)	(0.170)	(0.036)	(0.098)
First Stage F-Statistic		16.21		10.43		16.29
R^2	0.10		0.11		0.09	
Observations	295	200	269	178	269	203

Note: This table shows results of regressions of ruler ability on country performance, mediated by whether a monarch was constrained in his executive power. We define a monarch to be constrained if the "Constraints on the Executive" variable (from Acemoglu et al. (2005) in column 2 and recoded by us at the yearly level for all columns except 2 and 3, for details see section 3.5 and Appendix section 3.7.10) indicates at least "substantial limitations" on the executive. In column 3, we define a monarch to be constrained if the country was above the 90th percentile of the measure of parliamentary activity from Van Zanden et al. (2012). All regressions are run at the reign level. Standard errors clustered at the state level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

3.7 Appendix to Chapter 3

3.7.1 Data: Coverage, Validation, and Detail

3.7.1.1 States in the Sample

Figure 3.8 depicts the boundaries of states in our sample at four points in time: 1200, 1400, 1600, and 1790. Note that many states started out small and come to dominate the map over time.

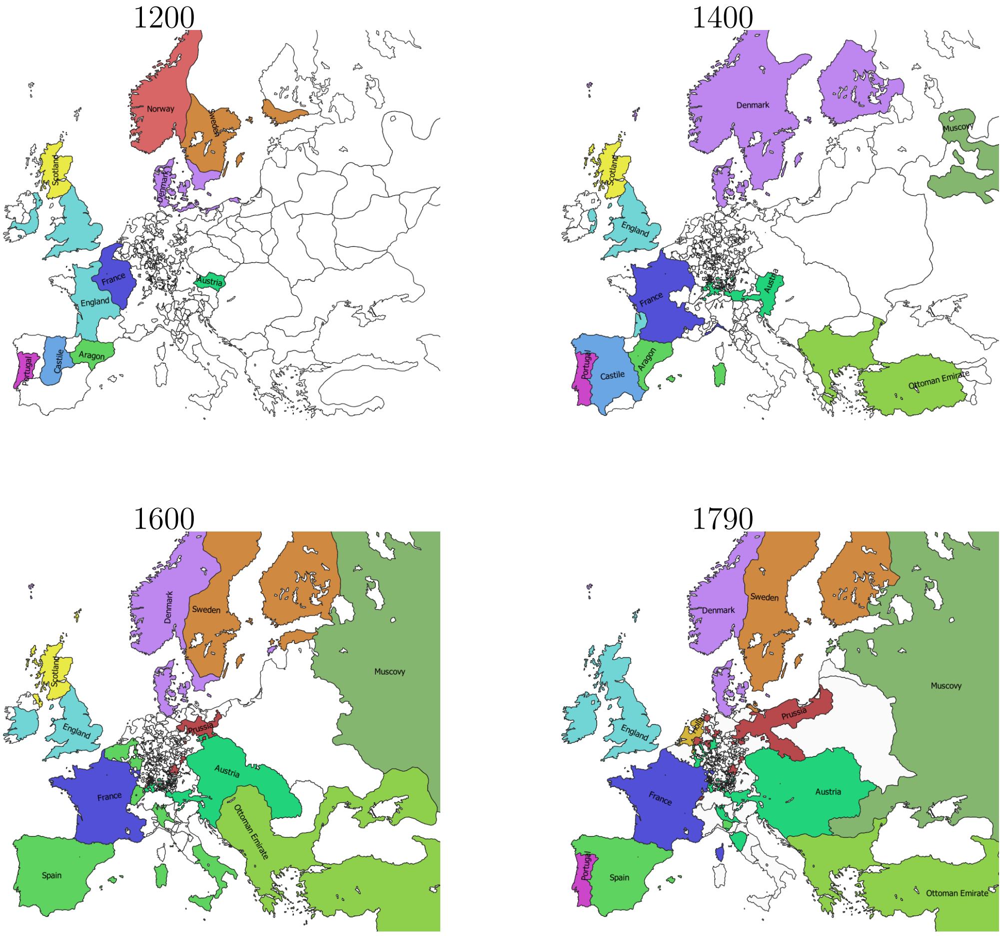


Figure 3.8: States in Sample

Note: The figure shows the boundaries of states in our sample at four points in time: 1200, 1400, 1600, and 1790. Data on state boundaries comes from Abramson (2017).

Figure 3.9 provides a time line for all states in our sample. The earliest state to enter our sample is France (in 990, when Hugh Capet founded the Capetian dynasty), and the last state to enter is Sweden, after it split from Denmark in 1623 under Gustavus Vasa to become a separate political entity.

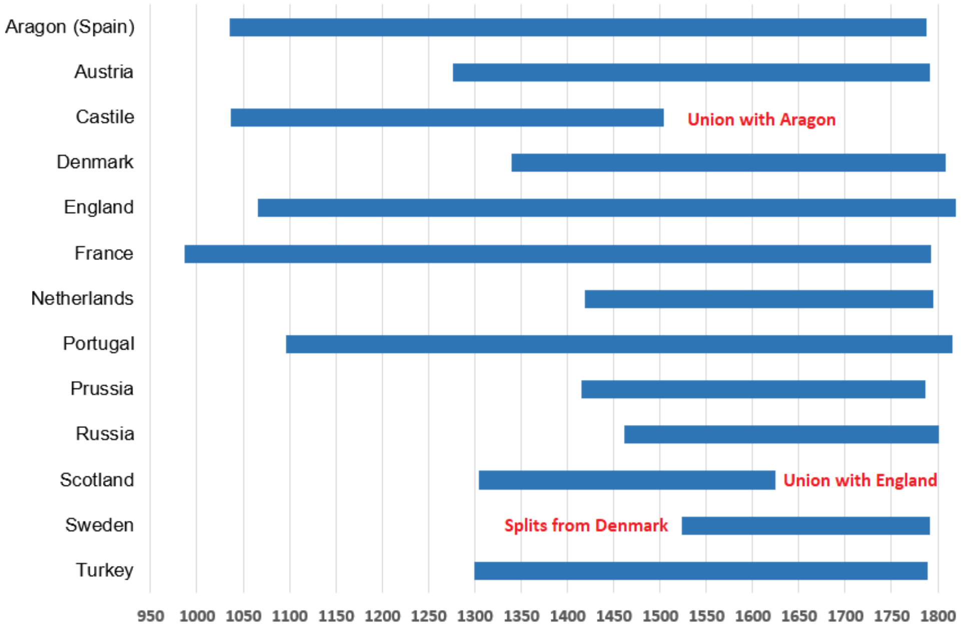


Figure 3.9: Timeline of Sample Coverage: States in Sample

Note: The figure shows the states in our sample together with the time period over which they are covered. See text for detail on the starting (and, where applicable) end point of coverage for each state.

3.7.1.2 Detail: Data Set and Variables

Data Set The data set is based on a list of reigns for 13 states. Specifically, Woods (1913) provides a table, listing for each reign the time period, the name of the ruler (or a description of the status when no monarch reigned, such as for interregna or Republican government in the Netherlands), an assessment of the rulers' ability, as well as the performance of the state during this reign. The latter two are coded categorically ranging from "-" to "+". We cross-checked this list of reigns and the time horizons with encyclopedias and adjusted accordingly with the more recent information in encyclopedias.

Sample Size Table 3.7 provides detail on the sample size. In total, 368 reigns are recorded. For 352 of these, Woods was able to assess state performance. The others are either very short reigns, other reigns for which Woods was not able to make a definitive assessment based on scarce sources, or those (brief) ones he did not include, but added by us. For 338 reigns, Woods assessed the ability of the ruler. He was unable to do so for instances where rule was short or for episodes of Republican government in the Netherlands. In total, both our main independent and dependent variable are available for 333 reigns. This however still includes episodes of regencies, i.e. when some other individual or a group of individuals, or interregna, i.e. periods between reigns with no ruler assigned. Focusing on those reigns where one individual ruled (and our main variables are available) results in 307 observations. Only for 233 of these were listed in our genealogical data source, so that we have measures of the coefficient of inbreeding for those, while only for 270 we could assess whether they ascended to power according to primogeniture.

Additional Variables Additional to the variables described in section 3.3 of the paper, we collect information on rulers where possible. Specifically, we collect for each monarch, the birth and death year, sex of each monarch, identify the dynasty the monarch belonged to, and assess whether the monarch was raised as a designated heir or rather educated for a different role (say, for ecclesiastical life as a younger born royal

Table 3.7: Sample

Sample	Observations
All reigns	368
Reigns with assessed state performance	352
Reigns with assessed ruler ability	338
Both	333
Both + individuals (gender assigned)	307
Both + genealogical data	233
Both + primogeniture	270
Both + primogeniture + genealogical data	222

Note: This table provides details on the sample of reigns, and sample size for different variables.

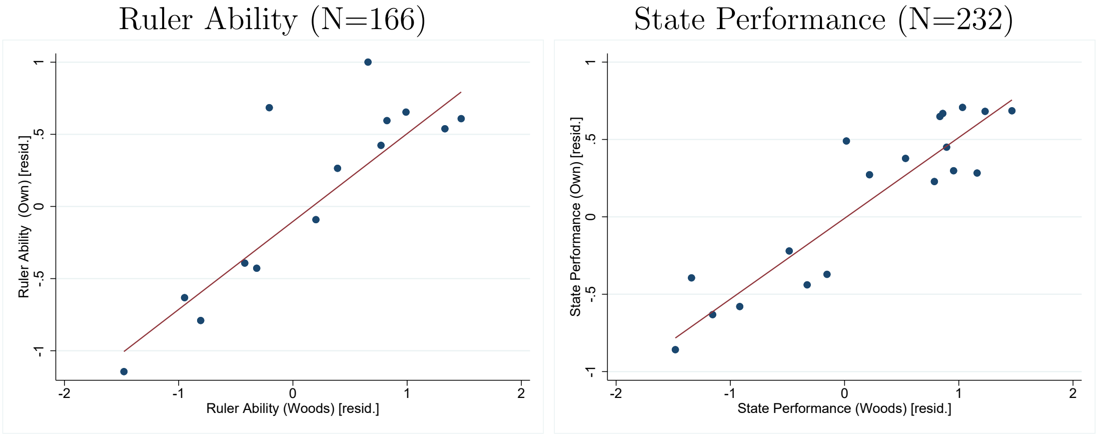
offspring, or as in the case of Catherine I. of Russia, for neither of such roles, but as a orphaned household servant), and whether the monarch ascended the throne due to primogeniture as heir apparent (or, due to agnatic succession, by election of a council, or by starting a new) dynasty. We collect this information from the English-language Wikipedia, but amend it whenever required by information from the corresponding national language Wikipedia.

3.7.1.3 Validation of the State Performance and Ruler Ability Coding

To check Woods' (1906; 1913) coding of state performance and ruler ability, we asked a research assistant to review the evidence in various encyclopedias and devise own assessments of ruler capability and state performance.

The left panel of figure 3.10 provides a binned scatter plot of our research assistant's assessment of monarchs ability with that of Woods. A clear assessment was possible based on online encyclopedias only for 166 rulers. In 94 out of 166 assessed cases, our research assistant reached the same assessment as Woods did, while in 20 he reached the opposite assessment ($\rho = 0.52$). Those examples for instance include Peter III of Russia. He ruled for less than a year in 1762, and Woods characterized him as a "[w]eak, dissolute, violent." This characterization has been reversed by historians since the time of Woods however (cf. e.g. Palmer (2005)), and is reflected in the assessment of our research assistant. Of the remaining 52 cases, in 16 cases Woods assigned a grade between -1,0, or 1. Our research assistant was not given this option and hence there cannot be exact agreement for those. Of the remaining 36 cases, 18 are instances where our research assistant assigned the monarch's ability "1", while Woods assigned "0". These cases include James IV of England and Leopold I of Hapsburg.

The right panel of Figure 3.10 provides a binned scatter plot of our research assistant's assessment of country performance with that of Woods ($\rho = 0.49$ for N=232 reigns). Of the 232 reigns for which our research assistant was confident in making an assessment, in 149 he completely agrees with Woods assessment. In 27 instances, our research assistant reached the opposite assessment than Woods did, in 18 Woods assigned a state performance between the values of 0, 1, and -1. The remaining 38 instances are cases where our research assistant and Woods disagree in their assessment of state performance by 1, but not diametrically so.


Ruler Ability (N=166)

State Performance (N=232)

Figure 3.10: Validation: Binscatters with country FE
Note: The figure shows our validation of the measures of Woods (1913). We code the performance of countries and the ruler ability during each reign possible from online encyclopedias and assess the association of our assessment with that of Woods. The left binned scatter shows residuals of state fixed effects of this association for state performance. The right binned scatter show this association for ruler ability.

In Table 3.8 we show the baseline regression using Woods' assessment and our own. Column 1 repeats the baseline OLS regression. In column 3, we show the baseline using our own assessments of state performance and ruler ability. These were coded – independently of those of Woods – by our RA based on internet encyclopedias, which in turn draw on historical sources. We document a smaller but still sizable and highly significant association. This difference in size is not driven by the different samples for which assessment of Woods' and ourselves are available. Columns 2 and 4 show the baseline associations, respectively, for the sample assessed by both Woods and us.

Table 3.8: Validation: Baseline

Dep. Var.: Performance of State During Reign of Monarch	(1)	(2)	(3)	(4)
Assessment:	Woods		Own	
Ruler Ability	0.620^{***}	0.641^{***}	0.460^{***}	0.464^{***}
	(0.050)	(0.076)	(0.054)	(0.072)
State FE	✓	✓	✓	✓
R^2	0.41	0.48	0.26	0.26
Observations	334	145	260	145

Note: All regressions are run at the reign level. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

3.7.1.4 Territorial Changes: Example

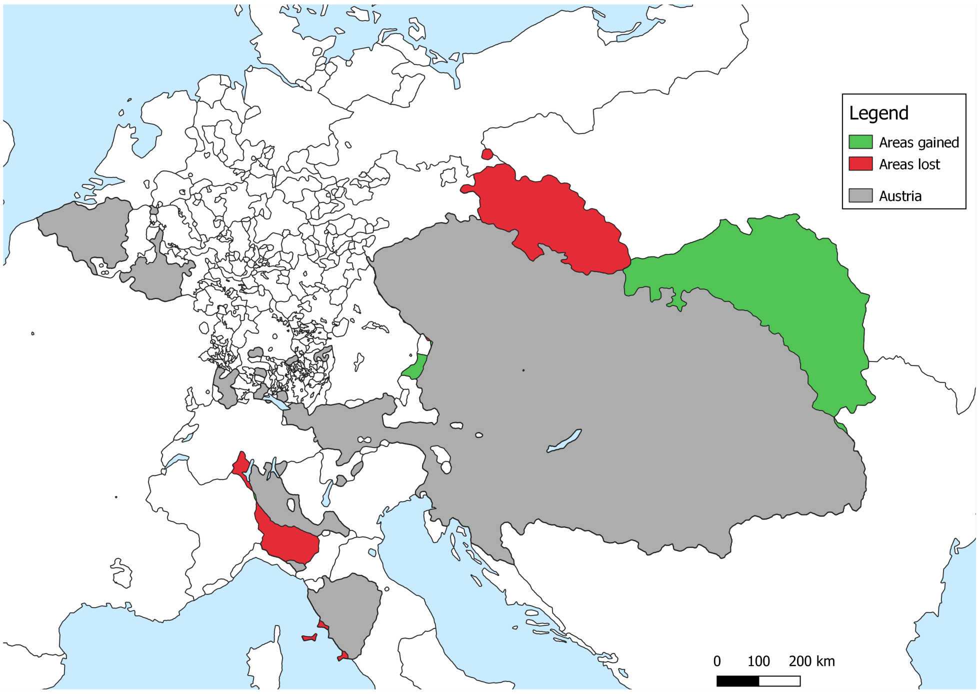


Figure 3.11: Austria's Territorial Changes During Reign of Maria Theresa
Note: The figure shows the change in land area under the control of the Austrian Habsburg from the beginning to the end of Queen Maria Theresa's reign from 1740 to 1780. The data on country borders is from Abramson (2017), and we calculate net gains of 7% during the reign of Maria Theresa.

3.7.2 OLS Robustness

3.7.2.1 Robustness Measurement

In Table 3.9 we return to using Woods assessments only. There, we restrict attention to selected variable values as coded up by Woods. Specifically, in column 1 we exclude any outcome variable that does not indicate a clearly good or bad state performance. Excluding intermediate cases, the point estimate increases considerably. Column 2 focuses only on reigns of clearly capable or incapable rulers, resulting in a point estimate that is very similar to the full sample. Column 3 restricts attention to cases where both ruler ability and state performance are required to be clearly good or clearly bad. In column 4, we exclude any reign where either variable takes the middling values of 0.5 or -0.5, and again find a very similar coefficient. For column 5, we recode all those middling values to work against a positive association between ruler ability and country performance.[56] Still, the coefficient remains sizable and significant.

3.7.2.2 Robustness Specification

Table 3.10 shows robustness to different specifications. Column one uses as outcome variable a dummy indicating that country performance was good, instead of using a continuous variable ranging from bad ("-1") to good ("1") country performance as before. Column 2 retains this dummy outcome variable and furthermore uses dummies for each possible value of the independent variable, ruler ability, instead of a continuous version thereof. The coefficients are as one would expect. Incapable rulers are negatively associated with good state performance, while capable rulers are positively associated with it. The middling values of ruler ability are imprecisely estimated, while the rulers that were not clearly good or bad ("0") are the omitted base level. Column 3 does justice to the categorical nature of both dependent and independent variable by estimating an

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[56] To do so, we reassign all the middling values of 0.5 or -0.5, where Woods was unsure to either of the closest value of 0,1, or -1. For this we consider the other variable and recode the variable to work against a positive association between both. For instance, if the ruler was coded as having low ability (-1), and the performance of the state as middling between 0 and 1, we recode state performance in this case to 1.

Table 3.9: Robustness: Measurement

Dep. Var.: Performance of State During Reign of Monarch

	(1)	(2)	(3)	(4)	(5)
Coding:	Woods	Woods	Woods	Woods	Woods
Note:	"+" or "-"	"+" or "-"	"+" or "-"	"+", "0", or "-"	Recoded
	State	Ruler	Both	Both	Conservatively §
Ruler Ability	0.770***	0.632***	0.770***	0.662***	0.500***
	(0.047)	(0.051)	(0.050)	(0.047)	(0.057)
State FE	✓	✓	✓	✓	✓
R^2	0.52	0.50	0.60	0.48	0.30
Observations	243.00	247.00	202.00	280.00	334.00

Note: This table documents robustness of our baseline regression to the measurement of ruler ability and country performance. Column 1-4 use Woods' coding and exclude all reigns that are not rated as either clearly bad (-1)or clearly good (1). Column 4 excludes all reigns that are not rated as either clearly bad (-1), clearly good (1) or mediocre (0). All regressions are run at the reign level. Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

§ Recode all variables which are not either clearly bad (-1), clearly good (1) or mediocre (0), such that they work against the positive association of country performance and ruler ability. We recode 36 ruler abilities and 24 country performances.

ordered probit regression. As in column two, the individual coefficients are sensible and significant. Table 3.11 includes further fixed effects to our estimation and provides robustness to clustering of standard errors at alternative level . In column 4 we cluster standard errors at the state level as in our baseline. Given the small number of clusters, we also employed bootstrapping, which yields an identical t-statistic. We report the wild bootstrap p-values in brackets below the standard errors. In column 2, we cluster at the country, dynasty, and century level, effectively reducing the sample to monarchs hailing from a dynasty. Again, size and significance of the main coefficient of interest is barely affected. Lastly, in column 3 we include fixed effects at all these levels, which further increases the size of the main coefficient while it remains highly significant.

Table 3.12 shows how the baseline varies by state. We interact state-fixed effects with ruler ability and show these coefficients in Column 1. Column 2 further splits England into two separate entities, one before 1600 and one after 1600, and depicts coefficient for both. Only for England before 1600 do we find a sizable and significant association between ruler ability and state performance. Columns 3 and 4 probe the robustness of these coefficients by including century and dynasty fixed effects. Their

Table 3.10: Robustness: Specification

Dependent Variable as Indicated in Table Header

Dep. Var.:	Dummy Performance †		State Performance
	(1)	(2)	(3)
Estimation:	OLS	OLS	Ordered Probit
Ruler Ability	0.523***		
	(0.068)		
Ruler Quality = -1		-0.141**	-0.925***
		(0.063)	(0.160)
Ruler Quality = -0.5		-0.001	-0.504*
		(0.153)	(0.275)
Ruler Quality = 0.5		0.107	0.033
		(0.151)	(0.289)
Ruler Quality = 1		0.496***	1.153***
		(0.083)	(0.187)
State FE	✓	✓	✓
R^2	0.29	0.34	
Observations	334	334	334

Note: This table documents robustness of our baseline regression to using dummy variables, probit estimations, clustering at different levels and additional fixed effects. All regressions are run at the reign level. Robust standard errors in parentheses in columns 1 to 3, clustered at the country level in column 4, three-way clustered at country, dynasty, and century in columns 5 and 6. * p<0.1, ** p<0.05, *** p<0.01.
†: Dummy if Woods coded performance of state as ”1”

Table 3.11: Robustness: Clustering and FE

Dep. Var.: Performance of State During Reign of Monarch

	(1)	(2)	(3)
Standard errors:	Cluster: State	Cluster: State, Dynasty, Century	Cluster: State, Dynasty, Century
Ruler Ability	0.620***	0.656***	0.687***
	(0.050)	(0.049)	(0.073)
	[0.0000]	[0.0013]	[0.0014]
Additional FE			
State FE	✓	✓	✓
Century FE			✓
Dynasty FE			✓
R^2	0.41	0.46	0.61
Observations	334	288	288

Note: All regressions are run at the reign level. Wild bootstrapped p-value in brackets. * p<0.1, ** p<0.05, *** p<0.01.

inclusion renders the coefficients of Russia and Holland smaller and insignificant, but in general does not affect the size or significance of our coefficients by much.

Table 3.13 shows how the baseline association varies over time. Column 1 shows the coefficients of interactions of ruler ability and a dummy indicating whether the majority of a reign was before 1500 and one indicating that the majority of the reign lay in the years after 1500. Column 2 instead shows coefficients of interactions of ruler ability with a dummy indicating that the reign started in a specific time period. Lastly, Column 3 shows the coefficients of interactions of ruler ability with an indicator for each century. This indicator is one whenever the majority of a reigns year lay in a specific century. Throughout, we document slightly smaller associations between ruler ability and state performance in later years.

Table 3.12: Baseline By Country

Dep. Var.: Performance of State During Reign of Monarch

	(1)	(2)	(3)	(4)
Aragon	0.602***	0.602***	0.455***	0.444***
	(0.000)	(0.000)	(0.047)	(0.055)
Austria	0.590***	0.590***	0.599***	0.625***
	(0.000)	(0.000)	(0.000)	(0.038)
Castile	0.725***	0.725***	0.789***	0.766***
	(0.000)	(0.000)	(0.027)	(0.042)
Denmark	0.374***	0.374***	0.241***	0.252***
	(0.000)	(0.000)	(0.000)	(0.048)
England	0.356***		0.554***	0.555***
	(0.000)		(0.043)	(0.060)
England (before 1600)		0.540***		
		(0.000)		
England (after 1600)		0.024***		
		(0.000)		
France	0.762***	0.762***	0.764***	0.769***
	(0.000)	(0.000)	(0.003)	(0.051)
Netherlands	0.530***	0.530***	0.525***	0.548***
	(0.000)	(0.000)	(0.077)	(0.120)
Portugal	0.762***	0.762***	0.697***	0.705***
	(0.000)	(0.000)	(0.014)	(0.014)
Prussia	0.979***	0.979***	0.979***	0.994***
	(0.000)	(0.000)	(0.000)	(0.027)
Russia	0.457***	0.457***	0.201***	0.194***
	(0.000)	(0.000)	(0.000)	(0.015)
Scotland	0.634***	0.634***	0.637***	0.624***
	(0.000)	(0.000)	(0.032)	(0.054)
Sweden	0.453***	0.453***	0.962***	0.930***
	(0.000)	(0.000)	(0.000)	(0.058)
Turkey	0.823***	0.823***	0.863***	0.914***
	(0.000)	(0.000)	(0.000)	(0.051)
State FE	✓	✓	✓	✓
Century FE				✓
Dynasty FE			✓	✓
R^2	0.44	0.45	0.58	0.59
Observations	334	334	334	334

Note: This tables documents the relationship between ruler ability and state performance by state. In column 1 we interact the baseline regression with a dummy for each state in the sample. Column 2 splits England into two observations, one for all reigns before 1600 and one for all those after 1600. All regressions are run at the reign level. Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.13: Baseline By Time Period

Dep. Var.: Performance of State During Reign of Monarch	(1)	(2)	(3)
pre1500	0.713^{***}		
	(0.061)		
post1500	0.532^{***}		
	(0.062)		
990-1340		0.707^{***}	
		(0.070)	
1340-1500		0.722^{***}	
		(0.094)	
1500-1660		0.596^{***}	
		(0.066)	
1660-1800		0.434^{***}	
		(0.127)	
1000s			0.928^{***}
			(0.129)
1100s			0.636^{***}
			(0.138)
1200s			0.575^{**}
			(0.238)
1300s			0.862^{***}
			(0.075)
1400s			0.647^{***}
			(0.137)
1500s			0.568^{***}
			(0.091)
1600s			0.498^{***}
			(0.146)
1700s			0.518^{***}
			(0.149)
State FE	✓	✓	✓
R^2	0.42	0.42	0.42
Observations	334	334	334

Note: This tables documents the relationship between ruler ability and state performance by broad time period. In column 1 we interact the baseline regression with a dummies indicating whether the reign began before or ater 1500. Column 2 shows coefficients of interactions with broader time periods, and column three shows the coefficient by century. All regressions are run at the reign level. Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

3.7.3 Extended Sample

In this Appendix, we extend Woods's original sample until WWI and to cover Poland and Hungary. To do so, similar as in our validation, we asked a research assistant to assess the capability of rulers from all of the states covered by Woods reigning after Napoleon until World War I, and for Poland and Hungary. From Woods' original sources and modern encyclopedias, the research assistant compiled a list of monarchs and assessed their capability of rulers and the performance of their countries on the three-point scale of Woods (1913).

Table 3.14 presents results. Columns 1 to 3 show OLS estimates, columns 4 to 6 first stage estimates, and columns 7-0 second stage estimates. We first repeat the baseline analysis in columns 1, 4, and 7, respectively. Columns 2, 5, and 8 use the sample of all states of Woods, extended by our research assistant until the last monarch available or ruling until the start of World War I in 1914. For instance, the list of monarchs of France ends with Napoleon III, who ruled from 1852 to 1870. [57] The baseline association between ruler ability and state performance is somewhat smaller in that extended sample, as is evident from column 2. In light of our results in section 3.5, this decrease in coefficient size might reflect the increase of executive constraints during this time period. The first stage is marginally weaker, while the second stage is marginally stronger. A similar picture emerges from the inclusion of 63 Polish(-Lithuanian) and Hungarian monarchs in columns 3, 6, and 9, respectively.

3.7.4 Ruler-pair Regressions

In our baseline regressions throughout the paper we compare rulers from the same country across time. Including various time period fixed effects in our baseline regression does not alter the main coefficient of interest. However, such time periods are always necessarily arbitrary and require us to allocate rulers on the margin into one time period over the other. Yet, rulers might be affected by continent-wide shocks, such as

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[57] We can assess ruler ability and state performance for 38 additional reigns, for 31 of which we are able to recover coefficients of inbreeding from `roglo.com`

Table 3.14: Extended Sample

	Dep. Var.: Performance of State During Reign of Monarch								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		OLS			First Stage			IV	
Sample:	Woods	+ until WWI	+ PL, HU	Woods	+ until WWI	+ PL, HU	Woods	+ until WWI	+ PL, HU
Ruler Ability	0.620^{***} (0.050)	0.534^{***} (0.045)	0.556^{***} (0.042)				0.794^{***} (0.147)	0.900^{***} (0.097)	0.929^{***} (0.095)
Coefficient of Inbreeding				-0.076^{***} (0.011)	-0.056^{***} (0.010)	-0.059^{***} (0.010)			
R^2	0.41	0.35	0.35	0.15	0.11	0.12	0.39	0.23	0.23
Observations	334	372	435	239	270	316	234	263	303

Note: This table shows OLS, first stage and second stage results for the baseline sample of Woods (columns 1, 4, and 7), and when extending it until World War I (columns 2, 5, and 8), as well to further include Poland and Hungary (columns 4, 6, and 9). All regressions are run at the reign level. Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

the Black Death or conflicts. If these or related factors make it easier or harder for rulers to distinguish themselves *and* affect their countries performance, this could still confound or baseline analysis.

In this appendix, we provide a more flexible approach by comparing rulers - additionally to other rulers in their country - with other rulers in different countries ruling contemporaneously. For instance, while Carlos III of Spain (assessed as a capable ruler by Woods (1913)) oversaw the "continued improvement" of many aspects of the performance of Spain from 1759 to 1788, Louis XV ruled over France from 1731 to 1774. Described by Woods (1913) as "weak, indolent" and of "inferior capacity", he oversaw the (for France) "disastrous Seven Years War", and domestically a 'decline in commerce. (...) Under excessive taxes, the peasantry were reduced to extreme misery".

To this end, we identify – for each ruler i – all those rulers j who overlap in their reign in different countries for at least one year. Then we calculate pairwise differences in their ability, the performance of their countries, and their coefficients of relationship. With this, we estimate regressions at the pair-level:

$$\Delta_{ij} \text{ Country Performance} = \beta \Delta_{ij} \text{ Ruler Ability} + \mu_{c(i)} + \mu_{c(j)} + \gamma X + \varepsilon_{ij}$$

where Δ_{ij} indicates the difference of a variable between ruler i and j. To this end, we first recode the categorical assessments of Woods (1913) to the range between zero and one, such that “0” for instance indicates bad and “1” indicates good country performance. When for instance comparing Louis XV to Carlos III, this difference is exactly one from the perspective of Carlos, as is the difference in ruler ability. For ease of interpretation of coefficients we again standardize the differences in the assessments of state performance and of ruler ability so that both variables have mean zero and standard deviation one. We further estimate IV regressions in this setting using the difference in the coefficient of inbreeding. For the above example of Carlos and Luis, this difference is negative (-5.65) from the perspective of Carlos, as Carlos has a lower coefficient of inbreeding (3.9) compared to Louis (9.55). In all regressions we further include country fixed effects of both rulers $(\mu_{c(i)}, \mu_{c(j)})$, and introduce the following additional fixed effects successively: country-pair fixed effects, ruler fixed effects (of ruler i), and country-pair times century fixed effects. Throughout for the ruler-pair regressions, standard errors are clustered at the country-pair level.

In total there are 5,490 pairs of overlapping rulers in our sample for which we have assessments of ability and state performance for both rulers,and for 4,476 of these we have coefficient of inbreeding for both rulers. In order to focus on the most relevant comparisons, we also restrict the sample, for each ruler, to the one ruler from each other country whom she or he shares the largest temporal overlap with.

Table 3.15 shows OLS results. Rulers with higher ability compared to another, contemporaneously ruling in a different country, are associated with relatively better performance in their country (column 1). This association also holds when comparing ruler-pairs only from the same country pairs over time (column 2), and even within the same century (column 4). Furthermore, it also holds when comparing rulers only with each other country’s ruler they share the largest overlap in reign with (column 5).

Comparing ruler with their pairs, comparatively more inbred rulers are assessed worse by Woods (1913). Table 3.16 shows that the first stage is strong in the ruler-pair setting, following the structure of the earlier table.

Relying on this first stage, in table 3.17 we show IV results at the ruler-pair level. Again following the structure of earlier table, we document sizable effects of pair-wise differences in ruler ability on differences in performance of the countries' ruler.

Table 3.15: Ruler-pair Regressions: OLS

Dep. Var.: Δ_{ij} Country Performance

	(1)	(2)	(3)	(4)	(5)
Note:					One-ruler match
Δ_{ij} Ruler Ability	0.637^{***}	0.640^{***}	0.619^{***}	0.602^{***}	0.581^{***}
	(0.012)	(0.013)	(0.015)	(0.017)	(0.026)
State FEs	✓	✓	✓	✓	✓
State-pair FE		✓	✓	✓	✓
Ruler FE			✓	✓	✓
Country-pair × Century FE				✓	✓
Observations	5,490	5,490	5,490	5,467	2,538

Note: This table shows that pair-wise differences in the ability of rulers predicts differences in their countries' performance. Columns 1 - 3 include, for each ruler, all rulers of other states that overlapped for at least a year in their reign. Column 4 keeps for each ruler only the one ruler from each other country that he or she shared the largest temporal overlap in their reigns with. All regressions are run at the reign-pair level. Standard errors, clustered at the country-pair level, in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.16: Ruler-pair Regressions: First Stage

Dep. Var.: Δ_{ij} Ruler Ability

	(1)	(2)	(3)	(4)	(5)
Note:					One-ruler match
Δ_{ij} Coefficient of Inbreeding	-0.034***	-0.034***	-0.037***	-0.031***	-0.023***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)
State FEs	✓	✓	✓	✓	✓
State-pair FE		✓	✓	✓	✓
Ruler FE			✓	✓	✓
Country-pair × Century FE				✓	✓
R^2	0.15	0.17	0.62	0.69	0.77
Observations	4,476	4,476	4,475	4,455	2,538

Note: This table shows that pair-wise differences in the coefficient of inbreeding of rulers predicts differences in their ability as monarchs. Columns 1 - 3 include, for each ruler, all rulers of other states that overlapped for at least a year in their reign. Column 4 keeps for each ruler only the one ruler from each other country that he or she shared the largest temporal overlap in their reigns with. All regressions are run at the reign-pair level. Standard errors, clustered at the country-pair level, in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.17: Ruler-pair Regressions: IV

Dep. Var.: Δ_{ij} Country Performance

	(1)	(2)	(3)	(4)	(5)
Note:					One-ruler match
Δ_{ij} Ruler Ability	0.904***	0.872***	0.873***	0.791***	0.954***
	(0.086)	(0.089)	(0.075)	(0.081)	(0.131)
State FEs	✓	✓	✓	✓	✓
State-pair FE		✓	✓	✓	✓
Ruler FE			✓	✓	✓
Country-pair × Century FE				✓	✓
First Stage F-statistic	90.52	82.13	130.44	75.22	65.28
Observations	4,476	4,476	4,475	4,455	2,538

Note: This table shows that pair-wise differences in the ability of rulers predicts differences in their countries' performance. We instrument for differences in their ability by differences in the coefficient of inbreeding. Columns 1 - 3 include, for each ruler, all rulers of other states that overlapped for at least a year in their reign. Column 4 keeps for each ruler only the one ruler from each other country that he or she shared the largest temporal overlap in their reigns with. All regressions are run at the reign-pair level. Standard errors, clustered at the country-pair level, in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

3.7.5 Background on Coefficient of Inbreeding

The coefficient of inbreeding measures of the degree of similarity in the genes of offspring due to common ancestors was developed by Wright (1921). It is the probability that both gene copies at any locus in an individual are identical by descent, i.e. from a common ancestor (Rédei, 2008), and is defined as follows:

$$F = \sum_{paths} (0.5)^n (1 + FA)$$

where F is the coefficient of inbreeding, *paths* is each path through which an individual can derive identical alleles from a common ancestors of both parents, n is the number of individuals in the paths (excluding the individual itself), and $1 + FA$ is a correction factor for the inbreeding coefficient of the common ancestor in the path. The 0.5 component comes from the fact that each individual has 0.5 chance to pass a particular allele to a particular offspring.

Consider the following illustrative example of the calculation of the inbreeding coefficient of an offspring of parent-child mating. A is the offspring of B and another individual. Let us assume that the parents of individual A are unrelated, so that we do not have to apply a correction factor for the common ancestor A. Lines signify blood relationship. If A were to mate with B, the offspring I would be inbred. To calculate the coefficient of inbreeding, we first note that only one common ancestor exists, B, and only one path. In this path, there are two individuals which are not I, A and B. Hence $F(I) = 0.5^2 = 0.25$. Were B inbred as well, we would have to adjust for that degree of inbreeding.

Figure 3.12: Example Calculation of Inbreeding Coefficient

Figures show the calculation of the coefficient of inbreeding for an parent-child offspring. Exactly one path through a common ancestor (B) of length $n = 2$ exists.

3.7.6 IV Robustness: Strategic Marriage

In this appendix, we discuss several concerns with our identification strategy related to the role of strategic marriages. We show evidence that neither strategic kin marriage under threat (section 3.7.6.1), nor the strategic marriage of unrelated individuals, and the spoils in state performance from doing so (3.7.6.2) can explain our results or invalidate our identification strategy. In section 3.7.6.3, we only employ the unanticipated component of inbreeding in our IV analysis. Thereby, we show that the possibility of parents of monarchs anticipating but willingly incurring incapable future monarchs is not driving our results either.

3.7.6.1 Strategic Kin Marriage Under Threat

It would constitute a threat to our exclusion restriction if royals marry kin when state performance is low, leading to a higher coefficient of inbreeding in the following generation, *and* if past bad state performance lowers performance during the reign of their offspring.

As we document in table 3.18, past state performance does not predict current state performance in our reduced form regression. Even more, we can account for dynamics of state performance in our analysis leaving our main results unaffected. Column one repeats our baseline reduced form.[58] Lags of state performance do not affect current state performance, as is evident from column 2.[59] Column 3 shows that the inclusion of such lags does not affect the coefficient on the current ruler's coefficient of inbreeding. Columns 4 includes the coefficients of inbreeding of earlier rulers, and column 5 further includes lags of state performance. From this, it appears that past state performance does not predict current state performance beyond the effect of the coefficient of inbreeding.

Table 3.19, organized in a comparable manner, documents that past state performance further does not predict ruler ability in our first stage. Past bad state performance

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[58] Each regression uses the largest available sample for estimation.

[59] Note that "time periods" in this setting refer to reigns, which naturally vary in length.

does not lead to significantly worse rulers. Hence, neither of the conditions required for strategic kin marriage to affect our exclusion restriction appear to be fulfilled. Therefore, including lags of state performance and lags of the coefficient of inbreeding, does also not affect our IV estimates, evident from table 3.20

Table 3.18: Past State Performance as Confounder: Reduced Form

Dep. Var.: Performance of Country During Reign of Monarch

	(1)	(2)	(3)	(4)	(5)
Coefficient of Inbreeding	-0.061***		-0.062***	-0.071***	-0.067**
	(0.011)		(0.013)	(0.020)	(0.027)
L.Coefficient of Inbreeding				0.020	0.016
				(0.038)	(0.041)
L2.Coefficient of Inbreeding				-0.010	-0.012
				(0.015)	(0.016)
L.State Performance		0.045	-0.021		0.002
		(0.067)	(0.071)		(0.094)
L2.State Performance		-0.017	-0.016		-0.040
		(0.059)	(0.074)		(0.089)
State FE	✓	✓	✓	✓	✓
R^2	0.11	0.05	0.11	0.14	0.14
Observations	234	284	200	148	136

Note: All regressions are run at the reign level. Lag varies in length depending on ruler lifetime. Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.19: Past State Performance as Confounder: First Stage

Dep. Var.: Ruler Ability

	(1)	(2)	(3)	(4)	(5)
Coefficient of Inbreeding	-0.077***		-0.071***	-0.088***	-0.088***
	(0.012)		(0.013)	(0.020)	(0.022)
L.Coefficient of Inbreeding				0.024	0.020
				(0.035)	(0.039)
L2.Coefficient of Inbreeding				-0.025	-0.028
				(0.017)	(0.018)
L.State Performance		-0.089	-0.072		-0.067
		(0.078)	(0.092)		(0.081)
L2.State Performance		0.065	0.052		-0.054
		(0.064)	(0.058)		(0.074)
State FE	✓	✓	✓	✓	✓
R^2	0.15	0.07	0.15	0.21	0.22
Observations	234	284	200	148	136

Note: All regressions are run at the reign level. Lag varies in length depending on ruler lifetime. Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.20: Past State Performance as Confounder: IV

Dep. Var.: Performance of Country During Reign of Monarch

	(1)	(2)	(3)	(4)	(5)
Ruler Ability	0.794***	0.868***	0.868***	0.812***	0.760***
	(0.099)	(0.157)	(0.157)	(0.154)	(0.220)
L.Coefficient of Inbreeding				0.000	0.001
				(0.020)	(0.023)
L2.Coefficient of Inbreeding				0.010	0.010
				(0.017)	(0.016)
L.State Performance		0.042	0.042		0.053
		(0.078)	(0.078)		(0.091)
L2.State Performance		-0.061	-0.061		0.001
		(0.077)	(0.077)		(0.083)
State FE	✓	✓	✓	✓	✓
First Stage F-statistic	42.15	31.64	31.64	20.05	15.44
Observations	234	200	200	148	136

Note: All regressions are run at the reign level. Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

3.7.6.2 Strategic Marriage Outside Of Kin Network

Alternatively, rulers might strategically marry outside of their dynasty network when they anticipate future expansion. Marrying outside a dynasty network also potentially mechanically increases state performance in the following period by enlarging territory due to the strategic marriage. This implicates the possibility of a direct effect of the degree of inbreeding on country performance, as a marriage between completely unrelated individuals would give a coefficient of inbreeding of $F = 0$ in the next generation. Note that we actually exclude monarchs with (likely) completely unrelated parents from our baseline IV analysis. For rulers without (known) family relations, our source *roglo.com* does not provide F. Yet, this does not imply that those are necessarily zero. In Column 2 of Table 3.21 below, we include the 43 rulers whose parents (likely) had no relationship: our results are stronger compared to the baseline results excluding these 43 rulers, presented in Column 1, but not solely driven by these.

Table 3.21: Strategic marriage outside of kin network: IV results

Dep. Var.: Performance of Country During Reign of Monarch

	(1)	(2)
Sample	Baseline	Include $F = 0$
Ruler Ability	0.794***	0.814***
	(0.099)	(0.098)
State FE	✓	✓
First Stage F-statistic	42.15	38.42
Observations	234	277

Note: All regressions are run at the reign level. Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

3.7.6.3 Hidden component of inbreeding IV

In our main analysis, the instrument is the coefficient of inbreeding, F. As is evident from the discussion in 3.7.5 and the pedigree of Carlos II, high values of F need not necessarily imply closely related parents. Instead, relationship links in temporal distance from the parents of an individual can build up, and account for a sizable share of the observed coefficient of inbreeding. Consider Carlos II again. With $F = 25.36$, he is the monarch with the highest coefficient of inbreeding in our data set. Yet, his parents were 'merely' uncle and niece, with most of the similarity in genes actually coming from a multitude of pathways through many distant common ancestor. The 'naive' coefficient of inbreeding of Carlos, based on his parents being uncle-niece, would be $F = 12.5$, implying that three quarters of the observed F of Carlos would require knowledge of relationship links beyond that of his grandparents. We calculate a hidden component of the coefficient of inbreeding by subtracting the coefficient of inbreeding implied by the closest relationship link between a rulers' parents indicated on *roglo.com*:

$$F(hidden) = F - F(naive)$$

where $F(naive)$ is 12.5 for monarchs whose parents were uncle and nieces (4 monarchs in total), and 6.25 for the (19) monarchs whose parents were (first) cousins. In the remainder we only use the hidden component as instrument for ruler ability. For Carlos, this would amount to $F(hidden) = 12.86$. Thereby, we isolate the component of the inbreeding coefficient that could be anticipated even without the advanced knowledge of calculating inbreeding coefficients and the intricate details of pedigrees. Table 3.22 shows that the IV results are if anything even larger, and retain high statistical significance. Table 3.23 documents the strong first stages.

Table 3.22: "Hidden" Components of Inbreeding: Second Stage Results

Dep. Var.: Performance of Country During Reign of Monarch

	(1)	(2)	(3)	(4)
Instrument:	F(full)	F(hidden)	F(hidden)	F(hidden)
Sample:	Full		Restricted†	Documented PG‡
Ruler Ability	0.794***	0.827***	0.778***	1.260***
	(0.099)	(0.136)	(0.164)	(0.246)
State FE	✓	✓	✓	✓
First Stage F-statistic	42.15	16.53	18.69	7.28
Observations	234	234	190	136

Note: All IV regressions are run at the reign-pair level. Standard errors, clustered at the state-pair level, in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

† Subsample excludes regencies, instances of foreign ruler and rulers having more than one reign ‡ Subsample includes only documented cases when rulers ascended to power due to primogeniture

Table 3.23: "Hidden" Components of Inbreeding: First Stage Results

Dep. Var.: Ruler Ability

	(1)	(2)	(3)	(4)
Instrument:	F(full)	F(hidden)	F(hidden)	F(hidden)
Sample:	Full		Restricted†	Documented PG‡
Coefficient of Inbreeding	-0.077***			
	(0.011)			
Coefficient of Inbreeding (hidden)		-0.118***	-0.114***	-0.083***
		(0.027)	(0.024)	(0.028)
State FE	✓	✓	✓	✓
R^2	0.15	0.16	0.14	0.15
Observations	234	234	190	136

Note: All IV regressions are run at the reign-pair level. Standard errors, clustered at the state-pair level, in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

† Subsample excludes regencies, instances of foreign ruler and rulers having more than one reign ‡ Subsample includes only documented cases when rulers ascended to power due to primogeniture

3.7.7 IV Robustness: Other Concerns

3.7.7.1 Founder and Descendant Effects

George and Ponattu (2018) show that dynastic politics generates a “reversal of fortune” development pattern, where places develop faster in the short run (due to “founder effects” where bequest motives are increase the relevant time horizon), but are poorer in the long run, because of descendant effects (i.e., intergenerationally transmitted political capital renders descendants less politically accountable) outweigh founder effect. One could argue that incest was worst at the end of dynasties, and that this also when this “reversal” effect would be strongest. Indeed, over time the coefficient of inbreeding increases while ruler ability decreases, as is evident from Figure 3.13.[60]

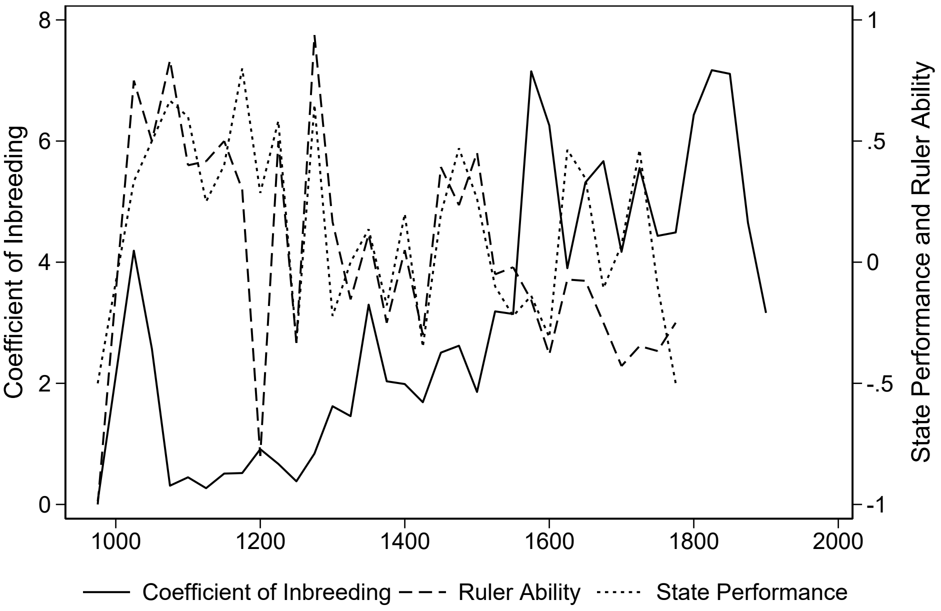

Figure 3.13: Trends in ruler ability, state performance and inbreeding

To address this concern, we code a categorical variable for the order of rulers within dynasties. For example, Carlos III is the 3rd of the Spanish Bourbons. Yet, he also hails from the Bourbon dynasty ruling France. He is the 8th of all Bourbons, ordered by the

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[60] The figure depicts means of the (non-standardized)variable at 25 year intervals, where each reign is linked to the 25 year interval in which it began.

year in which his reign began. We account flexibly for the potential importance of dynasty and founder effects by including fixed effects for the order of monarchs within their dynasties. Column 1 of Table 3.24 repeats our baseline IV result. Column 2 restricts attention to rulers part of any identifiable dynasty. Column 3 includes fixed effects for all rulers of the same order within their dynasty, treating rulers hailing from the same dynasty across countries as part of different dynasties. Column 4 instead includes fixed effects which treat such rulers as hailing from the same international dynasty. In both cases, our estimates are sizable and significant. While "reversal of fortunes" development patterns resulting from founder and descendant effects are potentially capturing some the effect of ruler ability on country performance running through inbreeding, the latter is operating distinctively from these.

Figure 3.14, depicting the association of monarchs' coefficient of inbreeding and their order within their (international) dynasty, provides clues as to why the development dynamics found in George and Ponattu (2018) cannot account for our main result. Why indeed monarchs of higher order within their dynasties tend to have higher coefficients of inbreeding, there are plenty of monarchs with comparably high coefficients of inbreeding among those of lower orders.

Table 3.24: IV Regressions Accounting for Monarch's Order in Dynasty

	Dep. Var.: Performance of Country During Reign of Monarch			
	(1)	(2)	(3)	(4)
Sample	Baseline	Dynastic Rulers		
Ruler Ability	0.794***	0.845***	0.893***	0.615**
	(0.099)	(0.115)	(0.121)	(0.257)
State FE	✓	✓	✓	✓
Order in Dynasty FE			✓	
Order in International Dynasty FE				✓
Country FE	✓	✓	✓	✓
First stage F-statistic	42.15	40.62	49.02	8.79
Observations	234	230	230	230

Note: All regressions are run at the reign level. "Order in Dynasty" is the order of a monarch in their dynasty in the same state, and "Order in International Dynasty" is the order of a monarch in their dynasty, considering that certain dynasties ruled in more than one state. For example, Carlos III is the 3rd of the Spanish Bourbons. Yet, he also hails from the Bourbon dynasty ruling France. He is the 8th of all Bourbons, ordered by the year in which his reign began.Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

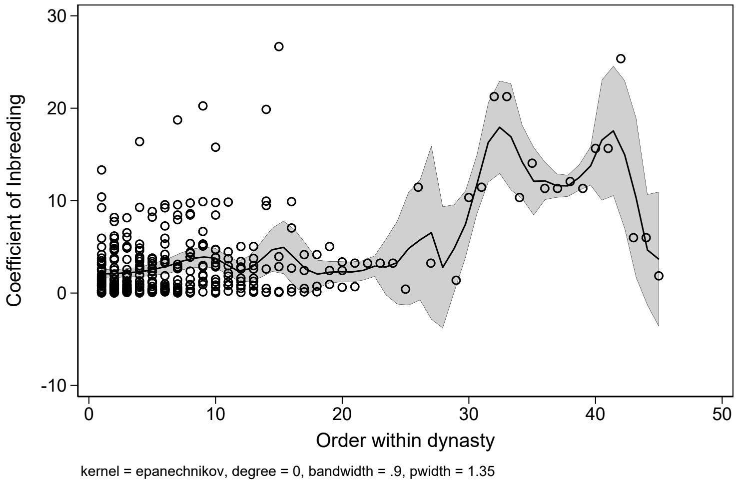

Figure 3.14: Association of Monarchs' Inbreeding Coefficient and Order in (International) Dynasty

3.7.7.2 Potential Confounding Effects of Wars Among Dynasties

Any causal chain from a ruler's inbreeding coefficient to country performance not operating through the ruler's ability would break our exclusion restriction. Benzell and Cooke (2018) find that more "blood links" between two rulers increases the probability of conflict. Thus, conflict could pose a threat to our exclusion restriction. However, conflict by itself would only affect our main outcome (country performance) if wars systematically resulted in either territorial gains or losses. On average, of course, these should cancel out to a zero mean. That being said, success at wars arguably depends on ruler ability – which in turn leads straight back to our argument. In fact, the most likely implication of the Benzell and Cooke finding is that inbreeding adds more variability in country performance due to more frequent wars. To be concrete, suppose that more related rulers fight more often (the OLS finding in Benzell and Cooke). Also, suppose that our inbred rulers have on average more "blood links", so they fight more often.[61] Then we would get that incapable (inbred) rulers have to fight more often – and our results strongly suggest that they would lose more often. Thus, we would have more identifying variation in our data, but this would be ultimately driven by our mechanism of ruler ability. We can directly address this concern in estimation and measurement. To do so, we simply code a dummy for whether a ruler was at war during his or her tenure, and include this in both stages of our IV regressions.[62] We perform this analysis in column 2 of table 3.25 the IV coefficient is barely affected. This is also the case when we control for the share of years during each monarchs reign under war (in column 3).

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[61] This is the more likely mapping from Benzell and Cooke's ruler-pair setting to our individual-ruler setting.

[62] The data comes from David Brecke's Conflict Catalogue (available from `https://brecke.inta.gatech.edu/research/conflict/`) and starts in 900 AD. We identify whether a state participated in any conflict (in Europe) within a given year,and then from this calculate the share of years of each reign in which a state participated in a conflict.

Table 3.25: IV Results Controlling for Conflict

Dependent Variable: Performance of Country During Reign of Monarch

	(1)	(2)	(3)	(4)
Ruler Ability	0.794***	0.794***	0.775***	0.743***
	(0.099)	(0.099)	(0.090)	(0.111)
Conflict: Dummy			-0.120	
			(0.184)	
Conflict: Share Years at War				-0.149
				(0.163)
State FE	✓	✓	✓	✓
First Stage F-stat	42.15	42.15	37.12	21.14
Observations	234	234	234	234

Note: All regressions are run at the reign level. Table shows results from instrumental variable regressions, in which ruler ability is instrumented with the coefficient of inbreeding. Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Second, we can address this concern directly in our measurement of state performance. Our main measure of state performance is a composite measure, including territorial changes as one of many assessed features (others being administrative reform, economic performance, etc). This directly sidesteps the potential confounding effects of warfare. In column 1 of Table 3.26 we show our baseline second stage results. In column 2 we use as outcome the residuals of a regression of the percentage change in territory under the control of a monarch during their reign from (Abramson, 2017) on our composite measure of state performance. Column 3 instead uses our measure of state performance residualized with a categorical variable of territorial expansion ("1") of decline ("-1") assessed by our research assistant.[63] In both column 2 and 3 the coefficient size is reduced, speaking to the importance of territorial changes as a measure of state performance. Yet, the fact that the coefficient retains significance and remains sizable across the different columns of Table 3.26 documents the importance of aspects of state

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[63] See section 3.7.8.2 for details.

performance unrelated to the narrow aspects captured solely by territorial changes.

Table 3.26: IV Results using state performance excluding territorial gains

Dep. Var.: Performance of State During Reign of Monarch

	(1)	(2)	(3)
Outcome:	Baseline	(1) resid. w/ % territorial changes	(1) res. w/ territorial changes
Ruler Ability	0.794***	0.731***	0.559***
	(0.099)	(0.109)	(0.173)
State FE	✓	✓	✓
R^2	0.39	0.34	0.30
Observations	234	200	234

Note: All regressions are run at the reign level. Table shows results from instrumental variable regressions, in which ruler ability is instrumented with the coefficient of inbreeding. Data on the percentage change in area during a monarch's reign comes from Abramson (2017). We calculate state performance residualized by this variable and use it in column 2. Column 3 instead residualizes the dependent variable by our own indicator of territorial change during each reign, where 1 (0,-1) indicate territorial growth (stagnation, decline). Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

3.7.8 Mechanism

In this Appendix we provide additional analysis on the mechanism underlying our main finding: Ruler ability causally affects the performance of the states they govern. We first show that this effect likely stems from the intellectual abilities of monarchs, rather then their physical abilities, by controlling for longevity and reproductive success in our estimation (section 3.7.8.1). Then we show that ruler ability affects both political and economic components of state performance (section 3.7.8.2).

3.7.8.1 Physical or Intellectual Ability?

Inbreeding has negative consequence for both intellectual abilities and physical abilities. The (potentially anticipated) early deaths of monarchs and their lack of reproductive success due to inbreeding – rather than their lack of intellectual capabilities that rendered them ineffective leaders – could also underlie the negative effects of inbred monarchs on state performance that we identify in our analysis (Alvarez et al., 2009). To account for this possibility, we re-estimate our second stage controlling for the longevity of monarchs an their number of (legitimate) children.[64] Column 1 of Table 3.27 shows our baseline second stage for comparison. In columns 2 and 3, respectively, we control for age at death of the monarchs and the number of (legitimate) offspring. Column 4 controls for both simultaneously. Our main coefficient of interest, that of assessed ruler ability is unaffected by the inclusion of this controls. This strengthens the interpretation advanced in the main body of the text: Inbred monarchs were incapable leaders because of the consequences of inbreeding for their intellectual abilities to effectively reign their states, and not because of inbreeding's consequences for the physical abilities to achieve longevity and produce heirs.

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[64] We recovered this information from online encyclopedias.

Table 3.27: IV Results controlling for Longevity and Number of Offspring

Dep. Var.: Performance of State During Reign of Monarch

	(1)	(2)	(3)	(4)
Ruler Ability	0.794***	0.805***	0.780***	0.799***
	(0.099)	(0.089)	(0.110)	(0.101)
Age at death		0.006		0.005
		(0.006)		(0.006)
Number of children			0.024	0.017
			(0.019)	(0.017)
State FE	✓	✓	✓	✓
First Stage F-statistics	42.15	43.51	39.31	40.40
Observations	234	232	233	231

Note: All regressions are run at the reign level. Table shows results from instrumental variable regressions, in which ruler ability is instrumented with the coefficient of inbreeding. Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

3.7.8.2 Which Aspects of State Performance Matter?

Our main dependent variable, state performance as assessed by Woods, is a composite measure. In particular, Woods covered economic and political aspects of reigns: “finances, army, navy, commerce, agriculture, manufacture, public building, territorial changes, condition of law and order, general condition of the people as a whole, growth and decline of political liberty, and the diplomatic position of the nation, or its prestige when viewed internationally,” (Woods, 1913, p. 10). While the main interest of this paper is the composite assessment of state performance, we further assess the various components state performance to understand which aspects of it are driving our result. We asked our research assistant to read through the full text of Woods (1913) again, and note positive or negative changes in each of the components, and assess what Woods himself understood of this components. Then, we validated and extended this using information available in online encyclopedias. In total, we assess 14 components, which we roughly group into political aspects and economic aspects of reigns. Here we provide a brief list of each of these, and some questions that display what aspects are covered by these measures.

- Political aspects of state performance
    - Territorial changes: Did the state's territory expand or shrink?
    - Law and order: Did the executive hold the monopoly on power/force? Have there been insurrections/ revolts? Did the executive counter them effectively?
    - Public liberty: Was there persecution of minorities? Was there serfdom?
    - Finances: What was the state of treasury, royal finances, and public debt?
    - Army: How well-equipped, large, and successful was the army?
    - Navy: Did a navy exist? How was the naval force equipped?
    - Administration: Was the public administration effective, was it corrupt?
    - Diplomacy and prestige: Was the state's diplomacy effectively implemented, was its diplomatic strategy successful? How was the state rated among other powers in Europe?
- Economic aspects of state performance
    - General conditions of inhabitants: Did the welfare of the general populace change during a reign?
    - Infrastructure: Were roads, bridges, ports built/destroyed, or did they decay?
    - Commerce: Was there more commercial activity, trade, and growing prosperity – or were restrictions on commerce and trade implemented?
    - Agriculture: Were there droughts, loss of farm land, or emigration of farmers?
    - Manufacture: Did the state produce and export more or less manufactures during a reign?

- Urban Growth: Did cities grow or decline during a reign?

For all these aspects, we code negative developments as "-1" and positive ones as "1". Where we have neither information on positive or negative developments, we presume no change and code zeros.

We discuss results for political and economic aspects separately. Table 3.28 shows results of our baseline second stage regressions, where the dependent variable –instead of our composite measure state performance – are our assessments of political aspects during each reign. Both dependent and independent variable are standardized to mean zero and stand deviation one. In column 1, we again document a sizable effect of ability on territorial change. Note however that this is a different measure than the the one used in the main body of the paper. This measure is a categorically assessed variable, while the earlier one employed actual data on polity borders from Abramson (2017). We also document sizable causal effects of ruler ability with a monopoly of violence in their states, public liberties, the strength and successes of the army, and the diplomatic approach and position of a state.

Next we consider economic aspects of state performance and the causal effect of ruler ability on each of these. Table 3.29 documents strong effects of ruler ability on the general welfare of a state's populace, the state and development of it infrastructure, and its commerce.

Table 3.28: IV Results: Assessments of Political Components of State Performance

Dependent Variable: ... During Reign of Monarch

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
...	Territorial Change	Law and Order	Public Liberty	Finances	Army	Navy	Administration	Diplomatic Prestige
Ruler Ability	0.703^{***}	0.435^{**}	0.546^{***}	0.325	0.616^{***}	0.346	0.239	0.500^{**}
	(0.224)	(0.189)	(0.179)	(0.203)	(0.236)	(0.294)	(0.188)	(0.250)
State FE	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.16	0.19	0.28	0.01	0.05	0.10	0.06	0.17
Observations	239	239	239	239	239	239	239	239

Note: All regressions are run at the reign level. Table shows results from instrumental variable regressions, in which ruler ability is instrumented with the coefficient of inbreeding. Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Table 3.29: IV Results: Assessments of Economic Components of State Performance

	Dependent Variable: ... During Reign of Monarch					
	(1)	(2)	(3)	(4)	(5)	(6)
...	General Condition	Infrastructure	Commerce	Agriculture	Manufactures	Urban Growth
Ruler Ability	0.442**	0.987***	0.654**	0.106	0.066	0.050
	(0.207)	(0.288)	(0.298)	(0.267)	(0.192)	(0.189)
State FE	✓	✓	✓	✓	✓	✓
R^2	0.17	-0.04	-0.08	0.07	0.06	0.09
Observations	239	239	239	239	239	239

Note: All regressions are run at the reign level. Table shows results from instrumental variable regressions, in which ruler ability is instrumented with the coefficient of inbreeding. Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

3.7.9 Urban Population as Outcome

In this appendix, we estimate the effect of monarchs ability on the change in urban population during their reign. We decompose this into changes stemming from (i) the growth of cities always under control of the monarch during the entire reign, (ii) the acquisition of territory containing cities, and (iii) the loss of cities during the reign.

We start by imputing the yearly population for each of the cities in Bairoch (assuming a linear growth rate), and identify which polities these cities lay in at each 5-year intervals using the borders provided by Abramson (2017). For each reign, we then calculate the total urban population between the beginning and the end of each reign (we use urban population at the 5-year intervals at which the territory data is available. Note that such changes can result from either changes in the population of the cities that remained in the polity throughout the reign ("intensive"), or from changes in the urban population located in areas lost or gained during a reign ("extensive"). We identify the cities and their population that have always remainder under control, and those that were gained, or lost, during the reign of each monarch.

We decompose changes in total urban population into these separate components. Note that the urban population in the area controlled by a monarch at the beginning of his or her reign consists of (i) urban population in areas that will remain under the control of that monarch until the end of the reign, and (ii) the initial urban population in areas are lost during the reign:

$$Pop_t^{Urb} = Pop_t^{Urb,remain} + Pop_t^{Urb,lost}$$

where t indicates the beginning of a reign, and Pop^{Urb} stands for urban population. Similarly, urban population at the end of a reign can be decomposed into a first component which remained under control by the monarch, and a second component, comprising the urban population at the end of a reign in areas that were gained due to territorial expansion during the reign:

$$Pop_T^{Urb} = Pop_T^{Urb,remain} + Pop_T^{Urb,gained}$$

Therefore:

$$\frac{Pop_T^{Urb}}{Pop_t^{Urb}} = \frac{Pop_T^{Urb,remain} + Pop_T^{Urb,gained}}{Pop_t^{Urb,remain} + Pop_t^{Urb,lost}}$$

Let $\gamma_{gained} = \frac{Pop_T^{Urb,gained}}{Pop_T^{Urb,remain}}$ be the urban population in territories gained during the reign relative to the that in territories that remained under control during the entire reign. Similarly, denote by $\gamma_{lost} = \frac{Pop_t^{Urb,lost}}{Pop_t^{Urb,remain}}$ the fraction of urban population in the beginning of the reign in territories lost, relative to the population in areas kept. Then:

$$\frac{Pop_T^{Urb}}{Pop_t^{Urb}} = \frac{Pop_T^{Urb,remain}(1+\gamma_{gained})}{Pop_t^{Urb,remain}(1+\gamma_{lost})} = (1+\gamma_{intensive})\frac{1+\gamma_{gained}}{1+\gamma_{lost}}$$

where $1+\gamma_{intensive} = \frac{Pop_T^{Urb,remain}}{Pop_t^{Urb,remain}}$ and $\gamma_{intensive}$ is the rate of urban population growth in areas that remained under a monarchs control during the reign.

Applying logarithms, this yields a composition of percentage change in urban population into three components:

$$log(Pop_T^{Urb}) - log(Pop_t^{Urb}) =$$

$$\underbrace{log(1+\gamma_{intensive})}_{\text{city growth in areas remaining under control}} + \underbrace{log(1+\gamma_{gained})}_{\text{acquisition of cities}} - \underbrace{log(1+\gamma_{lost})}_{\text{loss of cities}}$$

Table 3.30 shows the results of using log changes in total urban population and its components as outcomes in our baseline estimation. Columns 1 and 2 show sizable total effects. A one standard deviation in the ability of a monarch increases urban population by 10%. This largely stems from capable monarchs conquering territories containing relatively large urban populations (compared to their initial territories), as column 6 indicates.

Table 3.30: Urban Population as Measure of State Performance

Dependent Variable: Log Change in Urban Population During Reign ...

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(7)
Dep. Var:	Total		Intensive		Acquisition		Loss	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Ruler Ability	0.078***	0.106*	0.027***	-0.037	0.033	0.133***	-0.018	-0.014
	(0.022)	(0.058)	(0.008)	(0.024)	(0.019)	(0.039)	(0.012)	(0.034)
State FE	✓	✓	✓	✓	✓	✓	✓	✓
First Stage F-statistic		38.73		38.73		35.11		38.73
R^2	0.07		0.16		0.11		0.09	
Observations	280	192	280	192	281	193	280	192

Note: All regressions are run at the reign level. Table shows results from instrumental variable regressions, in which ruler ability is instrumented with the coefficient of inbreeding. Standard errors clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

3.7.10 Constraints on Executive

In this section we provide detail on our year-to-year constraints on the executive measure and an example. We show robustness to using different cutoffs for the definition of our constrained ruler dummy in the interaction analysis, and to restricting the sample to rulers ascending to the throne through primogeniture only.

3.7.10.1 Yearly "Constraints on Executive" Measure

Constraints on the Executive refer to legal and de-facto constraints limiting the actions of the executive branch of government. In a widely used measure, the Polity IV project provides a categorical variable measuring the relative strengths of these constraints across countries from 1800 onward (Marshall et al., 2017). Acemoglu et al. (2005) code up a similar variable at the 100 and 50 year interval from 1000 CE onwards. They base the measure on an encyclopedia of world history (Langer, 1972; Stearns and Langer, 2001). We follow their approach, but additionally identify the exact year when constraints on the executive (whereby we focus on the monarchs exclusively) changed.[65]

The categories of "constraints on the executive" range from "1" to "7", where "1" indicates unlimited authority of the monarch and "7" indicates "Executive Parity or Subordination" to other branches of government. In our baseline estimation, we define an indicator of a monarch being constrained when constraints on the executive are above "5" – "Substantial Limitations on Executive Authority".

We list the categories below:

- 1: Unlimited Authority: There are no regular limitations on the executive's actions (as distinct from irregular limitations such as the threat or actuality of coups and assassinations.)
- 2: Intermediate Category
- 3: Slight to Moderate Limitation on Executive Authority: There are some real

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[65] Except for Turkey, which is not covered by these sources, we do so for all states in our data set.

but limited restraints on the executive.

- 4: Intermediate Category
- 5: Substantial Limitations on Executive Authority: The executive has more effective authority than any accountability group but is subject to substantial constraints by them.
- 6: Intermediate Category
- 7: Executive Parity or Subordination: Accountability groups have effective authority equal to or greater than the executive in most areas of activity

3.7.10.2 Robustness

In the paper, we create a dummy indicating that a ruler was constrained when at the year prior to the reign the categorical variable took the value "5" or above. In Table 3.31, we relax this cutoff to the "intermediate" value "4".

Table 3.31: Robustness: Constrained Monarchs Matter Less - Different Cutoffs

Dep. Var.: Performance of Country During Reign of Monarch

	(1)	(2)	(3)	(4)
Constraints coding:	Author's Coding		AJR (century level)	
Estimation:	OLS	IV	OLS	IV
Ruler Ability	0.116^{***}	0.172^{***}	0.115^{**}	0.205
	(0.037)	(0.051)	(0.039)	(0.123)
Constrained Ruler	-0.054	-0.113^{*}	-0.020	0.069
	(0.055)	(0.062)	(0.079)	(0.183)
Constrained Ruler × Ruler Ability	-0.096^{*}	-0.095	-0.103^{**}	-0.061
	(0.050)	(0.213)	(0.039)	(0.170)
R^2	0.10		0.11	
Observations	295	200	269	178

Note: All regressions are run at the reign level. Standard errors clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

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