Gone With The Wind? Slaveholding and Immigrant Integration after Emancipation

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Abstract

Cooperation between culturally distant groups requires mechanisms that signal norm alignment and establish trust. This paper examines how the removal of such a mechanism affects immigrant economic integration, studying the abolition of slavery in the Southern U.S. between 1850 and 1870. In the antebellum South, slaveholding functioned as an institutionally codified marker of adherence to local racial norms, facilitating immigrant acceptance into the hosting society. Using a panel of linked individuals from Census data, I exploit emancipation in 1865 as an exogenous shock that eliminated this marker of belonging. I employ a triple-difference design comparing European immigrant and U.S.-born slaveholders to their non-slaveholding counterparts before and after abolition emancipation, and I proxy assimilation through occupational standing. The results show that only immigrant slaveholders experienced sharp occupational downgrading after 1865, while all other groups maintained or improved their positions. This decline is concentrated within white-collar occupations—the least dependent on enslaved labor—ruling out explanations based on wealth and technology shocks, or labor market discrimination. Additional evidence confirms that the penalty is absent among immigrants with U.S.-born spouses, and extends to the second generation. The findings demonstrate that institutionally recognized markers of cultural alignment shape immigrant economic outcomes, and that their removal imposes asymmetric costs.

1 Introduction

Humans are among the few animals that cooperate beyond kinship, and when considering the scale and scope of such cooperation, they are unique. Human cooperation operates on a global scale and includes complex, non-kin, and non-reciprocal interactions (e.g., donating blood to a stranger). The puzzle is why. Research points toward cultural similarity in norms and beliefs as the main driver, rather than merely genetic or geographic proximity (Melis and Semmann, 2010). Norms, beliefs, and shared cultural frameworks shape when and with whom humans cooperate (Boyd and Richerson, 2009). Cultural norms influence expectations about others' behavior, make cooperative behavior predictable and enforceable, and stabilize cooperation even with non-kin and strangers (Henrich and Henrich, 2007). Cultural similarity acts as a proxy for shared expectations of behavior and trustworthiness; and where norms differ, cooperation becomes weaker or more conditional (Handley and Mathew, 2020) [1].

Migration introduces a fundamental discontinuity into this system. Immigrants and natives constitute groups with potentially divergent cultural norms, creating an asymmetric information problem: in-groups face uncertainty about whether out-groups will comply with local expectations, honor implicit contracts, or behave predictably in economic transactions. A potential response could be exclusion or costly monitoring; barriers that impede immigrant economic integration. Successful coordination between immigrants and natives thus hinges not only on human capital or wealth, but on establishing shared expectations about behavior. In settings where cultural norms are difficult to observe directly or converge slowly across generations, institutional arrangements can serve as coordination devices (Alvarez-Benjumea et al., 2026). Certain behaviors or memberships function as credible signals of norm compliance (Alvarez-Benjumea and Valentim, 2024). For immigrants, adopting these markers provides a mechanism to overcome the informational barriers that would otherwise exclude them from in-group cooperation networks. However, what happens when an exogenous shock removes the marker that facilitated immigrant integration? In other words, what are the consequences for immigrants who adopt a host society's membership norm when that norm is no more available?

This paper addresses this question by exploiting a natural experiment: the abolition of slavery in 1865. In the antebellum South, slaveholding functioned as far more than an economic asset. It operated as an institutionally codified marker of adherence to the region's fundamental social and racial norms. The ownership of enslaved people signaled not only prosperity but also normative commitment to the hierarchical racial order that structured Southern society. As Gleeson (2001) documents, slaveholding conferred membership in what contemporaries explicitly termed the "ruling race," marking the owner as someone who had accepted and internalized the values of white supremacy and paternalistic social organization that defined elite standing in the region. For European immigrants arriving in the South during the mid-nineteenth century, this created both a challenge and an opportunity. Immigrants faced the informational barriers inherent to out-group

[1] These phenomena are often captured in social psychology as in-group favoritism and inter-group bias (Gächter et al., 2010).

status: they lacked the kinship networks, local reputations, and cultural markers that native-born Southerners possessed from birth (accent, lineage, family connections). Slaveholding offered a solution. By acquiring slaves, immigrants could signal their alignment with Southern norms in a manner that was both visible and institutionally recognized. Unlike kinship ties, which required generations to establish, or local reputations, which accumulated slowly through repeated interactions, slaveholding could be acquired relatively quickly through capital investment. It functioned as a fast track to demonstrating cultural integration and gaining acceptance into the networks that controlled access to elite social circles. The extent to which immigrants adopted this strategy is reflected in the data. As Oakes (1998) observed, the proportion of foreign-born slaveholders increased markedly in the 1850s, with ownership becoming "the single most important symbol of their success in the New World." Slaveholding thus served not merely as productive capital but as social capital. Emancipation eliminated not only the economic value of slave property but also the signaling function that slaveholding had performed. For all slaveholders, this constituted a substantial wealth shock and required economic adjustment to a transformed labor system. However, the consequences extended beyond material losses. The institution that had allowed immigrants to credibly signal their integration into Southern society vanished. Native-born Southerners, by contrast, retained the kinship networks, local reputations, and cultural markers that had always coexisted alongside slaveholding as indicators of insider status. This asymmetry creates a quasi-experimental setting: the same institutional shock removed an economic asset for both groups, but removed a critical coordination mechanism primarily for one.

In the mid-nineteenth-century United States, two defining and contemporaneous phenomena shaped the social and political landscape: the centrality of slavery as an economic and ideological institution (Masera and Rosenberg, 2025; Masera et al., 2024) and the onset of the Age of Mass Migration from Europe (Abramitzky and Boustan, 2017; Sequeira et al., 2020). While both developments have generated vast literatures, their intersection has received remarkably little systematic attention. Understanding what happened to immigrant slaveholders after emancipation is historically interesting in its own right. Yet the historical setting is not chosen solely for descriptive purposes. Rather, it provides a compelling natural experiment to study a more general question (Charnysh, 2024; Charnysh et al., 2023). This setting allows me to isolate how institutionally codified markers of cultural alignment shape immigrants' integration trajectories, conditional on observable endowments of human capital and wealth.

I rely on a balanced panel linking individuals across the 1850–1870 U.S. Censuses and estimate a triple-difference specification comparing European immigrant slaveholders to two benchmarks: immigrant non-slaveholders and U.S.-born slaveholders. The design exploits within-individual variation and across-group contrasts, absorbing county-by-year fixed effects and allowing returns to baseline endowments (education, wealth, and skills) to evolve flexibly over time. The key coefficient isolates the post-1865 occupational change (my proxy of assimilation) unique to immigrant slaveholders, net of general immigrant trends and common slaveholder effects (captured by the two double interactions), and time-invariant individual heterogeneity (captured by individual fixed

effects).

Threats to identification could come from several sources. A first concern relates to the timing and nature of emancipation. If abolition had been anticipated well in advance, immigrant slaveholders might have adjusted occupational choices, investment decisions, or portfolio allocations before 1865. Such anticipatory behavior would weaken the interpretation of emancipation as an exogenous shock. However, the Civil War was widely regarded as unexpected in both its outbreak and duration, and its outcome remained highly uncertain until very late in the conflict (Calomiris and Pritchett, 2016). Unlike the gradual abolition in the British Empire, emancipation in the United States was not accompanied by monetary compensation to slaveholders, nor was it implemented through a pre-announced legislative transition. The ratification of the Thirteenth Amendment in December 1865 formalized a process whose precise legal and economic contours had remained unsettled throughout the war. This institutional rupture therefore sharply constrained the scope for systematic pre-adjustment. A second concern is selection operating along two dimensions: first, into migration (and destination choice), and second, into slaveholding. Immigrants who chose to settle in the South—and especially those who became slaveholders—may have differed systematically from both natives and non-slaveholding immigrants in unobservable traits such as wealth, risk tolerance, or entrepreneurial ability. The empirical strategy mitigates this concern in two ways. The triple-difference design differences out time-invariant gaps between immigrants and natives as well as between slaveholders and non-slaveholders, isolating only differential post-1865 adjustments. Moreover, I extend the analysis for the second-generation children (at baseline), whose occupational outcomes cannot reflect their decision to migrate or to accumulate slaves, providing an additional check against first-generation selection driving the results. A third concern is that, absent emancipation, the difference in occupational trajectories between slaveholders and non-slaveholders might have evolved differently for immigrants and the U.S.-born. In other words, the pre-1865 evolution of the double difference between slaveholding and non-slaveholding status must be parallel across nativity groups. Event-study estimates support this restriction. The coefficients on the triple interaction for pre-trnds are small and statistically indistinguishable from zero, indicating that prior to emancipation the slaveholder premium evolved similarly for immigrants and natives. Divergence emerges only after 1865, consistent with emancipation generating the observed differential adjustment. A fourth concern involves compositional changes induced by migration, mortality, or other forms of selective attrition across Census waves. If immigrant slaveholders were differentially more likely to exit the sample, cross-sectional comparisons could confound true occupational change with sample reallocation. The linked panel design directly addresses this issue by tracking the same individuals across three Census waves, thereby eliminating biases driven by changing sample composition. The panel structure also allows me to control for time-invariant individual characteristics, such as unobserved ability, entrepreneurial skill, or family background, that could correlate with both slaveholding status and occupational trajectories. A fifth concern is that emancipation may have coincided with other war-related shocks that differentially affected immigrant slaveholders within counties; for instance through exposure to more export-oriented or

heavily devastated plantation areas. While the Civil War made abolition conceivable, the identification challenge arises from the possibility that concurrent shocks, rather than emancipation per se, drove the divergence. County-by-year fixed effects absorb all local time-varying shocks, and the triple difference structure removes shocks common to all slaveholders or all immigrants. Remaining bias would require shocks that differentially targeted immigrant slaveholders relative to native slaveholders within the same county. This is an implausible scenario, further undermined by the fact that analogous penalties are observed for the second generation, a distinct sample. Finally, differential measurement error in slaveholding status across nativity groups could potentially contaminate estimates. However, Census enumerators operated under strict confidentiality rules and legal penalties for misreporting, and were instructed to verify responses actively, reducing the scope for systematic nativity-specific misclassification.

The results reveal that after 1865, European immigrant slaveholders experienced a substantial occupational downgrade, a pattern that is absent among both native slaveholders and immigrant non-slaveholders. This asymmetry is striking: while immigrant slaveholders experienced occupational decline, U.S.-born slaveholders maintained or even improved their occupational standing over the same period, despite suffering similar wealth losses from emancipation. Likewise, immigrant non-slaveholders experienced occupational upgrading rather than downgrading. The penalty is thus specific to the intersection of being both an immigrant and a former slaveholder.

Several potential explanations that could rationalize the occupational downgrading warrant consideration. A first possibility could be that the results reflect wealth shocks rather than coordination failures (due to the loss of credibility) between immigrants (out-group) and natives (in-group). Emancipation eliminated slave property, which constituted a substantial share of Southern wealth, and this loss could have mechanically constrained access to capital-intensive occupations. I address this in two ways. First, all specifications control for baseline wealth measured in 1850, and I allow the returns to wealth to vary flexibly over time by interacting wealth with year fixed effects. Second, I document that U.S.-born slaveholders, who experienced equivalent wealth losses, did not suffer occupational downgrading, ruling out wealth shocks as the primary mechanism. Another explanation could be that slave labor was a direct input into immigrants' occupations, and emancipation disrupted production technologies. This channel would predict larger effects in occupations that relied heavily on enslaved labor. The evidence contradicts this prediction. I show that the occupational penalty is concentrated in white-collar positions (rather than in craft, operatives, or unskilled jobs). In particular, I show that within the subsample of individuals who remain in white-collar occupations throughout the period, immigrant slaveholders are sorted into systematically less-paid positions after 1865, a pattern inconsistent with technology-based explanations. A third possibility could be discrimination. If native Southerners became more hostile toward immigrants after the war, the penalties could reflect increased xenophobia. Several pieces of evidence rule this out. First, immigrant non-slaveholders experienced occupational upgrading after 1865, not downgrading, inconsistent with broad-based anti-immigrant sentiment. Second, immigrants who married native-born spouses show no occupational penalty, suggesting that integration into

kinship networks insulated them from any generalized hostility. Third, the penalty is concentrated in high-slave-intensity counties where there were few white-collar peers, not in areas with large immigrant populations where discrimination might plausibly be strongest. Taken together, these patterns point to coordination failures specific to immigrant slaveholders who lacked substitute mechanisms, not to economy-wide discrimination against foreigners.

Instead, the evidence points toward the fact that the penalty operated through loss of locally recognized coordination markers rather than alternative channels. First, geographic mobility reveals the local nature of 'belonging'. The penalty is entirely driven by individuals who changed counties between 1860 and 1870, while those who remained in the same county experienced no occupational downgrading. Critically, this pattern is specific to post-emancipation movers: immigrants who relocated during the 1850s while still owning slaves show no subsequent penalty, confirming that movement per se did not harm integration. The divergence emerged only when immigrants arrived in new communities without the credential that had previously signaled their integration. This geographic specificity demonstrates that slaveholding functioned as a locally validated marker of belonging rather than a portable form of human or financial capital. Stayers retained the personal relationships and reputational capital accumulated through years of residence, which substituted for the abolished institutional signal. Movers, arriving as strangers in post-emancipation communities, lacked both the credential and the time to build alternative networks. Second, the effect depends on the duration of slaveholding. Long-standing slaveholders (those who owned slaves in both 1850 and 1860) experienced significant penalties, while recent adopters (owners only in 1860) and former owners (just in 1850) show no decline. This pattern indicates that establishing social recognition required sustained engagement with the institution. Temporary or opportunistic ownership was insufficient to build the depth of local acceptance and identity capital that emancipation then removed. The coordination mechanism thus operated not through wealth or productivity but through the gradual accumulation of social legitimacy that prolonged institutional participation conferred. Third, intermarriage with native-born spouses fully insulates immigrant slaveholders from the occupational penalty. Marriage into U.S.-born families provided an alternative coordination mechanism, i.e. kinship networks that conveyed in-group status. The fact that this substitute marker completely offsets the effect of losing the institutional credential reinforces that coordination, not wealth or skills, drove the observed penalties. Immigrants who acquired locally embedded networks through marriage could weather the institutional shock; those who remained socially isolated could not. Lastly, I document parallel occupational penalties among the second generation, namely the children of immigrant slaveholders who made no migration or ownership decisions. This intergenerational persistence is inconsistent with wealth or human capital losses: children of native slaveholders, whose parents experienced equivalent economic shocks, show no comparable decline. The effect instead points toward status-based transmission: parents' capacity to confer social recognition mattered for children's labor-market entry, and this capacity evaporated when the inheritable signal disappeared.

This paper speaks to three strands of literature. Firstly, a large literature documents that

norms and attitudes can persist over long horizons through intergenerational transmission, generating durable heterogeneity in economic behavior (Bisin and Verdier, 2001, 2010; Voigtländer and Voth, 2012). At the same time, norms are not immutable: institutional shocks can induce contemporaneous cultural change (Cantoni et al., 2024). Yet we know far less about what happens when a deeply entrenched norm is not merely challenged but formally outlawed—when the institutional shock consists precisely in prohibiting the norm itself. This question is central because norms often regulate inter-group interactions, restrict exchange, and sustain discriminatory equilibria with substantial efficiency and equity costs (Agte and Bernhardt, 2024). A growing body of work shows that norms shape consumption and social behavior (Atkin et al., 2021) and that individuals strategically adjust identity investments in response to economic incentives (Akerlof and Kranton, 2000; Shayo, 2009, 2020). Moreover, minority groups frequently emulate practices associated with high-status groups to facilitate assimilation (Bisin et al., 2016; La Ferrara et al., 2012), underscoring that group membership (and the status attached to it) has first-order economic consequences. I contribute to this literature by studying how the prohibition of a status-defining norm reshapes identity choice and inter-group behavior. More broadly, the project speaks to the determinants of cultural convergence across space and the role of mobility and spatial frictions in shaping identity. While much of the literature emphasizes trade and globalization as drivers of cultural convergence (Bisin and Verdier, 2014), migration and homophily also structure the diffusion of norms (Rapoport and Silve, 2020). Quantitative spatial approaches show that cultural distance constrains mobility and economic adjustment (Falck et al., 2012; Jaworski et al., 2024), and that identity itself can operate as a barrier to migration and aggregate reallocation (Fouka and Serlin, 2024). By examining how the removal of a legally enforced norm alters the returns to cross-group interaction, my analysis provides individual evidence on how institutional change relaxes (or reinforces) cultural segmentation across space. Finally, the paper connects to the literature on cross-societal differences in cooperation. Economic accounts stress the interaction between moral values and incentives in sustaining broad versus narrow cooperation (Enke, 2019; Tabellini, 2008), while political science highlights how identical values can yield different cooperation outcomes under distinct payoff structures (Bednar and Page, 2018). By isolating the causal effect of banning a norm that structured hierarchy and exchange, I show how institutional reform reshapes both the material payoffs and the identity-based preferences that underpin cooperation, thereby linking institutional change, identity formation, and the scope of economic integration within a unified empirical framework.

Secondly, this paper contributes to the vast literature examining the determinants and consequences of immigrant assimilation. Economists have identified multiple forces shaping integration, including time spent in the host country (Abramitzky et al., 2014), exposure to culturally distant immigrant groups (Fouka et al., 2022), age at arrival (Arellano-Bover and San, 2025), coercive schooling (Maruthiah, 2024), religious organizations (Gagliarducci and Tabellini, 2024), and violence in the country of origin (Zaiour, 2024). A growing body of work also emphasizes the role of discrimination and coercive assimilation policies. Gould and Klor (2016) show that discrimination against Muslims after 9/11 discouraged assimilation, while Fouka (2019) and Abdelgadir and Fouka

(2020) document that policies such as the prohibition of German-language schooling or the French headscarf ban can strengthen minority identification rather than weaken it. Conversely, Saavedra (2021) finds that Japanese and German immigrants responded to wartime hostility by increasing their assimilation efforts. Together, these studies highlight that immigrant responses to external shocks depend critically on the surrounding institutional and social environment. This paper extends this literature by emphasizing the role of social status as a mediating factor in immigrant integration. While prior work has focused primarily on human capital, cultural proximity, and policy regimes, I show that assimilation also depends on whether immigrants hold locally recognized markers of legitimacy that facilitate access to elite occupations. Methodologically, the analysis exploits a balanced panel linking individuals across successive U.S. Census waves, which allows occupational trajectories to be followed within individuals over time. Finally, in contrast to much of the existing literature that examines shocks or policies that facilitated assimilation, this paper studies the consequences of a negative shock, namely the removal of a key social credential. Ex ante, it is not obvious that the mechanisms promoting integration operate symmetrically with those that obstruct it.

The third strand of literature to which this paper relates is the economics of slavery and slaveholders. American slavery has long been the most studied institution in the field of U.S. History, yet the economics literature on slaveholders is more recent. The advantages of slavery for Southern agriculture have been well documented in Economics (Fogel and Engerman, 1974; Wright, 1974), and scholars have emphasized the multiple roles slaves played as agricultural labor, collateral for credit, and liquid assets (Gonzalez et al., 2017; Martin, 2010; Tadman, 1990). Bellani et al. (2020) show that former slave owners remained dominant in Southern legislatures for decades after the Civil War, while Ager et al. (2021) demonstrate that their descendants eventually recovered the wealth lost through emancipation. This persistence was driven by mechanisms ranging from marriage-market advantages. Other investigated mechanisms are continued access to cheap Black labor under coercive arrangements, as well as westward migration to frontier areas with weak institutions where former elites could recreate hierarchical social orders (Acharya et al., 2016; Bazzi et al., 2023; Rubio-Ramos, 2022). While this literature has focused on native-born slaveholders, I highlight a previously overlooked group: European immigrants who used slaveholding as a means of assimilation. The institution of slavery has not previously been conceptualized as status capital, yet in the antebellum South, slaveholding was a signal of belonging to the local elite. By examining how emancipation differentially affected immigrant and native elites, I reveal that the same institutional shock had asymmetric consequences.

The remainder of the paper proceeds as follows. Section 2 describes the historical context. Section 3 discusses data construction. Section 4 outlines the empirical strategy. Section 5 presents the main results. Section 6 discusses mechanisms and alternative interpretations. Section 7 concludes.

2 Historical Context

This section provides the historical background necessary to understand the empirical setting and identification strategy. I first describe the scale and characteristics of European immigration to the antebellum South, emphasizing that immigrants were a non-trivial presence in the region's economy and society. I then explain how slaveholding functioned as both an economic asset and a status marker, with particular emphasis on why this dual role mattered for immigrant integration. Finally, I discuss the Civil War, emancipation, and Reconstruction, establishing why the abolition of slavery constituted an abrupt and largely exogenous shock to the social order.

2.1 European Immigrants in the Southern U.S.

The nineteenth century witnessed a sharp rise in European immigration to the United States, particularly beginning in the 1840s. This wave was propelled by both economic hardship and political upheaval in Europe, especially the Irish potato famine and the failed revolutions of 1848 in the German states. Technological improvements in transatlantic shipping further lowered costs and reduced travel time, opening migration opportunities to a broader set of households. Standard narratives emphasize that the South was an unlikely destination for these migrants. The region's predominantly rural economy, its entrenched reliance on enslaved labor, and its Protestant Anglo-American culture are often portrayed as creating a hostile environment for foreigners. By contrast, the urbanizing North, with its industrial demand for labor and dense port cities, is remembered as the natural entry point for European immigrants. This interpretation has led historians to overstate the extent to which immigrants "avoided" the South. Closer examination of the evidence, however, reveals a more nuanced story. Early scholarship by MacGill (1909) challenged the notion of immigrant absence in the South, documenting their presence. South Carolina, for instance, was described by Maguire (1868) as "probably the most Irish of any of the States of the Union," highlighting both the scale and cultural visibility of Irish settlement. Weaver (1947) underscored the important role of immigrant groups in Southern urban centers before the Civil War, complicating the image of a purely native-born white South. In the antebellum South, Southern White households had greater wealth than Northern ones across the entire distribution (Ager et al., 2021). Major port cities were critical nodes of immigrant settlement. Charleston, which by the end of the colonial period ranked as the fourth largest city in North America, served as a magnet for European arrivals and a hub of Atlantic trade. Even more striking was the role of New Orleans: between 1834 and 1844 it was the busiest port in the United States, and by 1850 it ranked second only to New York in foreign immigrant inflows (Gleeson, 2001). The city's economic dynamism was well recognized: contemporary observers noted that "there is no place in all of the states so good for making money as New Orleans" (Clark, 7 Apr., 1819). Such accounts testify to the fact that, far from being closed to outsiders, the Southern economy in certain areas offered lucrative opportunities that could rival or even exceed those available in the North.

2.2 Slavery in the U.S.

In 1860, about 21 percent of white Southern households owned at least one enslaved person, though ownership was highly concentrated: only 0.5 percent of households held more than 50 slaves (Soltow, 1975). Large planters such as Joshua J. Ward, often referred to as the "King of the Rice Planters", epitomized this concentration of wealth and prestige. Slaveholding was not merely an economic investment; it was a credential for political influence and a gateway to the highest ranks of society. Southern politicians, judges, and legislators were disproportionately drawn from the "ruling race", underscoring the institution's dual role as both an economic asset and a badge of social distinction. The market for slaves mirrored this dual function. Prices for enslaved people rose steadily throughout the 1850s, peaking on the eve of the Civil War (Calomiris and Pritchett, 2016). The surge in prices reflected not only the profitability of plantation agriculture but also the political protection enjoyed by the institution. In cultural and symbolic terms, slavery was deeply entrenched. Pro-slavery novels of the period, such as The Planter's Northern Bride (1854) or The Black Gauntlet (1860), portrayed slaveholders as benevolent patriarchs, often likened to Roman aristocrats, belonging to a natural social and moral aristocracy, or highlighted the fact that being a skaveholder was crucial in order to succeed in the marriage market. Slavery was also highly visible in urban settings: enslaved men and women performed domestic and public tasks that made the institution ubiquitous in everyday life. Contemporary observers noted that in New Orleans, for instance, it was rare to see white individuals in the city markets, as nearly all food purchasing was conducted by enslaved people.

2.3 The U.S. Civil War and the Abolition of Slavery

By the mid-nineteenth century, slavery occupied a central position in the political, economic, and social order of the Southern United States. Beyond its role as a labor system, slaveholding functioned as a widely recognized marker of elite standing, shaping access to political influence, social networks, and economic opportunities (Genovese, 1965; Wright, 1978). While sectional tensions intensified during the 1850s, the precise timing and scope of the institutional collapse of slavery remained highly uncertain prior to the Civil War. The outbreak of the war in 1861 did not immediately resolve the status of slavery. For much of the conflict, Union war aims focused on preserving the federal union rather than abolishing the institution. Emancipation emerged gradually through a sequence of military, legal, and political decisions, culminating only with the ratification of the Thirteenth Amendment in December 1865 (Foner, 1988). Contemporary accounts and subsequent scholarship emphasize that neither the timing nor the enforcement of abolition was fully anticipated by economic actors, limiting scope for forward-looking adjustment based on certain institutional change. This institutional rupture was particularly consequential because it dismantled a social order that had structured local hierarchies for decades. Slaveholding had served as a visible and legible signal of wealth, ideology, and social alignment within Southern communities. Its abolition therefore represented not only the elimination of a property right, but the sudden invalidation of a status credential that had governed access to elite positions in labor markets, politics, and civil

society (Wright, 1986). The disruption was uneven across space and individuals. Reconstruction unfolded heterogeneously, with variation in federal presence, political enforcement, and local resistance shaping post-war social relations (Foner, 1988; Alston and Ferrie, 1999). In many counties, prewar elites retained informal influence despite formal institutional change, while in others local hierarchies were rapidly reconfigured. This spatial heterogeneity implies that the consequences of emancipation depended critically on local recognition and embeddedness rather than on uniform economic shocks. Importantly for this study, European immigrants occupied a distinctive position within this social structure. While immigrants participated in Southern economic life and, in some cases, in slaveholding, they lacked the intergenerational ties and entrenched reputations of native-born elites. Historical evidence indicates that for such groups, slaveholding offered a particularly salient means of signaling alignment with Southern norms and gaining acceptance within local hierarchies (Oakes, 2012).

3 Data

The analysis draws on individual-level data from the IPUMS U.S. Census for 1850, 1860, and 1870. I leverage linked longitudinal data from Census Tree[2], which allows tracking the same individuals across successive censuses. The original linkage follows 10.9 million individuals from 1850 to 1860 and 8.45 million individuals from 1850 to 1870. This enables the construction of a balanced panel in which the same individuals are consistently observed over three decades. The use of linked Census data offers two critical advantages. First, it allows estimation of individual fixed effects, ensuring that identification comes from within-person changes in occupational standing rather than compositional shifts across groups. Second, it addresses concerns about selective attrition due to migration or mortality, as I can directly verify that individuals remain in the sample throughout the entire period.

I impose several restrictions to focus on the population most relevant for the research question and to minimize confounding factors. First, I restrict the sample to individuals who remained in the South throughout the 1850–1870 period [3]. This geographic restriction minimizes confounding from selective outmigration to other regions, ensuring that observed occupational changes reflect local labor-market adjustments rather than migration decisions. Second, I restrict to male household heads aged 15–64. The age restriction defines a cohort that is economically active throughout the period. Focusing on household heads ensures that I observe individuals with stable labor-market attachment and decision-making authority within their households. Third, I exclude individuals ever recorded as farmers/planters across the three census waves. This exclusion serves multiple purposes. Most importantly, they were big outliers in the economy. Only 0.5 percent of households held more than 50 slaves in 1860 (Soltow, 1975). Additionally, the existing literature on

[2] https://www.censustree.org/

[3] Following Ager et al. (2021), Southern states are defined as Delaware, Missouri, Virginia, Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas, Kentucky, Maryland, Oklahoma, Tennessee, West Virginia, and the District of Columbia.

slaveholders has focused almost exclusively on large agricultural planters (Ager et al., 2021; Bellani et al., 2020); examining non-farmer slaveholders provides a novel perspective on how slaveholding functioned as a marker of belonging beyond the plantation economy. Finally, IPUMS documentation notes a systematic misclassification for the period 1850-1880: laborers without an industry code are reassigned to farm labor when residing in a farmer's household, potentially confounding occupational mobility patterns [4]. As a last restriction, I exclude individuals who ever resided in group quarters during any of the three census waves. This ensures that the sample consists of individuals with stable household structures and comparable living arrangements. These restrictions yield a balanced panel of 22,267 individuals tracked across all three census waves (of which 3,338 are European immigrants), for a total of 66,801 person-year observations. See Figure 1, Figure 2, Figure 3, Figure 4, and Figure 5 for additional informations.

The primary outcome, a measure of assimilation, is the log occupational income score, constructed by IPUMS [5]. It assigns each occupation a value representing the median total income (in hundreds of 1950 dollars) of all persons with that occupation and provides a continuous measure of occupations according to the economic returns received by workers in them. Thus, the figures do not change over time and across space. Because individuals with no reported occupation receive a score of zero (yielding a missing value after the log transformation), the analysis is conditional on employment. This ensures that the sample consists of individuals actively participating in the labor market throughout the period [6].

The key treatment variable is a dummy variable for being for whether the individual resided in a slaveholding household in 1850 [7]. A household is classified as slaveholding if at least one member owned at least one enslaved person. This household-level definition captures the social and economic benefits of slaveholding that could accrue to individuals even if they were not the direct legal owner. The second treatment variable is a dummy variable for being an European immigrant. Figure 6 describes how the occupational income score varies among slaveholders and not. To further characterize the occupational composition of slaveholders, Figure 7 shows the share of slaveholding households across four broad occupational categories: White Collar, Craft, Operative, and Unskilled (see Collins and Zimran, 2023) [8]. This heterogeneity is important for two reasons. First, it demonstrates that slaveholding was not limited to a narrow elite of large agricultural planters. Second, it shows that white-collar slaveholders, who had the highest incidence of slaveholding, were the least reliant on enslaved labor for direct production. In white-collar occupations, enslaved individuals typically performed domestic or auxiliary tasks rather than serving as core productive inputs. This suggests that the value of slaveholding for white-collar workers was not purely economic but also symbolic: it signaled respectability and alignment with Southern norms.

These descriptive patterns establish three key facts that motivate the empirical analysis. First,

[4] https://usa.ipums.org/usa-action/variables/occ1950

[5] https://usa.ipums.org/usa-action/variables/OCCSCORE

[6] With the available data, it is not possible to disentangle whether a person is retired, unemployed or has become unable to work over time (e.g., as a result of injuries incurred in the Civil War).

[7] https://usa.ipums.org/usa-action/variables/SLAVEHH

[8] Figure 8 focuses only on immigrants

a non-trivial share of European immigrants owned slaves and occupied similar economic positions to native slaveholders at baseline. Second, slaveholding was concentrated in white-collar occupations, where status signals matter most for labor-market success. Third, within the sample, immigrant and native slaveholders were observably similar in 1850, making the post-1865 divergence in their occupational trajectories particularly striking and unlikely to be explained by pre-existing differences.

To characterize occupations in a way that captures their underlying task content rather than relying solely on historical titles, I draw on the task-based classification developed by Song et al. (2020). This framework maps nineteenth-century occupations into two mutually exclusive categories: professional/managerial (pm) and routine. The classification is constructed by linking Census occupational titles to standardized task measures that reflect autonomy, supervisory authority, and the intensity of non-routine cognitive activities. By adopting this harmonized scheme, I ensure comparability across Census waves and reduce measurement error stemming from changes in occupational nomenclature over time. Moreover, the task-based approach provides a consistent metric to distinguish higher-status white-collar positions from more routinized clerical roles, which is central to interpreting post-emancipation occupational reallocation.

Lastly, to address concerns related to selection into migration and slaveholding, I construct a second-generation sample. This sample includes individuals who were classified as children in the 1850 Census and are subsequently observed as independent household heads by 1860 or 1870. By construction, this sample differs from the main analysis sample, which focuses on household heads at baseline. It follows 3,085 individuals across all three census waves, yielding 9,255 person-year observations. Second-generation individuals represent 6.5 percent of the linked balanced panel. At baseline, 27.9 percent resided in slaveholding households. Among second-generation individuals with immigrant parents, this share rises to 31.8 percent, indicating that exposure to slaveholding was common even among children of immigrants. Like above, all individuals in this sample are male, never classified as farmers, and never observed residing in group quarters. Crucially, individuals in this sample did not choose whether to migrate to the United States, whether to settle in the South, or whether their household owned enslaved persons in 1850. This second-generation sample therefore allows me to isolate intergenerational persistence in occupational outcomes while mitigating concerns that results for the first generation are driven by endogenous selection into migration or slaveholding.

4 Empirical Strategy

The research question requires isolating an immigrant-specific penalty from the abolition of slavery while accounting for shocks that affected all slaveholders or all immigrants. A simple difference-in-differences comparing immigrant to native slaveholders before and after 1865 would confound status loss with differential exposure to common shocks such as wealth destruction, labor supply disruptions, or sectoral reallocation. Similarly, comparing immigrant slaveholders to immi-

grant non-slaveholders would capture the average effect of losing slave property rather than the immigrant-specific consequence of losing a marker of belonging.

The triple-difference design addresses both concerns by exploiting variation along three dimensions: nativity (European immigrant vs. U.S.-born), slaveholding status (owner in 1850 vs. non-owner), and time (pre-1865 vs. post-1865). The key insight is that shocks common to all slaveholders—such as wealth losses, disruption of slave-based production, or adjustment to free labor markets—should affect immigrant and native slaveholders similarly. By differencing across nativity, the estimator nets out these common components, isolating the portion of the post-1865 occupational response unique to immigrant slaveholders. This identifies the effect of losing an institutionally codified coordination marker, conditional on experiencing the same economic shocks as natives.

Formally, I estimate:

$$\begin{aligned}\log Y_{i,c,t} = \beta_1(\text{Immigrant}_i \times \text{Post}_t) + \beta_2(\text{Slaveholder}_i \times \text{Post}_t) \\ + \beta_3(\text{Immigrant}_i \times \text{Slaveholder}_i \times \text{Post}_t) + \Gamma X_{i,t} + \mu_i + \theta_{c,t} + \varepsilon_{i,c,t},\end{aligned} \tag{1}$$

where $\log Y_{i,c,t}$ is the outcome variable for individual i in county c at year t. Immigrant_i indicates European nativity, Slaveholder_i is a time-invariant indicator based on household slaveholding status in 1850, and Post_t equals one for the year 1870. The coefficient of interest is β_3, which captures the differential occupational change experienced by immigrant slaveholders relative to three implicit counterfactuals: immigrant non-slaveholders, native slaveholders, and native non-slaveholders. Intuitively, β_3 isolates the component of occupational adjustment specific to being both an immigrant and a former slaveholder, net of general post-war trends for immigrants (β_1) and general effects of losing enslaved property (β_2).

All specifications include individual fixed effects μ_i, ensuring that identification comes from within-person changes in occupational standing rather than compositional shifts or time-invariant unobservables such as ability, family background, or entrepreneurial skill. County-by-year fixed effects $\theta_{c,t}$ absorb all time-varying county-level shocks, including heterogeneous war damage, reconstruction policies, labor-market disruptions, and changes in local economic conditions. The vector $X_{i,t}$ includes baseline covariates (education, wealth, and skills) interacted with year fixed effects, allowing returns to observable endowments to evolve flexibly over time. This ensures that β_3 does not reflect differential returns to human capital or sectoral exposure to emancipation. Controlling for baseline education interacted with year fixed effects is crucial because more educated individuals may adjust differently to large institutional shocks. If schooling facilitated adaptation to new labor-market conditions after emancipation—through greater literacy, administrative skills, or access to white-collar networks—then immigrant slaveholders could experience distinct occupational trajectories simply because of differential returns to education rather than status loss. Allowing the return to education to vary over time ensures that identification does not rely on the assumption that human capital was rewarded similarly before and after 1865. Similarly, interacting baseline

wealth with year fixed effects ensures that comparisons are drawn between individuals with comparable pre-war economic resources. Slaveholders were, by construction, wealthier on average, and wealthier individuals may have been better positioned to buffer post-war shocks, transition into alternative sectors, or leverage financial capital to maintain occupational standing. By flexibly absorbing time-varying returns to wealth, the specification effectively compares immigrant and native slaveholders with similar pre-emancipation asset positions, isolating the immigrant-specific component of post-1865 adjustment. Finally, controlling for baseline industry interacted with year fixed effects addresses the possibility that different sectors were differentially exposed to emancipation and wartime disruption. Industries more reliant on enslaved labor, export markets, or plantation linkages may have experienced sharper post-war contractions. Allowing industry-specific trends to evolve over time ensures that β_3 does not capture mechanical sectoral reallocation or differential industry shocks, but instead reflects within-industry changes in occupational standing attributable to the loss of slaveholding as a social credential. Standard errors are two-way clustered at the county and baseline-industry levels to account for correlated shocks within geographic and sectoral units.

4.1 Identifying Assumption and Validation

The identifying assumption is that, absent emancipation, the difference in occupational trajectories between immigrant slaveholders and immigrant non-slaveholders would have evolved parallel to the analogous difference between native slaveholders and native non-slaveholders. Formally:

$$E[\Delta Y_{i,t}^{\text{Imm,SH}} - \Delta Y_{i,t}^{\text{Imm,Non-SH}}] = E[\Delta Y_{i,t}^{\text{Native,SH}} - \Delta Y_{i,t}^{\text{Native,Non-SH}}] \quad \text{for } t < 1865, \tag{2}$$

where $\Delta Y_{i,t}$ denotes the change in occupational standing from baseline. This assumption is weaker than the standard parallel trends assumption in a single difference-in-differences design: it allows group-specific trends to differ across nativity and slaveholding status, provided those differences remain stable over time. Crucially, it does not require that immigrant and native slaveholders follow identical occupational paths, only that the gap between slaveholders and non-slaveholders within each nativity group evolves similarly.

I assess this assumption using event-study specifications that estimate year-specific treatment effects relative to the 1860 baseline:

$$\begin{aligned} Y_{i,c,t} = & \sum_{\tau \neq 1860} \delta_\tau (\text{Immigrant}_i \times \text{Slaveholder}_i \times \mathbf{1}[t = \tau]) \\ & + \sum_{\tau \neq 1860} \gamma_\tau (\text{Immigrant}_i \times \mathbf{1}[t = \tau]) + \sum_{\tau \neq 1860} \lambda_\tau (\text{Slaveholder}_i \times \mathbf{1}[t = \tau]) \\ & + \Gamma X_{i,t} + \mu_i + \theta_{c,t} + \varepsilon_{i,c,t}. \end{aligned} \tag{3}$$

The coefficients δ_τ trace out the evolution of the triple-difference effect over time. Under the parallel trends assumption, δ_{1850} should be statistically indistinguishable from zero, indicating no

pre-existing differential trend. The coefficient δ_{1870} captures the post-emancipation effect.

Several potential confounds could threaten causal interpretation of β_3. The credibility of the triple-difference design rests on a parallel-trends assumption defined at the level of the difference-in-differences. Specifically, in the absence of emancipation, the evolution of the occupational gap between slaveholders and non-slaveholders would have followed a similar trajectory for immigrants and for the U.S.-born. The identifying variation therefore comes from deviations, after 1865, in the relative return to slaveholding across nativity groups. I assess this assumption using an event-study specification that estimates the dynamic coefficients associated with the triple interaction in each Census wave. The pre-emancipation coefficient for 1850 is small in magnitude and statistically indistinguishable from zero, indicating that prior to abolition the slaveholder premium evolved similarly for immigrants and natives. The differential adjustment emerges only in 1870, consistent with the institutional shock induced by emancipation rather than with pre-existing divergent trends.

Second, emancipation constitutes an abrupt and plausibly exogenous institutional shock. Although the Civil War rendered abolition conceivable, its precise timing, scope of enforcement, and economic implications remained uncertain until the ratification of the Thirteenth Amendment in December 1865. The institutional collapse was not implemented gradually at the state or county level in response to local economic conditions; rather, it was imposed uniformly across the former Confederacy through federal authority. This feature limits concerns that the timing of abolition was endogenously correlated with immigrant labor market trajectories.

A third key identification challenge concerns selection along two margins: selection into migration (including destination choice within the United States) and selection into slaveholding conditional on residence in the South. Immigrants who settled in slaveholding regions—and particularly those who acquired enslaved labor—may have differed systematically from both native-born residents and non-slaveholding immigrants in unobserved characteristics such as initial wealth, risk preferences, or entrepreneurial ability. The empirical specification addresses these concerns through the triple-difference framework. By differencing across nativity, slaveholding status, and time, the model removes all time-invariant differences between immigrants and natives, as well as between slaveholders and non-slaveholders. Identification therefore relies on differential changes after 1865, rather than on cross-sectional gaps that could reflect pre-existing selection. In addition, I estimate analogous specifications for the second generation. Because their occupational trajectories do not reflect a contemporaneous migration decision or direct selection into slaveholding, the second-generation results provide an additional test that the estimated effects are not driven by first-generation selection.

A fourth concern relates to compositional changes across the four nativity-by-slaveholding cells. If immigrant slaveholders experienced differential mortality, selective out-migration, or linkage attrition between 1860 and 1870, the estimated effect could reflect changes in sample composition rather than within-individual occupational adjustment. The analysis mitigates this concern by relying on a balanced linked panel that tracks the same working individuals across the 1850, 1860, and 1870 Censuses. By construction, the sample excludes individuals who exit the labor force,

are retired, or report no occupation, thereby holding constant labor force attachment. Because identification derives from within-person changes over time, time-invariant unobserved characteristics—such as entrepreneurial ability, risk preferences, or family background—are differenced out. Moreover, pre-treatment observables are balanced across the four cells, and linkage rates do not vary systematically by nativity and slaveholding status.

An additional threat would arise if war-related shocks differentially affected immigrant slaveholders within counties—for example, if they were disproportionately concentrated in more export-oriented areas, heavily damaged plantation regions, or locations subject to distinct Reconstruction policies. The specification includes county-by-year fixed effects, which absorb all time-varying local shocks common to residents of a county in a given year. The triple-difference structure further differences out any shock common to all slaveholders or all immigrants. Residual bias would require shocks that specifically targeted immigrant slaveholders relative to native slaveholders within the same county. Such a pattern is difficult to reconcile with the historical record and is further weakened by evidence that similar penalties arise in the second generation, a distinct sample not directly exposed to wartime destruction.

5 Main Results

Table 1 presents the baseline triple-difference estimates of the effect of emancipation on immigrant slaveholders' occupations. The specification follows Equation (1), regressing log occupational income scores on the interactions of immigrant status, slaveholding status, and a post-1865 indicator. Column (1) presents the baseline specification without industry controls. The triple interaction is negative and statistically significant at the 5 percent level ($\beta_3 = -0.055$, SE $= 0.027$). Column (2) adds baseline industry-by-year fixed effects, allowing each sector to evolve on its own trajectory post-1865. This addresses concerns that the result reflects differential sectoral exposure to the loss of enslaved labor or post-war economic reorganization. The coefficient remains negative and similar in magnitude ($\beta_3 = -0.062$, SE $= 0.027$), indicating that the penalty is not driven by industry-specific trends. The stability of the estimate across specifications suggests that the effect operates within industries rather than through sectoral reallocation. Two features of the lower-order coefficients are notable. First, β_1 is positive and statistically significant (+0.067, in Column 2), indicating that European immigrants on average experienced occupational upgrading after 1865. This pattern is inconsistent with a story of generalized anti-immigrant discrimination following the Civil War. Second, β_2 is also positive and significant (+0.034, in Column 2), indicating that slaveholders on average maintained or improved their occupational standing after emancipation. This rules out a simple wealth-shock explanation: if the loss of enslaved property mechanically depressed occupational outcomes, one would expect $\beta_2 < 0$. Instead, the post-1865 penalty is specific to the interaction of immigrant status and slaveholding. This suggests a mechanism in which slaveholding functioned as a status credential that mattered differentially for immigrants versus natives. Robustness checks are reported in Table 10 in the Appendix. Column (1) displays the results when

clustering the standard errors at the county level only. Column (2) shows that the main results are robust to an alternative classification of farmers, following the occupational coding proposed by Collins and Zimran (2023). This addresses concerns that misclassification of agricultural occupations may mechanically drive the estimated effects. A second concern is that immigrants from different countries of origin may have followed systematically different post-emancipation trajectories. Column (3) addresses this by including birthplace-by-year fixed effects, allowing occupational paths to vary flexibly across immigrant groups. The estimated triple-difference coefficient remains stable, indicating that the results are not driven by nationality-specific trends. Finally, Column (4) restricts the treatment definition by capping the number of enslaved persons owned by a household at one. The persistence of the effect in this specification indicates that the results are not driven by large slaveholders and do not rely on differential exposure to enslaved labor as a productive input.

To assess whether the baseline estimates reflect a causal effect of mancipation rather than pre-existing differential trends, I estimate a dynamic event-study specification. The specification normalizes coefficients to zero in 1860, the year immediately prior to the Civil War. The event-study design allows me to test for pre-trends and to visualize the timing of the treatment effect. Figure 9 plots the estimated coefficients on $\text{Immigrant}_i \times \text{Slaveholder}_i \times \text{Year}_t$ for each census year. The key identifying assumption needed for the triple diff-in-diff setting is supported by the data: the coefficient in 1850 is small and statistically indistinguishable from zero.

A natural concern with any design exploiting a single post-treatment observation is that the 1860–1870 decade bundles multiple concurrent shocks alongside emancipation itself — namely, the Civil War, Reconstruction loyalty regimes, political re-sorting of outsider elites, and the collapse of slave-backed credit markets. While county-by-year fixed effects absorb the geographic dimension of these shocks, they cannot rule out within-county institutional variation that differentially affected immigrant slaveholders through channels unrelated to credential loss. I address this concern through three complementary pieces of evidence. First, and most directly, the intergenerational analysis (Section 7) shows that children of immigrant slaveholders — who held no political record, had not participated in Confederate activities, and entered the labor market as young adults facing post-war conditions — exhibit occupational penalties indistinguishable from those of their parents. Reconstruction loyalty regimes and political enforcement operated on individuals with documented political histories; they did not extend to children who had made no migration or slaveholding decisions of their own. The intergenerational transmission is therefore inconsistent with any concurrent shock that targeted the first generation on the basis of wartime conduct, and instead points to a mechanism operating through inherited social standing. Second, the geographic mobility analysis (Section 6.4) reveals that the penalty is entirely absent among stayers (individuals who remained in the same county throughout 1850–1870) and is concentrated exclusively among immigrants who relocated after emancipation. This pattern directly contradicts the Reconstruction-shock interpretation: loyalty regimes and political enforcement were locally administered by officials who knew the wartime records of long-term residents, and should therefore have penalized stayers more heavily than movers arriving in new communities with no established local political record. The mover

penalty instead aligns with the credential-loss story, in which an immigrant arriving in an unfamiliar community after abolition could no longer present the institutional signal that had previously established their standing. Third, the comparison between European immigrants and Northern-born migrants (Section 8.1) weakens the political-targeting account: Northern migrants, who could more plausibly claim Union loyalty credentials by virtue of their birth, exhibit a comparable post-emancipation penalty, suggesting that the mechanism does not operate through differential exposure to political enforcement against Confederate sympathizers.

Lastly, it is important to note that these are average effects. Section 6 will show that the penalty varied substantially across subgroups, with larger declines among long-standing slaveholders, and among immigrants who migrated after emancipation. The baseline estimates thus mask important heterogeneity that sharpens the interpretation of the mechanism.

6 Mechanism

Building on the patterns documented in the previous section, where European immigrant slaveholders experienced a relative occupational downgrading following emancipation, this section investigates the mechanisms underlying that decline. Two possibilities are particularly salient. First, some immigrant slaveholders may have left a given occupational category altogether, reflecting a reallocation at the extensive margin[9]. Second, among those who remained in positions within a given broader occupational category, there may have been a systematic downward re-ranking, consistent with an intensive-margin adjustment. To assess these channels, I proceed in two steps. First, I analyze occupational sorting across broad occupational groups (white-collar, craft, operative, and unskilled work), capturing whether immigrant slaveholders were able to enter or remain in prestigious occupational categories after emancipation. Second, I examine outcomes within each category by restricting the sample to individuals employed in a given group and estimating regressions of occupational income status on the same triple-differences specification. This double approach allows me to disentangle whether abolition disrupted immigrants' occupational mobility primarily through sorting across categories (extensive margin) or through downward shifts within categories (intensive margin).

6.1 Sorting Across Occupations

To understand whether this penalty operated through reallocation across broad occupational categories, I examine the probability of working in each of four groups: white-collar, craft, operative, and unskilled. Table 2 reports triple-difference estimates for the likelihood of employment in each category. The results show no systematic sorting across occupational categories for immigrant slaveholders. Column (1) reports the white-collar probability: the triple interaction is negative but small and statistically insignificant. Similarly, the triple interactions for craft (Column 2), operative (Column 3), and unskilled (Column 4) are all close to zero and not statistically significant. This absence

[9]It could not be exiting the observable labor force, since the sample is conditional on working.

of across-category reallocation has an important implication: the occupational penalty documented in the baseline results did not operate by pushing immigrant slaveholders out of white-collar work and into other broader occupational categories. Instead, it must reflect downgrading within occupational categories; a pattern examined in the next subsection. The lower-order interaction terms are informative about general post-1865 trends. β_1 in Column (1) is positive and statistically significant (+0.032), indicating that European immigrants in general became more likely to occupy white-collar positions after 1865. Similarly, β_2 is positive (+0.027), indicating that slaveholding households (regardless of nativity) retained a white-collar advantage post-emancipation. The fact that the triple interaction is not significantly negative despite these positive lower-order terms suggests that immigrant slaveholders participated in the general upgrading into white-collar work but did not fall behind in terms of occupational category membership. The penalty thus should operate through a different channel: not exclusion from white-collar categories but downgrading to lower-prestige positions within those categories. The absence of compensating increases in craft, operative, or unskilled work reinforces this interpretation. If immigrant slaveholders had been pushed out of elite positions, one would expect to see them reallocating to manual or semi-skilled occupations. The fact that one observes neither significant declines in white-collar membership nor increases in lower-status categories suggests that the penalty operated on the intensive margin, rather than on the extensive margin of occupational category membership.

6.2 Within-Occupation Outcomes

The previous analysis showed no evidence that immigrant slaveholders systematically re-sorted across broad occupational categories after emancipation. In particular, the triple interaction is statistically indistinguishable from zero in all specifications examining sorting into white-collar, craft, operative, or unskilled occupations (Table 2). This sub-section focuses on the intensive margin: whether immigrant slaveholders who remained within a given occupational category experienced downgrading within that category. To examine within-occupation outcomes, I re-estimate Equation (1) separately by occupational class. This design isolates whether immigrant slaveholders were systematically sorted into lower-prestige positions within occupational categories, conditional on remaining employed in those categories. Table 3 reports the results. The evidence is concentrated entirely in white-collar occupations (Column 1). The triple interaction is negative and statistically significant at the 1 percent level ($\beta_3 = -0.055$, SE = 0.021). The lower-order interactions further clarify the mechanism. β_1 is positive and statistically significant (+0.046), indicating that European immigrants, on average, experienced upgrading within white-collar occupations after 1865. By contrast, β_2 is close to zero, implying that native slaveholders maintained their within-category standing. The negative triple interaction therefore captures a specific penalty: immigrant slaveholders not only failed to benefit from the general upgrading experienced by other immigrants, but were systematically relegated to lower-prestige white-collar roles. Columns (2)–(4) show no comparable effects in craft, operative, or unskilled occupations. In these categories, the triple interaction is small in magnitude and statistically indistinguishable from zero. Overall, there is no evidence of

systematic within-category downgrading outside the white-collar sector. The concentration of the intensive-margin penalty in white-collar occupations is difficult to reconcile with a productivity-based explanation. White-collar activities were the least intensive in enslaved labor. If the observed decline reflected the loss of enslaved workers as productive inputs, one would expect the strongest effects in occupations more directly reliant on such labor, such as artisanal crafts. Instead, the penalty appears precisely in the segment of the labor market where reputational capital, social networks, and visible markers of legitimacy are most salient.

Taken together, the within-occupation results point to a credential-based mechanism operating entirely at the intensive margin. Emancipation did not induce immigrant slaveholders to exit white-collar employment or re-sort across broad occupational categories. Rather, conditional on remaining employed and within the same occupational class, immigrant slaveholders experienced a sharp deterioration in their relative standing within the white-collar hierarchy.

Decomposing White-Collar Occupations by Task Content

To further unpack which segment of white-collar employment drives the intensive-margin penalty, I exploit the task-based classification proposed by Song et al. (2020), which partitions occupations into two categories: professional/managerial (pm) and routine. This classification allows me to move beyond broad occupational titles and examine whether the downgrading is concentrated in positions characterized by higher autonomy, authority, and non-routine cognitive content; i.e., the dimensions most closely associated with elite status and social recognition. The results are striking and sharply focused. Column (1) of Table 4 shows that the triple interaction is large, negative, and highly statistically significant in professional and managerial occupations. By contrast, the triple interaction is positive in routine occupations (Column 2). In other words, immigrant slaveholders are not exiting white-collar employment broadly defined; rather, they are reallocated away from the highest-status, authority-intensive positions within that sector and toward lower-autonomy routine roles. The lower-order interactions reinforce this interpretation. The coefficient on Immigrant $\times$ Post-1865 is strongly positive in professional/managerial occupations, indicating that immigrants as a whole upgraded into elite cognitive positions after emancipation. Native slaveholders instead did not experience upgrading within these roles. The negative triple interaction thus captures a specific relative penalty; immigrant slaveholders were disproportionately displaced from precisely those occupations in which reputational capital and social legitimacy are most valuable. Morevoer, the positive triple interaction in routine occupations further suggests reallocation within white-collar employment rather than wholesale occupational exit: immigrant slaveholders appear to move down the internal hierarchy of cognitive work, from positions of discretion and leadership toward more standardized, rule-bound tasks. This task-based evidence sharpens the status-loss interpretation. Emancipation did not generate generalized occupational decline; it specifically undermined immigrant slaveholders' access to elite, authority-bearing roles. The erosion of slaveholding as a social credential appears to have reduced their capacity to occupy positions requiring recognition, trust, and gatekeeper endorsement.

6.3 Social Networks as Moderating Factors

A natural alternative explanation is that the observed penalties operate through disruption of social networks rather than loss of belonging credentials. Networks could matter through multiple channels: co-ethnic ties may provide job referrals, professional networks may facilitate access to white-collar positions, and local racial composition may shape labor market competition. To investigate this possibility, I exploit cross-county variation in four dimensions of network environments: the share of European immigrants, the share of white-collar workers, the share of Black residents in 1870 (which proxies for antebellum slave intensity), and intermarriage status.

Table 5 reports triple-difference estimates separately across these dimensions. The results reveal patterns inconsistent with social networks serving as the primary mechanism generating the post-1865 penalties. First, immigrant network density plays no role: the triple-difference coefficient is statistically insignificant in both high and low immigrant-share counties (Columns 1–2), with comparable point estimates. This clean null result rules out co-ethnic embeddedness as a driver of occupational decline and suggests that access to immigrant social capital did not protect former slaveholders from the consequences of losing the institutional marker.

Second, the penalty is more pronounced in contexts where the institutionally codified norm had been most central to local social organization. The triple-difference coefficient is significant in counties with sparse white-collar employment and high Black population shares—characteristics of rural plantation economies where slavery structured social hierarchies most intensively (Columns 3, 6). In such settings, slaveholding constituted the dominant signal of adherence to local racial norms, and alternative markers of cultural alignment were scarce. When emancipation removed this credential, immigrant elites who lacked embedded kinship networks faced sharper barriers to demonstrating norm compliance. By contrast, in counties with dense white-collar employment or low slave intensity—where economic transactions were less dependent on rigid social hierarchies and alternative markers of belonging were more readily available—the penalty is attenuated or insignificant (Columns 4, 5). This geographic heterogeneity indicates that the salience of institutional credentials varies with the structure of local norm enforcement: where cultural alignment could be signaled through diverse mechanisms, the loss of any single marker imposed smaller costs.

Third, kinship networks acquired through intermarriage provided an alternative pathway for demonstrating cultural integration. Marriage into native families embeds immigrants within established kinship networks, facilitating reputational transfer and access to information channels that operate independently of occupational or ethnic networks. Columns (7) and (8) examine this margin by splitting the sample according to whether the immigrant married a U.S.-born spouse. The results reveal a sharp contrast. Among intermarried immigrants (Column 7), the triple-difference coefficient is negative but statistically insignificant. By contrast, among those who did not intermarry (Column 8), the penalty is economically meaningful and statistically significant (-0.064, $p < 0.05$). This pattern is consistent with the interpretation that kinship-based integration substitutes is an alternative credibility signal: when immigrants possess an alternative channel of social recognition, the loss of slaveholding does not translate into occupational downgrading. Two caveats

merit consideration. First, intermarriage may capture different mechanisms for different groups. For U.S.-born individuals, marrying an immigrant could reflect openness toward the outgroup rather than a strategy of assimilation, since natives are already socially embedded. The moderating role of intermarriage therefore speaks specifically to immigrant incorporation rather than to general network density. Second, the intermarried subsample is substantially smaller (2,874 observations versus 63,759), limiting statistical power and cautioning against over-interpretation of the null effect in Column (7). Nonetheless, the asymmetry across the two columns points toward the fact that where alternative kinship-based credentials exist, the occupational penalty dissipates; where immigrants lack such ties, the erosion of slaveholding as a marker of legitimacy produces persistent downgrading. This pattern reinforces that what mattered was not slaveholding per se, but access to mechanisms—whether institutional or kinship-based—that credibly signaled adherence to local cultural norms.

Taken together, these findings indicate that social networks condition the expression of norm-based integration but do not constitute an independent mechanism generating the observed penalties. The absence of effects in immigrant network environments, combined with penalties concentrated where institutional credentials were most central to signaling cultural alignment and attenuated where alternative mechanisms existed, points to loss of a norm-signaling device as the first-order force. Networks serve as moderators: kinship ties acquired through marriage can substitute for institutional markers, and dense economic environments provide alternative pathways for demonstrating cultural integration. However, for immigrants who lacked these substitutes and operated in settings where slaveholding had been the primary credential of cultural belonging, emancipation removed their principal means of signaling alignment with local norms, imposing penalties that persisted even as economic fundamentals recovered.

6.4 Geographic Mobility and the Portability of Status

A central implication of the credential-loss interpretation is that the value of slaveholding as a marker of belonging was locally anchored. If slaveholding facilitated immigrants' integration by signaling alignment with community norms and enabling access to local elite networks, then its value should have been tied to recognition within specific geographic communities. To test this prediction, I examine whether the post-emancipation penalty varied by geographic mobility. Table 6 reports triple-difference estimates separately for four groups: individuals who remained in the same county throughout 1850–1870 (stayers, Column 1), all individuals who changed county at any point (movers, Column 2), those who moved between 1850 and 1860 while still slaveholders (pre-emancipation movers, Column 3), and those who moved between 1860 and 1870 after emancipation (post-emancipation movers, Column 4). This disaggregation allows me to assess whether the timing of migration mediates the effect of losing slaveholding status. The results reveal a striking pattern. Among stayers (Column 1), the triple interaction is small and statistically insignificant, indicating that immigrant slaveholders who remained in the same county experienced no detectable occupational penalty relative to the comparison groups. This null result is consistent with the in-

terpretation that locally embedded social capital continued to protect these individuals even after the formal institution of slavery was abolished. In other words, their standing within the local hierarchy remained sufficiently intact to sustain access to prestigious occupations. By contrast, among movers (Column 2), the triple interaction is negative and statistically significant. This aggregate effect, however, masks important temporal heterogeneity. Column 3 restricts the sample to individuals who migrated before 1865; that is, while they were still slaveholders and while the institution remained legally and socially operative. For this group, the coefficient is negative but imprecise, suggesting no statistically detectable penalty. Critically, these pre-emancipation movers carried the visible credential of slaveholding into their new communities, where it could still function as a signal of wealth, ideology, and elite alignment. The effect is concentrated entirely among post-emancipation movers (Column 4), who relocated after slavery was abolished. For this group, the triple interaction is large, negative, and highly significant. The magnitude corresponds to a 17.8 percent decline in occupational income scores. These individuals moved into new counties without the ability to credibly signal their prior social standing. The marker that had previously facilitated acceptance was no longer available, and without long-standing local ties or inherited reputations, they faced barriers to accessing elite occupations in unfamiliar communities.

Several features of this heterogeneity strengthen the status-loss interpretation. First, the null result for stayers is inconsistent with wealth or productivity shocks, which should affect individuals regardless of migration. If the penalty reflected the loss of enslaved labor as a productive input, stayers should also experience declines. Second, the contrast between pre- and post-emancipation movers is difficult to reconcile with selection into migration. If movers were negatively selected on unobservables, one would expect both groups to decline. Instead, only those who moved after losing the status badge experience penalties. Third, the spatial contingency aligns with the broader evidence on the local and time-dependent nature of status documented in the next subsection: long-standing slaveholders (those who owned slaves in both 1850 and 1860) experienced larger penalties than recent slaveholders, reinforcing that what mattered was not the acquisition of slaveholding but the preservation of entrenched, locally validated social recognition.

It is important to acknowledge that migration decisions are endogenous. Individuals who moved after 1865 may have faced idiosyncratic shocks that simultaneously compelled relocation and depressed occupational outcomes. However, several considerations mitigate this concern. First, the pre-emancipation movers provide a within-mover comparison: individuals who chose to migrate in earlier periods (and thus are plausibly similarly selected on unobservables) do not exhibit comparable penalties. Second, the baseline specification includes individual fixed effects, county-by-year fixed effects, and baseline industry-by-year fixed effects, absorbing time-invariant individual characteristics, county-specific trajectories, and sectoral trends. Third, if individuals who moved after emancipation were negatively selected due to economic distress, this would tend to generate occupational declines among all movers, regardless of migration timing. The fact that the penalty is concentrated exclusively among post-emancipation movers therefore works against an interpretation based on negative selection.

These results support the interpretation that slaveholding functioned as a locally recognized credential whose value depended on community-specific recognition. Stayers retained the social capital embedded in long-standing relationships; movers who relocated while the institution was still operative could re-establish legitimacy in new settings. However, movers who relocated after emancipation entered communities where their prior standing was unknown and could no longer be signaled. This spatial contingency is consistent with a mechanism operating through social acceptance and elite gatekeeping rather than through the direct economic value of enslaved labor.

To further assess whether the penalty scales with the geographic scope of relocation, Table 7 replicates the analysis distinguishing movers across federal states from those who remain within their original state. If the loss of status primarily reflects the difficulty of re-establishing reputation in more distant or institutionally distinct environments, one would expect larger penalties among individuals who cross state borders, where legal regimes, political equilibria, and elite networks were more heterogeneous. The table reports triple-difference estimates separately for stayers, all movers, movers prior to 1865, and movers after 1865, now defined at the state level rather than the county level. The results provide little support for a distance-based gradient. While the aggregate mover sample (Column 2) displays a negative triple interaction, the effect is imprecisely estimated and attenuated relative to the county-level results. Most notably, the penalty is not concentrated among individuals who cross state borders after emancipation (Column 4). The triple interaction in this subsample is small and statistically indistinguishable from zero. Instead, the economically meaningful and statistically significant coefficient emerges among individuals who change county but remain within their original federal state. In other words, the occupational downgrading documented in Table 6 is not amplified by crossing state lines. The relevant margin of disruption appears to be the loss of local embeddedness at the county level rather than exposure to a broader or more distant institutional environment. This pattern reinforces the interpretation that slaveholding operated as a local marker of belonging. Elite recognition and access to high-prestige occupations were mediated by county-specific networks, reputational capital, and face-to-face interactions rather than by state-wide identity. Crossing a county boundary sufficed to sever these ties; crossing a state boundary did not impose an additional penalty. The absence of a state-distance gradient further weakens explanations based on large-scale economic dislocation or institutional heterogeneity across states, and instead underscores the localized nature of the coordination mechanism at the heart of the analysis.

6.5 Type of Slaveholding

Table 8 explores whether the occupational penalties documented in the baseline results vary across different types of slaveholding households. I distinguish three groups: long-standing slaveholders (households owning slaves in both 1850 and 1860), recent slaveholders (households that acquired slaves between 1850 and 1860), and previous slaveholders (households that held slaves in 1850 but not in 1860). This classification allows me to disentangle long-standing social embeddedness in the institution of slavery from more opportunistic or transient participation. Column (1) focuses

on old slaveholders. The triple interaction coefficient for European immigrant slaveholders after 1865 indicates a statistically significant decline in the occupational ranking relative to the US-born comparison group. In contrast, β_1 and β_2 are positive, suggesting that the observed downgrading is concentrated in the interaction: only immigrant slaveholders with a long-standing history of slaveholding experienced a relative loss. This suggests that abolition primarily disrupted the occupational trajectories of immigrants whose social standing and community embeddedness were anchored in entrenched slaveholding. Column (2) focuses on new slaveholders. Here, the triple interaction is no longer statistically significant. The attenuation of the effect implies that households acquiring slaves shortly before the Civil War did not leverage slaveholding as a durable status signal. Consequently, the abolition shock did not materially affect their relative placement within occupational hierarchies, consistent with the interpretation that social capital, rather than economic capital, drove the baseline penalties. Column (3) considers previously slaveholding households, i.e., those that had exited the institution before 1860. The triple interaction is statistically indistinguishable from zero. This reinforces the inference that households which had already divested from slaveholding were insulated from post-emancipation occupational penalties, having reoriented their social and economic strategies away from slavery-dependent prestige.

7 Addressing Selection into Migration and Slaveholding

Two sources of endogenous selection could threaten identification: (i) the decision to migrate and where to settle, and (ii) the decision to become a slaveholder. If immigrants who became slaveholders were positively selected on unobservables that also predicted post-war occupational resilience, the estimated penalty would understate the true effect. Conversely, if they were negatively selected, the penalty could be overstated. To assess whether selection drives the results, I rely on children of immigrant and native households who made neither migration nor slaveholding decisions themselves. The second-generation sample comprises males classified as "child" in the 1850 Census who later became independent heads of household. These individuals did not choose whether to migrate, where to settle, or whether their parents would own slaves. Estimating a triple-difference specification for this group provides a direct test: if the post-1865 penalty persists among children whose parents were immigrant slaveholders, it cannot be attributed to the endogenous decisions of the migrants themselves. Figure 10 presents event-study estimates for the second generation. The specification mirrors the baseline triple-difference design, interacting second-generation immigrant status and parental slaveholding in 1850. Several features of the results are notable. First, pretrends are flat: the triple-interaction coefficient for 1850 is small and statistically indistinguishable from zero, indicating that the gap between children of immigrant slaveholders and non-slaveholders evolved similarly to the analogous gap among children of natives prior to emancipation. Second, the post-1865 penalty emerges also in this case: the triple-interaction coefficient in 1870 is negative and statistically significant, indicating that children of immigrant slaveholders experienced occupational penalties similar to those of their parents. This contemporaneous drop is inconsistent with

several alternative explanations. If the first-generation penalty reflected selection into migration or slaveholding on unobservables correlated with post-war outcomes, those unobservables would need to transmit mechanically to children. I believe this is an implausible channel. Moreover, if the mechanism were purely economic, children of native slaveholders should exhibit similar declines, as their parents suffered equivalent property losses. They do not. The asymmetry suggests that what was transmitted across generations was not economic resources but social recognition: parents' capacity to confer status mattered for children's labor-market entry, and this capacity evaporated when the public, inheritable signal of slaveholding disappeared.

8 Additional Evidence

The baseline results document a differential post-emancipation trajectory consistent with the loss of a status credential. This section provides additional evidence testing the mechanism more directly. I proceed in two steps. First, I vary the comparison group to assess whether the penalty reflects foreign birth per se or outsider status more broadly. Second, I examine complementary cross-sectional outcomes that capture distinct domains of social integration.

8.1 Alternative Reference Groups

The baseline specification compares European immigrants to native-born individuals, conflating two dimensions: foreign birth and geographic outsider status. This section disentangles these by exploiting an alternative margin of social distance: U.S.-born individuals who migrated from Northern states to the South.

Northern-born migrants share key characteristics with European immigrants. Both groups are geographic outsiders who lack deep roots in Southern communities, but differ in a crucial dimension: Northern migrants possess U.S. citizenship and cultural familiarity, making them less reliant on visible status markers to signal legitimacy. If the baseline results reflect pure wealth shocks or disruptions to slave-dependent production, Northern migrants should experience similar post-1865 penalties. If instead the mechanism operates through contested social acceptance, Northern migrants may also suffer occupational losses, but the magnitude should depend on how slaveholding functioned as a credential for outsiders more broadly. Figure 11 presents event-study estimates using Northern-born migrants as the treated group and Southern natives as the control. The pre-1865 trends are parallel, validating the identification assumption. Following emancipation, Northern migrant slaveholders experience a sharp occupational decline relative to their counterparts, mirroring the pattern observed for European immigrants. This convergence across two distinct outsider groups—one foreign-born, one domestic—supports the interpretation that slaveholding served as a transferable status credential for individuals lacking established local networks, regardless of national origin. Figure 12 provides a complementary test by comparing European immigrants directly to Northern migrants, treating both as outsider groups with similar social distance from Southern elites. The event study reveals no differential effect: the triple-difference coefficients remain statis-

tically indistinguishable from zero throughout the post-emancipation period. This null result is not evidence against the status mechanism but rather confirms it. When both groups rely symmetrically on external markers of legitimacy, the removal of slaveholding imposes comparable penalties, eliminating any differential trajectory. The absence of divergence when comparing "two outsiders" rules out group-specific explanations based on discrimination, cultural distance, or human capital composition. These results demonstrate that the baseline findings reflect variation in social embeddedness rather than artifacts of the specific control group. Slaveholding conferred occupational advantages to multiple categories of outsiders, and its abolition imposed penalties on those whose integration depended on this credential. The asymmetry emerges only when comparing outsiders to insiders (and not when comparing across different types of outsiders) confirming that status operated as a locally recognized signal of belonging rather than a group-specific attribute.

8.2 Cross-Sectional Evidence

The panel analysis establishes that immigrant slaveholders experienced occupational downgrading after emancipation, but it cannot directly observe the mechanisms through which status operated. To provide complementary evidence on how slaveholding facilitated social integration, I exploit cross-sectional variation. Specifically, I examine two outcomes that reveal different dimensions of assimilation: intermarriage with native-born spouses at baseline and employment of domestic servants in the 1870 Census. The former captures acceptance into local kinship networks—a form of social capital that signals embeddedness and legitimacy. The latter captures consumption-based status signaling, which depends on purchasing power rather than community recognition. If slaveholding functioned primarily as a credential granting access to locally governed institutions, its effects should vary across these domains: immigrant ex-slaveholders should exhibit elevated intermarriage rates (reflecting past integration into elite networks) but symmetric use of domestic servants relative to native ex-slaveholders (reflecting wealth-based signaling that does not require local certification). This cross-sectional analysis thus distinguishes between social capital and conspicuous consumption.

Intermarriage

To provide direct evidence that slaveholding facilitated social integration for European immigrants, I examine marriage patterns as revealed in the 1850 Census. Column (1) of Table 9 reports estimates from a regression in which the dependent variable equals one if an individual is mix-married, and zero otherwise. This outcome captures a dimension of assimilation distinct from labor market success: acceptance into native kinship networks. The results are in line with the ones above. European immigrants without slaveholding backgrounds exhibit substantially higher rates of intermarriage with natives, reflecting a well-documented pattern of assimilation through marriage. Among the native-born, slaveholding is weakly associated with lower intermarriage rates, consistent with elite endogamy. The interaction term, however, is large, positive, and highly significant. Immigrant slaveholders were nearly 40 percentage points more likely to marry native-

born spouses than their non-slaveholding immigrant counterparts, conditional on observables. This pattern cannot be explained by wealth or human capital: the specification controls for initial wealth, education, age, and industry, as well as county fixed effects. Instead, it suggests that slaveholding granted immigrants access to marriage markets that would otherwise remain closed. In the antebellum South, marriage alliances served as mechanisms of social certification, and unions between immigrant men and native-born women signaled the immigrant's acceptance into local elite networks. Slaveholding appears to have operated as a credential that rendered such matches socially permissible, effectively bridging the gap between outsider status and insider kinship structures. The magnitudes are consistent with a credential-based interpretation. The interaction effect is four times larger than the baseline immigrant effect and operates in the opposite direction, implying that slaveholding more than offset the social distance associated with foreign birth. This finding complements the occupational results: just as slaveholding opened access to elite jobs, it also facilitated integration into native family networks; i.e. a form of social capital that native-born elites could maintain after emancipation but immigrant elites could not.

Employing Domestic Servants as a Second-Best Status Signal

After emancipation eliminated slaveholding as a credential, did immigrant ex-slaveholders adopt alternative status markers? Column (2) of Table 9 examines whether employing Black domestic servants in 1870 served as a substitute signal. This outcome is measured using Census records that identify households with live-in domestic workers, the vast majority of whom were recently emancipated freedpeople. The results reveal a sharp contrast to the occupational penalty documented earlier. Former slaveholders were 22 percentage points more likely to employ domestic servants in 1870, reflecting continuity in consumption patterns and a plausible mechanism for signaling prosperity through visible household staff. European immigrants, on average, were 7 percentage points less likely to employ servants, consistent with different cultural norms. Critically, the interaction term is statistically indistinguishable from zero. This null interaction suggests that immigrant ex-slaveholders behaved identically to native-born ex-slaveholders in their propensity to employ domestic workers. Both groups exhibited the same 22 percentage point increase relative to non-slaveholders, suggesting that this form of status signaling was equally accessible to immigrants and natives. The symmetry stands in stark contrast to the labor market, where immigrant ex-slaveholders experienced occupational downgrading while native ex-slaveholders did not. The divergence across domains supports the interpretation that status operated through socially contingent recognition rather than pure purchasing power. Employing domestic servants required wealth but not local certification: any household with sufficient income could hire help. In contrast, accessing elite occupations depended on networks and reputational capital that were locally embedded and difficult for outsiders to maintain without visible markers of belonging. Immigrant ex-slaveholders retained the capacity to signal status through consumption (domestic servants) but lost access to status-dependent positions in the labor market (elite occupations). This asymmetry reinforces the conclusion that emancipation disrupted social credentials along with economic funda-

mentals, imposing penalties only in markets where legitimacy was adjudicated by local gatekeepers.

9 Conclusion

This paper studies how social status shapes immigrant integration by exploiting a sharp historical disruption to a dominant status order: the abolition of slavery in 1865. Using a balanced panel of individuals linked across the 1850–1870 U.S. Censuses, I show that European immigrant slaveholders experienced large and persistent occupational declines after emancipation, while U.S.-born slaveholders and immigrant non-slaveholders did not. The penalty was transmitted across generations, concentrated in white-collar occupations, and driven by individuals who lost the ability to signal prior standing to new communities. The empirical patterns rule out standard economic explanations. The decline does not reflect productivity losses from the disappearance of enslaved labor, since it is strongest in occupations least reliant on such labor. It cannot be explained by wealth destruction, as native slaveholders experienced similar asset losses without comparable occupational consequences. Nor does it reflect generalized discrimination against immigrants, as non-slaveholding immigrants did not experience parallel declines. Instead, the evidence points to a mechanism operating through social recognition and elite gatekeeping. For European immigrants, slaveholding functioned as a locally recognized credential that conferred legitimacy and access to high-status occupations. When that credential disappeared, so did the social acceptance it enabled. The heterogeneity results clarify how status operated. The penalty is largest among long-standing slaveholders, among those who relocated after emancipation. Immigrants who remained embedded in the same local communities retained their standing despite emancipation, while those who moved after 1865 entered new settings unable to credibly signal prior status. These patterns indicate that status was neither portable nor intrinsic. The findings show that immigrant integration depends not only on skills, wealth, or formal rights, but also on access to socially validated credentials that signal belonging. In this setting, slavery operated as a gatekeeping institution that regulated entry into elite occupations. Its abolition did not level the playing field uniformly. Instead, it disproportionately harmed immigrants. The result is an integration paradox: those immigrants who had appeared most assimilated before the war, precisely because they had invested in the dominant status marker, became the most vulnerable once that marker disappeared (see Finseraas and Sønderskov, 2025). More broadly, the analysis highlights that status can be a powerful but fragile mechanism of integration. When access to opportunity is mediated by social recognition rather than productivity alone, the loss of a single credential can trigger lasting exclusion. Immigrant assimilation, in such environments, hinges not only on accumulation but on whether host societies continue to recognize the signals that once conferred legitimacy.

The findings have implications for contemporary immigration policy. While slaveholding is no longer relevant, modern societies continue to rely on visible credentials to assess immigrants' integration: educational degrees, professional certifications, language proficiency, or neighborhood of residence. The mechanism documented here—status as a gatekeeping device—persists in contempo-

rary labor markets through non-recognition of foreign credentials (which signals lack of legitimacy), citizenship status (which marks categorical boundaries), language proficiency (which signals cultural alignment), or residential location (which confers neighborhood-based reputational capital). Policies that fail to recognize foreign credentials, restrict access to credential-conferring institutions, or create categorical markers separating immigrants from natives risk perpetuating exclusion even when immigrants possess comparable skills. Understanding how status signals operate can inform the design of integration policies that address not only economic barriers but also social acceptance.

Several directions for future research emerge. First, richer data on social networks, elite memberships, or contemporary perceptions could provide more direct evidence of status mechanisms. Second, examining other historical episodes where status markers lost value—such as decolonization, regime changes, or credential devaluations—could test the generalizability of these findings. Third, studying how immigrants respond to the loss of status markers (do they invest in alternative credentials? exit certain markets?) would shed light on adaptive strategies.

10 Figures

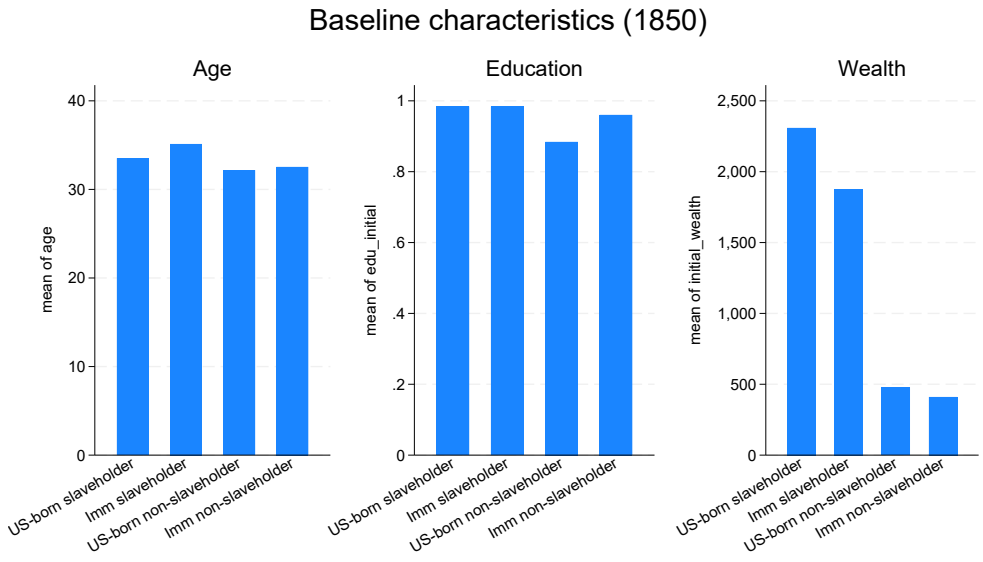


Figure 1: Summary Statistics

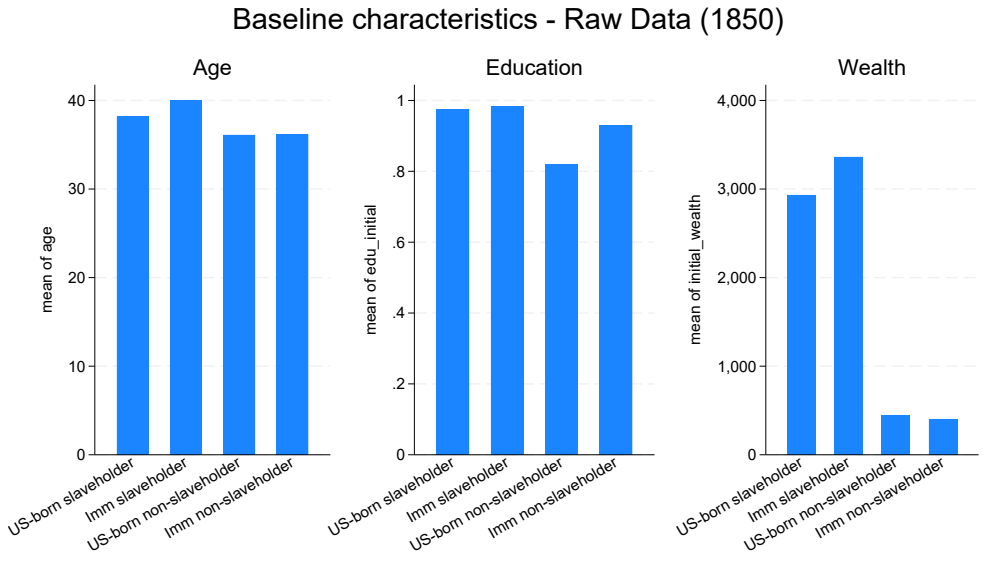


Figure 2: Summary Statistics - Raw Data

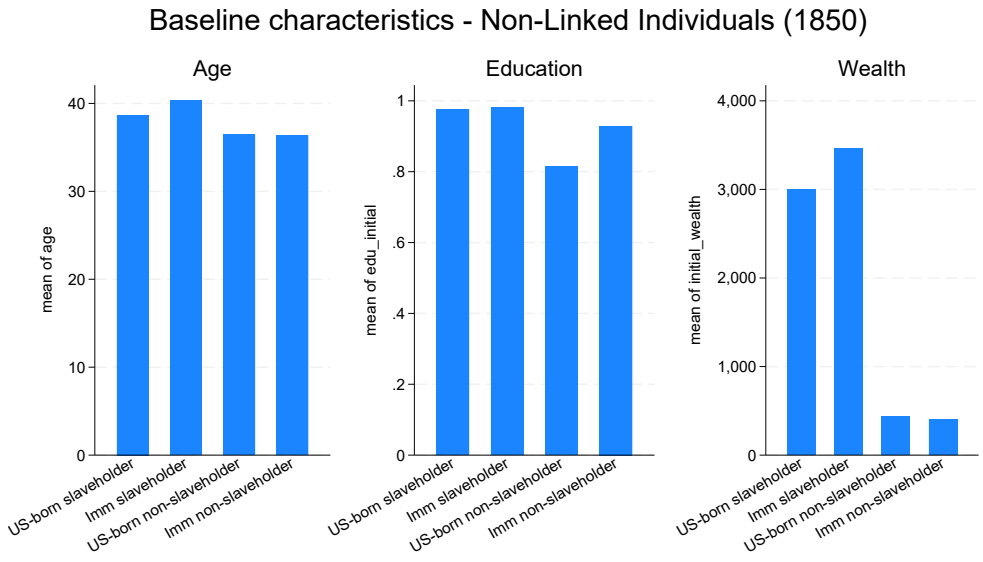


Figure 3: Summary Statistics - Unlinked

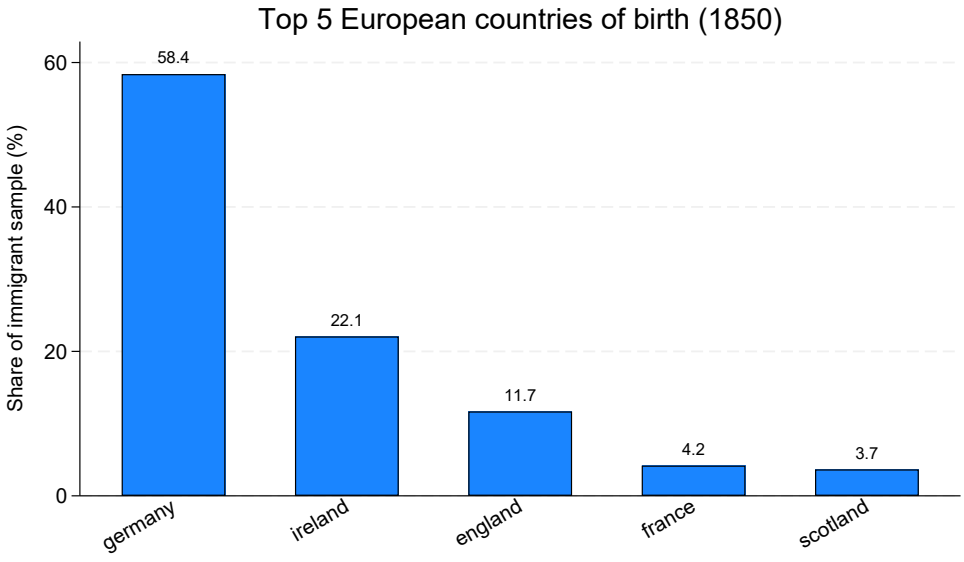


Figure 4: Most Representative Birthplaces

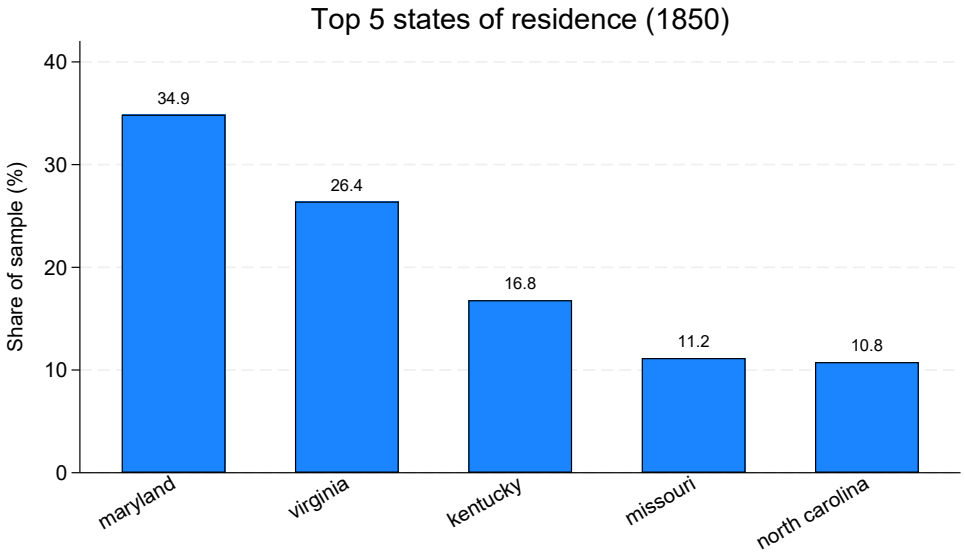


Figure 5: Most Representative Federal States

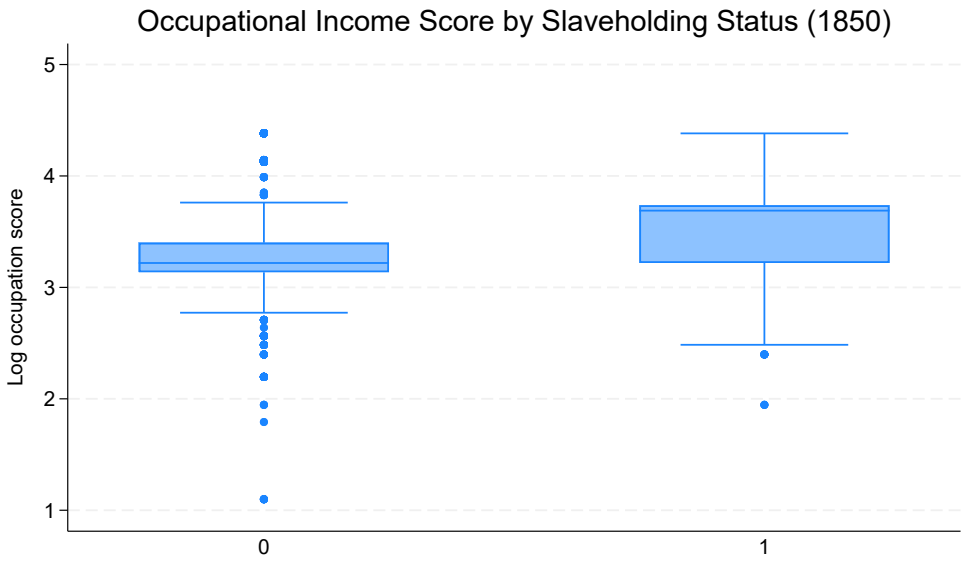


Figure 6: Box Plot

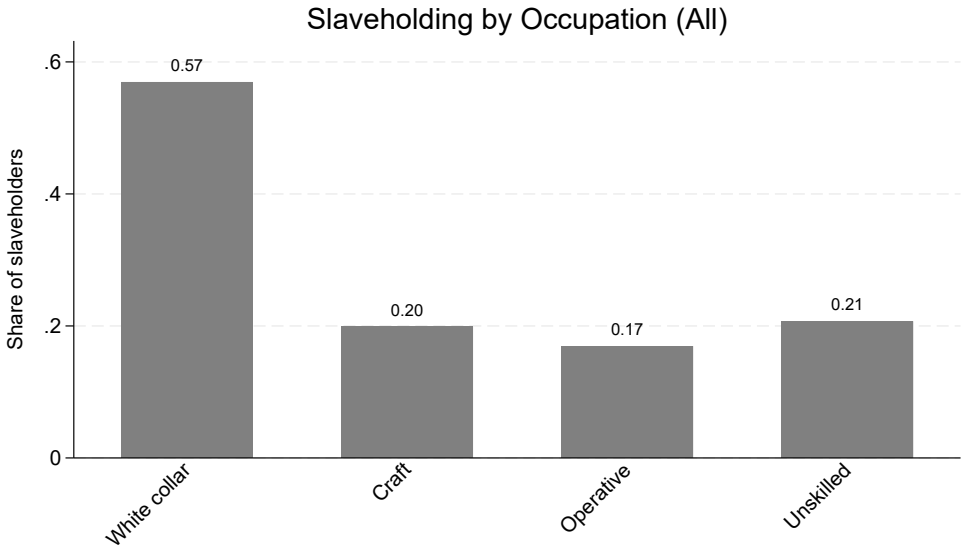


Figure 7: Slaveholding by Occupations

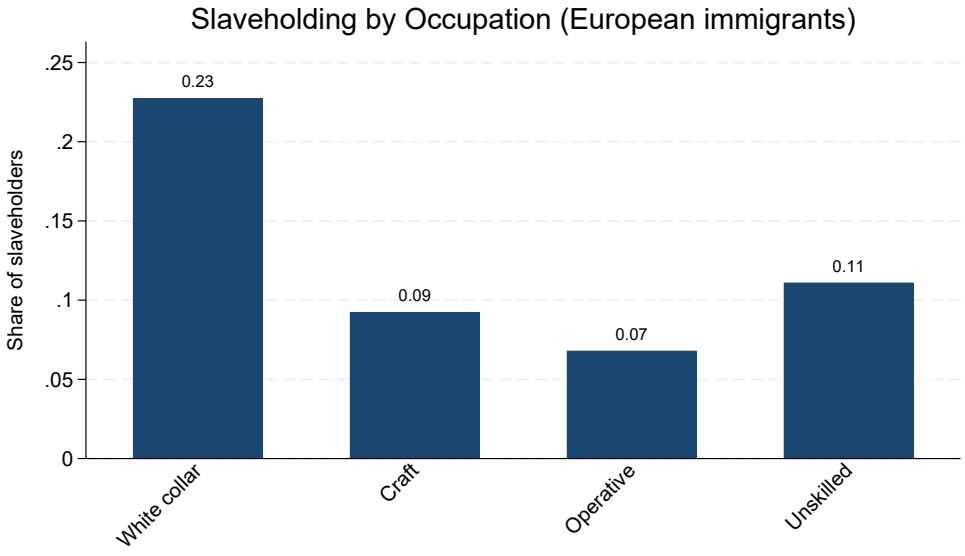


Figure 8: Slaveholding by Occupations (only Immigrants)

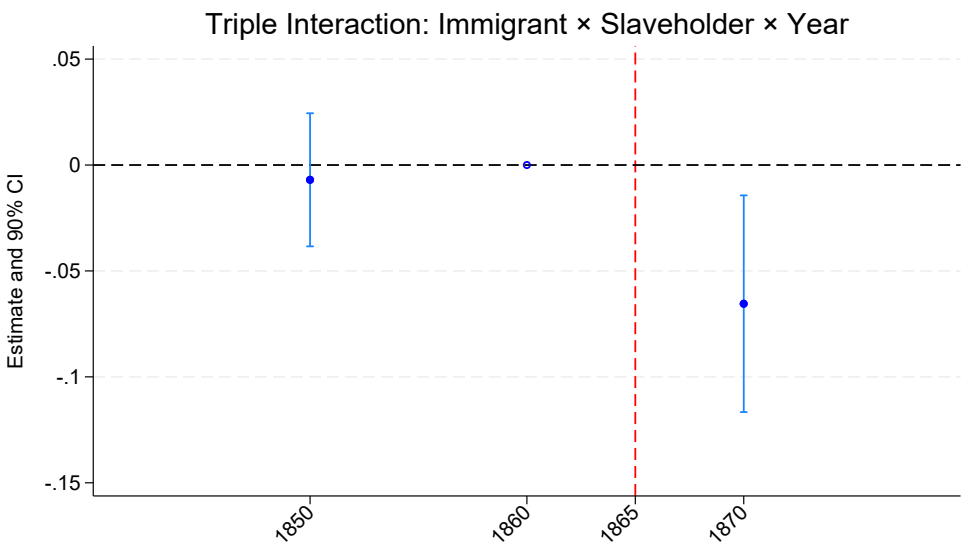


Figure 9: Event-study coefficients for Immigrant × Slaveholder × Year.

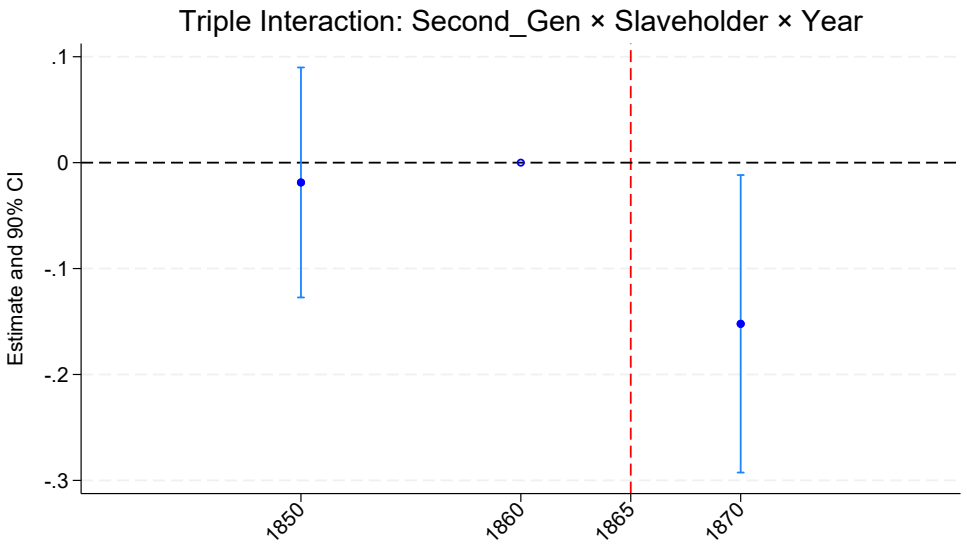


Figure 10: Event-study coefficients for Second Gen Immigrants × Slaveholder × Year.

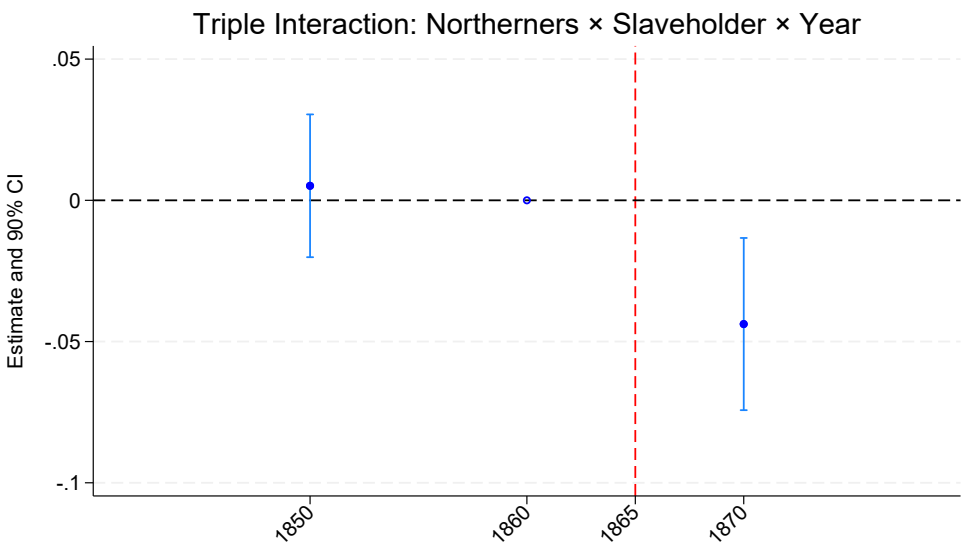


Figure 11: Event Study: Northern Migrants vs. Southern Natives

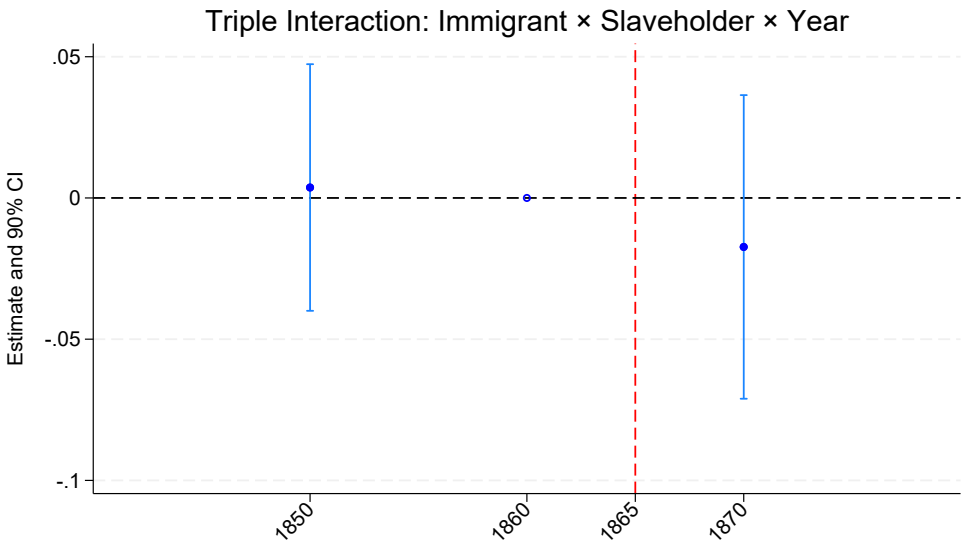


Figure 12: Event Study: European Immigrants vs. Northern Migrants

11 Tables

Table 1: Main Result

	(1)	(2)
Dependent variable: Log Occupational Income Score		
Immigrant × Post-1865	0.062***	0.067***
	(0.015)	(0.017)
Post-1865 × Slaveholder	0.023**	0.034***
	(0.011)	(0.009)
Immigrant × Post-1865 × Slaveholder	-0.055**	-0.062**
	(0.027)	(0.027)
Observations	66,778	66,778
R-squared	0.798	0.809
Industry FE	No	Yes
Mean dep var	3.371	3.371
Relative change	-0.00121	-0.00136
Percent change	-5.352	-6.013

Note. Standard errors (in parentheses) are clustered at the county and industry level. Asterisks denote statistical significance (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 2: Sorting across Occupations

Dependent variable:	(1) White Collar	(2) Craft	(3) Opertative	(4) Unskilled
Immigrant × Post-1865	0.032***	-0.008	-0.005	-0.043**
	(0.009)	(0.011)	(0.012)	(0.018)
Post-1865 × Slaveholder	0.027***	-0.013*	-0.002	-0.018**
	(0.008)	(0.008)	(0.007)	(0.008)
Immigrant × Post-1865 × Slaveholder	-0.029	0.000	0.032	0.013
	(0.028)	(0.031)	(0.025)	(0.020)
Observations	66,778	66,778	66,778	66,778
R-squared	0.835	0.805	0.689	0.565
Mean dep var	0.338	0.431	0.102	0.0497
Relative change	-0.00629	2.99e-05	0.0232	0.0186
Percent change	-2.835	0.0174	3.234	1.260

Note. Standard errors (in parentheses) are clustered at the county and industry level. Asterisks denote statistical significance (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 3: Within Occupation

Dependent variable: Log Occupational Income Score	(1)	(2)	(3)	(4)
Immigrant × Post-1865	0.046***	0.002	0.007	-0.003
	(0.017)	(0.004)	(0.010)	(0.099)
Post-1865 × Slaveholder	0.001	0.003	0.011	0.061
	(0.006)	(0.003)	(0.009)	(0.201)
Immigrant × Post-1865 × Slaveholder	-0.055***	-0.005	0.005	-0.202
	(0.021)	(0.008)	(0.019)	(0.255)
Observations	20,147	26,188	4,201	1,149
R-squared	0.883	0.897	0.877	0.949
Sample	White Collar	Craft	Operative	Unskilled
Mean dep var	3.800	3.254	3.181	2.683
Relative change	-0.00138	-9.46e-05	0.000101	-0.00495
Percent change	-5.365	-0.510	0.495	-18.27

Note. Standard errors (in parentheses) are clustered at the county and industry level. Asterisks denote statistical significance (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 4: Sorting across Task-Based Occupational Categories Within White Collar Occupations

Dependent variable:	(1) Professional/Managerial	(2) Routine
Immigrant × Post-1865	0.061***	-0.056**
	(0.023)	(0.023)
Post-1865 × Slaveholder	0.005	-0.003
	(0.008)	(0.008)
Immigrant × Post-1865 × Slaveholder	-0.055**	0.050*
	(0.027)	(0.029)
Observations	20,147	20,147
R-squared	0.676	0.671
Mean dep var	0.894	0.104
Relative change	-0.00592	0.0463
Percent change	-5.391	5.161

Note. Standard errors (in parentheses) are clustered at the county and industry level. Asterisks denote statistical significance (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 5: Social Networks and Occupational Decline: Heterogeneity by County Characteristics

	Immigrant Share		White-Collar Share		Black Share		Intermarriage	
	(1) Low	(2) High	(3) Low	(4) High	(5) Low	(6) High	(7) Yes	(8) No
Immigrant × Post-1865	0.053**	0.041***	0.074***	0.040***	0.048***	0.109***	0.037	0.066***
	(0.020)	(0.013)	(0.020)	(0.010)	(0.017)	(0.025)	(0.030)	(0.020)
Post-1865 × Slaveholder	0.040***	0.011	0.055***	0.010	0.025**	0.051***	0.051	0.034***
	(0.011)	(0.012)	(0.011)	(0.009)	(0.010)	(0.012)	(0.048)	(0.009)
Immigrant × Post-1865 × Slaveholder	-0.050	-0.024	-0.088**	-0.023	-0.082	-0.081**	-0.074	-0.064**
	(0.040)	(0.033)	(0.041)	(0.028)	(0.050)	(0.035)	(0.059)	(0.031)
Observations	39,305	24,758	35,215	28,407	38,453	25,904	2,874	63,759
R-squared	0.840	0.770	0.844	0.788	0.797	0.845	0.828	0.811
Mean Dependent Variable	3.389	3.343	3.371	3.371	3.331	3.434	3.385	3.370

Note. Standard errors (in parentheses) are clustered at the county and industry level. Asterisks denote statistical significance (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 6: Stayers vs Movers

	(1)	(2)	(3)	(4)
Dependent variable: Log Occupational Income Score				
Immigrant × Post-1865	0.068	0.063***	0.056***	0.111***
	(0.000)	(0.018)	(0.018)	(0.028)
Post-1865 × Slaveholder	0.041	0.020	0.018	0.021
	(0.000)	(0.014)	(0.015)	(0.014)
Immigrant × Post-1865 × Slaveholder	-0.053	-0.100**	-0.060	-0.196***
	(0.000)	(0.041)	(0.043)	(0.062)
Observations	36,609	30,107	22,473	14,677
R-squared	0.832	0.788	0.784	0.800
Sample	Stayers	Movers	Movers Pre	Movers Post
Mean dep var	3.382	3.358	3.355	3.360
Relative change	-0.00131	-0.00177	-0.00104	-0.00369
Percent change	-5.128	-9.488	-5.795	-17.77

Note. Standard errors (in parentheses) are clustered at the county and industry level. Asterisks denote statistical significance (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 7: Stayers vs Movers - Federal States

	(1)	(2)	(3)	(4)
Dependent variable: Log Occupational Income Score				
Immigrant × Post-1865	0.065***	0.079*	0.066*	0.073
	(0.019)	(0.040)	(0.036)	(0.081)
Post-1865 × Slaveholder	0.036***	0.020	0.015	0.054
	(0.009)	(0.025)	(0.028)	(0.061)
Immigrant × Post-1865 × Slaveholder	-0.055**	-0.135	-0.090	-0.012
	(0.027)	(0.096)	(0.105)	(0.156)
Observations	58,673	8,013	4,053	2,595
R-squared	0.814	0.813	0.842	0.853
Sample	Stayers	Movers	Movers Pre	Movers Post
Mean dep var	3.369	3.386	3.383	3.381
Relative change	-0.121	-0.266	-0.150	-0.0276

Note. Standard errors (in parentheses) are clustered at the county and industry level. Asterisks denote statistical significance (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 8: Old, New, and Previous Slaveholders

Dependent variable: Log Occupational Income Score	(1)	(2)	(3)
Immigrant × Post-1865	0.067***	0.064***	0.064***
	(0.018)	(0.016)	(0.017)
Post-1865 × Slaveholder	0.042***	0.020**	0.029***
	(0.011)	(0.009)	(0.010)
Immigrant × Post-1865 × Slaveholder	-0.061**	-0.046	-0.044
	(0.028)	(0.031)	(0.032)
Observations	63,073	55,335	51,619
R-squared	0.806	0.754	0.737
Sample	Old	New	Previous
Mean dep var	3.358	3.296	3.275
Relative change	-0.00139	-0.000764	-0.000770
Percent change	-5.931	-4.460	-4.320

Note. Standard errors (in parentheses) are clustered at the county and industry level. Asterisks denote statistical significance (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 9: Immigration, Slaveholding, and Social Outcomes

	(1) Intermarriage	(2) Domestic Servant
Immigrant (White, European-born)	0.175***	-0.070***
	(0.008)	(0.006)
Slaveholder (1850)	-0.010***	0.220***
	(0.002)	(0.007)
Immigrant × Slaveholder	0.397***	-0.004
	(0.027)	(0.024)
Controls	Yes	Yes
County FE	Yes	Yes
Observations	22,267	22,267
R-squared	0.196	0.129

Note. The table reports OLS estimates. The dependent variable in column (1) is an indicator for intermarriage, while in column (2) it is an indicator for employment as a domestic servant. All regressions control for age, age squared, initial wealth, initial education, and initial industry. County fixed effects are included in all specifications. Robust standard errors are reported in parentheses. Asterisks denote statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A Appendix

Table 10: Robustness Checks

Dependent variable: Log Occupational Income Score	(1)	(2)	(3)	(4)
Immigrant × Post-1865	0.067***	0.066***	-0.040	0.057***
	(0.010)	(0.017)	(0.234)	(0.012)
Post-1865 × Slaveholder	0.034***	0.034***	0.038***	0.012
	(0.007)	(0.009)	(0.009)	(0.009)
Immigrant × Post-1865 × Slaveholder	-0.062***	-0.062**	-0.077***	-0.043*
	(0.020)	(0.027)	(0.028)	(0.023)
Observations	66,778	66,659	66,778	36,994
R-squared	0.809	0.810	0.810	0.816
Robustness	SEs	Farmer	BPL	Max One Slave
Mean dep var	3.371	3.372	3.371	3.288
Relative change	-0.136	-0.136	-0.169	-0.101

Note. Asterisks denote statistical significance (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

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