The Hinge of the Golden Door: Labor Market Impacts of Immigrant Exclusion

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Abstract

I examine the impacts of the Chinese Exclusion Act of 1882, America's first ever immigration restriction that prohibited entry to Chinese laborers, on native labor market outcomes. To identify the causal effect, I utilize variation in pre-1882 Chinese settlement and match on individual- and labor market-level characteristics. Using linked Census data from 1880-1900 for over two million US workers, I find the Chinese exclusion significantly slowed the long-term occupational mobility of native workers, with the effects strongest for low-skilled and unemployed workers. I find evidence in support of what I term a "honeypot" effect: low-skilled natives likely benefited from the labor shortage through higher wages in the short-run, but in the long-run the shortage disincentivized upskilling and slowed occupational upgrading. Moreover, I show Chinese laborers were almost entirely substituted by immigrants from other countries in the long-run, likely negating the initial wage gains.

(*JEL*: J6, J15, J22, J24, N31)

1 Introduction

Low-skilled immigration restrictions are often proposed as a policy to alleviate the economic hardships of low-income native workers by reducing labor supply to improve wages and employment opportunities (Abramitzky & Boustan 2017, Clemens et al. 2018). Despite the large body of literature analyzing the relationship between immigration policy and the local labor market, it remains a contentious topic.[1] The basic labor market model predicts positive effects of low-skilled immigration restriction for low-skilled natives in the short-run, however the long-run effects are theoretically ambiguous, particularly when accounting for potential endogenous technological, migration or human capital responses to immigration restrictions

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[1]Partial summaries of both modern and historical immigration studies can be found in Peri (2016), Dustmann et al. (2016) and Abramitzky & Boustan (2017).

(Acemoglu 2010, Borjas 2006, Lewis 2011). It therefore remains an open empirical question. Furthermore, understanding the long-term labor market impacts of immigration restrictions has renewed importance in light of the most radical immigration restrictions since World War II in response to the COVID-19 pandemic, contributing to substantial low-skilled labor shortages in the US, Europe and Australia (Rodriguez-Sanchez 2022, Ramskogler 2022, Mackey et al. 2022).

With a lack of substantial modern immigration restrictions, economists are starting to turn to historical immigration restrictions to empirically evaluate the long-term labor market impacts (Lew & Cater 2018, Clemens et al. 2018, Price et al. 2020, Lee et al. 2022, Abramitzky et al. 2022). I contribute to this by analyzing the first ever significant immigration restriction in the US, the Chinese Exclusion Act of 1882, a landmark policy described as "the hinge on which the golden door began to swing almost shut" (Daniels 2002). Prior to 1882, the United States had an open border policy with effectively no restrictions, and over 300,000 predominantly low-skilled Chinese laborers arrived during this period. The Chinese Exclusion Act was passed largely in response to native economic hardships brought about by recession, and prohibited the immigration of Chinese laborers. Overnight, the inflow of Chinese migrants collapsed to virtually zero while outflows significantly increased.[2] The Chinese would remain the only ethnicity that could not freely immigrate to the United States for nearly half a century. I examine what effect the Chinese exclusion had on the long-term labor market outcomes of native workers.

This unique setting provides several key advantages. First, the Chinese exclusion provides an incredibly sharp and targeted immigration shock. The passing of the Act immediately transformed Chinese immigration from completely unrestricted to completely prohibited, while leaving all other nationalities unrestricted. Second, the relatively small but highly concentrated pre-1882 population of Chinese laborers also provides a cleaner labor market shock.[3] Chinese laborers had heavy presence in a small number of local labor markets, particularly in California where they represented over a quarter of the workforce (Daniels 2002), but virtually no presence in majority of markets.[4] Therefore, the exclusion likely had strong labor supply effects in these select few markets, but no significant spillover effects into the other markets, as opposed to broader restrictions that impact the majority of immigrants. Third, the Act worked to both prevent inflows and increase outflows. This helps to avoid the confounding issue of temporary migrants becoming permanent in response to restrictions (Constant et al. 2013), as well as reduce reliance on network effect assumptions in shift-share instruments that have been shown to be problematic (Jaeger et al. 2018). Fourth, the targeting of Chinese migrants over other low-skilled migrants was largely geopolitical and unrelated to migrant characteristics or the domestic labor market, making the exclusion plausibly exogenous.[5] Finally, the historical evidence suggests relatively limited illegal immigration from

[2]The Chinese Exclusion Act was accompanied by various legal and extralegal efforts to exclude and drive incumbent Chinese laborers out of labor markets. See Section 3 for more details.

[3]Chinese workers represented only 0.8% of the total male workforce in the US in 1880, and 3% of the male immigrant workforce.

[4]The top 10% of local labor markets by Chinese share contained over 95% of the Chinese male working population, while over 80% of local labor markets recorded less than 0.01% Chinese share. See Figure 1 for a visual representation of Chinese geographic concentration.

[5]There was significant public pressure during this period to extend the exclusion to other immigrant

China to potentially confounds analysis, as in the case of Mexican-US immigration (Kaestner 2020).[6]

My identification strategy utilizes variation in policy exposure based on pre-1882 Chinese settlement patterns. I construct an individual longitudinal dataset by linking the entire universe of native-born working age males in the US between the 1880 and 1900 Censuses, based on linkages provided by Abramitzky, Boustan & Rashid (2020).[7] This successfully links around a third of the target population, creating an extensive sample of over 2 million individuals. The individual longitudinal structure has been shown to be vital for accurate estimation of labor market effects as aggregate data fails to account for compositional changes due to endogenous migration response to restrictions, nor the individual-level unobservables and heterogeneous effects (Price et al. 2020, Lee et al. 2022).

I use a difference-in-difference estimation strategy where a worker's treatment status is determined by whether he resided in 1880 in a labor market with a significant Chinese share of the working-age male population (independent of the labor market they reside in post-1882).[8][9] Income was not recorded in the Census until 1940, therefore I proxy for income using occupational income score. This estimates the likely earnings based on the recorded occupation and is widely used in historical labor market studies as a proxy of income (Abramitzky et al. 2012, Biavaschi et al. 2017, Ward 2020, Price et al. 2020, Abramitzky et al. 2022).[10] I also analyze native employment outcomes.

The key identifying assumption is that trends in labor market outcomes would not have diverged between native workers in labor markets with and without significant Chinese presence in the absence of the Chinese Exclusion Act. The empirical challenge is that Chinese settlement location is clearly not random and endogenous to local labor market conditions and trends. I address these endogeneity concerns in three main ways. First, I control for the labor market's total foreign-born share in 1880, following the strategy of (Abramitzky et al. 2022), thereby comparing labor markets/industries with different foreign workforce

groups, particularly the Japanese and Catholic Europeans. However, during this period of largely open borders, targeted immigration restrictions was considered a hostile foreign act. The only country which was considered politically feasible to restrict was China, which was under heavy foreign influence (Daniels 2002). Evidence for this can be found in the 1878 veto of an earlier version of the Chinese Exclusion Act. While the legislation passed both Houses, it was vetoed by President Rutherford B. Hayes as the Burlingame Treaty of 1868 with China guaranteed free migration between the two countries. In response, China was pressured to renegotiate an unequal treaty in 1880, granting the US the right to unilaterally suspend Chinese immigration, and paving the way for the passing of the Chinese Exclusion Act in 1882 (Daniels 2004). Significant immigration restrictions would eventually be extended in the face of growing anti-immigrant sentiment: the Japanese in 1907, and Southern and Eastern Europeans in the 1920s.

[6]Chinese migrants did experience some success circumventing the exclusion, mostly via Mexico, Canada or falsified documentation. However, only an estimated 17,300 Chinese illegally entered the country between 1882 and 1920 (relative to over 40,000 entries in the year prior to the Act's implementation), with the majority of this occurring after my analysis period of 1880-1900 (Lee 2003).

[7]The 1890 Census was largely destroyed in a fire in 1921.

[8]I define significant share as above 5%, but a stronger 10% threshold is also considered for robustness

[9]I also consider a more targeted, industry-level measure of policy exposure, where a worker is considered treated if they were employed in an industry (within a labor market) in 1880 that employed a significant share of Chinese (independent of where they work or reside post-1882).

[10]I further validate this proxy using limited wage data from the period and find no significant differences or sensitivity in the results when different proxy are used. See Appendix A.2 for details.

composition, but with the same overall foreign share.[11] Second, I implement a coarsened exact matching (CEM) methodology, as proposed by (Iacus & King 2012), to address differences in baseline characteristics between exposed and unexposed labor markets. I match on both individual and labor market characteristics in 1880 to ensure comparisons between workers who i) work in identical occupations and industries,[12] ii) have the same rural/urban designation, and iii) are of similar age, as well as facing labor markets that have iv) similar industrial compositions and v) similar total foreign-born share. Finally, given the heavy regional concentration of Chinese workers (over 97% of Chinese lived in the Western Census region in 1880), I run additional analyses using only Western labor markets.[13] This addresses potential assumption violations due to different regional labor market trajectories. To validate that these methods satisfy the identifying assumption, I consider a placebo legislation date utilizing the presidential veto of an earlier version of the Chinese Exclusion Act in 1878 due to treaty obligations. I find Chinese labor market presence had no significant effect on occupational income under the placebo policy date, lending support to the conditional parallel trends assumption.

The main finding of the paper is that the Chinese Exclusion Act had a negative effect on the long-run occupational mobility of native workers. The effects are large in magnitude: workers in labor markets and industries exposed to the Chinese exclusion ended up in occupations with 6-15% (or 0.15-0.4 standard deviations) lower average income than similar unexposed workers. However, I find no significant effects on native long-term employment prospects. The results are robust to alternative matching covariates, Census linking methods, treatment definitions and measures of occupational standing. I also find considerable heterogeneity in the labor market impacts. Analyzing the treatment effects by skill quintile produces an inverted U-shaped pattern, with the highest and lowest quintiles exhibiting the most negative outcomes. Unemployed workers and new entrants into the labor market are also found to be more exposed to the negative effects of the exclusion. Central to this treatment heterogeneity is the extent to which Chinese laborers are complements or substitutes to native-born laborers in different skill occupations. Low-skilled immigrants have generally been found to be complements to higher-skilled occupations (Okkerse 2008, Cattaneo et al. 2015), which would explain why high-skilled native workers were worse-off under the Chinese exclusion. However, I analyse the changing ethnic composition of occupations in exposed labor markets and find the majority of jobs vacated by Chinese laborers after the exclusion were filled by native workers, indicating a high degree of substitutability in low-skilled occupations.

If low-skilled native workers were substitutes to Chinese immigrants, why did the Chinese Exclusion Act create negative long-term outcomes for these workers? To answer this question, I find evidence in support of what I term the "honeypot" effect. This is where low-skilled labor shortages from immigration restrictions cause a temporary increase in low-

[11]For example, the treatment labor market with around 30% total foreign share, consisting mostly of Chinese workers, would be compared to a labor market also with 30% total foreign share, but consisting entirely of European workers.

[12]The Census occupational codes are quite granular with over 300 occupations, while I collapse industry codes into nine rough categories.

[13]The Western region is defined by the Census as Arizona, California, Colorado, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, Washington and Wyoming.

skilled wages, creating the so-called "honeypot". Native workers are, in turn, attracted to these low-skilled occupations and disincentivized from upskilling and pursuing higher-skilled occupation. The initial wage gains from the "honeypot" are eventually negated through migration or industry response, while the exposed workers have now fallen behind their unexposed counterparts in occupational upgrading.

In support of the "honeypot" effect, I first find the exclusion did created significant labor shortages, with the Chinese share of the total male workforce decreased 55.4% in impacted labor markets, and 76.4% in key impacted industries. This shortage was largely filled by native workers, as already discussed above. I further find no significant contractions in affected occupations or industries as a result of the exclusion. The US Census does not record wages, but I use industry censuses to obtain average mining wages by county to provide suggestive evidence of wage changes.[14] The results suggest that low-skilled wages were indeed significantly higher in labor markets exposed to the Chinese Exclusion Act. Furthermore, the ratio of low-skilled to high-skilled wages (i.e. mining laborers/mining managers) was also higher in exposed labor markets, suggesting a decrease in the skill premium. Together, these results suggest low-skilled native workers initially benefited from the Chinese Exclusion Act.

Looking to the long-run effects, I find the Chinese Exclusion Act had a negative impact on human capital attainment. While there is no direct data on education levels, workers exposed to the Chinese exclusion were significantly more likely to remain illiterate and ended up in occupations with lower average education rates in the long-run. I also examine long-run migration and industry responses to the immigrant exclusion. I find that Chinese immigrants were largely replaced by non-Chinese immigrants in the long run, thus likely cancelling out any initial wage gains. However, I find no evidence of significant capital substitution, likely due to a lack of available labor-substituting technology in most major industries in the late 1800s (Hunter & Bryant 1979, Dix 1988, Lew & Cater 2018). Furthermore, I find industries in exposed markets did not significantly decrease output or employment levels relative to industries in similar, unexposed market.

In the final section, I examine the dynamic effects of the Chinese Exclusion Act using two different cohort analyses to trace out the short-run to long-run impacts of the "honeypot" effects. The analyses utilize two types of persistent shocks: the first is that shocks in labor market conditions when young workers enter the market have significant, long-term impacts on earnings and occupational mobility; the second is that income shocks in early childhood (defined as aged 0-8) have significant impacts on adult earnings.[15] Therefore, by comparing the long-term occupational income by year of labor market entry between exposed and unexposed labor markets, I can infer the year-by-year effects on labor market conditions for entry-level (i.e. predominantly low-skilled) occupations. Similarly, by comparing the adult earnings of different early childhood periods, I can infer year-by-year changes in the relative economic standing of their fathers.

The results of the dynamic analysis corroborate the "honeypot" effect dynamics. The effects for the earlier cohorts demonstrate the first stages of the "honeypot": the exclusion

[14]While wage data is only provided for the mining industry, it is the industry most impacted by the Chinese exclusion with the highest pre-1882 share of Chinese workers, and thus provides important insights.

[15]See von Wachter (2020) for a summary of the literature on the persistence of labor market shocks at entry, and see Heckman (2008), Duncan et al. (2010) and Smith (2015) for summaries of the literature linking early childhood economic shocks to adult earnings.

created significant benefits for workers in the short-run. Workers who entered exposed markets in the first years after the Act's implementation had significantly higher occupational income in 1900, while children with early childhood in exposed markets at the time of implementation had significantly higher income in 1940. The results suggest this "honeypot" lasted around 3-4 years. After these initial gains, the negative "honeypot" effects begin to manifest. Entrants into these same exposed markets are consistently worse off for the next decade, and the adult income gains for children born during this period begin to diminish.[16] In both analyses, the differences between exposed and unexposed cohorts disappear after around 15 years.

2 Related Literature

This paper contributes to the broad literature on the labor market impacts of immigration, specifically the impacts of immigration restriction. The increasing literature on historical immigration restrictions has focused predominantly on two main case studies: the 1920s US immigration quotas (Lew & Cater 2018, Tabellini 2020, Price et al. 2020, Abramitzky et al. 2022) and Mexican-US border restrictions (Clemens et al. 2018, Lee et al. 2022). My empirical strategy is most closely related to Abramitzky et al. (2022), who utilize foreign-born composition in labor markets and variation in how binding the intake quota is for each immigrant group. However, I am the first to use matching to provide cleaner identification of immigration restriction effects. My findings largely correspond with the general consensus of weakly negative long-term effects of immigration restrictions on the income, employment and occupational mobility of native workers. Immigration restrictions from this period have also been found to have wider impacts on a number of factors outside of the labor market including political and redistribution preferences (Tabellini 2020), invention and innovation (Moser & San 2020, Doran & Yoon 2018), and even the fate of Jews in Nazi Germany (Buggle et al. 2020).

The literature largely attributes the weakly negative labor market effects to migration and industry response to the restrictions, resulting in substitution with either capital or unrestricted workers, and potentially displacing incumbent native workers. Lafortune et al. (2015), Lew & Cater (2018) and Abramitzky et al. (2022) all find that the 1920s immigration restrictions significantly increased the pace of mechanisation in US agriculture, while Clemens et al. (2018) find similar effects for the restriction of Mexican *bracero* farm workers. Low-skilled out-migration due to natural disaster during this period has also been shown to increase mechanisation (Hornbeck & Naidu 2014). Conversely, low-skilled immigration inflows has also been shown to slow the rate of technology adoption (Lewis 2011). Furthermore, immigration restrictions have also been shown to create negative agglomeration effects in impacted regions (Lee et al. 2022, Long et al. 2022). However, I do not find evidence in support of the capital substitution mechanism, likely due to the lack of labor-substituting technology during the 1880s. I also do not observe significant negative agglomeration effects,

[16] The effects do still remain marginally positive for early childhood exposure in the medium- to long-term, suggesting the long-run negative occupational mobility effects likely only cancel out rather than dominate the short-run income gains (at least in the first 18 years of the Act), resulting in a net zero long-run welfare effect.

with no significant change in the working age population or industry output in exposed markets. On the other hand, I do find evidence in support of labor source substitution, with Chinese laborers largely replaced by unrestricted immigrants. Abramitzky et al. (2022) find similar effects with natives and unrestricted migrants largely replacing restricted migrants. Price et al. (2020) find native migration response to immigration restriction creates significant heterogeneity in long-term impacts, with younger and low-skilled workers "winning" and older and high-skilled workers "losing" due to reduced displacement effect.

However, this paper takes a different approach, being the first to examine human capital and immigrant competition mechanisms of immigration restriction with what I term the "honeypot" effect. The "honeypot" effect is closely related to literature examining the education and occupation switching responses to low-skilled immigration inflows. Low-skilled immigrants have generally been found to be substitutes in blue-collar occupations, but complements in white-collar occupations where there are communication skill premiums (Cattaneo et al. 2015). Therefore low-skilled immigration inflows work to increase competition in low-skilled occupations, incentivizing natives to invest in education and move into higher-skilled occupations. Indeed Cattaneo et al. (2015), Foged & Peri (2016) and Mandelman & Zlate (2022) all find immigration inflows had the effect of pushing low-skilled natives from blue-collar to white-collar occupations, resulting in net long-term gains. Looking directly at the human capital effects of immigration inflows, Hunt (2017) finds increased high school completion rates while Llull (2017) finds a general increase in education levels. This paper is the first to empirically show the reverse effect for immigration restrictions, however dynamic general equilibrium simulations of restrictions have predicted such effects (Dixon & Rimmer 2009).

The study of historical immigration restrictions is inherently limited by the infrequent timing of historical data, such as decennial Censuses. This narrows studies to largely long-run effects without insight into the dynamic short-run to long-run effects. Yet the "honeypot" effect demonstrates that the short- and long-run effects can be differ considerably, and distinguishing between them is imperative to understanding the full impact of immigration restrictions on the local labor market. To the best of my knowledge, this is the first paper to use cohort analysis to provide insight into the dynamic effects of historical immigration restrictions.

Finally, this paper is the first quantitative analysis of the effect of the Chinese Exclusion Act on native workers. As a landmark legislation that created the legal foundations as well as the border protection infrastructure that defines US immigration policy today(Lee 2003), understanding the long-term effects of the Chinese Exclusion Act represents an important contribution in its own right. Yet despite its significance to American immigration history, it remains relatively understudied by economists. Long et al. (2022) examine the wider impact on the regional economy, focusing on long-term economic development and industry productivity, while Chen (2015) and Chen & Xie (2020) examine the effects of the Act on incumbent Chinese migrants. However this is the first paper to examine whether the Act achieved its intended effect of improving economic outcomes for low-skilled native workers.

3 Historical Background

3.1 Chinese Immigration to the United States

The first meaningful Chinese immigration to the United States began in 1848, where thousands of Chinese joined the rush to find gold in California. This period is often labelled the 'Age of Mass Migration', where there were essentially no immigration restrictions and over 30 million people immigrated to the United States (predominantly from Europe) until the sweeping immigration quotas of the 1920s.

Chinese migrants initially worked mainly as mining laborers, but eventually expanded into other, predominantly low-skilled occupations in manufacturing, construction, agriculture and the services industries, particularly laundries and kitchens. From 1848 till the passing of the act in 1882, it is estimated that over 300,000 Chinese migrants entered the United States (Daniels 2002). These migrants were overwhelmingly male sojourners (roughly 20 to 1 sex ratio), coming to America to with the intention of returning home with a nest egg (Yang 2000). However, the Chinese were not unique in high rates of return migration or other immigrant characteristics. Many European ethnicities had similar rates of return migration, settlement concentrations and employment patterns (Bandiera et al. 2013).

Table 11 summarizes and compares demographic and labor market characteristics in 1880 between Chinese and other foreign working age males. There are significant differences with the average Chinese laborer being younger, having lower-paying and lower-skilled occupations, and more likely to work in the mining or services industry rather than agriculture. However, much of these differences simply reflect geographic differences, thus when Chinese are compared with other foreigners in the American West, their characteristics are far more similar. Figure 1 shows the 1880 geographic distribution of Chinese and total foreign shares by labor market. Chinese laborers are heavily present in the West, while overall foreigners are far more geographically dispersed. This Chinese western concentration is largely the result of the port of entry and the tendency of immigrants to follow existing transport connections rather than any unique Chinese characteristics (Sequeira et al. 2020). The vast majority of Chinese migrants arrived in western ports, particularly San Francisco, while the vast majority of other migrant groups arrived in eastern ports or through land borders.

3.2 The Chinese Exclusion Act of 1882

While Chinese labor was initially welcomed, the severe 'Long Depression' of the 1870s began to fuel anti-immigrant sentiments with the widespread belief that low-skilled migration from China and Europe depressed native wages and took native jobs (Chen & Xie 2020). Chinese immigrants soon became the focal point for all native worker's woes. Denis Kearney, leader of the Workingmen's Party of California, would end all of his speeches with the line "And whatever happens, the Chinese must go" (Lew-Williams 2018). With immigration having moved from a state to a federal jurisdiction following the Civil War, there was growing pressure from labor unions and politicians to enact a nationwide ban on Chinese immigration. A version of the Chinese Exclusion Act was initially passed in 1878, but was vetoed by President Rutherford B. Hayes due to treaty obligations. The Burlingame Treaty with China was hastily renegotiated and the Chinese Exclusion Act was eventually passed in 1882 with bipartisan support.

The Act prohibited the immigration of Chinese laborers and their relatives to the United States, with a small number of non-laborers exempted (namely merchants, diplomats and students) to prevent interruption of Chinese-American trade (Daniels 2002). The Act was originally only legislated to be in place for 10 years, however it was extended for another 10 years in 1892 and made permanent in 1902. This meant that for nearly 30 years, China was the only nationality unable to freely immigrate to the United States. The exclusion would not be lifted the repeal of the Act in 1943. The Chinese Exclusion Act was not a legislative anomaly, but rather the first step in a wider nativist, anti-immigrant movement in the US. It helped lay the legal and bureaucratic groundwork for progressively stronger restrictions against other perceived 'undesirable' immigrant groups, culminating in the 1920s border quotas that prevented any Asian immigration and severely limited immigration from southern and eastern Europe (Lee 2003).

The Chinese Exclusion Act was further accompanied with legal and extra-legal efforts to remove incumbent Chinese-Americans. Legally, this involved numerous discriminatory laws, such as requiring Chinese to obtain certificates of residence backed by credible non-Chinese witnesses or be deported, representing the first large-scale deportation effort in American history.[17] Extra-legally, there were widespread efforts to harass, exclude and force Chinese laborers out of communities, later known as the 'Driving-Out' period. These often became violent, resulting in several massacres of Chinese laborers including the Rock Springs massacre of 1885 and the Hells Canyon massacre of 1887, without any serious prevention by law enforcement (Pfaelzer 2008).

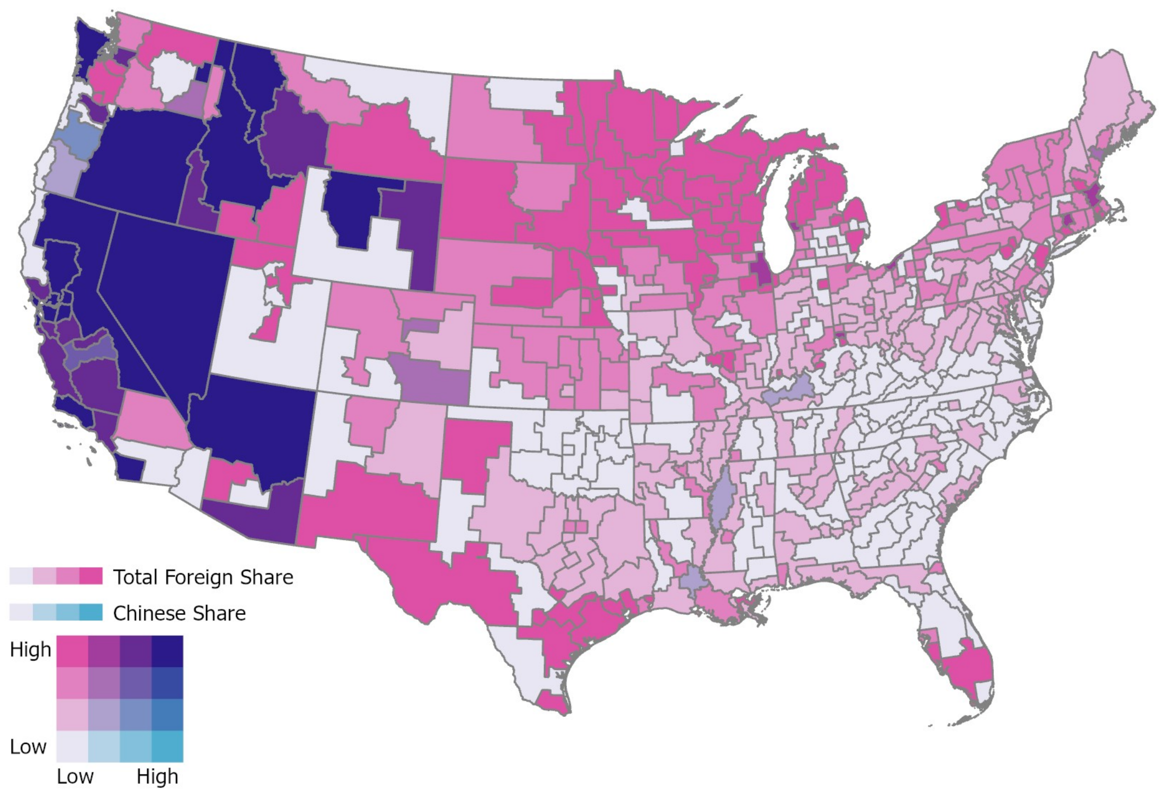
There was significant opposition to the Act from the business community in the West, led by prominent attorney and forty-niner Frederick Bee, that relied heavily on Chinese labor (Pfaelzer 2008). Accounts from the period suggest the exclusion did create significant labor shortages, particularly in agriculture and mining, with increased recruitment drives in the Eastern US to convince workers to move West (Murray 1903).

Figure 2 illustrates Chinese and total immigration inflow into the United States. Prior to 1882 the immigration patterns were relatively similar.[18] However post-1882 Chinese immigration collapsed to virtually zero while immigration from other regions continued to grow until World War I and the 1920s border closures. Figure 2 documents estimated Chinese immigration inflows and outflows. Pre-1882 is characterized by large inflows of Chinese migrants, however with significant return migration rates. Post-1882 the inflows collapse to virtually zero while departures increase substantially, demonstrating the success of both the immigration exclusion and the driving out efforts. Thus the Chinese Exclusion Act had sharp flow and stock effects on the Chinese immigrant population, with annual net migration shifting from 29,213 in 1882 to -19,633 just three years later.

[17] These efforts would, however, prove largely ineffective due to a lack of bureaucratic capacity to carry out large-scale deportations (Lew-Williams 2018).

[18] There is a spike in Chinese immigration in 1882, however there is a similar spike in total immigration in this year, suggesting the spike is not unique to Chinese immigration and thus unrelated to the passage of the Chinese Exclusion Act.

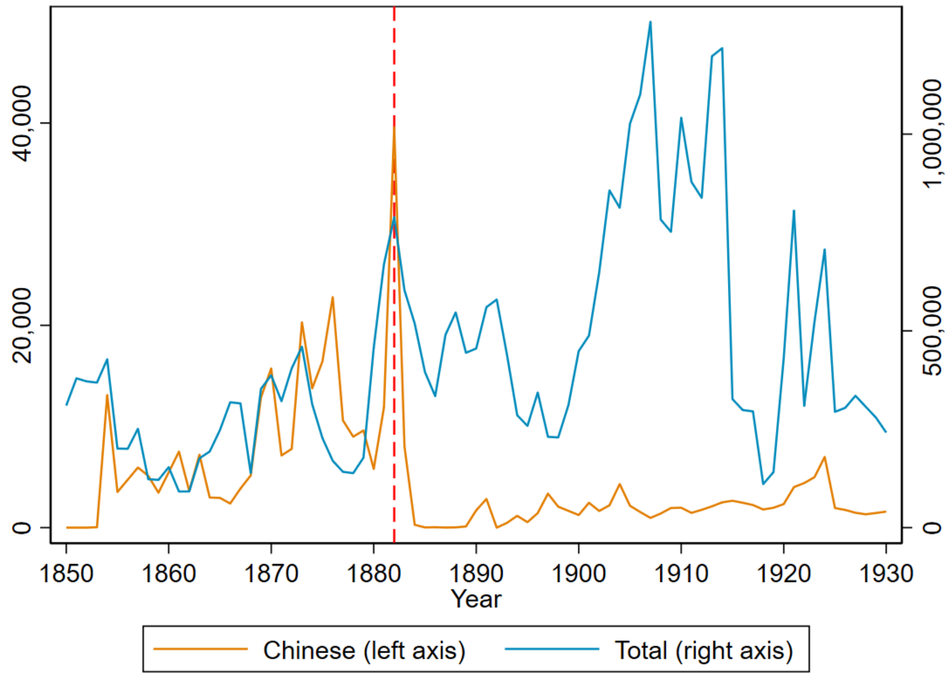
Figure 1: 1880 Chinese and Total Foreign Share of the Male Working Age Population



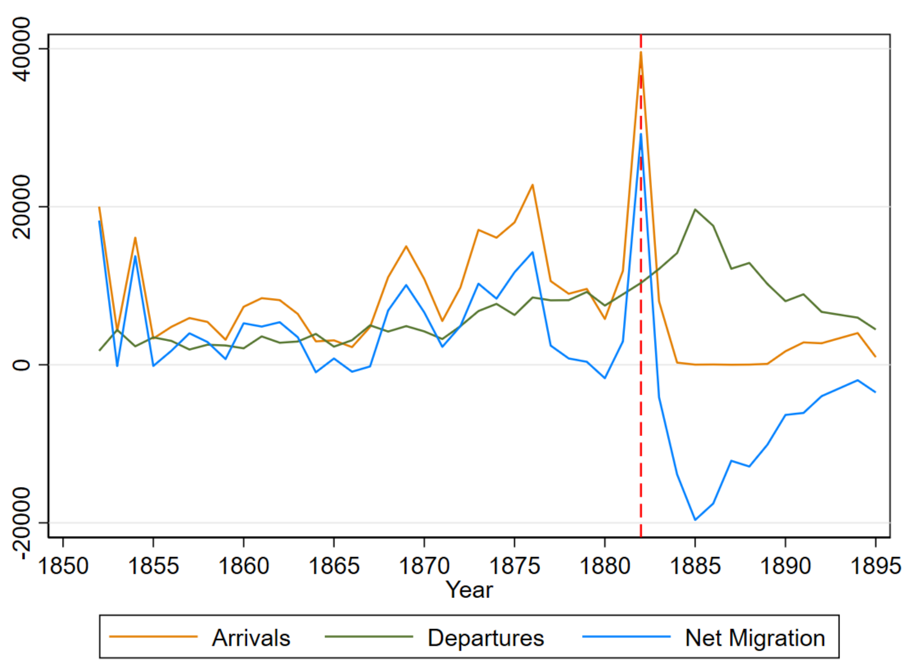
Notes: Chinese and total foreign share of the male population aged 18-70 by State Economic Area. Calculated from the full-count 1880 Census. State Economic Area defined by the Census as economically linked counties. The four categories of total foreign share correspond to 0-5%, 5-10%, 10-20% and 20%+, while for Chinese share they correspond to the various treatment thresholds used in the analysis (0-1%, 1-5%, 5-10%, 10%+).

Figure 2: Chinese Immigration to the United States

Panel A: Chinese vs. Total Annual Immigration Inflows, 1850-1930



Panel B: Chinese Net Annual Immigration, 1852-1895



Notes: Annual immigration into and out of the United States. Vertical dotted line denotes passing of Chinese Exclusion Act in 1882. Panel A figure constructed with data from Carter et al. (2006). Panel B figure constructed with data from Coolidge (1909).

4 Data, Measurement and Matching

4.1 US Population and Industry Censuses

To analyze the impact of the Chinese Exclusion Act on native labor market outcomes, I use full-count US Population Censuses from 1880 and 1900.[19] Individuals are linked between the two Censuses using crosswalks provided by the Census Linking Project (Abramitzky, Boustan, Eriksson, Pérez & Rashid 2020). Around 35% of the target population is successfully linked. The linked population is not substantially different from the full target population, although slightly older, rural and more educated. The linking probability is also uncorrelated with pre-treatment Chinese settlement. A full discussion of the methodology, performance and balance of the Census links can be found in Appendix A.1.

For the main analysis, the sample is limited to native-born males who are of working age in both 1880 and 1900 (i.e. ages 18 to 50 in 1880). African-Americans, native Americans and individuals in Alaska and Hawaii are excluded due to a lack of comparability and linking difficulties, leaving a final linked sample of 2,139,624 workers. Individuals working in unidentified occupations or not recorded as participating in the labor force are also excluded from the main occupational income analysis as occupational mobility is difficult to measure and compare if there is not a recorded occupation in both periods.[20] However, I consider the occupational outcomes for workforce non-participants in 1880 in a separate sub-analysis, and include these individuals when analyzing employment outcomes.

State Economic Areas (SEAs) are used as the main geographic unit of analysis. SEAs are groups of economically linked counties (as defined by the US Census Bureau) generally considered to be the historical local labor market, equivalent to 'commuter zones' in modern studies (Abramitzky et al. 2022).[21] Place of birth for foreign-born individuals is recorded at the country level, which is used to calculate share of Chinese and non-Chinese foreign-born workers by SEA. Summary statistics on individual and SEA characteristics can be found in Table 12.

As income data was not recorded until the 1940 Census, occupational income score is the main labor market outcome variable in this study.[22] The measure is widely used in labor market analysis from this period as a proxy for income (Abramitzky et al. 2012, Biavaschi et al. 2017, Abramitzky et al. 2022, Ward 2020, Price et al. 2020). Occupational income score is calculated as the national median income within a given occupation in 1950. Detailed occupation and industry is recorded for all individuals with over 300 occupation codes and over 150 industry and sub-industry codes.[23] The coding is harmonized to the

[19]Full-count Census data is provided through IPUMS USA using US Census Bureau source data (Ruggles et al. 2021). This data is restricted but can easily be accessed free of charge through an IPUMS account and data agreement.

[20]An individual could be considered a non-participant for a number of reasons, including being retired, in school, or, as is mostly the case, simply that the question was left blank in the Census. Around 7.1% of the final linked sample was recorded as a non-participant or in an unidentified occupation in 1880.

[21]I henceforth treat SEAs as the historical labor market and use the two terms interchangeably. However I also use counties instead of SEAs as the geographic unit in robustness checks.

[22]Other measures of occupational standing are also used for robustness (see Section 8).

[23]I collapse industry into 9 main categories for ease of matching: agriculture, fishing and forestry, mining, manufacturing, construction, transport and telecommunications, trade and retail, other services, public

1950 Census classification to ensure historical comparability across Census years.[24] The key deficiency of occupational income score is that it fails to account for within-occupation income shifts, particularly between regions. Since income improvements are only observed through occupational switching, the measure can be interpreted as occupational mobility. Thus, while it may represent a poor proxy for short-term changes in income, it represents a reasonable proxy for long-run changes in "permanent" income (Abramitzky et al. 2014). I consider employment outcomes using a simple binary variable equal to one when the individual has a recorded occupation. I use two main proxies for human capital: literacy and occupational education score. Education score is defined as the percentage of workers within a given occupation code that have completed at least one year of college, derived from the 1950 Census.[25] I define low-skilled workers as the bottom quintile of occupational education score.

For industry output, capital and wage data, I use US Industry Censuses for agricultural, manufacturing and mining industries (Haines et al. 2018, Haines 2010, Day 1892). This provides the total output value and capital used in production (defined as the value of machinery and equipment per unit of production) by industry at the county level.[26] The US Mining Census additionally provides the average wage of miners as well as mine managers by county. However, the Mining Census is only available in 1890, while the Agriculture and Manufacturing Censuses are available in 1880, 1890 and 1900. Industry employment levels (total and by nativity) are calculated from employment data in the full-count Population Censuses.

4.2 Measuring Exposure to the Chinese Exclusion Act

Identifying the causal impact of the Chinese Exclusion Act on native labor markets is challenging given that the Act was implemented nationwide concurrently. I address this by utilizing variation in a labor market or industry's exposure to the policy based on pre-1882 distribution of Chinese immigrants. Chinese laborers were heavily concentrated in certain SEAs, but over 80% of SEAs had less than 0.01% Chinese share in 1880. Thus the labor market effects of the exclusion were likely localized to SEAs with a substantial Chinese share of the labor force. Chinese workers also tended to be heavily concentrated in certain industries within an SEA, allowing me to refine exposure even further.

Therefore I use two main treatment definitions for exposure to the Chinese Exclusion Act. The first (Equation 1) is an overall labor market exposure ($ChineseSEA$), where individual i with 1880 residence in SEA j is considered exposed to the exclusion if the 1880 Chinese

administration, and no industry (either not participating or left blank).

[24] There is concern that income measures from 1950 income data may be systematically different from occupational income during the 1880-1900 period. To address this I validate occupational income score in Appendix A.2 using average occupational income data from the 1903 'Workers of the Nation' encyclopedia. I find that the two measures are highly correlated and the results do not significantly differ when using 1903 occupational income rather than the Census occupational income score.

[25] See the IPUMS User Guide for full information on occupational codes and the construction of the various occupation standing measures (https://usa.ipums.org/usa/chapter4/chapter4.shtml).

[26] Counties are smaller units than SEAs, with around 3,000 counties and 500 SEAs. Counties are always fully contained within a certain SEA as SEAs are defined by the Census Bureau as economically linked counties.

share of the male working age population in SEA j was above 5%. The second (Equation2) is an industry-specific exposure (*ChineseInd*), where individual i working in industry k within SEA j is exposed if the 1880 Chinese share of males employed in that industry and SEA was above 5%.[27] Both these definitions are independent of the individual's SEA residence or industry employment in 1900.

$$ChineseSEA_{ij,1880} = \mathbb{I}\left(ChinesePop_{j,1880}/TotalPop_{j,1880} \geq 0.05\right) \tag{1}$$

$$ChineseInd_{ijk,1880} = \mathbb{I}\left(ChineseEmploy_{jk,1880}/TotalEmploy_{jk,1880} \geq 0.05\right) \tag{2}$$

The treatment is discretized to facilitate matching, however a continuous treatment variable is used for robustness, along with an alternative treatment threshold of 10%. Partially treated SEAs or industries (defined as Chinese shares above 0.05% but below the treatment threshold) are excluded from the analysis. This is to ensure a sharp treatment break to help satisfy the CITVA (Conditionally Independent Treatment Value Assumption), which is essentially the continuous treatment version of the SUTVA, as proposed by Iacus & King (2012).

Using the main 5% treatment threshold, a total of 36 of the 429 SEAs are considered exposed to the Chinese Exclusion Act, containing 44,571 individuals in the final linked sample (around 2.1% of the total). Figure 1 shows the map of the exposed SEAs using different treatment thresholds (the SEAs in the top two quadrants of Chinese share are above the main 5% threshold).

4.3 Coarsened Exact Matching Methodology

Table 13 reports the treatment balance and confirms significant baseline differences at both the individual and labor market level. All differences are statistically significant due to the large sample size, but not all are economically significant. The economically meaningful differences lie predominantly in industrial composition. Treated workers are far more likely to work in mining, and less likely to work in agriculture. At the labor market level, treated SEAs also have substantially more non-Chinese foreign labor, indicating that these labor markets attracted more foreign labor in general.

To overcome these significant baseline differences, I implement a coarsened exact matching (CEM) methodology, as proposed by Iacus et al. (2012) on both individual and labor market pre-treatment characteristics. This allows the data to construct appropriate counterfactuals of very similar workers facing very similar labor markets. SEAs are matched on coarsened values of total foreign-born labor force share (Chinese and non-Chinese) and exact binary values of urban status and presence of major agricultural, mining and manufacturing industries.[28][29] Individuals across matching SEAs are then matched on coarsened values

[27] Chinese share and foreign-born shares are calculated using the full male working age population in 1880, irrespective of listed occupation or whether the individual is linked.

[28] Coarsening involves breaking up continuous variables into 'blocks' (whose number and size is optimally determined by the CEM algorithm), then matching within the blocks. See https://gking.harvard.edu/cem for discussion on the CEM method and accompanying code.

[29] Labor market characteristics are broken into binary designations for more intuitive and efficient matching. An SEA is designated as urban if more than 50% of the working age population is recorded as living in an

of age and exact values of occupation code, industry and urban/rural residence. This results in incredibly fine matching with over 150,000 matching strata and ensures the analysis compares individuals with near identical labor characteristics facing similar labor markets.

Individuals are not matched on 1880 occupational income score as matching on pre-treatment outcomes in a difference-in-difference setting can produce biased estimates (Daw & Hatfield 2018). However matching on pre-treatment occupation and industry codes does in practice result in matched individuals having the same pre-treatment occupational income score. There is also evidence to suggest that matching on time-varying covariates can also create biased estimates (Chabé-Ferret 2017). Therefore matching over largely time-invariant covariates (i.e. excluding occupation/industry variables) is considered for robustness.

5 Empirical Strategy

5.1 Estimating Labor Market Effects

A two-way fixed effects framework with CEM matching weights is used to identify the causal impact of the Chinese Exclusion Act on long-run native occupational mobility.[30] The estimating equation is:

$$y_{ijkt} = \alpha_j + \gamma_t + \beta_1 Chinese_{jk} \times 1882_t + \beta_2 Foreign_{jk} \times 1882_t + \epsilon_{ijkt} \tag{3}$$

where y_{ijkt} is the log occupational income score of native worker i at Census decade t who resides in SEA j and works in industry k in 1880.[31] Employment, literacy and log education score are also used as dependent variables in the separate analyses.[32] α_j and γ_t are SEA and Census decade fixed effects respectively. $Chinese_{jk}$ is the binary treatment variable equal to one if SEA j or industry k within SEA j (depending on the level of exposure used) has a significant Chinese share in 1880. $Foreign_j$ is the total foreign share of the working-age male population SEA j or industry k within SEA j in 1880. 1882_t is an indicator variable equaling one post 1882. Therefore β_1 is the coefficient of interest capturing the effect of exposure to the Chinese Exclusion Act on log occupational income score relative to matched but unexposed individuals. Standard errors are clustered at the 1880 SEA level.[33] The CEM matches are applied using importance weights on individuals to ensure comparisons between matched individuals. I also run SEA-level analyses to analyze migration and industry responses to the exclusion. The estimating equation used is the same as in Equation 3, except with SEAs

urban area, which the Census defines as a town with greater than 2,500 inhabitants. The presence of a major industry (agricultural, mining and manufacturing) is based on the share of working age male employed in that industry: above 50% for agriculture, 10% for manufacturing and 5% for mining. Maps illustrating the SEA designations can be found in Appendix B.

[30] The combination of two-way fixed effects with CEM can be interpreted as a form of non-parametric difference-in-difference methodology.

[31] The j and k subscripts are fixed at 1880 values to allow for endogenous migration response.

[32] As employment and literacy are binary dependent variables, the equation in these cases are the same but using a probit version.

[33] I also consider standard errors that account for spatial correlation between individuals in different SEAs, as proposed by Conley (1999), however the Conley standard errors only slightly higher (less than 20%) than the clustered standard errors.

as the observations rather than individuals, thus dropping the i subscript. CEM is still used, but matching only on SEA-level characteristics.

The key identifying assumption is that, conditional on the matching procedure and existing foreign-born share, trends in occupational income score would not have diverged between individuals in labor markets with different Chinese immigrant shares in the absence of the Chinese Exclusion Act. Conditioning on foreign-born share rules out any potential violation of the parallel trends assumption that would impact all immigrants, while CEM rules out differential trends based on industrial composition or other individual-level characteristics. I also run an analysis that limits the sample to the Western Census region to account for regional differential trends.

To validate the identifying assumption, I consider a placebo legislation date utilizing the 1878 veto of the Chinese Exclusion Act. I use the same empirical strategy, but with 1870 as the pre-treatment Census period and 1880 as the post-treatment period. This involves linking individuals between the 1870 and 1880 Censuses, then matching individuals with the same CEM covariates, however maintaining the same SEA and industry treatment designations from the main analysis. Thus, the placebo test is essentially equivalent to running a pre-trends analysis at the SEA or industry level, but composed of different individuals.

5.2 Estimating Native/Immigrant Substitutability

While I do not have the data to directly estimate any native/immigrant elasticities, I can infer the degree of substitutability by analyzing the changing ethnic composition of occupations as a result of the Chinese Exclusion Act. Namely, if the jobs vacated by displaced Chinese laborers are largely filled by native-born laborers, this would imply the two groups are substitutes in those occupations. To estimate this, I create treatment units based on occupation/industry/SEA groups (e.g. machinists employed in manufacturing in Queens, NY) and calculate the number of native, Chinese and other foreigners working in each group in the 1880 and 1900 Censuses. I drop groups with less than 10 workers in either period. I use an equivalent treatment definition that a group is treated if the Chinese share of individuals employed is greater than 5%. I then match on exact occupation and industry. The estimating equation is the same as Equation 3, except that y_{ijkt} is the number of native, Chinese or other foreigners (indexed at 1880 levels) working in occupation i in SEA j in industry k in Census decade t. Therefore the coefficient of interest β_1 now represents the percentage change in the number of workers in occupation groups with significant Chinese share in 1880 relative to the groups with exact same occupation and industry, and similar total foreign share, but without Chinese workers.

5.3 Estimating Wage Effects

For the wages analysis, the data is only given at the county level for a single period (1890). Therefore a simple OLS framework is used without matching:

$$y_{cj} = \beta_0 + \beta_1 Chinese_{j,1880} + \beta_2 Foreign_{j,1880} + \mathbf{X}_c \gamma + \epsilon_{cj} \tag{4}$$

where y_{cj} is the hourly wage index paid in county c in SEA j. $\mathbf{X}_c$ is a vector of controls including total production in the county and dummies variables for type of mineral mined in the county (either coal, iron ore or gold/silver for which there is data), to account for potential wage differences based on mining type. Standard errors are clustered at the SEA level. I analyze both low-skilled (miners) and high-skilled (mining managers) wages, but the main specification uses the ratio of low-skilled to high-skilled wages as the dependent variable. This allows me to control for regional wage differences and any confounding factors that would affect wages in mining industries generally.

5.4 Estimating Dynamic Effects

I estimate dynamic cohort effects of the Chinese Exclusion Act using new labor market entrants and the early childhood analyses. For both these I create new linked samples and apply an OLS version of the main estimating equation, as new entrant and early childhood analyses naturally do not have pre-treatment occupation or income.

I define new entrants as individuals who turn eighteen in the period 1882-1898 (i.e. born 1864-1880) and are not recorded as working in the 1880 Census. I use the same Census linking methodology to link these individuals between the 1880 and 1900 Census, creating a final linked sample of 2,444,280 individuals. I also apply the same CEM methodology, but without the occupation or industry covariates. I apply the matching weights to the OLS framework:

$$y_{ij} = \beta_0 + \beta_1 Chinese_j + \beta_2 Foreign_j + \beta_3 PopIncome_i + \epsilon_{ij} \tag{5}$$

where y_{ij} is the 1900 log occupational income score of individual i residing in SEA j in 1880. The other variables have the same definitions as above, with the additional control for father's income in 1880. Standard errors are clustered at the SEA level. β_1 represents the relative difference in log occupational income between workers who entered similar labor markets, but exposed and unexposed to the Chinese exclusion. I run the regression with different market entry cohorts to trace out the year-to-year labor market entry effects.

I define the early childhood exposure period as aged 0-8 or in utero, similar to other early childhood income shock studies (Smith 2015). I use recorded household linkages in the full-count 1900 Census to identify the fathers of all children with early childhood during the period 1882-1900 (i.e. born 1875-1900).[34] I then attach the father's treatment status and matching weights from the main analysis to these children. Income is recorded in the 1940 Census, hence I use the same Census linking method to link these children to the 1940 Census to obtain their adult income. The sample size of individuals linked between 1900 and 1940 as well as linked to a father is 829,249. The father's matching weights ensure comparisons between similar fathers and negating the need for father-level controls. This is applied to a basic OLS framework:

$$y_{if} = \beta_0 + \beta_1 Chinese_{f,1880} + \beta_2 Foreign_{f,1880} + \epsilon_{if} \tag{6}$$

where y_{if} is the log weekly income in 1940 of individual i born to father f. $Chinese_{f,1880}$ is the binary treatment variable equal to 1 if father f was exposed to the Chinese Exclusion

[34] The Census only records father-child links for those living in the same household. This limits how far I can link back as those born prior to 1875 (thus over 25 in 1900) are unlikely to still be living at home. This also brings potential selection issues into the older cohort in the analysis as these cohorts are old enough to move out of home prior to 1900.

Act as defined above. $Foreign_{f,1880}$ is the total foreign share in father f's SEA in 1880. Standard errors are clustered at the SEA level. β_1 represents the relative difference in log adult earnings between the children of treated and untreated, but otherwise similar fathers. As above, this analysis is run with different birth cohort to trace out the dynamic early childhood exposure effects.

6 Results

6.1 The Effect of the Chinese Exclusion Act on Native Labor Market Outcomes

Table 1 reports the estimated impact of the Chinese Exclusion Act on long-run occupational income using labor market-level and industry-level exposure measures. Column 1 reports the change in the Chinese share of working age males in exposed SEAs and industries, essentially a first-stage estimate of the effects of the Chinese Exclusion Act. The results confirm the exclusion had a sizable and significant impact on labor supply in markets and industries with high Chinese share prior to 1882, consistent with the historical evidence. Exposed SEAs experienced an average decrease of 8.2 percentage points in Chinese share from a pre-treatment average of 14.8, or a 55.4% decrease. The effects are even larger for exposed industries, decreasing 17.5 percentage points from a pre-treatment average of 22.9%, or a 76.4% decrease.

Column 2 reports the results using the difference-in-difference framework without matching and shows no significant effect of the exclusion on occupational mobility. However the inclusion of matching weights in Column 3 finds significant negative effects of the exclusion on occupational mobility in both exposure definitions, with a larger effect for industry exposure. Column 4 refines the analysis further by restricting the sample to the Western Census region. This substantially increases the magnitude and significance of the estimates, with industry exposure again having the larger effect. These estimates are large in magnitude, with SEA- and industry-level estimates being 0.15 and 0.4 standard deviations lower respectively, corresponding to a 6-15% decrease in occupational income. Comparing Columns 2-4 suggests differential baseline characteristics and regional labor market trends create upward bias in the estimates. Therefore failing to control for these potentially underestimates the negative impact of the Chinese Exclusion Act of occupational mobility.

Column 5 reports the results of a single regression that includes both treatment variables in the same specification. Since industry exposure is almost always within an exposed labor market, the coefficient on the SEA treatment variable can be interpreted as the effect of the exclusion on workers residing in high-Chinese SEAs but not actually working in the high-Chinese industries within the SEA. Meanwhile the industry treatment variable coefficient is the difference between those workers and ones that are working in Chinese-dominated industries within the SEA. Therefore, the results suggest that the negative effects of the exclusion were limited to those working in industries with Chinese laborers, with minimal aggregate demand or spillover effects onto other individuals in the labor market. This would also explain why the industry exposure estimate is consistently more negative than the SEA exposure estimate.

I also examine the effect of the Chinese Exclusion Act on native employment in Table 14. However, I find no significant effects under any specification, suggesting the negative labor market effects are being driven by slower occupational mobility rather than unemployment.

6.2 Heterogeneous Effects by Age, Skill and Unemployment

I explore potential individual heterogeneity in treatment effects by analyzing young, low-skilled, new, and unemployed workers. I define young workers as aged between 18-30 in 1880 and new market entrants as those turning 18 after 1882 and not working in 1880. For low-skilled workers, I examine both the bottom quintile of the occupation education score and illiterate workers in 1880. While unemployed workers are not identified in the Census by modern definitions, I instead analyze workforce non-participants, defined as workers with a zero occupation income score in 1880, excluding workers with undefined occupations.

Table 2 reports the estimates for these sub-groups. While young workers have a similar coefficient magnitude to the full sample coefficient of -0.012 (although no longer statistically significant), both measures of low-skilled workers have significant negative treatment effects that are substantially larger in magnitude than the baseline. The effects are strongest for illiterate workers, which is consistent with this group representing the lower tail of the skill distribution.[35] The estimates for workforce non-participants and new entrants are measured using a single-period model as they do not record a pre-treatment occupation by construction, and as such the estimates are not directly comparable to the baseline estimate. Nevertheless, the results remain highly significant and large in magnitude, suggesting the effects are likely stronger for these groups as well.

Figure 3 presents the treatment effects by occupational education score quintiles. This demonstrates significant heterogeneity by skill quintile in an inverted U-shaped pattern, with the lowest and highest quintiles experiencing the strongest negative effects.[36] The results for the highest quintile are consistent with findings that low-skilled immigrants typically complement high-skilled occupations (Okkerse 2008, Cattaneo et al. 2015). However, the results for the lowest skill quintile represents a conundrum, as native and foreign-born workers have typically been found to be substitutes, or at least neither complements nor substitutes, in low-skilled occupations (ibid.). The figure also raises the question why the middle skill quintile is better off under the exclusion. However, the middle quintile consists overwhelmingly of farm owners, for which occupational income is not a realistic measure of earnings, given that they typically do not switch occupations. Therefore, the results are not likely accurate for this quintile.

7 Mechanisms: The "Honeypot" Effect

Next I turn to an examination of mechanisms to explain the negative long-term effects of the Chinese Exclusion Act. Specifically, I propose a potential mechanism I term the "honeypot" effect, where the Chinese exclusion led to a temporary increase in low-skilled wages in Chinese-exposed markets, which attracted relatively more native workers to low-skilled

[35] Only 5.6% of workers in the sample were recorded as illiterate in 1880.

[36] The breakdown of the most frequent occupations within each quintile can be found in Table 15.

Table 1: Effect of Chinese Exclusion Act on Native Occupational Mobility

		Log Occupational Income Score			
	Chinese Share (1)	No Matching (2)	Matching (Full) (3)	Matching (West Only) (4)	Both Exp. Included (5)
A. SEA Exposure					
Chinese×1882	-0.082^{***}	0.004	-0.012^{**}	-0.066^{***}	0.007
	(0.007)	(0.019)	(0.005)	(0.009)	(0.008)
Constant	-0.035	2.906	2.407	2.253	-
1880 Mean in Exposed SEAs	0.148	2.980	2.891	2.891	-
R^2	0.776	0.008	0.103	0.083	-
N	205	3,363,282	216,674	46,134	-
B. Industry Exposure					
Chinese×1882	-0.175^{***}	-0.028	-0.021^{***}	-0.171^{***}	-0.034^{***}
	(0.014)	(0.028)	(0.008)	(0.010)	(0.010)
Constant	0.031	2.906	2.504	2.520	2.414
1880 Mean in Exposed SEAs	0.229	2.980	2.947	2.947	2.891
R^2	0.728	0.056	0.058	0.111	0.103
N	205	3,363,282	79,648	31,630	216,674
Two-Way FE	Y	Y	Y	Y	Y
Individual-Level	N	Y	Y	Y	Y
Matched Units	Y	N	Y	Y	Y
Western SEAs Only	N	N	N	Y	N
Both Treatment Var.	N	N	N	N	Y

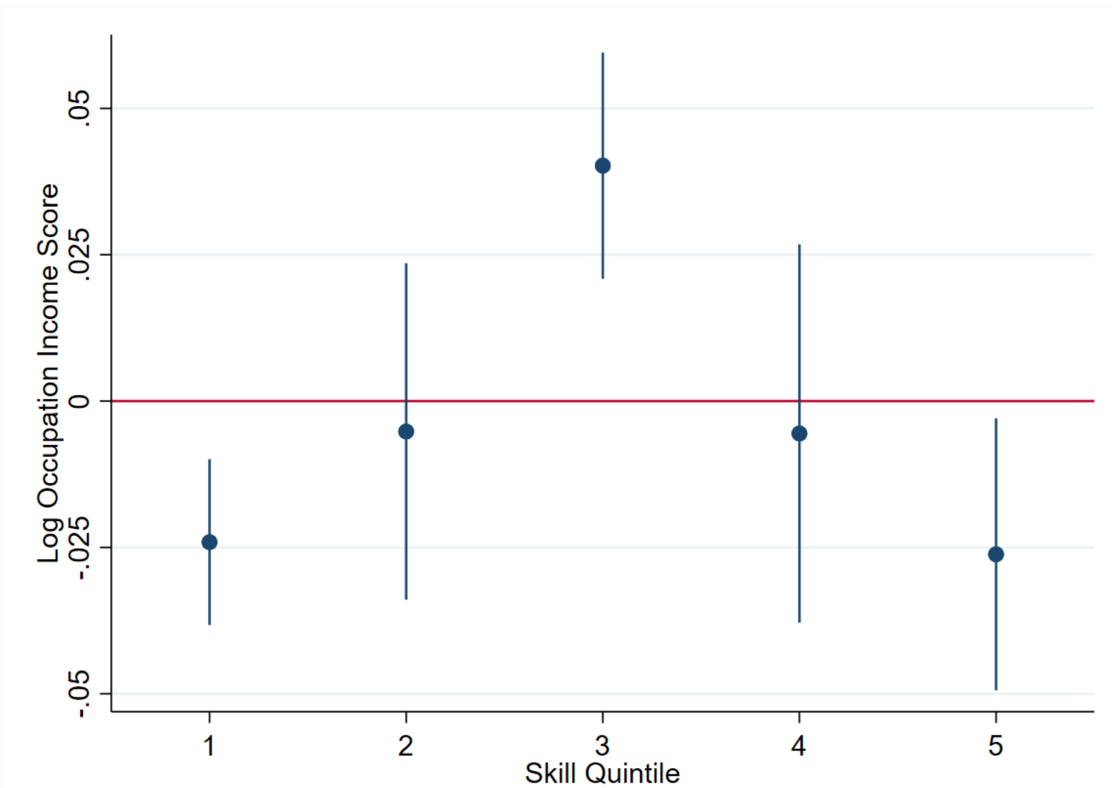
The table reports the effect of exposure to the Chinese Exclusion Act on Chinese share and log occupational income score. *Chinese* in Panel A is the binary treatment variable which equals 1 if the individual resided in an SEA with a Chinese share above 5% in 1880, while in Panel B is the equivalent treatment variable if the individual worked in an industry with a Chinese share above 5% in 1880. 1882 marks Census years post-1882. All regressions use the main estimating equation. Column 1 uses SEA-level observations with matching weights. Column 2 uses the full individual-level sample without matching weights. Columns 3-5 use individual-level observations with matching weights. Column 5 includes both variations of the treatment variable in a single regression. Clustered standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Effect of Chinese Exclusion Act on Exposed Sub-Groups

	Log Occupational Income Score				
	Young Workers (1)	Low-Skilled Workers (2)	Illiterate Workers (3)	Workforce Non-Participants (4)	New Entrants (5)
Chinese×1882	−0.010	−0.022***	−0.086***		
	(0.007)	(0.008)	(0.027)		
Chinese				−0.042***	−0.013***
				(0.014)	(0.003)
Constant	2.275	2.338	2.352	2.848	2.321
Two-Way FE	Y	Y	Y	N	N
Single Period	N	N	N	Y	Y
Matched Individuals	Y	Y	Y	Y	Y
R^2	0.154	0.394	0.094	0.025	0.077
N	121,780	51,854	13,080	10,156	207,772

The table reports the effect of exposure to the Chinese Exclusion Act on log occupational income score of select sub-groups from the full sample. *Chinese* is the binary treatment variable which equals 1 if the individual resided in an SEA with a Chinese share above 5% in 1880. 1882 marks Census years post-1882. Columns 1-3 use the main estimating equation, while Columns 4-5 use a single period version of the main estimating equation as they do not record income in 1880. All regressions use matching weights. Column 1 excludes workers over the age of 30 in 1880. Column 2 only includes workers in the bottom quintile of occupation education score in 1880. Column 3 excludes literate workers in 1880. Column 4 includes only workers with a zero occupational score in 1880 with the exception of undefined occupations. Column 5 includes who entered the labour market (defined not recording income in 1880 and turning 18) after 1882. Clustered standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 3: Effect of Chinese Exclusion Act by Skill Quintile



Notes: The figure shows the estimated treatment effects of the Chinese Exclusion Act on log occupational income score by quintile of occupational education score. The estimate ranges represent 95% confidence intervals, clustered by 1880 SEA.

occupations, dissuading them improving their human capital and pursuing higher-skilled occupations. The "honeypot" mechanism also helps to explain the individual heterogeneity by skill and unemployment. If low-skilled workers and workforce non-participants benefit most from the low-skilled labor shortage in the short-run, then the "honeypot" effect would be stronger for these groups, resulting in slower long-run occupational mobility.

To provide evidence for the proposed "honeypot" effect, I break down the mechanism into several parts. First, I estimate native/immigrant substitutability and provide suggestive wage evidence to show the short-term benefits. Second, I analyze the effects on human capital attainment. Third, I examine the long-run migration and industry response to the exclusion. Finally, I perform a dynamic cohort analysis to trace out the short-run to long-run labor market effects.

7.1 Native/Immigrant Substitutability

A fundamental factor determining the native labor market outcomes as a result of immigrant restriction is the degree of substitutability between immigrant and native labor in low-skilled occupations. Table 3 reports the results of the substitutability analysis. Column 1 reports the change in number of natives employed within occupation/industry/SEA groups indexed at 1880 levels. Groups that had a significant Chinese share employed in 1880 ended up employing significantly more natives as a result of the Chinese Exclusion Act than groups with the same occupation and industry and similar total foreign employment but without Chinese workers. This indicates a high degree of substitutability between Chinese and native workers. This is consistent with qualitative evidence suggesting that native laborers of this period were highly comparable and substitutable with immigrant laborers in skills, legal working rights and labor regulations (Abramitzky & Boustan 2017). Furthermore, given that Chinese laborers predominantly worked in low-skilled occupations, the results suggest the Chinese Exclusion Act did push native workers into lower-skilled occupations. Column 2 reports the same but with changes in non-Chinese foreigners employed. The coefficient is positive, but much smaller than the native coefficient and not statistically significant, suggesting most of the shortage created by the Chinese exclusion was actually filled by natives rather than other unrestricted immigrants. Column 3 reports the changes in the total number of people employed. The results suggest there was no significant effect on the total employment within these groups, suggesting there was no long-term contraction in these occupations and Chinese labor was more or less fully substituted in the long-run.

7.2 Wage Effects

While I do not directly observe wages or any short-run economic data, I use average mining wages by county in 1890 for evidence of low-skilled wage increases. Table 4 reports the effects of the Chinese Exclusion Act on average mining wages in 1890 by county. Column 1 shows counties within SEAs that had a sizeable Chinese share in 1880 had significantly higher low-skilled mining wages in 1890 than counties in SEAs with similar 1880 total foreign share but without Chinese. These effects are substantial in magnitude (and somewhat unrealistic): hourly wages are \$1.10 higher in Chinese-dominated labor markets (compared to a national average of \$2.16), or 53.8 percentage points higher than the national average.

Table 3: Native/Immigrant Occupation Substitutability

	Native Employed (1)	Non-Chinese Foreign Employed (2)	Total Employed (3)
Chinese×1882	1.945***	1.083	0.792
	(0.645)	(0.746)	(0.741)
Two-Way FE	Y	Y	Y
Matched Units	Y	Y	Y
R^2	0.020	0.003	0.024
N	65,358	65,358	65,358

The table reports the effect of the Chinese Exclusion Act on the numbers workers employed within occupation/industry/SEA groups by ethnicity, indexed at 1880 levels. Column 1 reports the changes in the native population employed. Column 2 reports the same for non-Chinese foreign population employed. Column 3 reports the same for total population employed. *Chinese* is the binary treatment variable which equals 1 if the Chinese share in the SEA was above 5% in 1880. All regressions use the main estimating equation with exact matching between occupation/industries. Clustered standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

However, Column 2 shows high-skilled mining wages are also significantly higher, although a smaller increase than the low-skilled wages. This would suggest that most of the increase in low-skilled wages can be attributed to pre-treatment level differences in mining wages between counties with and without Chinese workers. Column 3 reports the effect on the low-skilled/high-skill mining wage ratio, which addresses level differences between counties. The wage ratio is 4.8% higher (roughly a third of a standard deviation), suggesting . This is a meaningful effect size (and more realistic than the previous magnitudes), but not significantly significant at a 10% level.

As the wage data is at the county level and only covers a single industry in a single post-period without distinguishing between native and immigrant wages, the results are only suggestive of the wage effects of the Chinese Exclusion Act. The mining industry was arguably the industry most impacted by the exclusion as the industry with the highest proportion of Chinese laborers. Thus the wage effects in this industry are informative, but unlikely to be as large in other industries.

Nevertheless, combined with the evidence of high native/immigrant substitutability, the results appear to corroborate the first stage of the "honeypot" effect: the labor shortage caused by the Chinese exclusion likely increased low-skilled wages, which attracted native workers to these low-skilled occupations.

Table 4: 1890 Mining Wages by County

	Low-Skill Wages (1)	High-Skill Wages (2)	Low-Skill/ High-Skill (3)
Chinese SEA	0.538***	0.398***	0.048
	(0.087)	(0.089)	(0.044)
Foreign Share	0.050	0.100	−0.029
	(0.101)	(0.125)	(0.095)
Total Production	0.022***	0.022***	0.005
	(0.004)	(0.005)	(0.006)
Gold/Silver	−0.146	−0.102	−0.021
	(0.193)	(0.162)	(0.111)
Coal	0.365**	0.289**	−0.006
	(0.168)	(0.129)	(0.115)
Constant	0.585	0.656	1.020
R^2	0.275	0.187	0.009
N	251	233	233

The table reports the effect of the Chinese Exclusion Act on average mining wages by county. The dependent variable is the hourly wage of mining laborers (considered low-skilled) in Column 1, the hourly wage of mining managers (considered high-skilled) in Column 2, and the ratio between low-skilled and high-skilled wages in Column 3. All wages are indexed to the national average. $ChineseSEA$ is the binary treatment variable which equals 1 if the county is within an SEA with a Chinese share above 5% in 1880. $Foreign$ is the total foreign-born share of the labor force in 1880. $TotalProduction$ is the annual weight of the minerals extracted in the county. $Gold/Silver$ and $Coal$ are binary variables for the type of mining in the county. Clustered standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

7.3 Human Capital Acquisition

Next I examine whether this decrease in the skill premium impacted the human capital attainment of exposed native workers. As Census data does not directly record education, I use literacy and occupational education score as proxies for human capital. Table 5 reports the effects of the Chinese Exclusion Act on these education proxies. Column 1 and 2 show that in both measures of Chinese exposure, illiterate native workers exposed to the exclusion were significantly less likely to become literate by 1900.[37] It should be noted that as the average literacy rate in the sample was 93% in 1880, the results can only provide insight for

[37]Columns 1 and 2 report the results of a probit regression, therefore the coefficients should not be directly interpreted as marginal effects.

Table 5: Human Capital Effects of the Chinese Exclusion Act

	Literacy		Log Occupational Education Score	
	(1)	(2)	(3)	(4)
Chinese×1882	-0.265^{***}	-0.178^{***}	0.013	-0.106^{***}
	(0.028)	(0.042)	(0.009)	(0.021)
Constant	1.229	1.288	1.249	1.569
Two-Way FE	Y	Y	Y	Y
SEA Exposure	Y	N	Y	N
Industry Exposure	N	Y	N	Y
Matched Individuals	Y	Y	Y	Y
R^2	0.015	0.017	0.055	0.040
N	286,950	97,890	216,674	79,648

The table reports the effect of exposure to the Chinese Exclusion Act on proxies of human capital. The dependent variable of Column 1 and 2 is a binary variable equal to 1 if the individual can read and write. The dependent variable of Column 3 and 4 is the log occupational education score. Chinese is the binary treatment variable which equals 1 if the Chinese share was above 5% in 1880. Column 1 and 2 use a probit version of the main estimating equation with coarsened exact matching weights, while Columns 3 and 4 use the non-probit version. Clustered standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

the left tail of the human capital distribution.

For insights on the whole human capital distribution, Column 3 and 4 report the effects of the Chinese Exclusion Act on log occupational education score. While there is no significant effect at the labor market exposure level, there is a highly significant negative effect at the industry exposure level, suggesting workers in affected industries tended to end up in less educated occupations as a result of the exclusion. The difference in results between the two measures of exposure, similar to the main results in Table 1, reiterate that the "honeypot" effect seems to be predominantly impacting those who already worked in the affected industry rather than drawing in native workers from other industries. Overall, the results suggest that the decrease in the skill premium in exposed labor markets and industries did disincentivize upskilling, leading to lower human capital in the long run.

7.4 Migration and Industry Response

Higher low-skilled wages as a result of immigration restrictions are unlikely to be sustainable in the long run. Increased in-migration to affected labor markets has been shown to be one of the main long-run equilibrating mechanisms (Price et al. 2020, Abramitzky et al. 2022). Table 6 reports the effect of the Chinese Exclusion Act on working-age shares of different ethnicities and log total working-age population. Column 1 reports the change in the Chinese share of working age males, as already reported in Table 1. Column 2 reports

Table 6: Migration Response to the Chinese Exclusion Act

	Chinese Share (1)	Non-Chinese Foreign Share (2)	Native Share (3)	Log Total Population (4)
Chinese×1882	-0.082^{***}	0.069^{***}	0.013	0.339
	(0.007)	(0.019)	(0.018)	(0.293)
Constant	-0.035	0.035	1.000	9.300
1880 Mean in Exposed SEAs	0.148	0.311	0.542	8.875
Two-Way FE	Y	Y	Y	Y
Matched SEAs	Y	Y	Y	Y
R^2	0.776	0.773	0.763	0.271
N	205	205	205	205

The table reports the changes in ethnic shares and log total of the male working-age population by SEAs as a result of the Chinese Exclusion Act. Chinese is the binary treatment variable which equals 1 if the Chinese share was above 5% in 1880. All regressions use the main estimating equation with coarsened exact matching weights. Clustered standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

the change in the non-Chinese foreign-born share, i.e. unrestricted migrants. There is a large significant increase in the share of non-Chinese migrants in exposed SEAs relative to similar unexposed SEAs, about 22% higher relative to the 1880 mean. Column 3 also shows a smaller, but statistically insignificant increase in the native population share. This is consistent with evidence that immigrants in the US are far more mobile than natives (Sequeira et al. 2020), thus are more likely to migrate to take advantage of labor shortages. Column 4 also shows no significant change in the log total working age population for exposed labor markets relative to unexposed ones. This implies that any labor shortage caused by the Chinese exclusion was roughly equalled by increased in-migration in the long-run, mostly by unrestricted immigrants.

The response of industry to low-skilled labor shortages has also consistently been shown to be an important factor in determining long-run labor market outcomes, both through capital substitution (Lafortune et al. 2015, Lew & Cater 2018, Clemens et al. 2018) and negative agglomeration effects shrinking industry output (Lee et al. 2022, Long et al. 2022). Table 7 reports the effect on the relative capital intensity, total and native employment, and output for mining, manufacturing and agricultural industries. Column 1 reports the capital changes, and across all three industries there is evidence of increasing capital use in markets exposed to the Chinese exclusion. However, this is a marginally significant or insignificant increase in mining and manufacturing industries; only agriculture has a highly significant increase in relative capital. Column 2 reports the total employment changes, and despite evidence of capital increasing, there is no evidence of decreased employment levels to suggest capital substituting labor. Similarly, exposed markets have positive, but insignificant, changes in native employment, as shown in Column 3. Finally, Column 4 reports the changes in log total output, and all three industries show some evidence of slightly shrinking as a result of the Chinese exclusion, however these decreases are not statistically significant.

Overall, while there were small increases in capital usage and small decreases in output, the lack of decreasing industry employment suggest neither capital substitution nor negative agglomeration had a substantial impact on worker outcomes, in contrast to previous findings. The reasons for this contradiction are likely two-fold. Firstly, a lack of widespread capital substitution may simply be the result of a lack of labor-saving technology during the time period. Agricultural and manufacturing industries would not be revolutionized by tractors and mass production systems respectively until the early 1900s (Lew & Cater 2018, Hunter & Bryant 1979), while the mining industry was heavily reliant on manual labor until the 1940s (Dix 1988). Secondly, dynamic analysis by Long et al. (2022) suggests industries did not significantly shrink as a result of the Chinese Exclusion Act until at least 1910. So while there is evidence of the early stages of capital substitution and negative agglomeration, the twenty year time-frame for this study is likely too short for these effects to fully manifest and significantly impact workers.

7.5 Dynamic Cohort Analysis

I conclude by examining different cohort's labor market outcomes to infer the short-run to long-run dynamic effects of the Chinese Exclusion Act. I break this down into two separate cohort studies. The first uses variation in the move into working age to infer the labor market conditions in the year of entry. The second uses variation in early childhood exposure to infer the father's relative economic welfare during the early childhood period.

7.5.1 Labor Market Entrants

Figure 4 presents the effects of the Chinese Exclusion Act on occupational income in 1900 by two-year cohorts. Workers who entered labor markets exposed to the exclusion shortly after its implementation had substantially higher occupational income in 1900, suggesting that the Chinese Exclusion Act created favorable labor market entry conditions in the short-run. This initial gain quickly diminishes, however, and workers who entered exposed labor markets in the decade 1886-1895 were consistently worse off relative to workers entering similar, but unexposed SEAs. After 1895, the labor market appears to return to equilibrium with no significant differences in the entry conditions.

Tracking labor market entry conditions can provide insight into the 'honeypot' effect as many entry-level occupations are the low-skilled occupations most impacted by the Chinese exclusion. The initial favorable entry conditions represent the so-called "honeypot": sudden labor shortages create improvements in entry-level occupations that the analysis suggests lasted around 3-4 years. However, as natives increasingly move and stay in these low-skilled occupations, coupled with the migration and capital response, the labor market conditions become unfavorable for entrants in the medium and long run.

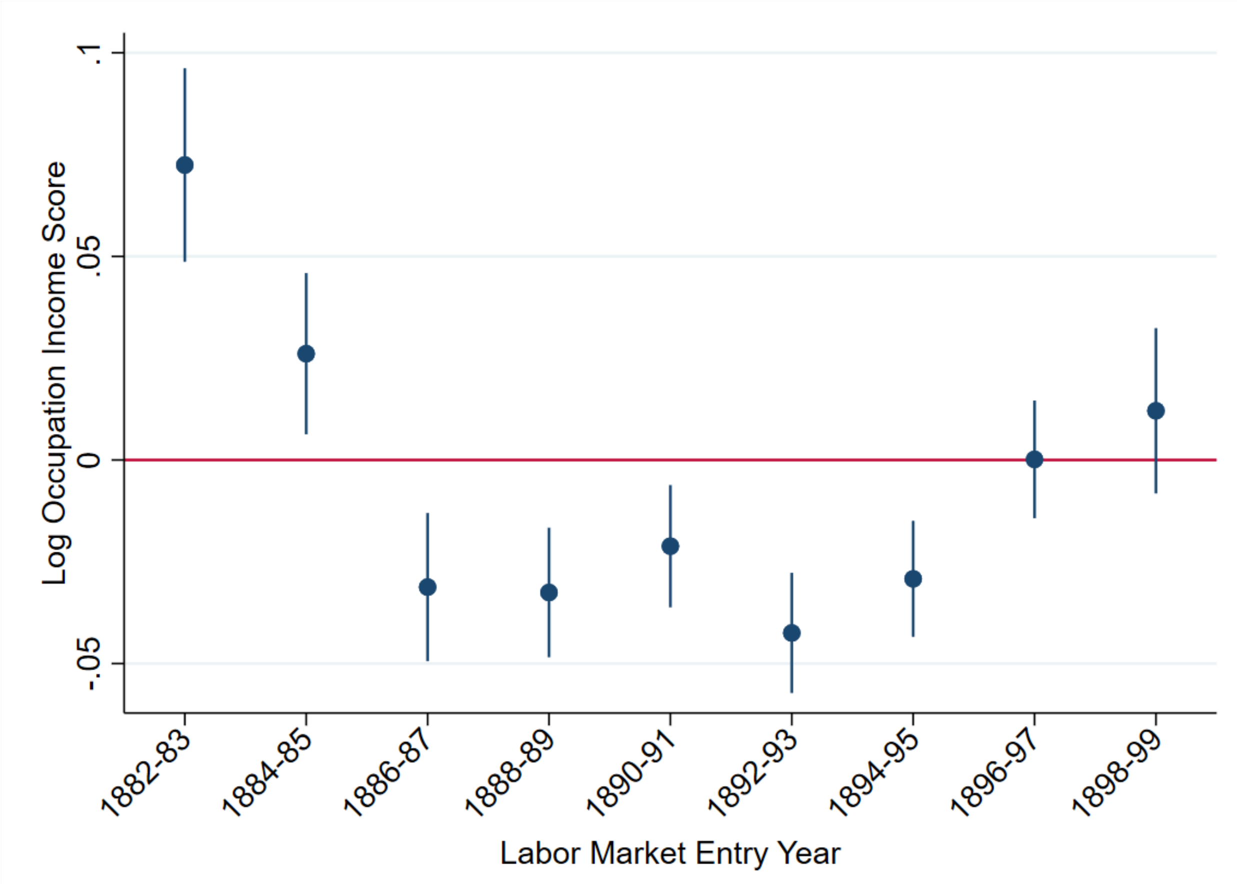
There may concerns that the estimates are imprecise as workers may not enter the market at 18 or in the SEA recorded in 1880. However, the timing and location of labor market entry is itself an endogenous response to changing labor market conditions (von Wachter

Table 7: Effect of Chinese Exclusion Act on Industry Capital, Employment and Output

	Capital Value per Output (1)	Log Total Employed (2)	Log Native Employed (3)	Log Total Output (4)
A. Mining				
Chinese×1882	0.598*	−0.045	0.778	−0.701
	(0.313)	(0.793)	(0.698)	(0.571)
Mean of Dep. Var.	0.982	4.307	3.619	5.143
B. Manufacturing				
Chinese×1882	0.097	0.464	0.519	−0.061
	(0.069)	(0.406)	(0.400)	(0.244)
Mean of Dep. Var.	0.725	7.254	6.578	12.841
C. Agriculture				
Chinese×1882	0.131***	0.336	0.221	−0.280
	(0.044)	(0.356)	(0.357)	(0.201)
Mean of Dep. Var.	0.223	9.011	8.531	13.315

The table reports the effect of exposure to the Chinese Exclusion Act on the capital, employment and output of the mining, manufacturing and agriculture industries. *Chinese* in Panel A is the binary treatment variable which equals 1 if the SEA has a Chinese share above 5% in 1880, or the county is situated within such an SEA. 1882 marks Census years post-1882. All regressions use a two-way fixed effects model. Column 1 reports the treatment effect on a county's total value of machinery/equipment per unit of output. Columns 2 and 3 report the treatment effect on the log number of people employed in the industry (total and native-born respectively). Column 4 reports the effect on the log total value of output in a county. Clustered standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 4: Dynamic Analysis: Labor Market Entrants



Notes: The figure shows the estimated treatment effects of the Chinese Exclusion Act on log occupational income score in 1900 by labor market entry cohort. The labor market entry year is considered the year and individual turns 18. The estimate ranges represent 95% confidence intervals, clustered by 1880 SEA.

2020). Furthermore, the vast majority of workers in the sample are recorded as residing in the same SEA in 1880 and 1900, consistent with the migration response analysis above showing relatively limited native internal mobility.

7.5.2 Early Childhood Exposure

First, to broadly distinguish between short- and long-run effects, I break the cohorts up into children in early childhood directly after implementation of the Act (i.e. born 1875-1882) and those whose early childhood was at least 10 years after implementation (i.e. born 1892-1900). Table 8 reports the difference in log income in 1940 between children of fathers exposed to the exclusion and children of comparable, unexposed fathers. The cohort who had early childhood exposure immediately after the exclusion had significantly higher relative adult earnings. This is evident for father's exposure at both the labor market level and industry level. These children of exposed fathers are earning roughly 4%, or 0.1 standard deviations, more than children of the same cohort with similar fathers unexposed to the exclusion. However the effect disappears for children born over 10 years after with no significant effect on adult earnings between the two groups. This would suggest there is indeed a significant positive economic shock for native workers in the short-run as a result of the exclusion, but that the exclusion had no significant net economic effect in the long-run.

Second, I break the data into 3-year cohort bands (individual cohort years are too noisy) to visually tracking out the progression of the Chinese exclusion effects. Panel A of Figure 5 displays the different exposure periods for each three year cohort (based on the 0-8 age band), and correspond to the estimated treatment effect for each 3-year birth cohorts in Panel B. There is a large positive effect for cohorts with exposure periods at the time of implementation, but it is fairly noisy, rendering the estimates only marginally significant. The estimates remain positive (with the exception of the 1883-1885 cohort) for cohorts born up to 9 years after the implementation. However, the treatment effect is essentially zero for cohorts born more than 10 years after the implementation, as in the regression results in Table 8.

These results are corroborate the "honeypot" mechanism. As with the labor market entry analysis, there are strong positive short-run economic shocks for exposed native workers, consistent with increased low-skilled wages as a result of the labor shortage. There is also diminishing gains after the initial shock as the occupational mobility and migration effects emerge, but no negative medium- to long-term estimates, unlike the entry analysis. The results suggest the long-run negative effects simply cancel out the short-run gains rather than dominating them, resulting in a net zero long-run welfare effect. However, this is only indicative of the first 18 years after the exclusion, and, given a longer time-frame, the occupational mobility effects may continue to impact welfare in the longer-run, resulting in a net negative welfare effect.

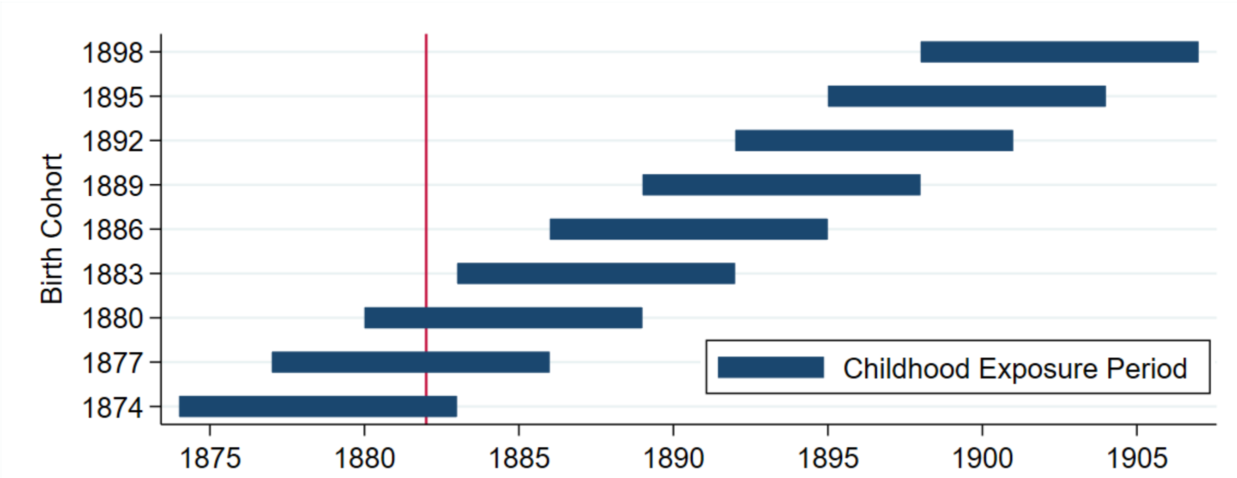
The findings of the early childhood cohort analysis should, however, be treated with more caution than the entrant cohort analysis, as it links over much longer periods, potentially introducing significant selection bias. Firstly, there is survivorship bias as children need to survive to 1940 (i.e to the age of 40-65 depending on year of birth) and still be earning income in order to be included in the analysis. This is a considerable constraint at a time when the life expectancy in the US was 44.4 years for males and the infant mortality rate 347 per 1,000. Those who benefited most from the Chinese Exclusion Act were more likely to have their children survive to older age, thus the estimates may be biased upwards. Secondly children must be living in the same household as the father for the Census to record the father-son link. Thus children that have already left home or the father does not live with them will not be included in the analysis. It is unclear what direction such a bias would take. In both types of selection, the oldest cohorts have the smallest samples and the most severe selection issues. They are older in both 1900 (thus more likely to have left home) and in 1940 (thus more likely to have retired or died). Therefore the short-term positive income shock result is particularly likely to be biased and should be treated as suggestive evidence only.

8 Robustness and Validity Checks

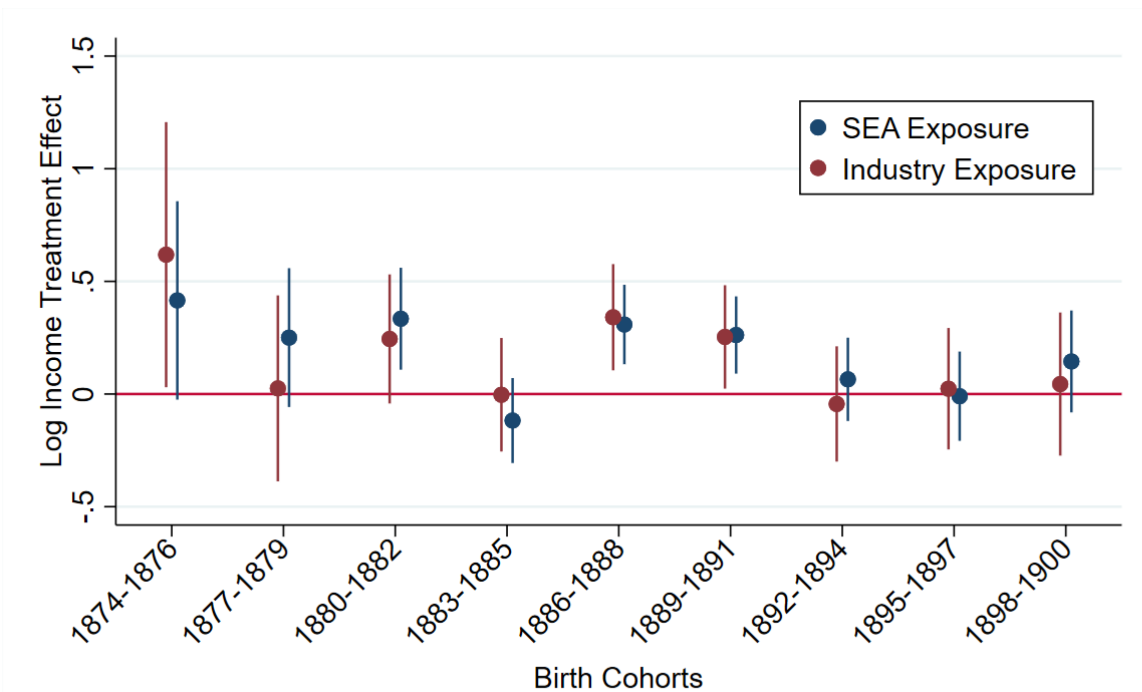
In this section I validate the identifying assumptions with a placebo test, and I provide a summary of the various robustness checks of the main results. The results of the placebo

Figure 5: Dynamic Early Childhood Exposure Effects

Panel A: Early Childhood Exposure Periods by Cohort



Panel B: Exposed Father Treatment Effect Estimates by Cohort



Notes: The figure shows the effects of early childhood exposure to the Chinese Exclusion Act by cohort. The sample is split into 3-year range birth cohorts. Panel A shows the critical childhood exposure periods (defined as 0-8 years old) for each cohort. The red vertical line indicates the implementation of the Chinese Exclusion Act. Panel B shows the estimated treatment effect by cohort of having a father exposed to the Chinese Exclusion Act on log weekly earnings in 1940 relative to children of matched but unexposed fathers. The estimates are shown for both SEA-level (blue) and industry-level (red) father exposures. The estimate ranges represent 95% confidence intervals, clustered by father's SEA in 1880.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Dynamic Analysis: Early Childhood Exposure Effects

	1875-1882 Birth Year		1892-1900 Birth Year	
	(1)	(2)	(3)	(4)
Exposed Father	0.332***	0.247**	0.044	0.013
	(0.096)	(0.121)	(0.058)	(0.076)
Mean of Dep. Var	7.993	7.792	7.815	7.696
SEA Exposure	Y	N	Y	N
Industry Exposure	N	Y	N	Y
Matched Fathers	Y	Y	Y	Y
R^2	0.002	0.001	0.000	0.000
N	6,403	2,313	13,245	4,526

The table reports the effects of father's exposure to the Chinese Exclusion Act during early childhood on the child's adult log weekly income. Columns 1 and 2 restrict the sample to children with early childhood exposure at the implementation of the Chinese Exclusion Act (i.e. born 1875-1882). Columns 3 and 4 restrict the sample to children with early childhood exposure at least 10 years after the implementation of the Chinese Exclusion Act (i.e. born 1892-1900). $ExposedFather$ is the binary treatment variable which equals 1 if the Chinese share of the father's SEA was above 5% in 1880. All regressions use the main estimating equation with coarsened exact matching weights. Clustered standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

policy implementation date based on the 1878 presidential veto can be found in Table 16 in the appendix. I use the same main analysis and SEA/industry-level treatment designations, only with 1870 and 1880 Census dates. I find no statistically significant effects using the main specification, lending support to the conditional parallel trends assumption. There is evidence of negative pre-trends when matching weights are not used, again reiterating the importance of matching to ensure unbiased estimation. There is also some evidence of negative pre-trends in the SEA-level West-only specification. The sample size drops substantially when including Western SEAs only. This suggests that although there is sufficient variation in the 1880 Chinese share among Western labor markets, it would appear that there is an insufficient number and diversity of labor markets in the West alone to construct appropriate counterfactuals and satisfy the identifying assumption. Therefore, the larger estimate sizes found in the West-only specification in the main results are likely over-inflated and should be treated with caution.

The results of the robustness checks can be found in Table 17 in the appendix. Firstly, I explore the sensitivity of the estimates alternative treatment definitions, namely a continuous treatment variable, 10% binary treatment threshold and county-level treatment. The estimates remain consistently negative, however county-level treatment estimates are no longer statistically significant. This is likely the result of county-level treatment designations being noisier and ignoring economic connections between counties. The labor market shocks may create spillover effects onto other counties, biasing the estimates towards zero.[38]

Secondly, I use two alternative measures of occupational standing provided by IPUMS: the occupational earnings percentile score and Duncan's Socioeconomic Index (SEI).[39] Both alternative measure estimates remain negative, however the SEI estimate is no longer statistically significant. It should be noted that the use of composite occupational standing measures is heavily debated and potentially misleading, particularly when measuring occupational mobility, and as such the SEI results should be treated with caution.[40]

Thirdly, I show the results are robust to more conservative Census linking methods (only exact and unique matches) and time-invariant matching covariates. I also show dropping the total foreign share control has little effect on the estimates. Finally, to address concerns that the results are being driven by low-population SEAs, I interact the treatment variables with log SEA population in 1880. I find it has virtually no effect on the results, suggesting no heterogeneity in the treatment effects by SEA size.

9 Conclusion

The Chinese Exclusion Act of 1882 was a landmark policy that represented the first substantial step the United States took in the transformation from a nation welcoming all immigrants to so-called 'gatekeepers' (Lee 2003). The Act has several unique features that makes it an

[38]Importantly, geographic markers in the Census are based on residence, not place of work where the labor supply effects would be felt.

[39]The percentile score is similar to the income score, but converts median incomes into standardized z-scores and then into a percentile rank rather than using raw median income. The SEI is a composite index based on occupational income, educational attainment and prestige.

[40]See the IPUMS Census User Notes for a discussion on the debate surrounding the use of composite occupational standing measures (available at https://usa.ipums.org/usa/chapter4/sei_note.shtml).

ideal natural experiment which, in combination with a cleaner identification strategy and large-scale individual longitudinal data, helps provide a clearer picture of the labor market effects of immigration restrictions. I find no evidence that the Act had the intended effect of improving native labor market outcomes in the long-run. Instead I find the Chinese exclusion had a significant negative effect on long-term occupational income, particularly for low-skilled and unemployed natives. To explain these findings, I find evidence in support of a "honeypot" effect. While natives likely benefited in the short-run from increased low-skilled wages, the decrease in the skill premium disincentivized upskilling and slowed occupational upgrading in the long-run. Furthermore, the initial wage gains were likely cancelled out by increased immigration from unrestricted regions. Overall, the Chinese Exclusion Act caused significant hardship, disruption and tragedy to Chinese immigrants while, for the most part, actually hurting rather than helping native workers in the long-run.[41]

The "honeypot" effect findings highlight the complex and multi-faceted labor market responses to immigration restrictions. However, much of the evidence for the "honeypot" effect is indirect and suggestive; further research with both short-term and long-term individual-level panel data is required to elucidate the mechanism. Furthermore, this study only examines the effects on native incumbents in the labor market. The impact of immigration restrictions on both targeted and non-targeted immigrant groups represents an important, understudied channel for future research.

[41] See Lee (2003), Pfaelzer (2008) and Lew-Williams (2018) for historical accounts from Chinese immigrants during the exclusionary period.

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Appendices

A Data and Methodology

A.1 Census Linking Methodology

Individual links between the 1880 and 1900 Censuses are provided by the Census Linking Project (Abramitzky, Boustan, Eriksson, Pérez & Rashid 2020) from historical linking algorithms originally developed by Abramitzky et al. (2012). This employs a rule-based method using observable first and last name, year of birth and place of birth from the Census to create links. The basic steps are:

1. First names are cleaned to remove non-alphabetic characters and account for common nicknames and misspellings.

2. Each individual in the 1880 Census is searched for in the 1900 Census for unique exact matches on cleaned first name, last name, year of birth and place of birth. Multiple matches are discarded.

3. If no matches are found, the year of birth bracket is steadily widened (up to ± 2 years) until a unique match is found.[42] Multiple matches are again discarded.

4. The entire process is then reversed to match from 1900 Census to the 1880 Census, and the intersection of the two matched samples are used.

There are two varieties of the above method: using the original cleaned names or using the NYSIIS phonetic alphabet to standardize the names based on pronunciation. The latter is used to account for minor spelling errors and mis-transcriptions which are common in these types of historical records. Abramitzky et al. (2021) use genealogically verified links to evaluate the performance of these methods and find the links to be highly accurate and, importantly, have robust regressions when estimating inter-generational mobility. This method has been shown to be more accurate (i.e. less false matches) than machine-learning or hand-linking methods, however at the cost of being less efficient (i.e. less correct matches).

For this analysis I use links found by both varieties of the algorithm, while discarding any links that have a discrepancy between the two methods. However, I also consider the most conservative linking parameters (only exact name and year of birth matches) for robustness. Of the 6,032,349 individuals eligible to be matched (i.e. native-born men of working age in both Censuses), the main method successfully creates 2,139,624 links, or 35.5% of the target population. This is match rate is roughly comparable to studies using similar linked full-count US Censuses, such as Price et al. (2020) (38-41%) and Lee et al. (2022) (29%).

Table 9 reports the balance between linked and unlinked individuals in the target population. All the differences are statistically significant due to the very large sample size, but most of the differences are not economically meaningful. However there is evidence that the linked sample is very slightly older, more educated and rural than the unlinked sample. The correlation coefficient between the linking probability and Chinese exclusion exposure

[42] The reason for allowing an year of birth bracket is that year of birth is obtained from age recorded, which is often misreported.

probability is 0.0069. While this coefficient is statistically significant, again due to the large sample size, the two are uncorrelated in practical terms. Therefore the linking methodology is unlikely to be introducing systematic bias in the estimates.

Table 9: Census Linking Balance

	Linked	Unlinked	Difference
Income Score	18.360	18.339	0.020*
	(0.004)	(0.001)	
Age	50.043	49.171	0.872***
	(0.006)	(0.004)	
Urban	0.317	0.348	−0.032***
	(0.000)	(0.000)	
Education Score	17.366	17.221	0.145***
	(0.020)	(0.015)	
Literacy	0.944	0.926	0.018***
	(0.000)	(0.000)	
Agricultural Worker	0.423	0.390	0.032***
	(0.000)	(0.000)	
Manufacturing Worker	0.069	0.071	−0.002***
	(0.000)	(0.000)	
Mining Worker	0.010	0.014	−0.004***
	(0.000)	(0.000)	
N	2,139,624	3,892,725	

The table compares pre-treatment baselines statistics of the linked sample between SEAs with a Chinese share above 5% and those below 0.05%. SEAs with partial treatment (below 5% but above 0.05%) are dropped. Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.2 Occupational Income Score Validation

As occupational income score is calculated from average income by occupation as recorded in the 1950 Census, there may have been structural changes between 1880 and 1950 that introduced systematic differences between the occupational income score and actual incomes that may bias results. To address this concern, I validate the use of occupational income score with the Workers of the Nation Encyclopedia (Willets 1903). The encyclopedia calculates the average national annual income at the turn of the century for one hundred common occupations from numerous government reports. I am able to connect 84 of these occupations to occupational codes used in the US Census. These 84 occupations cover about 77% of the workforce in the linked sample. I further drop any connections to non-specific Census occupations, leaving 64 occupations that cover about 66% of the workforce.[43]

The correlation between the encyclopedia occupational annual income and the Census occupational income score is highly significant and strongly positive: a correlation coefficient of 0.638 for the cleaned annual income and 0.570 when the non-specific occupations are included. I further validate the occupational income score by running the main analysis using log annual income from the encyclopedia rather than log occupational income score. Given the limited and specific set of occupations in the encyclopedia, this will naturally over-represent certain populations (in particular agriculture) and under-represent others. Nevertheless it will likely indicate if there are significant structural differences that would bias results. Table **??** reports the results, and while there is no effect for the full sample with SEA-level exposure, the results in the other specifications all remain significantly negative and of a similar magnitude to the main results. Together these results suggest there are no large, systematic differences in relative income between the analysis period and 1950 to significantly bias the estimates, and thus occupational income score represents a relatively good proxy for occupational mobility.

[43] This is due to these encyclopedia occupations being too specific. For example 'harnessmaker', 'soapmaker' and 'carriagemaker' are separate occupations in the encyclopedia, but are all connected to 'unspecified craftsman' in the Census.

Table 10: Occupational Income Score Validation

	Log Annual Income	
	Matching (Full Sample) (1)	Matching (West Only) (2)
A. SEA Exposure		
Chinese×1882	0.002	-0.079^{***}
	(0.004)	(0.006)
Constant	6.469	6.491
R^2	0.004	0.007
N	144,752	32,842
B. Industry Exposure		
Chinese×1882	-0.011^{**}	-0.045^{***}
	(0.005)	(0.007)
Constant	6.592	6.618
R^2	0.054	0.066
N	51,863	23,153
Two-Way FE	Y	Y
Matched Individuals	Y	Y
Western SEAs Only	N	N

The table reports the effect of exposure to the Chinese Exclusion Act on log annual income for occupations listed in the 1903 Workers of the Nation encyclopedia. *Chinese* in Panel A is the binary treatment variable which equals 1 if the individual resided in an SEA with a Chinese share above 5% in 1880, while in Panel B is the equivalent treatment variable if the individual worked in an industry with a Chinese share above 5% in 1880. 1882 marks Census years post-1882. All regressions use the main estimating equation with matching weights. Clustered standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B Additional Figures and Tables

Table 11: Chinese and Non-Chinese Foreign Workforce Characteristics (1880 Full Census)

	Chinese (1)	Non-Chinese (All) (2)	Non-Chinese (West Only) (3)	Difference (1)-(2)	Difference (1)-(3)
Age	32.431	40.207	38.210	-7.775^{***}	-5.779^{***}
	(0.033)	(0.007)	(0.023)		
Urban Residence	0.330	0.491	0.347	-0.161^{***}	-0.017^{***}
	(0.002)	(0.000)	(0.001)		
Occupation Score	19.179	21.205	21.582	-2.026^{***}	-2.403^{***}
	(0.027)	(0.006)	(0.021)		
Education Score	5.143	7.390	7.661	-2.247^{***}	-2.518^{***}
	(0.029)	(0.007)	(0.025)		
Literacy	0.775	0.889	0.899	-0.114^{***}	-0.124^{***}
	(0.001)	(0.000)	(0.001)		
Agricultural Worker	0.116	0.266	0.226	-0.150^{***}	-0.110^{***}
	(0.001)	(0.000)	(0.001)		
Manufacturing Worker	0.103	0.186	0.109	-0.083^{***}	-0.006^{***}
	(0.001)	(0.000)	(0.001)		
Mining Worker	0.231	0.037	0.149	0.194^{***}	0.082^{***}
	(0.001)	(0.000)	(0.001)		
Services Worker	0.357	0.245	0.268	0.112^{***}	0.089^{***}
	(0.001)	(0.000)	(0.001)		
N	95,341	3,109,633	241,504		

The table compares pre-treatment baselines statistics from the full-count Census sample between Chinese working-age males in the US (Column 1), non-Chinese foreign-born working-age males in the US (Column 2), and non-Chinese foreign-born working-age males in the Western Census region (Column 3). Column 4 reports the differences between Columns 1 and 2, while Column 5 the differences between Columns 1 and 3. Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Summary Statistics - Linked Sample in 1880

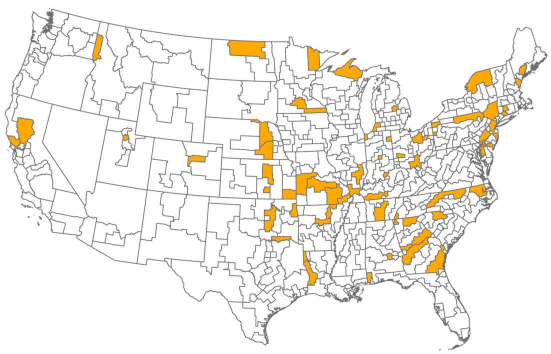
	Mean	Std. Dev.	Min	Max
Individuals				
Log Occupational Score	2.906	0.436	1.386	4.394
Log Socioeconomic Index	2.883	0.633	1.792	4.585
Log Prestige Score	3.507	0.389	0	4.413
Log Earnings Score	2.997	0.961	0	4.615
Log Educational Score	1.857	0.717	0	4.615
Literacy	0.934	0.248	0	1
Age	30.193	8.804	18	50
Urban	0.198	0.398	0	1
Agricultural Worker	0.494	0.500	0	1
Manufacturing Worker	0.108	0.310	0	1
Mining Worker	0.010	0.097	0	1
SEAs				
Population (000s)	27.540	42.541	0.089	617.413
Area (000s sq.km)	16.624	26.801	0.159	286.352
Chinese Share	0.012	0.046	0	0.360
Non-Chinese Foreign Share	0.208	0.175	0.001	0.810
Total Foreign Share	0.220	0.188	0.001	0.810

Table 13: Treatment Balance (1880 Census Linked Sample)

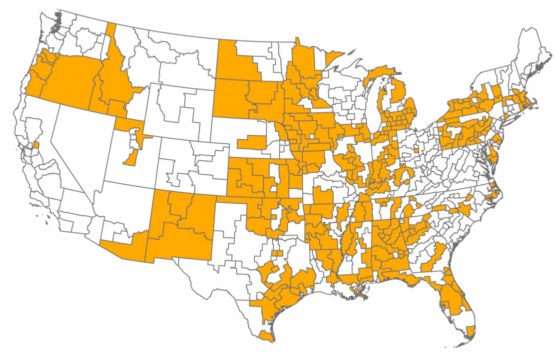
	Treatment	Control	Difference
Individuals			
Log Occupation Score	2.774	2.718	0.056***
	(0.004)	(0.001)	
Age	30.632	29.933	0.699***
	(0.045)	(0.006)	
Urban	0.237	0.213	0.024***
	(0.002)	(0.000)	
Agricultural Worker	0.376	0.496	−0.120***
	(0.002)	(0.000)	
Manufacturing Worker	0.082	0.108	−0.026***
	(0.001)	(0.000)	
Mining Worker	0.060	0.008	0.052***
	(0.001)	(0.000)	
N	44,571	2,095,053	
SEAs			
Chinese Share	0.148	0.000	0.148***
	(0.013)	(0.000)	
Non-Chinese Foreign Share	0.311	0.198	0.112***
	(0.015)	(0.009)	
Total Foreign Share	0.458	0.198	0.260***
	(0.019)	(0.009)	
N	36	393	

The table compares pre-treatment baselines statistics of the linked sample between SEAs with a Chinese share above 5% and those below 0.05%. SEAs with partial treatment (below 5% but above 0.05%) are dropped. Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

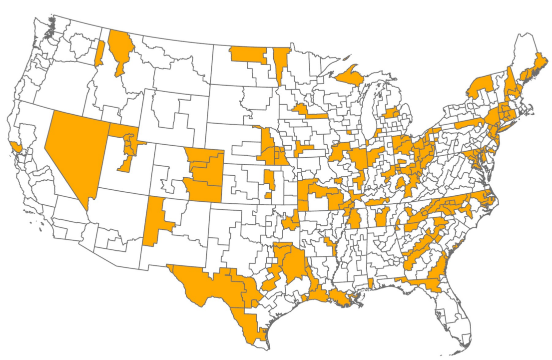
Figure 6: Labor Market Binary Designations



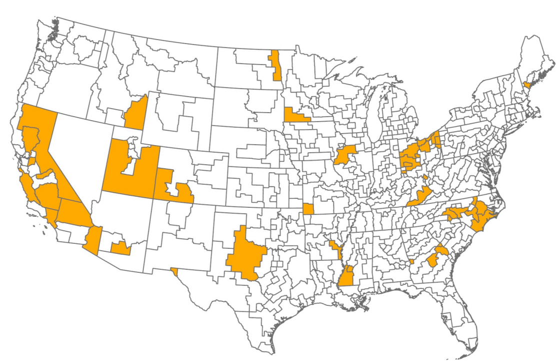
(a) Urban SEAs (>50% urban population)



(b) Agricultural SEAs (>50% employed)



(c) Manufacturing SEAs (>10% employed)



(d) Mining SEAs (>5% employed)

Table 14: Effect of Chinese Exclusion Act on Native Employment

	Employed 1900		
	No Matching (1)	Matching (Full) (2)	Matching (West Only) (3)
A. SEA Exposure			
Chinese×1882	−0.033	0.036	−0.088
	(0.060)	(0.062)	(0.261)
Constant	1.580	1.398	1.611
R^2	0.001	0.001	0.001
N	4,279,248	286,950	44,888
B. Industry Exposure			
Chinese	0.016	−0.019	−0.087
	(0.020)	(0.036)	(0.098)
Constant	1.524	1.449	1.342
R^2	0.001	0.000	0.001
N	2,139,624	48,945	14,479
Two-Way FE	Y	Y	Y
Matched Units	N	Y	Y
Western SEAs Only	N	N	Y

The table reports the effect of exposure to the Chinese Exclusion Act on native employment. The dependent variable is binary, equal to 1 if the individual is recorded as employed in 1900. *Chinese* in Panel A is the binary treatment variable which equals 1 if the individual resided in an SEA with a Chinese share above 5% in 1880, while in Panel B is the equivalent treatment variable if the individual worked in an industry with a Chinese share above 5% in 1880. 1882 marks Census years post-1882. Panel A regressions use a probit version of the main estimating equation. Panel B regressions use an OLS version with only 1900 observations (this is because being treated in Panel B implies employment, therefore DID creates perfect multicollinearity). Column 1 uses the full individual-level sample without matching weights. Columns 2 and 3 use individual-level observations with matching weights. Clustered standard errors in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 15: Top Occupations by Skill Quintile

Quintile 1	**Quintile 2**	**Quintile 3**	**Quintile 4**	**Quintile 5**
Farm laborer	Operative worker	Farm owner	Blacksmith	Manager/official/ proprietor
Laborer	Truck/tractor driver	Carpenter	Machinist	Salesman/clerk
Household worker	Mine operative/laborer	Painter (construction)	Meat cutter	Physician/ surgeon
Huckster/peddler	Fisherman/ oysterman	Driver/ chauffeur	Brick/ stonemason	Lawyer/judge

The table records the most frequently listed professions of male native-born workers in the 1880 US Census by skill quintile. Skill quintile is determined by the occupation education score.

Table 16: Placebo Policy Date

	Log Occupational Income Score		
	No Matching (1)	Matching (Full) (2)	Matching (West Only) (3)
A. SEA Exposure			
Chinese×1878	-0.035^{**}	-0.008	-0.068^{**}
	(0.013)	(0.019)	(0.025)
Constant	2.750	2.883	2.744
R^2	0.063	0.009	0.042
N	3,471,738	193,130	10,730
B. Industry Exposure			
Chinese×1878	-0.212^{***}	-0.015	0.000
	(0.019)	(0.014)	(0.060)
Constant	2.750	2.924	3.378
R^2	0.064	0.065	0.071
N	3,471,738	68,003	5,098
Two-Way FE	Y	Y	Y
Matched Units	N	Y	Y
Western SEAs Only	N	N	Y

The table reports the effect of exposure to the placebo 1878 Chinese Exclusion Act on native occupational income score. *Chinese* in Panel A is the binary treatment variable which equals 1 if the individual resided in an SEA in 1870 that had a Chinese share above 5% in 1880, while in Panel B is the equivalent treatment variable if the individual worked in an industry in 1870 which had a Chinese share above 5% in 1880. 1878 marks Census years post-1878. Column 1 uses the full individual-level sample without matching weights. Columns 2 and 3 use individual-level observations with matching weights. Clustered standard errors in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 17: Robustness Checks

		Alternative Treatment Definitions			Occupation Score Variants		Alternative Matching/Linking			
	Baseline Specification (1)	Continuous Treatment (2)	10% Threshold (3)	County-Level Treatment (4)	Duncan's SEI (5)	Earnings % Score (6)	Conservative Census Links (7)	Time-Invariant Matching (8)	No Total Foreign Share (9)	SEA Population Controls (10)
A. SEA Exposure										
Chinese×1882	-0.012^{**}	-0.009	-0.018^{***}	-0.007	-0.003	-0.036^{***}	-0.003	-0.017^{***}	-0.013^{**}	-0.012^{**}
	(0.005)	(0.031)	(0.006)	(0.006)	(0.008)	(0.011)	(0.007)	(0.005)	(0.005)	(0.005)
B. Industry Exposure										
Chinese×1882	-0.021^{***}	-0.350^{***}	-0.011^{*}	-0.005	-0.014	-0.048^{***}	-0.020^{*}	-0.045^{***}	-0.022^{***}	-0.021^{***}
	(0.008)	(0.007)	(0.007)	(0.010)	(0.012)	(0.016)	(0.011)	(0.006)	(0.008)	(0.008)
Mean of Dep. Variable	2.894	2.894	2.894	2.812	2.955	3.100	2.884	2.997	2.894	2.894
Two-Way FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Matched Individuals	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

The table reports the effect of exposure to the Chinese Exclusion Act on native occupational mobility. *Chinese* in Panel A is the binary treatment variable which equals 1 if the individual resided in an SEA with a Chinese share above 5% in 1880, while in Panel B is the equivalent treatment variable if the individual worked in an industry with a Chinese share above 5% in 1880 (with the exceptions of Columns 3 and 4). 1882 marks Census years post-1882. All regressions use the main estimating equation (with the exception of Column 9, which does not control for total foreign share) with individual matching weights. Log occupational income score (or some variation of occupational standing measure) is the dependent variable in all columns. Standard errors clustered at the SEA-level. Column 1 reports the main results from Table 1 as a baseline comparison. Columns 2-4 use alternative treatment definitions. Columns 5 and 6 use variations of occupational standing measures. Columns 7 and 8 use alternative Census linking and CEM methodologies. Column 9 drops the total foreign share control from the regression. Column 10 controls for SEA population in 1880.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$