The Impact of Skill-Based Immigration Restrictions: The Chinese Exclusion Act of 1882

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This paper considers the impact of skill-based immigration restrictions, using the Chinese Exclusion Act as a natural experiment. I find that restrictions reduced the average occupational standing of Chinese immigrants, suggesting substitution between observed and unobserved skills. Conversely, children of restricted immigrants have greater human capital than those of unrestricted immigrants, despite restricted immigrants themselves having lower skill. This suggests particularly strong intergenerational transmission of skill among Chinese immigrants of the exclusion era. More generally, the findings indicate that the effects of skill-based restrictions are not always straightforward and may be heterogeneous across groups.

I. Introduction

Restrictions imposed on Chinese immigrants mark two watershed moments in the history of US immigration policy. The Page Law of 1875, which barred any persons from entering the country "for lewd and immoral purposes" and referred specifically to "any subject of China, Japan, or any Oriental country," constituted the first legislated restriction on immigration to the United States. This act established the precedent for direct federal regulation of immigration, including the authority to deny entry to certain undesirable persons. Earlier statutes had regulated various aspects of the immigration process but had placed no explicit restrictions on immigration.[1] The next piece of immigration legislation to be en-

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[1] Legislation enacted between 1790 and 1875 covered the following: regulation of the naturalization process, registration of aliens entering the country, recognition of the government's right to deport aliens deemed dangerous to the country, vessel occupancy standards to safeguard the health and safety of passengers both entering and exiting the country, and prohibition of the transportation of "coolies" on American vessels.

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acted, the Chinese Exclusion Act of 1882, was the first in US history to differentiate by national origin and began a long history of racially motivated immigration restrictions, in place until the Immigration and Nationality Act of 1965. It was also the first law to impose restrictions on the basis of skill, a legacy that persists in current policy. The Chinese Exclusion Act of 1882 prohibited immigration of all Chinese laborers, either skilled or unskilled, providing specific exemptions for merchants, teachers, students, and officials.

In this paper, I utilize census data to examine the human capital of Chinese immigrants throughout history, using the restrictions of the Chinese Exclusion Act as a natural experiment. While a tremendous amount of work has been done on the effect of skill-based selection on outcomes for both natives and immigrants, relatively little is known about the effect of skill-based restrictions. On the one hand, the effect of such restrictions seems clear: an increase in the average skill of targeted immigrants. However, if skills are multidimensional, then restrictions on observed skills may affect self-selection on unobserved skills, perhaps even undermining the goals of the original policy. From an analytic standpoint, the case of Chinese exclusion is particularly useful for two reasons: the restrictions were enacted (1) without accompanying quotas and (2) at a time in which immigration was essentially unrestricted (excepting certain "undesirables," e.g., criminals, prostitutes). Moreover, because the same restrictions were later applied to Japan, I am able to use Japanese immigrants as a comparison group. This allows us to partially disentangle changes in selectivity imposed by US law from those that arose endogenously, in response to the new restrictions, and to provide additional insight into historical differences in self-selection across countries.

Using microdata from the US decennial censuses, I find that occupational outcomes among Chinese immigrants worsened after skill-based restrictions were imposed. Restricted Chinese immigrants exhibited weaker positive selection, relative to the Japanese. More surprisingly, postexclusion Chinese immigrants also exhibit weaker positive selection relative to their predecessors, who migrated without restriction. Because the Exclusion Act required higher levels of observed skill, the decline in aggregate skill suggests that observed and unobserved skills were substitutes in the migration decision.[2] Moreover, this finding suggests that the skill-based restrictions of the Chinese Exclusion Act were, in some respects, ineffectual. Although they were successful in limiting the immigration of laborers, narrowly defined, they did not, in fact, elevate the average occupational standing or human capital of the restricted group.

In contrast, I find that children of restricted Chinese immigrants have better outcomes (occupation, literacy, schooling) than children of restricted Japanese immigrants. But, given that restricted Chinese immi-

[2] Alternatively, these could be deemed targeted and untargeted skills, on the basis of the nature of the restriction.

grants had worse occupational outcomes and lower literacy rates, this cannot be explained solely by the intergenerational transmission of skill, either observed or unobserved. Rather, it seems that Chinese immigrants of the exclusion era invested particularly heavily in the human capital of their children, as is commonly found among immigrant populations (Jasso and Rosenzweig 1990, 409). This helps to explain the evolution of the Chinese stereotype from cheap, exploitable contract laborers to a successful "model minority" in less than 100 years. But skill-based restrictions cannot be credited with this upward progress because, among the Japanese, children of restricted immigrants fare worse than children of unrestricted immigrants, despite the fact that restricted Japanese had better occupational outcomes. The findings of this paper suggest that, where skill prices and distributions are such that there is significant scope for substitution between different types of skill, restrictions based on a single skill type may, in fact, have adverse effects on the average skill of targeted groups, in either the first or the second generation.

The following section briefly describes the historical context. Section III presents the empirical approach and data, and results are discussed in Section IV. Section V considers the intergenerational transmission of skill, and Section VI presents conclusions.

II. Historical Context

A. Chinese Immigration

Prior to 1854, immigration of Chinese had been fewer than 50 persons per year. In 1854, this number jumped to 13,100, in response to the discovery of gold in California, and then stabilized somewhat to a range of 3,000–7,000 for the next 10 years (see fig. 1). Chinese immigration declined steadily from 1864 to 1866 as gold discoveries slowed. However, construction of the transcontinental railroad and the adoption of the Burlingame Treaty led again to increasing numbers of Chinese immigrants through 1870. Completion of the railroad led to temporary high unemployment among Chinese in the United States and the passage of several anti-Chinese laws and ordinances, which depressed immigration for a short time before it again spiked in 1873. Extensive agricultural development and land reclamation in the western states, spurred by lower transport costs and expanded product markets, supported relatively high Chinese immigration through 1876. At this time, the national economy began to slump and anti-Chinese sentiment was renewed, leading to a period of depressed immigration during 1877–79.

In 1880, the American and Chinese governments signed a migration treaty, known as Angell's Treaty, which granted the US government permission to regulate, limit, or suspend the immigration of Chinese laborers but not prohibit it entirely (Tsai 1988, 23). Political discussions of an act to exclude Chinese immigrants altogether started almost immediately, and many Chinese seized this opportunity to migrate. The Chinese Exclu-

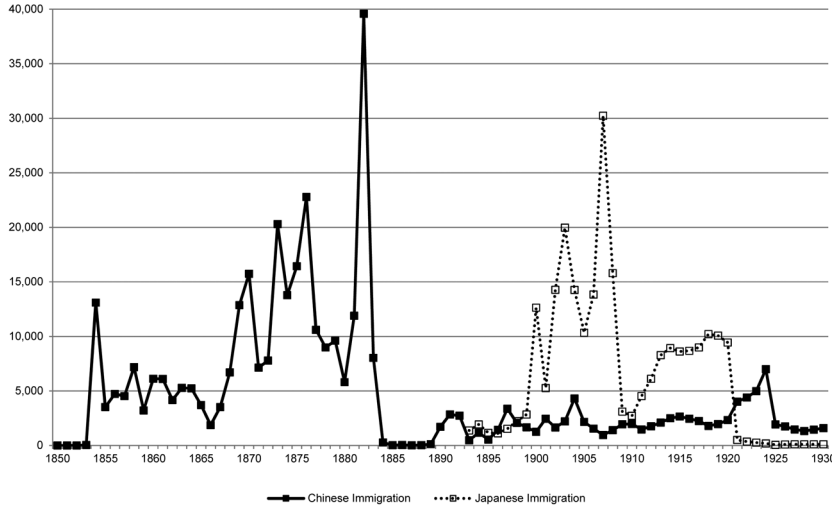

Figure 1.—Immigration to the United States, 1850–1930. Source: US Department of Labor (1930).

sion Act was signed into law on May 6, 1882, and Chinese immigration declined precipitously from 39,579 in 1882 to 8,031 and 279 in 1883 and 1884, respectively, with formal enforcement of the act commencing in 1884. Immigration of Chinese laborers, "both skilled and unskilled . . . and Chinese employed in mining," was suspended for a period of 10 years, beginning 90 days after the passage of the law. "Diplomatic and other officers of the Chinese Government traveling upon the business of that government . . . and their body and household servants" were explicitly exempted from the provisions of the act, while "teachers, students, merchants . . . together with their body and household servants" were designated exempt classes per Angell's Treaty. Various amendments were added over the next decade, and in 1904, all laws regarding Chinese immigration were reenacted and extended indefinitely.

To enforce these statutes, Chinese persons wishing to enter the United States for the first time were required to possess documentation from the Chinese government, endorsed by diplomatic representatives of the United States, verifying their occupation and eligibility for immigration. A merchant applying for such certification was required to "state the nature, character, and estimated value of the business carried on by him prior to and at the time of his application" (National Archives 1882), but the verification process used by the Chinese government is not clear. However, upon arriving in the United States, Chinese immigrants were detained and subjected to lengthy interrogations. Those claiming to be merchants were expected to be "wealthy, educated, and refined gentlemen" with high levels of literacy and business knowledge. At the Angel Island immigration station, officials would even inspect the applicant's hands and feet

for calluses indicative of manual labor (Lee 2003, 89–90). Immigration statistics (reproduced in table 1) confirm that the restrictions of the Chinese Exclusion Act were strictly enforced. Of the incoming immigrants reporting occupations between 1884 and 1891, over 80 percent were merchants and none were laborers. In 1890 and 1891, there were larger numbers of professional and skilled workers, but throughout this period there are very few "servants" or other household workers. There are a significant proportion reporting no occupation; these are predominantly the children and spouses of men in the exempt classes.[3]

B. *Japanese Immigration*

Immigration from Japan to the continental United States began gradually in the 1890s, increasing rapidly in 1900 (see fig. 1).[4] These immigrants were largely contract laborers, recruited to fill the void in the agricultural sector left by excluded Chinese laborers. However, natives soon expressed similar animosity toward the Japanese, and immigration of unskilled laborers from Japan was prohibited by the Gentlemen's Agreement of 1907. With this treaty, enacted in 1908, the Japanese government agreed to voluntarily limit emigration (Inui 1925) and to immediately begin enforcing these limits by denying passports to laborers intending to enter the United States for the first time.[5] Japanese immigration falls dramatically in 1909 but begins to rebound relatively quickly in 1911 before stabilizing around 1914. In 1921, as a final attempt to mitigate anti-Japanese sentiment in the United States, the Japanese government announced that it would stop issuing passports to "picture brides" (Daniels 1988, 147).[6] A marked decline in Japanese immigration is immediately evident, and annual immigration remains low from that point on (fig. 1).

Although the entry of laborers, skilled and unskilled, was prohibited for both the Chinese and the Japanese, there are several notable differences between the Chinese Exclusion Acts and the Gentlemen's Agreement. First, the Chinese Exclusion Act specified four exempted categories (diplomats, teachers, students, and merchants, together with their "body and household servants"), while the Gentlemen's Agreement referred only to "nonlaborers," noted in immigration law as "persons whose work is neither distinctively manual nor mechanical, but rather professional, mercantile, or clerical." Second, enforcement of the Gentlemen's Agreement

[3] Immigration reports during this era typically label the category as "no occupation including women and children."

[4] An estimated 30,000 Japanese contract laborers came to Hawaii to work on the sugar plantations between 1884 and 1898 (Daniels 1988, 100). However, Hawaii is not included in government records (including census and immigration) until after annexation in 1898.

[5] According to the 1910 annual report of the Superintendent of Immigration, "With respect to Hawaii, the Japanese Government of its own volition stated that, experimentally at least, the issuance of passports to members of the laboring classes proceeding to that Territory would be limited to 'former residents' and 'parents, wives, or children of residents.' " (US Department of Commerce and Labor 1910, 125).

[6] Women in Japan wed in absentia (with a "picture") Japanese men living in America.

was effectively conducted by the Japanese government, which assumed responsibility for determining immigrant eligibility, while enforcement of the Chinese Exclusion Acts fell entirely to US customs officials. Third, the Gentlemen's Agreement allowed prior immigrants to return, even as laborers, whereas returning Chinese laborers were prohibited by the Scott Act of 1888, 6 years after the initial exclusionary legislation.[7] Finally, the Gentlemen's Agreement allowed parents, spouses, and children of all Japanese immigrants to enter the country, while only family members of exempt-class Chinese immigrants were permitted.

Enforcement of the Gentlemen's Agreements is difficult to infer, as immigration statistics from this period began to combine returning and first-time immigrants. In the 10 years following the Gentlemen's Agreement, returning Japanese immigrants account for 25–45 percent of all immigration, and roughly 40–50 percent of these returning immigrants are laborers. Nonetheless, there is a large decline in the number and proportion of laborers after 1908 (table 1), and immigration of laborers remains much lower, as a fraction of total Japanese immigration, throughout the next decade. Beginning in 1908, the Bureau of Immigration also added a section to its annual immigration report detailing Japanese immigration, with the explicit purpose of documenting enforcement of the Gentlemen's Agreement. And, using census records, Suzuki (2002) documents increases in average skill levels among new Japanese immigrants beginning around 1908, with clear evidence for greater selection on education, work experience, and occupation during the exclusionary period.

The impact of these restrictions on the relative skill distributions for Japanese and Chinese immigrants is ambiguous. Because the Gentlemen's Agreement did not specifically identify exempted occupations, Japanese immigrants in other professional or clerical occupations may have been able to enter the United States while similar Chinese immigrants were not. Enforcement by Japanese officials rather than US customs agents may also have allowed greater immigration of skilled Japanese workers, to the extent that officials in the origin country can better assess occupational standing. Indeed, we see a much larger number of Japanese immigrants in professional and skilled occupations, though, again, we cannot determine how many were new immigrants restricted by the Gentlemen's Agreement. Conversely, the provisions of the Gentlemen's Agreement permitting return migration and migration of immediate family members, for both laborers and exempted categories, almost certainly reduced the average skill level of Japanese immigrants relative to Chinese immigrants. Although many wives and children of Japanese immigrants likely entered in the "no occupation" category, immigration reports also suggest a substantial number of first-time immigrants in the "laborer" category. For example, 1,511 Japanese laborers entered the United States in 1909, but

[7] Roughly 20,000 Chinese who were out of the country at the time the Scott Act was enacted were unable to return, and 600 who were currently in transit to the United States with government-issued return certificates were denied entry (Chang 2003, 135).

TABLE 1
Occupation of Incoming Immigrants, Before and After Enforcement of Skill-Based Immigration Restrictions

	Chinese, Before								
	1875	1876	1877	1878	1879	1880	1881	1882	1883
Merchants	3	8	2	. . .	5	3	8	38	73
Professionals	45	23	36	28	38	20	43	105	8
Skilled	452	481	411	309	462	400	560	407	334
Servants/domestics	7	14	1	6	3	7	12	13	1
Laborers	15,287	21,902	10,281	8,286	8,984	5,530	11,466	38,701	7,801
Other	291	21	8	40	. . .	61	8	. . .	3
No occupation	1,128	1,193	468	676	705	561	652	688	402
Total	16,437	22,781	10,594	8,992	9,604	5,802	11,890	39,579	8,031
	Chinese, After								
	1884	1885	1886	1887	1888	1889	1890	1891	1894
Merchants	226	12	14	4	25	89	1,229	2,530	1,036
Professionals	2	. . .	2	. . .	. . .	. . .	89	43	5
Skilled	. . .	. . .	. . .	. . .	. . .	. . .	35	1	89
Servants/domestics	2	. . .	. . .	1	. . .	. . .	7	2	. . .
Laborers	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	37
Other	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .	732
No occupation	49	10	24	5	1	29	356	260	16
Total	279	22	40	10	26	118	1,716	2,836	1,915

	Japanese, Before								
	1899	1900	1901	1902	1904	1905	1906	1907	1908
Merchants	601	797	652	1,205	1,184	777	632	772	685
Professionals	92	563	167	222	373	280	256	610	378
Skilled	121	1,793	603	1,047	641	358	329	546	457
Servants/domestics	53	223	181	173	317	207	195	166	285
Laborers	1,057	5,832	830	2,009	8,249	6,626	9,270	21,970	7,910
Other	912	2,713	2,231	5,411	374	561	1,115	2,905	2,121
No occupation	559	707	585	4,388	3,244	2,212	2,446	3,855	4,582
Total	3,395	12,628	5,249	14,455	14,382	11,021	14,243	30,824	16,418
	Japanese, After								
	1909	1910	1911	1912	1913	1914	1915	1916	1917
Merchants	295	327	359	447	584	660	718	753	811
Professionals	279	424	469	489	518	499	782	801	613
Skilled	146	189	143	362	427	479	595	636	616
Servants/domestics	188	223	188	176	200	152	166	277	316
Laborers	1,511	1,530	1,995	3,097	4,768	4,699	3,763	4,702	4,798
Other	667	607	670	1,290	1,987	2,317	2,616	2,547	2,637
No occupation	839	825	2,617	2,728	3,188	4,210	3,597	2,991	3,497
Total	3,925	4,125	6,441	8,589	11,672	13,016	12,237	12,707	13,288
Returning immigrants	1,034	979	1,559	2,513	4,118	4,986	4,951	5,382	5,682
Returning laborers	505	433	644	1,189	2,153	2,421	2,110	2,425	2,671

Source.—US Bureau of Immigration (1892–94, 1895–1905); US Department of Commerce and Labor (1906–13); US Department of Labor (1914–18, 1930); US Bureau of Statistics (1884–89).

Note.—The Chinese Exclusion Act was enforced beginning in 1884. Effective enforcement of the Gentlemen's Agreement began in mid-1908. Laborers includes farm and nonfarm; other includes farmers and other miscellaneous.

only 505 of those were returning immigrants; therefore, over 1,000 laborers must have entered the United States as family members of returning or exempted immigrants. Given the uncertain effect of the differing restrictions on Chinese and Japanese immigrants, we return to this issue in our empirical analysis below.

III. Empirical Approach

To determine how human capital among Chinese immigrants is related to selection on skill, we can utilize exclusionary legislation, which imposed a specific form of skill-based selection, as a natural experiment.[8] But, as Chiswick (1986) suggests, immigrants may be self-selected not only on observed skills (e.g., literacy, occupation) but also on unobserved skills such as ambition and initiative, which are also positively correlated with investments in human capital. In that case, exclusionary legislation may have altered, rather than obviated, patterns of self-selection among Chinese immigrants.

A. *Theoretical Framework*

To help fix ideas, suppose that wages in the source and host countries can be expressed as

$$w_0 = \mu_0 s + \varepsilon_0 k \quad \text{and} \quad w_1 = \mu_1 s + \varepsilon_1 k,$$

with $s \sim N(c, \sigma^2)$ and $k \sim N(0, \sigma_k^2)$, where s and k represent observed and unobserved skills, and μ and ε are the country-specific (fixed) prices paid for these skills. As in a typical Roy (1951) model, assume that an individual migrates on the basis of net wage gains, where π represents the cost of migrating:

$$I = 1 \quad \text{if } \mu_1 s + \varepsilon_1 k - \pi \geq \mu_0 s + \varepsilon_0 k. \tag{1}$$

Note that, with a fixed moving cost, observed and unobserved skills are substitutes in this simplified framework, and immigrants from the same source in the same year will display a mix of the two attributes. The Chinese Exclusion Act of 1882 (and its successors) ensured that any new immigrants would be admitted solely on the basis of high observed skill, effectively legislating $s \geq \underline{s}$ and shifting the skill composition of immigrants toward observed skill and away from unobserved skill. Therefore, the weaker the selection on observed skill (and the stronger the selection on unobserved skill) prior to exclusion, the larger the change in average unobserved skill after exclusion.

Assuming that immigration restrictions were binding, the average change in wages among immigrants, before and after skill-based restrictions, can be expressed as

[8] This paper does not utilize data on the human capital of native Chinese and therefore does not address the question of positive/negative selection of immigrants. Rather, the focus is on human capital of ethnic Chinese in the United States relative to natives.

$$\begin{aligned} E(w_1 \mid I = 1,\ T = 1) &- E(w_1 \mid I = 1,\ T = 0) \\ &= \mu_1[E(s \mid I = 1,\ s \geq \underline{s}) - E(s \mid I = 1)] \\ &\quad + \varepsilon_1[E(k \mid I = 1,\ s \geq \underline{s}) - E(k \mid I = 1)]. \end{aligned} \tag{2}$$

The first term reflects *legislated* changes in selection on observed skill, while the second term reflects *self-determined* changes in selection on unobserved skill. To learn something more about the effect of exclusionary legislation on self-selection then requires identifying a "control" group—a population subjected to the same immigration restrictions. The difference in wages between immigrants from China and the "control" country (J), before and after the exclusions, is

$$\begin{aligned} E(w_1^C \mid I = 1,\ T = 1) &- E(w_1^C \mid I = 1,\ T = 0) - E(w_1^J \mid I = 1,\ T = 1) \\ &- E(w_1^J \mid I = 1,\ T = 0) \\ &= \mu_1\{[E(s^C \mid I = 1,\ s \geq \underline{s}) - E(s^C \mid I = 1)] \\ &\quad - [E(s^J \mid I = 1,\ s \geq \underline{s}) - E(s^J \mid I = 1)]\} \\ &\quad + \varepsilon_1\{[E(k^C \mid I = 1) - E(k^C \mid I = 1,\ s \geq \underline{s})] \\ &\quad - [E(k^J \mid I = 1) - E(k^J \mid I = 1,\ s \geq \underline{s})]\}. \end{aligned} \tag{3}$$

If the two countries exhibit parallel trends in selection on observed skill, the first part of this expression will be zero, and the difference-in-difference will exactly identify relative changes in selection on unobserved skill. This assumption is discussed in detail in the following subsection.

B. *Empirical Specification*

The simple theoretical framework outlined above suggests a difference-in-difference type approach, comparing immigrants across ethnic groups, before and during the exclusion era, and using native-born whites as a comparison group to account for secular changes in human capital accumulation. To abstract from issues of endogenous labor supply, I focus only on men, aged 30–65. The wage of individual i in ethnic group j, cohort c, at time t can be expressed as a function of some group fixed effect (α), a period effect (β), a cohort effect (γ), age, and age squared. Additionally, I include an indicator for whether the individual was "treated" (T), migrated after exclusions were imposed, which is permitted to differ across ethnic groups:

$$W_{ijct} = \alpha_j + \beta_t + \gamma_c + \boldsymbol{\delta} \cdot f(\text{Age}_i) + \boldsymbol{\pi} T_i + \theta(T_i \times \text{Chinese}_i) + \varepsilon_{ijct}. \tag{4}$$

Note that this specification compares Chinese and Japanese immigrants in the restricted and unrestricted periods but not necessarily in the same

year, as the respective periods of immigration restriction overlap but do not fully coincide. The period and cohort effects, however, are based on Chinese, Japanese, and native-born whites and will therefore account for underlying annual changes in the occupational and skill distributions.

The coefficient of interest is the interaction term between the individual's "treatment" status and ethnic group (θ). This represents the change in immigrant selectivity among the Chinese, relative to the Japanese, after immigration restrictions are imposed. The exact timing of these restrictions was determined by the political and diplomatic environment rather than the skill composition of incoming immigrants per se. Implementation did occur during periods of increased migration by unskilled workers, although immigration reports do not indicate any significant change in the skill composition, only the volume of immigration. From table 1, we can discern a slight increase in the proportion of arriving immigrants reporting "laborer" as their occupation (less than 6 percentage points) and a slight increase in the proportion reporting a skilled or professional occupation (less than 4 percentage points). These trends are quite similar for both the Chinese and the Japanese in the 5 years leading up to the implementation of immigration restrictions. Thus, it seems plausible to interpret θ as the causal effect of the legislation. I also present an alternative specification that includes the number of years before and after the legislation was implemented to allow for differential trends.

C. *Identifying Assumption*

We can further interpret θ as the effect of exclusionary legislation on immigrant self-selection, net of legislated skill requirements, if Chinese and Japanese immigrants exhibited the same change in observed skill as a result of the immigration restrictions. Of course, the average skill of immigrants depends on the distribution of skill, moving costs, and skill prices. Given the geographic proximity of China and Japan and the similar timing of exclusionary legislation pertaining to each country, it seems reasonable to assume that moving costs were comparable. Moreover, in both cases, we are considering migration to the United States, which implies that skill prices in the host country are identical. However, historical data on skill premia in Asia are quite limited. Van Zanden (2009) and Allen et al. (2011) find the average skill premium of construction workers between 1750 and 1820 to be very similar between Japan and China, but it not clear whether this can be generalized across industries and over time. Empirical evidence on the historical distributions of observed skill in China and Japan is even more limited. Both countries began modernizing their educational systems in the late nineteenth century, moving away from traditional Confucian teachings toward a more Western-style curriculum; but access to education seems to have expanded more rapidly in Japan (Godo and Hayami 2002), suggesting greater improvements in observed skill throughout the population and a flatter distribution. Clearly,

with this dearth of historical data, an assumption of common skill prices and distributions of observed skill cannot be adequately justified. But comparing across ethnic groups will, at least, attenuate the direct effect of exclusion on observed skill, given that both groups were subject to the same binding restrictions (see eq. [3] above). What remains of the direct effect will reflect differences in selection on observed skill across countries, and we can sign the direction of this "bias" by looking at a measure of observed skill separate from unobserved skill.

Since we are interested in identifying selection on unobserved skill, another key factor is the stability of relative prices for observed and unobserved skills. That is, the change in self-selection among Chinese relative to Japanese can be attributed to the legislation as long as there were no changes in the origin countries that would have differentially altered the skill composition of emigrants. The Chinese and Japanese economies had already begun to diverge by the time immigration to the United States began. Both countries had been forcefully opened to international trade in the mid-nineteenth century but responded in very different ways. In Japan, the openness prompted the Meiji Restoration: a period of rapid westernization, marked by broad reform of economic and political institutions. There was widespread adoption of technology and an enormous export-led boom facilitated by an emerging merchant class (the Yokohama). Growth of manufacturing and exports was widespread, as were opportunities to capitalize on this growth (Ohno 2006). Openness also brought industrialization and the beginning of modern economic growth to China, but the Qing Dynasty resisted institutional change, allowing entrenched coalitions of merchants to stymie attempts to adopt new technologies and establish new industries, largely restricting innovation and keeping the gains from trade highly concentrated (Brandt, Ma, and Rawski 2014).

Opportunities for innovation and entrepreneurship were broadly accessible in Japan, and industrialization remained steady through the 1910s. In contrast, in China, elites and those with political influence successfully shut out the large majority of new entrants in not only the industrial sector but related sectors (e.g., transportation) as well. Late attempts to modernize political and economic institutions were largely unsuccessful, ultimately resulting in the collapse of the Qing Dynasty, and subsequent civil conflict left political and economic institutions unstable through the early 1920s. Without the needed reforms, opportunities for entrepreneurship in China remained available only to those with significant political and economic capital. This suggests that the returns to unobserved skill, relative to observed skill, were higher in Japan than in China. However, the broad survey pieces by Ohno (2006) and Brandt et al. (2014) also suggest that, although there were many important differences, the fundamental characteristics of the Chinese and Japanese economies remained relatively stable throughout the 1860–1920 period. Therefore, we would not expect substantial changes in selection on ob-

served versus unobserved skill around the time immigration restrictions were imposed.

The relative returns to unobserved skill likely began to diverge sharply for China and Japan around 1927. Japan experienced a severe financial crisis in 1927, and the ensuing recession was marked by stark deflation and severe rural poverty, prompting a turn toward militarism and economic policies influenced by Marxism (Ohno 2006). In contrast, 1927 brought the establishment of a new Kuomintang-led government in China and a recentralization of state power marked by the goal of a transparent apolitical bureaucracy with merit-based employment. These changes contributed to a period of considerable economic growth and declining inequality, but the Chinese soon engaged in war with Japan, followed by a period of civil conflict between the Kuomintang and the Communist Party, both of which caused significant loss of human and physical capital (Brandt et al. 2014). Given this divergence, I exclude from the analysis all immigrants arriving in or after 1927. Additionally, I allow for differential time trends, by ethnic group, before and during the exclusion period.

D. Data

Data are drawn from the 1860–1930 US decennial censuses of the Integrated Public Use Microdata Series (IPUMS; Ruggles et al. 2010) with all years pooled together. I limit the empirical analysis to males, aged 30–65, to abstract from labor supply decisions and because the terms of the exclusion acts did not apply to women and children immigrating with their spouses. Direct data on wages are not available during and preceding the exclusion era; instead, I examine the occupational income score (OCCSCORE) constructed by IPUMS. This measure maps occupational responses in each year to categories in the 1950 census and then assigns a score based on the median total income for each occupational category in 1950. The mapping of occupational responses into the 1950 categories is relatively straightforward. Responses prior to 1940, with the exception of 1910, can be coded directly into the 1950 classification system, and a series of detailed technical reports are available to assist with the mapping for other years (Sobek 1995).

But because the OCCSCORE is tied to earnings at a single point in time, changes in the relative earnings of various occupations will not be fully captured. To partially address this, IPUMS employs a weighting scheme wherein the score is a weighted average of the incomes of the components of each category, with weights derived from the occupation distribution in each census. Weighting captures some of the change in occupational earnings over time, but the data prior to 1910 are not sufficiently detailed to permit the use of this procedure. There is, however, empirical evidence supporting the relevance of the OCCSCORE in assessing group performance over time. Using data collected by the US Bureau of Labor Statistics on occupation-specific income from 1890, Sobek (1996) shows

that the relative economic standing of occupations was actually quite stable throughout the twentieth century. Most of the change in individual earnings occurred through changes in the occupational distribution rather than the relative standing of those occupations. And, in assessing the economic assimilation of immigrants, Abramitzky, Boustan, and Eriksson (2014) find that analysis of the OCCSCORE yields the same qualitative results as analysis of comparable scores based on earnings data from both the 1901 Cost of Living Survey and a historical earnings series dating back to the early 1920s (Goldin and Margo 1991).

Ideally, treatment status would be based on year of immigration. Unfortunately, this is available only in the 1900–1930 census records,[9] and it is not available for residents of Hawaii in 1900 and 1910. Given this limitation and the likely divergence in the relative returns to unobserved skill between China and Japan in 1927, I limit attention to the 1860–1930 census years and to immigrants arriving in or before 1927. We know that all foreign-born Chinese and Japanese enumerated in the 1860–80 censuses must have arrived prior to the passage of any immigration restrictions and can be assigned to the untreated group. And because the provisions of the Chinese Exclusion Act and the Gentlemen's Agreement did not apply in Hawaii until 1902 and 1907, respectively, we can also confidently assign all foreign-born Chinese and Japanese in Hawaii in 1900 to the untreated group. However, omitting the 1910 data from Hawaii would create significant sample selection problems. Over 30 percent of all observations on foreign-born Japanese males in the 1860–1930 IPUMS samples are residents of Hawaii enumerated in 1910.

Thus, when year of immigration is unavailable, treatment status is defined on the basis of age and birth year as follows. I examine the age distribution of the foreign-born, by ethnic group, and assign individuals to the treatment group if their age during the exclusion era falls within one standard deviation of the mean age at immigration among unrestricted migrants. Among foreign-born Chinese, the mean age was 24, with a standard deviation of 9; thus, all persons aged 15 and younger in 1884 are assigned to the treatment group. Similarly, foreign-born Japanese had a mean age of 26, with a standard deviation of 11, so all persons aged 15 and younger in 1908 are assigned to the treatment group. As a validation, we can compare treatment status based on this approach to treatment status based on actual year of immigration, where available. There is a high degree of correspondence between the two; 93 percent of all foreign-born Chinese and Japanese men, aged 30–65, are assigned the correct treatment status (table 2). The measure minimizes type I error (the probability of assigning an unrestricted immigrant to the treatment group),

[9] The 1900 census asked when the individual arrived in the United States, not the year of first arrival. Unrestricted immigrants returning after the Exclusion Act may therefore be incorrectly classified as "treated." However, census reports on occupations of incoming immigrants show very few laborers arriving between 1882 and 1897 (table 1), suggesting that misclassification of treatment status will be fairly limited.

TABLE 2
Treatment Status, Constructed versus Actual

	Unrestricted	Restricted
Untreated	14,366	548
	(95.7%)	(45.9%)
Treated	642	645
	(4.3%)	(54.1%)

Note.—Includes foreign-born Chinese and Japanese males, aged 30–65. Column percentages are in parentheses. Treated is defined on the basis of census and birth year. Restricted is defined on the basis of year of immigration.

with only 4.28 percent of those actually migrating prior to restrictions being incorrectly assigned to the treatment group, in favor of incorrectly assigning restricted immigrants to the control group. Type II error is indeed considerably higher, with 46 percent of those actually migrating during the exclusion era being incorrectly assigned to the control group. This approach effectively dilutes the treatment group, ensuring that the estimated effects can be interpreted as a lower bound (in absolute value) of the true effect of skill-based restrictions. But, given the coarseness of the treatment measure, I include an additional control for individuals who may have been treated (i.e., who are residing in the United States during the exclusion era but do not fit the above criteria), weighted by the number of years since the implementation of exclusionary legislation, as a rough proxy for the probability of being treated.

IV. Main Results

Looking at the occupational score (table 3, panel A), we see that both Japanese and Chinese (Asian, collectively) immigrants ranked considerably lower than whites, although the difference is significantly greater for the Japanese. Exclusionary laws (treatment) increased the occupational score among foreign-born Japanese, eradicating roughly 30–40 percent of the initial difference. In contrast, the average occupational score was much lower for Chinese immigrants admitted during the exclusionary era (ranging from 68 percent to over 500 percent greater than the difference between native-born whites and preexclusion Chinese), despite immigration exclusions being heavily biased in favor of high skill. Allowing for possible treatment among additional foreign-born Chinese and Japanese present in the United States during exclusion increases the magnitude of the point estimates, which is consistent with the conservative method used to assign treatment initially. Moreover, the coefficients on possible treatment coincide with the direct effects of treatment, again suggesting that the definition of treatment undercounts the number of immigrants subject to exclusionary laws. The time trends (col. 3) indicate that Japanese immigration was becoming less selective prior to exclusion, while Chinese immigration was becoming relatively more selective.

TABLE 3

THE EFFECT OF EXCLUSION ON THE FOREIGN-BORN, DIFFERENCE-IN-DIFFERENCE ESTIMATES

	A. Occupation Score			B. Literacy		
	(1)	(2)	(3)	(1)	(2)	(3)
Asian	−10.16***	−11.09***	−11.45***	−.268***	−.318***	−.319***
	(.132)	(.167)	(.175)	(.005)	(.007)	(.007)
Treated	3.138***	4.071***	4.624***	.204***	.255***	.212***
	(.611)	(.620)	(1.335)	(.016)	(.016)	(.044)
Possible treatment		.261***	.312***		.015***	.012***
		(.031)	(.086)		(.001)	(.003)
Years before restrictions			.792***			.002
			(.048)			(.002)
Years after restrictions			−.012			.003
			(.080)			(.002)
	Relative Effects, Chinese					
Chinese	5.993***	11.020***	9.365***	.038***	.194***	.095***
	(.198)	(.258)	(.362)	(.009)	(.010)	(.014)
Treated	−5.987***	−11.05***	−16.16***	−.218***	−.376***	−.364***
	(.675)	(.701)	(1.733)	(.020)	(.020)	(.058)
Possible treatment		−.500***	−.700***		−.021***	−.019***
		(.034)	(.095)		(.001)	(.003)
Years before restrictions			−.563***			.010***
			(.057)			(.002)
Years after restrictions			.241***			.002
			(.088)			(.003)
Observations	128,508	128,508	128,508	129,877	129,877	129,877
R^2	.034	.034	.034	.042	.044	.045

Source.—US Census, IPUMS, version 5.0, 1860–1930.

Note.—Controls for period, cohort, and age are included. The sample is limited to men aged 30–65. Immigrants are limited to those arriving prior to 1927. Where timing of immigration cannot be determined directly, treatment is defined as age 15 or younger at time of exclusion. Possible treatment is defined as residing in the United States during the exclusion era (1884–1928 for Chinese; 1908–28 for Japanese), excluding treated individuals, weighted by the number of years since restrictions were implemented.

*** Significant at 1 percent.

The main results indicate that Chinese immigrants of the exclusion era had worse occupational outcomes relative not only to restricted Japanese but to unrestricted Chinese immigrants as well. I discuss each of these findings in turn.

Weaker performance of restricted Chinese relative to restricted Japanese suggests that skill-biased restrictions resulted in weaker selection on skill for the Chinese. This is consistent with the economic conditions in the source countries. Returns to unobserved skill were likely higher in Japan than in China, so those with high observed skill and relatively low unobserved skill would have been more likely to emigrate, even before skill-based restrictions were imposed. The opposite was true in China, so unrestricted immigrants were primarily those with low observed skill and relatively high unobserved skill, in which case restrictions based on observed skill would have preempted migration for a much larger segment of the skill distribution. Consistent with this, table 1 shows that, among unrestricted immigrants, there was a much larger proportion of Japanese in skilled, professional, and merchant occupations, while the Chinese were heavily concentrated in the laborer category.

Since the Chinese and Japanese faced the same restrictions on observed skill, the difference-in-difference estimate should predominantly capture changes in selection on unobserved skill. But, given likely differences between China and Japan in skill prices and distributions, the estimated effect likely reflects changes in both observed and unobserved skill. In order to quantify the magnitude of this potential bias, we can look at a measure of observed skill separate from unobserved skill—literacy. These results (table 3, panel B) point to stronger selection on observed skill among the Chinese, relative to the Japanese, prior to exclusion, but the opposite after exclusion. However, the effect of treatment on literacy is stronger for the Japanese and more muted for the Chinese, compared to the results for occupation score: restrictions reduce (increase) the difference in literacy rates between whites and preexclusion Japanese (Chinese) by 66–80 percent (6–98 percent), compared to 30–40 percent (68–550 percent) for occupational score. Of course, literacy is not perfectly correlated with the "observed" skills targeted by the restrictions, and there is some ambiguity in whether literacy refers to English or any language. Still, the results suggest that, even if restrictions did weaken selection on observed skill, the effect on unobserved skill was much stronger. At most, the weaker performance of treated Chinese can be only partially explained by changes in selection on observed skill; the estimated changes in literacy rates suggest that selection on unobserved skill must have played a significant role as well. Moreover, it seems unlikely that selection on observed skill would have weakened at a time when immigration restrictions began targeting observed skills.

It is, however, surprising that restricted Chinese immigrants had lower skill, on average, than unrestricted Chinese immigrants. Within the context of the model described above, this suggests that, for the Chinese, not

only did skill-based restrictions cause greater negative selection on unobserved skill but, moreover, this effect entirely dominated any greater positive selection on observed skill. This is likely the result of conditions in China around the time of the Exclusion Act, which substantially weakened the typical positive correlation between observed and unobserved skills. Opportunities for entrepreneurship in China were limited to elites, substantially reducing the average returns to unobserved skill relative to observed skill and, therefore, creating differential incentives to invest as well as differential returns to migration for the two types of skill. Moreover, with a limited educational system available primarily to elites as well, many with high unobserved skill were excluded from the acquisition of observed skills, further weakening the usual positive correlation between observed and unobserved skills.

Still, de facto enforcement may not have been as successful as immigration records suggest, and this may have differed between Chinese and Japanese immigrants. Under the Gentlemen's Agreement, the Japanese government assumed responsibility for screening emigrants to the United States, agreeing to issue passports only to those eligible for entry, and Japanese immigrants were almost assured admission to the United States prior to making the journey. In contrast, under the Chinese Exclusion Acts, the Chinese government was required to provide official documentation of occupation status, but US customs officials were granted the authority to determine eligibility for entry. Chinese immigrants were subject to long detainment and interrogation periods, particularly at Angel Island, and anecdotal evidence suggests that customs officials had great discretion in evaluating documents for entry (Chang 2003). This essentially created a double hurdle for Chinese immigrants, resulting in a much more stringent burden of proof as well as greater risk inherent in the act of migration itself, given that eligibility of Chinese immigrants was evaluated after transportation costs had already been sunk. Increased risk would tend to encourage migration among those with lower reservation wages, and this may, in fact, be driving the results. Among those who met the criteria for legal immigration, the Chinese would tend to have lower reservation wages (lower unobserved skill, given the floor on observed skill), on average, as a result of greater uncertainty at the destination. However, among those not meeting the criteria for legal immigration, it seems likely that uncertainty and transportation costs were sufficiently high that even those with very low reservation wages would not find it optimal to migrate. Moreover, it seems unlikely that low-skill Chinese who were otherwise ineligible for entry would disproportionately choose to immigrate after restrictions were imposed, given the risk of being debarred, high transport costs, and the added cost of obtaining false documents.

Of course, restrictions could not prevent immigrants from switching occupations after being admitted, and census data suggest that occupations were often subject to change after arrival (table 4). In fact, what appears to be driving the results is not the change in occupations legislated by the

TABLE 4
Occupation of Foreign-Born Men, Aged 30–65, Before and After Enforcement of Skill-Based Immigration Restrictions

	Chinese		Japanese	
	Unrestricted	Restricted	Unrestricted	Restricted
Managers, officials, proprietors	5.61	13.08	3.12	7.12
Professionals	1.77	1.76	.84	1.69
Skilled	48.95	56.83	15.06	23.05
Laborers	37.91	24.30	72.96	51.53
Farmers	3.27	2.18	6.62	13.22
Nonoccupational response	2.51	1.84	1.41	3.39

Source.—US Census, IPUMS, version 5.0, 1860–1930.
Note.—Limited to foreign-born men arriving prior to 1927. Restricted is defined on the basis of year of immigration.

Chinese Exclusion Act and Gentlemen's Agreement but compositional change in the entire occupational distribution, that is, the extent of occupational upgrading/downgrading among immigrants after having arrived in the United States. This is consistent with the notion of unobserved skills determining how individuals transition from the initial occupations that are determined by observed skills. Less than 38 percent of Chinese census respondents immigrating before 1884 report being laborers, while immigration records indicate that over 95 percent of incoming Chinese immigrants during this period report laborer as their occupation. Conversely, immigration records indicate that no Chinese laborers were admitted between 1884 and 1890, while 24 percent of census respondents immigrating after 1883 reported laborer as their occupation. There is evidence of movement both up and down the occupational ladder, with upgrading more common among unrestricted Chinese immigrants and downgrading more common among restricted immigrants. Conversely, among the Japanese, occupational downgrading is more prevalent than upgrading both before and after skill-based restrictions. To the extent that unobserved skills are important for economic assimilation, greater occupational downgrading among restricted Chinese immigrants would also be indicative of weaker selection on unobserved skill.

A. *Differences in Restrictions*

There is also an important difference between the restrictions imposed on Chinese and Japanese immigrants that could be driving the results. The Chinese Exclusion Act specifically noted four exempted occupational categories (diplomats, teachers, students, and merchants), whereas the Gentlemen's Agreement referred only to nonlaborers, noted in immigration law as "persons whose work is neither distinctively manual nor mechanical, but rather professional, mercantile, or clerical." Thus, a greater number of professional and clerical Japanese immigrants may have been granted entry during the restricted era. To account for this, I

repeat the estimation excluding all individuals in professional and clerical occupations (table 3, col. 1). The point estimates are remarkably similar to the main results in table 3, suggesting that the results are not driven by more stringent restrictions for skilled Chinese immigrants.

B. Measurement Error

Given measurement error in the assignment of immigrants to the restricted versus unrestricted groups, I repeat the estimation using two alternative definitions for the treatment variable. First, I consider a narrow treatment window such that only individuals born after restrictions have been imposed are assigned to the treatment group. With this definition, it is even more likely that restricted immigrants are incorrectly assigned to the control group, further diluting the estimated treatment effect. Indeed, we see that the treatment variables retain the same sign and significance and are slightly smaller in magnitude (table 5, col. 2), although not statistically different from the main results discussed above. Another alternative is to exclude from the analysis all those for whom treatment status is imputed. However, as described above, given limited data on year of immigration, this definition of treatment excludes over 30 percent of all foreign-born Japanese males in the sample. Accordingly, the point estimates with this sample are quite noisy and not able to discern any significant difference between restricted and unrestricted immigrants (table 5, col. 3), although the point estimates have the same sign.

C. Cohort Composition

Alternatively, we might be concerned that the treatment effect is picking up underlying differences across immigrant cohorts rather than the effect of skill-based restrictions. To check this, I further restrict the sample of immigrants to only those arriving within 5 years before and after restrictions were implemented, again noting the caveat about sample attrition. As before, I find evidence of significantly improved selection among restricted Japanese immigrants and significantly weaker selection among restricted Chinese. The point estimates for both groups are much smaller in magnitude (col. 4), which could indicate that, while the exact timing of restrictions may have been exogenous, they were enacted in response to changing immigrant skill composition. However, restrictions are found to have opposite effects on Japanese and Chinese immigrants, while historical and immigration records suggest that both groups exhibited decreasing average skill prior to restrictions.

D. Return Migration

Finally, I consider whether the results may be driven by differences in the length of time in the United States and selective return migration. To do so, I control for the number of years since immigration, which again has

TABLE 5

THE EFFECT OF EXCLUSION ON OCCUPATION SCORE, ALTERNATIVE SPECIFICATIONS

	Select Occupations Only[a,b] (1)	Narrow Treatment Window[b,c] (2)	No Imputed Treatment[b,d] (3)	5 Years Before/ After Restriction[e] (4)	Labor Market Assimilation[b] (5)
Asian	−10.80***	−11.46***	−9.291***	−8.125***	−10.86***
	(.168)	(.175)	(.434)	(.399)	(.571)
Treated	3.814***	4.742***	−.246	1.959**	1.935**
	(1.187)	(1.333)	(.706)	(.996)	(.874)
Years since immigration					.189***
					(.049)
	Relative Effects, Chinese				
Chinese	9.930***	9.525***	5.985***	4.454***	7.495***
	(.343)	(.367)	(.538)	(.581)	(.656)
Treated	−15.61***	−10.54***	−1.037	−3.286***	−3.034***
	(1.605)	(1.793)	(.867)	(1.267)	(1.076)
Years since immigration					−.174***
					(.055)
Observations	119,269	128,508	120,074	73,184	113,666
R^2	.046	.034	.028	.017	.028

Source.—US Census, IPUMS, version 5.0, 1860–1930.

Note.—The sample is limited to men aged 30–65. Controls for period, cohort, and age are included. Immigrants are limited to those arriving prior to 1927.

[a] Excludes individuals in professional and clerical occupations; includes controls for possible treatment.

[b] Includes controls for years before/after restrictions were implemented.

[c] Where timing of immigration cannot be determined directly, treatment is defined as being born after restrictions were implemented. Includes controls for possible treatment (residing in the United States during the exclusion era [1884–1928 for Chinese; 1908–28 for Japanese], excluding treated individuals, weighted by the number of years since restrictions were implemented) and years before/after restrictions.

[d] Immigrants are limited to those for whom timing of immigration can be determined directly from census year and/or year of immigration.

[e] Limited to 1880–1930. Immigrants are limited to those arriving 5 years before/after restrictions were implemented.

** Significant at 5 percent.

*** Significant at 1 percent.

the caveat of restricting the sample to those for which the timing of immigration (before/after restrictions) is known with certainty. For census records prior to 1900, the first year in which year of immigration was enumerated, I impute years since immigration with age minus 15 (one standard deviation below the mean age of immigration) using a lower bound of zero. Including years of immigration does not affect the main results, although the point estimates are significantly smaller in magnitude (table 5, col. 5). As expected, we see that time in the United States has a significant positive effect on occupational performance, consistent with labor market assimilation (Chiswick 1978). However, the relative effect for Chinese immigrants is negative, suggesting that Chinese immigrants may have been slower to acquire society-specific human capital. Alternatively, the Chinese immigrants who were able to achieve the most occupational upgrading may have also been the most likely to return. Thus, society-specific capital and/or return migration may have offset some of the positive effects of skill-based restrictions, but restricted Chinese immigrants still exhibit worse occupational outcomes, on average, than their unrestricted counterparts, even accounting for time spent in the United States.

In summary, the results indicate that immigration restrictions—albeit somewhat narrowly defined on the basis of occupation—had very heterogeneous effects, even when heavily biased in favor of higher skill. The occupation-based restrictions applied to the Chinese and Japanese in the late nineteenth and early twentieth centuries significantly improved the occupational standing of Japanese immigrants, while the occupational scores of Chinese immigrants actually declined during the exclusion era. The results are robust to alternative specifications as well as to controlling for labor market assimilation. Since both groups were subject to the same restrictions, these contrasting effects suggest that skills were multidimensional with a relatively high degree of substitutability. Moreover, for the Chinese, any increased positive selection on the targeted (observed) skills was more than offset by changes in selection on untargeted (unobserved) skills. This is consistent with the differing economic conditions in China and Japan. The latter likely had relatively higher returns to unobserved skill such that, with total skill held constant, the skill composition of unrestricted émigrés was already skewed in favor of observed skill. Conversely, with relatively lower returns to unobserved skill in China, émigrés would have selected more strongly on unobserved skill, resulting in a larger change to the overall skill composition after restrictions were implemented.

V. Intergenerational Transmission of Skill

Given that human capital is transmitted intergenerationally, we can also estimate the longer-term effect of exclusionary legislation by examining wages for second-generation Chinese and Japanese. Suppose, similarly to Borjas (1993), that skills are transmitted from generation $t - 1$ to generation t according to the following Markov process:

$$x_t = s_t + k_t = \rho^i(s_{t-1} + k_{t-1}) + \epsilon_t, \tag{4}$$

where ρ captures the extent of intergenerational transmission for ethnic group i. The average skill difference between children of restricted and unrestricted Chinese and Japanese immigrants can then be expressed as

$$\begin{aligned}&\rho^C[E(x_{t-1,C} \mid I = 1,\ T_2 = 1) - E(x_{t-1,C} \mid I = 1,\ T_2 = 0)]\\ &- \rho^J[E(x_{t-1,J} \mid I = 1,\ T_2 = 1) - E(x_{t-1,J} \mid I = 1,\ T_2 = 0)],\end{aligned}$$

where I refers to the immigration decision of the parent(s), and T_2 refers to having parents who were "treated" by exclusionary immigration laws. Dropping I for ease of notation and rearranging terms, we see that the above expression is positive if and only if

$$\begin{aligned}&[E(x_{t-1,C} \mid T_2 = 1) - E(x_{t-1,C} \mid T_2 = 0)]\\ &- [E(x_{t-1,J} \mid T_2 = 1) - E(x_{t-1,J} \mid T_2 = 0)]\\ &> [(\rho_n^J/\rho_n^C) - 1][E(x_{t-1,J} \mid T_2 = 1) - E(x_{t-1,J} \mid T_2 = 0)].\end{aligned} \tag{5}$$

The difference-in-difference then reflects both the effect of exclusionary legislation on parents' skill and the extent to which these skills are transmitted across generations. The left-hand-side expression is exactly the difference-in-difference for first-generation immigrants, which we know from the preceding analysis is negative (i.e., immigration restrictions increase the average skill of immigrants less for the Chinese than for the Japanese). Then, given $E(x_{t-1,J} \mid T_2 = 1) - E(x_{t-1,J} \mid T_2 = 0) > 0$ (restricted Japanese immigrants have higher skill than those who were unrestricted), if the difference-in-difference estimate for second-generation Chinese is positive (negative), it must be the case that $\rho_n^J < \rho_n^C$ ($\rho_n^J > \rho_n^C$). However, because fertility (F) is endogenous, the distribution of skill may differ across immigrants with and without children, suggesting that

$$E(x_{t-1} \mid T_2 = 1) = E(x_{t-1} \mid T = 1,\ F = 1) \neq E(x_{t-1} \mid T = 1). \tag{6}$$

Determining the sign of $\rho_n^C - \rho_n^J$, the difference in preferences for human capital investments across ethnic groups, then requires estimating the relationship between skill and fertility, as well as how it may have been affected by exclusionary immigration policies.

Using an approach analogous to the one for first-generation immigrants, I compare American-born Chinese and Japanese on the basis of whether their parents migrated under the exclusionary laws to estimate the effect of parental treatment on children's outcomes. I limit attention to males aged 30–65 to abstract from both labor market choices and gender differences in the returns to human capital. Individuals are assigned to the treatment group on the basis of birth year and parents' place of birth. All persons born in the United States during the exclusion period

with at least one foreign-born parent are considered treated in the sense that their parent(s) likely immigrated under the restricted laws. Analogous to the analysis for first-generation immigrants, I omit those born after 1928 in order to compare children whose parents migrated under more similar conditions. All third- and higher-generation Chinese and Japanese are omitted as well. Again, given the coarseness of this measure, I include a measure for possible treatment (born during the exclusion era to parents of unknown origin, weighted by the number of years between the birth year and the cutoff year of 1928).

Among those whose parents were unrestricted by exclusionary laws, the Japanese have higher occupational scores than the Chinese, but among those whose parents were subject to exclusionary laws, the Chinese score significantly higher than the Japanese (table 6). In fact, being the child of a treated immigrant reverses the positive gap between second-generation Japanese and native whites and completely eliminates the negative gap between second-generation Chinese and native whites. This result is robust to including differential time trends (before restrictions were implemented and after they were repealed), as well as an alternative definition of treatment (born within 10 years after restrictions were implemented). Additionally, I repeat the estimation excluding a subset of observations from the western states of Washington, Oregon, California, Alaska, and Hawaii. As a result of World War II and the internment of 1942–45, Japanese Americans in these states may have experienced greater discrimination in both labor markets and educational institutions, as well as interruptions in their education or labor market trajectories. This would result in relatively worse outcomes for the Japanese, particularly among the second generation who would have acquired their education and work experience exclusively in the United States. To test for this, I exclude from the sample all individuals between the ages of 7 and 17 during the internment period and residing in the states affected by internment. Estimates for this sample are qualitatively unchanged and quantitatively quite similar as well, suggesting that the main results are not explained by differential treatment and discrimination of Chinese and Japanese immigrants.

That children of restricted Chinese immigrants had better occupational outcomes than children of restricted Japanese immigrants is surprising given that the reverse was true among the immigrants themselves. From expressions (5) and (6), we see that there are two ways to reconcile these results. One possibility is that exclusion allowed only a highly selective group of individuals to marry and have children. This effect, if present, would likely have been more pronounced for the Chinese, as wives and minor children of Japanese laborers domiciled in the United States were granted entry (Inui 1925) until the Immigration Act of 1924, while the Chinese Exclusion Acts offered no such exception. Thus, Chinese immigrants who both entered the United States and had children during the exclusion era are more likely to have immigrated with their spouses and/or families, which may be indicative of higher skill. To test for this directly,

TABLE 6
THE EFFECT OF EXCLUSION ON OCCUPATION SCORE AMONG THE SECOND GENERATION, DIFFERENCE-IN-DIFFERENCE ESTIMATES

	Main Sample[a] (1)	Differential Trends[a,b] (2)	Expanded Treatment Window[c] (3)	Excluding Western States[a,d] (4)
Asian	2.219***	−1.246	1.959***	2.321***
	(.141)	(.888)	(.170)	(.154)
Treated	−3.317***	−.877	−3.212***	−3.442***
	(.432)	(.666)	(.470)	(.445)
	Relative Effects, Chinese			
Chinese	−3.560***	−2.375*	−3.555***	−3.898***
	(.727)	(1.245)	(.681)	(.767)
Treated	4.877***	2.463*	6.498***	5.305***
	(1.259)	(1.328)	(1.367)	(1.300)
Observations	184,223	184,223	185,167	180,601
R^2	.057	.057	.057	.057

Source.—US Census, IPUMS, version 5.0, 1860–2000.
Note.—The sample is limited to men aged 30–65. Controls for period, cohort, and age are included. Chinese and Japanese are limited to those born in the United States prior to 1927. Controls are included for possible treatment (born after restrictions were implemented with parents of unknown origin, weighted by difference between birth year and end of period).
[a] Treatment is defined as being born after restrictions were implemented.
[b] Includes controls for number of years before restrictions were implemented and number of years after restrictions were repealed.
[c] Treatment is defined as being born within 10 years after restrictions were implemented. Chinese and Japanese are limited to those born in the United States prior to 1939.
[d] Excludes individuals aged 7–17 residing in Washington State, Oregon, California, Alaska, or Hawaii during Japanese American internment.
* Significant at 10 percent.
*** Significant at 1 percent.

I return to the sample of immigrants and add an indicator for having at least one child currently living in the household, interacted with ethnic group and treatment status (table 7).[10] While having a child at home is positively correlated with the occupational score, and even more so for the Chinese, I find no evidence of a change in the relationship between fertility and skill after exclusionary laws are implemented. Therefore, the results do not appear to be driven by differential selection into childbearing, as the effect of immigration restrictions does not differ with fertility.

The other possible explanation is that Chinese immigrants invested more heavily in their children's human capital, relative to the Japanese ($\rho_n^J < \rho_n^C$), despite being drawn themselves from the lower portion of the distribution for unobserved skill. This is consistent with the notion of a "middleman minority" (Bonacich 1973; Hirschman and Wong 1986), a minority racial or ethnic group that comes to occupy a "middleman" position

[10] This is, unfortunately, the best proxy for fertility that is available prior to the Chinese Exclusion Act of 1882. Controls for age, period, and cohort are included to help account for differences in home leaving and coresidence.

TABLE 7
THE EFFECT OF EXCLUSION ON FERTILITY AMONG THE FOREIGN-BORN, DIFFERENCE-IN-DIFFERENCE ESTIMATES

	Occupation Score (1)	Number of Kids at Home (2)
Asian	−11.89***	−.800***
	(.191)	(.044)
Treated	5.061***	−1.829***
	(1.336)	(.295)
Any kids at home	2.329***	
	(.257)	
Any kids × treatment	.884	
	(1.336)	
	Relative Effects, Chinese	
Chinese	9.967***	−.185
	(.364)	(.216)
Treated	−16.53***	1.956***
	(1.725)	(.639)
Any kids at home	2.649***	
	(.693)	
Any kids × treatment	1.444	
	(1.804)	
Observations	128,508	70,325
R^2	.035	.074

Source.—US Census, IPUMS, version 5.0, 1860–1930.
Note.—The sample is limited to men aged 30–65. Controls for period, cohort, and age are included. Immigrants are limited to those arriving prior to 1927. Where timing of immigration cannot be determined directly, treatment is defined as age 15 or younger at time of exclusion. Possible treatment is defined as residing in the United States during the exclusion era (1884–1928 for Chinese; 1908–28 for Japanese), excluding treated individuals, weighted by the number of years since restrictions were implemented. Number of kids at home is defined as the number of children ever born, conditional on having at least one child. Any kids at home is defined as having at least one child currently living in the household.
*** Significant at 1 percent.

between the majority group and another, lower-status minority group. This, along with a sojourner mentality and, in the case of the Chinese, exclusionary laws pertaining specifically to occupation, tends to foster occupational concentration and intensive investment in both human and economic capital, resulting in rapid convergence and even surpassing of the majority. Of course, both the Chinese and Japanese fit this model in various respects, but the results here suggest that preferences for human capital investment, in addition to economic conditions and self-selection, have also played a role, with the Chinese displaying stronger preferences for human capital than the Japanese. However, Chiswick (1988) suggests that what appear to be preferences may, in fact, be differences in the relative prices of child quantity and quality (e.g., as a result of urban vs. rural residence or greater education of mothers). This does help explain differences between some groups (e.g., Jews and Mexicans), but Chinese and

Japanese immigrants, for whom data are available, display similar labor force participation rates and fertility rates in the United States, with fertility actually being slightly higher for the Chinese (Chiswick 1988). And, when I rerun the difference-in-differences specification with number of children at home, conditional on having children, as the dependent variable, I find evidence of significantly higher fertility rates for restricted Chinese immigrants (table 7).

It is also important to note that preferences for human capital investment may apply to the first generation, the second generation, or both. That is, we could consider a richer model of human capital investment in which immigrants may continue to acquire skills after arriving at the destination. Then, with parental altruism and/or direct preferences for children's human capital, parents' investment decisions will incorporate the benefit of human capital transfer across generations (Ehrlich and Kim 2007). Immigrant parents may even be willing to endure a period of occupational downgrading at the destination in order to invest in location-specific (unobserved) skills that have a high degree of intergenerational transmission (Mayer 2008; Gradstein 2009). Empirically, it is difficult to distinguish between direct human capital investments in the second generation and the transfer of postimmigration human capital investments made by first-generation immigrants. Without sufficient data on actual earnings, we can discern only human capital investments that result in a change in occupational category, not those that increase earnings within an occupation, and we cannot discern any investments that do not yield a return until the subsequent generation. And, while the ultimate effect on the second generation may be the same, these two explanations can have very different implications: postimmigration investment in human capital could suggest more rapid "catch-up" among immigrants themselves, while investment in the human capital of the second generation would imply greater intergenerational inequality, perhaps with additional implications for old-age support and public finance.

Finally, we can look at some more direct measures of human capital for evidence of greater investment among exclusion-era Chinese immigrants. For high school and college completion, we observe the same pattern as for occupational score (table 8). Among the Japanese, children of restricted immigrants have lower high school and college completion rates than children of unrestricted immigrants, while the net effect is close to zero for children of restricted Chinese immigrants. However, exclusionary laws are found to have the opposite effect for literacy: children of restricted Japanese immigrants have higher literacy rates, and the relative effect for children of restricted Chinese immigrants is negative. This suggests that exclusionary laws had heterogeneous effects on different parts of the immigrant skill distribution. Among the Japanese, restrictions succeeded in increasing the average skill of incoming immigrants, primarily by increasing positive selection on observed skill (occupation). But chil-

TABLE 8
THE EFFECT OF EXCLUSION ON HUMAN CAPITAL AMONG THE SECOND GENERATION, DIFFERENCE-IN-DIFFERENCE ESTIMATES

	Occupation Score (1)	Literacy (2)	High School (3)	College (4)
Asian	2.089***	.048***	−.162***	.140***
	(.132)	(.003)	(.004)	(.005)
Treated	−3.218***	.110***	−.095***	−.131***
	(.429)	(.012)	(.015)	(.012)
	Relative Effects, Chinese			
Chinese	.091	.031	−.003	−.039**
	(.860)	(.021)	(.017)	(.017)
Treated	2.896*	−.148***	.154***	.144***
	(1.478)	(.046)	(.048)	(.044)
Observations	85,575	85,575	85,491	85,208
R^2	.038	.235	.26	.058

Source.—US Census, IPUMS, version 5.0, 1940–2000.
Note.—The sample is limited to men aged 30–65. Information on high school and college completion is available only beginning in 1940, so all outcomes are limited to this period. Controls for period, cohort, and age are included. Chinese and Japanese are limited to those born in the United States prior to 1929. Treatment is defined as born after restrictions were implemented with at least one foreign-born parent. Because literacy is not recorded after 1930, this measure is constructed from schooling attainment. An individual is considered literate if he or she has completed at least the fifth grade. High school and college refer to completion of the schooling level.
* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

dren of restricted Japanese immigrants did better than children of unrestricted immigrants only in terms of literacy, which suggests stronger transmission of skill only at the lower tail of the distribution. Conversely, among the Chinese, restrictions reduced the average skill of incoming immigrants, likely by reducing positive selection on unobserved skill. However, the intergenerational transmission of skill seems to have been much stronger, if only at the upper tail of the distribution. While a more thorough discussion is beyond the scope of this paper, these results do suggest differing relationships between observed and unobserved skill and the intergenerational transmission of skill.

VI. Conclusion

Chinese immigration to the United States, as with all immigration, has been highly selective over time. But, unlike many other ethnic groups, the Chinese were subject to skill-based restrictions, even before numerical quotas. Per the Chinese Exclusion Act of 1882, immigration of all laborers was prohibited, and only merchants, teachers, students, and officials were granted entry. However, these restrictions did not increase the av-

erage skill level among new arrivals. Rather, I find that Chinese immigrants of the exclusion era have worse occupational outcomes relative to both Japanese immigrants, who were subject to the same restrictions, and unrestricted Chinese immigrants. This points to significant substitution between observed (targeted) and unobserved skills and suggests that skill-based restrictions may not be successful in altering the overall skill composition of incoming immigrants, depending on the skills that are targeted and the scope for substitution among skills.

However, when we turn to the second generation, the data suggest that there may be some truth to the "model minority" label. Among the Chinese, human capital accumulation was much more intensive for children of restricted immigrants, compared to children of unrestricted immigrants, despite restricted immigrants themselves having lower aggregate skill. This suggests particularly strong intergenerational transmission of skill. Moreover, we observe the opposite effect among the Japanese, suggesting that it is not immigrant selectivity per se driving the results; rather, group-specific preferences for human capital seem to matter as well. And, again, the impact of skill-based restrictions appears to be heterogeneous; for the group in which restrictions succeeded in increasing the skill of immigrants, the gains were largely undone in the subsequent generation, while for the group in which restrictions were unsuccessful, significant gains were observed in the second generation. Additionally, I find suggestive evidence that these effects were heterogeneous throughout the skill distribution, even within ethnic groups. Taken together, the results suggest that the impact of skill-based immigration restrictions is not always straightforward and should be the subject of further research.

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