Call Me by Your Name: Immigration Restriction Laws and Child Naming in the Early Twentieth Century U.S.[*]

Dafeng Xu[†]

Abstract

Researchers point out that names are signals of cultural identity. I follow the classical measure of name foreignness and estimate effects of immigration restriction laws on child naming in the early twentieth century U.S. I find significant evidence that after the passage of immigration restriction laws, there was a particular decline in name foreignness among second-generation immigrant children whose parents were from more restricted countries. The results are robust to changes to samples and specifications, and the effects of immigration restriction laws on child naming were unlikely to be through other channels, such as parents' assimilation and selection on migration.

Keywords: immigration restriction laws, child naming, name foreignness, assimilation, early twentieth century U.S.

1 Introduction

Researchers point out that names are signals of cultural identity. Anthropologists and human biologists have long observed that names are related to transmission of cultural identity across generations (e.g., Chibnik, 1991; Guglielmino et al., 2000). Earlier findings based on small ethnographic data have been supported by recent quantitative studies based on larger samples. For example, Lieberson and Mikelson (1995) find that many African American parents create unique given names for children. Further studies (Cook et al., 2014) suggest that this is due to long-standing cultural norms among African Americans. Similarly, Hacker (1999) examines the relationship between parental religiosity and children's biblical names in the late nineteenth century U.S., and observes the association between child naming and trends in marital fertility.

As the reflection of cultural transmission, child naming is equivalently—if not more—important for immigrants. Having unique patterns of children's given names signals ethnic maintenance, while giving more localized names to children signals cultural assimilation (Gordon, 1964). In the age of mass migration, immigrant families eventually closed half of the gap in child naming with natives (Abramitzky et al., 2017), which suggests possible heterogeneity in child naming among immigrant families, in the sense that some parents were culturally more assimilated when making naming decisions, while some parents tended to keep cultural identity by giving ethnic names to their children.

In this paper, I investigate the effects of immigration restriction laws on child naming in the early twentieth century U.S. The U.S. passed immigration restriction laws in the early 1920s (Ngai, 1999, 2004), which severely limited new immigration from Southern and Eastern Europe; on the other hand, immigration from Western and Northern Europe was less restricted, and immigration from Americas was not restricted. Although immigration restriction laws did not directly target old immigrants who had settled down in the U.S., the old immigrants might still have behavioral responses to immigration restriction laws, which were passed in a social environment of anti-immigration populism (Higham,

1963; Goldin, 1994; Ngai, 1999; Haines, 2000). The anti-immigration attitude had been developed among the native-born population long before immigration restriction laws were passed. However, the passage of immigration restriction laws reflected the official closure of the "open borders" of the U.S. (Ngai, 2004), and signaled that such an anti-immigration attitude approached its peak in the 1920s. In the empirical analysis, I calculate name foreignness among second-generation immigrants by cohort in the 1930 census, and estimate differences in patterns of child naming between more and less restricted groups, before and after the passage of immigration restriction laws.

The main results of this paper suggest that immigration restriction laws led to the particular decline in name foreignness among immigrant children whose parents came from more restricted countries, while such trends in child naming were generally insignificant prior to the passage of immigration laws. Names of both males and females were affected. This indicates that immigrant parents gave less-foreign first names to their children as responses to immigration restriction laws, even if they (and especially their native-born children) had settled down in the U.S. and were not directly restricted.

I then test the robustness of the main results. One concern about the control group (i.e., immigrant groups that were less or not restricted) is that (a) many immigrant families in the control group were originally from English-speaking countries, and (b) some immigrant groups might have unique trajectories of name-based assimilation even in the absence of immigration laws (Abramitzky et al., 2017). I discuss the construction of the control group, and show that the main results are arguably not driven by the sample. Several additional tests suggest that the main results are robust to changes to specifications, and placebo tests yield insignificant results.

I further discuss other channels through which immigration restriction laws might affect child naming. I first examine impacts of parents' assimilation by focusing on children who were born when their immigrant parents had stayed in the U.S. long enough. The results are similar to the main findings. Another possible channel was selection on migration. There

were disproportionately fewer immigrant parents from more restricted countries after the passage of immigration restriction laws. Moreover, immigration laws led to selection on both in-migration and out-migration (Massey, 2016; Ward, 2017). I argue that this channel was unlikely to be the main mechanism behind trends in child naming.

This paper adds to the literature of demography and population economics along several dimensions. First, it relates to prior research on the effects of immigration restriction laws in the early twentieth century U.S. Scholars find that immigration restriction laws passed in the early 1920s affected both the U.S. economy (Goldin, 1994) and demographics (Haines, 2000). In this paper, I further show that immigration restriction laws affected the process of cultural assimilation in terms of child naming among immigrant families that had arrived and settled down in the U.S.

Second, this paper has relevant policy implications, as it relates to a large body of literature of name foreignness. Many studies document the relationship between names and earnings (e.g., Arai and Thoursie, 2009), which can be explained by both assimilation (Gerhard and Hans, 2009) and discrimination (Bertrand and Mullainathan, 2004; Oreopoulous, 2011; Rubinstein and Brenner, 2014). In this historical context, researchers also find that name foreignness was negatively related to labor market outcomes (Biavaschi et al., 2017; Abramitzky et al., 2017), which was similarly because of both assimilation (Goldstein and Steklov, 2014) and discrimination (Moser, 2012). This paper provides empirical evidence that policy shocks such as the passage of immigration restriction laws could cause sudden changes in patterns of child naming among immigrant families.

The above point leads to another policy relevant question: was the change in child naming a "good" result for the country? Prior to the passage of immigration restriction laws, the gap in child naming was partially erased between second-generation immigrants and natives (Abramitzky et al., 2017), suggesting that immigrant families started to assimilated, but still maintained own cultural identity at a moderate degree. This paper shows that child naming—as a proxy for cultural transmission—was affected by immigration restric-

tion laws, signaling the possible decline in cultural diversity. Immigration restriction laws could have effects on economic growth through the influences on cultural diversity (Lian and Oneal, 1997; Ottaviano and Peri, 2006; Ager and Brückner, 2013), and such effects might even persist in the long run (Sequeira et al., 2017).

The rest of the paper is structured as follows. Section 2 introduces the background of this paper. Section 3 discusses data and empirical strategies. Section 4 reports the empirical results of this paper. Section 5 concludes the paper.

2 Historical Background

I now introduce the background of this paper. I start with the discussion of immigration restriction laws in the early twentieth century U.S. I then discuss patterns of given names among immigrants in the historical context of this paper.

2.1 Immigration Restriction Laws in the Early Twentieth Century

I first briefly discuss immigration restriction laws in the early twentieth century U.S. The U.S. remained "open borders" in the age of mass migration, absorbing nearly 30 million immigrants (Haines, 2000; Ngai, 2004; Abramitzky and Boustan, 2017) in the late nineteenth and early twentieth century; the majority of immigrants were from Europe. Researchers find empirical evidence of long-run positive effects of immigration on the U.S. economy. For example, Dunlevy and Hutchinson (1999) observe positive effects of immigration on transatlantic trade flows. Kim (2007) finds that immigration in the age of mass migration contributed to the spread of factories and even the growth of cities by changing the local labor market structure. Moser et al. (2012) show that migration of German Jewish scientists encouraged U.S. invention in and after the 1930s. In sum, immigration generally has positive effects on income, educational attainment, and urbanization at the local level, and the effects appear to be larger in the long run (Sequeira et al., 2017).

However, mass immigration from Europe also had negative effects on some social and economic outcomes for natives. Researchers observe the negative impacts in the U.S. labor market caused by immigration. For example, Goldin (1994) finds that the increase in the immigration population led to the decline in wages of unskilled workers. Using data on a longer historical period, Biavaschi (2013) observes the qualitatively similar conclusion, although with a smaller magnitude of the negative effect of immigration. There are also other "indirect" evidence pointing out negative impacts of immigration. Hatton and Williamson (1998) argue that immigrants might crowded out the native labor force. Collins (1997) studies the interaction between immigration and migration of African Americans, and finds that Great Black Migration was delayed by immigration from Europe. In sum, immigration might not necessarily generate positive impacts on the economy that benefited the majority of the labor force, especially in the short run. As a result, discrimination towards immigrants was not rare (e.g., Higham, 1963; Brown and Warner, 1992; Moser, 2012): using city-level data, Tabellini (2018) shows that in the early twentieth century, political opposition to immigration arose even if immigration encouraged economic growth. Such a social environment of anti-immigration populism finally led to the passage of immigration restriction laws in the 1920s (Goldin, 1994; Ngai, 1999).

Immigration restriction laws were not a "one-shot" policy. There were several relevant laws enacted in the 1900s and 1910s, which were, however, more like regulations rather than restrictions (such as the Naturalization Act of 1906 that required immigrants to learn English). The 1921 immigration restriction law was the first act that restricted the number of new arrivals using a quota system, which was based on sizes of immigrant populations in the 1910 census (Ngai, 1999). This was later replaced by the 1924 immigration restriction laws, in which the quota system was based on the 1890 census (Ngai, 2004). Restrictions on new arrivals were much more severe in the 1924 law, and thus effects of the 1924 law on social, economic, and demographic outcomes were much larger than those of the 1921 law (Hatton and Willaimson, 1998; Ngai, 1999; Haines, 2000). Similar to other related papers,

this paper focuses on effects of the 1924 immigration restriction law, but the effects might be observed as early as 1921. I will further discuss this in Section 4.

The passage of immigration restriction laws generated immediate effects on the U.S. demographics. Although the quota system limited all source countries in Europe, the effects were highly heterogeneous, as quotas were origin-specific (Ngai, 2004). Immigration from Southern and Eastern Europe ("new source countries" such as Russia) was severely restricted, while immigration from Northern and Western Europe ("old source countries" such as the U.K.) was less restricted. Researchers find that immigration restriction laws led to positive selection on in-migration and negative selection on out-migration (Greenwood and Ward, 2015; Massey, 2016). Furthermore, while return migration back to Europe was not rare (Haines, 2000), immigrants originally from more restricted countries became less likely to leave the U.S. after the passage of immigration laws (Wood, 2017). Immigration restriction laws also changed the pattern of immigrant segregation (Xu, 2018). Finally, researchers document economic consequences of immigration restriction laws. Ager and Hanson (2018) find that after the passage of immigration laws, productivity in the manufacturing sector declined, native white workers faced earning losses after the passage of immigration laws, but black workers improved their economic status. Earlier studies of income inequality in the 1940s and 1950s (Goldin and Margo, 1992) point out that the wage compression was partially due to the decline in the labor supply of immigrants (Maloney, 1994), which occurred after immigration restriction laws were passed (Goldin, 1994).

2.2 Immigrants' Naming Patterns

I now focus specifically on U.S. immigrants' naming patterns in the age of mass migration. Researchers have long pointed out that (first) name assimilation is a typical signal of cultural assimilation (Gordon, 1964; Zelinsky, 1970; Lieberson and Bell, 1992; Watkins and London, 1994; Sue and Telles, 2007). Abramitzky et al. (2017) study trends in child naming among immigrants in the age of mass migration, and find that immigrant families

eventually closed half of the "name gap" with natives in the second generation. In particular, many immigrants gave less foreign names to their children, while there were still some second-generation immigrants who had foreign-sounding first names.

Economic historians observe that name assimilation generally led to better labor market outcomes among both first- and second-generation immigrants in the age of mass migration (Abramitzky et al., 2017; Biavaschi et al., 2017). This is similar to findings of the effects of names in the contemporary society (e.g., Arai and Thoursie, 2009). One explanation is that immigrants in the early twentieth century U.S. experienced economic assimilation after arrival, and economic assimilation (reflected by earnings) and cultural assimilation (reflected by naming) were positively correlated, as both economic assimilation and cultural assimilation were the function of years since migration (Goldstein and Stecklov, 2014; Abramitzky et al., 2017). Another explanation is that immigrant families tried to avoid name-based discrimination, which has been found in many societies (e.g., Bertrand and Mullainathan, 2004; Oreopoulos, 2011; Rubinstein and Brenner, 2014), including in the early twentieth century U.S. (Moser, 2012). In particular, in the context of this paper, immigration restriction laws might create sudden changes in anti-immigration attitudes, or at least changes in immigrants' opinions regarding natives' attitudes towards immigration, which could further lead to behavioral responses to the passage of immigration restriction laws.

On the other hand, keeping cultural identity (such as names) might still be economically beneficial. Many immigrant families settle down in ethnic enclaves, especially upon arrival (Massey and Denton, 1985; Bartel, 1989). It is possible that immigrants receive help and support from ethnic social networks (e.g., Edin et al., 2003; Munshi, 2003; Damm, 2009). Furthermore, immigrants might highlight the value of identity (Akerlof and Kranton, 2000) and thus keep cultural identity for themselves and their children (Casey and Dustmann, 2010), even if it is economically not beneficial (Battu et al., 2007; Battu and Zenou, 2010). These could explain that not all immigrant families chose to culturally assimilate into the U.S. by adopting Americanized child naming patterns.

3 Data and Empirical Strategies

In this section, I introduce data and empirical strategies. I first introduce the 1930 full-count census, and discuss the construction of the sample used in the empirical analysis. I then present descriptive statistics. Subsequently, I analyze the empirical strategies for estimating the effects of immigration restriction laws on child naming.

3.1 Data and Descriptive Statistics

This paper uses the 1930 full-count U.S. census from IPUMS (Ruggles et al., 2017). The main empirical analysis focuses on second-generation immigrants in the 1930 census. I restrict the sample to second-generation immigrants (a) who were born in the U.S., (b) whose parents were born in the *same* (foreign) country of origin in Europe or Americas, (c) who lived with parents in 1930, and (d) who were younger than 17 years old. Note that most individuals younger than 17 years old lived with their parents in 1930.

Table 1 presents cohort sizes by age. U.S. censuses survey age instead of year of birth, hence it is impossible to perfectly associate age with year of birth. In this paper, I consider the cohort of age 5 and 6 (born between late 1923 and early 1925) as the reference group (omitted in the regression analysis). Second-immigrants younger than those in this group are considered to be exposed to the *treatment*, i.e., immigration restriction laws. I split the sample into two groups by origin: the target group consists of second-generation immigrants whose parents came from more restricted regions, i.e., Southern and Eastern Europe (Ngai, 2004). The control group consists of second-generation immigrants whose parents came from less restricted regions, including Northern and Western Europe, and Americas where immigrants were not restricted at all.

Note that one might worry about the construction of the control group. Specifically, (a) many immigrant families were of origins in English-speaking countries (e.g., the U.K. and Canada); (b) immigrant families of different origins might follow different trajectories of

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Table 1: Cohort Sizes: Second-Generation Immigrants

	Full sample	Target (More res.)	Control (Less res.)	Control (Alter.)†
Age 16	432,594	306,524	126,430	50,063
Age 15	425,212	304,580	120,632	46,714
Age 14	428,562	310,090	118,472	45,872
Age 13	417,022	306,921	110,101	43,091
Age 12	431,221	322,196	109,025	45,495
Age 11	375,839	282,612	93,227	40,629
Age 10	392,461	299,988	92.473	45,465
Age 9	369,008	286,808	82,200	42,700
Age 8	370,567	289,929	81,038	43,871
Age 7	341,277	269,165	72,112	41,279
Age 6	332,833	262,168	70,665	40,950
Age 5	315,856	248,097	67,759	40,706
Age 4	283,422	222,417	61,005	36,563
Age 3	276,242	216,298	59,944	37,790
Age 2	253,016	197,701	55,315	34,391
Age 1	221,295	172,603	48,692	32,472
Age 0	210,209	163,505	46,704	36,270

The alternative control group excludes individuals whose origins were of countries with West Germanic languages (e.g., English and Dutch).

name-based assimilation even in the absence of immigration restriction laws, and in particular, researchers find that immigrant families from culturally close countries had *slower* rates of name-based assimilation (Abramitzky et al., 2017). I further construct an alternative control group that is culturally more comparable to the target group. This control group consists of second-generation immigrants whose parents were *not* from countries where a West Germanic language (e.g., English, German, Dutch) was the major language; such origins in this control group include, for example, Mexico and France.

Table 1 shows significant declining trends in newborns from the cohort of age 16 (born in late 1913 or early 1914) to the cohort of age 0 (born in late 1929 or early 1930). This, however, was unlikely to be correlated with immigration restriction laws, as Table 1 shows that trends in childbearing in the target and control group were fairly parallel, both before and after the passage of immigration restriction laws.

In the empirical analysis of this paper, the dependent variable F is the index of name foreignness used by Fryer and Levitt (2004). In a similar historical context, Abramitzky et al. (2017) use the same variable to measure name foreignness among second-generation immigrants. In particular, for a specific name, F is calculated as:

$$F_{name} = 100 \times \frac{\frac{\# \ foreigner_{name}}{total \ \# \ foreigners}}{\frac{\# \ foreigner_{name}}{total \ \# \ foreigners} + \frac{\# \ natives_{name}}{total \ \# \ natives}} \tag{1}$$

Table 2: Second-Generation Immigrants' Name Foreignness

	Full sample	Target (More res.)	Control (Less res.)	Control (Alter.)
Pre-Legislation Cohorts:				
Name foreignness	53.381	54.807	49.156	53.052
	(22.021)	(22.093)	(21.253)	(28.414)
Observations	3,984,523	2,987,813	1,005,710	445,179
Post-Legislation Cohorts:				
Name foreignness	49.360	50.264	46.121	56.595
	(23.678)	(24.100)	(21.796)	(28.951)
Observations	1,244,184	972,524	271,660	177,486

Table 2 shows the statistics of second-generation immigrants' name foreignness. I find that for second-generation immigrants born in the pre-legislation period, the degree of name foreignness was generally higher in the target group; the difference in name foreignness was relatively smaller between the target group and the alternative control group that excludes individuals whose parents were from West Germanic language countries. The gap in name foreignness between the target and control group became smaller among second-generation immigrants born after the passage of immigration restriction laws.

In Table 3, I present descriptive statistics of the dataset of second-generation immigrants by origin in the pre-legislation and post-legislation period separately. In the pre-legislation period (the cohort of age 16 to the cohort of age 7), the average age in the target group was 11.6 years old, slightly younger than that in the control group. On average, parents in the control group (from less restricted countries) had settled down in the U.S. longer than parents in the target group (from more restricted countries). Upon childbearing, parents in

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Table 3: Descriptive Statistics: Basic Demographics

	Full sample	Target (More res.)	Control (Less res.)	Control (Alter.)
Pre-Legislation Cohorts:				
Age	11.705	11.603	12.005	11.626
	(2.846)	(2.848)	(2.819)	(2.893)
Years since mig., mother	22.021	21.219	24.154	20.496
	(7.642)	(7.238)	(8.256)	(9.871)
Years since mig., father	24.203	23.611	26.032	22.530
	(7.739)	(7.321)	(8.654)	(10.845)
Years since mig. when born, mother	11.012	10.414	12.548	11.416
	(6.942)	(6.479)	(7.803)	(8.123)
Years since mig. when born, father	13.036	12.600	14.359	13.392
	(7.298)	(6.873)	(8.317)	(9.170)
Urban	0.799	0.836	0.688	0.541
Male	0.497	0.497	0.496	0.495
Observations	3,984,523	2,987,813	1,005,710	445,179
Post-Legislation Cohorts:				
Age	2.162	2.166	2.147	2.033
	(1.400)	(1.400)	(1.402)	(1.427)
Years since mig., mother	16.296	16.753	15.081	13.549
	(7.865)	(7.548)	(8.534)	(7.827)
Years since mig., father	19.240	19.778	17.283	15.275
	(7.684)	(7.148)	(9.107)	(8.744)
Years since mig. when born, mother	14.195	14.654	12.983	11.822
	(7.745)	(7.445)	(8.364)	(7.628)
Years since mig. when born, father	17.126	17.664	15.179	13.499
	(7.620)	(7.119)	(8.944)	(8.567)
Urban	0.823	0.844	0.748	0.494
Male	0.494	0.495	0.494	0.494
Observations	1,244,184	972,524	271,660	177,486

the control group also had settled down in the U.S. longer. Second-generation immigrants in the target group were more likely to live in urban areas, and the sex ratio was similar in both the target and control group. In general, the differences in parents' characteristics appeared to be relatively smaller between the target group and the alternative control group. The exception is the rate of urban residence, where second-generation immigrants in this control group were significantly less likely to reside in urban areas. For example, the 1930 census shows that the majority of Mexican families lived in rural areas.

In the post-legislation period, I find the slight difference in the average age between two

groups (2.166 versus 2.147). In contrast to the pattern shown earlier, in the post-legislation period, parents of second-generation immigrants in the target group had settled down in the U.S. longer, both in 1930 and when having the child. This is not surprising, as immigration restriction laws severely limited new arrivals from more restricted countries, hence there should be disproportionately more "older immigrant parents" (who moved to the U.S. earlier) in the target group in the post-legislation period. Finally, second-generation immigrants in the target group were still more likely to live in urban areas, but the difference between two groups became much smaller. The sex ratio was similar in both groups.

I further present descriptive statistics of parents' socioeconomic status in Appendix A. The table shows the similar employment rate among fathers of both pre-legislation and post-legislation cohorts, but mothers of pre-legislation cohorts were more likely to be employed. In general, parents of pre-legislation cohorts were more likely to speak English, be citizens, and had higher occupational scores (although slightly less likely to be literates). This is mainly because that parents of pre-legislation cohorts were generally older and arrived in the U.S. earlier, and thus were economically and culturally more assimilated.

3.2 Empirical Strategies

I now discuss the empirical strategies of this paper. I start with the baseline estimation:

$$F_{ict} = \kappa_c + \lambda_t + \tau_{g(i)} + \mathbf{X}_{ict}\beta + \sum_{y=7}^{16} \pi_y D_c \mathbb{1}(t=y) + \sum_{y=0}^{4} \pi_y D_c \mathbb{1}(t=y) + \epsilon_{ict} \quad (2)$$

where i indexes the individual, c indexes the origin (parents' country of birth), and t indexes the cohort (the age group). F_{ict} is the degree of name foreignness of i's first name. κ_c, λ_t, and $\tau_{g(i)}$ are origin, cohort (age group), and gender fixed effects, respectively. $\mathbf{X}_{ict}$ is the vector of control variables. D_c is the indicator of the target group, i.e., whether the country of origin c was more restricted (Southern and Eastern Europe) under immigration restriction laws. Here, π_y for $y \in [7, 16]$ and π_y for $y \in [0, 4]$ are event-study estimates of

effects of immigration restriction laws on patterns of child naming, quantified by second-generation immigrants' name foreignness. In particular, π_y for $y \in [0, 4]$ reflect changes in child naming in the target group after the passage of immigration restriction laws. The standard errors are clustered at the origin (i.e., parents' country of origin) level.

I further consider changes to specification based on the above equation. First, I include state fixed effects in the specification. Immigrant families living in different areas might be exposed to different cultural environments and natives' attitudes towards immigration (Tabellini, 2018), and state fixed effects could capture such regional differences. Second, I run another regression based on parent fixed effects, which capture all time-invariant family-level observable and unobservable characteristics.

I also consider several types of changes to sample. First, I run regressions in the male and female sub-sample separately. Abramitzky et al. (2017) find little gender differences in name foreignness in the age of mass migration; on the other hand, in a study based on Hispanic families in California in the 1990s, Sue and Telles (2007) find significant gender differences in name-based assimilation. In this paper, I test whether immigration restriction laws generated similar effects on child naming for second-generation boys and girls. Second, I reconstruct the control group, and redo the analysis by including the alternative control group introduced in Section 3.1.

Note that results based on the above specifications might still not reflect the true impacts of immigration restriction laws on child naming. There are two other major channels through which immigration restriction laws might be correlated with child naming. First, parents' assimilation could contribute to trends in child naming, as parents who had children after the passage of immigration restriction laws might had already fully assimilated into the U.S. Second, immigration restriction laws limited new arrivals from more restricted countries (Ngai, 2004), and led to selection on both in-migration (Massey, 2016) and out-migration (Greenwood and Ward, 2015; Ward, 2017) particularly among more restricted groups. Hence, the effects of immigration restriction laws on child naming might not be

really through changes in parents' naming decisions, but only through changes in U.S. demographics (such as the number of new arrivals). In Section 4, I conduct several additional tests to study these channels following the main empirical analysis.

4 Results

In this section, I present empirical results. I start with the main empirical analysis. Subsequently, I conduct several additional tests and discuss the robustness of the main results. I conclude this section by analyzing other possible channels through which immigration restriction laws were correlated with trends in child naming, including parents' assimilation and selection on migration.

4.1 Main Results

I present the main analysis in Table 4, in which I estimate the effects of immigration restriction laws on child naming in both the full sample and sub-samples by gender.

In Column 1, I examine the baseline event-study specification by regressing second-generation immigrants' name foreignness on age fixed effects, origin (parents' country-of-birth) fixed effects, the interaction between age fixed effects and the indicator of more restricted groups (i.e., cohort-specific treatment dummies), and gender fixed effects. The first panel of Column 1 shows that almost all pre-legislation coefficients are insignificant. The only exception is the coefficient for the treatment dummy for the age 7 cohort, which is significantly positive at the 0.1 level. In contrast, the second panel shows evidence that immigration restriction laws led to the significant decline in second-generation immigrants' name foreignness, especially for cohorts who were born later.

In Column 2 I include birth order fixed effects and rerun the specification. The birth order was possibly associated with parents' assimilation, as immigrants should be culturally more assimilated when staying in the host country longer. The birth order might also be

Table 4: Immigration Restriction Laws and Child Naming

	Dependent variable: the degree of name foreignness among second-generation immigrants								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Age 16, restricted	1.254 (1.695)	1.252 (1.700)	1.218 (1.719)	1.214 (1.721)	1.214 (1.721)	1.520 (1.933)	1.011 (1.469)	1.434 (1.958)	1.008 (1.495)
Age 15, restricted	1.176 (1.634)	1.169 (1.641)	1.139 (1.652)	1.132 (1.655)	1.134 (1.657)	1.407 (1.839)	0.962 (1.433)	1.336 (1.861)	0.952 (1.458)
Age 14, restricted	1.238 (1.430)	1.232 (1.440)	1.205 (1.451)	1.198 (1.457)	1.201 (1.458)	1.337 (1.618)	1.152 (1.260)	1.260 (1.645)	1.148 (1.288)
Age 13, restricted	1.130 (1.406)	1.125 (1.417)	1.095 (1.424)	1.088 (1.431)	1.093 (1.433)	1.246 (1.546)	1.015 (1.274)	1.173 (1.573)	1.000 (1.298)
Age 12, restricted	1.174 (1.178)	1.168 (1.192)	1.144 (1.200)	1.137 (1.211)	1.141 (1.213)	1.133 (1.361)	1.186 (1.001)	1.065 (1.388)	1.186 (1.045)
Age 11, restricted	1.044 (1.036)	1.035 (1.049)	1.007 (1.048)	0.997 (1.058)	0.999 (1.060)	1.199 (1.155)	0.962 (0.929)	1.042 (1.174)	0.939 (0.955)
Age 10, restricted	1.027 (0.944)	1.122 (0.956)	1.101 (0.953)	1.096 (0.964)	1.098 (0.964)	1.044 (1.081)	1.196 (0.824)	1.008 (1.096)	1.178 (0.851)
Age 9, restricted	0.894 (0.637)	0.904 (0.653)	0.874 (0.644)	0.883 (0.658)	0.884 (0.660)	0.901 (0.762)	0.887* (0.528)	0.866 (0.787)	0.900* (0.547)
Age 8, restricted	0.605 (0.518)	0.617 (0.534)	0.581 (0.516)	0.592 (0.529)	0.595 (0.532)	0.599 (0.617)	0.612 (0.433)	0.561 (0.624)	0.692 (0.450)
Age 7, restricted	0.495* (0.292)	0.511* (0.303)	0.476 (0.292)	0.491 (0.302)	0.494 (0.304)	0.590* (0.327)	0.381 (0.277)	0.565* (0.330)	0.393 (0.294)
Age 4, restricted	−0.417 (0.411)	−0.430 (0.420)	−0.399 (0.398)	−0.411 (0.306)	−0.413 (0.408)	−0.702* (0.433)	−0.157 (0.417)	−0.675 (0.421)	−0.178 (0.421)
Age 3, restricted	−0.957* (0.551)	−0.980* (0.570)	−0.902* (0.542)	−0.924* (0.561)	−0.930* (0.565)	−1.119* (0.643)	−0.804 (0.509)	−1.041 (0.617)	−0.811 (0.533)
Age 2, restricted	−1.297* (0.753)	−1.326* (0.780)	−1.200 (0.741)	−1.227 (0.766)	−1.233 (0.773)	−1.522 (0.928)	−1.090* (0.623)	−1.415 (0.927)	−1.079* (0.658)
Age 1, restricted	−1.569* (0.894)	−1.605* (0.925)	−1.451* (0.887)	−1.484 (0.917)	−1.491 (0.923)	−1.813* (1.041)	−1.347* (0.792)	−1.672 (1.046)	−1.331 (0.841)
Age 0, restricted	−2.105** (1.012)	−2.152** (1.055)	−1.932* (1.006)	−1.976* (1.048)	−1.985* (1.057)	−2.546** (1.177)	−1.699** (0.864)	−2.317* (1.195)	−1.683* (0.930)
Sample	Full	Full	Full	Full	Full	Male	Female	Male	Female
Birth order	No	Yes	No	Yes	Yes	No	No	Yes	Yes
State FE	No	No	Yes	Yes	Yes	No	No	Yes	Yes
Controls	No	No	No	No	Yes	No	No	Yes	Yes
Adj. R^2	0.100	0.100	0.106	0.106	0.106	0.089	0.073	0.097	0.078
Obs.	5,741,381	5,741,381	5,741,381	5,741,381	5,741,381	2,892,574	2,848,807	2,892,574	2,848,807

Standard errors are in parentheses. Origin, age, and gender fixed effects are included. *: $p < .1$; **: $p < .05$; ***: $p < .01$.

associated with parents' attitudes towards naming, if they had different naming strategies for children in terms of name assimilation. However, results almost remain unchanged, suggesting that including birth order fixed effects does not change the conclusion. In Column 3 I control for state fixed effects. In Column 4 I control for both birth order and state fixed effects. In Column 5, I further add other control variables and rerun the event-study specification. With the inclusion of controls, a few post-legislation coefficients become marginally significant, but changes in effect sizes, compared to Column 1, are fairly small;
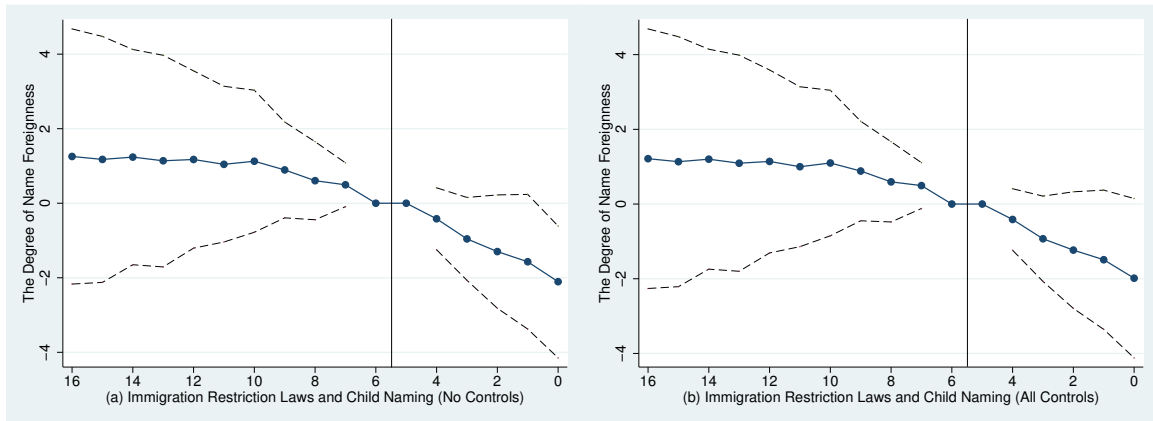
Figure 1: The Effects of Immigration Restriction Laws on Child Naming

on the other hand, all pre-legislation coefficients are insignificant. Column 5 first shows insignificant pre-trends, suggesting that all immigrant groups had relatively similar patterns of child naming in the pre-legislation period, and trends in child naming should be parallel in the absence of immigration restriction laws. On the other hand, post-legislation coefficients suggest that immigration restriction laws had significantly negative effects on the degree of name foreignness among second-generation immigrants whose parents were from more restricted countries. From Column 5 to 9, I rerun the regression in Column 1 and 5 based on the male and female subsample. I observe some evidence that the effects of immigration restriction laws on child naming were stronger among second-generation boys. Nevertheless, the qualitative pattern of Column 7 and 9 show that immigration restriction laws also affected child naming among girls, although with smaller effect sizes.

Figure 1 presents graphic results of event-study estimates presented in Column 1 and 5 of Table 4. The x-axis is age in 1930 (i.e., cohort) and the y-axis is the degree of name foreignness among second-generation immigrants. These graphic event-study results appear to be similar regardless of the inclusion of control variables in the event-study specification. Figure 1 illustrates the insignificant pre-trends, and the effects of immigration restriction laws on second-generation immigrants' child naming. Note that while it is clear that immigration restriction laws generated negative effects on name foreignness for cohorts born after the passage of immigration laws, it is useful to further discuss pre-trends in the early

1920s: although pre-trends appear to be almost horizontal among earlier cohorts (age 16 to age 9), the coefficients become smaller for cohorts born after 1921 (age 8 and 7 cohorts), although they are positive and insignificant. It is worth noting that naming decisions between 1921 and 1924 might be affected by the passage of the 1921 immigration restriction law (i.e., Emergency Quota Act). The 1921 immigration restriction law was similarly a quota-based restriction policy, and quotas were determined based on immigrant populations in the 1910 census (Goldin, 1994; Ngai, 1999). In particular, the 1921 law could reflect anti-immigration attitudes among natives, and have some restrictions on the number of new arrivals from more restricted European countries. However, it did not generate sufficiently large influences on immigration even if it was the prelude to the 1924 immigration restriction law.[1] This explains (a) moderately downward trends in pre-legislation coefficients, and (b) results of pre-trends that still appear to be insignificant overall.

4.2   Additional Tests

Before discussing other possible mechanisms behind the above findings in Section 4.3, I first conduct several additional tests to check the robustness of the main results.

Table 5 presents four regressions that study changes to sample and specification. In Column 1, I reconstruct the control group and redo the empirical analysis. In Section 3, I discuss that (a) many second-generation immigrants were of English-speaking origin, and (b) immigrant families from distant cultures might assimilate faster in terms of child naming, and some countries of origin involved in the control group (e.g., Austria) clearly had closer cultures with the U.S. than countries of origin involved in the target group (Southern and Eastern Europe). I thus reconstruct the control group by including only second-generation immigrants whose families were originally from culturally distant and less restricted countries (defined by language), such as Mexico and France. Results show very similar effects of immigration restriction laws on child naming, suggesting that the construction of the

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[1]This was also a reason behind the passage of the 1924 immigration law (Higham, 1963; Ngai, 1999).

Table 5: Changes to Sample and Specification

Specifications	(1) Alternative control group	(2) Parent FE	(3) Ethnic name index (ENI)	(4) ENI and Parent FE
Age 16, restricted	1.189 (2.688)	0.118 (3.268)	1.351 (4.634)	0.260 (5.410)
Age 15, restricted	1.134 (2.525)	0.639 (3.026)	1.307 (4.441)	0.675 (5.035)
Age 14, restricted	1.220 (2.233)	0.451 (2.577)	1.407 (3.965)	0.769 (4.469)
Age 13, restricted	1.263 (2.033)	0.500 (2.284)	1.718 (3.674)	0.739 (4.089)
Age 12, restricted	1.172 (1.701)	0.561 (1.848)	1.936 (3.105)	1.168 (3.237)
Age 11, restricted	1.181 (1.406)	1.132 (1.480)	1.968 (2.691)	1.636 (2.704)
Age 10, restricted	1.216 (1.121)	0.838 (1.172)	2.115 (2.222)	1.817 (2.324)
Age 9, restricted	1.122 (0.691)	0.966 (0.743)	1.912 (1.459)	1.780 (1.522)
Age 8, restricted	1.057 (0.661)	0.612 (0.672)	1.612 (1.259)	1.183 (1.230)
Age 7, restricted	0.670** (0.251)	0.355* (0.188)	1.114* (0.562)	0.697 (0.492)
Age 4, restricted	−0.743** (0.360)	−0.674** (0.270)	−1.359** (0.652)	−1.326** (0.417)
Age 3, restricted	−1.162** (0.528)	−0.810** (0.242)	−1.807** (0.890)	−1.497** (0.649)
Age 2, restricted	−1.413* (0.750)	−0.922 (0.606)	−2.790* (1.406)	−2.164 (1.387)
Age 1, restricted	−1.642** (0.829)	−0.919** (0.309)	−3.389** (1.543)	−2.466** (1.190)
Age 0, restricted	−1.972* (1.011)	−1.128** (0.458)	−3.720* (1.879)	−2.889** (1.353)
Adj. R^2	0.119	0.208	0.100	0.220
Obs.	3,444,926	3,444,926	3,444,926	3,444,926

Standard errors are in parentheses. *: $p < .1$; **: $p < .05$; ***: $p < .01$.

control group should not drive the main findings.

In Column 2 I run the specification with parent fixed effects. Parent fixed effects capture time-invariant family-level characteristics. After controlling for parent fixed effects, I find smaller magnitudes of effects of immigration restriction laws on child naming. However, the qualitative pattern appears to be similar: pre-trends are insignificant, and post-legislation coefficients indicate the relative decline in name foreignness among second-generation immigrants whose families were originally from more restricted countries.

In Column 3 I use the ethnic name index (ENI) used by Goldstein and Stecklov (2014) as the dependent variable and rerun the specification. As this is a different measure, I do

find changes in effect sizes, but the empirical conclusion still holds: after the passage of immigration restrictions, immigrant parents from more restricted countries tended to give less foreign names to their newborns. I repeat the exercise in the model with parent fixed effects in Column 4, and find similar results.

Table 6: Placebo Tests: Different Legislation Years

	(1)	(2)	(3)	(4)
Placebo	Age 9 & 10	Age 11 & 12	Age 13 & 14	Age 15 & 16
Year	1920	1918	1916	1914
Age 16,	0.399	0.195	0.050	—
restricted	(0.998)	(0.627)	(0.288)	
Age 15,	0.364	0.160	0.015	—
restricted	(0.929)	(0.562)	(0.220)	
Age 14,	0.409	0.204	—	0.027
restricted	(0.744)	(0.376)		(0.236)
Age 13,	0.288	0.084	—	-0.093
restricted	(0.740)	(0.332)		(0.272)
Age 12,	0.289	—	-0.060	-0.093
restricted	(0.463)		(0.267)	(0.509)
Age 11,	0.109	—	-0.240	-0.273
restricted	(0.287)		(0.453)	(0.691)
Age 10,	—	-0.073	-0.218	-0.250
restricted		(0.212)	(0.559)	(0.798)
Age 9,	—	-0.343	-0.488	-0.520
restricted		(0.559)	(0.900)	(1.141)
Age 8,	-0.377	-0.582	-0.726	-0.759
restricted	(0.281)	(0.645)	(0.984)	(1.222)
Age 7,	-0.654	-0.859	-1.004	-1.036
restricted	(0.557)	(0.921)	(1.253)	(1.490)
Adjusted R^2	0.142	0.142	0.142	0.142

Standard errors are in parentheses. *: $p < .1$; **: $p < .05$; ***: $p < .01$.
Specifications are the same to that in Column 5, Table 4. Observations: 5,234,817.

In Table 6, I conduct four placebo tests by assuming that immigration restriction laws were passed in different years prior to actual years of the passage in history. I focus specifically on the pre-legislation period and study placebo effects on pre-trends, supposing that the laws were enacted in 1920, 1918, 1916, and 1914, respectively. I find no significant coefficients in the (fake) post-legislation period in all four regressions. These placebo tests show that trends in child naming observed in this paper should indeed be related to the passage of immigration restriction laws.

4.3 Other Channels

I conclude the empirical section by discussing two other possible channels through which immigration restriction laws could be related to child naming. First, the findings of this paper might be driven by parents' assimilation. Suppose, for example, an immigrant family arrived in the U.S. before the passage of immigration restriction laws, but the child was born after the laws were passed. In such cases, name Americanization might simply be because of parents' assimilation. To study this, I focus on sub-samples in which individuals' parents had been in the U.S. sufficiently long when they were born.

Table 7: Other Possible Channels

	(1)	(2)	(3)	(4)	(5)	(6)
Channels	(1) - (4): Parents' assimilation, childbearing after				(5), (6): Selective mig.	
Specification	> 10 yrs. since mig.		> 15 yrs. since mig.		State FE	Parent FE
Age 16,	0.540	0.190	1.255	0.641	1.480	0.428
restricted	(2.023)	(3.129)	(1.880)	(2.688)	(3.186)	(3.346)
Age 15,	0.454	0.986	0.879	1.378	1.508	0.861
restricted	(2.038)	(3.211)	(1.955)	(3.055)	(2.604)	(2.814)
Age 14,	0.480	0.342	0.964	0.503	1.183	0.662
restricted	(1.665)	(2.484)	(1.509)	(2.252)	(2.355)	(2.386)
Age 13,	0.702	0.592	1.127	0.867	1.206	0.621
restricted	(1.595)	(2.331)	(1.615)	(2.324)	(2.236)	(2.313)
Age 12,	0.514	0.533	0.406	0.447	1.428	0.807
restricted	(1.261)	(1.629)	(1.129)	(1.299)	(1.706)	(1.620)
Age 11,	0.696	1.319	0.654	1.496	1.491	1.331
restricted	(1.194)	(2.031)	(1.120)	(1.807)	(1.318)	(1.327)
Age 10,	0.922	0.793	0.943	0.791	0.930	0.569
restricted	(0.877)	(1.180)	(0.752)	(0.634)	(1.036)	(1.050)
Age 9,	0.955*	0.856	0.943**	0.911**	0.791	0.776
restricted	(0.508)	(0.773)	(0.360)	(0.378)	(0.643)	(0.616)
Age 8,	0.809	0.523	0.545	0.662	0.670	0.432
restricted	(0.647)	(0.663)	(0.534)	(0.413)	(0.529)	(0.512)
Age 7,	0.537**	0.202	0.346	0.301	0.304	0.108
restricted	(0.227)	(0.206)	(0.209)	(0.629)	(0.206)	(0.123)
Age 4,	−0.860**	−0.685**	−0.721**	−0.574	−1.108**	−0.723**
restricted	(0.224)	(0.339)	(0.275)	(0.574)	(0.472)	(0.358)
Age 3,	−1.111**	−0.860**	−0.777**	−0.794**	−1.540	−0.860**
restricted	(0.464)	(0.271)	(0.350)	(0.299)	(0.610)	(0.331)
Age 2,	−1.410**	−0.906	−0.554	−0.653	−1.794**	−0.972
restricted	(0.701)	(0.762)	(0.383)	(0.869)	(0.858)	(0.667)
Age 1,	−1.864**	−1.141**	−1.450**	−1.207	−2.030**	−0.965**
restricted	(0.707)	(0.370)	(0.369)	(0.790)	(0.829)	(0.384)
Age 0,	−1.714**	−1.228**	−1.197**	−0.933*	−2.388**	−1.180**
restricted	(0.840)	(0.439)	(0.567)	(0.565)	(1.036)	(0.529)
State FE	Yes	No	Yes	No	Yes	No
Parent FE	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.145	0.238	0.134	0.225	0.123	0.213
Observations	2,077,968	2,077,968	1,275,438	1,275,438	1,751,594	1,751,594

Standard errors are in parentheses. *: $p < .1$; **: $p < .05$; ***: $p < .01$.

I report the results from Column 1 to 4, Table 7. In Column 1, I study the sub-sample in which second-generation immigrants were born when their parents had arrived in the U.S. over 10 years. I run a regression similar to that in Column 5, Table 4, i.e., I include all control variables and state fixed effects. I find quantitative similar results compared to the main findings reported in Table 4. In Column 2 I include parent fixed effects, and the estimates are similar to those reported in Table 5 based on the full sample. In Column 3 and 4 I further restrict the sample to individuals who were born when their parents had arrived in the U.S. over 15 years. The coefficients do become smaller, which is not surprising as immigrant parents who stayed in the U.S. longer indeed gave more Americanized names to children (Abramitzky et al., 2017), but the empirical conclusion is still similar to the main findings: the passage of immigration restriction laws led to the decline in name foreignness among second-generation immigrants whose parents were from more restricted countries, even after considering the potential impacts of parents' assimilation on child naming.

In Column 5 and 6 I study another possible channel, i.e., selection on migration. Immigration restriction laws affected U.S. demographics in the sense that immigration became highly limited, and both in-migration and out-migration were selected (Haines, 2000; Greenwood and Ward, 2015; Massey, 2016). Note that immigrants with children were substantially more likely to stay in the U.S. (Ward, 2017), but selection on migration caused by immigration restriction laws might still affect trends in child naming. For example, suppose that after the passage of immigration restriction laws, there were disproportionately fewer new immigrant parents from more restricted countries, then the main findings of this paper might be simply due to changes in the ratio of newly arrived parents between the target and control group. It is thus helpful to focus on immigrant parents who had children both before and after the passage of immigration restriction laws. Based on this idea, I rerun the specifications in a sub-sample of second-generation immigrants whose parents had already had children born in the U.S. prior to the passage of immigration restriction laws.

Column 5 and 6 present the results. In Column 5, I control for state fixed effects, and

in Column 6 I control for parent fixed effects. In both regressions, I find that the empirical conclusion is similar to the main findings: pre-trends are generally insignificant, and post-legislation coefficients suggest that immigration restriction laws led to the relative decline in name foreignness among second-generation immigrants whose parents were from more restricted countries. The effect sizes are similar to those reported in Table 4 and 5. These tests suggest that selection on migration should not be the major channel through which immigration restriction laws influenced trends in child naming among immigrant families.

5   Conclusion

Researchers have long discussed that names are signals of cultural identity (e.g., Chibnik, 1991; Gugliemino et al., 2000), and patterns of child naming among immigrant families can reflect cultural assimilation (e.g., Gordon, 1964). Abramitzky et al. (2017) show that a significant proportion of immigrant parents gave Americanized names to their children in the early twentieth century U.S., and immigrant families eventually closed half of the gap in child naming with natives. The declining trends in name foreignness among second-generation immigrants could be explained by both assimilation (Gerhard and Hans, 2009; Goldstein and Steklov, 2014) and avoidance of discrimination (Bertrand and Mullainathan, 2004; Oreopoulous, 2011).

In this paper, I estimate the effects of immigration restriction laws on second-generation immigrants' name foreignness in the early twentieth century U.S. Immigration restriction laws were passed in the 1920s in the background of anti-immigration populism, and immigrant parents might have behavioral responses to immigration restriction laws by adjusting patterns of child naming. In the empirical analysis, I compare cohorts of second-generation immigrants born before and after the passage of immigration restriction laws, between the target group (in which immigrant families were from more restricted countries) and the control group (in which immigrant families were from less restricted countries).

The event-study estimates show that pre-trends are indistinguishable from zero, suggesting that trends in child naming across immigrant groups should be parallel in the absence of immigration restriction laws. On the other hand, among post-legislation cohorts, I observe the particular decline in name foreignness among second-generation immigrants in the target group. This suggests that immigration restriction laws did affect naming decisions among immigrant parents who came from countries that were severely restricted under immigration laws in the 1920s. The main findings of this paper are robust to changes to sample and specification. Furthermore, I find empirical evidence that other channels, including parents' assimilation and selection migration, were unlikely to be the main mechanisms behind trends in child naming.

This paper adds to the literature of the demographic and economic history of U.S. immigration. Researchers find significant effects of immigration restriction laws on the U.S. economy (Sequiera et al., 2017) and demographics (Ngai, 2004). In particular, immigration restriction laws limited new arrivals by country of origin, and led to selection on both in-migration (Massey, 2016) and out-migration (Greenwood and Ward, 2015; Ward, 2017). This paper further shows that immigration restriction laws influenced the U.S. society and culture by affecting naming outcomes among second-generation immigrants, even if they were native-born and, in theory, were not directly targeted by immigration restriction laws. Based on the findings of this paper, one can further explore changes in cultural diversity in the U.S. society after the passage of immigration restriction laws. Another possible avenue for future research is to analyze various types of social and economic consequences of second-generation immigrants' name Americanization, such as earnings, ethnic enclave residence, intermarriage, and internal migration.

Appendix A: Descriptive Statistics, Continued

I briefly report descriptive statistics of parents' socioeconomic status in this appendix.

Table 8: Descriptive Statistics: Parents' Socioeconomic Status

	Full sample	Target (More res.)	Control (Less res.)	Control (Alter.)
Pre-Legislation Cohorts:				
Employed, mother	0.129	0.123	0.152	0.158
Employed, father	0.843	0.840	0.855	0.888
Speak English, mother	0.819	0.820	0.815	0.658
Speak English, father	0.918	0.925	0.889	0.788
Literate, mother	0.761	0.729	0.891	0.799
Literate, father	0.832	0.811	0.915	0.853
Citizenship, mother	0.499	0.481	0.571	0.452
Citizenship, father	0.590	0.589	0.610	0.478
Occupational score, mother (non-zero)	18.972 (9.734)	20.076 (9.572)	14.951 (9.246)	13.264 (8.973)
Occupational score, father (non-zero)	24.125 (8.391)	24.450 (8.192)	22.826 (9.025)	20.210 (8.602)
Observations	3,984,523	2,987,813	1,005,710	445,179
Post-Legislation Cohorts:				
Employed, mother	0.059	0.054	0.071	0.072
Employed, father	0.841	0.836	0.852	0.890
Speak English, mother	0.776	0.693	0.811	0.444
Speak English, father	0.893	0.805	0.930	0.633
Literate, mother	0.796	0.777	0.839	0.705
Literate, father	0.853	0.840	0.884	0.789
Citizenship, mother	0.337	0.376	0.250	0.147
Citizenship, father	0.479	0.548	0.324	0.180
Occupational score, mother (non-zero)	18.688 (9.659)	20.693 (9.341)	14.735 (9.037)	13.126 (8.947)
Occupational score, father (non-zero)	23.821 (8.399)	24.582 (8.164)	22.030 (8.667)	19.094 (7.976)
Observations	1,244,184	972,524	271,660	177,486

The above table shows similar employment rates among parents in both periods, but mothers of pre-legislation cohorts were more likely to be employed. Overall, parents of pre-legislation cohorts were more likely to speak English, be U.S. citizens, and had higher occupational scores, as they generally lived in the U.S. longer (see Table 3) and thus were economically and culturally more assimilated, although they were also more likely to be illiterates as they were relatively older. Note that different from the descriptive statistics in Table 3, here it is clear that there were significant differences in parents' socioeconomic

status between the target and control group (including the alternative control group that excludes immigrant families of West Germanic origins), among both pre-legislation and post-legislation cohorts.

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