Immigration displaces women

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Abstract: I use geographic variation in immigration over time to establish a previously undocumented stylized fact: foreign immigration to the US reduces the employment rates of native female workers. This effect persists across skill groups, has become less pronounced over time, and is robust to the specification used to estimate it, the definition of the geographical area, and the potential for geographic self-selection among immigrants. It also contrasts sharply with typical findings from studies that focus on native men, as well as my own estimates for men. The pattern of declining female employment effects is consistent with well-documented declines in female labor-supply elasticities, and I find that the female employment effect is driven primarily by married women and those with children, among whom labor supply is known to be relatively elastic. While I find that immigration does not impact the average wages of either low- or high-skilled native women, there is a pronounced negative wage effect for highly skilled native women who are married or have children, with smaller positive effects for other groups. I argue that the female-male difference in native employment effects cannot be explained by gender differences in native skill distributions. As further evidence of this, I show that the female employment effect is driven primarily by competition from female immigrants.

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1 Introduction

In 1960, 44% of working-age women in the US were gainfully employed. That number rose to 72% in 2000, before contracting slightly to 69% in 2010. Over this same period, the fraction of the working-age population comprised of immigrants born outside of the US rose continuously, from a low of 6% in 1960 to 18% by 2010. Without exaggeration, these forces—the revolutionary rise of women in the workplace and the resurgence of the immigrant worker—have reshaped the economy, indeed society, of the US. Given their mutual significance, it would be surprising to learn that these simultaneous economic sea changes unfolded independently, neither affecting the course of the other. This paper contains no surprises.

Using geographic variation over time in employment-population ratios and immigrant population shares calculated from samples of the US Census and American Community Survey spanning 1960-2010, I estimate the effects of immigration on native employment. Consistent with previous studies (see, e.g., Card, 1990; Altonji and Card, 1991; Card, 2001), I find no evidence of large employment effects for native males. I do find appreciable disemployment effects for native women, both absolutely and relative to those for men. My baseline estimates imply that a ten percentage-point increase in the immigrant share of the population decreases employment for women by between 1.3 and 6.6% more than for men (or between 1.8 and 2.5% absolutely), depending on the decade and skill group. At interstate standard deviations of immigrant shares, these imply employment declines in excess of those for men by between 1.4 and 2%.

The estimated female-male differences in employment effects cross skill groups defined by educational attainment. They also decline systematically over time, although they remain negative and statistically significant at the end of the sample period. These findings are robust to a number of alternative, non-causal interpretations. In particular, I use several variations on the standard immigrant-enclaves instrumental variables approach (Altonji and Card, 1991; Card, 2001) to address the possibility that immigrants select into locations within the US on the basis of the economic conditions prevailing in those locations. I also assess the sensitivity of my findings to the specification used to estimate them and the level of geography over which they are estimated.

I then examine potential explanations for the estimated gender differences in employment effects. The declines over time in estimated employment effects for women are similar to the declining labor-supply elasticities for married women estimated by Blau and Kahn (2007), suggesting that gender differences in elasticities may help explain the differential employment effects. Consistent with this interpretation, I find that the negative female employment effects are driven entirely by married women (with similar results for those with children), among whom labor supply is relatively elastic. For single women, I find smaller positive effects.

While a labor-supply elasticity explanation for gender differences in employment effects requires that immigration decreases natives' wages, I find no evidence of large average wage effects for men or women of any skill group, another null finding consistent with previous research (see, e.g. Card, 1990; Altonji and Card, 1991; Card, 2001; Clemens and Hunt, 2019). Disaggregating by marital status, however, I find substantial negative wage effects for married women and smaller positive effects for some single women. This pattern suggests that some married women exit the labor force in response to competition from immigrants, offsetting the effects of immigration for, and encouraging labor-force participation among, single women.

I continue to investigate the sources of gender-differences in the effects of immigration on wages and, by implication, employment. My analysis is limited to the role of gender differences in educational attainment and occupation and industry of employment, two factors identified by Blau and Kahn (2017) as having substantial power to explain the gender gap in wages. I show that national trends in female-male differences in these variables are inconsistent with declining gender differences in the effects of immigration. I support this conclusion with evidence from wage regressions that control for these factors.

Finally, I estimate models of the gender-specific effects of gender-specific immigration on native employment. The estimates suggest that native employment is decreasing in own-gender immigration and increasing in cross-gender immigration, and that that the magnitudes of these effects have also declined over time. This implies that gender differences in the effects of immigration arise because of imperfect, though increasing, substitutability of female for male labor. However, as my prior results show, this apparent effective imperfect substitution cannot be a consequence of gender differences in education or industrial and occupational choices.

My findings add to a small, but growing, literature on gender dimensions of the labor-market effects of foreign immigration. Much of this literature centers on the relationship between immigration, household services, and female work outcomes. In the most widely cited paper, Cortés and Tessada (2011) find that women in the right tail of the wage distribution work longer hours in response to inflows of less-skilled foreign immigrants. They attribute this phenomenon to immigrant-induced decreases in the prices of household services such as childcare and housecleaning, a sector that disproportionately employs immigrants. Supporting this conclusion, they provide evidence that women spend less time on household tasks, and more money on household services, when immigrants comprise a greater share of the less-skilled labor force.

Several other studies have found evidence of this phenomenon outside of the US, including Farr et al. (2011) for Spain, Barone and Mocetti (2011) for Italy, and Forlani et al. (2015), who analyze international data. Furtado (2016) provides evidence that less-skilled immigration increases fertility among married, educated women, attributing this response to immigration-induced decreases in the prices of household services, which reduce the tradeoff between parenthood and working. This evidence that immigration increases female employment along its intensive margin is not at odds with my finding that immigration has effects in the opposite direction along the extensive margin. In fact, Cortés and Tessada (2011) also estimate negative, although statistically insignificant, extensive-margin effects, and Furtado

(2015) finds significant effects.[1]

This literature is closely related to broader literatures on female labor supply (Killingsworth and Heckman, 1986; Blundell and Macurdy, 1999; Goldin, 2006; Blau and Kahn, 2007, 2013, e.g.) and gender inequality in the labor market (see Goldin, 2014; Blau and Kahn, 2017, for excellent reviews of recent trends and evidence). Edo and Toubal (2017), analyzing French data, also estimate imperfect substitution between men and women conditional on education. Using structural simulations similar to those in Borjas (2003) and Ottaviano and Peri (2011), they find that imperfect substitution coupled with increases in the female share of the immigrant labor force have increased the gender wage gap in France. While my findings do not suggest that immigration has exacerbated aggregate gender wage inequality in the US, they do imply that it has slowed gender convergence in employment, altering the female wage structure along the way. Gender differences in the effects of immigration on natives may be both cause and consequence of gender inequality if effective imperfect substitution between men and women arises in part from labor-market discrimination against women, although I emphasize that I have no evidence either for or against this possibility.

I detail the data used in this study and provide motivating summary statistics in Section 2. I present estimates of the effects of immigration on employment rates for native men and women in Section 3. I investigate the mechanisms behind gender differences in the effects of immigration in Section 4. I conclude in Section 5.

2 Data and summary statistics

The data for this study are drawn from Integrated Public Use Microdata Series extracts of the 1960–2000 U.S. Decennial Censuses and a pooled extract of the 2009-2011 American Community Survey, which I refer to as the 2010 sample (Ruggles et al., 2010). From these extracts, I retain only individuals between the ages of 16 and 65. The key variables used in

[1] One potential reason why Cortés and Tessada (2011) find less robust evidence of negative effects along the extensive margin is that their specifications are intended to identify the effects of changes in price indices for household services, rather than immigration per se.

this analysis are employment, which I define as earning nonzero wage and salary income in the year preceding enumeration, and immigrant status, which I define according to whether one was born in the United States. As part of the standard immigrant-enclave instrumental-variables strategy that I discuss in further detail below, I supplement these data with extracts of the 1940 and 1950 Censuses, from which I only retain information on the places of birth and current residence. Before using them in the analysis, I perform some minimal preprocessing of the data, which I detail in Appendix A.

To motivate the empirical analysis, and give a sense of the magnitudes of and trends in the key variables, Figure 1 plots the national employment-population ratios for native men and women alongside the immigrant fraction of the population over the period spanning 1960-2010. As Clemens and Hunt (2019) note, an emerging consensus finds that foreign immigration has relatively small impacts on the labor-market outcomes of low-skill natives and little to no impact for high-skill natives. For this reason, it is common in the immigration literature to stratify analyses by skill group. Accordingly, I present separate trends for those with at most a high-school diploma and those with more education, although I acknowledge that this coarse classification takes a narrow view of the definition of skill.[2]

As the figure shows, there are clear breaks in the employment trends of men and women alike in 1970 and 2000. For men, the 1970 break marks the beginning of a long decline in employment rates, which begins to accelerate in 2000. For women, the 1970 break represents the beginning of a period of slower growth in employment, which turns to a period of contraction in 2000. These breaks, which occur across gender and skill groups, are not accompanied by analogous breaks in the immigrant population share. Between 1970 and 2000, the employment trend for low- and high-skill native women exhibits a concavity not shared by the trend for native men, which is decreasing over this period. The concave female employment trend is mirrored by a convex immigrant share trend. This dual relationship

[2] Card (2009), working with 2000 data, provides evidence that workers within these broad categories are perfect substitutes, and suggests that because immigrants are natives are similarly skilled by this definition, immigration has not had much of an impact on wage inequality in the US.

between the rates of change in female employment rates and immigrant shares suggests that foreign immigration to the U.S. may have dampened employment growth for native women in the second-half of the 20th century and the beginning of the 21st. Much of the remainder of this paper examines whether this descriptive relationship appears causal upon further scrutiny.

Table 1 provides formal summary statistics for the variables summarized in Figure 1. Because most of the estimates presented below relate state-level average immigration and native labor-market outcomes, the table presents state-level employment rates, by gender and skill group, immigrant shares, and their standard deviations, by decade. These unweighted, across-state averages of state-level means differ slightly from the national trends plotted above, though they paint a similar picture. One conclusion from the table is that there is considerable variation in the immigrant share of the population, even at the state level, within and across decades. Figure 2 contains cloropleths of the immigrant share of the population over time, supporting this conclusion and showing that variation in immigration is driven by more than a handful of high-immigration states.

3 Employment effects

I use variation in the immigrant share of the population over time and across states to identify the effect of immigration on native female employment as well as the female-male difference in employment. Specifically, I estimate a series of variations on the model

$$y_{gkst} = \beta_{kst} p_{st} + \delta_{kst} \text{Female}_g \cdot p_{st} + \lambda_{gks} + \mu_{gkt} + \varepsilon_{gkst}, \tag{1}$$

where y_{gkst} represents the employment-population ratio for those of gender $g \in \{\text{Male}, \text{Female}\}$ and skill group $k \in \{\text{Low}, \text{High}\}$ in state s during decade $t \in \{1960, \ldots, 2010\}$, p_{st} is the foreign-immigrant share of the population in state s during decade t, Female_g is an indicator for whether group g consists of women, and λ_{gks} and μ_{gkt} are gender- and skill-group-specific state and decade effects. This model allows for the possibility that the effect of immigration on employment varies by year as well as gender and skill. The immigrant share p_{st} is not indexed by either gender or skill. Consequently, β_{kst} and $\beta_{kst} + \delta_{kst}$ identify the effects of immigration from *all* skill groups on male and female employment, as opposed to the effects of immigrants belonging to a particular skill group on the employment of natives in that skill group.[3]

Figure 3 provides a visual summary of this paper's central empirical finding. The figure plots least-squares estimates of the female-male difference δ_{kst} in native employment effects of foreign immigration between 1960 and 2010. The estimates plotted in the figure, as well as all of the remaining estimates in this paper, are weighted by the number of observations used compute the dependent variable. The plotted bars represent 95% confidence intervals obtained using standard error estimates that are clustered at the state level, as are the remaining standard error estimates presented in this paper (except for those that use alternative geographical units).

The clear implication of the estimates in Figure 3 is that, however immigration impacts the employment of native males, it does so more negatively for native females. For less-skilled natives, the female-male difference in employment effects is negative and statistically significant in all but one decade. The point estimates, which are presented in Table 2 and discussed below, range from about -.66 in 1970 to -.18 in 2010. Evaluated at the interstate standard deviations of immigrant shares in Table 1, these translate to disemployment effects for females that exceed those for males by between 2 and 1.4%. While most studies find that the effects of immigration are concentrated on less-skilled natives, the estimated gender difference in employment effects persists across broad skill groups. Though smaller than for the low-skilled, the estimated gender-differences for the highly skilled remain large (they are also nearly all statistically significant at the 10% level).

[3]The latter effect is what Ottaviano and Peri (2011) term the direct partial effect of immigration. They refer to the former as the total effect, which represents the sum of the direct partial effect and indirect partial effects due to immigration from all other skill groups.

A second implication of Figure 3 is that, after a period of stability during the 1960s and 70s, the female-male difference in employment effects declines monotonically from 1980 onward. Like the gender differences themselves, these declines appear for both skill groups. Despite this pattern of declining gender differentials, the estimated effects of immigration on the employment of native women remain larger than those for men, and the difference between them statistically significant, for members of each skill group, even in the latest period in the sample.

A natural question is whether the estimated gender differences in employment effects summarized in Figure 3 translate to economically meaningful absolute employment effects for women. Figure 4 suggests that they do. The points plotted in that figure represent least-squares estimates of the gender-specific overall effects of immigration on employment (that is, replacing the β_{kst} in (1) with gender-specific coefficients β_{gkst} and dropping the $\text{Female}_g \cdot p_{st}$ term). The estimated effects for men are statistically insignificant for each skill group and in every period, except for low-skilled men in 1960. From 1980 on, the point estimates are also numerically close to zero.

For women, the story is much different. With the exception of low-skilled women in 1960, the female point estimates are negative for both skill groups in each period. The point estimates are statistically significant for high-skilled women beginning in 1980 and for low-skilled women beginning in 1990. In the interest of completeness, I present the full set of corresponding point estimates in Appendix Table 14 (which also contains instrumental variables estimates based on the standard immigrant-enclave strategy, which I discuss at length below). As that table shows, the negative point estimates range from about -.3 for low-skilled women in 1980 to about -.18 for high-skilled women in 1990. The estimated overall female employment effects also decline in absolute value over time, although the decline is less severe than for the estimated gender differences in employment effects, which are sensitive to the large positive male employment effects in 1960 and 1970.

In Table 2, I assess the sensitivity of the results presented so far to the specification used to estimate them. The first two columns of Table 2 reproduce the point estimates plotted in Figure 3. One caveat to the interpretation of the estimated gender differences in the effects of immigration on employment is that men and women might participate in the labor market at different points in their lives, which may help explain why female employment appears to be more sensitive to competition from foreign immigrants, although this would not challenge the conclusion that immigration impacts women differently than for men. To rule out this interpretation, I estimate models that replace employment rates with the state fixed effects from gender-, skill- and decade-specific linear probability models that relate individual employment to a full set of age indicators and state fixed effects. As the estimates in panel (2) of Table 2 show, this change has little effect on the estimated gender differences in employment effects; if anything, it increases their statistical significance.

A more substantive challenge to the causal interpretation of the estimated employment effects is that the geographic distribution of immigrants throughout the U.S. is not random. Instead, immigrants may self-select into the areas where they live and work at least in part with respect to the economic conditions prevailing in those areas. The typical concern is that immigrants sort into areas experiencing relative booms, attenuating estimates of the impacts of immigration on outcomes such as wages or employment. Since one of my primary estimands is the gender difference in immigration effects, this type of endogeneity may be less of a concern here, at least to the extent that local economic conditions affect male and female employment similarly. However, even if the use of female-male comparisons mitigates concerns about self-selection among migrants, those concerns do remain, especially given evidence that female labor supply tends to be more responsive to wages than male supply (Blau and Kahn, 2007).

As a first step towards addressing the possibility of bias due to selective migration, I also estimate models that control for a vector of time-varying state characteristics which includes the fraction of individuals with no greater than a high-school diploma, the average age, the fraction black, and the fraction Hispanic. As the results in panel (3) of Table 2 show, including these covariates does increase the magnitudes of the estimated employment effects for men in some decades, although the majority of these estimates remain statistically insignificant. The estimated female-male differences in employment effects, on the other hand, are essentially unaffected by this change.

To more fully address the possibility of selective-migration bias, I use the standard immigrant-enclave instrumental-variables strategy, introduced in Altonji and Card (1991) and Card (2001), and motivated by Bartel's (1989) observation that immigrants prefer locations where other immigrants before them have settled. The theory behind the instrument is that immigrant shares predicted from historical settlement patterns are necessarily unrelated to idiosyncratic contemporaneous local economic shocks. Provided that they are based on sufficiently lagged settlement patterns, predicted immigrant shares are also likely to be less related to serially correlated local shocks than observed immigrant shares, especially conditional on area fixed effects.

In my baseline implementation of this strategy, I predict the number of immigrants from source country j living in state s in decade t as the number M_{jt} of immigrants from source country j living in the U.S. in decade t times the fraction μ_{js1940} of all immigrants from j living in s in 1940. I then predict the immigrant share p_{st} of the population of s in t as $\hat{p}_{st} = (\sum_j M_{jt}\mu_{js1940})/(N_{st} + M_{st})$, where $N_{st} + M_{st}$ is the total population of s in t (that is, including natives and immigrants alike). Finally, I use $\hat{p}_{st}$ as an instrument for the immigrant share of the population living in s at t.

Table 3 presents IV estimates of each of the specifications introduced in Table 2 above. The first-stage regressions are summarized in Appendix Tables 15 and 16. Briefly, Appendix Table 15 shows that predicted immigration for each decade is strongly and positively related to observed immigration in that decade.[4] The top panel of Appendix Table 16 shows that the instrument is generally stronger in earlier decades, particularly for men, though for all groups the first-stage F-statistics dip below the Staiger and Stock (1997) threshold of 10, suggesting

[4]This table only shows on representative set of first-stage regression estimates. The gender- and skill-group-specific first-stages differ only in the weights applied to each observation.

that the instrument may be weak in later decades.[5] The second-stage estimates presented in Table 3 are very similar for all three specification. Focusing on the main specification, the IV estimates for men are generally more negative for all decades than the corresponding WLS estimates, although they are nearly all statistically insignificant, and the point estimates for 1960 and 1970 remain positive and somewhat large. Overall, the IV estimates for men are broadly similar to those found elsewhere in the immigration literature (see, e.g., Altonji and Card, 1991; Card, 2001).

The IV estimates of the female-male differences tend to be somewhat smaller in absolute value than their OLS counterparts for the low-skill group and somewhat larger for the high-skill group. Given the relatively small first-stage F-statistics for the low skilled, this pattern may signify that examining the gender difference in employment effects reduces the endogeneity of immigration itself, leaving the estimated gender difference more sensitive to minor violations of the exogeneity of the instrument. Regardless, the IV and WLS point estimates of the gender difference are similar, and their magnitudes are much larger than the male point estimates.

3.1 Alternative estimates and robustness tests

Although the immigrant-enclave approach is widely used, it is also imperfect, and has been the subject of several critiques. Wozniak and Murray (2012) argue that predictions of immigrant shares for each state based on historical settlement patterns may themselves be endogenous, since the national stock of immigrants is partly comprised of the potentially endogenous stock of immigrants living in each state. They advocate an alternative approach that excludes the contemporaneous stock of immigrants living in each state when using historical immigration patterns to apportion immigrants to that state. Panel (1) of Table 4 presents estimates of the employment effects of immigration using this implementation of the

[5] Baum et al. (2007) recommend falling back to the Staiger and Stock (1997) threshold when testing for weak instruments in models with non-i.i.d. errors.

instrument.[6] Modifying the instrument in this way increases the magnitudes of the estimated female-male differences in employment effects, which are now larger than the corresponding WLS estimates. This pattern is consistent with the above interpretation of the IV estimates for less-skilled women.

Jaeger et al. (2018) argue that IV estimates predicated on historical settlement patterns may be inconsistent for the short-run effects of immigration if labor markets adjust slowly to labor-supply shocks. The reason for this is that capital accumulated to offset previous immigration-induced supply shocks may induce a positive correlation between contemporaneous economic outcomes and predictions of current immigration based on historical immigration. The estimates presented above are based on models that already adopt the solution to this problem proposed by Jaeger et al. (2018), which is to control for immigration in previous periods, using predictions of lagged immigration to account for its potential endogeneity. Since these models do not control for immigration prior to 1960, this phenomenon likely explains why both the WLS and IV estimates of the employment effects for men are positive in 1960 and 1970—they partially reflect structural adjustments in response to immigration in previous decades.

My estimates are therefore mostly robust to this dynamic adjustment bias. However, the identification strategy advocated by Jaeger et al. (2018) hinges on whether historical settlement patterns independently predict current as well as lagged immigration. The Kleibergen-Paap LM tests (Kleibergen and Paap, 2006) presented in the top panel of Appendix Table 16 reject the null hypothesis of joint underidentification for each skill group. However, the F-statistics reported in that table also show that the instrument is weaker in later decades.[7] While there is a tradeoff here between using immigration patterns that are sufficiently recent that they are correlated with current immigration but sufficiently historical that they are

[6] To be precise, for these estimates I predict the immigrant share in state s and decade t as $\tilde{p}_{st} = [\sum_j (\sum_{s' \neq s} M_{js't})\mu_{js1940}]/(N_{st} + M_{st})$, where $M_{js't}$ is the number of immigrants from source j living in state $s' \neq s$ in decade t.

[7] A broader implication of this phenomenon may be that immigrant-enclave IV estimates of models with time-invariant effects identify local average treatment effects that are biased towards earlier sample periods.

uncorrelated with contemporaneous local economic conditions, Jaeger et al. (2018) suggest that a two-decade lag is probably sufficient to ensure that the instrument is exogenous to contemporaneous shocks. Accordingly, I also present versions of the IV estimates that use immigration patterns in decade $t - 20$ to instrument for immigration in decade t. As the bottom panel of Appendix Table 16 shows, the F-statistics for this two-decade-lag version of the instrument are considerably larger than those based entirely on 1940 settlement patterns, as are the Kleibergen-Paap LM statistics. On the other hand, the IV estimates, presented in the second panel of Table 4, are similar to those presented in Table 2 and based on 1940 shares.

Clemens and Hunt (2019), citing work by Kronmal (1993), show that in some cases immigrant-enclave IV estimates may be invalidated by a spurious correlation between contemporaneous and historical immigrant shares, arising because of their similar denominators. The objects of their critique are analyses of refugee waves, which typically use periods of time just spanning the arrival of the refugee wave, and during which the populations of labor markets are roughly constant. Although my inter-decadal analysis is therefore unlikely to suffer from this bias, I also implement the placebo test that they develop. Specifically, for each state and decade, I modify the enclave instrument by replacing the predicted number of immigrants with draws from an exponential distribution with mean equal to the decade-specific across-stage average number of immigrants. The resulting placebo IV estimates, displayed in panel (3) of Table 4, are all highly statistically insignificant, implying that the power of the instrument arises from actual enclaving behavior and not statistical artifice.

The estimates presented so far have been obtained using pooled samples of data spanning 1960 to 2010. An alternative approach is to estimate the employment effects separately for each pair of successive decades, which amounts to regressing decadal changes in employment on changes in immigration and year effects. Unlike the pooled estimates, the decade-pair approach allows for the possibility that the unobserved state effects vary over time, potentially assuaging concerns about the exogeneity of immigrant shares or their predictions based on historical patterns. Table 5 presents the results from such an exercise. The point estimates for men differ considerably from the pooled results presented above and vary in sign from period to period, although they are mostly small and statistically insignificant. The signs of the estimated gender differences also vary across decade pairs, though they are mostly negative and roughly comparable to the pooled-sample estimates, and more of them are statistically significant.

A closer look at the decade-pair estimates suggests that their differences from the pooled-sample estimates arise from differences in the specification used to estimate them that make the pooled estimates preferable. For men, the WLS point estimates change wildly from .5 in the 1960-70 period to -.24 in the 1970-1980 period (the IV estimates change similarly). The positive estimates for earlier decades are consistent with pooled estimates presented previously; the large negative effects for the 70s and 80s are not. This sign change coincides with a large drop in the pooled WLS point estimate (Table 2) between 1970 and 1980. Similarly, the decade-pair WLS estimates of the female-male differences are negative in every period but the one spanning 1970-1980. The largest decline in pooled WLS point estimates (an increase from -.67 to -.31) occurs between these decades. This pattern suggests that the constraint imposed by the decade-pair models that the employment effects are constant across decades is a specification error that produces severely misleading estimates.[8]

All of the estimates so far have related state-level employment and immigrant shares. There is another tradeoff here. State-level averages are probably measured more precisely than would be averages computed at lower levels of geography, and surely effects estimated at the state level imply that similar effects operate at lower levels. On the other hand, local immigrant shares may provide better measures of the extent of labor-market competition between immigrants and natives, at least insofar as such competition is constrained by geographical proximity.

[8]Somewhat more formally, suppose that the correct model is $y_{st} = \beta_t x_{st} + \lambda_s + \varepsilon_{st}$. Since $\Delta y_{st} = \beta_t \Delta x_{st} + (\beta_t - \beta_{t-1}) x_{st} + \Delta\varepsilon_{st}$, the decade-pair estimates identify $\beta_t + \rho_{t,t-1}(\beta_t - \beta_{t-1})$, where $\rho_{t,t-1} > 0$ is the slope coefficient from a population regression of Δx_{st} on x_{st}.

To assess the sensitivity of the employment effects to the geographical areas over which they are estimated, I estimate versions of (1) in which s indexes Metropolitan Statistical Areas (MSAs), rather than states. The results are presented in Table 6. Nearly all of the estimates are higher on the real line than their state-level counterparts from Tables 2 and 3. The WLS estimates of the female-male differences in employment effects are all negative, most of them significantly so. Because, as the first-stage results summarized in Appendix Tables 17 and 18 show, the immigrant-enclave instrument is less powerful when based on MSA-level settlement patterns, I have presented estimates based on two-decade-lagged patterns (rather than using 1940 patterns for each decade).[9] Despite this, the IV point estimates of the gender differences are smaller in absolute value than the WLS estimates. As I note above, this may be because the gender differencing reduces the endogeneity of the female-immigrant interaction, increasing its sensitivity to exogeneity violations of the instrument. These differences notwithstanding, the MSA-level estimates support the broad conclusion that immigration has a larger disemployment effect for women than for men, particularly among less-skilled natives.

The pattern of outcome effect estimates that decline when moving from the state to MSA level has been documented elsewhere in the immigration literature. Borjas (2006) finds that MSA-level estimates of wage effects are smaller than state-level estimates, which he attributes to the offsetting effect of native internal migration on relative labor-supply shocks. While this explanation seems plausible, especially since in my case the estimates for relatively mobile high-skilled workers are more sensitive to geography, the evidence on internal migration is mixed (cf. Borjas, 2006, and in a historical context, Boustan et al., 2010, with Card, 2001, and evidence from Peri and Sparber, 2011, that the estimates in Card, 2001, are better-suited to identify the degree of native outmigration). Hunt (2019), studying the impact of immigration on natives' educational attainment, also finds smaller

[9] One challenge with implementing the enclave instrument at the MSA level is that, for confidentiality reasons, IPUMS only identifies MSAs that are sufficiently populous. As a consequence, the set of identifiable MSAs changes over time (although the use of two-decade-lag shares partially addresses this). I have also made no attempt to ensure that the MSA boundaries are consistent over time.

MSA- than state-level effects, concluding that immigration may be more endogenous at the metro level. This explanation also seems plausible in this case, especially if highly skilled immigrants are more mobile. Without taking a position on precisely why the MSA-level effects are smaller in this case, I add that greater endogeneity of immigration at the MSA level may reduce the validity of the immigrant-enclave instrument as well.

4 Mechanisms

4.1 Wage effects and labor-supply elasticities

The evidence presented above suggests a pattern of declining native female-male differences in the effect of immigration on employment. These declines neatly mirror well-documented declines in female labor-supply elasticities over a similar time period. Blau and Kahn (2007) show that the labor-supply elasticities of married women decreased significantly between 1980 and 2000, arguing that the decline may not have begun until the 1980s. By comparison, my gender-difference estimates are roughly stable between 1960 and 1970 and decline continuously from 1980 to 2010.[10] Although, unlike Blau and Kahn (2007), my analysis focuses on the extensive margin of employment for all women, the correspondence between their findings and mine suggests that differences in the elasticity of female labor supply may help explain why immigration appears to affect employment for women by more than for men, and why this difference has decreased over time.

A labor-supply-elasticity interpretation of the gender difference in estimated employment effects requires that immigration also decreases natives' wages. The consensus from the literature is that, while immigration may cause small declines in the wages of less-skilled natives, it has little long-run effect on the average wage.[11] To my knowledge, no studies have

[10]My point estimates for highly skilled women suggest a decline beginning in 1970, although as Blau and Kahn (2007) note, there is also some evidence that labor-supply elasticities for married women also began their decline prior to 1980.

[11]To give a few examples, Altonji and Card (1991) find evidence of a negative wage effect for less-skilled natives between 1970 and 1980, while Card (2001) finds only modestly negative effects for natives (but not for

systematically explored gender differences in the effects of foreign immigration to the U.S. on natives' wages. In Table 7, I present estimates of male, and female-male differences in, wage effects for the same population studied above. I estimate these effects using specification (1), replacing employment-population ratios with group-average income from salary and wages, and present results for annual as well as weekly wages in order to capture potential changes along intensive margins.

The estimates for males are consistent with those found elsewhere in the literature. The WLS estimates suggest that immigration has little impact on wages for low-skilled men and small positive impacts for high-skilled men, and the IV point estimates are generally more negative. As I note in Section 3, the significantly positive male wage estimates in earlier decades likely represent adjustments to pre-1960 immigrants, for which the model does not control (Jaeger et al., 2018). The estimated female-male differences are actually positive and statistically significant for low-skilled workers, even when estimated by IV. For high-skilled workers, the estimated gender differences are negative, though mostly statistically insignificant. The annual wage estimates are generally slightly larger than the weekly estimates. These estimates may understate the underlying wage effects, especially since they do not account for selection into employment. However, they do not suggest that the estimated gender differences in employment effects, which appear across decades and skill groups, are driven by either secular differences in the effects of immigration on the wages of native women and men or secular gender differences in the labor-supply responses of native women and men to common wage effects.

The declining elasticities that Blau and Kahn (2007) estimate are for married women, among whom they also document that labor-supply is also more elastic overall. If differential labor-supply elasticities help explain the gender differences in employment effects, another

men). Borjas (2003) provides evidence that skill-group-specific immigration decreases natives' wages holding immigration from other skill groups constant, but his structural simulations reveal an average effect of zero, a point underscored in Ottaviano and Peri (2011), who note that in a constant-returns-to-scale economy, the long-run average wage effect must be zero. Card (1990) finds that the Mariel Boatlift had no effect on the wages of less-skilled Miami workers, a point contested by Borjas (2017) and counter-contested by Clemens and Hunt (2019).

possibility is that the employment differences are driven by subpopulations of women with relatively elastic labor supply. The estimates presented in Table 8, which disaggregate employment effects by marital status, support this hypothesis. The estimates for single and married men are essentially indistinguishable from each other, as well as from the pooled estimates presented above. For women, conditioning on marital status reveals considerable heterogeneity. Among single women, immigration increases the likelihood of employment in each decade, regardless of skill group or estimation method, and these positive effects decline over time. Among married women, again regardless of skill group and estimation technique, there are negative employment effects whose absolute values exceed the positive effects for single women as well as the negative effects for all women presented previously, and decline over time.

The marital-status specific employment estimates in Table 8 are highly consistent with the theory that heterogenous labor-supply elasticities mediate the effects of immigration on natives' employment rates. As further evidence of this, in Table 9 I replicate these estimates with parent status in place of marriage. As before, male employment is insensitive to employment across all groups. Here, the estimated employment effects for women without children are smaller, although they remain positive, while there remains a significant and declining negative impact for low- and high-skill women alike. The difference in effects between unmarried women and those without children presumably arises because labor-supply is less elastic among the former group.

Even within relatively elastic subgroups, a prerequisite for a labor-supply-elasticity explanation of gender differences in employment effects is that immigration decreases natives' wages, at least for women. Table 10, which presents estimates of the impact of immigration on natives' wages by marital status, shows that it does, and in such a way that the decreases are masked by estimates such as those in Table 7 that pool married and unmarried women. As in the wage regressions presented previously, the estimates do not suggest significant wage effects in any direction for men, regardless of decade, skill group, marital status or estimation method. In contrast, the estimated wage effects for women vary substantially by group. Among married women, there are large negative employment effects for the high-skilled and smaller, statistically insignificant effects for the low-skilled. These are accompanied by positive effects for unmarried women of any skill level. The analogous results, disaggregated by parent status, in Table 11 are similar.

While I have so far avoided interpreting my estimates through the lens of a particular, explicit economic model, the evidence on the impacts of immigration on female employment and wages by marital and parent status in Tables 8–11 has a relatively straightforward competitive general-equilibrium interpretation. Initially, immigration-induced supply shocks put downward pressure on the wages of all women. Faced with these wage declines, some married women exit the labor force (ignoring parent status for simplicity). Since single women are closer substitutes to married native women than to immigrants, these exits more-than-offset competition between single women and immigrants, increasing the marginal product of labor for single women, and hence the wages they face and the amount of labor they supply.[12] Finally, since the labor supply of married women is relatively elastic, their attrition from the labor force is not fully offset by the entry of single women, resulting in a negative average female employment effect.[13]

4.2 Observable skill differences

The mutual correspondence between the declining female-male differences in employment effects presented above in Tables 2 and 3, the declining labor-supply elasticities for married women estimated in Blau and Kahn (2007), the marital- and parent-status-specific employment effects in Tables 8 and 9, and the analogous group-specific wage effects in Tables 10

[12] One point of departure between this explanation and the evidence is that I observe a negative employment effect, but no such wage effect, for less-skilled married women. Two potential reasons for the discrepancy are that sample selection on the basis of employment attenuates the wage estimates or that, as I discuss above, IV estimates of the female-male difference in wage effects are more sensitive to minor violations of the exogeneity of the instrument.

[13] This explanation can be justified rigorously from within the nested constant-elasticity-of-substitution framework of Ottaviano and Peri (2011, also see Card, 2001; Borjas, 2003).

and 11 indicate that gender differences in the elasticity of labor supply are a crucial part of why immigration reduces employment for women, but not men. Absent wage effects for men, however, differential elasticities cannot be the whole story. And neither my estimates, nor those elsewhere in the literature, suggest wage effects for men on the scale of those presented above for married women. Furthermore, the estimated female wage effects themselves decrease over time, which suggests that declining female labor-supply elasticities cannot be the only reason why the employment effects decline as well.

Thus the question becomes, why does immigration affect the wages of native women but not native men? A natural place to start looking for an answer to this question is with gender differences in natives' educational attainment. To provide simple evidence on whether educational attainment can explain the gender gap in employment effects, in the first panel of Figure 5 I graph the national fractions of native men, native women, and immigrants who are highly skilled (that is, have a better-than-high-school education). A limitation of this comparison is that, because native educational attainment may respond to immigration (Hunt, 2019), these comparisons may misstate the degree of immigrant-native competition within skill groups. As the top panel shows, native women are not systematically more similar to immigrants than are men according to this definition of skill, minor interdecadal variation notwithstanding.

Evidently, gender differences in broad skill distributions cannot explain the estimated gender differences in employment and wage effects. Moreover, I find negative gender differentials in employment effects for both skill groups, which cannot be explained by gender differences in the distribution of a single binary skill. In the remaining panels of Figure 5, I examine gender differencees in skill distributions within these broad skill groups. The second panel shows the fractions of low-skilled workers (i.e., those with a high-school education or less) who have a high-school degree. In every decade, low-skilled native women are more educated than men, who in turn are more skilled than immigrants. The final panel of Figure 5 displays the fractions of highly skilled (greater than high-school) native men, native women, and immigrants with a college degree or more education. Here, native women are less educated than immigrants and native men, although the native gender differences disappear over time. Within broadly defined skill groups, women's skills are more different from immigrants' than are men's. These education distributions do not suggest that skill differences drive the gender difference in immigration effects.

Education is not the only observable correlate of skill, which may also manifest itself in the occupations and industries into which workers select. Figure 6 summarizes national gender-specific indices of occupational and industrial dissimilarity between natives and immigrants.[14] As in the case of education, these indices do not account for the possibility that natives switch occupations or industries in response to competition from immigrants.

The top panel, which presents the results for occupational dissimilarity, shows that there are considerable differences in the occupations in which natives and immigrants work. However, in every decade native women are more occupationally dissimilar from immigrants than native men are. This is prima facie inconsistent with the notion that occupational selection explains why immigration impacts women more than men. The bottom panel of the table shows the results for industrial dissimilarity, which also suggests considerable native-immigrant differences. Here, women are initially more similar to immigrants than men are, achieve parity in 1990, and are more different thereafter. This is also inconsistent with the estimated gender differences in employment effects, which are negative throughout the entire sample period.

Neither the national distributions of educational attainment nor the national indices of occupational and industrial dissimilarity indicate that observable skill differences between native men and women explain gender differences in the effects of immigration. However, I identify those effects using variation in state-level immigrant shares. To test whether those

[14] The native-immigrant dissimilarity index is $.5\sum_j |N_j/N - M_j/M|$, where N_j and M_j are the numbers of natives and immigrations in occupation or industry j and N and M are the total native and immigrant populations. The index can be interpreted as the fraction of natives that would need to change occupation or industry for the native and immigrant occupational or industrial distribution to be identical. I use three-digit IPUMS 1990 occupation and industry codes (*occ1990* and *ind1990*) to calculate the indices.

effects are robust to differences in education, occupation, and industry measured at the state level, and also to formalize the graphical evidence in Figures 5 and 6, I also estimate regressions that control for those factors. Specifically, I estimate models that replace average wages with state fixed effects from gender- and decade-specific regressions of individual log wages on narrow educational attainment (having a high-school degree for the low-skilled and a college degree for the high-skilled) or indicators for two-digit industry and occupation codes, in addition to state fixed effects.

To limit tabular proliferation, I only report IV results for annual wages by marital status. The left-panel of Table 12 reports results that control for education within broad skill groups. The estimated gender differences in wage effects are very similar to the unadjusted results in Table 10; for highly skilled married women, the effects are actually larger. The estimates in the right panel of the table control for occupation and industry of employment. These results also show large negative wage effects for highly skilled married women and moderate positive effects for their single counterparts. Here, the magnitudes of the estimated positive effects for less-skilled single women are smaller, which may suggest that industry and occupation are an important channel through which imperfect substitution between these and other women arises. Neither set of estimate implies that the estimated gender differences in wage (or employment) effects are a consequence of educational, occupational, or industrial differences between native men and women.

4.3 Gender-specific immigration

To provide additional evidence on the cause of the gender difference in immigration effects, in Table 13 I present estimates of the effects of immigrant fractions of the male and female populations on the employment rates of native men and women. The least-squares point estimates resemble a substitution matrix, with native employment decreasing in own-gender immigration and increasing in cross-gender immigration. This pattern holds across skill groups, although the magnitudes are larger for less-skilled natives, and the magnitudes for all genders and skill groups decrease over time. In contrast to the total employment estimates presented above, the gender-specific models do imply that there are direct partial employment effects for men, which may be offset by attrition of women from the labor force. Note that the estimates in the bottom half of the table represent the effects of immigration on female employment itself, and not gender differences in those effects.

The immigrant shares of the male and female populations are highly correlated, and their separate effects on native male and female employment are difficult to identify, and estimated imprecisely.[15] For the same reason, the IV estimates, which in this case are based on gender-specific historical immigration patterns, are all statistically insignificant, and (unreported) Kleibergen-Paap LM tests fail to reject the null of underidentification by a wide margin. The instrument is simply not strong enough to predict male immigration separately from female immigration. Conclusions about the effects of gender-specific immigration must therefore be somewhat tentative.

With this caveat in mind, what the estimates in Table 13 suggest is that, regardless of skill group or nativity, male and female labor are, effectively, imperfect substitutes, and their substitutability has increased over time. They also imply that the negative employment effects for native women are driven by competition from female immigrants. As the results in Section 4.2 show, this imperfect substitution is not a consequence of differences in educational attainment or selection into different occupations and industries. One potential explanation for this apparent imperfect substitution is that there are gender differences in skills that are unmeasured and uncorrelated with observable factors such as education, occupation and industry. Another still is that it arises because of discrimination against women in the labor-market. While both explanations may be consistent with decreasing gender differences in the effects of immigration over time, it is difficult to provide direct evidence to support either, let alone disentangle them. It is also possible that neither of these factors explains the differential immigration effects.

[15] Pooling across decades, the slope coefficient from a regression of the female share on the male share is about 1.1.

5 Conclusion

The results presented in this paper show that foreign immigration reduces the employment rates of native women, both in absolute terms and relative to men. These effects persist across skill groups and decades, although they become less pronounced over time. They are robust to the potential for geographical sorting among immigrants, as well as the specification and level of geographical variation used to estimate them. The declining female employment effects suggest a connection to female labor-supply elasticities, which decline similarly. I find that those effects are driven by responses of relatively elastic subgroups of married women and mothers to negative immigration-induced wage shocks. The attrition of these women from the labor force appears to offset competition between immigrants and other women, who actually face positive wage shocks, and increase their labor supply accordingly. I find no systematic employment or wage effects for native men, either in aggregate or in any subgroup.

Neither gender differences in educational attainment nor choice of occupation or industry, factors that Blau and Kahn (2017) highlight for their power to explain the gender wage gap, account for gender differences in the effects of immigration on wages or employment. I further find that that native employment is decreasing in own-gender, and increasing in cross-gender, immigration, implying that men and women are effectively imperfect substitutes, and that the employment effects for native women are driven by competition from female immigrants. Although it is not explained by observable skill differences, the source of this apparent imperfect substitution is an open question. Two standard candidates are gender differences in unobserved skills and labor-market discrimination against women; for a sufficiently broad definition of skill, these potential explanations are exhaustive.

One implication of these findings is that immigration slowed the expansion of the female labor force in the US during the second half of the 20th century. Absent immigration, women would have made even larger inroads. But the consequences are not only historical. The female employment effects that I estimate remain negative in 2010, the most recent period that I study. Competition between native women and immigrants, therefore, also contributes to our understanding of the persistent gender gap in employment. However, as Blau and Kahn (2017) caution, covariation is not explanation; fully accounting for this gap requires that we know not only which factors contribute to it, but also why.

Appendix A: Data, sample selections, and variable definitions

The main data source for this study is a combination of the IPUMS 5% extracts of the 1960, 1980, 1990 and 2000 U.S. Censuses, a pooled sample of the state and metro form-1 extracts of the 1970 Census, and the three-year 2011 American Community Survey extract, which pools observations spanning 2009-2011 and represents a 3% sample of the population (I refer this as the 2010 sample).[16] To estimate historical immigration patterns, I augment these data with 1% extracts of the 1940, 1950, and 1970 (state and metro form 2) Censuses, from which I use only country of birth and state and metropolitan area of residence at the time of enumeration.

I exclude from the sample those aged 16 or younger and those aged 65 or older, as well as those living in Puerto Rico or on military bases. I also exclude individuals who are currently enrolled in school, except for in the 1970 samples, for which that information is not available.

I code individuals as immigrants if their country of birth is not the United States. I define the annual wage as annual income from wages and salaries (the IPUMS variable *incwage*), and code individuals as employed if their annual wage is nonzero. I replace top-coded values with 1.5 times the annual top-code, or 1.5 times the state-specific top-code in the ACS samples. I use the IPUMS-supplied CPI deflator (*cpi99*) to express annual wages in 1999 dollars. I define the weekly wage as the annual wage divided by weeks worked. When weeks

[16] In 1970, metropolitan areas are only available in the metro-level extracts, so only this sample is used for the metropolitan-area-level analysis for that year.

worked is only available in an intervalled format, I define weeks worked as the midpoint of the interval.

I apply the IPUMS-supplied probability weights (*perwt*) when aggregating data to the state or metropolitan-area level.

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Tables and figures

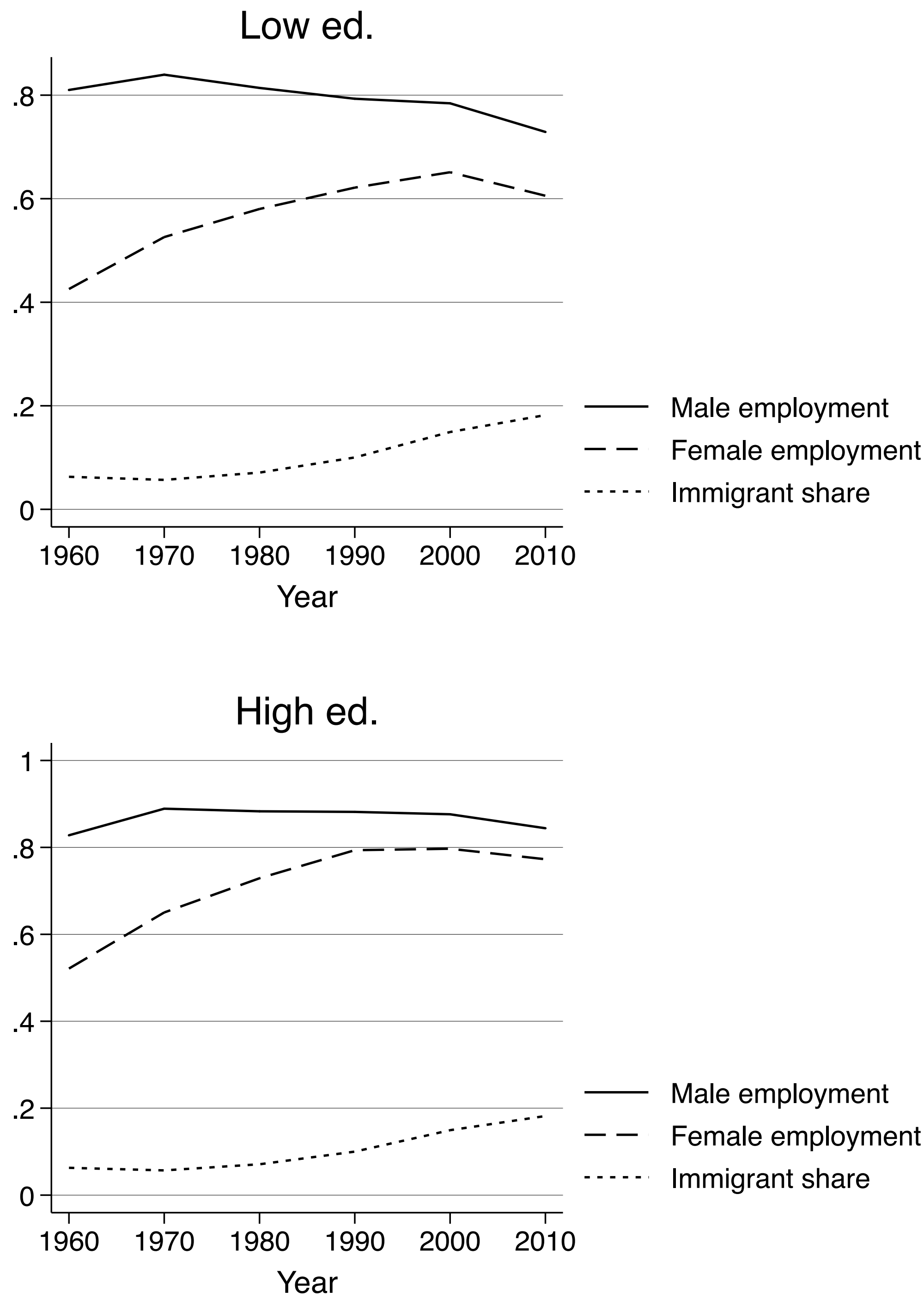

Figure 1: Native employment/population ratios and immigrant shares

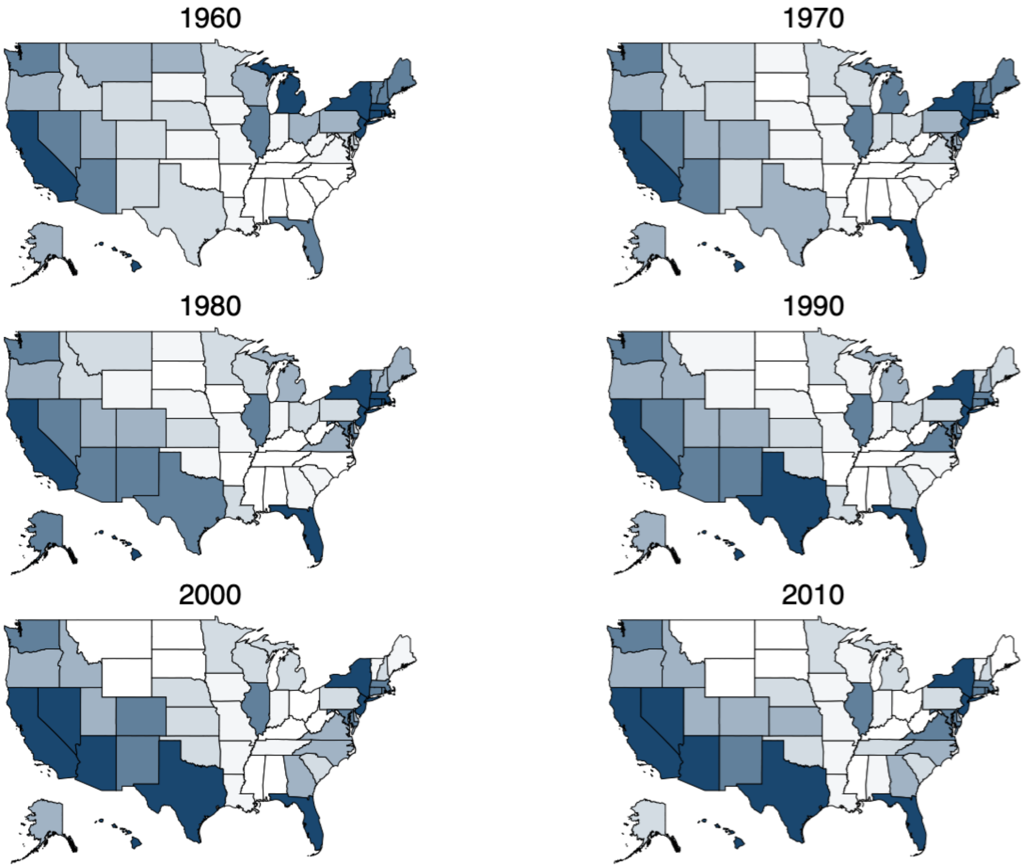

Figure 2: Immigrant population shares by decade

Table 1: Descriptive statistics for state-level variables

	Employment/population									
	Male				Female					
	Low ed.		High ed.		Low ed.		High ed.		Immig./pop.	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
1960	0.79	0.07	0.82	0.04	0.43	0.06	0.52	0.04	0.05	0.04
1970	0.83	0.06	0.88	0.03	0.53	0.05	0.64	0.04	0.04	0.03
1980	0.81	0.04	0.88	0.02	0.59	0.05	0.73	0.03	0.05	0.04
1990	0.79	0.04	0.88	0.02	0.64	0.05	0.80	0.03	0.06	0.06
2000	0.78	0.04	0.87	0.02	0.68	0.05	0.81	0.03	0.10	0.08
2010	0.72	0.05	0.84	0.02	0.64	0.05	0.79	0.03	0.12	0.08

Notes: Unweighted across-state averages of state-level variables. "Employment/population" denotes the fraction of the population with nonzero annual wage and salary income. "Immig./pop." denotes the fraction of the population born outside of the US. "Low" denotes a high school degree or less; "High" denotes some college or more education.

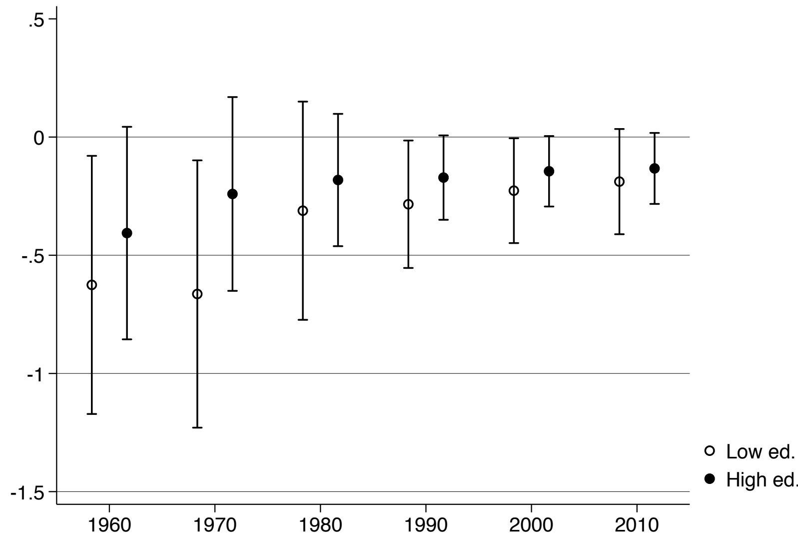

Figure 3: Gender differences in the employment effects of immigration

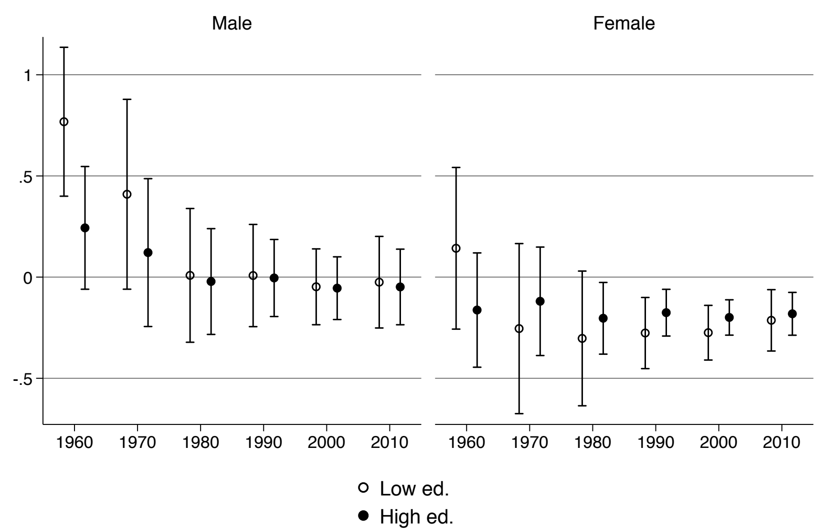

Figure 4: Gender-specific employment effects of immigration

Table 2: Immigration and native employment

		(1)		(2)		(3)	
		Low	High	Low	High	Low	High
Male	1960	0.768***	0.243	0.695***	0.293**	0.720***	0.253*
		(0.183)	(0.151)	(0.183)	(0.140)	(0.193)	(0.137)
	1970	0.409*	0.121	0.360	0.180	0.324	0.148
		(0.234)	(0.182)	(0.234)	(0.162)	(0.236)	(0.168)
	1980	0.00854	-0.0218	0.0150	0.0421	-0.0499	0.0197
		(0.165)	(0.130)	(0.166)	(0.124)	(0.169)	(0.133)
	1990	0.00775	-0.00436	-0.0273	0.0257	-0.156	-0.0139
		(0.126)	(0.0949)	(0.127)	(0.0902)	(0.122)	(0.0839)
	2000	-0.0479	-0.0547	-0.0871	-0.0255	-0.182*	-0.0527
		(0.0934)	(0.0772)	(0.0962)	(0.0718)	(0.0969)	(0.0633)
	2010	-0.0251	-0.0487	-0.0718	-0.0411	-0.273**	-0.0748
		(0.113)	(0.0931)	(0.115)	(0.0863)	(0.125)	(0.0750)
Female – Male	1960	-0.625**	-0.406*	-0.606**	-0.451**	-0.629**	-0.403*
		(0.272)	(0.224)	(0.267)	(0.219)	(0.273)	(0.224)
	1970	-0.664**	-0.241	-0.675**	-0.353*	-0.668**	-0.236
		(0.281)	(0.204)	(0.278)	(0.197)	(0.282)	(0.204)
	1980	-0.312	-0.182	-0.317	-0.207	-0.315	-0.177
		(0.230)	(0.139)	(0.222)	(0.148)	(0.230)	(0.139)
	1990	-0.284**	-0.171*	-0.265**	-0.199**	-0.286**	-0.169*
		(0.134)	(0.0889)	(0.131)	(0.0912)	(0.135)	(0.0885)
	2000	-0.227**	-0.145*	-0.218**	-0.165**	-0.229**	-0.143*
		(0.110)	(0.0742)	(0.108)	(0.0738)	(0.111)	(0.0740)
	2010	-0.188*	-0.133*	-0.177	-0.154**	-0.190*	-0.131*
		(0.111)	(0.0748)	(0.109)	(0.0729)	(0.111)	(0.0745)
Observations		612	612	612	612	612	612
R-squared		0.978	0.984	0.755	0.789	0.981	0.985

Notes: Unless otherwise noted, the dependent variable is the native employment population ratio and the key independent variable is the immigrant fraction of the population. "Female – Male" denotes the coefficient on an interaction between the immigrant fraction and indicators for female and decade. "Low" denotes a high school degree or less; "High" denotes some college or more education. The dependent variable in panel (2) is the state fixed effect from a gender- and decade-specific regression of individual employment status on age indicators and state fixed effects. The models in panel (3) includes as state-level covariates the average age, the fractions with at least some college, and the black and hispanic shares of the population. All regressions weighted by the number of observations used to calculate the employment rate. Standard errors clustered on state. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Immigration and native employment, IV estimates

		(1)		(2)		(3)	
		Low	High	Low	High	Low	High
Male	1960	0.607***	0.146	0.527***	0.223**	0.657***	0.193**
		(0.166)	(0.110)	(0.166)	(0.106)	(0.158)	(0.0972)
	1970	0.312	0.00178	0.243	0.0962	0.390*	0.0936
		(0.216)	(0.151)	(0.217)	(0.137)	(0.217)	(0.150)
	1980	-0.0635	-0.0734	-0.0677	0.0160	0.0128	0.00835
		(0.177)	(0.127)	(0.176)	(0.125)	(0.210)	(0.142)
	1990	-0.0995	-0.0604	-0.139	-0.00986	-0.161	-0.0429
		(0.128)	(0.0852)	(0.126)	(0.0851)	(0.171)	(0.0996)
	2000	-0.131	-0.0977	-0.178*	-0.0582	-0.178	-0.0797
		(0.0970)	(0.0719)	(0.0958)	(0.0672)	(0.149)	(0.0845)
	2010	-0.0548	-0.0845	-0.109	-0.0686	-0.195	-0.0986
		(0.120)	(0.0910)	(0.120)	(0.0838)	(0.196)	(0.114)
Female – Male	1960	-0.513***	-0.457***	-0.500***	-0.530***	-0.519***	-0.454***
		(0.177)	(0.175)	(0.174)	(0.177)	(0.177)	(0.175)
	1970	-0.567***	-0.224	-0.584***	-0.382**	-0.573***	-0.224
		(0.196)	(0.154)	(0.194)	(0.158)	(0.195)	(0.155)
	1980	-0.328	-0.264*	-0.334*	-0.335**	-0.332*	-0.262*
		(0.201)	(0.149)	(0.194)	(0.164)	(0.201)	(0.150)
	1990	-0.242**	-0.238**	-0.226**	-0.287**	-0.245**	-0.237**
		(0.110)	(0.110)	(0.109)	(0.117)	(0.110)	(0.111)
	2000	-0.151*	-0.171**	-0.145*	-0.205***	-0.153*	-0.171**
		(0.0823)	(0.0779)	(0.0806)	(0.0789)	(0.0825)	(0.0784)
	2010	-0.0765	-0.131*	-0.0672	-0.167**	-0.0797	-0.131*
		(0.0770)	(0.0694)	(0.0785)	(0.0699)	(0.0774)	(0.0695)
Observations		588	588	588	588	588	588
R-squared		0.977	0.984	0.748	0.784	0.981	0.985

Notes: Unless otherwise noted, the dependent variable is the native employment population ratio and the key independent variable is the immigrant fraction of the population. "Female – Male" denotes the coefficient on an interaction between the immigrant fraction and indicators for female and decade. "Low" denotes a high school degree or less; "High" denotes some college or more education. The dependent variable in panel (2) is the state fixed effect from a gender- and decade-specific regression of individual employment status on age indicators and state fixed effects. The models in panel (3) includes as state-level covariates the average age, the fractions with at least some college, and the black and hispanic shares of the population. All regressions weighted by the number of observations used to calculate the employment rate. Standard errors clustered on state. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Immigration and native employment, alternative IV estimates

		(1)		(2)		(3)	
		Low	High	Low	High	Low	High
Male	1960	0.605***	0.188*	0.604***	0.132	-9.398	-3.173
		(0.190)	(0.100)	(0.150)	(0.124)	(15.33)	(4.699)
	1970	0.366	0.0650	0.303	-0.0204	-12.11	-4.032
		(0.245)	(0.140)	(0.194)	(0.170)	(17.36)	(5.060)
	1980	-0.0524	-0.0155	-0.0687	-0.0885	-9.158	-3.006
		(0.218)	(0.131)	(0.151)	(0.127)	(12.02)	(3.455)
	1990	-0.121	-0.0327	-0.0902	-0.0669	-6.424	-1.891
		(0.169)	(0.0966)	(0.107)	(0.0836)	(8.319)	(2.193)
	2000	-0.140	-0.0743	-0.123	-0.109	-4.949	-1.512
		(0.119)	(0.0753)	(0.0802)	(0.0663)	(6.036)	(1.702)
	2010	-0.0503	-0.0536	-0.0921	-0.108	-4.157	-1.373
		(0.137)	(0.0940)	(0.0951)	(0.0826)	(4.839)	(1.468)
Female – Male	1960	-0.630***	-0.610***	-0.546***	-0.413**	-0.193	-4.860
		(0.187)	(0.168)	(0.191)	(0.184)	(3.041)	(6.577)
	1970	-0.722***	-0.342*	-0.621***	-0.148	0.437	-4.620
		(0.230)	(0.180)	(0.205)	(0.152)	(3.464)	(6.620)
	1980	-0.498**	-0.447**	-0.348*	-0.187	0.422	-3.234
		(0.235)	(0.182)	(0.189)	(0.124)	(2.386)	(4.342)
	1990	-0.327**	-0.370**	-0.266***	-0.183**	0.254	-2.146
		(0.153)	(0.148)	(0.100)	(0.0852)	(1.593)	(2.738)
	2000	-0.197*	-0.260**	-0.186**	-0.136**	0.378	-1.728
		(0.118)	(0.104)	(0.0744)	(0.0627)	(1.126)	(2.123)
	2010	-0.110	-0.213**	-0.142*	-0.116*	0.294	-1.442
		(0.114)	(0.0965)	(0.0737)	(0.0592)	(0.931)	(1.818)
Observations		588	588	604	604	612	612
R-squared		0.977	0.983	0.978	0.984	<0	0.431

Notes: Unless otherwise noted, the dependent variable is the native employment population ratio and the key independent variable is the immigrant fraction of the population. "Female – Male" denotes the coefficient on an interaction between the immigrant fraction and indicators for female and decade. "Low" denotes a high school degree or less; "High" denotes some college or more education. The instrument in panel (1) excludes immigrants in state s when using 1940 shares to predict immigration to that state. The instrument in panel (2) predicts immigration in year t using immigrant shares from year $t-20$ (instead of using 1940 for all decades). The instrument in panel (3) replaces the predicted number of immigrants based on historical shares with exponentially distributed white noise with mean equal to the (decade-specific) across-state average number of immigrants. Standard errors clustered on state. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Immigration and native employment, decade-pair-specific estimates

		WLS		IV	
		Low	High	Low	High
Male	1960-1970	0.502	0.314	0.692**	0.600***
		(0.488)	(0.294)	(0.280)	(0.176)
	1970-1980	-0.244	-0.279**	-0.369*	-0.185
		(0.161)	(0.110)	(0.195)	(0.136)
	1980-1990	0.0357	0.0384	-0.285	-0.0424
		(0.154)	(0.0538)	(0.178)	(0.0621)
	1990-2000	-0.166	-0.139	-0.148	-0.201**
		(0.135)	(0.0839)	(0.197)	(0.0940)
	2000-2010	0.0422	0.198	-5.745	-0.181
		(0.511)	(0.268)	(9.185)	(1.282)
Female – Male	1960-1970	-0.180	-0.680***	-0.364**	-1.195***
		(0.263)	(0.240)	(0.167)	(0.338)
	1970-1980	0.318***	0.187**	0.244*	-0.0281
		(0.111)	(0.0903)	(0.136)	(0.209)
	1980-1990	-0.390***	-0.184***	-0.179	-0.231***
		(0.140)	(0.0494)	(0.219)	(0.0598)
	1990-2000	-0.175	-0.128*	0.136	0.0637
		(0.117)	(0.0755)	(0.0995)	(0.0798)
	2000-2010	-0.135	-0.227	-6.088	-3.077
		(0.241)	(0.242)	(7.397)	(2.726)
Observations		204	204	196	196

Notes: Each row represents a regression of the (skill- and gender-specific) employment rate on the (overall) immigration share of the population, an interaction between the immigrant share and female, and state and year effects, estimated using a different pair of consecutive decades. "Low" denotes a high school degree or less; "High" denotes some college or more education. All regressions weighted by the number of observations used to calculate the employment rate. Standard errors clustered on state. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Immigration and native employment, MSA level

		WLS		IV	
		Low	High	Low	High
Male	1960	0.367***	0.0406	0.273***	-0.129
		(0.0696)	(0.0921)	(0.0650)	(0.0901)
	1970	0.287***	0.0554	0.232**	-0.169
		(0.0925)	(0.100)	(0.101)	(0.127)
	1980	0.0542	0.00128	-0.0162	-0.114
		(0.0597)	(0.0548)	(0.0538)	(0.0721)
	1990	0.0457	0.0238	-0.0241	-0.0579
		(0.0604)	(0.0489)	(0.0442)	(0.0522)
	2000	-0.0153	-0.00884	-0.0773**	-0.0899**
		(0.0441)	(0.0438)	(0.0308)	(0.0449)
	2010	0.0453	0.0142	-0.00255	-0.0728
		(0.0398)	(0.0456)	(0.0307)	(0.0508)
Female – Male	1960	-0.227	-0.130	-0.0403	0.0639
		(0.140)	(0.0958)	(0.117)	(0.0770)
	1970	-0.385**	-0.133	-0.228	0.120
		(0.153)	(0.123)	(0.140)	(0.101)
	1980	-0.196	-0.116*	-0.127	-0.0109
		(0.126)	(0.0694)	(0.101)	(0.0646)
	1990	-0.211**	-0.135**	-0.138**	-0.0681
		(0.0895)	(0.0571)	(0.0665)	(0.0479)
	2000	-0.187***	-0.130***	-0.106**	-0.0564
		(0.0707)	(0.0470)	(0.0496)	(0.0384)
	2010	-0.173**	-0.137***	-0.0877*	-0.0563
		(0.0674)	(0.0455)	(0.0485)	(0.0381)
Observations		2,710	2,710	1,956	1,956
R-squared		0.980	0.976	0.983	0.980

Notes: Dependent variable is the native employment population ratio; key independent variable is the immigrant fraction of the population. "Low" denotes a high school degree or less; "High" denotes some college or more education. The instrument predicts immigrant shares in decade t using shares in decade $t-20$. All regressions weighted by the number of observations used to calculate the employment rate. Standard errors clustered on metropolitan area. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Immigration and native wages

		WLS				IV			
		Annual		Weekly		Annual		Weekly	
		Low	High	Low	High	Low	High	Low	High
Male	1960	2.392**	0.571*	2.230**	0.587*	2.208***	-0.179	2.049***	-0.0663
		(0.956)	(0.329)	(0.873)	(0.311)	(0.753)	(0.479)	(0.715)	(0.472)
	1970	1.150	0.525	1.540*	0.662	1.146	-0.544	1.462*	-0.0527
		(1.008)	(0.434)	(0.916)	(0.462)	(0.829)	(0.702)	(0.761)	(0.640)
	1980	0.0290	0.103	0.279	0.290	-0.00614	-0.827	0.207	-0.505
		(0.680)	(0.278)	(0.640)	(0.257)	(0.656)	(0.586)	(0.626)	(0.563)
	1990	0.375	0.627***	0.576	0.702***	0.250	0.0277	0.500	0.227
		(0.494)	(0.191)	(0.488)	(0.187)	(0.480)	(0.336)	(0.466)	(0.316)
	2000	-0.0202	0.410**	0.175	0.502***	-0.170	-0.0832	0.0400	0.0950
		(0.411)	(0.156)	(0.397)	(0.149)	(0.355)	(0.273)	(0.348)	(0.258)
	2010	0.0763	0.431**	0.208	0.511***	0.0157	-0.0481	0.0929	0.112
		(0.470)	(0.206)	(0.435)	(0.183)	(0.438)	(0.270)	(0.394)	(0.255)
Female – Male	1960	1.716***	-0.185	1.225***	-0.153	1.636***	-0.390	1.357***	-0.175
		(0.629)	(0.356)	(0.320)	(0.271)	(0.473)	(0.369)	(0.234)	(0.284)
	1970	1.285	-0.891	0.674	-0.605	0.876	-1.141**	0.718*	-0.710*
		(0.872)	(0.541)	(0.497)	(0.384)	(0.764)	(0.566)	(0.421)	(0.418)
	1980	1.163**	-0.149	0.623*	-0.175	0.767	-0.498	0.537*	-0.337
		(0.538)	(0.294)	(0.316)	(0.239)	(0.548)	(0.401)	(0.298)	(0.312)
	1990	0.963***	-0.0528	0.621***	0.0805	0.787**	-0.297	0.621***	-0.0474
		(0.290)	(0.154)	(0.158)	(0.127)	(0.342)	(0.269)	(0.156)	(0.195)
	2000	0.645**	-0.114	0.408***	0.0132	0.469	-0.304	0.398***	-0.0926
		(0.246)	(0.136)	(0.138)	(0.113)	(0.303)	(0.218)	(0.137)	(0.167)
	2010	0.617**	-0.190	0.408**	-0.0322	0.453	-0.344	0.420***	-0.0922
		(0.293)	(0.152)	(0.175)	(0.132)	(0.313)	(0.226)	(0.142)	(0.176)
Observations		612	612	612	612	588	588	588	588
R-squared		0.979	0.991	0.973	0.989	0.979	0.990	0.973	0.988

Notes: Dependent variable is average log annual or weekly (annual/weeks worked) wage as noted; key dependent variable is immigrant share of population."Female – Male" denotes the coefficient on an interaction between the immigrant fraction and indicators for female and decade. "Low" denotes a high school degree or less; "High" denotes some college or more education. All regressions weighted by the number of observations used to calculate the wage. Standard errors clustered on state. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8: Immigration and native employment, by marital status

		WLS				IV			
		Low		High		Low		High	
		Single	Married	Single	Married	Single	Married	Single	Marred
Male	1960	0.902***	0.660***	0.356*	0.173	0.840***	0.545***	0.241	0.107
		(0.157)	(0.208)	(0.188)	(0.148)	(0.136)	(0.175)	(0.150)	(0.109)
	1970	0.472*	0.345	0.148	0.0930	0.592**	0.263	-0.00669	0.0239
		(0.237)	(0.240)	(0.263)	(0.162)	(0.240)	(0.216)	(0.225)	(0.143)
	1980	-0.0376	0.00275	-0.0569	-0.0287	-0.0275	-0.0135	-0.156	-0.0376
		(0.139)	(0.182)	(0.156)	(0.123)	(0.149)	(0.190)	(0.152)	(0.128)
	1990	-0.0206	0.0256	0.0120	-0.0219	-0.0531	-0.0379	-0.0672	-0.0509
		(0.102)	(0.135)	(0.110)	(0.0902)	(0.110)	(0.137)	(0.104)	(0.0866)
	2000	-0.118*	0.0148	-0.0470	-0.0614	-0.124	-0.0482	-0.0961	-0.0886
		(0.0698)	(0.102)	(0.0861)	(0.0748)	(0.0787)	(0.0975)	(0.0892)	(0.0709)
	2010	-0.0548	0.0404	-0.0332	-0.0463	0.0299	-0.00368	-0.0592	-0.0721
		(0.0959)	(0.116)	(0.106)	(0.0859)	(0.114)	(0.115)	(0.118)	(0.0837)
Female – Male	1960	0.261*	-1.013***	0.153	-0.790***	0.441***	-0.967***	0.351***	-0.952***
		(0.152)	(0.234)	(0.116)	(0.253)	(0.121)	(0.170)	(0.104)	(0.225)
	1970	0.412**	-1.205***	0.448***	-0.819***	0.616***	-1.232***	0.740***	-0.998***
		(0.163)	(0.267)	(0.126)	(0.240)	(0.168)	(0.201)	(0.136)	(0.239)
	1980	0.272*	-0.634***	0.163*	-0.511***	0.350**	-0.728***	0.358***	-0.742***
		(0.154)	(0.226)	(0.0816)	(0.180)	(0.157)	(0.210)	(0.113)	(0.218)
	1990	0.127*	-0.502***	0.0853*	-0.362***	0.204**	-0.498***	0.200***	-0.518***
		(0.0679)	(0.144)	(0.0436)	(0.124)	(0.0848)	(0.121)	(0.0743)	(0.164)
	2000	0.118*	-0.424***	0.0767*	-0.304***	0.194***	-0.358***	0.166***	-0.381***
		(0.0650)	(0.108)	(0.0404)	(0.0962)	(0.0670)	(0.0805)	(0.0562)	(0.110)
	2010	0.144*	-0.407***	0.0921*	-0.304***	0.233***	-0.262***	0.208***	-0.348***
		(0.0730)	(0.116)	(0.0500)	(0.0921)	(0.0746)	(0.0827)	(0.0583)	(0.0979)
Observations		612	612	612	612	588	588	588	588
R-squared		0.921	0.987	0.907	0.989	0.917	0.987	0.905	0.989

Notes: Dependent variable is the native employment population ratio; key independent variable is the immigrant share of the population. “Low” denotes a high school degree or less; “High” denotes some college or more education. “Married” denotes married with spouse present, “single” the opposite. All regressions weighted by the number of observations used to calculate the employment rate. Standard errors clustered on state. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 9: Immigration and native employment, by parent status

		WLS				IV			
		Low		High		Low		High	
		No kids	Kids	No kids	Kids	No kids	Kids	No kids	Kids
Male	1960	0.940***	0.642***	0.394**	0.167	0.806***	0.506***	0.238*	0.152
		(0.171)	(0.207)	(0.164)	(0.156)	(0.164)	(0.174)	(0.123)	(0.116)
	1970	0.520**	0.339	0.149	0.131	0.502**	0.214	-0.0721	0.132
		(0.234)	(0.229)	(0.213)	(0.169)	(0.224)	(0.209)	(0.176)	(0.148)
	1980	0.0503	0.0282	-0.0155	0.0147	0.00833	-0.0299	-0.143	0.0530
		(0.156)	(0.167)	(0.143)	(0.126)	(0.177)	(0.177)	(0.137)	(0.134)
	1990	0.0770	-0.0281	0.0544	-0.0302	-0.00840	-0.119	-0.0602	-0.0170
		(0.122)	(0.130)	(0.109)	(0.0890)	(0.124)	(0.133)	(0.0919)	(0.0933)
	2000	-0.0356	-0.0467	-0.0169	-0.0660	-0.103	-0.116	-0.101	-0.0634
		(0.0902)	(0.0957)	(0.0847)	(0.0755)	(0.0989)	(0.0945)	(0.0778)	(0.0744)
	2010	-0.00827	-0.0259	-0.000588	-0.0699	-0.00618	-0.0693	-0.0733	-0.0636
		(0.108)	(0.115)	(0.110)	(0.0772)	(0.123)	(0.112)	(0.106)	(0.0799)
Female – Male	1960	-0.0971	-0.910***	0.0367	-0.789***	0.0164	-0.836***	0.120	-0.944***
		(0.217)	(0.297)	(0.177)	(0.234)	(0.163)	(0.211)	(0.156)	(0.224)
	1970	0.0608	-1.106***	0.266*	-0.822***	0.219	-1.086***	0.464***	-1.054***
		(0.231)	(0.317)	(0.156)	(0.243)	(0.199)	(0.237)	(0.175)	(0.274)
	1980	0.123	-0.600**	0.0817	-0.532**	0.186	-0.691***	0.218*	-0.819***
		(0.160)	(0.267)	(0.0937)	(0.212)	(0.150)	(0.252)	(0.129)	(0.255)
	1990	0.0596	-0.484***	0.0388	-0.399***	0.101	-0.463***	0.106	-0.573***
		(0.102)	(0.147)	(0.0629)	(0.121)	(0.102)	(0.130)	(0.0896)	(0.165)
	2000	0.0286	-0.378***	0.0221	-0.311***	0.0638	-0.297***	0.0693	-0.387***
		(0.0832)	(0.124)	(0.0510)	(0.0962)	(0.0818)	(0.101)	(0.0711)	(0.110)
	2010	0.0167	-0.313**	0.0316	-0.288***	0.0620	-0.163*	0.101	-0.331***
		(0.0881)	(0.127)	(0.0606)	(0.0858)	(0.0840)	(0.0982)	(0.0723)	(0.0970)
Observations		612	612	612	612	588	588	588	588
R-squared		0.955	0.986	0.939	0.990	0.954	0.985	0.939	0.990

Notes: Dependent variable is the native employment population ratio; key independent variable is the immigrant share of the population. “Low” denotes a high school degree or less; “High” denotes some college or more education. “Kids” denotes at least one child present in the home, “no kids” the opposite. All regressions weighted by the number of observations used to calculate the employment rate. Standard errors clustered on state. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 10: Immigration and native wages, by marital status

		WLS				IV			
		Low		High		Low		High	
		Single	Married	Single	Married	Single	Married	Single	Married
Male	1960	4.003***	2.050***	0.693*	0.561	3.819***	1.967***	-0.450	-0.0305
		(1.111)	(0.721)	(0.391)	(0.338)	(1.008)	(0.596)	(0.672)	(0.493)
	1970	1.703	1.337*	0.354	1.003**	1.780	1.450**	-1.220	0.367
		(1.205)	(0.770)	(0.534)	(0.474)	(1.219)	(0.667)	(1.103)	(0.631)
	1980	0.367	0.287	-0.353	0.402	0.262	0.326	-1.924**	-0.242
		(0.738)	(0.527)	(0.393)	(0.272)	(0.888)	(0.531)	(0.910)	(0.538)
	1990	0.736	0.635	0.492**	0.742***	0.612	0.597	-0.497	0.328
		(0.565)	(0.380)	(0.229)	(0.200)	(0.619)	(0.419)	(0.523)	(0.308)
	2000	-0.00709	0.239	0.166	0.531***	-0.114	0.115	-0.606	0.172
		(0.434)	(0.316)	(0.180)	(0.157)	(0.484)	(0.279)	(0.428)	(0.251)
	2010	0.196	0.351	0.161	0.643***	0.218	0.343	-0.578	0.320
		(0.482)	(0.374)	(0.247)	(0.198)	(0.559)	(0.342)	(0.407)	(0.259)
Female – Male	1960	1.235***	1.202**	-0.0428	-1.004*	1.395***	0.952*	0.483**	-1.713***
		(0.389)	(0.552)	(0.225)	(0.520)	(0.346)	(0.489)	(0.205)	(0.555)
	1970	1.775***	0.540	-0.0518	-2.069***	1.748***	-0.113	0.561*	-3.105***
		(0.561)	(0.770)	(0.377)	(0.678)	(0.518)	(0.769)	(0.331)	(0.795)
	1980	1.294***	0.523	0.562**	-1.053**	1.298***	-0.00414	1.164***	-2.008***
		(0.359)	(0.484)	(0.251)	(0.412)	(0.398)	(0.548)	(0.243)	(0.620)
	1990	0.961***	0.465*	0.403***	-0.600**	1.039***	0.183	0.817***	-1.241***
		(0.182)	(0.273)	(0.142)	(0.260)	(0.181)	(0.397)	(0.164)	(0.465)
	2000	0.795***	0.344	0.313***	-0.473**	0.669***	0.179	0.557***	-0.914***
		(0.130)	(0.220)	(0.0878)	(0.199)	(0.182)	(0.304)	(0.0846)	(0.330)
	2010	0.773***	0.316	0.339***	-0.593***	0.639***	0.106	0.593***	-0.992***
		(0.188)	(0.260)	(0.106)	(0.208)	(0.226)	(0.305)	(0.0991)	(0.328)
Observations		612	612	612	612	588	588	588	588
R-squared		0.918	0.989	0.961	0.994	0.918	0.989	0.958	0.994

Notes: Dependent variable is the average native log annual wage; key independent variable is the immigrant share of the population. "Low" denotes a high school degree or less; "High" denotes some college or more education. "Married" denotes married with spouse present, "single" the opposite. All regressions weighted by the number of observations used to calculate the employment rate. Standard errors clustered on state. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 11: Immigration and native wages, by parent status

		WLS				IV			
		Low		High		Low		High	
		No kids	Kids	No kids	Kids	No kids	Kids	No kids	Kids
Male	1960	3.005***	1.878**	0.693*	0.561	2.731***	1.696***	-0.115	-0.450
		(1.076)	(0.776)	(0.391)	(0.338)	(0.885)	(0.627)	(0.593)	(0.537)
	1970	1.278	1.102	0.354	1.003**	1.117	1.069	-0.620	-0.375
		(1.098)	(0.836)	(0.534)	(0.474)	(1.014)	(0.712)	(0.986)	(0.677)
	1980	0.156	0.0606	-0.353	0.402	-0.0285	0.00383	-1.078	-0.637
		(0.722)	(0.587)	(0.393)	(0.272)	(0.759)	(0.570)	(0.778)	(0.579)
	1990	0.429	0.418	0.492**	0.742***	0.255	0.285	-0.0243	0.0219
		(0.537)	(0.420)	(0.229)	(0.200)	(0.533)	(0.437)	(0.443)	(0.340)
	2000	-0.156	0.0633	0.166	0.531***	-0.354	-0.0984	-0.208	-0.0725
		(0.444)	(0.355)	(0.180)	(0.157)	(0.418)	(0.302)	(0.364)	(0.281)
	2010	-0.0637	0.223	0.161	0.643***	-0.166	0.172	-0.220	0.0614
		(0.494)	(0.418)	(0.247)	(0.198)	(0.495)	(0.372)	(0.359)	(0.274)
Female – Male	1960	1.448***	1.100*	0.0347	-1.417**	1.561***	0.857*	0.263	-2.449***
		(0.511)	(0.597)	(0.199)	(0.565)	(0.353)	(0.510)	(0.170)	(0.716)
	1970	1.699**	0.335	-0.580	-2.231***	1.699***	-0.317	-0.0560	-3.907***
		(0.668)	(0.901)	(0.406)	(0.743)	(0.547)	(0.870)	(0.314)	(1.040)
	1980	1.345***	0.431	0.141	-1.289**	1.382***	-0.224	0.541**	-2.862***
		(0.408)	(0.613)	(0.228)	(0.526)	(0.360)	(0.683)	(0.221)	(0.891)
	1990	1.128***	0.477	0.176	-0.822***	1.174***	0.202	0.452***	-1.810***
		(0.212)	(0.315)	(0.131)	(0.305)	(0.227)	(0.415)	(0.153)	(0.612)
	2000	0.937***	0.271	0.144	-0.654***	0.876***	0.0542	0.293***	-1.343***
		(0.151)	(0.276)	(0.0896)	(0.230)	(0.203)	(0.348)	(0.0945)	(0.437)
	2010	0.703***	0.412	0.0763	-0.666***	0.621***	0.186	0.234**	-1.328***
		(0.204)	(0.321)	(0.0851)	(0.233)	(0.212)	(0.355)	(0.109)	(0.421)
Observations		612	612	612	612	588	588	588	588
R-squared		0.943	0.990	0.978	0.994	0.943	0.990	0.976	0.993

Notes: Dependent variable is the average native log annual wage; key independent variable is the immigrant share of the population. “Low” denotes a high school degree or less; “High” denotes some college or more education. “Kids” denotes at least one child present in the home, “no kids” the opposite. All regressions weighted by the number of observations used to calculate the employment rate. Standard errors clustered on state. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

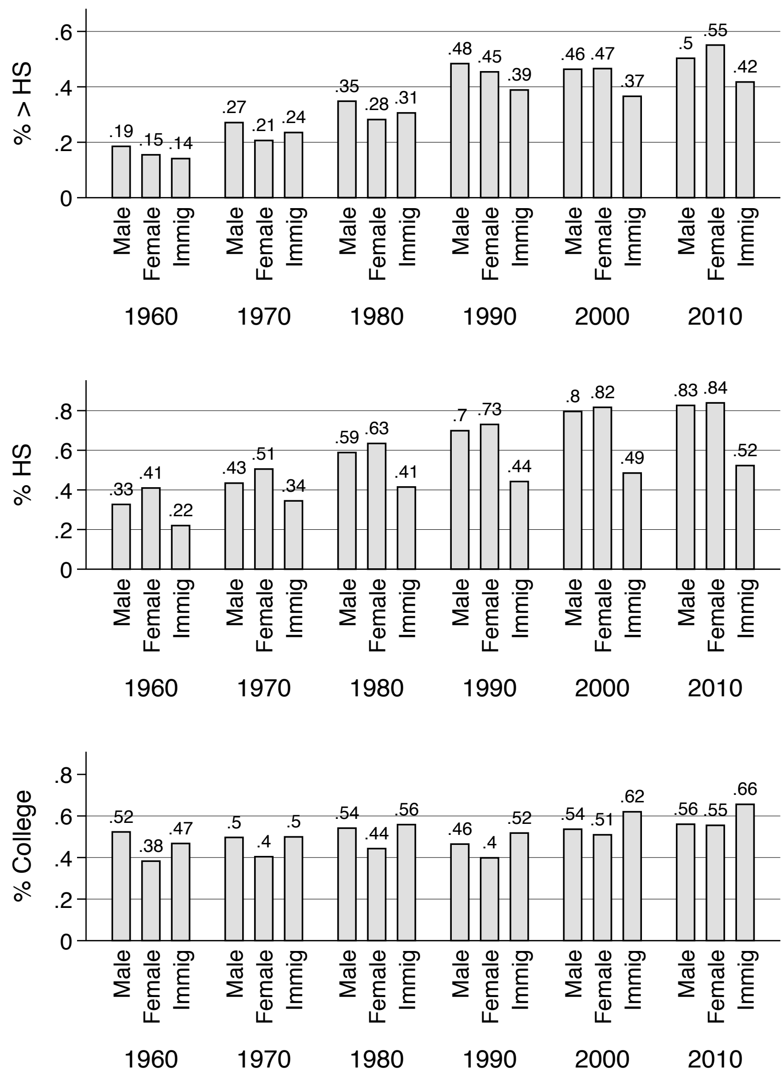

Figure 5: Education distributions

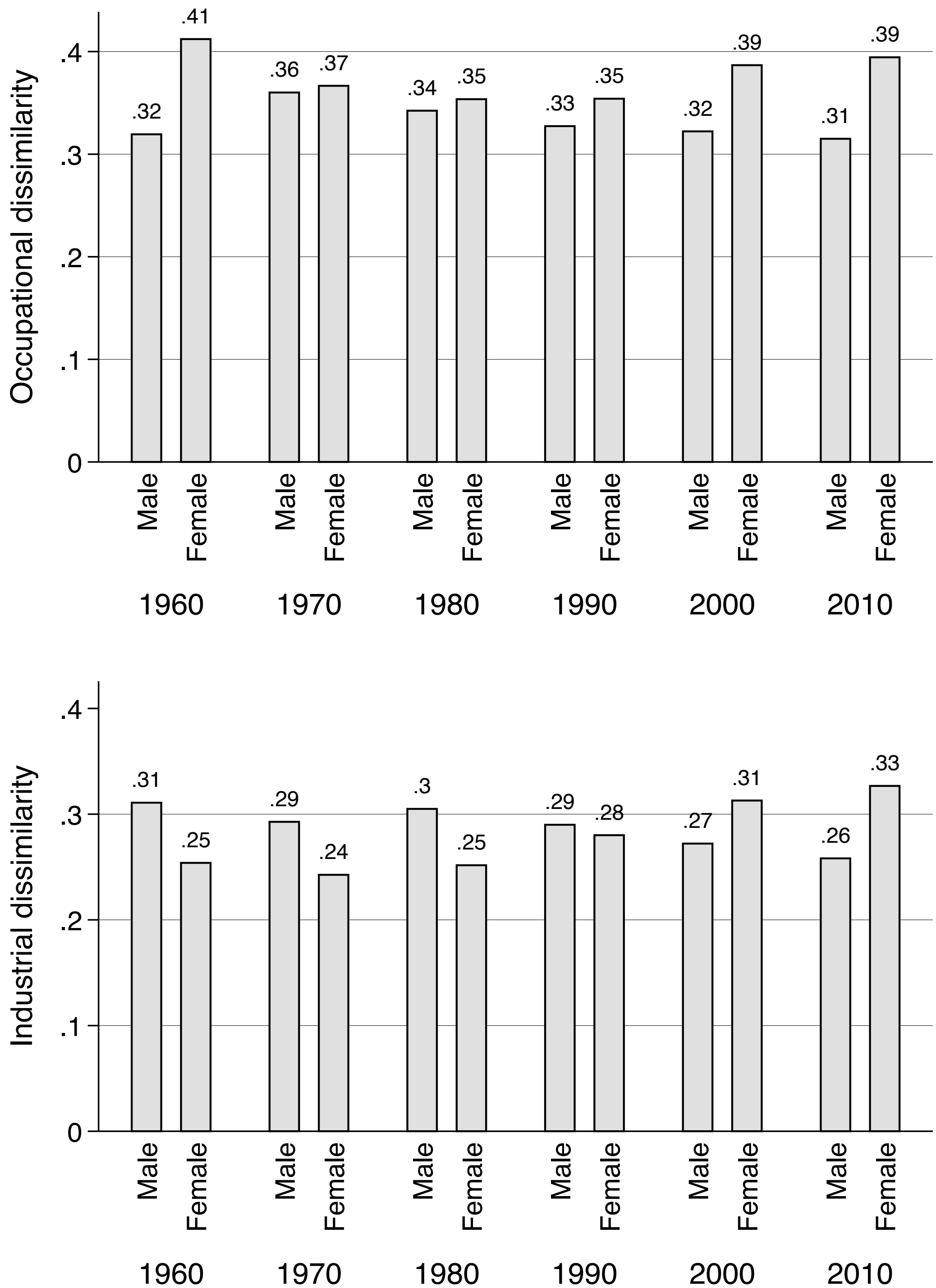

Figure 6: Gender-specific indices of dissimilarity between natives and immigrants

Table 12: Immigration and covariate-adjusted native wages, by marital status

		Education (IV)				Occupation and industry (IV)			
		Low		High		Low		High	
		Single	Married	Single	Married	Single	Married	Single	Married
Male	1960	3.876***	2.089***	-0.194	0.214	1.563**	0.740	-0.654	-0.226
		(0.996)	(0.601)	(0.570)	(0.390)	(0.777)	(0.496)	(0.532)	(0.363)
	1970	1.493	1.609**	-1.071	0.624	0.0471	0.473	-1.420	0.0745
		(1.202)	(0.669)	(0.868)	(0.493)	(0.976)	(0.568)	(0.869)	(0.495)
	1980	0.291	0.427	-1.649**	-0.0289	-0.818	-0.330	-1.679**	-0.341
		(0.858)	(0.531)	(0.747)	(0.426)	(0.709)	(0.432)	(0.711)	(0.424)
	1990	0.636	0.658	-0.403	0.407*	-0.148	0.185	-0.501	0.181
		(0.611)	(0.418)	(0.417)	(0.241)	(0.433)	(0.321)	(0.405)	(0.238)
	2000	-0.0760	0.175	-0.556	0.230	-0.580	-0.163	-0.600*	0.0636
		(0.464)	(0.277)	(0.350)	(0.198)	(0.363)	(0.220)	(0.336)	(0.191)
	2010	0.288	0.435	-0.567	0.362*	-0.251	0.0516	-0.598*	0.182
		(0.539)	(0.334)	(0.351)	(0.216)	(0.432)	(0.282)	(0.343)	(0.199)
Female – Male	1960	1.040***	0.961*	0.869***	-1.537***	-0.107	-0.572	0.882***	-1.383***
		(0.351)	(0.517)	(0.226)	(0.473)	(0.418)	(0.495)	(0.163)	(0.435)
	1970	1.421***	-0.122	1.188***	-2.923***	0.277	-1.180*	0.647*	-2.789***
		(0.528)	(0.804)	(0.401)	(0.717)	(0.552)	(0.714)	(0.376)	(0.632)
	1980	1.010**	-0.00165	1.369***	-1.833***	0.220	-0.758	0.752***	-1.940***
		(0.402)	(0.577)	(0.290)	(0.554)	(0.423)	(0.515)	(0.209)	(0.517)
	1990	0.838***	0.194	0.930***	-1.141***	0.200	-0.458	0.469***	-1.285***
		(0.201)	(0.424)	(0.197)	(0.419)	(0.293)	(0.436)	(0.140)	(0.389)
	2000	0.545***	0.206	0.656***	-0.825***	0.0945	-0.227	0.354***	-0.905***
		(0.209)	(0.320)	(0.113)	(0.285)	(0.276)	(0.313)	(0.0909)	(0.262)
	2010	0.542**	0.125	0.691***	-0.906***	0.0558	-0.296	0.443***	-0.914***
		(0.243)	(0.318)	(0.119)	(0.280)	(0.293)	(0.303)	(0.0986)	(0.252)
Observations		588	588	588	588	588	588	588	588
R-squared		0.791	0.837	0.867	0.911	0.799	0.848	0.869	0.901

Notes: Dependent variable is the state-fixed effect from a gender- and decade-specific regression of log annual wages on covariates and state fixed effects. In the panel labeled "Education," covariates are an indicator for having a high-school degree for the low skilled and an indicator for having a college degree for the high skilled. In the panel labelled "Occupation and industry," covariates are indicators for two-digit occupation and industry codes. Key independent variable is the immigrant share of the population. "Low" denotes a high school degree or less; "High" denotes some college or more education. "Married" denotes married with spouse present, "single" the opposite. All regressions estimated by IV, weighted by the number of observations used to estimate the dependent variable. Standard errors clustered on state. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 13: Native employment and gender-specific immigration

		WLS				IV			
		Low		High		Low		High	
		Male immig.	Fem. immig.	Male immig.	Fem. immig.	Male immig.	Fem. immig.	Male immig.	Fem. immig.
Male	1960	-5.440*	5.664**	-2.913**	3.163	1.756	-0.617	4.267	-2.299
		(3.233)	(2.507)	(1.411)	(3.016)	(13.82)	(8.115)	(8.286)	(4.851)
	1970	-4.721	5.987**	-2.785**	1.698	-5.807	4.344	3.433	-2.208
		(3.371)	(2.523)	(1.281)	(1.921)	(9.155)	(5.929)	(5.373)	(3.504)
	1980	-5.198**	2.004	-1.779*	1.169	-6.872	5.337	0.112	0.0537
		(2.122)	(2.018)	(1.000)	(1.193)	(8.696)	(6.416)	(5.620)	(4.185)
	1990	-0.842	2.155**	0.0913	1.071	2.443	-2.274	3.414	-2.750
		(1.130)	(0.852)	(0.496)	(0.780)	(8.405)	(6.626)	(5.004)	(3.899)
	2000	-1.471*	0.373	-0.109	0.386	-1.893	1.386	1.309	-1.107
		(0.838)	(0.595)	(0.400)	(0.439)	(4.700)	(3.705)	(2.540)	(1.956)
	2010	-1.621*	0.409	0.239	0.856**	-4.240	3.520	1.298	-1.099
		(0.949)	(0.784)	(0.390)	(0.397)	(4.267)	(3.432)	(2.230)	(1.746)
Female	1960	3.985*	-3.323**	2.066**	-2.110	11.13	-6.774	-4.708	2.214
		(2.018)	(1.622)	(0.902)	(1.943)	(12.99)	(7.600)	(11.43)	(6.564)
	1970	3.546	-4.515**	2.100**	-1.285	6.681	-4.968	-7.553	4.769
		(2.471)	(1.842)	(0.931)	(1.454)	(8.349)	(5.462)	(7.932)	(5.020)
	1980	4.002**	-1.885	1.379	-1.121	1.189	-1.252	-7.279	5.134
		(1.691)	(1.634)	(0.875)	(0.974)	(8.828)	(6.639)	(9.450)	(7.018)
	1990	0.567	-2.056***	-0.0978	-1.052*	7.907	-6.955	-2.720	1.650
		(0.876)	(0.661)	(0.409)	(0.600)	(7.928)	(6.254)	(8.567)	(6.686)
	2000	1.076	-0.598	0.0102	-0.532	0.0426	-0.318	-2.967	2.007
		(0.687)	(0.445)	(0.341)	(0.330)	(4.078)	(3.200)	(4.637)	(3.606)
	2010	1.260	-0.551	-0.271	-0.905***	-2.785	2.287	-2.791	1.981
		(0.836)	(0.640)	(0.373)	(0.314)	(3.438)	(2.781)	(3.697)	(2.886)
Observations		612		612		588		588	
R-squared		0.979		0.985		0.971		0.977	

Notes: Dependent variable is the native employment population ratio; key independent variables are the immigrant shares of the male and female populations. "Low" denotes a high school degree or less; "High" denotes some college or more education. All regressions weighted by the number of observations used to calculate the employment rate. Standard errors clustered on state. *** p<0.01, ** p<0.05, * p<0.1.

Appendix tables

Table 14: Immigration and native employment, overall effects

		WLS		IV	
		Low	High	Low	High
Male	1960	0.768***	0.243	0.607***	0.146
		(0.183)	(0.151)	(0.166)	(0.110)
	1970	0.409*	0.121	0.312	0.00178
		(0.234)	(0.182)	(0.216)	(0.151)
	1980	0.00854	-0.0218	-0.0635	-0.0734
		(0.165)	(0.130)	(0.177)	(0.127)
	1990	0.00775	-0.00436	-0.0995	-0.0604
		(0.126)	(0.0949)	(0.128)	(0.0852)
	2000	-0.0479	-0.0547	-0.131	-0.0977
		(0.0934)	(0.0772)	(0.0970)	(0.0719)
	2010	-0.0251	-0.0487	-0.0548	-0.0845
		(0.113)	(0.0931)	(0.120)	(0.0910)
Female	1960	0.142	-0.163	0.0942	-0.310**
		(0.199)	(0.141)	(0.202)	(0.154)
	1970	-0.255	-0.120	-0.255	-0.223
		(0.209)	(0.134)	(0.244)	(0.155)
	1980	-0.303*	-0.204**	-0.392*	-0.337***
		(0.166)	(0.0883)	(0.206)	(0.126)
	1990	-0.276***	-0.176***	-0.342***	-0.298***
		(0.0876)	(0.0575)	(0.121)	(0.0872)
	2000	-0.275***	-0.199***	-0.283***	-0.269***
		(0.0673)	(0.0435)	(0.103)	(0.0681)
	2010	-0.214***	-0.181***	-0.131	-0.216***
		(0.0755)	(0.0527)	(0.117)	(0.0744)
Observations		612	612	588	588
R-squared		0.978	0.984	0.977	0.983

Notes: Dependent variable is the native employment population ratio; key independent variable is the immigrant fraction of employment. "Low" denotes a high school degree or less; "High" denotes some college or more education. All regressions weighted by the number of observations used to calculate the employment rate. Standard errors clustered on state. *** p<0.01, ** p<0.05, * p<0.1.

Table 15: First-stage regressions

	Immig.×1960	Immig.×1970	Immig.×1980	Immig.×1990	Immig.×1990	Immig.×2010
Predicted immig. 1960	0.784***	-0.0655	0.0788	0.0999	0.0737	0.0114
	(0.0912)	(0.0415)	(0.0575)	(0.0979)	(0.179)	(0.104)
Predicted immig. 1970	-0.258**	0.827***	0.0927	0.117	0.0821	0.00495
	(0.112)	(0.0783)	(0.0624)	(0.114)	(0.224)	(0.133)
Predicted immig. 1980	-0.219***	-0.0764*	1.001***	0.0633	0.0321	-0.0186
	(0.0821)	(0.0450)	(0.202)	(0.0655)	(0.154)	(0.0949)
Predicted immig. 1990	-0.155***	-0.0536*	0.0328	0.955***	0.0328	-0.00673
	(0.0575)	(0.0319)	(0.0287)	(0.263)	(0.0999)	(0.0620)
Predicted immig. 2000	-0.0909**	-0.0318	0.0169	0.0263	0.729***	-0.00316
	(0.0356)	(0.0194)	(0.0177)	(0.0225)	(0.196)	(0.0332)
Predicted immig. 2010	-0.0736***	-0.0259*	0.0130	0.0210	0.0180	0.571***
	(0.0276)	(0.0156)	(0.0149)	(0.0176)	(0.0414)	(0.160)
Observations	294	294	294	294	294	294

Notes: First-stage regressions for less-educated females, based on 1940 shares. First-stage results for other groups are similar. Weighted by number of individuals used to compute employment rates and clustered on state. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 16: First-stage diagnostics

		1940 shares			
		Low		High	
		Statistic	p-value	Statistic	p-value
Male	F, (Immig.×1960)	200.92	0.0000	157.84	0.0000
	F, (Immig.×1970)	86.80	0.0000	52.87	0.0000
	F, (Immig.×1980)	11.55	0.0000	14.01	0.0000
	F, (Immig.×1990)	5.43	0.0002	6.00	0.0001
	F, (Immig.×2000)	6.70	0.0000	4.78	0.0007
	F, (Immig.×2010)	4.09	0.0022	6.13	0.0001
	Kleibergen-Paap LM stat	4.35	0.0369	3.99	0.0458
Female	F, (Immig.×1960)	208.93	0.0000	160.85	0.0000
	F, (Immig.×1970)	89.62	0.0000	54.52	0.0000
	F, (Immig.×1980)	16.30	0.0000	11.81	0.0000
	F, (Immig.×1990)	4.67	0.0003	10.25	0.0000
	F, (Immig.×2000)	6.79	0.0000	5.02	0.0005
	F, (Immig.×2010)	4.57	0.0010	7.03	0.0000
	Kleibergen-Paap LM stat	4.37	0.0365	4.07	0.0436
		Two-decade-lag shares			
		Low		High	
		Statistic	p-value	Statistic	p-value
Male	F, (Immig.×1960)	736.34	0.0000	347.22	0.0000
	F, (Immig.×1970)	204.97	0.0000	205.02	0.0000
	F, (Immig.×1980)	230.08	0.0000	1085.09	0.0000
	F, (Immig.×1990)	46.14	0.0000	350.86	0.0000
	F, (Immig.×2000)	133.15	0.0000	171.67	0.0000
	F, (Immig.×2010)	45.06	0.0000	53.37	0.0000
	Kleibergen-Paap LM stat	5.84	0.0156	5.38	0.0203
Female	F, (Immig.×1960)	731.59	0.0000	368.21	0.0000
	F, (Immig.×1970)	192.46	0.0000	222.76	0.0000
	F, (Immig.×1980)	201.42	0.0000	998.10	0.0000
	F, (Immig.×1990)	35.68	0.0000	484.08	0.0000
	F, (Immig.×2000)	136.60	0.0000	182.38	0.0000
	F, (Immig.×2010)	41.55	0.0000	53.27	0.0000
	Kleibergen-Paap LM stat	5.74	0.0166	5.50	0.0190

Notes: First-stage diagnostics for IV estimates of main panel. In the panel labeled "1940 shares," the instrument for immigration in year t is predicted immigration based on 1940 shares. In the panel labeled "1940 shares," the instrument is predictions based on shares in year $t-20$.

Table 17: First-stage regressions, MSA level

	Immig.×1960	Immig.×1970	Immig.×1980	Immig.×1990	Immig.×1990	Immig.×2010
Predicted immig. 1960	0.438***	-0.0110**	-0.0254	0.0254	-0.0237	-0.00899
	(0.0886)	(0.00434)	(0.0349)	(0.0410)	(0.0549)	(0.0420)
Predicted immig. 1970	-0.124***	0.329***	-0.0298	0.00391	-0.0156	-0.00311
	(0.0222)	(0.0288)	(0.0196)	(0.0273)	(0.0387)	(0.0329)
Predicted immig. 1980	-0.191***	-0.0229***	0.666***	-0.00758	-0.0111	0.00381
	(0.0356)	(0.00707)	(0.1000)	(0.0286)	(0.0528)	(0.0446)
Predicted immig. 1990	-0.289***	-0.0387***	-0.108***	1.539***	0.00758	0.0245
	(0.0630)	(0.0105)	(0.0409)	(0.254)	(0.0704)	(0.0648)
Predicted immig. 2000	-0.0845***	-0.0120***	-0.0410***	-0.0187	0.612***	0.00459
	(0.0299)	(0.00254)	(0.00799)	(0.0133)	(0.0636)	(0.0175)
Predicted immig. 2010	-0.0671***	-0.0101***	-0.0374***	-0.0223	-0.0115	0.529***
	(0.0244)	(0.00219)	(0.00676)	(0.0137)	(0.0112)	(0.0949)
Observations	978	978	978	978	978	978

Notes: First-stage regressions for less-educated females, based on 1940 shares. First-stage results for other groups are similar. Weighted by number of individuals used to compute employment rates and clustered on MSA. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 18: First-stage diagnostics, MSA-level

		Low		High	
		Statistic	p-value	Statistic	p-value
Male	F, (Immig.×1960)	15.09	0.0000	21.83	0.0000
	F, (Immig.×1970)	46.36	0.0000	68.70	0.0000
	F, (Immig.×1980)	155.84	0.0000	178.60	0.0000
	F, (Immig.×1990)	67.54	0.0000	119.72	0.0000
	F, (Immig.×2000)	19.02	0.0000	40.54	0.0000
	F, (Immig.×2010)	24.24	0.0000	31.34	0.0000
	Kleibergen-Paap LM stat	8.35	0.0039	7.64	0.0057
Female	F, (Immig.×1960)	16.53	0.0000	21.35	0.0000
	F, (Immig.×1970)	47.17	0.0000	63.23	0.0000
	F, (Immig.×1980)	159.62	0.0000	152.67	0.0000
	F, (Immig.×1990)	63.93	0.0000	112.41	0.0000
	F, (Immig.×2000)	18.35	0.0000	43.99	0.0000
	F, (Immig.×2010)	24.00	0.0000	35.81	0.0000
	Kleibergen-Paap LM stat	8.25	0.0041	8.02	0.0046

Notes: First-stage diagnostics for IV estimates of main panel, based on two-decade lag immigrant shares.