NBER WORKING PAPER SERIES

IMMIGRATION AND INEQUALITY IN THE NEXT GENERATION

Mark Borgschulte
Heepyung Cho
Darren Lubotsky
Jonathan L. Rothbaum

Working Paper 33961
http://www.nber.org/papers/w33961

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2025

We thank David Card, Ronald Lee, and Giovanni Peri, as well as audiences and discussants at NBER Cohort Studies, NBER Immigration and the U.S. Economy, Opportunity Insights, Southern Economics Association Annual Meetings, Society of Labor Economists Annual Meetings, Stanford, Census Research Data Center Conference, Syracuse, Berkeley, Davis, and University of Illinois at Chicago. Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau or the National Bureau of Economic Research. The authors have no financial relationships that bear on this research. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product (Data Management System (DMS) numbers: P-7503840 (P-7529415), Disclosure Review Board (DRB) approval number: CBDRB-FY22-SEHSD003-002, CBDRB-FY23-SEHSD003-005, CBDRB FY23 0513, CBDRB-FY24-0220, CBDRB-FY25-0073, and CBDRB FY25 0249). The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Immigration and Inequality in the Next Generation
Mark Borgschulte, Heepyung Cho, Darren Lubotsky, and Jonathan L. Rothbaum
NBER Working Paper No. 33961
June 2025
JEL No. J15, J31, J62

ABSTRACT

We estimate the causal impacts of immigration to U.S. cities on the intergenerational economic mobility of children of U.S.-born parents. Immigration raises the educational attainment and earnings among individuals who grew up in poorer households and reduces the earnings, educational attainment, and employment among those who grew up in more affluent households. On net, immigration diminishes the link between parents' and their children's economic outcomes in the receiving population, and thus increases intergenerational mobility. The increase in mobility is strikingly similar in models estimated across cities and in within-city models that control for the trajectories of immigrant destinations.

Mark Borgschulte
University of Illinois Urbana-Champaign
markborg@illinois.edu

Heepyung Cho
Korea University
heepyungcho@gmail.com

Darren Lubotsky
University of Illinois Chicago
Department of Economics
and NBER
lubotsky@uic.edu

Jonathan L. Rothbaum
U.S. Census Bureau
jonathan.l.rothbaum@census.gov

1 Introduction

The United States experienced historic levels of immigration during the latter part of the 20th century. A generation later immigration remains among the most contentious social, political, and economic phenomena in the United States. There is still basic disagreement among scholars about the impact of immigration on the economy, inequality, and on many choices that families make. Some of the most foundational questions, such as the long-run impacts of immigration in the modern economy, remain open due the lack of data. Given the demographic patterns of declining birth rates and populations in the richest areas of the world, the long-run effects of immigration on the receiving population will only grow in importance over the coming century.

In this paper we study how growing up in a city with a significant increase in immigration affects the labor market outcomes of native-born children. We focus on outcomes of children born between 1977 and 1985, a group that are now prime working-age adults and of particular interest because they were born at the start of a uniquely large and sharp increase in both immigration and labor market inequality. To understand the labor market effects, we further investigate educational attainment, geographic mobility, marriage, and fertility. Our analysis leverages administrative data to link over 23 million children born in these cohorts to their and their parents' tax records and Census survey data. The timing of immigrant arrivals over the course of the 20th Century and their relation to our data are depicted in Figure 1. The 1977 to 1985 cohort, in shaded red, is born at the start of the sharp increase in the immigrant population.

A primary focus of our work is how immigration affects intergenerational inequality. Economic theory predicts the causal effect of immigration on children's later-life outcomes will differ based on the exposure of the parents in the labor market, resources of the family, and other forces that can vary dramatically between rich and poor families. Immigration may lead to differential residential segregation, school quality, or incentives to acquire human capital. Immigration may therefore alter the relationship between parents' and children's adult incomes and alter the persistence of economic success from one generation to the next. We study this by estimating the effect of immigration separately at each decile of parental income. This is a crucial lens through which to examine the impact of immigration because of the widespread agreement that equality of opportunity is an important societal goal. We also estimate effects separately by race and sex. These are important because of a long-standing interest in understanding the sources of race and sex earnings differentials and whether immigration disproportionately affects the outcomes of Black individuals.[1]

Our study broadens the existing literature in several important ways. Leading studies of the economic impacts of contemporary immigration in the United States focus on average, concurrent outcomes at the level of the labor market, for example, by estimating the effect of the arrival of immigrants on the average local wage level of the native-born.[2] These studies generally find small effects of immigration on wage levels and wage inequality, though, this literature is contentious and other scholars, such as in Borjas (2003), have argued that immigration has more substantive effects on contemporaneous wage levels. Our project makes the point that the narrow focus on contemporaneous effects likely misses many important effects of immigration on natives. These effects play out over decades and have largely not been studied.[3] Our study also focuses on causal effects on people, rather than effects on places. This distinction is important because families relocate to different parts of the country, perhaps in response to immigration. We also study education and employment margins that are largely not included in the other studies.

Our work is closely aligned with a new and growing set of studies that look to the 19th and early 20th centuries to better understand the long-run effects of immigration. These studies generally relate immigration to specific locales in the United States, such as counties, to economic outcomes in these places measured decades later. A range of mechanisms beyond labor market competition, such as innovation and productivity, changes in marriage and families, and investments in human capital are often found to determine the long-run effects on income.[4] The focus on our study is

[1]For example, Hammermesh and Bean (1998).

[2]See, for example Altonji and Card (1991); Card (2009); Ottaviano and Peri (2012). Dustmann, Schönberg and Stuhler (2016) provides an overview of the recent literature on immigrant wage impacts.

[3]One exception is Hunt (2017), who estimates that increased immigration has a positive, but small effect on high school graduation rates among both White and Black native-born individuals.

[4]For studies in this vein see, for example, Sequeira, Nunn and Qian (2019); Long et al. (2024); Carlana and Tabellini (Forthcoming), as well as those surveyed in Abramitzky and Boustan (2017) and Tabellini (2022).

somewhat different; in particular, our data allows us to estimate causal effects on individuals, not on places, and on mobility from one generation to the next. A central contribution of the our paper is thus to extend this thread to understand the effect of immigration in the midst of the vast changes in the economy in the second half of the 20th Century.

Our interest in the effect of immigration on income mobility is also unique and represents a novel contribution to understanding the causal effects that underlie the transmission of inequality across generations. A burgeoning literature on intergenerational mobility studies the causal effects of policies and other early-life events on long-run and intergenerational outcomes; these studies generally focus on effects on children who grow up in lower-income families.[5] An important contribution of our work is a broader assessment of how immigration affects families throughout the income distribution and therefore speaks to the society-wide determination of economic mobility.

This project is made possible by our access to unique administrative records, available through the U.S. Census Bureau. In particular, we utilize data from IRS tax records, including information from 1040s and W-2, that allow us to link parents to their children claimed as dependents. Tax records contain information on labor market earnings for both generations, as well as other information, such as location of residence and marital status. We further link these records to restricted-access data from the 2000 and 2010 Decennial Census and 2000-2019 American Community Survey, which includes information on race, ethnicity, educational attainment, among other things, and to the Social Security Administration Numident file, which records individuals' county of birth. This data allows us to relate immigration in a person's birthplace to their adult outcomes using large samples. This data source is perhaps the largest and most comprehensive data source available for studying the impact of immigration and allows us to answer a broad set of questions that have thus far been difficult to address.

Our research design leverages the large and geographically diverse increase in immigration that came after a several decade pause. We estimate cross-sectional models that relate individual-level outcomes, such as the rank in national income distribution, to immigrant inflows during the 1980s

[5] See, for example, Chetty and Hendren (2018) and East et al. (2023).

to the Commuting Zone (CZ) where the individual was born. A key empirical challenge is that immigrants may move to places that offer better (or worse) outcomes for local native-born children (Abramitzky et al., 2021). We address this in three ways. First, we instrument immigrant inflows using a shift-share instrument that captures flows to a CZ predicted by the existing stock of immigrants from various origin countries. An important recent literature has clarified the identification conditions underlying such shift-share instruments and we so pay particular attention to assessing and validating our design. Second, to further strengthen our design, we control for lagged measures of city-level income mobility measured from children born between 1921 and 1940, whose incomes are measured in tax data from the 1970s (highlighted in blue in Figure 1). Third, we estimate CZ fixed effects models that directly addresses the primary threats to the validity of the other approaches, in particular, the cities with enclaves at the start of the era of high immigration have ex ante differences that predict future growth. Our main results on the increase in mobility due to immigration are quite robust across these and other specification checks.[6]

Our key conclusion is that immigration tends to raise the incomes of individuals born in poorer families and reduce the incomes of those born in richer families. The net effect is to increase intergenerational mobility. A 10 percentage point difference in the immigrant inflow rate during the 1980s, which is roughly the difference between that in Philadelphia and San Francisco, is associated with a 1.5 percentage point increase in the income rank of those whose parents were at the 3rd-decile of the distribution of parental income, and a 1.4 percentage point decrease in the rank of natives whose parents were in the 8th decile. These effects vary nearly linearly across the income distribution, with the largest positive effects in the lowest decile and largest negative effects in the highest decile. The effect on average income across the whole population is small and statistically indistinguishable from zero. We run our empirical model separately for Black men, Black women, White men, and White women and find only modest heterogeneity. The similarity in effects across Blacks and Whites, despite notable residential segregation, helps guide

[6] In particular, the robustness to CZ fixed effects and controls for lagged mobility address concerns highlighted in Burchardi et al. (2020) that persistent productivity shocks and other unobserved factors specific to that CZ may bias shift-share research designs.

our discussion of mechanisms below. Taken together, these results indicate that immigration tends to lessen the correlation between parents' and their children's earnings.

We find that education responses mirror the responses in income, indicating that mechanisms in childhood play a crucial role in determining the later life effects on income. A 10-percentage-point increase in the immigrant inflow rate raises the probability of completing high school among children from 3rd decile families by 2.2 percentage points, but has no effect on high school completion among children from richer families. The same immigrant inflow, however, reduces the probability of a child from an 8th decile family completing college by 3.5 percentage points. In contrast, migration responses moderate the effects we find, as poor families tend to move to low-mobility areas when they out-migrate, while wealthy families tend to migrate to high-mobility places. We conclude the paper with an analysis of how responses in education and employment can explain the main results on income.

2 Theoretical framework

Our study examines how immigration during childhood affects social and economic outcomes as an adult. There are a number of potential mechanisms through which immigration could affect these longer-term outcomes. These can broadly be classified into those that operate through parental income and labor market opportunities, through children's human capital accumulation, through internal migration, and through a variety of community-level changes. Some of these have been empirically studied, though their long-run implications remain unclear.

Immigration may affect contemporaneous wages and employment opportunities and thereby raise or lower the income available to parents to invest in their children. The importance of this channel depends on how immigration affects the labor market. Perhaps surprisingly given the contentious debate on the effect of immigrants on native labor markets, the most basic model of immigration—the *capital-labor* model—predicts that immigrants will have small and only temporary effects on native labor market outcomes. This prediction stems from the strong economic intuition that capital flows should offset long-run aggregate changes in labor supply; stated plainly, firms will increase their investment and production commensurate with the local workforce's productive capacity. Thus, aside from short-run adjustments, the primary effects of immigration on natives are thought to work through changes in the relative wages of high- and low-skilled labor.

The associated empirical literature is vast and one clear conclusion from it is that wage effects are heterogeneous, with potentially positive effects of immigration on higher-skilled individuals and negative effects on the lowest-skilled.[7] Negative effects on the lowest-wage native-born are consistent with labor market competition with the largely lower-skilled immigration in this era. Negative wage effects on lower-income households are particularly concerning since resource constraints in these families may be more likely to bind and therefore affect child outcomes, for example, as emphasized in the literature on the long-run effects of public programs.[8]. This mechanism would have the effect of increasing intergenerational inequality.[9]

Another potentially important mechanism is that immigration can lead to residential relocation among native-born families. In particular, immigration may induce "White flight," whereby White and wealthier residents move to different neighborhoods within a city, a different city, or send children to private schools in response to immigration.[10] This changes the composition of children in neighborhood public schools and perhaps alters overall levels of school funding.[11] This type of neighborhood sorting may have heterogeneous effects based on parent background, which further motivates our desire to study the heterogeneous impacts of immigration.

Immigration may alter the character of cities in other ways. Housing prices may increase (Saiz, 2007). Immigration may influence future immigrant and native migration. Labor markets may become thicker or more dynamic. Native-born individuals' preferences for spending on local public goods may change. There may be changes in neighborhood segregation, crime levels, diversity,

[7]See Borjas (2003) and studies cited in footnote 2.

[8]See, for example, Bailey et al. (2023)

[9]Research on intergenerational linkages in income is surveyed in Black and Devereux (2011).

[10]For example, Betts and Fairlie (2003) document the effect of immigration on native flight to private schools. Boustan, Cai and Tseng (2024) document White flight in response to Asian immigration in California. Evidence on the first-order effect of immigration on natives' internal migration is studied by Card and DiNardo (2000); Borjas (2006); Saiz and Wachter (2011).

[11]See Cascio and Lewis (2012); Figlio et al. (2023).

and social capital. The characteristics of people a child grows up with may change. As very little work has directly assessed the effect of immigration on later-life outcomes, many of these potential mechanisms remain unexplored.

Finally, immigration may affect the perceived costs and benefits of education for natives. Schools may become more crowded (e.g. Gould, Lavy and Paserman (2009)). To the extent that low-skilled immigration raises the relative return to skills, as suggested by theory, it may induce increases in human capital accumulation by the native-born. Hunt (2017) indicates that immigration raises the probability of native-born individuals completing high school. McHenry (2015) similarly shows that low-skilled immigration is associated with improved school performance and increased educational attainment.

3 Data

Our statistical analysis is based on individual-level data on the adult outcomes of individuals born in the United States between 1977 and 1985 to parents who were themselves born in the United States. We build our dataset from microdata on individual federal tax returns linked to the 2000 and 2010 Decennial Census, the American Community Survey (ACS), and the Social Security Administration (SSA) Numerical Identification System (or Numident). These data are described in Section 3.1. We construct our key treatment variable, immigrant inflows to U.S. commuting zones, using the 1980 and 1990 Decennial Census. This is described in Section 3.2. We further use data from the 1940 Decennial Census linked to tax return data from the 1970s to construct lagged measures of intergenerational income mobility for each commuting zones, from Rothbaum and Massey (2022) and discussed in Appendix A. Finally, we use 1940 through 2000 Decennial Censuses and 2008-2012 American Community Survey in construction of control variables and specification tests. These are described in Section 3.3.

3.1 Microdata on parents and children

Our analysis sample consists of individuals born in the United States between 1977 and 1985. We construct a dataset containing information on their parents' income, their own income when they reach adulthood, their educational attainment, their commuting zone of birth, their commuting zone of residence when aged 16 to 18, and demographic information. We exclude individuals who have a foreign-born parent since the effects of immigration on immigrant families living in the United States may be different and is worthy of its own separate analysis.

To construct our dataset, we use Form 1040 tax returns from 1994, 1995, and 1998 through 2020 to create parent-child pairs by assigning to each dependent child the parent(s) that claimed the child on their tax return when the child was 16 years old or the closest available age up to 18. We then link individuals identified from the 1040 tax returns to their records in the Decennial Censuses, the American Community Survey (ACS), and the Social Security Administration (SSA) Numerical Identification System (or Numident).[12] The Numident contains information on the date and place of birth provided by each individual when they file for a Social Security Number (SSN). We used the crosswalk from Black et al. (2015) that matches the strings in the Numident place and state fields to counties to identify the county of birth of each linked individual, both children and their parents, which allows us to assign individuals as either native- or foreign-born and, for the native-born, to their birth Commuting Zones (CZs).[13]

We draw demographic information from the 2000 and 2010 Decennial Censuses and the 2001-2019 American Community Surveys (ACS). We observe location, sex, race, and ethnicity in these

[12] This linkage relies on Person Identification Keys (PIKs), assigned by the Census Bureau. PIKs are assigned by a probabilistic matching algorithm that compares characteristics of records in census and survey data to characteristics of records in a reference file constructed from the Social Security Administration (SSA) Numerical Identification System (or Numident) as well as other federal administrative data. The Numident is a record of every Social Security Number (SSN) ever issued by the SSA. Once assigned, the PIK uniquely identifies a particular person and is consistent for that person over time (PIKs correspond one-to-one with a specific SSN), allowing us to link individuals across other de-identified data sources using only the PIK. See Wagner and Layne (2014) for more information regarding the PIKing process.

[13] We estimate that our data contain information on about 82 percent of the full 1977 through 1985 birth cohort. We drop individuals whose birthplace in the Numident is unclear. Taylor, Stuart and Bailey (2016) estimate that about five percent of individual's birth places are unclear. We also are missing data on a small number of children whose parents never file tax returns. As noted, we also drop individuals born abroad or whose parents were born abroad.

files. We assign race and ethnicity based on responses to the 2010 census, if available, filling in missing data from the 2000 census next and, last, the ACS, if needed. From the 2000 longform (for parents) and the ACS, we also observe their educational attainment as adults and their self-reported business (self-employment) income.

Parental income is measured as Total Money Income (TMI), with which we intend to capture the resources available in the household, and more broadly, the socioeconomic status of the family. TMI is a field constructed for the Census Bureau by the Internal Revenue Service that includes wage and salary earnings, total interest (taxable and non-taxable), taxable dividends, alimony, business income, net rental income, income from royalties and estates and trusts, and farm income, and total pensions and annuities reported on Form 1040.[14] We create a summary measure of parental income as the average TMI across the years 1998-2003 to proxy for parents' long-run income. This measure of parental income is similar to that in recent work on intergenerational mobility (e.g. Chetty et al. (2014)).

Children's income in adulthood is measured from wage and salary earnings from the Form W-2 filings from 2014 to 2020, when the individuals were between the ages of 29 and 43. In additional analyses, we also study the children's Total Money Income, which includes other sources of income not reported on a W-2 and accords with the measure of parental income, as well as whether they have self-employment income.

As in Chetty et al. (2014) and other recent work on intergenerational mobility, we focus on parents' and children's income ranks. Specifically, we take the set of all parent-child pairs, weighted by the number of children, and rank parents against all other parents; similarly, children are ranked against all other children. We also provide estimates of the effects of immigration on log income. In all measures, we inflation adjust income in each year using the R-CPI-U-RS, as was the case official Census Bureau income publications until 2023 (Semega and Kollar 2022 and Guzman and

[14]For each parent who files jointly, we assign to them half of the TMI from their tax return. This does not affect parents who file jointly together (i.e. they are still married) as the total TMI for the couple is then equal to the TMI on their return. However for parents who file separately (for example, if they got divorced), the TMI would be the sum of half of the TMI's from their separate, jointly filed returns. Due to a change to the 1040 form, total pensions and annuity income was not available separately in 2018 and is therefore not included in TMI in that year.

Kollar 2023).

3.2 Data on immigrant inflows

Our key treatment variable is immigrant inflows into a commuting zone between 1980 and 1990. We measure this flow from the 1980 and 1990 Decennial Census files from the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al., 2022). Immigrants are defined as anyone who reports having been born abroad. Our instrumental variable relies on the geographic distribution of the stock of immigrants, separately by country of origin, across U.S. commuting zones in 1970 or 1980, which we measure in the 1970 and 1980 Decennial Census files. We separate immigrants into 37 countries and regions of origins. We include each country of origin with at least 4,000 observations in the 1980 census. All remaining countries are merged into the following regions: Rest of Americas, Western and Southern Europe, Eastern Europe, Southeast Asia, Rest of Asia, Africa, and Oceania.

We focus on a commuting-zone-level analysis throughout the paper, where commuting zones represent local labor markets in the United States and encompass both metropolitan and non-metropolitan areas. To generate the commuting-zone level variables, we use the crosswalk between PUMA (Public Use Microdata Areas) in the Census and commuting zones by Autor and Dorn (2013).[15] We focus our analysis on the 214 commuting zones with population greater than two-hundred thousand in 1980, which covers 84 percent of the population.

3.3 Other data

An important innovation in our research design is the ability to control for a lagged measure of commuting zone intergenerational mobility. For our primary pre-period measure of intergenerational mobility in the commuting zone, we use data on the average income of children born between 1921 and 1940, who have parents in the 3rd and 8th decile of the 1940 national income distribu-

[15] Autor and Dorn (2013) provide the fractions of the population in the PUMA that belongs to each commuting zone. For individual observations in the microdata sample whose PUMA straddles commuting zones, we assign the observation to both commuting zones, weighted by this fraction.

tion, computed separately for Black and White children. These individuals were enumerated in the 1940 Decennial Census, which contains information on their race, geographic location, and parents' income. Their adult income is measured with linked tax returns from 1974 and 1979. Construction of this data is described in more detail in Rothbaum and Massey (2022).

Finally, we use the 1980 public-use Decennial Census to compute other covariates used below, including the logarithm of commuting zone population, the share of college graduates, and the share of workers employed in manufacturing. We use the 1940 through 2000 public-use Decennial Census and the 2008-2012 American Community Survey files to assess the validity of our research design, described in more detail in Section 5.4 below.

4 Research Design and Econometric Specification

We study the cross-sectional relationship between immigrant inflows to U.S. Commuting Zones and the economic mobility of individuals who were born there. A well-known empirical challenge is that immigrant inflows may be correlated with other factors that influence potential outcomes. For example, a local economic boom may both draw in immigrants and also raise the incomes of residents who live there. To surmount this, we rely on two strategies which we motivate before describing in detail within the econometric framework. We then describe a fixed effects approach which resolves the primary concerns with the other approaches at the cost of removing across-city variation.

Enclave instrument. In the first part of the identification strategy we adapt the shift-share instrumental variables strategy that uses variation in immigrant inflows to cities that are driven by the desire of new immigrants to live in cities that have larger communities of people from their home country, immigrant enclaves. The timing of our analysis sample's births around 1980 allows us to leverage the period of fastest growth in immigrant arrivals, when the identifying assumptions of the enclave instrument are most likely to hold, e.g. that the enclaves are small relative to the city and have no effect on outcomes other than through their effect on new arrivals. Figure 1 makes this point, showing how the 1977 to 1985 cohorts, in shaded red, are born during a surge in immigration that follows a long period of quiescence.[16] We view the confluence of historical forces as offering a unique opportunity to identify the long-run effect of immigration and providing particularly strong justification for the enclave instrument. The enclave-based pull is arguably unrelated to other contemporaneous influences on residents' outcomes, though we pay particularly close attention to validating this condition holds in our setting. The timing of our study aligns with canonical applications of this instrumental variables strategy that have used 1980 as the base period (as in Card (2009) and Goldsmith-Pinkham, Sorkin and Swift (2020)).

Controls for Economic Mobility in the Preceding Generation. In the second part of our identification strategy, our empirical models control for economic mobility of the preceding generation in the commuting zone of birth using new estimates of the income mobility of children born between 1921 and 1940 (Rothbaum and Massey (2022)). Administrative tax data in 1974 and 1979 provide adult income for these birth cohorts. Including these controls in our models can account for persistent and otherwise-unobserved differences in the economic prospects of children who grow up in different areas. Given our sample is born around 1980, these controls capture the mobility of the most recent cohorts of individuals to reach peak lifecycle earnings in the CZ of birth. Figure 1 depicts the relationship between these earlier cohorts, in blue, and the analysis sample, in red. The 1921 to 1940 birth cohorts grew up and entered the labor market in a period of very low immigration.

These controls may be particularly useful in accounting for expectations about local economic mobility that could influence both immigrants and the parents of 1977 to 1985 birth cohort. A fundamental concern with the enclave instrument is that larger enclaves are located in special places that one would expect to grow faster regardless of the arrival of additional immigrants. This could occur if immigrants are particularly forward-looking and move to high-performing places or if enclaves cause growth independent of the effect of new arrivals. Although extensive checks e.g.

[16]The immigrant inflow rate in Figure 1 is calculated as the change in the foreign-born population over a decade divided by the total population at the beginning of the decade. It is therefore influenced by the flow of new arrivals, emigration, birth and deaths, and changes in the native-born population size. For more information on the history of immigration to the United States over the 20th Century, see Abramitzky and Boustan (2017) and cites within.

in Goldsmith-Pinkham, Sorkin and Swift (2020) find little evidence of endogeneity, it is possible that immigrant enclaves endogenously form in areas of high or low economic mobility in ways that were not reflected in previous work. In our case, the controls for economic mobility in the preceding generation specifically address any long-run imbalance in the instrument, particularly for our primary outcome, adult income rank conditional on family background. The use of the two strategies together provides doubly-robust research design. We discuss threats to identification and alternative specifications in Section 5.[17]

Specification. We estimate cross-sectional models separately by parental income decile. Our specification builds on Card (2009) and is of the form

$$y_{ic} = \alpha + \beta \frac{M_{c,90} - M_{c,80}}{Pop_{c,80}} + X'_{c,80}\gamma + W'_{c,40}\delta + e_{ic} \tag{1}$$

where y_{ic} are the economic, educational, geographic, and demographic outcomes of native-born child i born in CZ c between 1977 and 1985. In particular, the primary measure of income we study is the individual's W2 income rank. We also report effects on whether the individual's income puts them in the top twenty percent of the national income distribution, the log of W2 income, whether they report any self-employment income, TMI rank, and the log of TMI. We also study two measures of employment, whether the person has W2 income greater than zero and greater than the income associated with full-time employment at the minimum wage. The measures of educational attainment we examine are whether the individual graduated high school and graduated college. Geographic mobility is measured two ways, capturing moves during childhood (up to age 16 to 18) and during adulthood. We also measure the quality of the destination CZ based on average W2 income rank of children born there who are of the same race and parental income decile. Finally, the demographic outcomes we study are whether the person is ever married, whether they have any children, and the number of children.

Our treatment variable, $(M_{c,90} - M_{c,80})/(Pop_{c,80})$, is the immigrant inflow rate to commuting

[17] We prefer this cross-sectional approach over a panel approach that would require strong assumptions, for example, about how the timing of immigration during a child's life affects their adult outcomes; and how to address the long pause and dramatic shifts in immigration and policy in the middle of the 20th Century.

zone c from 1980 to 1990. $M_{c,t}$ and $Pop_{c,t}$ are the number of foreign born and total population in commuting zone c at time t. The numerator captures the change in the total stock of immigrants in a CZ and is influenced both by new immigration to the United States, internal migration across CZs among the existing immigrant population, as well as births and deaths.[18] We also control for two sets of commuting zone characteristics. $X_{c,80}$ is a vector of commuting-zone-level control variables measured in 1980 and include the logarithm of commuting zone population, the share of college graduates, and the share of workers employed in manufacturing. An innovation of our research design is that we also control for the average income mobility of White and Black children born between 1921 and 1940 who have parents in the 3rd and 8th deciles of the 1940 parent earnings distribution, which are denoted by $W_{c,40}$. Standard errors are clustered at the CZ-level.

The central identification challenge to obtain consistent estimates from equation (1) is that unobservable commuting-zone characteristics may be correlated with both immigrant inflows and the adult economic outcomes of native children. Immigrants tend to move to disproportionately booming cities where both wages and prices are high, although less is known about how immigrants target the *long-run* economic trajectory of an area (Borjas, 2001; Albouy, Cho and Shappo, 2021). If immigrants are more likely to sort into commuting zones that are on a faster growth trajectory, OLS estimates of β capture a spurious relationship between the past immigrant inflows and the outcomes of native children who have continuously lived in their hometowns. Even if immigrants do not particularly target long-run growth, it is possible that short-run economic growth in the 1980s, which attracted immigrants at that time, also influenced investments in children's human capital, leading to biased estimates.

We use a shift-share instrumental variables strategy to address concerns about the non-random nature of immigrant inflows across commuting zones. The key idea behind this strategy is to isolate variation in immigrant inflows that are driven by immigrants' desire to live in cities that have existing enclaves of migrants from their home country. The variation in historical settlement

[18] We focus on the effect of the aggregate immigrant inflow rate to a CZ. Estimating separate effects of high- and low-skilled immigration is complicated by the fact that these flows are highly correlated with one another. Nevertheless, below we estimate effects excluding Mexican immigrants, who are the largest immigrant group and disproportionately low-educated. These results are largely similar to our preferred specification.

patterns is arguably unrelated to the contemporaneous shocks to labor demand or the prospective economic growth in the city, though we assess this important condition below. Versions of this type of network instrument have been used extensively in the immigration literature and other contexts and their statistical properties and assumptions have been rigorously studied in a series of recent papers.[19]

Formally, we define the enclave instrument as follows:

$$z_c = \sum_o \frac{M_{oc,80}}{M_{o,80}} \frac{M_{o,90} - M_{o,80}}{Pop_{c,80}} \tag{2}$$

where $M_{oc,80}/M_{o,80}$ is the *share* of immigrants from country o who live in commuting zone c in 1980. This captures the immigrant enclave. $M_{o,90} - M_{o,80}$ is the *growth* of the national number of immigrants from country o between 1980 and 1990. The instrument thus apportions the national flow of immigrants in the 1980s from each origin country, not to the commuting zones where they actually lived, but in proportion to how immigrants were dispersed in 1980.[20]

The validity of the instrument rests on the exogeneity of either the enclave shares, $M_{oc,80}/M_{o,80}$, or the inflows across countries, $M_{o,90} - M_{o,80}$, conditional on the covariates in the model. In our case, we rely on the exogeneity of the enclave shares and thus interpret our estimates as identified by the historical settlement patterns of immigrants from each country. For example, the inflow of migrants from Mexico disproportionately affects Los Angeles, which has a large Mexican enclave, while the inflow from China disproportionately affects New York and San Francisco, which have relatively larger enclaves of Chinese immigrants. Our identification assumption is that conditional on both 1940 mobility and the 1980 city characteristics ($W_{c,40}$ and $X_{c,40}$), the enclave shares are independent of other determinants of outcomes, e_{ic}. The lagged outcome control is particularly important because it implies that identification comes from whether there were larger

[19]The shift-share instrument was first suggested by Bartik (1991). Examples from the immigration literature include Altonji and Card (1991), Card (2001), Card (2009), Lewis (2011), and Monras (2020). Recent work that studies the shift-share instrument include Goldsmith-Pinkham, Sorkin and Swift (2020); Jaeger, Ruist and Stuhler (2018); Borusyak, Hull and Jaravel (2021).

[20]When constructing the enclaves, we include each country of origin with at least 4,000 observations in the 1980 census. All remaining countries are merged into the following regions: Rest of Americas, Western and Southern Europe, Eastern Europe, Southeast Asia, Rest of Asia, Africa, and Oceania.

changes in outcomes in cities that had greater exposure to the national immigrant inflow, similar to a differences-in-differences type of estimator.

We implement a number of tests of the validity of the instrument suggested in the recent literature (see footnote 19). For example, we follow Goldsmith-Pinkham, Sorkin and Swift (2020) and compute Rotemberg weights for each country of origin, which reflects the share of variation in the instrument attributable to each source country. The top three countries with the largest weights are Mexico, El Salvador, and the Philippines, with Mexico itself receiving more than one-half of the weight. This could pose a potential problem if, hypothetically, overall migration from Mexico is influenced by shocks to specific U.S. cities that have large Mexican enclaves. We thus assess the robustness of our results to the exclusion of Mexican immigration when constructing the enclave instrument. We further assess the instrument by examining pre-trends in outcomes in Section 5.4 below.

We make the largest modification to the primary specification by adding fixed effects for individuals' commuting zones of birth. To do so, we pool the data and include interactions between the deciles of parent income and the instrument. The fixed effects in this specification control for features of the CZ, such as the initial enclave shares, persistent productivity shocks, the level and composition of immigration, industry structure, and overall growth trajectory. This addition accounts for essentially all of the remaining sources of endogeneity; however, it comes at the cost of removing cross-CZ variation, meaning that we can no longer identify the effect of immigration that is shared by all deciles within a CZ. For this reason, we prefer the primary specification. In our reported results, we use the fifth decile as the reference group and report the effect of immigration on the other deciles as it relates to this group. Taken in conjunction with our tests for long-run mobility trends across CZs in Section 5.3, we view the robustness of the main results on mobility to the CZ fixed effects model as leaving little or no possibility of endogeneity in our main finding.

It is important to clarify a limitation of our cross-sectional design. Immigration may have effects on innovation and the national capital stock, and there may be other spillovers of immigration from one area of the United States on people who grow up in other areas. These forces would affect all people in our target birth cohort, regardless of where they grew up, and thus are not captured by β in Equation 1. Our data does allow us to follow individuals who migrate in response to immigration, addressing a chief criticism of the area studies literature where an ongoing concern is that inter-city migration attenuates the effect of immigration to a city on relative wages in that city. Similarly, concerns about capital dilution may be less applicable in our setting as capital is thought to be supplied elastically in the long-run.

5 Understanding and Assessing the Research Design

We conduct a number of analyses to assess the validity of our research design and highlight the implied comparisons within the empirical strategy. These analyses focus on the identification of plausibly exogenous inflows of immigrants to commuting zones using publicly-available census data and intergenerational mobility outcomes of the children of the 1940 Census.

5.1 First-stage relationship between predicted and actual immigrant inflows

Figure 2, Panel A is a graphical representation of the first stage relationship between actual immigrant inflows into a commuting zone during the 1980s and the enclave instrument. Each data point represents a commuting zone. The horizontal axis is the enclave instrument, calculated with equation (2). The vertical axis is the actual immigrant inflows to a commuting zone during the 1980s relative to the 1980 total population. The bivariate regression line has a slope of 1.06 with a standard error of 0.08. The pooled first-stage regression model, which includes the control variables in equation (1), has a coefficient of 1.05 with a standard error of 0.08. Appendix Table A1 reports the first stage coefficient by decile and decile by race; reassuringly, these estimates are indistinguishable from the pooled model. The instrument has strong predictive power and the first stage is estimated quite precisely.[21]

[21] Our instrument is also correlated with CZ-specific inflows between 1990 and 2000, with a coefficient of about 0.4, as we document in Figure A1. The instrument is not correlated with inflows after 2000. Identification arises from the enclave shares, meaning the cross-decade correlation does not bias our estimates or qualitative conclusions but does alter the interpretation of their scale Goldsmith-Pinkham, Sorkin and Swift (2020). Under the assumption that inflows

5.2 Matching-based interpretation of research design

We next provide a graphical analysis that illustrates the intuition behind our research design. A typical concern with cross-sectional analyses of immigration is that high-immigration cities, like Los Angeles and Miami, are different in many ways from low-immigration cities, like Pittsburgh. Here we demonstrate that there is substantial overlap in observable characteristics between cities with very different levels of predicted immigration.

Motivated by the standard matching strategy, we first regress the instrumental variable (the predicted immigrant inflow rate) on the covariates $X_{c,80}$ and $W_{c,40}$ from equation (1). We then obtain the predicted value of the instrument for each commuting zone based on these regression estimates. This index is a generalized propensity score for enclave-based immigration based on observable characteristics ($E(z \mid X_{c,80}, W_{c,40})$), and is represented on the horizontal axis of Panel B in Figure 2. The vertical axis is the value of the instrument for each commuting zone. The intuition of our estimator compares cities with similar observable characteristics but with different values of the instrument. For example, Philadelphia, Phoenix, Atlanta, Houston, Miami, and San Francisco all have an immigrant inflow index value of about 0.05, but have quite different values of the instrument and thus experienced different levels of "treatment." More generally, inspection of the other data points in Panel B of Figure 2 indicates that there is substantial variation in the value of the instrument among cities with similar observable characteristics, proxied by the index of immigrant inflows.

5.3 Correlation with Long-run Mobility

Perhaps the chief concern for our research design is that immigrants move to places with high mobility. This pattern of targeted migration is documented in Abramitzky et al. (2021) for immigrant arrivals between 1880 and 1940. To test for this potential bias, we examine the relationship between our enclave-based identification and the mobility estimates for the children in the 1940

in the 1990s, when our target birth cohort is older, have the same effect as inflows in the 1980s, our estimates should be scaled down by about 30% to capture purely the effect of immigration in the 1980s.

Census in Rothbaum and Massey (2022).

Figure 3 reports the results of this exercise. To construct the figure, we regress the adult income rank outcomes of children in the 1940 Census who live with parents in a particular income decile and CZ on immigrant inflows to that CZ in the 1980s instrumented with z_c. The gray "No Controls" line reports the IV regression without controls and reveals the pattern documented in earlier research. Immigrants move to places with high mobility for all children. Although the each decile is insignificant, a consistent positive relationship is evident. When we include our 1980 controls, the positive association between immigrant destinations and local mobility in the preceding generation disappears. Instead, the conditional relationship reveals small negative estimates for children born in the lowest deciles, and this relationship is not statistically significant. There is no relationship for children born to families above the third decile. From the point of view of the research design, the weak negative relationship for children in the lowest deciles has the opposite relationship of our main findings presented below, that children in the lowest deciles experience higher mobility as a result of immigration.

Based on this evidence, we conclude that the correlation between immigrant inflows and mobility in the preceding generations presents no concern for our estimation. Nevertheless, we include the controls for 1940 economic mobility in our estimation to address any remaining potential bias. A related concern that is more difficult to address is that cities that received larger immigrant inflows differ in systematic and unobserved ways not captured by the controls and are uncorrelated with mobility in the preceding generation. For robustness to this possibility, we rely on the within-city CZ fixed effects model that non-parametrically absorbs city-level differences.

5.4 Is the instrument correlated with trends in outcomes?

We next address a potential concern with our instrumental variables strategy that the immigrant enclaves that exist in 1980 capture other factors that affect the outcomes of the native-born, even conditional on our control variables in equation (1). We assess this concern by examining the CZ-level relationship between immigrant inflows between 1980 and 1990 and the trends in outcomes of the native-born in each decade between 1940 and 2010, using public-use Census files.[22] This analysis is in line with Goldsmith-Pinkham, Sorkin and Swift (2020), who test the plausibility of this enclave-based and other shift-share instruments by testing for parallel pretrends. The analysis is presented in Figure 4. Panel (a) in the upper-left shows the average income percentile rank of individuals aged 25 to 34, separately by quartile of the instrument (the predicted immigrant inflow) in the 1980s and by census year. In 1940, individuals in the CZs that had the highest predicted immigrant inflow were on average at the 60th percentile of the national income distribution, while individuals in CZs with the lowest predicted inflow were at the 49th percentile. That is, areas that had larger predicted inflows in the 1980s were places that had higher incomes in the 1940s. Inspection of the trend lines shows that this pattern continued through 1980, throughout the pre-period of our study.

In Panel (b) we assess how these differences are affected by the inclusion of our control variables. The vertical axis is a regression coefficient from a model of $\bar{y}_{ct} = a + b_t Z_c + u_{ct}$, where $\bar{y}_{ct}$ is the average income percentile of individuals aged 25 to 34 who live in commuting zone c in Census year t; Z_c is our instrument, the predicted immigrant inflow to commuting zone c during the 1980s. Estimates of b_t from this model are shown in the light grey line labeled "No Controls". These range from 0.5 to 1.0, again indicating that inflows are positively correlated with past income levels. The remaining two lines show estimates of b_t when we include the 1980, and 1940 and 1980, control variables ($X_{c,80}$ and $W_{c,40}$ above). The 1980 control variables are the logarithm of commuting zone population, the share of college graduates, and the share of workers employed in manufacturing. The 1940 variables are the average income of White and Black children born between 1921 and 1940 who have parents in the 3rd and 8th deciles of the 1940 parent earnings distribution. Conditional on these variables, there is no relationship between predicted inflows in the 1980s and the income of native-born individuals between 1940 and 1980.

The remaining panels of Figure 4 repeat this analysis for the employment rate and probability

[22] Ideally we would conduct this analysis by individuals' commuting zone of birth, to mirror our main research design. Public-use Census files contain state of birth and current place of residence. Our analysis here classifies individuals by their current CZ of residence. The results are similar if we classify people by their state of birth.

of graduating college. The results in Panel (c) indicate that differences in employment rates across quartiles of the instrument are small in magnitude, in particular relative to the overall growth in employment rates during this period. When we condition on our control variables in Panel (d), the relationship between the instrument and past employment rates shows a small positive relationship in the decades before 1980. For example, a 10 percentage point increase in predicted migration, such as that experienced by San Francisco, is associated with a small, two percentage point higher employment rate in 1960 through 1980. This positive relationship between employment and immigrant inflows is in contrast to our key findings, described in the next section, that these inflows are negatively related to employment rates in subsequent decades. Finally, the figures in Panels (e) and (f) indicate that immigration in the 1980s was concentrated in cities that had faster growth in the stock of college graduates. Once we condition on our 1980 control variables, however, there is no statistically significant relationship between the share of the native-born population with a college degree and immigrant inflows in the 1980s throughout the pre-period. For employment in particular, and possibly for college graduation, there is a change in the relationship at the time of treatment. Taken together, we interpret this evidence to indicate that the enclave instrument provides credible identifying variation in immigration shocks across CZs.

6 The Impact of Immigration on Intergenerational Mobility

In this section we present the main results on the causal effect of immigration on intergenerational mobility in labor market income and associated employment, educational attainment, and migration outcomes. We display the results in figures that show the effects of immigrant inflows on the average outcomes of all native-born individuals and then separately by decile of their parents' income. We pair these with figures that illustrate the impact of a 10 percentage point increase in the immigrant inflow rate (e.g. a large inflow in the sample, analogous to the difference between growing up in Pittsburgh versus San Francisco) on mean outcomes by parental income decile. We then break down the estimates by race and sex and also present additional results that probe the robustness of the estimates and utilize alternative measures of income. Finally, in Section 7 we assess the degree to which our findings on educational attainment, employment, and migration can explain the effects on income.

6.1 Immigration Effects on Wage and Salary Income

We begin with the effects of immigration on intergenerational mobility in wage and salary income, as reported on W2 returns, in Figure 5 and Table 1. Panel (a) in the upper-left of Figure 5 shows the IV estimates of the effect of immigrant inflows from 1980 to 1990 on the income rank of native-born children measured in their adulthood. The effect of immigrant inflow rate on the average income rank of all people is -0.07 with a standard error of 0.08, economically small and not statistically significant. The magnitude of this number implies that a person who grows up in a commuting zone that experienced a 10 percentage point immigration shock would have a 0.7 percentage point lower income rank (e.g. be at the rank of 49.3 rather than 50th percentile). This estimate is illustrated by the dashed horizontal line labeled "Avg."

This null average effect, however, hides a striking pattern. Immigration increases the rank of the native-born who have low-income parents (those in the 1st through 5th income decile), while it decreases the rank of the native-born who have high-income parents (those in the 6th through 10th income decile). Our key result that immigration tends to lessen the correlation between parent and children's income can be seen in panel (b) to the right. The dashed line shows the average adult income rank by parental income decile. For example, individuals who grew up in households in the bottom decile of the income distribution had adult incomes, on average, at the 36.9th percentile of the income distribution, while children who grew up in households in the top decile had adult incomes at 63.9th percentile. The solid line shows how these averages would change as a result of a 10 percentage point increase in immigration during the 1980s. The rank of children from the bottom decile would rise by about 1.5 points, to the 38.4th percentile. The rank of children from the top decile would fall by 2.4 points to the 61.5th percentile. Table 1 presents the point estimates and associated standard errors and, indeed, the effects in the upper and lower income deciles are statistically different from zero.[23]

The remaining panels of Figure 5 show similar analogous separately by sex and race. Point estimates and standard errors are in columns (2) through (5) of Table 1. These estimates are from subsamples that include non-Hispanic White and Black individuals; other groups are dropped. Child and parental income ranks reflect their standing within the national distribution.[24] First, we find the same basic pattern of results for each group: immigration tends lessen the correlation between parent and children's incomes. There are, however, some key differences in the point estimates across groups. The effects among Black individuals are generally attenuated relative to the effects among Whites, especially in the upper half of the income distribution. The rank-rank plots in panels (d) and (f) of Figure 5 mirror those in Chetty et al. (2019): Black men have considerably lower income ranks than White men, conditional on parental income ranks. Although immigration has the effect of flattening the relationship between parent and children's income ranks, especially among White men, it is clear that this effect is quite small relative to the large gap in income ranks between White and Black men.

Immigration also lessens the relationship between parent and children's income ranks among women, though the pattern is somewhat different than that among men. Among both White and Black individuals, the point estimates among women are smaller than those of men. Indeed, there is no effect of immigration on income ranks among women from poorer families and the negative effects are more pronounced among women who grew up in richer families. Like Chetty et al. (2019), we find that Black women have a slightly higher income rank, conditional on parental income, compared to White women. Immigration has the effect of reducing the income ranks among women from who grew up in richer families.

Our key result, that immigration tends to lessen the correlation in earnings between parents and their children, is robust to a variety of other ways to estimate this relationship. We explore

[23] Although the national income rank is zero-sum, so increases in rank must be balanced by decreases elsewhere, it is possible that all children who grow up in a city do better as a result of immigration.

[24] More precisely, the estimates do not reflect within-city ranks and include all race groups as well as immigrants in the national income distribution. Crucially, even though immigrants disproportionately fill in the lower ranks of the national income distribution, the resulting shift of the rank distribution affects all cities equally, and hence, does not explain or bias our results.

these in columns (6) through (10) of Table 1. Column (6) shows results in which the dependent variable is an indicator that the child's average adult income is in the top 20 percent of the national income distribution. Consistent with our earlier results, immigration raises this probability among those who grew up in poorer families. For example, nine percent of children who grow up in families in the third decile of the income distribution end up in the top 20 percent when they are an adult. Our estimates indicate that a 10 percentage point immigration shock raises this by two percentage points. The positive estimates among the bottom five deciles are large enough to be statistically different from zero; the point estimates for the top deciles are negative, consistent with immigration having a negative effect on earnings, but are not statistically different from zero.

In column (7) we provide estimates from models in which the dependent variable is the log of W2 income, rather than the rank in the national distribution. This transformation produces estimates in the spirit of the classic intergenerational elasticity literature Solon (1992). These estimates are generally positive and considerably larger among the lower-income deciles, which implies a similar flattening of the relationship between parent and children's incomes. As well, the effect of immigration on average log income is not statistically different from zero. So, for example, a 10 percentage point immigration shock raises the average W2 income of children who grew up in the bottom of the income distribution by about six percent, while there is no statistically significant relationship in the higher-income deciles. These results are consistent with our key conclusions from models of income ranks.

Our key conclusions regarding both a null effect on average income and an increase in intergenerational mobility are also not sensitive to the sources of income we include. Our measure of income used above come from W2 forms and therefore include wage and salary income, but exclude other sources of income, such as that from self-employment. We focus on W2 income because we are primarily interested in how immigration affects individual labor market earnings and opportunities. To address the shortcomings of this measure, we estimate models where the dependent variable is an indicator that the person had any self-employment income [column (8)], Total Money Income rank [column (9)], and the log of Total Money Income [column (10)]. The latter measures account for income from nearly all sources at the family level, including the financial effects of marriage.

Immigration tends to raise self-employment rates and this effect is more pronounced among individuals who grew up in richer families. For example, the coefficients in Column (8) are around 0.04 to 0.06 in lower deciles and rise to 0.09 to 0.13 in the top deciles. However, these effects have only modest effects on the main results; a 10 percentage point increase in immigration raises average self-employment rates of 6 percent by less than a percentage point in most deciles.

The effects of immigration on Total Money income rank and the log, shown in columns (9) and (10), are also quite similar to the analogous estimates for W2 ranks and logs. If anything, the positive effects of immigration on the incomes of people who grow up in low-income families are somewhat larger in these models, however, our key conclusion that immigration lessens the relationship between parent and children's incomes is not substantively altered. We return to document effects on marriage and dependents in Section 6.5.

In Tables 2 and 3 we explore the robustness of our main result to a variety of factors. The dependent variable in all models is the W2 income rank and all models are estimated using all people. Column (1) shows results using OLS rather our preferred IV strategy. The OLS estimates show the same basic pattern whereby immigration raises income ranks among individuals who grow up lower deciles, and lowers income among those from higher deciles. The OLS coefficients are generally about 0.06 smaller than the corresponding IV estimates, which implies that non-random sorting of immigrants to commuting zones biases OLS estimates downwards, though the practical difference between the OLS and IV estimates is small. Interestingly, these small differences are in the wrong direction for the primary bias concern, that immigrants differentially move to high-performing areas.

In columns (2) through (4) we return to the IV estimates, but vary the controls used in the model. First we estimate models with no controls; then we include the 1980 controls ($X_{c,80}$ in equation 1) but omit the 1940 controls ($W_{c,40}$); and finally in column (4) we return to our preferred specification that controls for $X_{c,80}$ and $W_{c,40}$, and also controls for the average income rank in 1940 of individuals aged 25 to 34 who live in that commuting zone. These estimates indicate that the conclusion that immigration has no effect on average incomes while increasing intergenerational mobility is insensitive to the control variables that we include in the model. When there are no controls in the model, the positive effect of immigration on the incomes of individuals who grew up in lower-income families is larger than in our preferred specification, and the effect on those from upper-income families is smaller. Columns (3) and (4) are almost identical to the main estimates in Table 1. The stability of the results suggests that endogeneity concerns about the long-run process of enclave formation have little empirical content in this setting.

In column (5) we estimate the model omitting Mexican immigrants entirely from the analysis. Thus they do not appear in the treatment variable or the instrument. This specification is important for three reasons. One, Mexican immigrants make up a disproportionate share of the total immigrant inflow. As Goldsmith-Pinkham, Sorkin and Swift (2020) note, an important robustness check is to assess how the estimate vary with the exclusion of groups with large Rotenberg weights. Two, it is also important to assess the sensitivity of the results to the exclusion of Mexican immigrants because the flow of Mexican immigrants during this period may be particularly mismeasured due to their uneven coverage in the 1980 and 1990 Decennial Censuses. And three, Mexican immigrants are considerably less-educated than the average immigrant. Excluding them thus alters the skill-composition of inflows, which allows us to assess the sensitivity to this aspect of the treatment. Although less precisely estimated, the estimates in column (5) are quite similar to our preferred estimates and show the same pattern that immigration tends to raise incomes of individuals born in lower-income families and lower the incomes those born in upper-income families with no effect on the average.

In column (6) we assess whether our estimates are sensitive to whether we adjust our income ranks for differences in the cost of living across cities. Our preferred estimates follow the literature and ignore differences in the cost of living across cities. Cost of living adjustments are fraught due to the joint determination of wages, rents and amenities.[25] Nevertheless, to probe how differences

[25]For example, Diamond (2016) finds that welfare gaps between high- and low-skilled workers actually exceed wage gaps when adjusting for wages, rents and amenities.

in costs of living across cities influences our estimates, we follow Diamond and Moretti (2024) and create a cost-of-living index for each commuting zone based on reported rents in the 2014 through 2018 American Community Survey.[26] We then control for this housing-based cost of living based on each individual's CZ of residence in 2019 (or earlier if they did not file a tax return in 2019). Adding this control to the model does not alter our key conclusion that immigration lessens the correlation in incomes between parents and children and thus increases intergenerational mobility. However, the addition of the COLA control has the effect of decreasing all coefficient estimates. So the positive effect of immigration on the income rank of individuals from poor families becomes indistinguishable from zero, while the negative effect on those from richer families more than doubles. The higher cost of living associated with immigrant arrivals is consistent with several mechanisms, such as immigrants increasing local amenities. It is also important to note that the education outcomes we document below are not influenced by cost-of-living considerations and are consistent with our results on income.

In the final column of Table 2, we report estimates from the CZ fixed effects model. The coefficients reflect the interaction between the parental decile and the instrument, omitting the fifth decile as the reference group. The downward slope across deciles is quite similar, if anything moderately steeper, than the effects estimated in the primary specifications in Table 1. The bottom row is empty because the inclusion of fixed effects means that we can no longer estimate an average effect across the whole population. We conclude that the increase in mobility due to immigration is robust to the geography of immigration and the wide range of associated identification concerns.

We next turn to the robustness of the enclave construction. A concern with our instrumental variables strategy is that enclaves that exist in 1980 may be correlated with unobserved, contemporaneous shocks to commuting zones that both affect immigrant inflows and the outcomes of families that live there. To address this, we assess the sensitivity of our estimates to measurement

[26] Specifically, we classify housing units based on the combination of the year the structure was built, whether it is a single-family or multi-family unit, the number of rooms, the number of bedrooms, and the presence or absence of key facilities, such as plumbing. We then calculate the mean rent for each type of housing unit among renters in each CZ. Our CZ cost-of-living index is the weighted average of rents for each housing unit type, weighted by the distribution of housing types of both renters and owners in each CZ.

of enclaves in 1970. These results are presented in column (1) of Table 3. These estimates are generally more positive in magnitude than the estimates based on the 1980 enclaves, but indicate the same pattern that immigration tends to raise the income ranks of individuals who grow up in poorer families and reduce the ranks of those from richer families, with the effect of increasing intergenerational mobility. The positive shift in outcomes indicates that any hypothetical endogeneity arising from contemporaneous shocks in the construction of the 1980 enclaves biases the main results towards a more negative effect of immigration, the opposite of traditional concerns in the enclave construction. Columns (2) and (3) show analogous results for educational outcomes, which we return to below.

Finally, in our results above we measure parental income as the average between 1998 and 2003. We choose this period because parents are mid-career and their income likely better corresponds to their average long-run income. The literature on intergenerational mobility has emphasized the value of measurement of both parent and child income in mid-life to address lifecycle bias (Mazumder 2005; Haider and Solon 2006). However, a natural concern is that the parental income decile measured in 1998-2003 is itself affected by immigration, so children who grow up in poor families according to this measure are disproportionately those negatively affected by immigration. We address this by re-estimating our key model stratifying individuals by parental income measured in 1979. The cost of this change in the measurement of parent income in the earlier time periods is that parents are younger and their income is therefore lower and presumably a more noisy measure of their long-run income. Estimates of the effect of immigration on W2 income ranks are presented in column (4) of Table 3. We note that more than ten percent of parents had no reported income or did not file taxes in 1979 and thus what we label as the second decile of parental income refers to the bottom 20 percent of family income.

The estimates from this exercise are consistent with our preferred estimates in column (1) of Table 1. The downward slope of estimates across deciles is clear and consistent with our preferred estimates. The coefficient estimates are generally more negative in magnitude than the analogous estimates in in Table 1, so the effects on the lower deciles are not statistically different from zero (or from our preferred estimates) while the effects on top deciles are somewhat larger. Thus we do not view our use of parental income measured between 1998 and 2003 as problematic or likely to influence our findings. Again, we return to columns (5) and (6) in Section 6.3.

Our key conclusion is that immigration in the 1980s had the effect of raising the adult earnings of individuals who grew up lower-income families and lowering the earnings of individuals who grew up in higher-income families. This reflects a "tilting" of the intergenerational relationship. The effect of immigration on average income among all people is near zero in most models. This result is robust to a variety of important specification checks. Importantly, the result is at odds with the view that immigration harms the economic fortunes of low-income native-born families and their children. Instead, it suggests that the impacts of immigration are more complex, are not limited to the effects of contemporaneous labor market competition, and that there are different mechanisms at play across the familial income distribution.

6.2 Employment

We next use our empirical model to estimate the effect of immigration on the adult employment rates of the children who were exposed to varying levels of immigration. The effects on W2 earnings reflect a combination of the effects on employment and the returns to work. We return in Section 7 and discuss the implications of the employment responses for the effects of immigration on income mobility.

The estimated effects of immigration on the employment rates of the native-born are presented in Table 4 and Figure 6. We use two measures of employment. Our key measure is the share of years during the ages of 29 to 43 when the individual had W2 earnings greater than what would be obtained from working full-time at the minimum wage. This measure captures a somewhat intense measure of employment since lower-wage, part-time workers will generally be classified as not being employed in a given year. IV estimates based on this measure of employment are presented in columns (1) through (5) of Table 4. Figure 6 graphs the IV estimates and their confidence intervals in the left side and simulated effects from a 10 percentage point increase in immigration on the right. Our second measure is the share of years when the individual had any earnings in a given year. Estimates from this model are presented in column (6) of Table 4.

Consistent with our income estimates, the effect of immigration on share of years with positive employment is heterogeneous across parental income deciles. This is true for both measures of employment. The employment effects are zero, or small and positive, for natives with low-income parents, but become more negative and larger in magnitude among children who grow up in higher-income households. For example, a 10 percentage point immigrant inflow has no economically or statistically significant effect on the employment rate among native-born individuals whose parents were in the third decile of parental income. The same immigration shock decreases employment rates by 3.5 percentage points for natives in the 8th income decile (column (1) of Table 4). In Section 7 we calibrate how changes in employment contribute to the changes in income we document in Table 1. We would also note that while these effects are informative about the income responses, it is also difficult to draw strong conclusions about welfare from them as changes in labor supply reflect a combination of income and substitution effects.

All four race and sex groups generally show the same pattern of estimates, though the negative employment effects of immigration are distinctly larger among high-income women than high-income men. These results contrast with findings on the contemporaneous effects of immigration on the employment of more educated women, as in Cortés and Pan (2019), which finds positive effects of immigration on employment due particularly to labor supply in the childcare sector. Heterogeneity by parent income decile is less pronounced among Black individuals, especially Black women, for whom immigration lowers employment rates across the entire income distribution. We return to discuss sex effects and the role of household labor supply in Section 6.5.

6.3 Educational Attainment

We next estimate the effect of immigration on the probabilities that a person completes high school, enrolls in college, and graduates college. IV estimates shown in Table 5 and Figures 7 (high school) and 8 (college graduation). As before, figures on the left show IV estimates and confidence intervals. Figures on the right show the simulated effect on levels from a 10 percentage point increase in immigration. We discuss effects on college enrollment below and the point estimates are in Appendix Table A3.

We again find striking differences in how immigration affects children who grow up in poorer and richer households. While the average effect of immigration on high school completion is 0.04, which is small and not statistically different from zero, the effects on individuals who grew up in households in the bottom half of the income distribution are quite large. For example, the estimated effects on children from the lowest three deciles are about 0.22 to 0.25, so a 10-percentage-point increase in immigration raises high school completion by 2.5 percentage points off of a base of about 83 percent in the bottom deciles. Immigration has essentially no effect on high school completion among individuals from higher-income families. The lack of response above the fourth decile of parent income likely reflects a ceiling effect, since high school graduation rates exceeded 90 percent for these groups.

In panels (c) through (f) of Figure 7, we disaggregate the high school effects by sex and race. In panels (c) and (e), we find remarkably similar effects in levels for all four groups across the income distribution. In panel (d) it is clear that the percentage reduction in high school dropout of Black men is somewhat smaller than for White men due to the lower baseline high school completion rates. Nevertheless, the broadest conclusion is that there is very little evidence of heterogeneity in the high school response across demographic groups. The similarity in effects between Black and White individuals is noteworthy because the groups tend to live in different neighborhoods and attend different schools within cities.

Immigration tends to reduce the probability of completing college, particularly among children who grow up in richer families. This is documented in Figure 8. A 10-percentage-point immigration shock leads to a 5.6 and 5.9 percentage point decrease in the probability of college completion among individuals from the top two deciles of family income. This is a fairly large effect relative to the average college completion rate of 70 percent among this group. In panels (c) through (f), we once again find similar patterns across race and sex, although decreases in college completion are notably larger for Whites. The decline in average college attendance, reported at the bottom row of column (6) of Table 5, is driven by declines among Whites.

Effects on college completion reflect both effects on starting and completing college. We find that while college completion rates among children who grow up in lower-income families are not affected by immigration, the increase in high school completion that we document does, in fact, translate into an increase in college attendance. College attendance rates rise in the lower six deciles of the parental income distribution.[27] By contrast, the decline in college completion among individuals from top deciles is largely explained by a decline in graduation rates among those who attend. Thus, some portion of the decline in college completion may reflect crowding from higher enrollment rates in the initial years, as found in other settings (Bound and Turner, 2007).

We assess robustness in Table 3 using 1970 enclaves and the measurement of parent income in 1979. Results on both high school graduation and BA completion show the same pattern of positive coefficients for children from low-income families and negative coefficients for children from high-income families. Using the 1970 enclaves, the results for high school completion in column 2 are exhibit modestly larger coefficients and standard errors across the distribution, though we cannot reject equality. In column 3, BA completion estimates using 1970 enclaves display more evidence of positive effects for children from low-income families and nearly identical coefficients for high-income families. When we use the 1979 measurement of family income, we find significantly less precise estimates, though the negative relationship across the deciles remains clear. In sum, the conclusions of the primary analysis hold in these alternative specifications.

Our estimated effects on educational attainment are consistent with the increase in mobility we find in our results on income and employment. Our results echo prior work by Hunt (2017), which shows that immigration during this era increased high school completion. Our evidence indicates that those effects are concentrated among children of lower decile parents. But, importantly, the gains in attainment among some are offset by reductions in college completion among others. In Section 7 we calibrate the contribution of education to the income mobility findings.

[27]These results are in Appendix Table A3 and Figure A2.

6.4 Internal Migration

We next proceed to an analysis of the effects of immigration on internal migration across commuting zones. CZs are large, so these migration responses mostly reflect long-distance moves rather than neighborhood changes. We measure migration of families in several ways. Our first measure is an indicator that the child moved between birth (which is recorded in the Numident) and age 16 to 18 (measured from the parental 1040 tax records we use to link parents and children). This measure thus captures mobility decisions by parents while the child is still living at home. Our second measure is an indicator that the child moved between birth and adulthood, where adult location is measured in the tax data used to measure adult income. We also ask whether people who move tend to move to places where children from their own family background do better. Specifically, we measure CZ quality as the average adult rank in the national income distribution of children of the same race and parental income decile who are born in that CZ.

Estimates of the effect of immigration on migration outcomes are presented in Table 6 and Figure 9. The estimates in column (1) show large out-migration responses among parents that are largely constant across the parental income distribution, though somewhat lower at the very top decile. In particular, the average effect across all people implies that a 10 percentage point immigrant inflow rate leads to a 12 percentage point increase in the probability of changing CZs between birth and age 16. Similarly, immigration has a positive effect on the probability of moving between birth and adulthood is on the order of a third larger, implying that immigration also influences moves during adulthood. The overall probability of moving is about fifty percent and thus immigration does exert a sizable influence on internal migration.

Internal migration could explain our results on income and educational attainment if poorer families systematically move to cities where poorer children tend to do better or richer families move to places where they tend to do worse. This does not appear to be the case, as we show in column (3). The dependent variable in this model is the difference between the CZ quality where the individual was born and the CZ quality where the individual is recorded as an adult in their W2 records.[28] The estimates indicate that individuals born in lower-decile families were more likely to move to lower quality CZs, while those from upper-decile families tend to move to slightly higher quality CZs. More specifically, from panel (b) of Figure 5, children born in the bottom two deciles tend to end up at about the 40th percentile of the adult income distribution. The coefficients of -7 in column (3) of Table 6 indicates a 10 percentage point immigration shock to one's birth leads these individuals to move to CZs where low-income children end up at about the 39.3rd percentile. At the other end of the parental income distribution, a 10 percentage point immigration shock induces children who grow up in top-decile families to move to cities where high-income children end up around the 63.9th percentile, rather than the 63rd. Importantly, these effects are in the opposite direction of what could explain the increase in income among children born to low-decile parents.

6.5 Marriage and Dependents

Understanding the effect of immigration on marriage and fertility is important for several reasons. Most immediately for our primary results, household formation and composition may affect the interpretation of effects on labor market outcomes. These outcomes are also interesting in their own right, particularly as the U.S. has experienced declining birth rates during this period. One potential policy response to declining birth rates is to permit more immigration, as immigrants make net positive contributions to the government budget in large part due to their higher fertility rate (Lee and Miller, 2000; National Academies of Sciences and Medicine, 2017). However, reductions in native fertility due to the arrival of additional immigrants would reduce these benefits in future generations.

We measure marriage using individuals' tax filing status. Specifically, we create an indicator variable for whether the individual ever filed as married in their tax return. We also create two measures of fertility to capture the distinction between intensive and extensive margin fertility (Aaronson, Lange and Mazumder, 2014). The first measure is an indicator that the person has any

[28] Because we assign CZ quality separately by race, these models only include Black and White individuals. Individuals who do change CZ are included in these models with a difference in their CZ quality coded as zero.

dependents. The second is the number of dependents (including zeros for those who have none).

Table 7 presents estimates of regressions of equation 1 in which the dependent variable is an indicator that the individual is married or a measure of dependents. The estimates in column (1) indicate that immigration raises marriage rates among individuals born in lower-income families. In particular, a 10 percentage point increase in immigration is associated with about a three to four percentage point increase in marriage among those from the bottom four deciles. These effects are generally larger for Black men than White men, and for Black women relative to White women. Indeed, the positive effects of immigration on marriage among Black individuals extends through the eighth decile of family income. Immigration reduces marriage rates among those born in the top decile, an effect that is driven by White individuals. These effects on marriage may alter the interpretation of the employment declines discussed in Section 6.2 if considered through the lens of household labor supply.

Columns (6) and (7) present estimates of the effect of immigration on the number of dependents. Both measures indicate that immigration has no effect on whether individuals have any children or the number of children.[29]

7 Explaining the Effect on Intergenerational Mobility

To what extent do our findings on educational attainment and employment explain our findings on income rank? To answer this, we first calibrate the relationships between income ranks, educational attainment, and employment among 40 subgroups defined by parental income decile, race, and sex. Specifically for education, we regress the average income rank for each subgroup on the average high school completion and on college completion rates, plus fixed effects for each race-by-sex group. The results from this model are reported in column (1), Panel A, of Table A4. We then multiply these estimates by the estimated effects of immigration on high school completion and on college completion among all people that are reported in columns (1) and (6) of Table 5 to

[29]Results by race are similarly not statistically significantly different from zero for any decile.

obtain an estimate of the decile-specific effects of immigration on income rank among all people that is predicted solely by the effects of immigration on educational attainment. This exercise is not meant to estimate the causal effect of, for example, education on income rank, but rather asks how correlated the effects on education are with the effects on income.

We report the results from this exercise in Figure 10. In Panel (a) we report the actual coefficient estimates of the effect of immigration on income rank for all people from column (1) of Table 1, as well as the predicted coefficient estimates. We report predicted results based solely on the estimates effects on high school graduation, solely on the effects on college graduation, and based on both the high school and college effects together. The figure shows that the effects of immigration on high school graduation nearly perfectly predict the actual effects on income rank among the bottom five deciles of the income distribution. The effects on college graduation track the effects on income rank among the upper deciles, though actual effects on income rank are somewhat more negative than that predicted by the effects on college graduation. In summary, the effects on education can account for most of the effects on income rank.

In the remaining panels of Figure 10 we conduct similar analyses to assess how well the effects on educational attainment explain the effects on employment (Panel (b)) and how well the effects on employment explain the effects on income (Panel (c)). Again, the underlying coefficient estimates are in Table A4. The declining pattern of effects of immigration on high school completion mirrors the effects on employment among children who grow up in families in the lower half of the income distribution. The actual effects of immigration on employment are generally shifted downward than that predicted by the effects on educational attainment, suggestive of some other mechanism that influences employment. The employment effects among those who grew in richer families are not explained at all by educational attainment.

Finally, the results in Panel (c) demonstrate that the pattern of effects on employment can explain the declining pattern of effects on income rank, though here there is a upward shift. The results are broadly similar whether we define employment based on having a W2 income above zero or above the minimum wage. Among the bottom two deciles, the income rank effects are nearly perfectly predicted by the employment results, which suggests little room for effects on hourly wages or the extensive margin of labor supply. In the remainder of the family income distribution, the effects on income rank are not as negative as what would be predicted by the employment changes. This pattern suggest the effects on income in these groups combine competing effects of reduced employment rates and higher incomes for those who do work.

Taken together, the analysis in this section indicates that much of the effects on adult labor market outcomes are explained by effects on educational attainment, implicating causal factors that work in adolescence. These could include neighborhoods, school quality, and effects from peers. Additionally, our analysis in Figure 4 documented declines in employment in the young-adult labor market in 1990, when our target cohort was still school age. We suspect these changes in the labor market in the cohort preceding our sample may have motivated the responses in human capital investment we find.

8 Conclusion

We use the large influx of immigrants to the United States in the second half of the 20th Century to study the long-run effect of local immigration on the economic mobility of the next generation. Unlike the vigorous debate over the short- and long-run effects of immigration, there is wide consensus that increasing intergenerational mobility is a good thing. Greater economic mobility means that the economic status one was born into plays less of a role in determining one's own economic status. We find robust evidence that immigration increases intergenerational mobility. In particular, in response to immigration to their city, children from low-income families are more likely to graduate high school and enroll in college, while children from richer families are less likely to complete college. These effects drive changes in employment and earnings as adults: immigration increases earnings among individuals born to families at the bottom of the income distribution and decreases among those born to parents at the top of the income distribution. This has the effect of lessening the correlation in economic outcomes across generations, flattening the well-known intergenerational slope. Average effects for most of the outcomes are small and statistically indistinguishable from zero.

These findings have important implications for how we think about the economic consequences of immigration. Contemporaneous labor market competition—whereby the earnings of low-skilled natives are potentially harmed by immigration—is likely not the primary way that immigration affects well-being, especially that of subsequent generations. A particular example of this pattern within our results is that Black workers are no more affected than White workers exposed to immigration in their youth, despite long-running concerns about their (and their parents') vulnerability to immigrant competition in the labor market. Unlike the consequences of international trade and skill-biased technological progress, the distributional consequences of immigration tend to improve the outcomes of lower-income families.

Instead of the classic labor market competition mechanisms emphasized in the previous literature, our results point to important responses that occur in youth and at the level of the aggregate labor market. Effects on education are highly correlated with effects on adult income rank, consistent with shifts in educational and life-cycle investments in the anticipation of changing labor market opportunities. These shifting opportunities appear to reward human capital investments independent of parent background. For children born into richer families, these changing opportunities have a two-sided nature. There are declines in employment and college completion; however, these declines over-predict effects on income, implying that a subset of children from high-income families have higher incomes in adulthood as a result of immigration. Changes in labor market opportunities at the aggregate level are also supported by the similarity of effects between Blacks and Whites, in spite of segregation and the resulting differential exposure to peer effects and other hyper-local mechanisms. Similarly, any effects of the children of immigrants appear to work primarily through their education and labor market outcomes. Taken together, the predominant role of changing labor market-level opportunities points to important impacts of immigration on innovation, entrepreneurship, and economic dynamism, as emphasized in Burchardi et al. (2020), Azoulay et al. (2022), and Peri and Sparber (2009).

References

Aaronson, Daniel, Fabian Lange, and Bhashkar Mazumder. 2014. "Fertility transitions along the extensive and intensive margins." *American Economic Review*, 104(11): 3701–3724.

Abramitzky, Ran, and Leah Boustan. 2017. "Immigration in American Economic History." *Journal of Economic Literature*, 55(4): 1311–45.

Abramitzky, Ran, Leah Boustan, Elisa Jácome, and Santiago Pérez. 2021. "Intergenerational mobility of immigrants in the United States over two centuries." *American Economic Review*, 111(2): 580–608.

Albouy, David, Heepyung Cho, and Mariya Shappo. 2021. "Immigration and the pursuit of amenities." *Journal of Regional Science*, 61(1): 5–29.

Altonji, Joseph G, and David Card. 1991. "The effects of immigration on the labor market outcomes of less-skilled natives." In *Immigration, trade, and the labor market*. 201–234. University of Chicago Press.

Autor, David, and David Dorn. 2013. "The growth of low-skill service jobs and the polarization of the US labor market." *American economic review*, 103(5): 1553–97.

Azoulay, Pierre, Benjamin F Jones, J Daniel Kim, and Javier Miranda. 2022. "Immigration and entrepreneurship in the United States." *American Economic Review: Insights*, 4(1): 71–88.

Bailey, Martha J, Connor Cole, Morgan Henderson, and Catherine Massey. 2017. "How Well Do Automated Methods Perform in Historical Samples?: Evidence from New Ground Truth." *National Bureau of Economic Research Working Paper #24019*.

Bailey, Martha J, Hilary Hoynes, Maya Rossin-Slater, and Reed Walker. 2023. "Is the Social Safety Net a Long-Term Investment? Large-Scale Evidence From the Food Stamps Program." *The Review of Economic Studies*, 91(3): 1291–1330.

Bartik, Timothy J. 1991. "Who benefits from state and local economic development policies?"

Betts, Julian R, and Robert W Fairlie. 2003. "Does immigration induce 'native flight' from public schools into private schools?" *Journal of Public Economics*, 87(5): 987–1012.

Black, Dan A, Seth G Sanders, Evan J Taylor, and Lowell J Taylor. 2015. "The impact of the Great Migration on mortality of African Americans: Evidence from the Deep South." *American Economic Review*, 105(2): 477–503.

Black, Sandra E., and Paul J. Devereux. 2011. "Recent Developments in Intergenerational Mobility." In . Vol. 4B of *Handbook of Labor Economics*, , ed. David Card and Orley Ashenfelter, 1487–1541. Elsevier.

Borjas, George J. 2001. "Does Immigration Grease the Wheels of the Labor Market?" *Brookings Papers on Economic Activity*, 2001(1): 69–119.

Borjas, George J. 2003. "The Labor Demand Curve is Downward Sloping: Reexamining the Impact of Immigration on the Labor Market." *Quarterly Journal of Economics*, 118(4): 1335–1374.

Borjas, George J. 2006. "Native Internal Migration and the Labor Market Impact of Immigration." *Journal of Human Resources*, XLI(2): 221–258.

Borusyak, Kirill, Peter Hull, and Xavier Jaravel. 2021. "Quasi-Experimental Shift-Share Research Designs." *The Review of Economic Studies*, 89(1): 181–213.

Bound, John, and Sarah Turner. 2007. "Cohort crowding: How resources affect collegiate attainment." *Journal of public Economics*, 91(5-6): 877–899.

Boustan, Leah, Christine Cai, and Tammy Tseng. 2024. "JUE Insight: White flight from Asian immigration: Evidence from California Public Schools." *Journal of Urban Economics*, 141: 103541.

Burchardi, Konrad B, Thomas Chaney, Tarek Alexander Hassan, Lisa Tarquinio, and Stephen J Terry. 2020. "Immigration, Innovation, and Growth." National Bureau of Economic Research Working Paper 27075.

Card, David. 2001. "Immigrant Inflows, Native Outflows, and the Local Labor Market Impacts of Higher Immigration." *Journal of Labor Economics*, 19(1): 22–64.

Card, David. 2009. "Immigration and inequality." *American Economic Review*, 99(2): 1–21.

Card, David, and John DiNardo. 2000. "Do Immigrant Inflows Lead to Native Outflows?" *American Economic Review*, 90(2): 360–367.

Carlana, Michela, and Marco Tabellini. Forthcoming. "Happily Ever After: Immigration, Natives' Marriage, and Fertility." *Journal of Economic History*.

Cascio, Elizabeth U., and Ethan G. Lewis. 2012. "Cracks in the Melting Pot: Immigration, School Choice, and Segregation." *American Economic Journal: Economic Policy*, 4(3): 91–117.

Chetty, Raj, and Nathaniel Hendren. 2018. "The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects." *The Quarterly Journal of Economics*, 133(3): 1107–1162.

Chetty, Raj, Nathaniel Hendren, Maggie R Jones, and Sonya R Porter. 2019. "Race and Economic Opportunity in the United States: an Intergenerational Perspective*." *The Quarterly Journal of Economics*, 135(2): 711–783.

Chetty, Raj, Nathaniel Hendren, Patrick Kline, and Emmanuel Saez. 2014. "Where is the land of Opportunity? The Geography of Intergenerational Mobility in the United States." *The Quarterly Journal of Economics*, 129(4): 1553–1623.

Cortés, Patricia, and Jessica Pan. 2019. "When time binds: Substitutes for household production, returns to working long hours, and the skilled gender wage gap." *Journal of Labor Economics*, 37(2): 351–398.

Diamond, Rebecca. 2016. "The Determinants and Welfare Implications of US Workers' Diverging Location Choices by Skill: 1980-2000." *American Economic Review*, 106(3): 479–524.

Diamond, Rebecca, and Enrico Moretti. 2024. "Where is Standard of Living the Highest? Local Prices and the Geography of Consumption."

Dustmann, Christian, Uta Schönberg, and Jan Stuhler. 2016. "The impact of immigration: Why do studies reach such different results?" *Journal of Economic Perspectives*, 30(4): 31–56.

East, Chloe N., Sarah Miller, Marianne Page, and Laura R. Wherry. 2023. "Multigenerational Impacts of Childhood Access to the Safety Net: Early Life Exposure to Medicaid and the Next Generation's Health." *American Economic Review*, 113(1): 98–135.

Figlio, David, Paola Giuliano, Riccardo Marchingiglio, Umut Ozek, and Paola Sapienza. 2023. "Diversity in Schools: Immigrants and the Educational Performance of U.S.-Born Students." *The Review of Economic Studies*, 91(2): 972–1006.

Garin, Andrew, and Jonathan Rothbaum. 2022. "Was the Arsenal of Democracy an Engine of Mobility? Public Investment and the Roots of Mid-century Manufacturing Opportunity."

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift. 2020. "Bartik Instruments: What, When, Why, and How." *American Economic Review*, 110(8): 2586–2624.

Gould, Eric D., Victor Lavy, and Daniele M. Paserman. 2009. "Does Immigration Affect the Long-Term Educational Outcomes of Natives? Quasi-Experimental Evidence." *The Economic Journal*, 119(540): 1243–1269.

Guzman, Gloria, and Melissa Kollar. 2023. "Income in the United States: 2022." *US Census Bureau, Current Population Reports*, , (P60-279).

Haider, Steven, and Gary Solon. 2006. "Life-Cycle Variation in the Association between Current and Lifetime Earnings." *American Economic Review*, 96(4): 1308–1320.

Hammermesh, Daniel S, and Frank D Bean. 1998. *Help or hindrance?: The economic implications of immigration for African Americans.* Russell Sage Foundation.

Hunt, Jennifer. 2017. "The impact of immigration on the educational attainment of natives." *Journal of Human Resources*, 52(4): 1060–1118.

Jaeger, David A, Joakim Ruist, and Jan Stuhler. 2018. "Shift-share instruments and the impact of immigration." National Bureau of Economic Research 24285.

Lee, Ronald, and Timothy Miller. 2000. "Immigration, social security, and broader fiscal impacts." *American Economic Review*, 90(2): 350–354.

Lewis, Ethan. 2011. "Immigration, Skill Mix, and Capital Skill Complementarity*." *The Quarterly Journal of Economics*, 126(2): 1029–1069.

Long, Joe, Carlo Medici, Nancy Qian, and Marco Tabellini. 2024. "The Impact of the Chinese Exclusion Act on the Economic Development of the Western U.S."

Massey, Catherine G., Katie R. Genadek, J. Trent Alexander, Todd K. Gardner, and Amy O'Hara. 2018. "Linking the 1940 U.S. Census with modern data." *Historical Methods: A Journal of Quantitative and Interdisciplinary History*, 51(4): 246–257.

Mazumder, Bhashkar. 2005. "Fortunate sons: New estimates of intergenerational mobility in the United States using social security earnings data." *Review of Economics and Statistics*, 87(2): 235–255.

McHenry, Peter. 2015. "Immigration and the Human Capital of Natives." *Journal of Human Resources*, 50(1): 34–71.

Monras, Joan. 2020. "Immigration and Wage Dynamics: Evidence from the Mexican Peso Crisis." *Journal of Political Economy*, 128(8): 3017–3089.

National Academies of Sciences, Engineering, and Medicine. 2017. *The Economic and Fiscal Consequences of Immigration.* Washington, DC:The National Academies Press.

Nissim, Kobbi, Sofya Raskhodnikova, and Adam Smith. 2007. *Smooth Sensitivity and Sampling in Private Data Analysis. Proceedings of the 39th Annual ACM Symposium on Theory of Computing*, STOC'07.

Ottaviano, Gianmarco IP, and Giovanni Peri. 2012. "Rethinking the effect of immigration on wages." *Journal of the European economic association*, 10(1): 152–197.

Peri, Giovanni, and Chad Sparber. 2009. "Task specialization, immigration, and wages." *American Economic Journal: Applied Economics*, 1(3): 135–169.

Rothbaum, Jonathan, and Catherine Massey. 2022. "The Geography of Opportunity Over Time."

Ruggles, Steven, Sarah Flood, Ronald Goeken, Megan Schouweiler, and Matthew Sobek. 2022. IPUMS USA: Version 12.0 [dataset], Minneapolis, MN:IPUMS.

Saiz, Albert. 2007. "Immigration and housing rents in American cities." *Journal of Urban Economics*, 61(2): 345–371.

Saiz, Albert, and Susan Wachter. 2011. "Immigration and the Neighborhood." *American Economic Journal: Economic Policy*, 3(2): 169–88.

Semega, Jessica, and Melissa Kollar. 2022. "Income in the United States: 2021." *US Census Bureau, Current Population Reports*, , (P60-276).

Sequeira, Sandra, Nathan Nunn, and Nancy Qian. 2019. "Immigrants and the Making of America." *The Review of Economic Studies*, 87(1): 382–419.

Solon, Gary. 1992. "Intergenerational income mobility in the United States." *The American Economic Review*, 393–408.

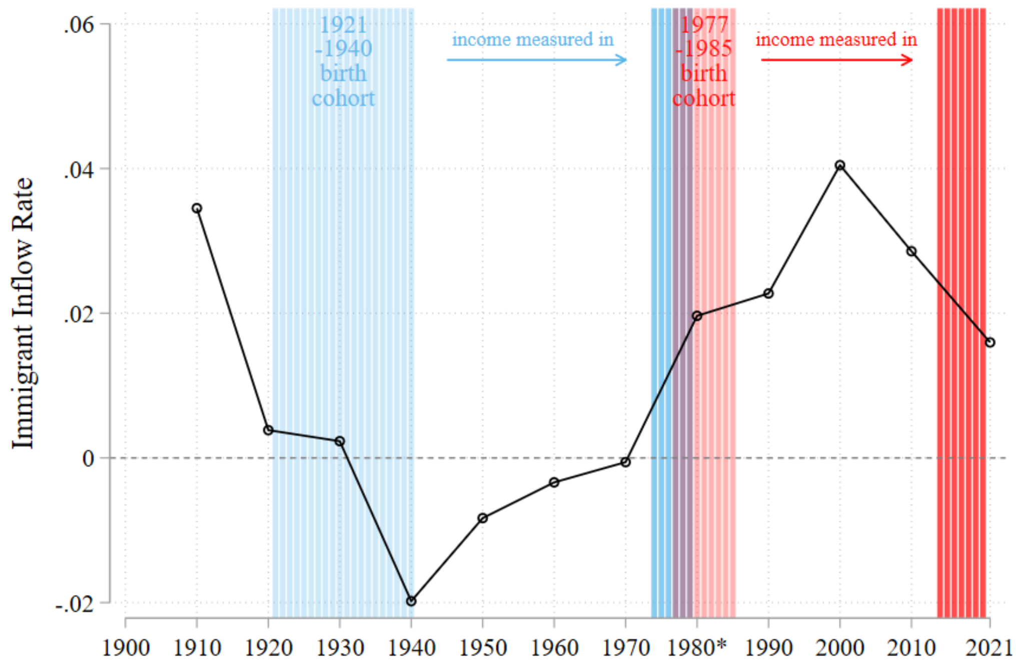
Tabellini, Marco. 2022. "The Economic and Political Effects of Immigration: Evidence from the Age of Mass Migration." In *Oxford Research Encyclopedia of Economics and Finance*.

Taylor, Evan J., Brian A. Stuart, and Martha J. Bailey. 2016. "Summary of Procedure to Match NUMIDENT Place of Birth County to GNIS Places." CES-TN-2016-01.

Wagner, Deborah, and Mary Layne. 2014. "The Person Identification Validation System: Applying the Center for Administrative Records and Research and Applications' Record Linkage Software." *Center for Administrative Records Research and Applications Working Paper*.

Figures and Tables

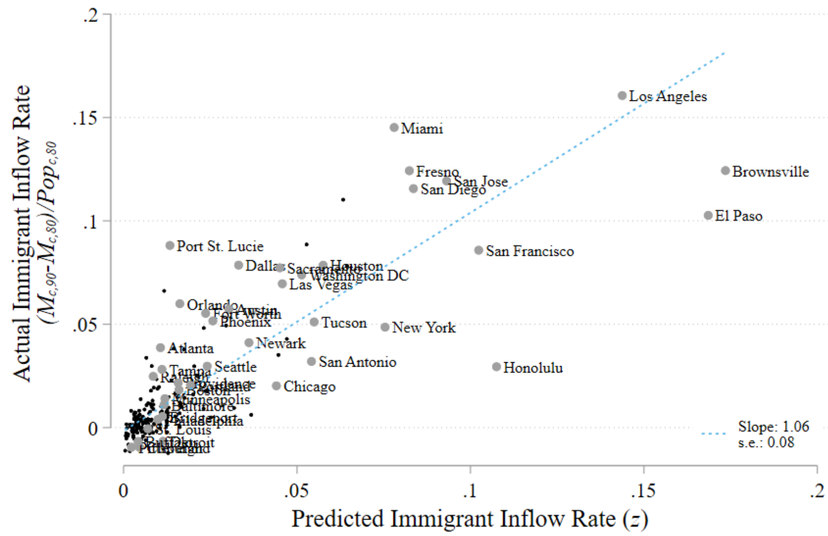
Figure 1: Immigrant Inflows and Sample Construction



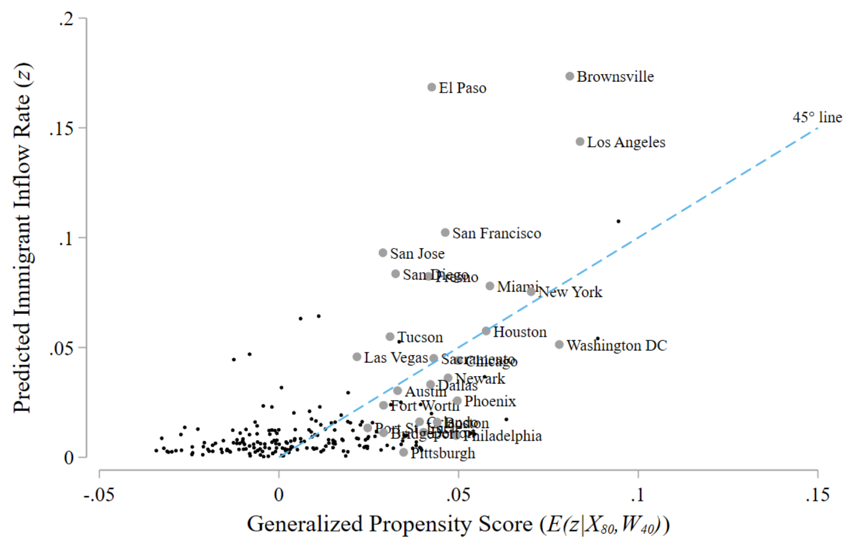
Notes: The black line depicts the immigrant inflow rate, defined as {(number of foreign born in t) - (number of foreign born in $t-10$)}/(total population in $t-10$), where t is the year on the x-axis. The analysis focuses on the immigrant inflow rate between 1980 and 1990, with immigrant enclaves measured in 1980 (denoted 1980*). Blue shades represent the birth cohorts born between 1921 and 1940, with their income measured between 1974 and 1979. Red shades represent the birth cohorts born between 1977 and 1985, with there income is measured between 2014 and 2020.

Figure 2: Actual, Predicted, and Index Immigrant Inflows

(a) Panel A: Actual and Predicted Immigrant Inflows, 1980-1990

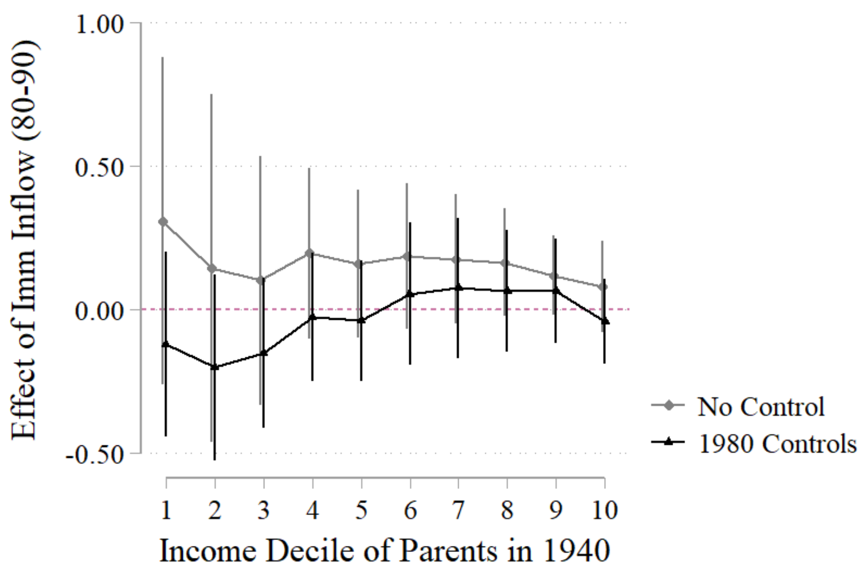


(b) Panel B: Predicted vs. Index Immigrant Inflows, 1980-1990



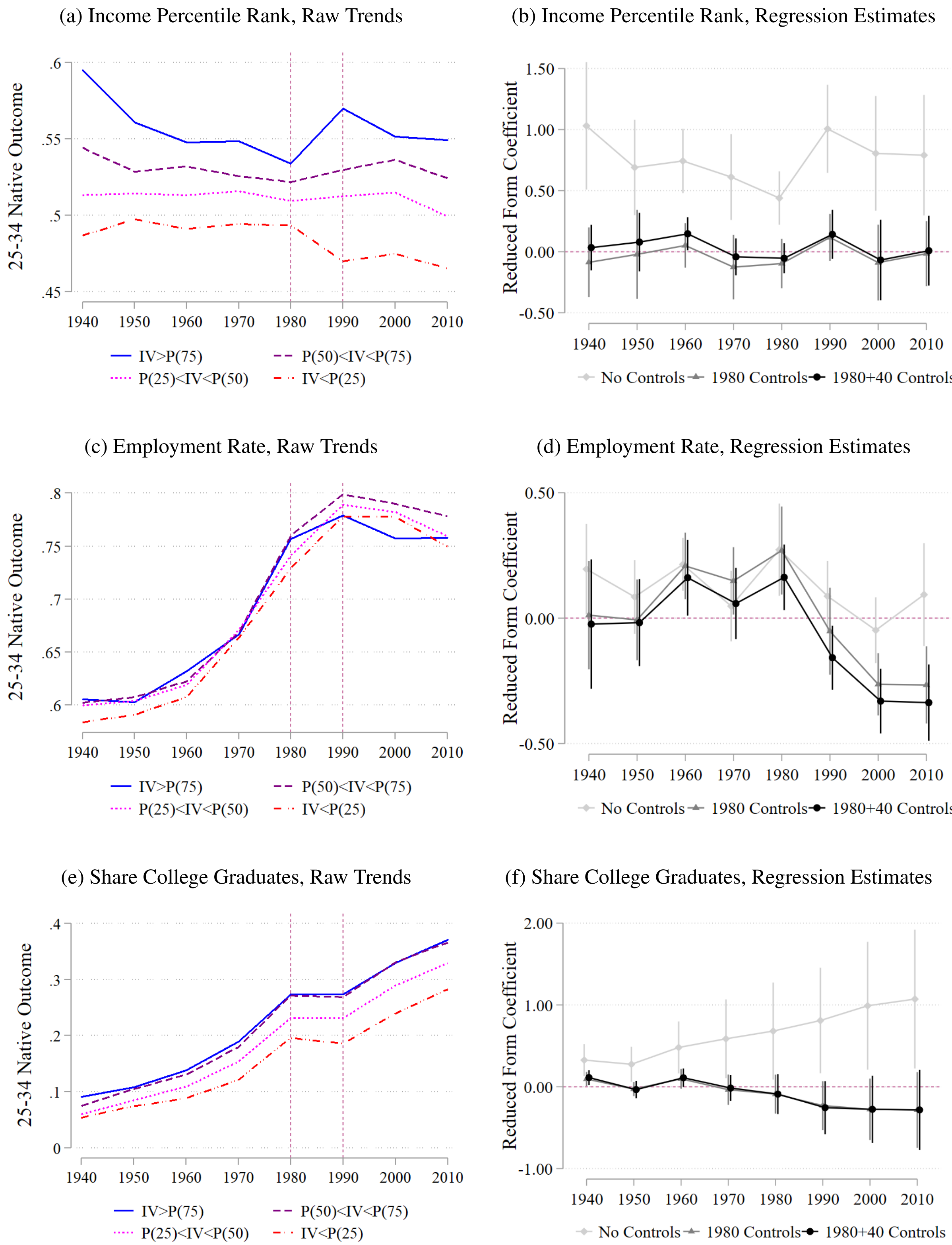
Notes: Panel A depicts the actual immigrant inflow rate (y-axis) and the predicted immigrant inflow rate (x-axis) from 1980 to 1990. Commuting zone population in 1980 is used as weights for the fitted line. The slope and its standard error are reported at the bottom right of the figure. In Panel B, the generalized propensity score is the predicted value of z, the instrumental variable (predicted immigrant inflow rate from 1980 to 1990) for each commuting zone, obtained from regressing the instrument on the covariates $X_{c,80}$ and $Z_{c,40}$ from equation (1).

Figure 3: Immigrant Inflows in 1980-1990 and Income Mobility of Native Children in 1940



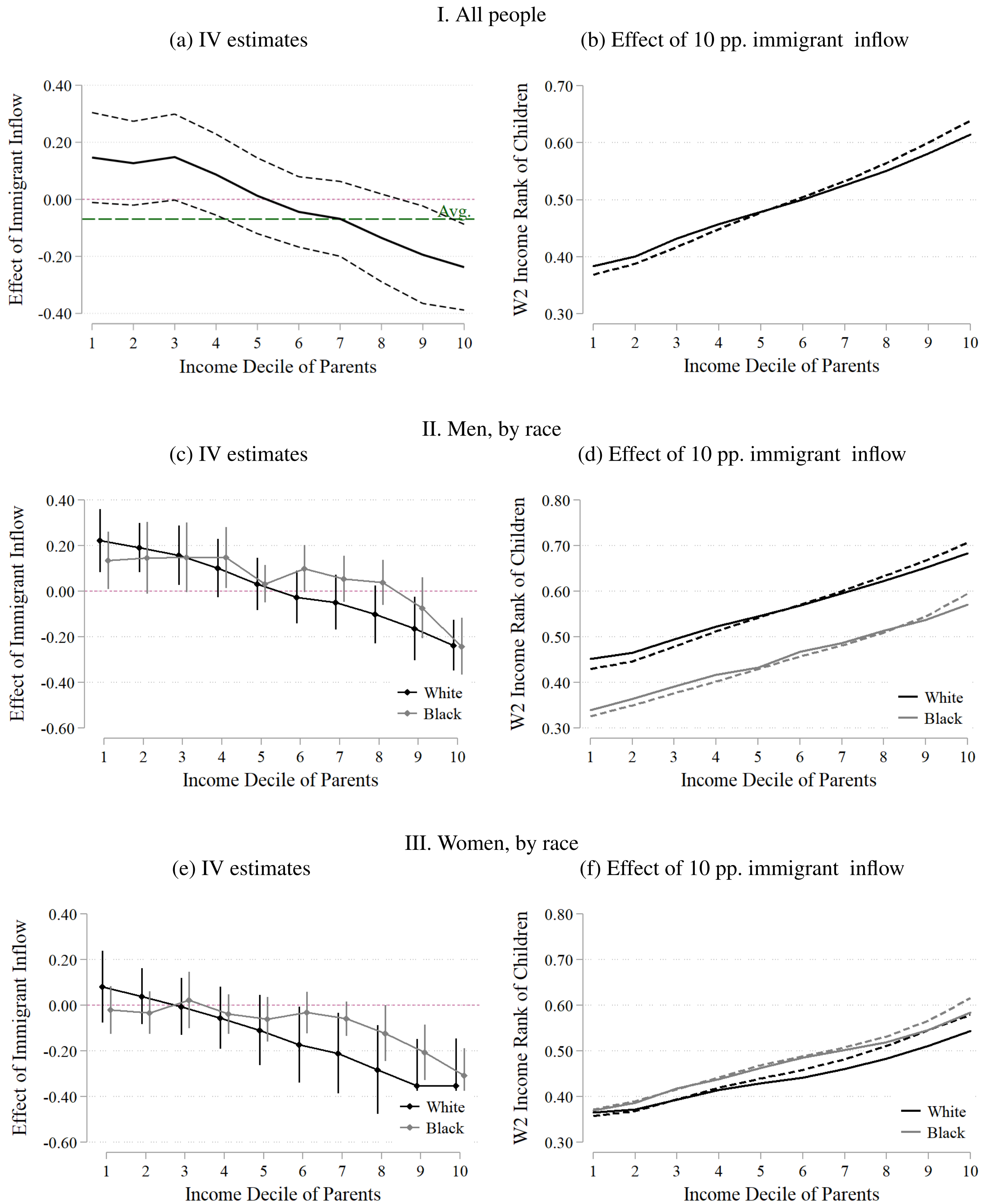
Notes: Figure plots IV estimates with the immigrant inflow rate from 1980 to 1990 as the independent variable, as in ($\hat{\beta}$ in equation 1). The dependent variable is income mobility of children born between 1921 and 1940 who have native-born parents. The line presents estimates separately by parental income decile. Appendix Table A2 reports the coefficients and standard errors. The gray line includes no controls in the model, while the black line includes the 1980 controls. 95% confidence intervals are presented in vertical lines. The regressions are weighted by the number of observations used for calculating the 1940 income mobility.

Figure 4: Predicted Immigrant Inflows and CZ-level Outcomes, 1940-2020 (Native-born aged 25 to 34)



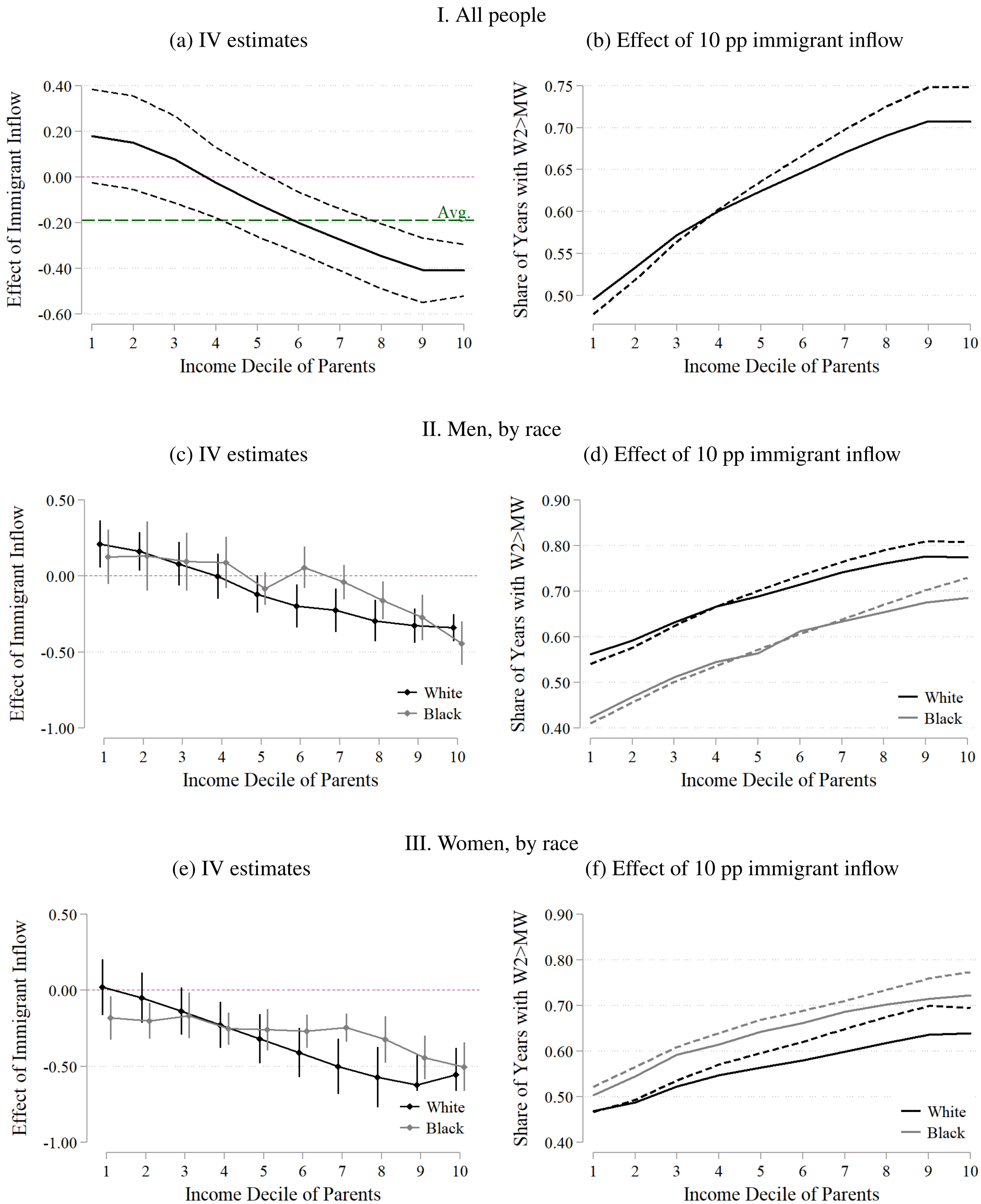
Note: Figures on the left plot the raw trends in outcomes for native-born individuals aged 25 to 34 in a given year, separately by quartile of the predicted immigrant inflow in a CZ between 1980 and 1990. Figures on the right plot the coefficient from a regression of the outcome on the instrument with no other controls, with the 1980 CZ-level controls, and with the 1940 and 1980 CZ-level controls.

Figure 5: Effects of Immigrant Inflows on W2 Income Rank



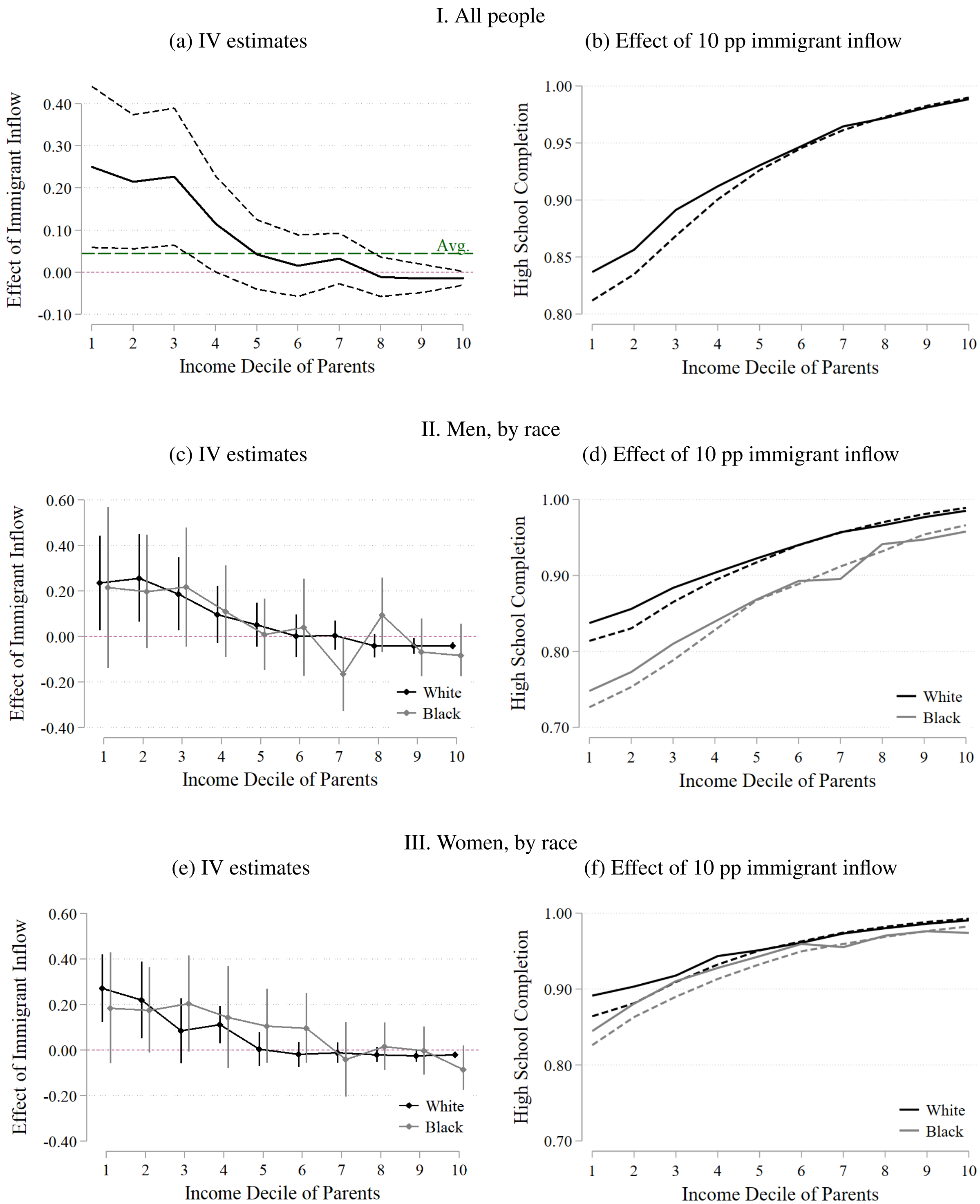
Note: Figures on the left plot IV estimates of the effects of immigrant inflow rate from 1980 to 1990 ($\hat{\beta}$ in equation 1) by parental income decile, with 95% confidence intervals. The dashed horizontal line in subfigure (a) is the average effect among all people. Table 1 reports the coefficients and standard errors. The dashed lines in the figures on the right present the average income rank by parental income decile. The solid lines indicate the effect of a 10 percentage point increase in immigration on average rank.

Figure 6: Effects of Immigrant Inflows on Share of Years with W2 Income > Minimum Wage



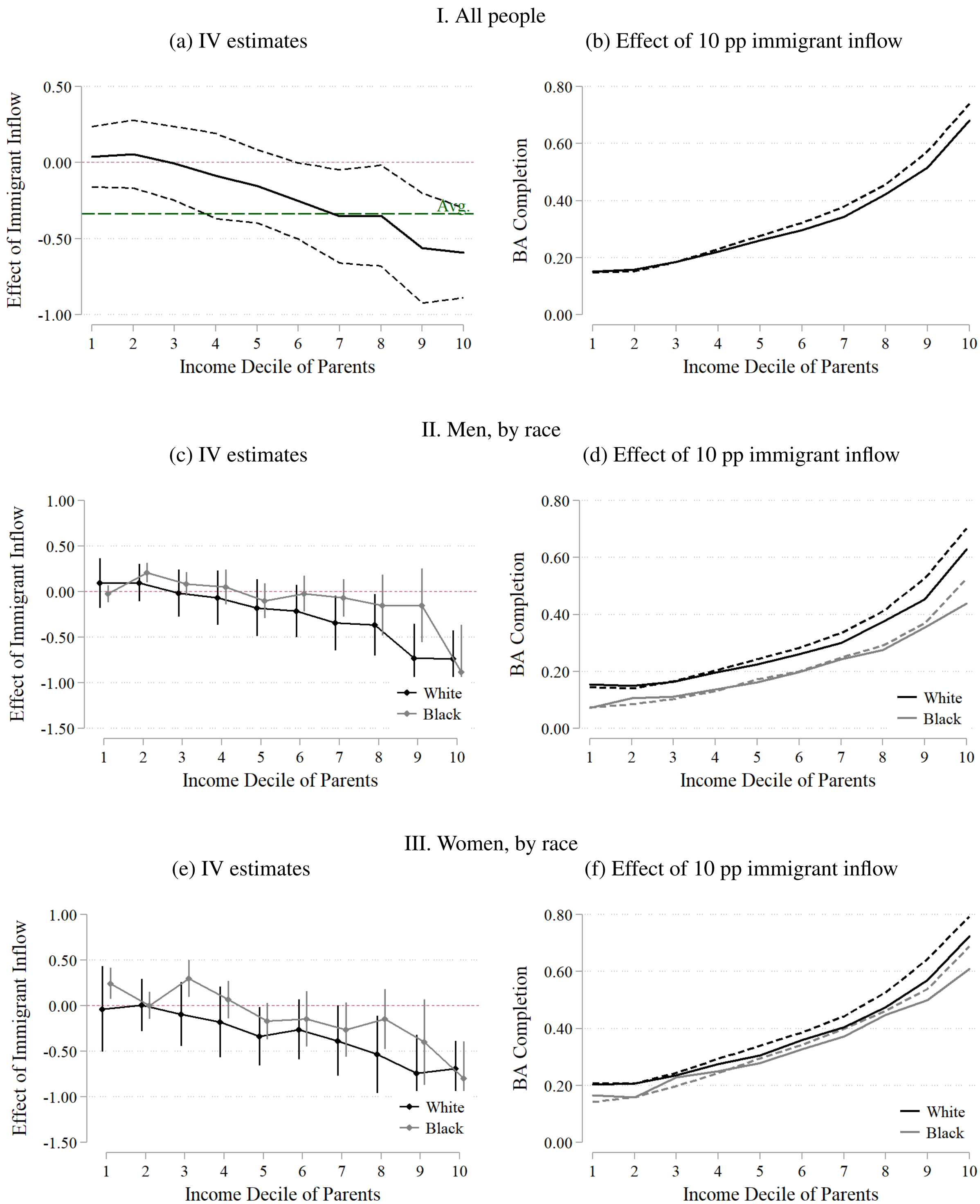
Note: The outcome variable is share of years in which the individual had W2 income greater than what would have obtained from working 40 hours per week at the minimum wage. See note to Figure 5 for additional details. Coefficient estimates and standard errors are presented in Table 4.

Figure 7: Effects of Immigrant Inflows on High School Completion



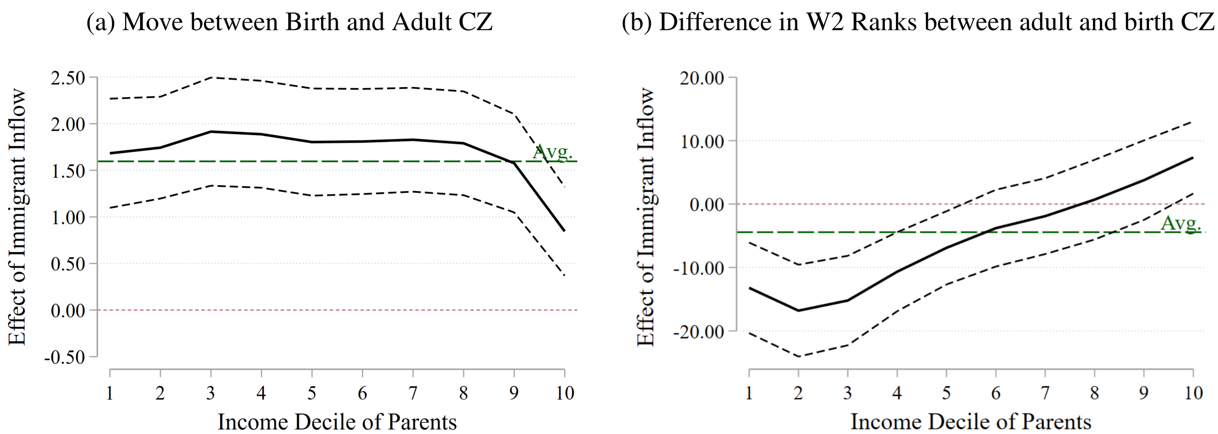
Note: The outcome variable is the indicator variable for completing high school. See note to Figure 5 for additional details. Coefficient estimates and standard errors are presented in Table 5.

Figure 8: Effects of Immigrant Inflows on BA Completion



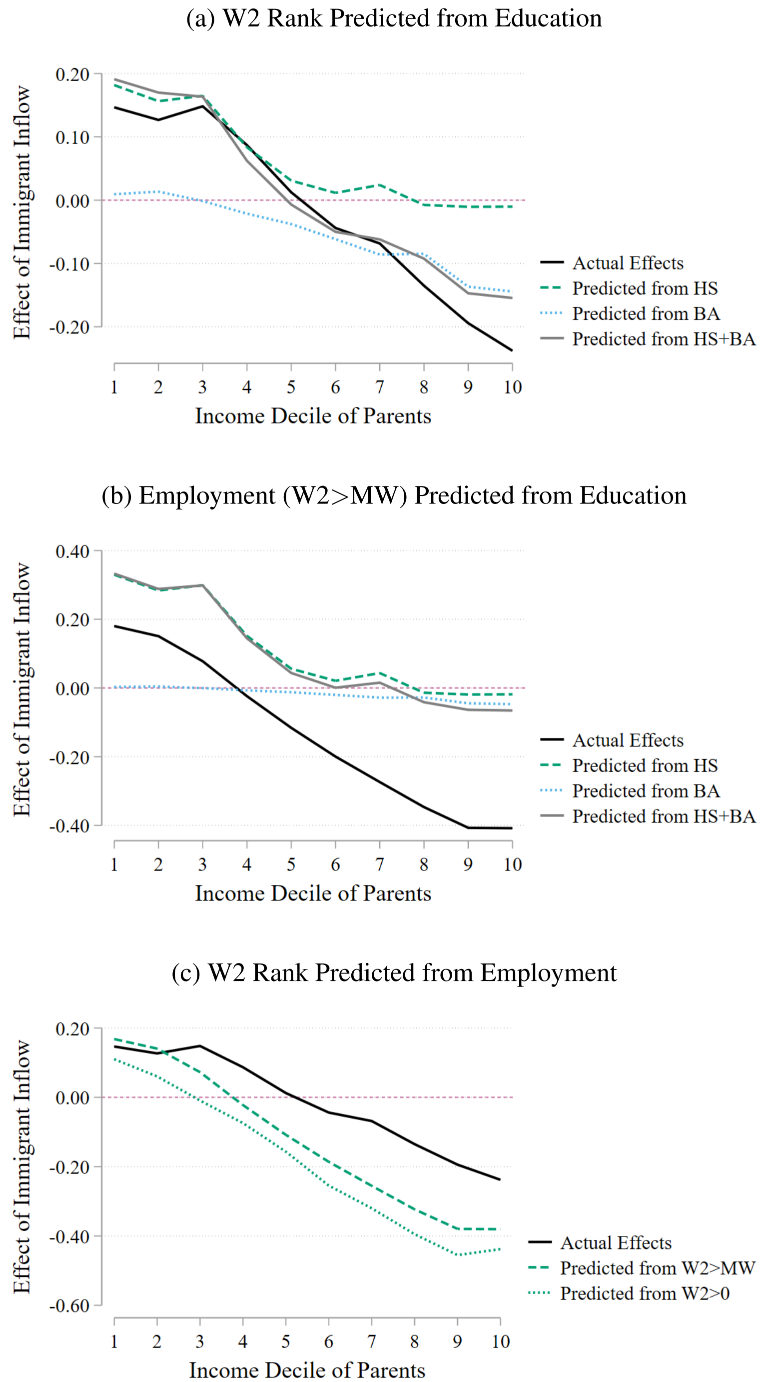
Note: The outcome variable is the indicator variable for obtaining a BA degree. See note to Figure 5 for additional details. Coefficient estimates and standard errors are presented in Table 5.

Figure 9: Immigrant Inflows and Intergenerational Mobility in Internal Migration



Note: The outcome variable in the left panel is an indicator variable that the individual moved CZs between birth and adulthood. The outcome variable in the right panel is the difference in W2 income ranks between the adult and birthplace CZs. See note to Figure 5 for additional details. Coefficient estimates and standard errors are presented in Table 6.

Figure 10: Predicted Effects on W2 Rank and Employment Based on the Effects on Education and Employment



Notes: The figures show the predicted effects of immigration inflows on W2 rank (Panels A and C) and employment (Panel B), which are derived from the effects of immigration inflows on education (Panels A and B) and employment (Panel C). The actual effects of immigration inflows are obtained from column 1 of Table 1 and column 1 of Table 4. The predicted effects on W2 rank in Panel A are calculated by multiplying the effects on high school completion (column 1 of Table 5) and BA completion (column 6 of Table 5) by the coefficients in column 1 of Panel A, Table A4. The predicted effects on employment (defined as having W2 income greater than the minimum wage) in Panel B are calculated by multiplying the effects on high school completion and BA completion by the coefficients in column 1 of Panel B, Table A4. The predicted effects on W2 rank in Panel C are calculated by multiplying the effects on employment (column 1 of Table 4) by the coefficients in columns 2 and 3 of Panel A, Table A4.

Table 1: Effects of Immigrant Inflows on Income

	W2 Income Rank					Prob. Top20	W2 Log	Self Employed	Total Income Rank	Total Income Log
	All People	White Male	White Female	Black Male	Black Female	All People				
Decile	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1	0.147*	0.221***	0.081	0.135**	-0.021	0.205**	0.601***	0.044*	0.248***	0.589**
	(0.080)	(0.070)	(0.079)	(0.064)	(0.053)	(0.083)	(0.206)	(0.023)	(0.067)	(0.245)
2	0.127*	0.191***	0.039	0.146*	-0.033	0.151**	0.556***	0.061***	0.245***	0.653***
	(0.075)	(0.055)	(0.062)	(0.080)	(0.047)	(0.068)	(0.175)	(0.019)	(0.065)	(0.242)
3	0.148*	0.157**	-0.006	0.148*	0.023	0.206**	0.567***	0.046*	0.234***	0.675***
	(0.076)	(0.066)	(0.063)	(0.077)	(0.063)	(0.089)	(0.197)	(0.026)	(0.071)	(0.258)
4	0.087	0.101	-0.055	0.147**	-0.039	0.207**	0.471**	0.066***	0.171**	0.562**
	(0.072)	(0.065)	(0.069)	(0.068)	(0.044)	(0.097)	(0.193)	(0.025)	(0.066)	(0.253)
5	0.012	0.031	-0.109	0.032	-0.061	0.160*	0.404**	0.077***	0.095	0.328
	(0.067)	(0.058)	(0.078)	(0.042)	(0.050)	(0.094)	(0.195)	(0.026)	(0.064)	(0.233)
6	-0.044	-0.026	-0.173**	0.099*	-0.032	0.133	0.343*	0.072***	0.040	0.186
	(0.063)	(0.059)	(0.085)	(0.052)	(0.046)	(0.094)	(0.207)	(0.019)	(0.061)	(0.234)
7	-0.068	-0.049	-0.211**	0.053	-0.059	0.137	0.336	0.087***	-0.014	0.117
	(0.067)	(0.061)	(0.090)	(0.051)	(0.038)	(0.125)	(0.210)	(0.021)	(0.067)	(0.242)
8	-0.135*	-0.102	-0.282***	0.037	-0.123**	0.063	0.280	0.094***	-0.074	-0.011
	(0.078)	(0.065)	(0.098)	(0.050)	(0.062)	(0.129)	(0.217)	(0.027)	(0.073)	(0.264)
9	-0.194**	-0.164**	-0.353***	-0.073	-0.207***	-0.008	0.152	0.087***	-0.143*	-0.155
	(0.087)	(0.071)	(0.104)	(0.068)	(0.062)	(0.148)	(0.236)	(0.031)	(0.074)	(0.265)
10	-0.238***	-0.237***	-0.353***	-0.241***	-0.307***	-0.144	-0.045	0.130***	-0.220***	-0.317
	(0.076)	(0.056)	(0.105)	(0.064)	(0.061)	(0.134)	(0.272)	(0.036)	(0.072)	(0.315)
Avg Effect	-0.069	-0.109	-0.248**	0.124**	-0.025	0.070	0.263	0.082***	0.012	0.093
	(0.085)	(0.078)	(0.104)	(0.057)	(0.050)	(0.137)	(0.265)	(0.021)	(0.084)	(0.306)
Mean	0.505	0.589	0.479	0.404	0.442	0.203	10.83	0.062	0.515	10.45

Notes: Each coefficient is an IV estimate of β from equation (1), including the 1940 and 1980 control variables. Each row corresponds to a decile of parental income; the penultimate row is the estimate among all people. The final row shows the overall mean of the dependent varaible. The dependent variables are listed at the top of the table and described in Section 6.1. Standard errors are clustered by commuting zone and shown in parentheses. Stars denote statistical significance at the 0.1 (*), 0.05 (**), and 0.01 (***) levels.

Table 2: Effects of Immigrant Inflows on Income, Robustness

	OLS	No Controls	No 1940 Controls	Control Y in 1940	No Mexico in Enclaves	COLA Adjust	CZ Fixed Effects
				All People			
Decile	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1	0.080**	0.294***	0.151	0.193**	0.214*	0.010	0.236***
	(0.040)	(0.105)	(0.095)	(0.081)	(0.109)	(0.081)	(0.028)
2	0.060	0.280***	0.132	0.173**	0.171*	0.018	0.194***
	(0.039)	(0.097)	(0.086)	(0.077)	(0.096)	(0.085)	(0.022)
3	0.029	0.227***	0.108	0.146*	0.151	-0.023	0.138***
	(0.038)	(0.083)	(0.086)	(0.080)	(0.105)	(0.084)	(0.017)
4	-0.005	0.146**	0.040	0.086	0.095	-0.106	0.065***
	(0.035)	(0.073)	(0.079)	(0.071)	(0.102)	(0.069)	(0.009)
5	-0.052	0.088	-0.009	0.027	0.047	-0.203***	
	(0.037)	(0.070)	(0.080)	(0.071)	(0.104)	(0.063)	
6	-0.102***	0.026	-0.067	-0.032	0.011	-0.271***	-0.075***
	(0.039)	(0.070)	(0.077)	(0.070)	(0.117)	(0.064)	(0.013)
7	-0.139***	-0.037	-0.106	-0.078	-0.030	-0.334***	-0.154***
	(0.044)	(0.062)	(0.077)	(0.072)	(0.119)	(0.065)	(0.021)
8	-0.185***	-0.082	-0.160*	-0.134*	-0.058	-0.427***	-0.220***
	(0.052)	(0.068)	(0.083)	(0.079)	(0.136)	(0.066)	(0.026)
9	-0.253***	-0.096	-0.232**	-0.206**	-0.098	-0.525***	-0.271***
	(0.059)	(0.088)	(0.092)	(0.086)	(0.155)	(0.065)	(0.028)
10	-0.292***	-0.017	-0.290***	-0.265***	-0.118	-0.579***	-0.235***
	(0.061)	(0.123)	(0.090)	(0.080)	(0.144)	(0.059)	(0.034)
Avg Effect	-0.127**	0.091	-0.100	-0.052	0.002	-0.293***	
	(0.051)	(0.099)	(0.105)	(0.092)	(0.139)	(0.074)	

Notes: Each coefficient is an IV estimate of β from equation (1), except for column 1 which is OLS and column 7 which is reduced form. Each row corresponds to a decile of parental income; the final row is the estimate among all people. The specifications are described in Section 6.1. Standard errors are clustered by commuting zone and shown in parentheses. Stars denote statistical significance at the 0.1 (*), 0.05 (**), and 0.01 (***) levels.

Table 3: Robustness Checks Using 1970 Enclaves and Parent Income in 1979

	1970 Enclaves			Parent Income Measured in 1979		
	All People					
	W2 Rank	High School	BA	W2 Rank	High School	BA
Decile	(1)	(2)	(3)	(4)	(5)	(6)
1	0.332***	0.401***	0.296			
	(0.119)	(0.128)	(0.199)			
2	0.306***	0.399***	0.285*	0.029	0.126	-0.132
	(0.110)	(0.135)	(0.161)	(0.091)	(0.085)	(0.214)
3	0.278**	0.252**	0.260	0.144	0.159*	0.189
	(0.117)	(0.115)	(0.203)	(0.096)	(0.085)	(0.131)
4	0.203*	0.225**	0.115	0.014	0.046	-0.034
	(0.109)	(0.090)	(0.202)	(0.088)	(0.056)	(0.175)
5	0.138	0.091	-0.001	-0.083	0.018	-0.425**
	(0.105)	(0.061)	(0.211)	(0.096)	(0.051)	(0.190)
6	0.068	0.036	0.034	-0.160*	-0.044	-0.373**
	(0.102)	(0.048)	(0.228)	(0.091)	(0.043)	(0.176)
7	0.020	0.031	-0.145	-0.194*	0.022	-0.394
	(0.103)	(0.037)	(0.225)	(0.103)	(0.036)	(0.251)
8	-0.049	0.003	-0.256	-0.218**	-0.036	-0.579**
	(0.107)	(0.025)	(0.236)	(0.108)	(0.030)	(0.255)
9	-0.117	-0.030	-0.540**	-0.244***	-0.037**	-0.628**
	(0.118)	(0.019)	(0.259)	(0.093)	(0.019)	(0.247)
10	-0.180	-0.032***	-0.596***	-0.287***	-0.033***	-0.750***
	(0.111)	(0.011)	(0.191)	(0.075)	(0.011)	(0.173)
Avg Effect	0.087	0.084	-0.134	-0.161*	-0.021	-0.497**
	(0.134)	(0.062)	(0.280)	(0.092)	(0.032)	(0.201)

Notes: Each coefficient is an IV estimate of β from equation (1), including the 1940 and 1980 control variables. Each row corresponds to a decile of parental income; the final row is the estimate among all people. The dependent variables are listed at the top of the table and described in Section 6.1. Columns (1) through (3) use the 1970 immigrant enclaves as instrumental variables. Columns (4) through (6) use parental income measured in 1979 to assign deciles. Standard errors are clustered by commuting zone and shown in parentheses. Stars denote statistical significance at the 0.1 (*), 0.05 (**), and 0.01 (***) levels.

Table 4: Effects of Immigrant Inflows on Employment

	Share of Years with W2 Income > Minimum Wage					W2 > 0
	All People	White Male	White Female	Black Male	Black Female	All People
Decile	(1)	(2)	(3)	(4)	(5)	(6)
1	0.180*	0.211***	0.021	0.126	-0.182**	0.074
	(0.103)	(0.079)	(0.093)	(0.091)	(0.073)	(0.074)
2	0.151	0.161**	-0.050	0.131	-0.201***	0.041
	(0.104)	(0.065)	(0.083)	(0.115)	(0.059)	(0.074)
3	0.078	0.080	-0.137*	0.095	-0.167**	-0.006
	(0.097)	(0.073)	(0.079)	(0.097)	(0.076)	(0.068)
4	-0.024	-0.002	-0.228***	0.089	-0.253***	-0.050
	(0.078)	(0.075)	(0.077)	(0.086)	(0.054)	(0.057)
5	-0.116	-0.119*	-0.319***	-0.083	-0.259***	-0.106**
	(0.073)	(0.063)	(0.082)	(0.054)	(0.069)	(0.053)
6	-0.200***	-0.197***	-0.410***	0.056	-0.269***	-0.172***
	(0.068)	(0.072)	(0.083)	(0.069)	(0.056)	(0.049)
7	-0.274***	-0.226***	-0.501***	-0.040	-0.246***	-0.216***
	(0.068)	(0.073)	(0.093)	(0.058)	(0.048)	(0.050)
8	-0.347***	-0.294***	-0.570***	-0.160**	-0.323***	-0.266***
	(0.072)	(0.069)	(0.101)	(0.064)	(0.077)	(0.052)
9	-0.407***	-0.327***	-0.623***	-0.273***	-0.442***	-0.307***
	(0.071)	(0.057)	(0.101)	(0.075)	(0.072)	(0.049)
10	-0.408***	-0.340***	-0.556***	-0.443***	-0.504***	-0.295***
	(0.057)	(0.045)	(0.090)	(0.073)	(0.082)	(0.038)
Avg Effect	-0.188**	-0.243***	-0.472***	0.060	-0.218***	-0.163***
	(0.084)	(0.069)	(0.095)	(0.071)	(0.057)	(0.057)
Mean	0.650	0.732	0.626	0.530	0.627	0.780

Notes: Each coefficient is an IV estimate of β from equation (1), including the 1940 and 1980 control variables. Each row corresponds to a decile of parental income; the penultimate row is the estimate among all people. The final row shows the overall mean of the dependent variable. The dependent variables and samples are listed at the top of the table and described in Section 6.2. Standard errors are clustered by commuting zone and shown in parentheses. Stars denote statistical significance at the 0.1 (*), 0.05 (**), and 0.01 (***) levels.

Table 5: Effects of Immigrant Inflows on Education

	High School Completion					BA Completion				
	All People	White Male	White Female	Black Male	Black Female	All People	White Male	White Female	Black Male	Black Female
Decile	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1	0.250**	0.235**	0.272***	0.215	0.185	0.038	0.094	-0.038	-0.023	0.242***
	(0.097)	(0.105)	(0.075)	(0.179)	(0.124)	(0.100)	(0.138)	(0.237)	(0.047)	(0.087)
2	0.215***	0.257***	0.221**	0.198	0.176*	0.056	0.097	0.008	0.208***	0.002
	(0.081)	(0.098)	(0.086)	(0.127)	(0.095)	(0.113)	(0.103)	(0.145)	(0.053)	(0.075)
3	0.227***	0.187**	0.085	0.217	0.205*	-0.006	-0.017	-0.093	0.083	0.298***
	(0.083)	(0.082)	(0.073)	(0.133)	(0.107)	(0.123)	(0.131)	(0.177)	(0.065)	(0.103)
4	0.115**	0.097	0.111***	0.111	0.145	-0.088	-0.068	-0.180	0.049	0.065
	(0.058)	(0.064)	(0.041)	(0.102)	(0.113)	(0.142)	(0.151)	(0.196)	(0.097)	(0.105)
5	0.042	0.052	0.004	0.009	0.106	-0.155	-0.178	-0.337**	-0.102	-0.170*
	(0.042)	(0.049)	(0.038)	(0.080)	(0.083)	(0.122)	(0.158)	(0.163)	(0.096)	(0.101)
6	0.016	0.003	-0.018	0.040	0.097	-0.253**	-0.211	-0.262	-0.023	-0.148
	(0.037)	(0.047)	(0.028)	(0.108)	(0.078)	(0.127)	(0.145)	(0.166)	(0.101)	(0.154)
7	0.033	0.005	-0.012	-0.165**	-0.041	-0.353**	-0.343**	-0.385*	-0.070	-0.264*
	(0.030)	(0.033)	(0.023)	(0.082)	(0.083)	(0.155)	(0.152)	(0.196)	(0.103)	(0.152)
8	-0.010	-0.041	-0.019	0.094	0.016	-0.348**	-0.366**	-0.535**	-0.150	-0.148
	(0.024)	(0.026)	(0.017)	(0.083)	(0.053)	(0.168)	(0.171)	(0.214)	(0.169)	(0.166)
9	-0.014	-0.041**	-0.024*	-0.067	-0.002	-0.562***	-0.731***	-0.743***	-0.150	-0.401*
	(0.017)	(0.018)	(0.014)	(0.074)	(0.053)	(0.183)	(0.192)	(0.213)	(0.204)	(0.238)
10	-0.014*	-0.040***	-0.021***	-0.084	-0.086	-0.593***	-0.734***	-0.690***	-0.882***	-0.795***
	(0.008)	(0.008)	(0.007)	(0.072)	(0.054)	(0.150)	(0.157)	(0.153)	(0.262)	(0.205)
Avg Effect	0.045	-0.021	-0.008	0.103	0.129*	-0.334*	-0.492**	-0.562**	0.002	0.016
	(0.034)	(0.041)	(0.028)	(0.096)	(0.075)	(0.170)	(0.218)	(0.246)	(0.049)	(0.067)
Mean	0.927	0.935	0.958	0.820	0.902	0.368	0.370	0.468	0.153	0.261

Notes: Each coefficient is an IV estimate of β from equation (1), including the 1940 and 1980 control variables. Each row corresponds to a decile of parental income; the penultimate row is the estimate among all people. The final row shows the overall mean of the dependent variable. The dependent variables and samples are listed at the top of the table and described in Section 6.3. Standard errors are clustered by commuting zone and shown in parentheses. Stars denote statistical significance at the 0.1 (*), 0.05 (**), and 0.01 (***) levels.

Table 6: Effects of Immigrant Inflows on Migration

	Change CZ by Age 16	Change CZ by Adult	Difference in CZ W2 Rank
		All People	
Decile	(1)	(2)	(3)
1	1.244***	1.683***	-7.153*
	(0.167)	(0.297)	(4.313)
2	1.252***	1.743***	-7.179*
	(0.158)	(0.277)	(3.899)
3	1.427***	1.915***	-6.471
	(0.168)	(0.294)	(4.249)
4	1.433***	1.887***	-4.616
	(0.174)	(0.291)	(3.804)
5	1.380***	1.803***	-2.082
	(0.176)	(0.292)	(3.763)
6	1.373***	1.809***	-0.215
	(0.193)	(0.286)	(3.570)
7	1.359***	1.828***	0.531
	(0.204)	(0.283)	(3.262)
8	1.345***	1.790***	2.550
	(0.214)	(0.283)	(3.187)
9	1.168***	1.576***	5.492*
	(0.182)	(0.268)	(3.204)
10	0.717***	0.845***	9.285***
	(0.153)	(0.241)	(2.699)
Avg Effect	1.210***	1.596***	-0.105
	(0.156)	(0.257)	(3.481)
Mean	0.292	0.497	-0.366

Notes: Each coefficient is an IV estimate of β from equation (1), including the 1940 and 1980 control variables. Each row corresponds to a decile of parental income; the penultimate row is the estimate among all people. The final row shows the overall mean of the dependent variable. The dependent variables and sample are listed at the top of the table and described in Section 6.4. Standard errors are clustered by commuting zone and shown in parentheses. Stars denote statistical significance at the 0.1 (*), 0.05 (**), and 0.01 (***) levels.

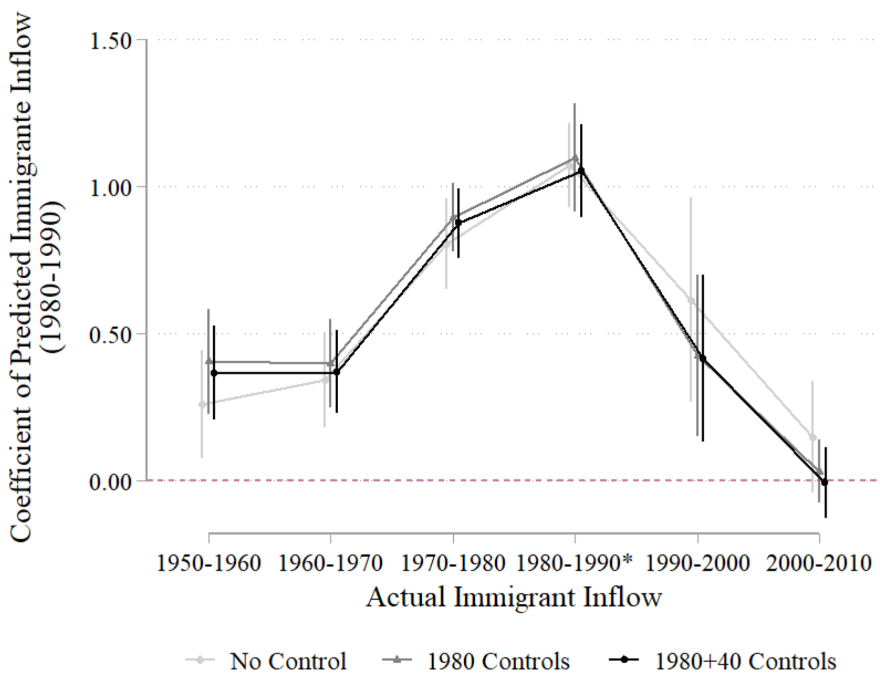
Table 7: Effects of Immigrant Inflows on Marriage and Number of Dependents

	Married (Tax Filing)					Dependents>0	# of Dependents
	All People	White Male	White Female	Black Male	Black Female	All People	All People
Decile	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1	0.368***	0.291***	0.313***	0.363***	0.263***	0.022	0.258
	(0.060)	(0.075)	(0.070)	(0.088)	(0.092)	(0.054)	(0.223)
2	0.405***	0.321***	0.297***	0.441***	0.379***	0.011	0.251
	(0.063)	(0.070)	(0.068)	(0.101)	(0.096)	(0.058)	(0.225)
3	0.379***	0.230***	0.258***	0.497***	0.456***	-0.018	0.179
	(0.066)	(0.078)	(0.070)	(0.114)	(0.106)	(0.065)	(0.278)
4	0.282***	0.173***	0.164***	0.459***	0.406***	-0.029	0.167
	(0.058)	(0.064)	(0.061)	(0.112)	(0.103)	(0.063)	(0.302)
5	0.167**	0.100	0.081	0.347***	0.321***	-0.023	0.301
	(0.065)	(0.074)	(0.066)	(0.089)	(0.115)	(0.072)	(0.316)
6	0.108*	0.078	0.051	0.339***	0.263***	0.010	0.341
	(0.063)	(0.065)	(0.065)	(0.095)	(0.096)	(0.075)	(0.341)
7	0.037	0.014	-0.026	0.339**	0.212	0.039	0.489
	(0.073)	(0.077)	(0.070)	(0.134)	(0.139)	(0.082)	(0.371)
8	-0.003	-0.011	-0.057	0.181*	0.287**	0.045	0.458
	(0.064)	(0.069)	(0.060)	(0.102)	(0.133)	(0.078)	(0.390)
9	-0.100	-0.114*	-0.126*	0.130	0.156	0.037	0.414
	(0.067)	(0.062)	(0.067)	(0.159)	(0.122)	(0.075)	(0.376)
10	-0.167***	-0.185***	-0.159***	-0.003	-0.032	-0.043	0.104
	(0.036)	(0.040)	(0.034)	(0.100)	(0.147)	(0.055)	(0.317)
Avg Effect	0.108**	-0.014	-0.026	0.394***	0.347***	0.005	0.312
	(0.049)	(0.051)	(0.047)	(0.097)	(0.101)	(0.066)	(0.316)

Notes: Each coefficient is an IV estimate of β from equation (1), including the 1940 and 1980 control variables. Each row corresponds to a decile of parental income; the penultimate row is the estimate among all people. The final row shows the overall mean of the dependent variable. The dependent variables and sample are listed at the top of the table and described in Section 6.5. Standard errors are clustered by commuting zone and shown in parentheses. Stars denote statistical significance at the 0.1 (*), 0.05 (**), and 0.01 (***) levels.

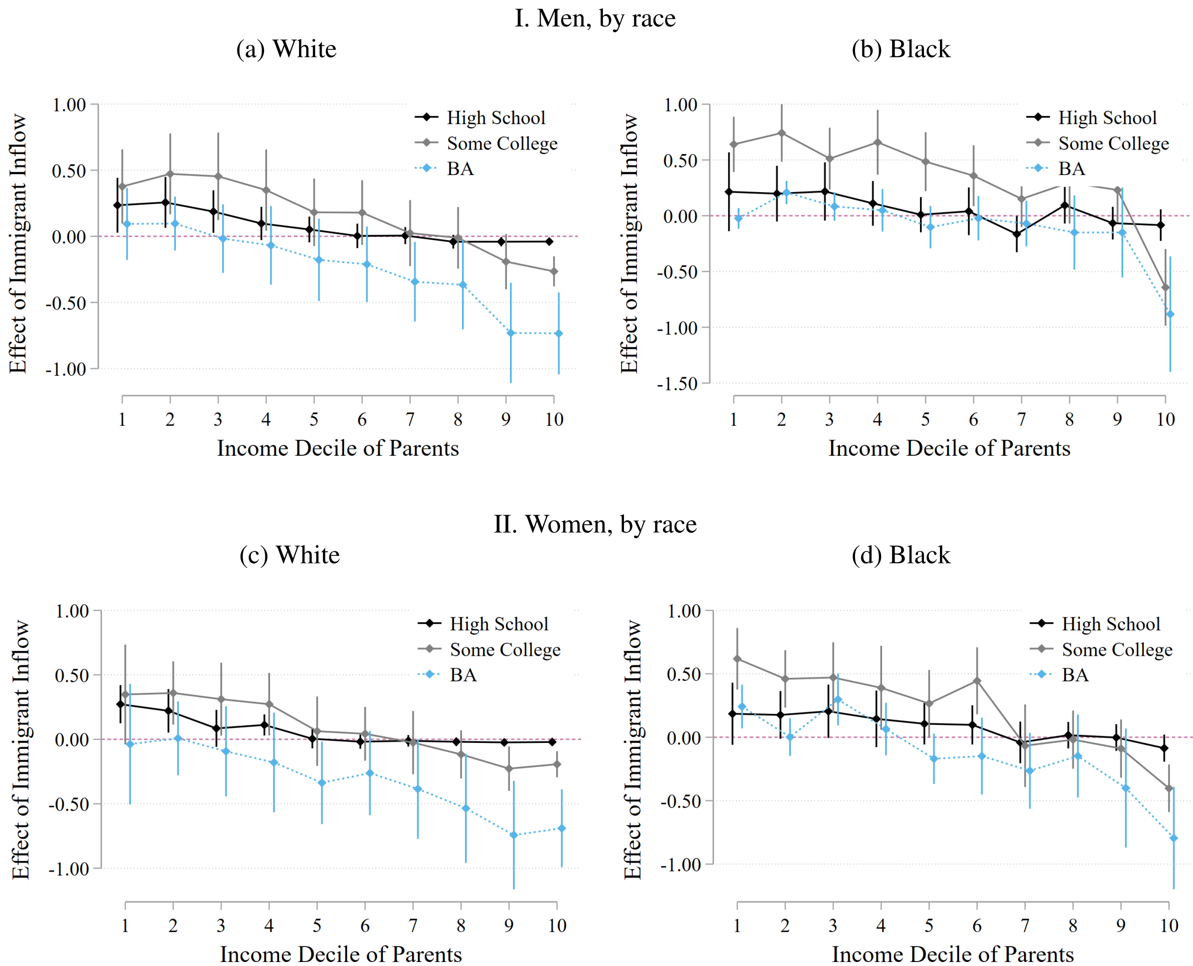
Appendix Figures and Tables

Figure A1: Correlation Between Predicted Immigrant Inflows (1980-1990), Past, and Future Inflows



Notes: Figure plots the coefficients from the first stage regressions, where the dependent variable is the actual immigrant inflow in various decades (x-axis) and the independent variable is the predicted immigrant inflow from 1980 to 1990 (the instrument *z*). The vertical lines represent 95 percent confidence intervals. The light gray line corresponds to regressions with no controls. The dark gray line represents regressions that include 1980 controls (the logarithm of the commuting zone population, the share of college graduates, and the share of workers employed in manufacturing). The black line incorporates the full set of controls, including both the 1980 and 1940 controls.

Figure A2: Effects of Immigrant Inflows on Education



Note: The outcome variable is the indicator variable for completing high school (black line), having some college experience (gray line), and obtaining BA degree (blue line). See note to Figure 5 for additional details. Coefficient estimates and standard errors are presented in Table A3.

Table A1: First-Stage Results by Race and Parent Income Decile

Decile	All (1)	White (2)	Black (3)
1	1.037***	1.088***	1.065***
	(0.084)	(0.060)	(0.062)
2	1.018***	1.086***	1.064***
	(0.088)	(0.059)	(0.066)
3	1.021***	1.089***	1.048***
	(0.087)	(0.059)	(0.070)
4	1.036***	1.090***	1.044***
	(0.084)	(0.059)	(0.069)
5	1.046***	1.088***	1.045***
	(0.081)	(0.060)	(0.068)
6	1.053***	1.090***	1.044***
	(0.077)	(0.059)	(0.070)
7	1.061***	1.092***	1.037***
	(0.074)	(0.058)	(0.073)
8	1.063***	1.088***	1.042***
	(0.072)	(0.060)	(0.069)
9	1.060***	1.081***	1.045***
	(0.072)	(0.062)	(0.066)
10	1.063***	1.074***	1.052***
	(0.068)	(0.064)	(0.058)
Average	1.046***	1.084***	1.051***
	(0.079)	(0.061)	(0.067)

Notes: Each coefficient is an estimate of a first-stage regression, where the dependent variable is the actual immigrant inflow rate in an individual's CZ and the independent variable is the instrument. The 1940 and 1980 controls are included in all regressions. Standard errors are clustered by commuting zone and shown in parentheses. Stars denote statistical significance at the 0.1 (*), 0.05 (**), and 0.01 (***) levels.

Table A2: Immigrant Inflows in 1980-1990 and Income Mobility of Native Children in 1940

	1940 W2 Income Rank	
	No 1980 Controls	With 1980 Controls
Decile	(1)	(2)
1	0.309	-0.120
	(0.291)	(0.165)
2	0.145	-0.201
	(0.310)	(0.165)
3	0.101	-0.151
	(0.221)	(0.134)
4	0.196	-0.025
	(0.152)	(0.114)
5	0.161	-0.038
	(0.131)	(0.107)
6	0.187	0.055
	(0.130)	(0.127)
7	0.177	0.076
	(0.115)	(0.125)
8	0.164*	0.066
	(0.096)	(0.109)
9	0.120*	0.064
	(0.071)	(0.093)
10	0.080	-0.041
	(0.082)	(0.075)

Notes: Each coefficient is an IV estimate of β from equation (1), as described in Section 5.3. The dependent variable is the W2 income rank using the 1940 Census data. Column (1) has no controls. Column (2) includes the 1980 CZ-level controls. Each row corresponds to a decile of parental income. Standard errors are clustered by commuting zone and shown in parentheses. Stars denote statistical significance at the 0.1 (*), 0.05 (**), and 0.01 (***) levels.

Table A3: Effects of Immigrant Inflows on College Attendance

	Some College or More			
	White Male	White Female	Black Male	Black Female
Decile	(1)	(2)	(3)	(4)
1	0.377***	0.348*	0.640***	0.618***
	(0.142)	(0.196)	(0.126)	(0.123)
2	0.472***	0.359***	0.742***	0.460***
	(0.155)	(0.124)	(0.131)	(0.115)
3	0.454***	0.311**	0.512***	0.470***
	(0.168)	(0.143)	(0.141)	(0.141)
4	0.349**	0.272**	0.658***	0.389**
	(0.156)	(0.123)	(0.147)	(0.168)
5	0.182	0.063	0.485***	0.266**
	(0.129)	(0.136)	(0.134)	(0.134)
6	0.179	0.043	0.358**	0.445***
	(0.124)	(0.106)	(0.138)	(0.134)
7	0.024	-0.026	0.151	-0.067
	(0.127)	(0.124)	(0.128)	(0.165)
8	-0.011	-0.117	0.298	-0.019
	(0.118)	(0.095)	(0.187)	(0.116)
9	-0.191*	-0.227**	0.231	-0.089
	(0.106)	(0.088)	(0.149)	(0.116)
10	-0.265***	-0.194***	-0.642***	-0.402***
	(0.058)	(0.051)	(0.174)	(0.095)
Average	-0.091	-0.118	0.492***	0.357***
	(0.147)	(0.128)	(0.088)	(0.090)

Notes: Each coefficient is an IV estimate of β from equation (1), including the 1940 and 1980 control variables. Each row corresponds to a decile of parental income; the final row is the estimate among all people. The dependent variable is whether the individual has at least one year of college. The samples are listed at the top of the table and described in Section 6.3. Standard errors are clustered by commuting zone and shown in parentheses. Stars denote statistical significance at the 0.1 (*), 0.05 (**), and 0.01 (***) levels.

Table A4: Correlation between W2 Rank, Employment, and Education

	(1)	(2)	(3)
Panel A: W2 Income Rank			
High School Completion	0.726***		
	(0.049)		
BA Completion	0.243***		
	(0.010)		
Employment (W2 > Minimum Wage)		0.932***	
		(0.057)	
Employment (W2 > 0)			1.483***
			(0.114)
N	40	40	40
Panel B: Employment (W2 > Minimum Wage)			
High School Completion	1.319***		
	(0.065)		
BA Completion	0.079***		
	(0.028)		
N	40		
Race by Sex Fixed Effects	Y	Y	Y

Notes: The table presents the correlation between the mean W2 rank, employment, and education, where the unit of observation is decile-race-sex. Race and ethnicity are categorized into two groups: non-Hispanic white and non-Hispanic black. In Panels A and B, the dependent variables are the W2 income rank and employment (defined as having W2 income greater than the minimum wage). The explanatory variables are listed in the row titles. All columns include race-by-sex fixed effects. The regressions are weighted by the number of observations in each cell. Robust standard errors are in parentheses. Stars denote statistical significance at the 0.1 (*), 0.05 (**), and 0.01 (***) levels.

A Data Appendix

We use child outcome estimates from Rothbaum and Massey (2022).[30] In this section, we provide a brief, self-contained, discussion of that data and how those results were estimated and disclosed. Rothbaum and Massey (2022) estimate the expected child outcome for the following set of subgroups:

1. Parent earnings decile,
2. Child age in 1940 (0-6, 7-12, 13-18),
3. Race of the child (Black and White) or foreign-born status of parents (any foreign-born parent and no foreign born parent),
4. Sex, and
5. County and CZ).

As an example, they estimated the expected child outcome for a 13-18 year-old, White, daughter of 3rd decile parents in Marion County, Indiana.

They utilized a special crosswalk constructed for the 1940 census to link individuals to their Social Security Numbers (SSNs), and subsequently the Protected Identification Key (PIK) used to facilitate linkages to other survey and administrative datasets. Massey et al. (2018) detail the linkage process for the 1940 decennial census, which was unique for two reasons. First, the Census Bureau does not possess a file that contains information on location of residence as far back as 1940, making linkage more difficult than in more recent data where residential location can be used to improve linkage. Second, the 1940 census contains information that can aid in linkage that is not traditionally used in the linkage process. In particular, Massey et al. (2018) use the parents' names for children co-residing with their parents in 1940. As a result, despite an overall linkage rate of 40 percent for the 1940 census, 70 percent of children were linked.

As noted in Rothbaum and Massey (2022), there are several reasons children may not receive a PIK in the 1940 census. Their information may not be sufficiently unique (name, age, birth location, etc.) to cross the pre-defined linkage threshold in the PVS process used by Massey et al. (2018). Additionally, it is possible that unmatched children never received an SSN. Until relatively recently, SSNs were not assigned at birth.[31] Instead, workers received SSNs when they started a job that was covered by the Social Security system. This excluded large classes of jobs until the late 1970s. Furthermore, the Numident, which forms the backbone of the linkage reference file, was not created until 1972.[32]

To construct those estimates, they linked the 1940 census to 1040 filings in tax years 1974 and 1979. The 1940 census asked *all* individuals 14 years old and above about their income. However, the questions were less detailed than modern large-scale surveys, such as the American Community Survey. Individuals were asked about the "Amount of money, wages or salary received (including commissions)" in 1939. They were also asked "Did this person receive income of $50

[30] The discussion in this section follows Garin and Rothbaum (2022) closely.

[31] Several states piloted SSN assignment at birth in 1987, with implementation in most states by 1991.

[32] More information on the history of SSNs can be found at `https://www.ssa.gov/policy/docs/ssb/v69n2/v69n2p55.html`, accessed 5/26/2021.

or more from sources other than money, wages or salary (Y or N)?" This second question covers self-employment earnings, farm self-employment earnings, property income (such as from interest or dividends), rental income, etc.

Rothbaum and Massey (2022) find that 65 percent of children have at least one parent with a reported value for wage and salary earnings and other income is reported for 36 percent of their fathers and 9 percent of their mothers. As they note, the available data on parent earnings presents several challenges to measuring intergenerational mobility. First, even for those parents with wage and salary earnings, we observe survey-reported earnings in only one year. Therefore, many of the known measurement issues in the intergenerational mobility literature will be present in this data, including life-cycle bias, bias from transitory shocks, survey-reporting error, etc.[33] Second, for a non-random subset of the population, such as farmers and the self-employed, we do not observe some or all of their earnings, but instead only whether those earnings were $50 or more. Without other data on earnings and income for the parent cohort, these issues will likely attenuate measures of the persistence of status across generations.

Furthermore, not all children are assigned a PIK. Although a 70 percent linkage rate is high by historic standards,[34] there is still a substantial share of the sample that cannot be linked to a PIK and therefore cannot be found in the 1970's tax returns.

Rothbaum and Massey (2022) include children in their analysis sample if their parents reported any wage and salary earnings and the child was assigned a PIK. However, there is substantial selection into that sample. For example, the analysis sample is more likely to be White and has more educated and younger parents. About 19 million children, slightly under 50 percent of all children in the 1940 census, are in the final analysis sample.

To adjust the sample for selection on observable characteristics, to the extent possible, into the analysis sample, they created inverse probability weights (IPW). The weights condition on 1) child characteristics: race, sex, school attendance, and income as an adult (controlling for both an indicator for nonzero income and rank), 2) parent characteristics: age, citizenship, education, log of wage and salary earnings, wage and salary and self-employment earnings dummies, number of children in the family, 3) location characteristics: urban/rural status of the household, and 4) location summary information at the county and CZ level. All child outcomes statistics in their paper are estimated using the IPW adjustment.

Additionally, for disclosure protection, the baseline (county) x (parent earnings decile) x (age) x (sex) x (race or parent nativity) estimates had $(\varepsilon, \delta)-$differentially private Gaussian noise added (Nissim, Raskhodnikova and Smith, 2007) with $\varepsilon = 2$ in each estimate. The county-level, disclosure protected estimates can be aggregated up, including to the CZ and national levels and pooled by sex, age, and foreign-born status to create additional estimates.

[33] See Black and Devereux (2011) for a discussion of measurement error and estimates of intergenerational mobility.

[34] See Bailey et al. (2017) for match rates in various historical data linkage projects