The Short-Run Impacts of Immigration on Native Workers: A Sectoral Approach

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Abstract

We use imperfect instruments (Nevo and Rosen, 2012) to derive new bounds for the short-run impacts of immigration on the labor market outcomes of US-born workers. We focus on six lower-skill economic sectors with high immigrant penetration and instrument for the sectoral immigrant share using the immigrant share in other sectors. We uncover negative effects on native earnings in the construction, food service, and personal service sectors, with upper bounds ranging from -2.9% to -6.6% for each 10 percentage point increase in the immigrant share. Effects on the native employment rate are negative and significant across all six sectors.

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1 Introduction

The impact of immigration on the labor market outcomes of native-born citizens is, or at least ought to be, a key element of the debate on immigration policy (Scheve and Slaughter, 2001; Borjas, 2017). There is a long tradition of empirical work on this question in labor economics, starting with the seminal work of Grossman (1982), followed by influential contributions by Card (1990), Altonji and Card (1991), Friedberg and Hunt (1995), Borjas et al. (1997), Card (2001), Borjas (2003), Card (2009), Peri and Sparber (2009), Ottaviano and Peri (2012), and Dustmann et al. (2017), to name a few. However, there is little agreement among empiricists about the magnitude, or even the sign, of the effect of increased immigration on the labor market outcomes of native-born workers (Basso and Peri, 2015; Dustmann et al., 2016).

This paper provides new estimates of the short-run impacts of immigration on the employment conditions of US-born workers based on a fixed-effects panel regression of US metropolitan areas spanning the years 1990-2011, a period during which the US experienced a remarkable increase in immigration. We use a novel partial identification strategy that has not been exploited in the related literature to date. Our approach requires estimating immigration impacts at a sectoral, rather than economy-wide, level. While this restriction may be seen by some as a weakness, it allows us to paint a contrasted picture of immigration impacts across sectors of the US economy with both high immigrant worker penetration and low skill requirements: construction, transportation, manufacturing, maintenance, food service, and personal services.[1] Together, these sectors have employed 30% of the total native workforce and 42% of the low-skilled native workforce over the period 1990-2011.[2]

The sectoral approach yields upper bounds for the short-run impacts of immigration on native workers' earnings, occupational levels, and employment rate. In the personal service, food service, and construction sectors, upper bounds on earnings are typically negative, statistically significant, and of much larger magnitude than recently published estimates for the US economy as a whole, suggesting that there exist transitory costs to immigration for part of the native population. We find that a 10 percentage point increase in the share of immigrant workers in personal services (resp. food service, resp. construction), which is less than the increases that occurred over the period of investigation, causes at least a 6.6%, (resp. 6.0%, resp. 2.9%) drop in the annual earnings of natives. These effects are generally more pronounced for occupations within these sectors most exposed to immigrant inflows. For example, when focussing on workers within personal services (resp. construction) in occupations with the highest immigrant shares, we find effects about twice as large as those for the sector as a whole.

Results in the remaining three sectors are more nuanced. Although our point estimates suggest negative effects on the annual earnings of natives in maintenance and transportation, these effects

[1]We do not look at agriculture, mostly because our dataset focusses on workers located in metropolitan areas. Our identification strategy does not make much sense to analyze immigration impacts in high-skilled sectors like computer engineering, where most of the immigrant workforce is invited to the US through targeted work visas.

[2]Here we define "low-skilled" as having no more than a high school diploma. Our empirical analysis includes native workers with any level of educational achievement.

are only statistically significant once we focus on more disaggregated portions of these sectors. In the maintenance sector, we find significantly negative effects for natives in occupations related to landscaping; interestingly the effect on these workers is of comparable magnitude as that found in the immigrant-exposed occupations of the construction sector such as roofers or painters. In the transportation sector, which includes such varied occupations as aircraft pilots, boat operators, or garbage collectors, we find significantly negative effects in immigrant-exposed occupations like drivers or loaders. We do not find significant earnings effects in the manufacturing sector, likely due to the traded nature of the goods produced.

An important insight of our analysis is that annual earnings effects, where present, may be partly driven by reductions in the occupational levels of natives, i.e., fewer weeks worked per year. This is particularly true for construction occupations, as well as immigration-exposed personal service occupations such as child and personal care. In these occupations, income is often earned "per job" and workers compete for jobs, sometimes through a formal bidding process. Such occupations also have high rates of self-employment, and work may be undeclared. To the extent that immigrant workers, some of which are illegal, have a preference for work unreported to the government or are willing to accept lower pay, they cost less to employers and may be in a position to displace natives.[3] In construction, where the effects of immigration on weeks worked are the most evident, we find that a 10 percentage point increase in the immigrant share causes at least a 2.1 percentage point decrease in the share of native workers working 40 weeks or more per year. The effects are again larger in the more immigrant-exposed construction occupations (3.9 percentage points).

In line with earlier literature, our results on earnings are derived conditioning on workers earning a strictly positive income (among other criteria). We show that immigration has also had sizable effects on native workers' employment rate in all six sectors considered, including manufacturing. Again the largest effects are found in the construction, food service, and personal services sectors where a 10 percentage point increase in the share of immigrants causes at least a 2.3 (resp. 1.8, resp. 1.7) percentage point decrease in the employment rate of natives when considering the entire sector and a 3.6 (resp. 1.8, resp. 3.0) percentage point decrease when focussing on immigrant-exposed occupations.

Generally, the empirical identification of the immigration-native-outcome relationship is fraught with difficulty. In an ideal experiment, one could observe two *identical* but *disconnected* cities, one of which would receive an influx of immigrants and the other not. By comparing labor outcomes between these cities, one could deduce the impact of additional immigration on the employment conditions of native-born workers. Unfortunately, such exogenous influxes of immigrants rarely occur in practice.

[3]The construction sector in particular is notorious for having high rates of "under the table" employment (i.e., workers paid in cash without reporting employment to the government). Contractors engaging in this type of employment have a competitive advantage over others who strictly employ workers "on the books" because of the additional cost of workman's compensation, unemployment insurance, and other payroll taxes, which must be factored into bids (Fishman, 2013).

First, cities are not identical. Immigrants sort into locations, supposedly following employment opportunities. Locations with better opportunities for immigrant workers are plausibly those where demand for labor is higher, potentially confounding the effect of immigration on native wages or employment. That is, a positive estimate of the impact of immigration on native labor outcomes might simply reflect unobserved demand pulls that increase both immigration and native employment and/or wages. This omitted variable bias is perhaps the main threat to identification of the relevant immigration effect. It affects cross-sectional and time-series/panel approaches alike: just as cities with relatively high native wages may attract immigrants, periods when native wages are high due to factors other than immigration may coincide with times of increased immigration.

Studies based on city comparisons may suffer from an additional problem: cities are not disconnected. To the extent that native-born workers are displaced into areas less affected by immigration, their movement depresses local wages and works towards wage equalization across cities: comparing native wages between immigration-affected and immigration-free areas may then reveal an absence of wage effect. Even if native workers do not relocate, cities may trade goods and capital. Under constant returns to scale in production and identical technologies, if output price and the price of capital are equated across cities, so will local wages. Of course, this does not imply that native workers are not hurt by immigration: in addition to the costs of possible relocation, the new equilibrium wage, while equalized across space, might still be lower than it would have been without immigration.[4]

The spatial correlation literature, which exploits naturally occurring variation in immigrant inflows across geographical labor markets, has resorted to instrumental variables approaches in order to correct the first source of bias identified above. In a first-difference panel model with two periods, Altonji and Card (1991) use the share of immigrants in the total population of a city in the baseline period as an instrument for the increase in the immigrant share in that city, based on the idea, borrowed from the work of Bartel (1989), that immigrants are attracted to places with large concentrations of previous immigrants. Card (2001) later refines the instrument by differentiating immigrants by country of origin and interacting the fraction of immigrants from a country who are observed living in a city in the reference period by the *national* inflow of immigrants from that country in the current period (and then summing up across origin countries). The instrument thus represents the total influx of immigrants in the current period that would be obtained if new influxes were perfectly correlated with the geographical distribution in the reference period. This approach, sometimes referred to as the "supply push," "shift-share," "past settlement," or "immigrant network" instrument, has been a staple in the branch of the immigration literature seeking to correlate immigrant inflows to native labor outcomes. Nonetheless, many authors have questioned the validity of the shift-share instrument due to the possible spatial correlation between initial immigrant settlement patterns and subsequent growth in employment opportunities (e.g., Reed and Danziger (2007); Borjas (2014); Basso and Peri (2015), and more recently Goldsmith-

[4]See Appendix B for a proof of this claim. We also show that under relatively weak assumptions, movement of capital alone partially masks the wage effect of immigration when the good is not traded across cities.

Pinkham et al. (2019)).

Perhaps more importantly, a recent paper by Jaeger et al. (2018) demonstrates that estimates obtained from the shift-share instrument conflate short-run (negative) impacts with long-run recovery processes whenever there is limited change in the composition of immigrant inflows at the national level over time, as has been the case in the US since the 1980s. According to the authors, the only time period in the US where the shift-share instrumental variable approach—or the improved strategy they propose—may be successfully leveraged is the decade 1970-1980, which saw a considerable shift in the country-of-origin composition of US immigration inflows due to the enactment of the Immigration and Nationality Act of 1965. Although they find evidence of negative short-run impacts of immigration on natives' wages based on this earlier period, it is not clear whether these impacts can be extrapolated to current conditions, due e.g. to the secular increase in immigration and the fact that effects may not be globally linear.

Building upon Card's seminal insights regarding the effects of the Mariel boatlift on employment in Miami (Card, 1990), another branch of literature has tried to exploit assumingly exogenous variation in the timing, geographical distribution, or skill composition of immigration shocks induced by policy changes (e.g., recently, Glitz (2012); Beerli and Peri (2015); Foged and Peri (2016); Dustmann et al. (2017)). A possible criticism of such event studies may be the lack of external validity associated with the often anecdotal nature of the variation used in identification.

The second source of bias—spatial arbitrage—is addressed in an influential study by Borjas (2003). The author exploits plausibly exogenous variation in the skill composition of immigrant influxes at the national level over time to identify immigration impacts. Because the approach considers the relevant labor market to be national in scope, it is not subject to the caveat that control cities may be indirectly affected by immigration. However, since it includes time effects to control for shocks common to all skill groups, the estimate obtained from the national skill approach only reflects the *relative* impact of immigration on labor outcomes *across* different skill groups (Ottaviano and Peri, 2012; Dustmann et al., 2016). This suggests a fundamental tradeoff between the bias arising from spatial arbitrage in spatial correlation studies and that arising from ignoring effects common to all skill groups in skill composition studies.[5]

In this paper, we leverage a novel partial identification method formalized by Nevo and Rosen (2012) to address the effect of increased immigration on the employment opportunities of native-born workers in the context of the spatial correlation approach. Our partial identification strategy relies on the use of a series of so-called "imperfect instruments:" instruments for the sectoral immigrant share in a given city and year that, although still potentially correlated with the error term (unobserved demand shocks about city and year averages), are plausibly less correlated with it than the regressor itself, albeit in the same direction. In this sense, they represent imperfect instrumental variables or IIVs. Because of the remaining correlation, which violates the exclusion restriction, the IIV estimate is biased. However, Nevo and Rosen (2012) show that under certain

[5] A variant of the skill composition approach, coined as the "mixture approach" by Dustmann et al. (2016), uses variation in the skill composition trends of immigration across spatial units in a triple-difference setting. This approach also delivers a *relative* impact measure across skill groups while being subject to potential bias from spatial arbitrage.

conditions, the IIV estimate can be used as a lower or upper bound to the coefficient of interest.[6] We use their insights to derive upper bounds for the negative effects of immigration on native employment conditions over the period 1990-2011. Because our approach relies on spatial differences, it delivers estimates that are also possibly subject to spatial-arbitrage bias. But since both sources of bias (imperfect instrument and spatial arbitrage) work in the same direction, our estimates are conservative in nature. Nonetheless, we find that in the food service and personal service sectors, immigration impacts are negative, statistically significant, and larger in magnitude than comparable estimates derived in the US context for recent decades. Once we focus on immigrant-exposed occupations, we also find evidence of large negative effects in the construction sector. In these sectors our estimates for earnings effects are consistent with the latest figures derived by Jaeger et al. (2018) for the earlier decade 1970-1980. Our estimate for the construction sector appears consistent with that derived by Bratsberg and Raaum (2012) for the Norwegian constructor sector.[7]

In terms of empirical implementation, the dual requirement that the correlation between the IIV and the error term be of the same sign as, but of a lower magnitude than, the correlation between the regressor and the error term does have a cost.[8] Our approach focusses on one sector of the economy at a time in order to use as an IIV for the sectoral share of immigrants the share of immigrants *across all sectors*, or *across all other sectors*. These instruments are plausibly correlated with demand pulls that affect native employment/earnings in the sector of interest in the same direction as the sectoral immigrant share: economic booms attract immigrants across all sectors, and they increase employment opportunities for natives in any given sector. However, since the immigrant share pertains to the entire economy (or the rest of the economy), it is likely less correlated with the sectoral demand pulls than the sectoral immigrant share itself. Our IIV estimates, which are typically much more negative than the OLS estimates, confirm this intuition. Our result that the immigrant share has a negative effect on natives' employment rate across all sectors considered also provides some evidence that labor may not be completely mobile across sectors in the short run, underscoring the relevance of a sectoral approach.

Our paper contributes to the literature on the impacts of immigration on the employment conditions of natives in several ways. First, we deploy a novel instrumental variable strategy that represents an alternative to the much criticized shift-share instrument in the context of the spatial correlation approach. Our strategy acknowledges the inherent remaining correlation between our instrument and unobserved sectoral demand shocks but leverages it to derive an upper bound on

[6]They also show how one may derive two-sided bounds, but for reasons highlighted below, our setting does not allow such derivation.

[7]Bratsberg and Raaum (2012) rely on differences in immigrant shares *across* construction trades, rather than intercity comparisons. As such, the interpretation of their estimate differs from ours: whereas our estimate can be interpreted as capturing the *total* sectoral effect of immigration on native outcomes—assuming away spatial arbitrage—theirs represents a *relative* effect across construction trades. Following the argument of Dustmann et al. (2016), our estimate is closer to the relevant effect because it encompasses effects of immigration that are common to all construction trades.

[8]One may argue that the shift-share instrument discussed above already constitutes an IIV. In some studies, like Dustmann et al. (2005), the use of the shift-share instrument actually results in a less negative impact of immigration. In others (e.g., Reed and Danziger (2007); Basso and Peri (2015); Jaeger et al. (2018)) the estimate becomes more negative but the change is minimal, suggesting that the IIV correlation with the error term remains high in comparison with that of the regressor.

the negative impacts of immigration on natives' employment conditions. Second, we are able to produce estimates of immigration impacts for a relatively recent period, whereas Jaeger et al. (2018) show that the shift-share instrument approach may only produce reliable impacts for the period 1970-1980 in the US context. Third, in spite of the fact that spatial correlation estimates may mask larger national effects (Borjas, 2003), several of our estimated effects are larger in magnitude than most recent estimates for the US, suggesting that natives can be hurt by immigration in the short run. Fourth, the sectoral approach allows us to provide a nuanced picture of immigration impacts across the economic sectors most exposed to immigration.[9] We relate our findings to critical differences across sectors and occupations regarding goods tradability, immigrant penetration, and skill requirements.

The rest of the article is organized as follows. Section 2 discusses recent immigration trends in the sectors we investigate. Section 3 describes our data sources. Section 4 describes the IIV strategy we deploy, building upon the work of Nevo and Rosen (2012). Section 5 discusses our results, and Section 6 concludes.

2 Background

Since the enactment of the Immigration and Nationality Act of 1965, the US has experienced a remarkable increase in immigration, with the share of foreign-born individuals in the total population increasing from 4.7% in 1970 to 13.4% in 2015 (López and Bialik, 2017). There were 27,400,000 foreign-born (immigrant) individuals in the US labor force in 2017, representing 17.1% of the total labor force (U.S. Department of Labor, Bureau of Labor Statistics, 2018). Construction and extraction occupations attracted 9.3% of employed immigrant workers, making these occupations the single category with the highest number of immigrants, and one with an immigrant share of 30.4%. Building and grounds cleaning and maintenance occupations employed 8.4% of immigrant workers, and the immigrant share in that sector reached 37.4%.

[Figure 1 about here.]

Low-skilled sectors of the economy with high immigrant penetration, as defined in this paper, have seen a remarkable increase in the share of immigrant workers in the sectoral workforce (Figure 1). According to our data (see Section 3), between 1990 and 2011 the share of immigrants in the construction sector has increased from 10% to 26% while that in the maintenance sector has increased from 18% to 37%. Other sectors have seen comparable trends. To the extent that the increase in the sectoral share of immigrant workers has not been uniform across geographical labor markets, this pronounced trend represents an opportunity to empirically identify immigration

[9] The idea that the effects of immigration on native workers may be more pronounced in industries with high immigrant share was recently explored by Dustmann et al. (2017) in their study of a commuting policy along the German-Czech border. Their main analysis considers all industries together as the spatial variation in immigrant inflows that they exploit cannot address the selection of immigrant workers into industries experiencing positive labor demand shocks. Nonetheless, the results they report in Appendix V.D suggest larger negative effects on wages and employment in immigrant-exposed industries.

impacts on the employment conditions of native workers while controlling for common national shocks such as recessions or business cycles.

[Figure 2 about here.]

The evolution of annual earnings of native workers at the national level is depicted in Figure 2 for the period 1990-2011. The figure shows a clear clustering of earnings across the six sectors considered. While the earnings of construction, transportation, and manufacturing workers cluster around $40,000, those of maintenance, food service, and personal service workers are much lower, around $20,000. As depicted in Appendix Figure A.1, trends in estimated weekly earnings tell a very similar story.

Appendix Figure A.2 depicts annual earnings for native workers in occupations with the highest immigrant shares, by sector. When compared to Figure 2, the figure shows that immigrants tend to select into lower-paying occupations within each sector.

[Figure 3 about here.]

Figure 3 depicts the evolution of employment by sector over the period 1990-2011. The figure shows the early effects of the US subprime mortgage crisis on employment in the construction sector, followed by cascading effects on employment in other sectors during the Great Recession.

[Figure 4 about here.]

Note that it is difficult to relate the immigrant share to native earnings or employment by simply looking at national-level aggregates. The immigrant share shows a clear upward trend over the period. Earnings appear relatively stable while employment seems to be mostly driven by macroeconomic factors. Indeed, Figure 4 shows the unemployment rate over time for the entire US economy from the Bureau of Labor Statistics. Consistent with our sectoral data, unemployment increased after 2001, and then again after 2008. Importantly, our empirical strategy nets out any common national effects through year fixed effects and relies on differences across MSAs in the evolution of the immigrant share about the MSA average.

3 Data

The data used for this analysis were obtained from the Integrated Public Use Microdata Series (IPUMS) provided by the University of Minnesota (Ruggles et al., 2017). These data include US Census data from the 1990 5% State sample and 2000 5% sample as well as American Community Survey (ACS) data between the years 2001 and 2011. Due to a missing geographic variable ("metarea") used to assign a location to workers, the years 2001, 2002, and 2004 are excluded from our data set, and our analysis does not extend beyond 2011. Our analysis is conducted separately for several sectors of the economy as identified by the Census Bureau's 2010 classification using the variable “occ2010.” This variable provides a “consistent, long-term classification of occupations” (Ruggles et al., 2017), which identifies sectors of the labor market as well as individual occupations within each sector.

Our analysis is conducted on the following sectors of the US economy: Food Preparation and Serving (“food service”), Building and Grounds Cleaning and Maintenance (“maintenance”), Personal Care and Service (“personal services”), Construction, Production (“manufacturing”), and Transportation and Material Moving (“transportation”). These sectors are selected because they have a large immigrant worker penetration (see Figure 1) and employ a relatively low-skilled workforce.

In order to reduce attenuation bias caused by measurement error, our analysis is conducted on the largest 150 MSAs in terms of population. Smaller MSAs are likely to include only few surveyed individuals from a given sector in a given year, which can lead to noisy measures of our regressor of interest (the sectoral share of immigrants working in each MSA).

The data we use is a repeated cross-section of individual-level data that includes the annual earnings of the individual during the preceding year, the number of weeks worked in the preceding year, the employment status (employed/unemployed/out of the labor force), the MSA where the individual lives (taken to be the relevant labor market), their birthplace, as well as information about educational attainment, race, gender, and marital status. This last set of variables is used to construct “residualized" dependent variables that are purged of potentially confounding demographic factors (see Section 4.1). The birthplace variable is used to select natives and to construct the sectoral and multi-sectoral immigrant shares. Between 2008 and 2011, the variable identifying the number of weeks worked (“wkswork2”) is only available as a categorical variable that assigns individuals to time intervals (e.g., 50-52 weeks). We transform this variable into a continuous one by assigning the midpoint of the relevant interval as the number of weeks worked.

The income amounts reported in the surveys are nominal values. We convert these values to constant 2017 dollars using the CPI provided by the Bureau of Labor Statistics for all items (US city average, all urban consumers) at `https://www.bls.gov/data`. The income values for the 1990 and 2000 Census years represent income from the previous calendar year, and the ACS data between 2001 and 2011 report income from the previous 12 months. We adjust the 1990 (resp. 2000) income values using the corresponding 1989 (1999) CPI values, but we use the CPI values corresponding to the sample years for the ACS samples.[10] We define annual earnings as the sum of wages and income from a person’s own business or farm. We compute weekly earnings by dividing annual earnings by the number of weeks worked.

To avoid outliers, when generating regional averages of the earnings variable we exclude workers reporting annual earnings of $300,000 or more. Since our dependent variable is the

[10]Because the ACS is administered throughout the year, income amounts reported by individuals surveyed in January will represent mostly income generated in the previous calendar year, and they will represent income generated mostly during the current year for those surveyed in December. Although the Census Bureau provides a variable that attempts to adjust for this, the adjustments are imperfect, and Ruggles et al. (2017) find that the adjusted and unadjusted income values are essentially perfectly correlated. As a result, Ruggles et al. (2017) does not recommended using the adjustment variable, thus we refrain from using it.

logarithm of earnings, we also exclude individuals reporting zero earnings and those for which the reported value is $1 (a code for "breaking even"). When generating the weekly earnings variable, we exclude workers making at least $300,000/52 = $5,769.23 per week and those making $50 per week or less.

To make sure that we capture the effect of immigration on individuals who are actually in the workforce, we follow the literature by including only working-age adults (18 to 64 years of age) who are not in school and do not live in group quarters (e.g., jails or other institutions). Because we perform our analysis at the MSA-year level, our analysis only considers individuals that are identified in the data as living in a specific MSA.[11] In addition, we exclude individuals whose birthplace is not identified and those who jointly report being out of the labor force at the time of the survey and working zero weeks during the previous year.

The individual-level data samples used in our analysis are "weighted," and as such IPUMS recommends using weighted averages to construct variables that are representative at the regional level. We follow this recommendation by applying the personal weights (variable "perwt") provided in the data sample to generate our immigrant share regressors and instruments. The resulting MSA-panel datasets in each sector are unbalanced as some MSAs are not represented in the year 2003.[12]

[Table 1 about here.]

Table 1 summarizes our data. Note that the mean and standard deviations are calculated across MSAs and years. Since MSAs have different population sizes, the mean values are representative of an average MSA included in our analysis rather than national averages.

4 Methodology

The main difficulty in measuring the effect of immigration on the labor market outcomes of native-born workers in a sector $s = 1, \dots, S$ of the economy is that increases in immigration in sector s are likely correlated with unobserved demand-pull factors in that sector that also affect natives' earnings and employment. We use the imperfect instrumental variable approach described below to partially identify the effect of immigration on the labor market outcomes of native-born workers by economic sector.

4.1 Model specification and instrument choice

We estimate a sectoral regression of the form

$$y_{it}^{s} = \beta p_{it}^{s} + \alpha_i + \phi_t + \epsilon_{it} \tag{1}$$

[11] That is, we ignore individuals for which the MSA identifier is missing in the data.
[12] Our results are robust to removing the year 2003 from the sample.

where i denotes a metropolitan statistical area, t denotes a year, α_i and ϕ_t are fixed effects, p^s_{it} is the immigrant share in sector s, and y^s_{it} is the outcome of interest.

Our outcome variables include the natural logarithm of the annual or weekly earnings of native-born workers, the proportion of native-born workers working full time, and the proportion of natives in the labor force who are employed—our measure of the native sectoral employment rate.[13] In order to identify the effect of immigration on the distribution of native workers across occupational levels, we use several definitions of "full-time" workers: workers who worked 48 weeks or more, 40 weeks or more, 27 weeks or more, 14 weeks or more, and 1 week or more.[14]

In order to address MSA-specific changes in the composition of the native sectoral workforce over time, for each choice of dependent variable (say the logarithm of annual earnings) we first regress individual-level observations around a set of observable individual characteristics related to gender, marital status, race, education, and work experience as well as a full set of MSA-year fixed effects. The estimated MSA-year fixed effects are then used as the dependent variable in the IIV regression on immigrant shares (see Appendix D).[15]

Our main regressor is a measure of immigration defined as the fraction of foreign-born workers in sector s relative to the total workforce in that sector in each MSA and year, p^s_{it}. Apart from the fact that we focus on one sector at a time and do not differentiate by skill, this is the same regressor as that used by Altonji and Card (1991), Borjas (2003), Borjas (2014), or Llull (2017), and it is directly related to the one used in Dustmann et al. (2005) (the ratio of immigrant to native workers) or Bratsberg and Raaum (2012) (a transformation thereof).

In a recent review of George Borjas' *Immigration Economics*, Card and Peri (2016) criticize the use of the immigrant share regressor on the grounds that due to possible *native* inflows correlated with demand pulls that affect native wages, the regressor might be negatively correlated with the error term, resulting in *negative* bias on the correlation of interest. If both immigrants *and* natives are attracted to areas with positive demand pulls, whether the immigrant share is positively or negatively correlated with the error term ultimately depends on whether natives or immigrants are more responsive to these pulls. It seems reasonable to believe that the immigrant population would respond more promptly to local demand shocks than natives, so that the net bias, in fact, remains positive. A basic reason why the immigrant population would be more responsive is that in any given period, part of this population is migrating from abroad (current inflow), i.e., it is already mobile (Borjas, 2001). In addition, Card (2001)'s results suggest little migratory response of natives to immigration shocks, while Cadena and Kovak (2016) show that low-skilled Mexican-born immigrants respond much more strongly to local labor demand shocks than natives, even

[13]The proportion of employed workers is calculated by dividing the number of respondents indicating being employed in the previous week by the number of respondents indicating either being employed in the previous week or having been looking for a job in the previous four weeks.

[14]These cutoffs are chosen to match the definition of the categorical variable "wkswork2" that identifies the range of weeks worked in IPUMS starting in 2008.

[15]This technique mirrors that used by Reed and Danziger (2007) in a cross-sectional context. We get very similar results if instead we average the dependent variable over observations in each MSA-year cell using the personal weights provided in IPUMS.

after arrival. Card and Peri (2016)'s preferred regressor, used in a regression where the dependent variable is the growth in wages rather than the current wage, is defined as the ratio of the *current inflow* of immigrants to the *previous workforce* (including natives and previously arrived immigrants). Our specification reflects the idea that it is the stock of foreign-born workers, rather than the current inflow, that may affect native wages.[16]

Our instruments are variables that measure the proportion of immigrants across many sectors of the economy, including (resp. excluding) sector s itself (p_{it}^{S}, resp. p_{it}^{S-s}). The instrument p_{it}^{S} is calculated across all economic sectors and corresponds to the regressor used in a spatial correlation approach that considers all sectors of the economy rather than one in isolation. The instrument p_{it}^{S-s} removes the contribution of sector s itself to the immigrant share and it is our preferred instrument. We also use a variant of the instrument p_{it}^{S} constructed using the ten economic sectors with the highest proportions of immigrants.

Although p_{it}^{S} and p_{it}^{S-s} are likely correlated with unobservable labor demand shocks in sector s, perhaps due to macroeconomic shocks that affect all sectors,[17] they are plausibly less correlated with the sectoral error term than the sectoral regressor p_{it}^{s}, making them good candidates for an imperfect instrument. Still, the ability of imperfect instruments constructed using information from other economic sectors to improve on OLS estimates of the immigrant share-outcome relationship partially hinges on whether labor demand shocks about MSA (α_i) and year (ϕ_t) effects are sufficiently heterogeneous across sectors. That is, if labor demand shocks were perfectly correlated across sectors, there would be no reason to expect much bias reduction from the use of the imperfect instrument. Fortunately, this seems not to be the case in our data. For instance, a regression of the log annual earnings of natives at the MSA by year by sector level (using the six economic sectors that are the focus of this study) on a set of sector and MSA-by-year fixed effects has a R-squared of 0.73, meaning that a significant amount of variation remains in the outcome after netting out common shocks.

In addition to the fact that common macroeconomic shocks may result in a correlation between the overall immigrant share (say p_{it}^{S}) and sectoral labor demand shocks (ϵ_{it}), overall immigration may have a direct effect on native labor demand in a given sector. In particular, one may worry that although sectoral immigration may hurt natives in that sector because native and immigrant

[16]Card and Peri (2016) also argue that their regressor better captures Borjas' "relevant wage elasticity," defined as the derivative of the log wage of a given skill group with respect to the "immigration-induced percent increase in the labor supply of (the) group." Borjas defines the immigration-induced percent increase in the labor supply as the ratio of *current foreign-born workers* to *current US-born workers*. With this definition, the relevant wage elasticity can be directly deduced from the estimate of the coefficient on the immigrant share (Borjas, 2003), whereas it cannot be deduced from Card and Peri (2016)'s regression (unless there are only two periods, no immigrants in the first period, and no change in the native workforce between periods, see Appendix C). An important difference between the two specifications is that Borjas' specification considers that immigrants affect native outcomes in levels irrespective of the timing of their arrival, whereas Card and Peri (2016)'s specification identifies effects from changes in outcomes using the most recent inflow, measured relative to the previous workforce irrespective of its immigrant-native composition. The fact that we exploit year-to-year variation to identify short-run effects, coupled with the fact that our panel is missing some years, makes the latter approach less justifiable in our context.

[17]Another reason why p_{it}^{S} may be correlated with sectoral demand pulls is that sector s itself is included in the calculation of the immigrant share.

labor are substitutable, immigration as a whole may affect the economy in ways that improve natives' employment conditions.[18] In that case, our instrument would be an omitted variable of the following underlying immigration-native outcome relationship:

$$y_{it}^{s} = b_1 p_{it}^{s} + b_2 p_{it}^{S} + a_i + f_t + e_{it}.$$

While our framework does not allow identification of a causal relationship between *overall* immigration and *sectoral* outcomes, it can speak to the sign of the bias that would be caused on the estimate of the sectoral effect b_1. Denoting by $\tilde{p}_{it}^{s}$ the residuals of a regression of p_{it}^{s} on location and time fixed effects, our IIV estimate of b_1 has a probability limit equal to

$$\beta_{p^S}^{\mathrm{IV}} = b_1 + b_2 \frac{\operatorname{var}(p^S)}{\operatorname{cov}(\tilde{p}^s, p^S)} + \frac{\operatorname{cov}(p^S, e)}{\operatorname{cov}(\tilde{p}^s, p^S)}.$$

It is natural to assume that $\operatorname{cov}(\tilde{p}^s, p^S) > 0$. It is then clear that if overall immigration improves sectoral native labor outcomes ($b_2 > 0$), then our estimate of b_1 will be biased upwards. That is, our IIV strategy provides a conservative estimate of the sectoral impact of sectoral immigration.

We provide results for all IPUMS occupations within each sector, as well as results pertaining to what we call "immigrant-exposed occupations" within a sector. To define immigrant-exposed occupations, we select occupations with the highest immigrant shares within a sector, until the total number of native workers in those occupations exceeds 50% of the total native workforce in the sector. Notably, we do not redefine our regressor of interest (or the imperfect instruments) when focussing on immigrant-exposed occupations, that is, we look at the effect of the overall sectoral immigrant share on outcomes for workers in occupations with the highest immigrant penetration.

4.2 The IIV strategy

We use the results contained in Proposition 2 of Nevo and Rosen (2012). This proposition provides us with a one-sided bound given by the IIV estimate.

For the purpose of this section, let us adopt the same notation as Nevo and Rosen (2012). We write the DGP underlying model (1) as

$$Y = X\beta + \mathbf{W}\boldsymbol{\delta} + U \tag{2}$$

where Y is the dependent variable, X is the sectoral immigrant share, $\mathbf{W}$ is a row vector of covariates comprising dummy variables for each MSA and dummy variables for each year, and U is the error term, which satisfies $\mathbb{E}[\mathbf{W}'U] = 0$. We denote by Z (or Z_1, when necessary to avoid confusion) our preferred instrument, p_{it}^{S-s}. We denote by Z_2 an alternative instrument, for instance the instrument constructed as the share of immigrant workers in sectors with the ten highest shares of immigrant workers. For two random variables, say X and Y, σ_{xy} denotes the covariance between X and Y.

[18] This situation can be thought of as a variant of Ottaviano and Peri (2012)'s argument that immigration-induced shocks to a skill group have effects on wages in other skill groups.

We use σ_x to denote the standard deviation of X. We denote the correlation between X and Y as ρ_{xy}. We further denote by β^{OLS} (resp. β_z^{IV}) the probability limits of the OLS estimator (resp. the IV estimator using instrument Z) of parameter β in Equation (2).

We denote by $\tilde{X}$ (resp. $\tilde{Y}$) the residuals from the OLS regression of X (resp. Y) on $\mathbf{W}$, that is,

$$\begin{cases} \tilde{X} = X - \mathbf{W}\mathbb{E}[\mathbf{W}'\mathbf{W}]^{-1}\mathbb{E}[\mathbf{W}'X] \\ \tilde{Y} = Y - \mathbf{W}\mathbb{E}[\mathbf{W}'\mathbf{W}]^{-1}\mathbb{E}[\mathbf{W}'Y] \end{cases}. \tag{3}$$

$\tilde{X}$ and $\tilde{Y}$ thus represent the residualized regressor and the residualized outcome variable about location and year effects, respectively. Nevo and Rosen (2012) show that $\tilde{Y} = \tilde{X}\beta + U$. Using the Frisch-Waugh-Lovell theorem (Frisch and Waugh, 1933; Lovell, 1963) and its extension to IV estimation (Giles, 1984), it is straightforward to show that

$$\begin{cases} \beta^{\text{OLS}} = \beta + \frac{\sigma_{\tilde{x}u}}{\sigma_{\tilde{x}}^2} \\ \beta_z^{\text{IV}} = \beta + \frac{\sigma_{zu}}{\sigma_{\tilde{x}z}} \end{cases}. \tag{4}$$

To fix ideas, consider the case where the dependent variable represents the annual earnings of natives, which implies that $\sigma_{\tilde{x}u} = \sigma_{xu} > 0$ since unobserved demand pulls would tend to increase native earnings and the immigrant share. Given Equation (4), we would expect the OLS estimate to be asymptotically biased upwards. That is, $\beta \leq \beta^{\text{OLS}}$. We now make the following two-part assumption, referred to as Assumptions 3 and 4 in Nevo and Rosen (2012):

Assumption 1 $0 \leq \rho_{zu} \leq \rho_{xu}$.

Assumption 1 implies that the direction of correlation with the error term in (2) is the same for the regressor and the instrument, but the "intensity" of the correlation is lessened when using the instrument. In that sense, the instrument is "less endogenous" than the regressor. It is also natural in our setting (and we systematically test this condition) to expect that $\sigma_{\tilde{x}z} = \sigma_{\tilde{x}\tilde{z}} > 0$, that is, the shocks in the immigrant share about city and year means are positively correlated across a given sector and the rest of the economy.[19] Because $\sigma_{zu} \geq 0$ from Assumption 1, Equation (4) implies that the IV estimate is also asymptotically biased, in the same direction as the OLS estimate, that is, $\beta \leq \beta_z^{\text{IV}}$. In addition, $\beta_z^{\text{IV}} < \beta^{\text{OLS}} \Leftrightarrow \sigma_{zu}\sigma_{\tilde{x}}^2 - \sigma_{\tilde{x}u}\sigma_{\tilde{x}z} < 0 \Leftrightarrow \rho_{zu} < \rho_{\tilde{x}u}\rho_{\tilde{x}z} = \rho_{xu}\rho_{\tilde{x}z}$. Importantly, the fact that the instrument be less endogenous than the regressor in the sense of Assumption 1 is necessary, but not sufficient, for the IV estimate to improve on the OLS estimate. In particular, if the correlation between the residualized sectoral immigrant share and its economy-wide counterpart is positive but weak, it could be the case that $\beta^{\text{OLS}} < \beta_z^{\text{IV}}$ even if Assumption 1 holds.

Nevo and Rosen (2012)'s analysis suggests that under our Assumption 1, the verified assumption that $\sigma_{\tilde{x}z} > 0$, and the additional assumption that $\sigma_{\tilde{x}x}\sigma_z - \sigma_x\sigma_{\tilde{x}z} > 0$ (which is also satisfied in our setting), one may be able to improve on the upper bound β_z^{IV} by using a combined instrument

[19] $\tilde{Z}$ denotes the residual from the regression of Z on $\mathbf{W}$.

defined as $V(1) = \sigma_x Z - \sigma_z X$.[20] The probability limit of the corresponding IV estimator can be derived as

$$\beta^{\mathrm{IV}}_{V(1)} = \beta + \frac{\sigma_x \sigma_{zu} - \sigma_z \sigma_{xu}}{\sigma_x \sigma_{\tilde{x}z} - \sigma_z \sigma_{\tilde{x}x}}. \tag{5}$$

Under the above assumptions, it turns out that $\beta^{\mathrm{IV}}_{V(1)} < \beta^{\mathrm{IV}}_z \Leftrightarrow \beta^{\mathrm{OLS}} < \beta^{\mathrm{IV}}_z \Leftrightarrow \beta^{\mathrm{IV}}_{V(1)} < \beta^{\mathrm{OLS}}$. Therefore, the use of $V(1)$ as an instrument does not improve on either β^{IV}_z or even β^{OLS} when $\beta^{\mathrm{IV}}_z < \beta^{\mathrm{OLS}}$. In cases where $\beta^{\mathrm{OLS}} < \beta^{\mathrm{IV}}_z$ however, $\beta^{\mathrm{IV}}_{V(1)}$ improves on β^{OLS}.

Finally, Nevo and Rosen (2012)'s analysis suggests a way to derive a lower bound for our effect of the immigrant share on annual income. The idea, developed in Proposition 5 and Lemma 2 of their paper, is that if the analyst has not only one, but two IIVs, say Z_1 and Z_2, she may be able to construct a weighted difference, say $\omega(\gamma) = \gamma Z_2 - (1-\gamma) Z_1$, with $\gamma \in (0,1)$, that satisfies $\sigma_{\omega(\gamma)u} \geq 0$ and $\sigma_{\tilde{x}\omega(\gamma)} < 0$. That is, by differencing the two IIVs, one may be able to obtain a new IIV that is still positively correlated with the error term, but is now negatively correlated with the regressor. The probability limit of the corresponding IV estimator is

$$\beta^{\mathrm{IV}}_{\omega(\gamma)} = \beta + \frac{\sigma_{\omega(\gamma)u}}{\sigma_{\tilde{x}\omega(\gamma)}} \tag{6}$$

implying that $\beta^{\mathrm{IV}}_{\omega(\gamma)}$ constitutes a lower bound for β. Nevo and Rosen (2012) even provide a testable sufficient condition for $\omega(\gamma)$ to meet these requirements for some unknown value $\gamma^* \in (0,1)$: it must be that $\sigma_{\tilde{y}z_1}\sigma_{\tilde{x}z_2} - \sigma_{\tilde{y}z_2}\sigma_{\tilde{x}z_1} < 0$. Even if this condition, which guarantees the existence of a value γ^* from which a lower bound can be derived, is satisfied in our analysis, we have no guidance as to what this value of γ^* should be. In fact, without an additional assumption on γ^* (besides $\sigma_{\tilde{x}\omega(\gamma)} < 0$, which, given $\sigma_{\tilde{x}z_j} > 0$, is equivalent to $\gamma < \bar{\gamma} \equiv \frac{\sigma_{\tilde{x}z_1}}{\sigma_{\tilde{x}z_1} + \sigma_{\tilde{x}z_2}}$), one can only deduce that $-\infty = \beta^{\mathrm{IV}}_{\omega(\bar{\gamma})} < \beta$, that is, the lower bound is uninformative. In what follows, we therefore only report the values of β^{OLS} and the upper bound $\beta^{\mathrm{IIV}} = \min(\beta^{\mathrm{IV}}_z, \beta^{\mathrm{IV}}_{V(1)})$. In the vast majority of regressions we report, the IV estimate does improve on the OLS estimate and therefore the estimate we report is $\beta^{\mathrm{IIV}} = \beta^{\mathrm{IV}}_z$.

5 Results

We begin by presenting results pertaining to the short-run impact of immigration on the earnings of natives, organized by sector of the economy. We do not report earnings results for the manufacturing sector, as none of them are statistically significant.[21] We then report short-run effects of immigration on natives' employment rate across all six sectors. All our first-stage partial F-statistics for our preferred instrument are larger than 70 and are not reported.

[20]This instrument $V(1)$ is a limit value of the set of instruments $V(\lambda) = \sigma_x Z - \lambda \sigma_z X$. The authors show that for $\lambda = \lambda^* = \frac{\rho_{zu}}{\rho_{xu}}$, a value unknown to the analyst, the instrument $V(\lambda)$ is valid. Assumption 1 essentially implies that $\lambda^* \in [0,1]$, which is used to derive bounds for β.

[21]Results are available upon request.

5.1 Personal services, food service, and construction

We first report results for the effect of the immigrant share on the annual earnings of native-born workers. We provide results for all occupations within a sector, as well as results pertaining to immigrant-exposed occupations.

Table 2 shows that the annual earnings of native workers in the personal service, food service, and construction sectors are negatively affected by the sectoral share of immigrants. Although the OLS estimate is never statistically significant, the IIV estimates often are, and they are much larger in magnitude.

Importantly, the move from the IIV constructed from the share of immigrants in immigration-exposed sectors (IIV-10) to that constructed from the share of immigrants across all sectors (IIV-All) and across all other sectors (IIV-All but) has the expected effect on the point estimate: the effect systematically becomes more negative as the correlation between the IIV and the error term is attenuated. Personal service, food service, and construction all belong to the 10 sectors with the highest immigrant shares and each is therefore included in the calculation of the IIV-10. The attenuation in the correlation between the error term and the series of imperfect instruments likely comes from the fact that in sectors less prone to immigration, a positive shock in labor demand (which we assume would be positively correlated with a positive shock in the demand for labor in the sector of interest, say construction) may not correlate as much with an increase in the share of immigrant workers as in sectors with larger immigrant shares.[22] In addition, since the correlation of interest is with sectoral demand pulls, the fact that the share of immigrants is calculated across a broader set of industries mechanically "dilutes" the correlation with any sectoral-specific shock in labor demand, and completely eliminates it when the immigrant share is calculated for all other sectors.[23]

[Table 2 about here.]

As explained in Section 4.2, the preferred IIV estimate should be interpreted as an upper bound. That is, the true underlying parameter is likely more negative. Our preferred estimates, given by the "IIV-All but" estimate, imply that a 10 percentage point increase in the share of immigrants is associated with at least a 6.6% (resp. 6.0%, resp. 2.9%) decrease in the annual earnings of native workers in the personal service (resp. food service, resp. construction) sector. Table 2 further shows that in the personal service and construction sectors, the effect is almost doubled for workers in occupations where the share of immigrants is higher.

On balance, these upper bounds appear large relative to recent econometric estimates reported in the literature. Estimates obtained from location-year or location-year-skill comparisons of

[22]An alternative explanation may be that labor-demand shocks are more correlated among immigration-exposed sectors than between immigration-exposed and immigration-poor sectors.

[23]As suggested by Equation (4), the tightness of the upper bound afforded by the IIV estimate relative to the OLS estimate is also inversely related to the correlation between the sectoral immigrant share and the instrument. Our results suggest that this effect either reinforces, or at least does not supersede, the changing strength of correlation between the error term and the various instruments.

average wages across *all occupations* range from -0.22 (Borjas, 2003) to positive values (Basso and Peri, 2015). Borjas (2014) reports an estimate of -0.21 for the period 1990-2010 (-0.24 for males) using the same data source as ours, a MSA-year-skill regression and a shift-share instrumental variable approach. Using our data set, which extends to 2011, and implementing a shift-share approach across all sectors of the economy at the MSA-year level yields an estimate of the annual earnings effect that is not significantly different from zero (see Appendix E). Card (2001) reports that city comparisons typically estimate the effect of a 10 percentage point increase in the fraction of immigrants to correlate with a less than 1% decrease in native wages.[24]

There are two essential channels by which the annual earnings of native-born workers may be affected by immigration flows: their wage rate may decrease and/or they may work fewer weeks per year (none in the extreme). The second channel is particularly relevant for the construction sector because construction workers are typically paid per "job." A year's worth of earnings is made up of earnings from a potentially large number of jobs. If workers have difficulty filling in their schedule due to increased competition from cheaper, and perhaps illegal immigrant labor, they may end up with lower annual earnings even if their weekly earnings (annual earnings divided by the number of weeks worked) do not change. A similar remark may hold in certain personal service occupations with high immigrant penetration, like child and personal care, where self-employment is high.

[Table 3 about here.]

Table 3 reports weekly earnings effects. Weekly earnings are constructed by dividing annual earnings by the number of weeks worked. Weekly (or hourly) effects partially mask annual earnings effects insofar as one margin of response to increased immigration may be the reduction in the quantity of labor supplied by natives. Indeed, a general rule here is that point estimates for weekly effects are smaller in magnitude than for annual earnings effects. For instance, we find that a 10 percentage point increase in the share of immigrant workers causes at least a 3.5% (resp. 3.0%) decrease in the weekly earnings of native personal service (resp. food service) workers. Effects are again more pronounced in the immigration-exposed occupations. Weekly earnings effects are not statistically significant for the construction sector as a whole, although they are for immigration-exposed occupations, with an estimated effet of minus 3.0%.

[Table 4 about here.]

Looking at impacts on occupational levels confirms a redistribution of natives away from full-time and high-time work towards part-time work. Table 4 shows the effects on the share of native construction workers having worked at least a certain number of weeks in the past year. Effects are shown for all construction occupations and for immigrant-exposed construction occupations.

[24] Admittedly, our upper bounds fall short of the larger effect on lower-skilled natives' earnings found in Altonji and Card (1991), a 12% decrease for each 10 percentage point increase in the immigrant share. On balance, they are also less negative than the estimate derived for the decade 1970-1980 by Jaeger et al. (2018).

Preferred IIV estimates (based on p^{S-s}) are statistically significant, and the pattern of increasingly negative effect as the instrument becomes likely less endogenous is maintained. Overall, the estimates suggest that immigration has a negative effect on the occupational level of native construction workers. For instance, a 10 percentage point increase in the share of immigrants is predicted to result in at least a 2.1 percentage point decrease in the share of natives construction workers working at least 40 weeks. For exposed construction trades, the effects are more pronounced (3.9 percentage points).

[Figure 5 about here.]

To get a better idea of the effect of immigration on the occupational level of natives, Figure 5 uses the estimates reported in Table 4 to depict the shift in the distribution of native construction workers across occupation levels, from unemployed to full-time workers, induced by a 20 percentage point increase in the share of immigrant workers in construction. (We choose 20 percent rather than 10 percent so that the change in the distribution is more legible.) The initial distribution is constructed by using occupational shares averaged across sample years and MSAs.

[Table 5 about here.]

[Table 6 about here.]

We also find that occupational levels of native workers are affected negatively by the immigrant share in the food service sector (Table 5) and, for exposed occupations, in the personal services sector (Table 6), thereby contributing to the negative annual earnings effects reported above.

5.2 Maintenance and transportation

The results from the maintenance and transportation sectors are much less clear-cut than those in the sectors analyzed in Section 5.1, at least when considering all occupations within each sector together. Although we find small negative effects on the employment rate of natives (see Section 5.3), we do not find significant effects on annual or weekly earnings. However, once we focus on occupations within these sectors with higher immigrant penetration and/or lower skill requirements, we uncover significant negative effects that were masked when these occupations were grouped with higher-skill occupations. For example, the transportation sector includes aircraft pilots as well as laborers who load freight trucks. One would not expect to find low-skilled immigrants competing with aircraft pilots, so including pilots in the analysis is not very informative.

In the transportation sector, which includes many occupations, we select occupations with a high immigrant share. The occupations selected include taxi drivers, truck drivers, vehicle cleaners, packers, etc. In IPUMS, the maintenance sector as a whole only includes four broad occupations: janitors (and supervisors), landscapers (and supervisors), housekeepers, and pest control workers. Among those, the occupations with highest immigrant penetration are landscapers (34.9%) and housekeepers (44.7%). The next high-immigrant occupation is janitorial workers (25.8%). Our data indicates that landscapers and housekeepers also have the lowest average educational attainment in the maintenance sector. We explore immigration impacts for each of the four maintenance occupations, but, perhaps surprisingly, only find significant effects for landscapers. While housekeeping has a high immigrant penetration, housekeepers have by far the lowest annual and weekly earnings of all occupations in the maintenance sector.[25] Therefore, it is plausible that the absence of an effect is due to earnings having reached a floor below which native workers would stop supplying labor. It is also plausible that as we focus on more narrowly defined occupations, too few individuals are left in the IPUMS data set to construct the MSA averages of immigrant penetration, causing attenuation bias.

[Table 7 about here.]

Table 7 shows selected results for landscaping, housekeeping, and immigrant-exposed transportation activities.[26] We find that a 10 percentage point increase in the share of immigrants causes at least a 7.5% (resp. 2.6%, column (3)) decrease in the annual earnings of landscaping (resp. exposed transportation) workers. This earnings effect appears to be channeled through lower rates of employment in both sectors, as well as a reduced incidence of full-time employment in immigration-exposed transportation occupations (see Table 8). For example, in exposed transportation occupations a 10 percentage point increase in the share of immigrants leads to at least a 1.2 percentage point decrease in the share of workers working at least 40 weeks/year and a 1.2 percentage point decrease in the employment rate.[27] For landscapers, the same change in the share of immigrants leads to a 2.6 percentage point decrease in the native employment rate. This evidence suggests that immigrants are displacing native workers, causing them to work less in some of the occupations in the maintenance and transportation sectors, which is likely attributable to low skill requirements and, subsequently, a high substitutability between natives and immigrants.

[Table 8 about here.]

5.3 Effects on the employment rate

[Table 9 about here.]

Table 9 reports estimates of the effect of immigration on natives' self-reported employment status. The sectoral employment rate is defined as the share of the active population (those in the sector reporting working the previous week or having been in search of a job for the previous four weeks) who reported working the previous week. It is therefore equal to one minus the sectoral unemployment rate. Results show that the immigrant share has a negative effect on

[25] For instance, our data indicates that housekeepers' weekly earnings are about one third lower than those of landscapers.

[26] Results for other maintenance occupations are not statistically significant and are available upon request.

[27] For a precise definition of the employment rate, see Section 5.3.

natives' employment rate. In some sectors, these effects are relatively large. For instance, in construction (resp. food service, resp. personal service), a 10 percentage point increase in the share of immigrants causes at least a 2.3 (resp. 1.8, resp. 1.7) percentage point decrease in the employment rate, and larger effects amongst workers in exposed construction and personal service occupations. The mean unemployment rate in our sample of MSA-years lies between 7% for native personal service workers and 13% for native construction workers, so these effects are not trivial. Importantly, we find negative and statistically significant effects in all six sectors, including manufacturing. Overall, these employment effects contrast with the zero to positive correlations reported by Basso and Peri (2015) for the period 1970–2010. They also provide some empirical support for the view that labor may not be totally mobile across economic sectors in the short run.

6 Conclusion

Economists have long sought to understand the impacts of immigration on the employment conditions of natives. Recently, there has been renewed interest in this question by policy makers and the general public. While most economists would agree that in the long run, any wage effects of immigration-induced labor supply shocks will be buffered by capital adjustments, there has been disagreement in the empirical literature about whether short-run impacts should even be of concern. Admittedly, the question is difficult to answer. Exogenous labor supply shocks rarely happen in practice, and the use of observational data on wages and employment limits the range and usefulness of the effects that can be estimated empirically (Borjas, 2003; Ottaviano and Peri, 2012; Dustmann et al., 2016) while potentially affecting the reliability of the estimates (Jaeger et al., 2018).

The present study does not purport to completely resolve these fundamental tradeoffs. However, it offers a novel approach to the problem—a sectoral analysis that relies on imperfect instruments—as well as meaningful bounds on short-run immigration impacts in important sectors of the US economy for a recent period. We find negative effects of immigration on native earnings in sectors where we would most expect to find them: low-skilled sectors that produce non-traded goods where immigrant penetration has been high in recent decades. The negative effects that we find are perhaps best exemplified by looking at the construction sector, which employs a sizable share of the native and immigrant workforce over the period of our study (an average of 5.9% and 9.4%, respectively, according to our data). In that sector, we find that a 10 percentage point increase in the share of immigrants, which falls short of the historical increase over the period 1990-2011, causes at least a 6.9% (resp. 3.6 percentage point, resp. 3.9 percentage point) decline in the annual earnings (resp. employment rate, resp. full-time occupational rate) of native workers when we focus on immigration-exposed occupations such as painters and roofers. We find qualitatively similar results in other low-skilled sectors of the US economy such as personal services and food service. Our estimated impacts should be interpreted as upper bounds (meaning that the true effect is larger in magnitude) for at least two reasons: first, our strategy does not entirely correct for endogeneity bias, and second, the area-year variation we exploit may mask larger effects due to spatial arbitrage by native workers or capital flows across areas.

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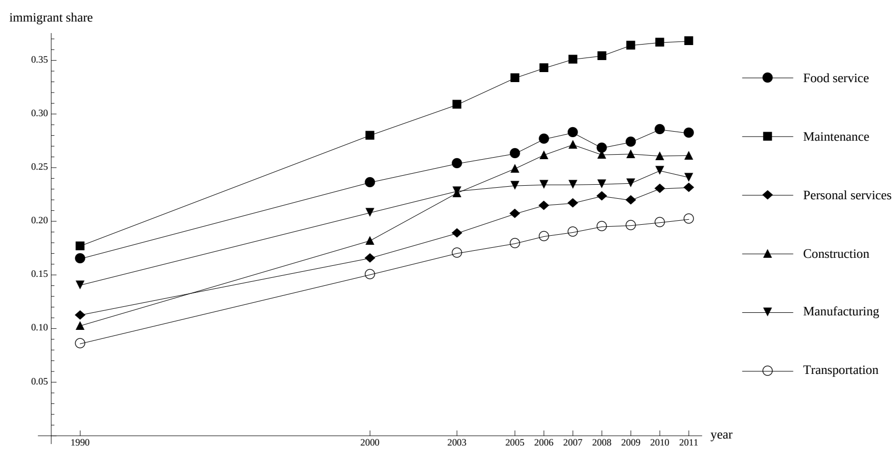
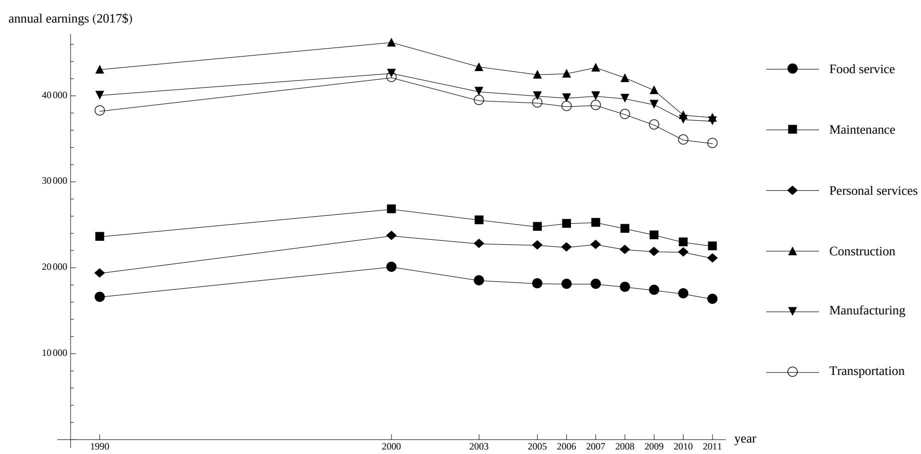
Figure 1: Evolution of the immigrant share by sector



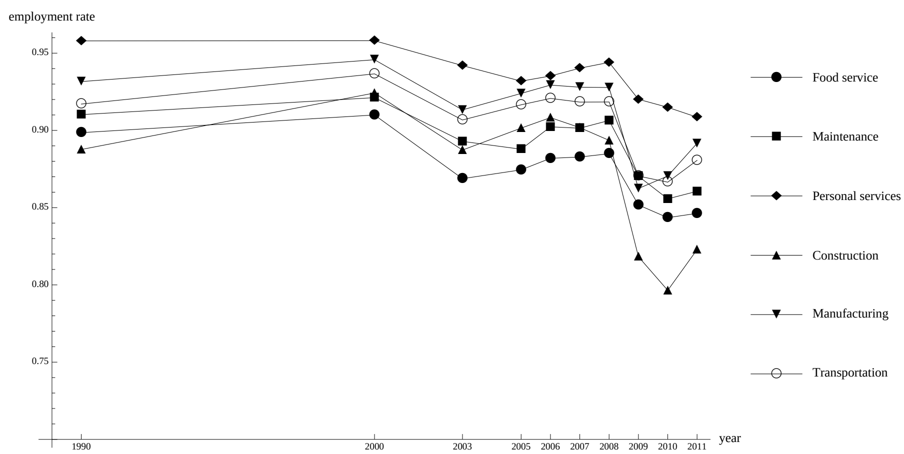
<u>Source</u>: IPUMS data processed by the authors. The immigrant share is calculated over individuals aged 18-64, not living in group quarters, not in school, and in the labor force. Individuals are considered not to be in the labor force if they report being out of the labor force at the time of the survey and working zero weeks during the previous year.

Figure 2: Evolution of annual earnings of natives by sector, all occupations



Source: IPUMS data processed by the authors. Earnings are calculated over natives aged 18-64, not living in group quarters, not in school, in the labor force, and with annual earnings above zero and below $300,000 (in 2017$). Individuals are considered not to be in the labor force if they report being out of the labor force at the time of the survey and working zero weeks during the previous year. Annual earnings include wages and income from a person's own business or farm.

Figure 3: Evolution of the native employment rate by sector

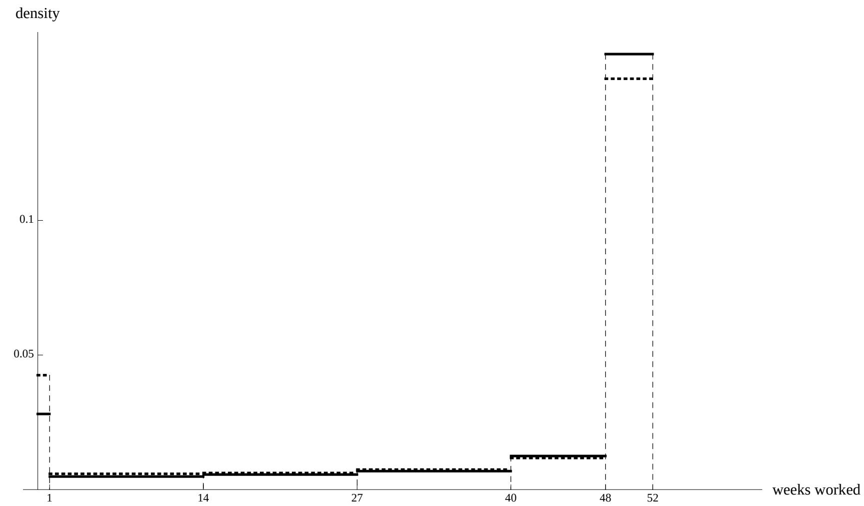


Source: IPUMS data processed by the authors. The employment rate is calculated over natives aged 18-64, not living in group quarters, not in school, and in the labor force at the time of the survey.

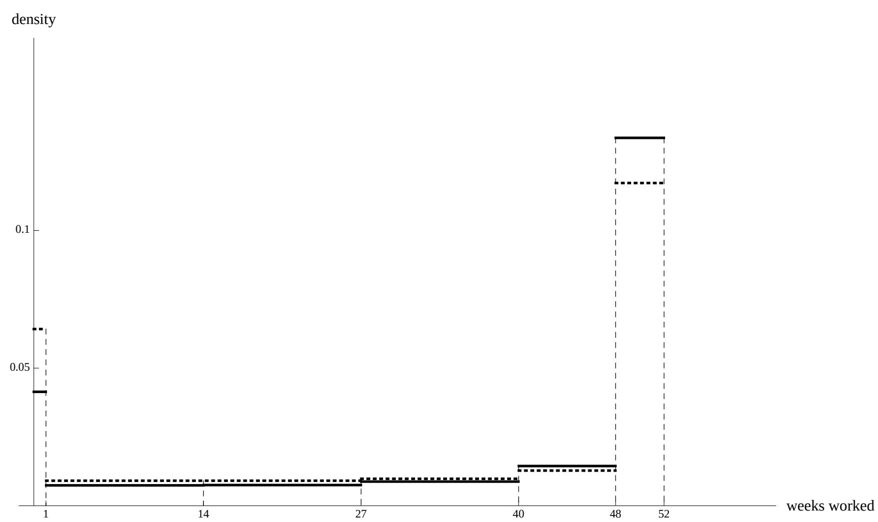
Figure 4: Evolution of the national unemployment rate

Source: Bureau of Labor Statistics, seasonally adjusted monthly unemployment rate for individuals 16 years and older.

Figure 5: Effect of a 20 percentage point increase in the immigrant share on native workers' occupational levels in the construction sector



(a) All occupations



(b) Immigrant-exposed occupations

Note: The solid (resp. dashed) line represents the distribution of native workers across occupational levels before (resp. after) the increase in immigration.

Table 1: Summary statistics

		All occ.		Exposed occ.	
Sector/Variable	Unit	Mean	S.D.	Mean	S.D.
Personal service					
Annual earnings (natives)	2017$	21,433	4,666	16,756	4,927
Weekly earnings (natives)	2017$	511	96	430	105
Employment Rate (natives)		0.934	0.048	0.919	0.073
Share of immigrant workers		0.157	0.142	0.190	0.173
Share of immigrant workers in all other industries		0.137	0.113		
Food service					
Annual earnings (natives)	2017$	17,909	3,843	16,763	3,866
Weekly earnings (natives)	2017$	444	82	424	86
Employment Rate (natives)		0.877	0..055	0.861	0.079
Share of immigrant workers		0.218	0.172	0.243	0.188
Share of immigrant workers in all other industries		0.134	0.112		
Construction					
Annual earnings (natives)	2017$	40,261	7,504	32,412	7,189
Weekly earnings (natives)	2017$	944	158	811	155
Employment Rate (natives)		0.871	0.069	0.822	0.101
Share of immigrant workers		0.194	0.176	0.246	0.211
Share of immigrant workers in all other industries		0.134	0.111		
Maintenance					
Annual earnings (natives)	2017$	24,136	4,840	22,003	4,622
Weekly earnings (natives)	2017$	581	101	542	98
Employment Rate (natives)		0.891	0.057	0.863	0.092
Share of immigrant workers		0.262	0.222	0.273	0.230
Share of immigrant workers in all other industries		0.132	0.109		
Transportation					
Annual earnings (natives)	2017$	36,474	4,972	33,297	4,758
Weekly earnings (natives)	2017$	825	103	764	98
Employment Rate (natives)		0.904	0.047	0.892	0.053
Share of immigrant workers		0.152	0.149	0.165	0.161
Share of immigrant workers in all other industries		0.137	0.112		
Manufacturing					
Annual earnings (natives)	2017$	40,050	6,735	33,370	6,053
Weekly earnings (natives)	2017$	884	136	764	127
Employment Rate (natives)		0.912	0.048	0.898	0.084
Share of immigrant workers		0.217	0.180	0.264	0.209
Share of immigrant workers in all other industries		0.132	0.111		
Observations		1,387	1,387	1,387	1,387

Note: These figures are based on the 150 most populous MSAs and are representative of an average MSA in our sample during the period 1990-2011. They may differ from national averages due to differences in population amongst MSAs. To define immigrant-exposed occupations ("Exposed occ."), we select occupations with the highest immigrant shares within a sector, until the total number of native workers in those occupations exceeds 50% of the total native workforce in the sector.

Table 2: Effect of immigration on the annual earnings of native-born workers

		(1) OLS	(2) IIV-10	(3) IIV-All	(4) IIV-All but
Personal services	All occ.	-0.012	-0.114	-0.499**	-0.658**
		(0.127)	(0.254)	(0.234)	(0.287)
	Exposed occ.	-0.105	-0.597*	-0.955***	-1.233***
		(0.177)	(0.358)	(0.307)	(0.375)
Food service	All occ.	0.056	-0.280**	-0.386***	-0.598***
		(0.066)	(0.136)	(0.131)	(0.202)
	Exposed occ.	0.122	-0.249*	-0.347**	-0.585***
		(0.093)	(0.129)	(0.139)	(0.218)
Construction	All occ.	0.019	-0.029	-0.158	-0.293*
		(0.068)	(0.098)	(0.116)	(0.160)
	Exposed occ.	-0.162**	-0.308***	-0.487***	-0.689***
		(0.082)	(0.116)	(0.135)	(0.194)
Observations		1,387	1,387	1,387	1,387

Note: All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level. Column (1) reports the OLS estimate of β in Equation (1). Column (2) reports the IV estimate obtained by using the immigrant share in the 10 sectors with the highest immigrant shares. Column (3) reports the IV estimate obtained by using the immigrant share across all sectors. Column (4) reports the IV estimate obtained by using the immigrant share across all other sectors. * (resp. **, resp. ***) denotes statistical significance at the 10% (resp. 5%, resp. 1%) level.

Table 3: Effect of immigration on the weekly earnings of native-born workers

		(1) OLS	(2) IIV-10	(3) IIV-All	(4) IIV-All but
Personal services	All occ.	-0.034	-0.036	-0.275*	-0.353*
		(0.067)	(0.076)	(0.149)	(0.186)
	Exposed occ.	-0.069	-0.163	-0.409**	-0.525**
		(0.117)	(0.208)	(0.203)	(0.249)
Food service	All occ.	0.052	-0.162*	-0.190**	-0.296**
		(0.048)	(0.083)	(0.085)	(0.129)
	Exposed occ.	0.085	-0.167*	-0.191*	-0.325**
		(0.063)	(0.091)	(0.103)	(0.158)
Construction	All occ.	0.026	0.020	0.002	-0.037
		(0.045)	(0.061)	(0.074)	(0.100)
	Exposed occ.	-0.081	-0.153**	-0.217**	-0.300**
		(0.059)	(0.075)	(0.090)	(0.125)
Observations		1,387	1,387	1,387	1,387

Note: All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level. Column (1) reports the OLS estimate of β in Equation (1). Column (2) reports the IV estimate obtained by using the immigrant share in the 10 sectors with the highest immigrant shares. Column (3) reports the IV estimate obtained by using the immigrant share across all sectors. Column (4) reports the IV estimate obtained by using the immigrant share across all other sectors. * (resp. **, resp. ***) denotes statistical significance at the 10% (resp. 5%, resp. 1%) level.

Table 4: Effect of immigration on the distribution of weeks worked among native-born construction workers

		OLS	IIV-10	IIV-All	IIV-All but
All occ.	48 weeks or more	0.021	-0.024	-0.098*	-0.183**
		(0.034)	(0.041)	(0.051)	(0.073)
	40 weeks or more	0.034	-0.032	-0.113**	-0.212***
		(0.029)	(0.037)	(0.048)	(0.071)
	27 weeks or more	0.026	-0.021	-0.094**	-0.175***
		(0.024)	(0.030)	(0.038)	(0.058)
	14 weeks or more	0.005	-0.023	-0.080***	-0.138***
		(0.018)	(0.021)	(0.026)	(0.041)
	one week or more	0.010	-0.014	-0.037**	-0.072***
		(0.012)	(0.014)	(0.016)	(0.024)
Exposed occ.	48 weeks or more	-0.033	-0.121**	-0.211***	-0.328***
		(0.047)	(0.059)	(0.076)	(0.109)
	40 weeks or more	-0.034	-0.151***	-0.254***	-0.394***
		(0.038)	(0.051)	(0.069)	(0.102)
	27 weeks or more	-0.007	-0.096**	-0.202***	-0.327***
		(0.032)	(0.043)	(0.057)	(0.087)
	14 weeks or more	-0.035	-0.085***	-0.147***	-0.224***
		(0.029)	(0.031)	(0.038)	(0.058)
	one week or more	0.007	-0.035*	-0.064***	-0.114***
		(0.016)	(0.019)	(0.021)	(0.032)
Observations		1,387	1,387	1,387	1,387

Note: The dependent variable is the share of native workers working at least a certain number of weeks in the past year. All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level. Column (1) reports the OLS estimate of β in Equation (1). Column (2) reports the IV estimate obtained by using the immigrant share in the 10 sectors with the highest immigrant shares. Column (3) reports the IV estimate obtained by using the immigrant share across all sectors. Column (4) reports the IV estimate obtained by using the immigrant share across all sectors but construction. * (resp. **, resp. ***) denotes statistical significance at the 10% (resp. 5%, resp. 1%) level.

Table 5: Effect of immigration on the distribution of weeks worked among native-born food service workers

		OLS	IIV-10	IIV-All	IIV-All but
All occ.	48 weeks or more	-0.010	-0.083	-0.109	-0.161
		(0.038)	(0.058)	(0.072)	(0.111)
	40 weeks or more	-0.010	-0.134**	-0.164**	-0.239**
		(0.035)	(0.056)	(0.065)	(0.096)
	27 weeks or more	-0.015	-0.099**	-0.138**	-0.208**
		(0.027)	(0.048)	(0.061)	(0.096)
	14 weeks or more	0.001	-0.002	-0.015	-0.035
		(0.018)	(0.035)	(0.040)	(0.055)
	one week or more	-0.005	-0.007	-0.007	-0.006
		(0.014)	(0.025)	(0.019)	(0.019)
Exposed occ.	48 weeks or more	0.004	-0.060	-0.091	-0.143
		(0.036)	(0.055)	(0.066)	(0.100)
	40 weeks or more	-0.017	-0.131***	-0.159***	-0.230***
		(0.038)	(0.050)	(0.058)	(0.087)
	27 weeks or more	-0.012	-0.076	-0.088	-0.139
		(0.027)	(0.057)	(0.073)	(0.108)
	14 weeks or more	0.017	0.012	0.014	-0.002
		(0.024)	(0.045)	(0.058)	(0.081)
	one week or more	-0.001	-0.001	0.010	-0.002
		(0.021)	(0.036)	(0.037)	(0.031)
Observations		1,387	1,387	1,387	1,387

Note: The dependent variable is the share of native workers working at least a certain number of weeks in the past year. All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level. Column (1) reports the OLS estimate of β in Equation (1). Column (2) reports the IV estimate obtained by using the immigrant share in the 10 sectors with the highest immigrant shares. Column (3) reports the IV estimate obtained by using the immigrant share across all sectors. Column (4) reports the IV estimate obtained by using the immigrant share across all sectors but food service. * (resp. **, resp. ***) denotes statistical significance at the 10% (resp. 5%, resp. 1%) level.

Table 6: Effect of immigration on the distribution of weeks worked among native-born personal service workers

		OLS	IIV-10	IIV-All	IIV-All but
All occ.	48 weeks or more	0.006	-0.022	-0.007	-0.008
		(0.040)	(0.047)	(0.043)	(0.043)
	40 weeks or more	0.012	0.010	-0.001	-0.001
		(0.035)	(0.044)	(0.078)	(0.099)
	27 weeks or more	0.006	-0.046	-0.065	-0.087
		(0.031)	(0.066)	(0.068)	(0.085)
	14 weeks or more	0.009	-0.046	-0.072	-0.099
		(0.030)	(0.047)	(0.046)	(0.061)
	one week or more	-0.007	-0.036	-0.035	-0.044
		(0.012)	(0.029)	(0.028)	(0.036)
Exposed occ.	48 weeks or more	-0.031	-0.101	-0.077	-0.086
		(0.063)	(0.155)	(0.167)	(0.210)
	40 weeks or more	-0.028	-0.255**	-0.229*	-0.290*
		(0.056)	(0.130)	(0.123)	(0.153)
	27 weeks or more	-0.021	-0.298**	-0.294**	-0.382***
		(0.054)	(0.122)	(0.116)	(0.145)
	14 weeks or more	0.021	-0.145*	-0.144*	-0.200**
		(0.046)	(0.081)	(0.077)	(0.099)
	one week or more	-0.012	-0.086*	-0.089*	-0.115*
		(0.021)	(0.052)	(0.049)	(0.063)
Observations		1,387	1,387	1,387	1,387

Note: The dependent variable is the share of native workers working at least a certain number of weeks in the past year. All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level. Column (1) reports the OLS estimate of β in Equation (1). Column (2) reports the IV estimate obtained by using the immigrant share in the 10 sectors with the highest immigrant shares. Column (3) reports the IV estimate obtained by using the immigrant share across all sectors. Column (4) reports the IV estimate obtained by using the immigrant share across all sectors but personal service. * (resp. **, resp. ***) denotes statistical significance at the 10% (resp. 5%, resp. 1%) level.

Table 7: Landscaping, housekeeping, and exposed transportation occupations

		(1) OLS	(2) IIV-10	(3) IIV-All	(4) IIV-All but
Landscaping	Annual earnings	-0.011	-0.544**	-0.586**	-0.750**
		(0.106)	(0.251)	(0.289)	(0.376)
	Weekly earnings	-0.039	-0.208	-0.193	-0.221
		(0.064)	(0.143)	(0.156)	(0.214)
	Employment rate	0.036	-0.130**	-0.181***	-0.264***
		(0.027)	(0.057)	(0.054)	(0.079)
	Observations	1,386	1,386	1,386	1,386
Housekeeping	Annual earnings	-0.074	-0.177	-0.106	-0.099
		(0.157)	(0.182)	(0.166)	(0.166)
	Weekly earnings	0.025	-0.004	0.006	0.013
		(0.081)	(0.095)	(0.094)	(0.093)
	Employment rate	0.045	-0.080	-0.071	-0.098
		(0.028)	(0.070)	(0.073)	(0.090)
	Observations	1,382	1,382	1,382	1,382
Exposed transportation	Annual earnings	-0.187*	-0.201	-0.261*	-0.274
		(0.098)	(0.145)	(0.142)	(0.167)
	Weekly earnings	-0.060	-0.107	-0.081	-0.084
		(0.067)	(0.087)	(0.085)	(0.100)
	Employment rate	-0.053	-0.075*	-0.104***	-0.119***
		(0.032)	(0.038)	(0.036)	(0.044)
	Observations	1,387	1,387	1,387	1,387

Note: All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level. Column (1) reports the OLS estimate of β in Equation (1). Column (2) reports the IV estimate obtained by using the immigrant share in the 10 sectors with the highest immigrant shares. Column (3) reports the IV estimate obtained by using the immigrant share across all sectors. Column (4) reports the IV estimate obtained by using the immigrant share across all other sectors. * (resp. **, resp. ***) denotes statistical significance at the 10% (resp. 5%, resp. 1%) level.

Table 8: Effect of immigration on the distribution of weeks worked in immigration-exposed transportation occupations

	OLS	IIV-10	IIV-All	IIV-All but
48 weeks or more	-0.092**	-0.119*	-0.109*	-0.119*
	(0.036)	(0.062)	(0.058)	(0.069)
40 weeks or more	-0.079**	-0.123**	-0.112**	-0.122*
	(0.032)	(0.057)	(0.052)	(0.063)
27 weeks or more	-0.072**	-0.086*	-0.070	-0.075*
	(0.032)	(0.049)	(0.049)	(0.043)
14 weeks or more	-0.060**	-0.063	-0.064	-0.060
	(0.028)	(0.039)	(0.043)	(0.051)
one week or more	-0.006	-0.010	-0.021	-0.026
	(0.018)	(0.023)	(0.022)	(0.026)
Observations	1,387	1,387	1,387	1,387

Note: The dependent variable is the share of native workers working at least a certain number of weeks in the past year. All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level. Column (1) reports the OLS estimate of β in Equation (1). Column (2) reports the IV estimate obtained by using the immigrant share in the 10 sectors with the highest immigrant shares. Column (3) reports the IV estimate obtained by using the immigrant share across all sectors. Column (4) reports the IV estimate obtained by using the immigrant share across all sectors but personal service. * (resp. **, resp. ***) denotes statistical significance at the 10% (resp. 5%, resp. 1%) level.

Table 9: Effect of immigration on the employment rate of native-born workers

		(1) OLS	(2) IIV-10	(3) IIV-All	(4) IIV-All but
Personal services	All occ.	-0.043	-0.136***	-0.139***	-0.168***
		(0.028)	(0.050)	(0.50)	(0.064)
	Exposed occ.	-0.070	-0.217***	-0.239***	-0.296***
		(0.046)	(0.072)	(0.077)	(0.101)
Food service	All occ.	0.012	-0.082**	-0.113**	-0.179**
		(0.027)	(0.039)	(0.047)	(0.074)
	Exposed occ.	0.014	-0.075	-0.106*	-0.176*
		(0.033)	(0.049)	(0.061)	(0.095)
Construction	All occ.	-0.054**	-0.099***	-0.158***	-0.234***
		(0.025)	(0.034)	(0.039)	(0.058)
	Exposed occ.	-0.099***	-0.173***	-0.249***	-0.355***
		(0.033)	(0.045)	(0.052)	(0.078)
Transportation	All occ.	-0.037	-0.061**	-0.076***	-0.087**
		(0.029)	(0.031)	(0.028)	(0.034)
	Exposed occ.	-0.053	-0.075*	-0.104***	-0.119***
		(0.032)	(0.038)	(0.036)	(0.044)
Maintenance	All occ.	-0.011	-0.035	-0.049*	-0.062*
		(0.020)	(0.026)	(0.027)	(0.034)
	Exposed occ.	- 0.017	-0.044	-0.062**	-0.075**
		(0.020)	(0.028)	(0.028)	(0.035)
Manufacturing	All occ.	-0.036	-0.078**	-0.098**	-0.118**
		(0.024)	(0.037)	(0.041)	(0.050)
	Exposed occ.	-0.045	-0.082*	-0.109**	-0.124*
		(0.028)	(0.048)	(0.055)	(0.068)
Observations		1,387	1,387	1,387	1,387

Note: All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level. Column (1) reports the OLS estimate of β in Equation (1). Column (2) reports the IV estimate obtained by using the immigrant share in the 10 sectors with the highest immigrant shares. Column (3) reports the IV estimate obtained by using the immigrant share across all sectors. Column (4) reports the IV estimate obtained by using the immigrant share across all other sectors. * (resp. **, resp. ***) denotes statistical significance at the 10% (resp. 5%, resp. 1%) level.

For Online Publication: Appendices

A Background figures

[Figure A.1 about here.]

[Figure A.2 about here.]

B City comparisons and the short-run wage effects of immigration

This section shows that in the presence of trade in capital between cities, the spatial correlation approach tends to underestimate the overall impact of immigration on wages, even if there is no trade in goods across cities.

Consider two cities, A and B. In the short run, capital is mobile between cities, but fixed in the aggregate at $\bar{K}$. Labor L_i, $i \in \{A, B\}$, is immobile. For the sake of the argument, here we assume that immigrant and native labor are perfectly substitutable and that labor is supplied perfectly inelastically. Each city uses the same constant-returns-to-scale technology to produce a homogenous good Q_i: $Q_i = f(L_i, K_i)$. The production function satisfies the law of diminishing marginal returns. The associated unit cost function is denoted $c(w, r)$, with w the wage rate and r the rental on capital. The labor endowment of city B is assumed to be fixed at $\bar{L}_B$, while city A experiences an increase in its labor endowment due to immigration, $\Delta L_A > 0$. For simplicity, we assume that demand in city A is unaffected by immigration, and we write the demand functions as $Q_i = D_i(p_i)$, with $D_i' < 0$ and p_i the local price of the good.

We are interested in the comparative statics $\frac{\partial w_i}{\partial L_A}$, for $i = A, B$, and also in the difference between them, which is what would be identified by exploiting city comparisons in a spatial correlation approach.

B.1 Scenario 1: traded good

If the good is traded between cities, then in equilibrium we have $p_A = p_B$. Under constant returns to scale, we also have $p_i = c(w_i, r)$. Therefore, we must have $w_A = w_B$ (the cost function is monotonically increasing in input prices), and as a result the wage is equalized between cities. Intercity comparisons will reveal an absence of a wage effect.

Nonetheless, the wage decreases in both cities. To see why, note that in equilibrium the wage-to-output-price ratio must be equal to the marginal product of labor in each city, i.e., $\frac{w_i}{p_i} = \frac{\partial f}{\partial L}\left(\frac{L_i}{K_i}, 1\right)$, where we have used the fact that the marginal product of labor is homogenous of degree zero. Since total labor increases in the aggregate due to $\Delta L_A > 0$, while total capital is fixed, the ratio $\frac{L_i}{K_i}$ increases in each city. Because the marginal product of labor decreases in the labor argument, the ratio $\frac{w_i}{p_i}$ declines. Since demand slopes down in each city and the additional labor results in more output in each city, output prices must decline. As a result, wages w_i decline as well.

In this scenario with traded good and traded capital between cities, intercity comparisons of wages would thus reveal *none* of the short-run wage effects of immigration. Note that if the good was traded internationally rather than just between cities, the same conclusion would obtain as long as capital is fixed in the aggregate. If capital and the good were traded internationally, then there would be no wage effect of immigration.

B.2 Scenario 2: non-traded good

If the good is not traded between cities, then the equilibrium can be described by the following set of equations:

$$D_A(p_A) = f(L_A, K_A) \tag{A-1}$$

$$D_B(p_B) = f(\bar{L}_B, K_B) \tag{A-2}$$

$$\frac{w_A}{p_A} = \frac{\partial f}{\partial L}\left(\frac{L_A}{K_A}, 1\right) \tag{A-3}$$

$$\frac{w_B}{p_B} = \frac{\partial f}{\partial L}\left(\frac{\bar{L}_B}{K_B}, 1\right) \tag{A-4}$$

$$p_A \frac{\partial f}{\partial K}\left(1, \frac{K_A}{L_A}\right) = p_B \frac{\partial f}{\partial K}\left(1, \frac{K_B}{\bar{L}_B},\right) \tag{A-5}$$

$$K_A + K_B = \bar{K} \tag{A-6}$$

which constitute a system of 6 equations in 6 unknowns: p_A, p_B, K_A, K_B, w_A, and w_B. We are interested in the effect of a change in L_A, $\Delta L_A > 0$, on these equilibrium variables, specifically the wages w_i.

Case 1: gross complements

First assume that in each city, labor and capital are *gross complements*: that is, an increase in the labor endowment results in an increase in the derived demand for capital. As shown, for instance, in Muth (1964), labor and capital are gross complements whenever the substitution elasticity in production is lower than the (absolute) output demand elasticity. This happens if capital and labor are not too substitutable and output demand is not too inelastic.

Under the assumption of gross complements, the demand for capital rises in city A, which leads to a transfer of capital from city B to city A: $\Delta K_A = -\Delta K_B > 0$. Because labor and capital are gross complements, the outflow of capital from city B results in a reduction in the derived demand for labor, and therefore a reduction in the wage w_B. Output declines in city B, and thus output price increases: $\Delta p_B > 0$. But then, condition (A-5) together with the fact that the marginal product of capital decreases in the capital-to-labor ratio implies that either p_A increases or the ratio $\frac{K_A}{L_A}$ decreases or both. Since $\Delta L_A > 0$ and $\Delta K_A > 0$, output increases in city A and therefore $\Delta p_A < 0$. Therefore, $\Delta\left(\frac{K_A}{L_A}\right) < 0$, and condition (A-3) implies that the wage-to-output-price ratio declines in city A. Since $\Delta p_A < 0$, $\Delta w_A < 0$.

Summarizing, the wage w_i declines in both cities. If we relax the assumption that the inflow of labor into city A does not change the output demand, the conclusion that w_B declines still holds because capital still flows to city A due to the combined effects of labor-capital gross complementarity and the increase in output demand. The conclusion that w_A declines holds as long as it is still the case that $\Delta p_A < 0$, that is, the increase in output demand is not so high as to result in an output price increase. This will hold if the immigrant inflow makes local goods cheaper.

Case 2: gross substitutes

Now assume that labor and capital are gross substitutes in both cities. If the immigrant inflow does not shift the output demand in city A (or not too much), then the derived demand for capital decreases and capital flows towards city B. In city B, the derived demand for labor declines due to gross substitutability, hence the wage rate decreases. Output increases and output price decreases. Condition (A-5) then implies that p_A decreases. Condition (A-3) implies that $\frac{w_A}{p_A}$ decreases, and therefore w_A decreases as well.

Because the spatial correlation approach identifies the effect of immigration from comparing wage changes between city A (the treatment city) and city B (the control city), and the wage declines in both cities, this approach underestimates the total effect and might even predict a positive wage effect if the wage decline in city A is less than in city B.

C Borjas' "relevant wage elasticity"

In his book *Immigration Economics*, as well as in earlier work (Borjas, 2003), George Borjas defines the "relevant wage elasticity" as the percentage change in native wages associated with a percent change in labor supply attributable to immigration (past and present). Denote by w the native wage, by $m = \frac{M}{N}$ the ratio of the immigrant to native workforce, and by $p = \frac{M}{M+N}$ the share of immigrants in the workforce. Borjas' relevant elasticity is then $\eta = \frac{\partial \ln w}{\partial m}$, while the elasticity given by the coefficient on the immigrant share in a regression of the log wage is $\beta = \frac{\partial \ln w}{\partial p}$. Because $p = \frac{m}{1+m}$, it follows that $\eta = \frac{\beta}{(1+m)^2} = \beta\,(1-p)^2$. Therefore, Borjas' "relevant wage elasticity" is directly deducible from the regression of log wage on the immigrant share.

Card and Peri (2016) (and other authors) choose to regress the first-difference of the log wage, $\Delta \ln w$, on the regressor $\frac{\Delta M}{M_{-1}+N_{-1}}$, where $\Delta M = M - M_{-1}$ is the (net) immigrant inflow between the prior and current periods and N_{-1} is the number of native workers in the prior period. Although this specification is sometimes referred to as a first-difference model in the literature, it cannot be obtained by first-differencing any underlying data generating process (DGP) for the determination of wages. Rather, it is a *sui generis* DGP that specifies wage growth as a function of the relative inflow of immigrants. The wage elasticity in Card and Peri (2016)'s model is $\epsilon = \frac{\partial \Delta \ln w}{\partial\left(\frac{\Delta M}{M_{-1}+N_{-1}}\right)}$. Only if $N_{-1} = N$ and $M_{-1} = 0$ can ϵ be related to η. In that case, $\frac{\Delta M}{M_{-1}+N_{-1}} = \frac{M}{N} = \frac{M}{N} - \frac{M_{-1}}{N_{-1}} = \Delta\left(\frac{M}{N}\right)$ and Card and Peri (2016)'s regression becomes the first-differenced version of a regression with m as the regressor, which implies $\epsilon = \eta$.

D Residualized dependent variables

As explained in Section 4.1, the dependent variables used in our analysis are generated by running sector-specific regressions with individual-level outcomes on a full set of MSA-year fixed effects as well as a set of individual observables.[28] Following Reed and Danziger (2007), we use the MSA-year effects to construct "residualized" dependent variables that are used in our final analysis, the difference being that we construct MSA-year effects separately for each economic sector considered. The regressions we use to residualize our dependent variables are commonly referred to as Mincer models, which originates from the work of Mincer (1958) who is accredited with pioneering the use of factors other than school, such as work experience, to explain differences in individual labor market outcomes. Our model controls for educational attainment, race, work experience, gender, and marital status, as follows (to alleviate notation, there is no explicit index to denote the sector):

$$\begin{aligned} \mathrm{O}_{kit} &= \gamma_0 + y_{it} + \gamma_1 \mathrm{HS}_{kit} + \gamma_2 \mathrm{AA}_{kit} + \gamma_3 \mathrm{Black}_{kit} + \gamma_4 \mathrm{Other}_{kit} \\ &\quad + \gamma_5 \mathrm{Exp}_{kit} + \gamma_6 \mathrm{Exp}^2_{kit} + \gamma_7 \mathrm{Fem}_{kit} + \gamma_8 \mathrm{Mar}_{kit} + \psi_{kit} \end{aligned}$$

[28] This is not a panel regression. Although the sample contains multiple time periods, we are not able to identify individuals over time. For each sector, we pool the individual observations from each cross-section into a single sample and run the regression on the sample of pooled cross-sections.

where O_{kit} is the outcome for individual k in MSA i in survey year t, HS_{kit} is a dummy variable that identifies individuals who have at least a high school education but not an Associate's (or higher) degree, AA_{kit} is a dummy variable for having at least an Associate's degree, Black_{kit} is a dummy variable that identifies black individuals, Other_{kit} is a dummy variable that identifies individuals who are neither white or black, Exp_{kit} is an individual's potential work experience, which is defined as the individual's age minus their years of schooling minus six (the typical age for starting school), Exp^2_{kit} is potential work experience squared, Fem_{kit} is a dummy variable for being female, Mar_{kit} is a dummy variable for being married, and ψ_{kit} is the error term. The MSA-year fixed effects from these regressions y_{it} are then used as the dependent variables in our main sectoral analysis. For a given sector, y_{it} effectively captures the average outcome for each MSA in each year after controlling for a set of individual-level observables.

E Shift-share instrument

The shift-share instrument we use is equivalent to the one presented in Borjas (2014) except that we construct it to predict the immigrant share for each MSA-year cell across all skill groups rather than for individual skill groups within each MSA-year cell. Borrowing Borjas' notation and omitting the reference to skill group, let $M_r^k(1980)$ be the labor supply of immigrants from country of origin k in MSA r in the baseline year (1980). The share of immigrants in an MSA who are from a particular country of origin in the baseline year can be expressed as:

$$g_r^k(1980) = \frac{M_r^k(1980)}{\sum_r M_r^k(1980)}.$$

Defining $M^k(t)$ as the *national* number of immigrants from country of origin k at time t, we generate the predicted number of immigrants in an MSA at time t $\hat{M}_r(t)$ as follows:

$$\hat{M}_r(t) = \sum_k g_r^k(1980) M^k(t).$$

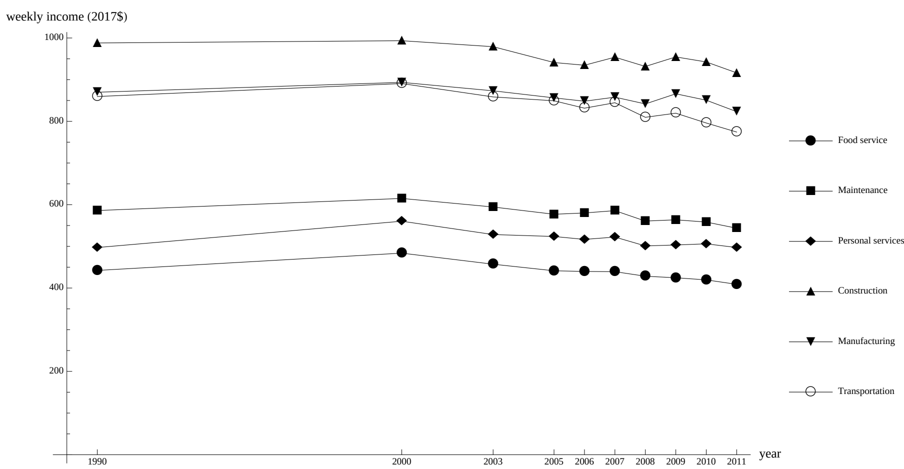
Using $\hat{M}_r(t)$ and the corresponding actual number of natives in MSA r in year t $N_r(t)$, we generate the predicted share of immigrants in each MSA for each time period (i.e. the shift-share instrument):

$$\hat{P}_r(t) = \frac{\hat{M}_r(t)}{\hat{M}_r(t) + N_r(t)}.$$

[Table E.1 about here.]

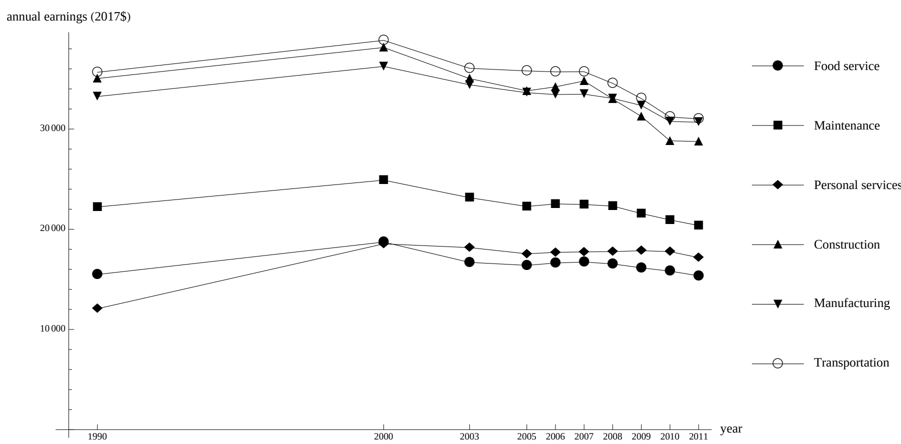
Results from using this instrument are presented in Table E.1. The analysis is conducted using all sectors of the economy in the 150 most populous MSAs. Using this method, we are unable to reject the null hypothesis that the effects of immigration on any of our outcomes are significantly different from zero. This stands in sharp contrast to the sectoral results presented in this paper.

Figure A.1: Evolution of weekly earnings of natives by sector, all occupations



Source: IPUMS data processed by the authors. Weekly earnings are calculated over natives aged 18-64, not living in group quarters, not in school, in the labor force, and with weekly earnings above \$50 and below \$5,769.23 (in 2017\$). Individuals are considered not to be in the labor force if they report being out of the labor force at the time of the survey and working zero weeks during the previous year. Weekly earnings include wages and income from a person's own business or farm.

Figure A.2: Evolution of annual earnings of natives by sector, immigration-exposed occupations



Source: IPUMS data processed by the authors. Earnings are calculated over natives aged 18-64, not living in group quarters, not in school, in the labor force, and with annual earnings above zero and below $300,000 (in 2017$). Individuals are considered not to be in the labor force if they report being out of the labor force at the time of the survey and working zero weeks during the previous year. Annual earnings include wages or income from a person's own business or farm. To define immigrant-exposed occupations, we select occupations with the highest immigrant shares within a sector, until the total number of native workers in those occupations exceeds 50% of the total native workforce in the sector.

Table E.1: Shift-share instrument results

Annual earnings	0.047
	(0.271)
Weekly earnings	0.163
	(0.182)
Employment rate	0.018
	(0.103)
share working 48 weeks or more	0.003
	(0.124)
share working 40 weeks or more	0.051
	(0.154)
share working 27 weeks or more	0.036
	(0.118)
share working 14 weeks or more	-0.039
	(0.063)
share working 1 week or more	0.054
	(0.049)
Observations	1,387

Note: All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level. * (resp. **, resp. ***) denotes statistical significance at the 10% (resp. 5%, resp. 1%) level.