

The Short-Run Impacts of Immigration on Native Workers: Evidence from the US Construction Sector

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Abstract

This paper provides new empirical estimates of the short-run impacts of immigration on the employment opportunities of US-born workers. We focus on the construction sector, a primary employer of immigrant workers in the US and one of the economic sectors with the highest share of immigrants, about 29% in 2016 according to the Bureau of Labor Statistics. Using panel data at the metropolitan area-year level of aggregation constructed from US Census and American Community Survey data, we find that a 10 percentage point increase in the share of immigrant workers reduces annual earnings of US-born construction workers by at least 3.7%, with workers in immigrant-prone trades experiencing earnings reductions in excess of 7.3%. These bounds are derived using a so-called “imperfect instrument approach” (Nevo and Rosen, 2012), whereby the share of immigrant workers is instrumented by the share of immigrants across all sectors of the economy. Our partial identification strategy relies on the assumptions that the share of immigrants across all economic sectors in a market is positively correlated with construction-specific labor demand shocks about location and year effects, but less so than the share of immigrants in construction. Our results further indicate that US-born workers experience lower annual wages through reduced employment (fewer weeks worked per year) rather than lower weekly wages. In immigrant-prone trades, the unemployment rate of natives is predicted to increase by at least 3.7 percentage points for each 10 percentage point increase in the immigrant share.

1 Introduction

The impact of immigration on the labor market outcomes of native-born citizens is, or last least ought to be, a key element of the debate on immigration policy (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), and Ottaviano and Peri (2012), 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. We use a novel partial identification strategy that, to our knowledge, has not been exploited in the related literature to date. Our approach yields upper bounds for the short-run impact of immigration on native workers' earnings, occupational levels, and employment rates that are consistently negative and of larger magnitude than most recent estimates, suggesting that there exist transitory costs to immigration for the native population.

Admittedly, the empirical identification of the immigration-native-outcome relationship is riddled 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.

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 encountered by researchers. It affects cross-sectional and time-series/panel approaches alike: just like cities with relatively high native wages may attract immigrants, periods of time where 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 will depress local wages until wages are equalized across cities: comparing native wages between immigration-affected and immigration-free areas will then reveal an absence of a 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, local wages will be equalized. Of course, this does not necessarily mean that native workers are not negatively affected 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.¹

¹See Appendix A for a proof of this claim.

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 obtain 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.² 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)).

Perhaps more importantly, a recent paper by Ruist et al. (2017) 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.

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.

In this paper, we leverage a novel partial identification method formalized by Nevo and Rosen

²As explained by Dustmann et al. (2016), there are two main types of spatial correlation studies. The first type uses variation in *total* immigrant influxes across space to identify the *total* impact of immigration on native outcomes. The second type, coined as the “mixture approach,” exploits spatial variation in the *skill composition* of immigrant influxes to identify the *relative* impact of immigration between skill groups. Our approach belongs to the first category.

(2012) to address the effect of increased immigration on the employment opportunities of native-born workers. Our partial identification strategy relies on the use of a series of so-called “imperfect instruments:” instruments for the share of immigrants in a given city and year that, although still potentially correlated with the error term (unobserved demand pulls about city and year averages), are plausibly less correlated with it than the regressor itself (but 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 still biased. However, Nevo and Rosen (2012) show that under certain conditions, the IIV estimate can be used as a lower or upper bound to the coefficient of interest.³ We use their insights to derive upper bounds for the negative effects of immigration on native employment conditions. These upper bounds are negative, statistically significant, and larger in magnitude than comparable estimates derived in the US context for recent decades. However, our estimates for wage effects are consistent with the latest figures derived by Ruist et al. (2017) for the earlier decade 1970-1980.

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.⁴ Our approach focusses on one sector of the economy—construction—in order to use as an IIV for the share of immigrants in that sector the share of immigrants *across all sectors*. This variable is plausibly correlated with demand pulls that affect native employment/wages in the construction sector in the same direction as the immigrant share in construction: economic booms attract immigrants across all sectors, and they increase employment opportunities for natives in construction. However, since the immigrant share pertains to the entire economy, rather than the construction sector itself, it is likely less correlated with the construction-specific demand pulls than the immigrant share in construction. Our IIV estimates, which are systematically much more negative than the OLS estimates, confirm this intuition. We find that a 10 percentage point increase in the share of immigrant workers in construction (which falls short of the increase that occurred in the US over the period of investigation) causes at least a 3.7% drop in the annual earnings of natives. This figure is nearly doubled for construction trades most exposed to immigrant inflows. Our findings further suggest that the income impact is channeled through a reduction in the occupational level of natives (fewer weeks worked per year) rather than a reduction in the weekly wage. Indeed, a 10 percentage point increase in the immigrant share is found to cause at least a 2.3 percentage point increase in native unemployment. This figure rises to 3.7 percentage points for exposed construction trades.⁵

³They also show how one may derive two-sided bounds, but for reasons highlighted below, our setting does not allow such derivation.

⁴One 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); Ruist et al. (2017)) the improvement is minimal, suggesting that the IIV correlation with the error term remains high in comparison with that of the regressor.

⁵The unemployment result for exposed construction trades is larger in magnitude than the figures derived by

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 classical shift-share instrument. Our strategy acknowledges the inherent remaining correlation between unobserved labor demand shocks and our instrument but leverages it to derive a meaningful upper bound on the negative impacts of immigration on natives' employment conditions. Second, in spite of the fact that spatial correlation estimates may mask larger national effects (Borjas, 2003), our empirical estimates of these impacts are larger in magnitude than most recent estimates for the US, suggesting that natives can be hurt by immigration in the short run. Third, we shed light on the US construction sector, one of the industries most exposed to immigration in recent decades. There are other reasons to focus on the construction sector: because the goods produced (buildings, roads) are not traded across cities or internationally, the impacts of immigration are less likely to quickly dissipate in the broader economy than in other sectors like manufacturing, making it a particularly suitable setting for the spatial correlation approach.⁶ Finally, our findings highlight the importance of investigating effects on occupational levels and the employment rate, in addition to wages. While we detect absolutely no effect of immigration on weekly wages, we find relatively large effects on unemployment and occupational levels, leading to sizable impacts on annual earnings.

The rest of the article is organized as follows. Section 2 discusses recent immigration trends in the construction sector in the US. 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

There were 25,779,000 foreign-born workers aged 16 and above in the US in 2016 (U.S. Department of Labor, Bureau of Labor Statistics, 2017). Construction and extraction occupations attracted 9.1% of these workers, making these occupations the single category with the highest number of immigrants, and one with an immigrant share of 29%. According to the US Bureau of Labor Statistics, the US construction industry itself employed about 6.7 million workers in 2016. Over 5 million of these workers were involved in non-supervisory and production activities (<https://www.bls.gov/iag/tgs/iag23.htm#workforce>).

This section to be expanded.

Reed and Danziger (2007), who study the impacts of immigration on low-skilled native workers by race in the US over the period 1989-1999.

⁶See Appendix A. There we show that if labor is immobile across cities, immigration impacts only partially diffuse if goods are not traded, even if capital is mobile.

3 Data

The data used for this analysis was obtained from the Integrated Public Use Microdata Series provided by the University of Minnesota at ipums.org. Our data includes US Census data from 1990 and 2000 as well as American Community Survey (ACS) data between the years 2001 and 2011. Due to a missing geographic variable that prevents us from assigning a geographic location to workers, the years 2001, 2002, and 2004 are excluded from our data set.

Table 1: Summary statistics

variable	unit	mean	s.d.
log annual earnings (all trades)	log 2005\$	10.01	0.24
log annual earnings (immigration-exposed trades)	log 2005\$	9.74	0.32
log weekly earnings (all trades)	log 2005\$	6.41	0.19
log weekly earnings (immigration-exposed trades)	log 2005\$	6.25	0.23
unemployment rate (all trades)		0.14	0.08
unemployment rate (immigration-exposed trades)		0.17	0.11
share of immigrant workers in construction		0.15	0.15
share of immigrant workers in top-5 immigrant industries		0.14	0.13
share of immigrant workers in top-10 immigrant industries		0.11	0.10
share of immigrant workers in all industries		0.11	0.10

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 Metropolitan Statistical Area (MSA) where the individual lives (taken to be the relevant labor market), and their birthplace (used to generate the immigrant variable). We generate the following variables at the MSA-year level: the average (log) earnings of native-born construction workers, the proportion of native-born workers working full time, and the proportions of immigrant workers working in construction and other sectors of the economy. Our final panel, which includes 283 MSAs, is unbalanced as some MSAs are not represented in some years. We explore the effect of removing MSAs with a small number of individual-level observations in Appendix D.

Table 1 summarizes our data. Note that the mean and standard errors are calculated across MSAs and years. Since MSAs have different sizes, the mean values may not be representative of national averages. In particular, our average measure of immigrant share in construction (15%) is well below the one computed at the national level.

4 Methodology

The main difficulty in measuring the effect of immigration on the labor market outcomes of native-born construction workers is that increases in immigration are likely correlated with unobserved

demand-pull factors in the construction industry which also affect natives’ income and employment. In order to estimate the effect of immigration on the labor market outcomes of native-born construction workers, we use the instrumental variable approach described below.

4.1 Model specification and IV

Our dependent variables include the average of the natural logarithm of the annual earnings of native-born construction workers in each MSA m and year t ($\overline{\ln(I_{m,t})}$), the proportion of native-born construction workers working full time in each MSA and year ($F_{m,t}$), and the proportion of natives in the construction labor force who are employed.⁷ In order to identify the effect of immigration on the distribution of native construction 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.

Our main regressor is a measure of immigration defined as the fraction of foreign-born construction workers relative to the total construction workforce in each MSA and year ($S_{m,t}^C$). Apart from the fact that we focus on the construction sector and do not differentiate by skill, this is the same regressor as that used by Altonji and Card (1991), Borjas (2003), or Borjas (2014), and it is directly related to the one used in Dustmann et al. (2005)—the ratio of immigrant to native workers.

In a recent review of George Borjas’ *Immigration Economics*, Card and Peri (2016) criticize the use of this 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 us to believe that the immigrant population, by nature, 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. Card (2001)’s results further suggest little migratory response of natives to immigration shocks. 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.⁸

⁷The 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.

⁸Card and Peri (2016) also write that Borjas, in spite of using the immigrant share regressor in his empirical chapters, implies elsewhere that their regressor is more relevant than his when he defines the “relevant wage elasticity”

Our preferred instrument is a variable that measures the proportion of immigrants across all occupations in the economy, including construction ($S_{m,t}^A$). This instrument would be the regressor used in a spatial correlation approach that considers all sectors of the economy rather than one in isolation. Although $S_{m,t}^A$ is likely still correlated with unobservable construction labor demand-pull factors, it is plausibly less correlated with the error term than the regressor. We also use two variants of the instrument $S_{m,t}^A$, constructed using either the 5 or the 10 industries with the highest proportion of immigrants (construction belongs to these two groups).

We first estimate the effect of immigration on the annual income of native workers:

$$\overline{\ln(I_{m,t})} = \beta S_{m,t}^C + \alpha_m + \phi_t + \epsilon_{m,t} \quad (1)$$

where α_m is a MSA fixed effect, ϕ_t is a year fixed effect, and our coefficient of interest is β . The regressor $S_{m,t}^C$ is instrumented using the share of immigrants across all occupations, $S_{m,t}^A$. The coefficient β is interpreted as follows. A 10 percentage point increase in the share of immigrants working in construction causes a $10 \times \beta$ percent change in the annual income of native-born construction workers.

We also run a model with weekly income, that is, annual income divided by the number of weeks worked, conditional on being employed.

Finally, we estimate models where the dependent variable is the share of full-time workers amongst natives, $F_{m,t}$ (with variations regarding the actual definition of full-time workers), or the share of natives in the labor force who are employed:

$$F_{m,t} = \gamma S_{m,t}^C + \alpha_m + \phi_t + \epsilon_{m,t} \quad (2)$$

The coefficient of interest in this series of models is γ . The interpretation is that a 10 percentage point increase in the proportion of immigrants working in construction causes a $10 \times \gamma$ percentage point change in the proportion of native workers working full time. In all regressions, standard errors are clustered at the MSA level.

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.” Defining the relevant wage elasticity the way Borjas does in no way implies that the inflow regressor is more relevant, because Borjas defines the immigration-induced percent increase in the labor supply as the ratio of *current foreign-born workers* to *current US-born workers*. In fact, as Borjas explains, this relevant wage elasticity can be directly deduced from the estimate of the coefficient on the immigrant share, whereas it could not 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 B). The main difference is that Borjas’ (and our) regressor considers immigrants irrespective of the timing of their arrival, whereas Card and Peri (2016) implicitly consider that the effect of immigration on native wages is only channeled through the most recent inflow. In particular, their specification assumes that the effect of an immigrant inflow on native wage growth does not depend on the initial native-immigrant composition of the host city.

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. To ease the reader's understanding of our application of IIV theory, let us adopt the same notation as in Nevo and Rosen (2012). We write the DGP underlying models (1) and (2) as

$$Y = X\beta + \mathbf{W}\delta + U \quad (3)$$

where Y is the dependent variable, X is the immigrant share in construction (S^C), W is row a 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, S^A . We denote by Z_2 the alternative instrument constructed as the share of immigrant workers in industries with the 10 highest shares of immigrant workers.

For two random variables, say X and Y , σ_{xy} denotes the covariance between X and Y . 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 (3).

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

$$\begin{cases} \tilde{X} = X - \mathbf{W}\mathbb{E}[\mathbf{W}'\mathbf{W}]^{-1}\mathbb{E}[\mathbf{W}'X] \\ \tilde{Y} = Y - \mathbf{W}\mathbb{E}[\mathbf{W}'\mathbf{W}]^{-1}\mathbb{E}[\mathbf{W}'Y] \end{cases} \quad (4)$$

\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

$$\begin{cases} \beta^{\text{OLS}} = \beta + \frac{\sigma_{\tilde{x}u}}{\sigma_{\tilde{x}}^2} \\ \beta_z^{\text{IV}} = \beta + \frac{\sigma_{zu}}{\sigma_{\tilde{x}z}} \end{cases} \quad (5)$$

To fix ideas, consider the case where the dependent variable is annual native income, which implies that $\sigma_{xu} > 0$ since unobserved demand pulls would tend to increase native wages and the native employment rate. Since U is uncorrelated with \mathbf{W} , $\sigma_{\tilde{x}u} = \sigma_{xu} > 0$ and 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 (3) 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 the construction sector and the rest of the economy.⁹ Because $\sigma_{zu} \geq 0$ from Assumption 1, equation (5) 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, $\beta_z^{\text{IV}} < \beta^{\text{OLS}} \Leftrightarrow \sigma_{zu}\sigma_x^2 - \sigma_{\tilde{x}u}\sigma_{\tilde{x}z} < 0 \Leftrightarrow \rho_{zu} < \rho_{\tilde{x}u}\rho_{\tilde{x}z} = \rho_{xu}\rho_{\tilde{x}z}$. Therefore, 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.

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 case), one may be able to further improve on the upper bound β_z^{IV} by using a combined instrument defined as $V(1) = \sigma_x Z - \sigma_z X$.¹⁰ The probability limit of the corresponding IV estimator can be derived as

$$\beta_{V(1)}^{\text{IV}} = \beta + \frac{\sigma_x\sigma_{zu} - \sigma_z\sigma_{xu}}{\sigma_x\sigma_{\tilde{x}z} - \sigma_z\sigma_{\tilde{x}x}}. \quad (6)$$

But under the above assumptions, it turns out that $\beta_z^{\text{IV}} < \beta_{V(1)}^{\text{IV}} \Leftrightarrow \beta_z^{\text{IV}} < \beta^{\text{OLS}} \Leftrightarrow \beta^{\text{OLS}} < \beta_{V(1)}^{\text{IV}}$, and therefore the use of $V(1)$ as an instrument does not improve on either β_z^{IV} or even 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

$$\beta_{\omega(\gamma)}^{\text{IV}} = \beta + \frac{\sigma_{\omega(\gamma)u}}{\sigma_{\tilde{x}\omega(\gamma)}} \quad (7)$$

implying that $\beta_{\omega(\gamma)}^{\text{IV}}$ 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 $\sigma_{\tilde{y}z_1}\sigma_{\tilde{x}z_2} - \sigma_{\tilde{y}z_2}\sigma_{\tilde{x}z_1} < 0$. While 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

⁹ \tilde{Z} denotes the residual from the regression of Z on \mathbf{W} .

¹⁰This 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 .

$\sigma_{\bar{x}\omega(\gamma)} < 0$, which, given $\sigma_{\bar{x}z_j} > 0$, is equivalent to $\gamma < \bar{\gamma} \equiv \frac{\sigma_{\bar{x}z_1}}{\sigma_{\bar{x}z_1} + \sigma_{\bar{x}z_2}}$), one can only deduce that $-\infty = \beta_{\omega(\bar{\gamma})}^{IV} < \beta$, that is, the lower bound is uninformative. In what follows, we therefore only report the values of β^{OLS} and β_z^{IV} , with β_z^{IV} being our tightest upper bound.

5 Results

We first report results for the effect of immigration on the annual income of native-born workers. Table 2 shows that the annual income of native construction workers is negatively affected by the share of immigrants in construction. Although the OLS estimate is not statistically significant, the IIV-All estimate (corresponding to the share of immigrants across all industries) is, and it is much larger in magnitude. Importantly, the move from the IIV constructed from the share of immigrants in immigrant-prone industries to that constructed from the share of immigrants across all industries has the expected effect on the point estimate: the effect becomes more negative as the correlation between the IIV and the error term is attenuated. The attenuation comes from the fact that in industries less prone to immigration, a positive shock in labor demand (which we assume is positively correlated with a positive shock in the demand for construction labor) may not correlate as much with an increase in the share of immigrant workers as in industries with larger immigrant shares. In addition, since the correlation of interest is with demand pulls *in construction*, the fact that the share of immigrants is calculated across a broader set of industries mechanically “dilutes” the correlation with any construction-specific shock in labor demand.

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

	OLS	IIV-5	IIV-10	IIV-All
All construction occupations	-0.112 (0.072)	-0.182 (0.112)	-0.255* (0.123)	-0.366** (0.120)
Exposed construction occupations	-0.245* (0.124)	-0.426 (0.237)	-0.517* (0.222)	-0.728** (0.199)

Note: All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level. Estimates correspond to coefficient β in Equation (1). * (resp. **) denotes statistical significance at the 5% (resp. 1%) level.

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 estimate implies that a 10 percentage point increase in the share of immigrants in construction is associated with at least a 3.7 percent decrease in the annual earnings of native workers. Table 2 further shows that the effect is greatly accentuated for workers in trades where the share of immigrants is higher (e.g., carpenters, painters, masons, roofers). For those workers, a 10 percentage point increase in the

share of immigrants in construction is associated with at least a 7.3 percent decrease in annual earnings.

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 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 and a shift-share instrumental variable approach. 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.¹¹

There are two essential channels by which the annual earnings of native-born workers may be impacted by immigration flows: their wage rate may decrease and/or they may work fewer weeks per year. The second channel is particularly relevant for the construction sector because construction workers are usually paid per “job.” That is, they go from one job to the next and bill hours spent on each job. If they have difficulty filling in their schedule, perhaps due to increased competition from cheaper, abundant immigrant labor, they may end up with lower annual earnings even if their weekly earnings have not changed. The “stickiness” of natives’ weekly earnings in construction is a possibility since a large share of construction workers are unionized.¹²

Our results supports this hypothesis: the effect of immigration on the weekly earnings of natives is insignificant, however immigration triggers a clear redistribution of natives away from full-time and high-time work towards part-time work and unemployment.

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

	OLS	IIV-5	IIV-10	IIV-All
All construction occupations	-0.002 (0.048)	-0.001 (0.081)	0.033 (0.084)	-0.009 (0.083)
Exposed construction occupations	-0.046 (0.089)	-0.044 (0.172)	-0.068 (0.146)	-0.178 (0.127)

Note: All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level.

Evidence for this preferred channel is presented in Tables 3 and 4. Table 3 shows that the effect of immigration on the weekly earnings of natives is small and not statistically significant. Here, the clear pattern of increasing sensitivity when moving towards the use of less endogenous instruments disappears.

¹¹Admittedly, 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. They are also less negative than the estimates derived for the decade 1970-1980 by Ruist et al. (2017).

¹²A non-negligible share of construction workers are self-employed, although the majority are salaried. Our sample also reveals that a given worker may receive wage income even if they are categorized as self-employed, suggesting that workers may move in and out of salaried work. For that reason, we do not exclude self-employed workers from the sample, and we use total annual income (as opposed to just wage income) as our measure of income.

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

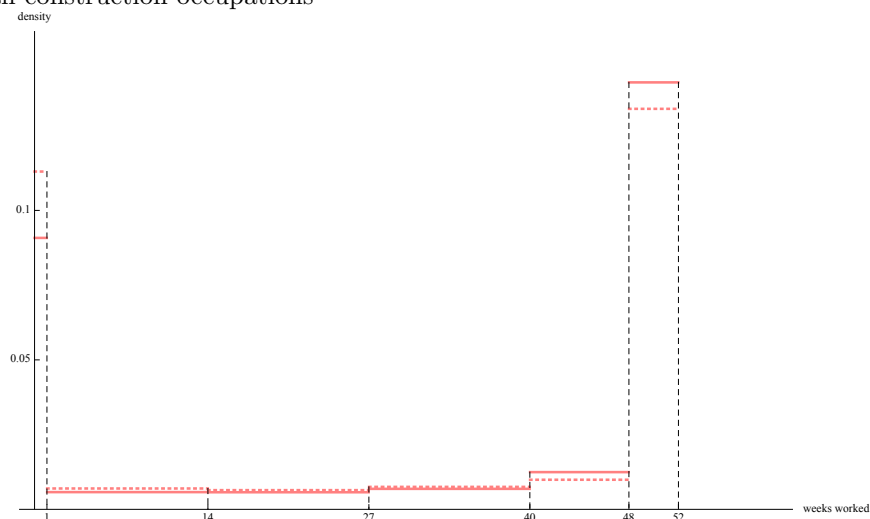
		OLS	IIV-5	IIV-10	IIV-All
All construction occupations	share working 48 weeks or more	-0.036 (0.038)	-0.078 (0.059)	-0.146* (0.068)	-0.177** (0.066)
	share working 40 weeks or more	-0.083* (0.035)	-0.168** (0.055)	-0.240** (0.063)	-0.279** (0.064)
	share working 27 weeks or more	-0.077* (0.031)	-0.134** (0.049)	-0.201** (0.054)	-0.236** (0.054)
	share working 14 weeks or more	-0.070** (0.027)	-0.105** (0.045)	-0.159** (0.047)	-0.190** (0.048)
	share working one week or more	-0.033 (0.022)	-0.071* (0.033)	-0.102** (0.037)	-0.111** (0.039)
Exposed construction occupations	share working 48 weeks or more	-0.115* (0.057)	-0.205* (0.098)	-0.272* (0.107)	-0.298** (0.108)
	share working 40 weeks or more	-0.156** (0.053)	-0.277** (0.090)	-0.336** (0.099)	-0.373** (0.103)
	share working 27 weeks or more	-0.147** (0.051)	-0.280** (0.081)	-0.304** (0.086)	-0.333** (0.089)
	share working 14 weeks or more	-0.121** (0.044)	-0.180* (0.074)	-0.220** (0.076)	-0.250** (0.077)
	share working one week or more	-0.051 (0.034)	-0.147** (0.053)	-0.175** (0.056)	-0.175** (0.055)

Note: All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level. Estimates correspond to coefficient γ in Equation (2). * (resp. **) denotes statistical significance at the 5% (resp. 1%) level.

Table 4 presents the effects of the immigrant share on the share of native construction workers working at least a certain number of weeks per year. Effects are shown for all construction occupations and for immigrant-exposed construction occupations. IIV estimates are mostly significant, and the pattern of increasingly negative effect as the instrument becomes less endogenous is re-established. Overall, the estimates suggest that immigration has a negative effect on the level of employment of native construction workers. For instance, a 10 percentage point increase in the share of immigrants is predicted to result in a 1.8 percentage point decrease in the share of natives working more than 48 weeks. For exposed construction trades, the effect is more pronounced.

To get a better idea of the effect of immigration on the occupational level of natives, Figure 1 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

Figure 1: Effect of a 20 percentage point increase in the immigrant share on native occupational levels, for all construction occupations



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

constructed by using occupational shares averaged across sample years and MSAs. Figure 2 depicts those effects for workers in immigrant-exposed construction trades.

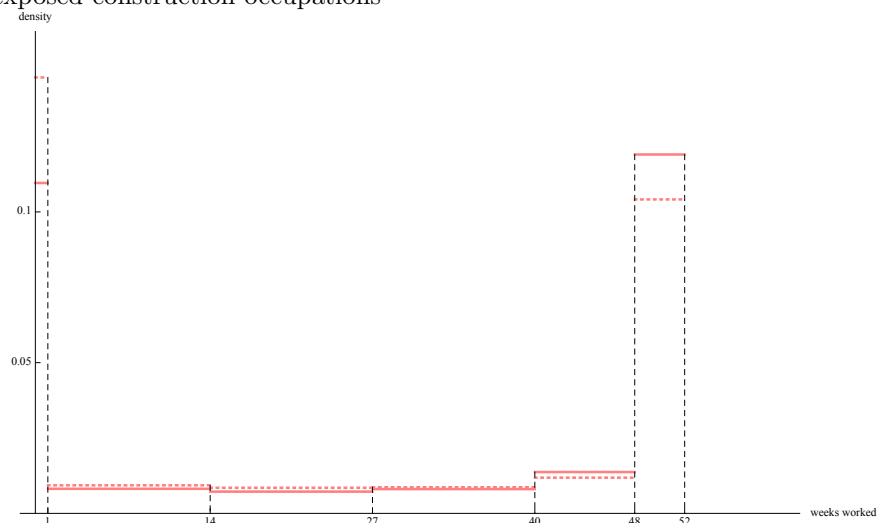
Table 5: Effect of immigration on natives' employment rate

	OLS	IIV-5	IIV-10	IIV-All
All construction occupations	-0.064* (0.026)	-0.150** (0.048)	-0.219** (0.053)	-0.231** (0.049)
Exposed construction occupations	-0.113** (0.043)	-0.264** (0.080)	-0.335** (0.091)	-0.368** (0.089)

Note: All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level.

Finally, we estimate the effect of immigration on natives' self-reported employment status. The employment rate is defined as the share of the active population (those reporting working the previous week or having been in search of a job for the previous four weeks) who reported working the previous week. Table 5 summarizes our findings and shows that the immigrant share has a large and negative effect of natives' employment rate: at least -2.3 percentage points for each 10 percentage point increase in the immigrant share, and a larger effect amongst workers in exposed trades.

Figure 2: Effect of a 20 percentage point increase in the immigrant share on native occupational levels, for exposed construction occupations



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

6 Conclusion

This paper provides new estimates of the effects of immigration on the employment conditions of natives, focussing on the US construction sector over the period 1990-2011. The focus on this sector of the economy is necessary for the deployment of our partial identification strategy, which uses the share of immigrant workers in the broader economy as an imperfect instrumental variable for the share of immigrant workers in construction.

We find evidence that immigration deteriorates the employment conditions of natives in construction in the short run. We are not able to detect any significant effect on weekly earnings, however we find strong evidence that immigration displaces native construction workers towards lower occupational levels, resulting in significantly lower annual earnings, at least minus 3.7% per each 10 percentage point increase in the immigrant share, and minus 7.3% in construction trades most exposed to immigration. Effects on natives' employment rates (defined as the percentage of workers employed during the week prior to being surveyed or actively seeking work during the four weeks prior to being surveyed) are also negative and meaningful: a 10 percentage point increase in the immigrant share is predicted to result in an increase in native unemployment by 2.3 percentage points, and by 3.7 percentage points in exposed trades. These figures should be interpreted as upper bounds (meaning that the true effects are larger in magnitude) for at least two reasons: first, the IIV 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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Appendices

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

The goal of this section is to show 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 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.

A.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 real 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.

A.2 Scenario 2: non-traded good

If the good is not traded between cities, as is the case with most construction-sector outputs, 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 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 .

B 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 immigrant inflow between the prior and the 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 $\epsilon = \eta$.

C Treatment of data

This section to be completed.

D Robustness checks

D.1 Selection of MSAs

We remove MSAs for which the total number of construction workers (native or immigrant) in the 2000 Census (5% sample of the population, the largest sample across years in our data set) is below 250 workers. This reduces the number of MSAs in the sample from 283 to 188.

Table A.1: Effect of immigration on the annual earnings of native-born construction workers (small MSAs removed)

	OLS	IIV-5	IIV-10	IIV-All
All construction occupations	-0.109 (0.084)	-0.266** (0.111)	-0.356** (0.122)	-0.409** (0.124)
Exposed construction occupations	-0.196 (0.131)	-0.364 (0.241)	-0.574** (0.219)	-0.681** (0.200)

Note: All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level. Estimates correspond to coefficient β in Equation (1). * (resp. **) denotes statistical significance at the 5% (resp. 1%) level.

Table A.2: Effect of immigration on the weekly earnings of native-born construction workers (small MSAs removed)

	OLS	IIV-5	IIV-10	IIV-All
All construction occupations	-0.044 (0.058)	-0.109 (0.071)	-0.124 (0.076)	-0.124 (0.077)
Exposed construction occupations	-0.062 (0.092)	-0.065 (0.167)	-0.150 (0.140)	-0.226 (0.120)

Note: All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level.

Table A.3: Effect of immigration on the share of native-born construction workers working full time (small MSAs removed)

		OLS	IIV-5	IIV-10	IIV-All
All construction occupations	share working 48 weeks or more	-0.010 (0.042)	-0.075 (0.064)	-0.127 (0.070)	-0.160* (0.070)
	share working 40 weeks or more	-0.045 (0.036)	-0.161** (0.054)	-0.223** (0.062)	-0.254** (0.065)
Exposed construction occupations	share working 48 weeks or more	-0.069 (0.063)	-0.116 (0.092)	-0.213* (0.97)	-0.242* (0.095)
	share working 40 weeks or more	-0.092 (0.056)	-0.211* (0.084)	-0.313** (0.092)	-0.349** (0.094)

Note: All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level. Estimates correspond to coefficient γ in Equation (2). * (resp. **) denotes statistical significance at the 5% (resp. 1%) level.

Table A.4: Effect of immigration on natives' employment rate (small MSAs removed)

	OLS	IIV-5	IIV-10	IIV-All
All construction occupations	-0.062* (0.029)	-0.187** (0.052)	-0.263** (0.054)	-0.264** (0.050)
Exposed construction occupations	-0.141** (0.042)	-0.262** (0.068)	-0.353** (0.069)	-0.354** (0.067)

Note: All regressions include MSA and year fixed effects. Standard errors are clustered at the MSA level.