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Immigration, Local Crowd-Out and Undercoverage Bias

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Abstract
Using decadal census data since 1960, I cannot reject the hypothesis that new immigrants crowd out existing residents from US commuting zones and states one-for-one. The effect is entirely driven by a reduction in internal inflows rather than larger outflows. My estimate is precise and robust to numerous specifications, as well as accounting for local dynamics - and I show how it can reconciled with apparently conflicting results in the literature. On imposing more structure, I attribute about 30 percent of the observed effect to mismeasurement - specifically undercoverage of undocumented migrants. Though labor demand does respond, the burden of adjustment falls mostly on population. These results have important methodological implications for the estimation and interpretation of the impact of immigration, both locally and nationally.

Key words: immigration, geographical mobility, local labor markets, employment
JEL Codes: J61; J64; R23

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1 Introduction

In the US and elsewhere, new immigrants cluster heavily in particular cities and regions. This variation is regularly exploited to identify the labor market impact of immigration, often using historical migrant enclaves as instruments for causal identification. Seminal examples include Altonji and Card (1991), Card (2001), Cortes (2008), Peri and Sparber (2009), Dustmann, Frattini and Preston (2012), Monras (forthcoming) and Dustmann, Schoenberg and Stuhler (2017); see Dustmann, Schoenberg and Stuhler (2016) and Jaeger, Ruist and Stuhler (2018) for excellent surveys. But as Borjas, Freeman and Katz (1997) note, the interpretation of such estimates depends crucially on the response from internal mobility: the local effect may understate the aggregate-level impact if new immigrants crowd out existing residents geographically.

This insight has generated a long-standing debate on the magnitude of this internal response. I re-evaluate this question using decadal US census data covering 722 commuting zones (CZs) and 50 years, and applying an empirical model which explicitly accounts for dynamic adjustment (borrowed from Amior and Manning, 2018, and Amior, 2019a). Using the enclave instrument, I estimate a surprisingly large and precise crowding out effect: for each new foreign arrival to a CZ, 1.1 existing residents leave on net. This effect is robust to numerous specification choices; and even without the instrument, it is very large (the OLS effect is 0.76). It is also educationally “balanced”: the college share of both foreign inflows and the residual population response (elicited by the instrument) resemble the existing population. And it is entirely driven by a reduction in internal inflows rather than larger outflows, consistent with Dustmann, Schoenberg and Stuhler (2017) - and hence my preference for the “crowding out” terminology over (the more typical) “displacement”. I base my main estimates on CZs, but I cannot reject one-for-one crowd-out across US states either.

The size of my 1.1 crowding out estimate is certainly puzzling. Not only does it conflict with much of the existing literature (as I describe below), but even with my own results - and for two reasons in particular. First, the crowding out effect declines significantly (from 1.1 to 0.9) once I condition on local employment growth (suitably instrumented). But to the extent that employment responds to population, theory predicts that the unconditional effect (the focus of this paper) should be smaller. Second, I identify small but significant negative effects of foreign inflows on local employment rates\(^1\), especially among the low educated: this suggest that local labor market adjustment is partial, which is difficult to reconcile with perfect crowd-out.

I argue that these apparent inconsistencies are a consequence of undercoverage of undocumented migrants in the census. In an effort to quantify this bias, I impose more structure. Crucially, I show that controlling for employment growth (in the crowding

\(^1\)See also Card (2001), Smith (2012), Edo and Rapoport (2017) and Gould (2019).
out equation) can in principle eliminate the bulk of this bias: intuitively, the bias in the employment control effectively partials out the bias in foreign inflows. Using estimates of the employment elasticity, I can then impute a “semi-structural” estimate of unconditional crowd-out: I calibrate this to about 0.8, which implies an undercoverage bias of 30 percent (relative to my original 1.1 estimate). I attribute the remaining effect to both the labor market impact and compositional disamenities, i.e. native distaste for immigration. Though labor demand does respond, the burden of adjustment falls mostly on population.

This undercoverage bias has important methodological implications for the estimation and interpretation of the impact of immigration, both locally and nationally. Not only will empirical estimates of such effects be upward biased, but it also poses challenges to structural estimates: undercoverage bias will distort estimates of the skill composition of migrants, which is the key determinant of the aggregate impact of immigration in standard models (see e.g. Borjas, 2006; Ottaviano and Peri, 2012). An average bias of 30 percent over the last fifty years does not seem unreasonable in the context of existing estimates of undercoverage: see e.g. Warren and Passel (1987), Borjas, Freeman and Lang (1991), Van Hook and Bean (1998), Marcelli and Ong (2002), US Department of Homeland Security (2003) and Card and Lewis (2007). My interpretation of the evidence is also consistent with the much larger estimates of unconditional crowd-out in 1960s and 1970s (reaching almost two-for-one), when coverage was much poorer. As the model predicts though, the conditional crowd-out estimates do not vary systematically over time.

I am not the first to identify substantial geographical crowd-out (see e.g. Filer, 1992; Frey, 1995; 1996; Borjas, Freeman and Katz, 1997; Hatton and Tani, 2005; Borjas, 2006; Borjas, 2014), though Wright, Ellis and Reibel (1997) have disputed Frey’s methodology, and Peri and Sparber (2011) and Card and Peri (2016) have disputed Borjas’. Still, even Borjas’ (2006) crowd-out estimates are smaller than mine: he finds that each immigrant crowds out 0.6 natives from US metro areas, and 0.3 from states. Monras (forthcoming) does identify an initial one-for-one effect following the surge of Mexican migrants during the Peso crisis of 1995, but he finds much less crowd-out over longer horizons. On the other hand, applying a structural model to local wage data, Colas (2018) imputes that crowd-out increases over time following a one-off low skilled migration shock, reaching 0.5 by the tenth year. In complementary work, Burstein et al. (2018) show that migrants crowd out natives from employment in migrant-intensive non-tradable jobs, but this is specifically a within-CZ effect. Dustmann, Schoenberg and Stuhler (2017) find that Czech workers who were permitted to commute across the German border in the early 1990s crowded out German residents one-for-one in local employment. The bulk of the effect (about two thirds) materializes in local non-employment rather than population, though this decomposition only relates to a three year horizon.

Still, the US literature has more typically gravitated to small negative or even pos-

One might account for weak crowd-out in three ways. First, native-born workers may be relatively immobile geographically (Cadena and Kovak, 2016). Second, migrants may shelter natives from labor market effects if they are imperfect substitutes in production: see e.g. Card (2009b), Manacorda, Manning and Wadsworth (2012) and Ottaviano and Peri (2012).2 And third, labor demand may adjust endogenously to immigration, whether through production technology or migrants’ consumption: see Lewis (2011), Dustmann and Glitz (2015) and Hong and McLaren (2015). However, my estimates cast doubt on the importance of these factors: (1) I find that crowd-out is mostly driven by natives; (2) I estimate similar effects of immigration on the employment rates of both natives and migrants; and (3) my estimates of the employment elasticity are theoretically insufficient to offset the very large internal mobility response. The final point is consistent with Blanchard and Katz (1992), Hornbeck (2012) and Amior and Manning (2018), who find that local adjustment (following labor demand shocks) comes almost entirely through population flows rather than employment.

In the final part of the paper, I attempt to reconcile my crowding out results with the existing literature. The seminal work has typically addressed the challenge of omitted local effects by exploiting variation across skill groups within geographical areas (e.g. Card and DiNardo, 2000; Card, 2001, 2005; Borjas, 2006; Cortes, 2008; Monras, forthcoming). That is, they study the effect of skill-specific foreign inflows on local skill composition. But small composition effects are not necessarily inconsistent with large geographical crowd-out - for two reasons. First, these effects reflect not only differential internal mobility, but also changes in the characteristics of local birth cohorts. Indeed, I find that cohort effects have historically offset the impact of geographical crowd-out. And second, as Card (2001) and Dustmann, Schoenberg and Stuhler (2016) point out, within-area estimates do not account for the impact that new migrants exert outside their own skill group - the importance of which depends on elasticities of substitution. This can be seen in the sensitivity of my within-area estimates to the delineation of skill groups. This is not to say that within-area estimates are uninteresting: the impact on local skill composition is certainly an important question. My point is merely that it is not the same as geographical crowd-out.

2For example, Peri and Sparber (2009), D’Amuri and Peri (2014) and Foged and Peri (2016) argue that natives have a comparative advantage in communication-intensive tasks.
In Section 2, I set out my model and derive my empirical specification. Section 3 describes the data; and Section 4 presents my aggregate-level estimates of crowding out, together with numerous robustness checks. In Section 5, I report estimates of the impact of immigration on employment rates, wages and housing costs, and study variation by education. Section 6 assesses the implications of undercoverage for my results; and as part of this exercise, I offer estimates of the elasticity of local employment growth. Finally, Section 7 presents estimates of crowd-out which exploit skill variation within areas, based on a modified version of the model.

2 Model of local crowding out

2.1 Overview

Like Amior and Manning (2018), I set out a theoretical framework with two components: (1) a Roback (1982) style model for local labor market equilibrium, conditional on population, and (2) dynamic equations describing local population adjustment. And like Amior (2019a), I distinguish between the contributions of foreign and internal mobility to population change.

The point of departure here is my choice of outcome. In Amior (2019a), I focus on the size of the foreign contribution and its dynamic implications for local population adjustment. A key insight is that the foreign contribution crowds out the internal migratory response to local employment shocks. As I show there, this effect can be summarized by the impact of foreign inflows on net internal outflows, conditional on local employment growth. I call this “conditional” crowd-out. In contrast, the focus of this paper is “unconditional” crowd-out (i.e. without the employment control), which is the more traditional concern of the migration literature. This unconditional effect accounts for the role of labor demand and the housing market in bringing about adjustment.

To simplify the theoretical exposition, I assume in the model that native and migrant labor are identical and perfectly substitutable. This does not seem unreasonable in the context of the existing literature, which has typically estimated them to be close substitutes. Of course, to the extent that this is false, the model will overstate any effect of immigration on local native welfare. But I do not exclude a role for imperfect substitutability in my empirical specifications. Rather, I identify these equations (derived from the model) using instruments, and I consider the validity of my model’s assumptions ex

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3For example, using US data, Ottaviano and Peri (2012) estimate an elasticity of substitution between natives and migrants of 20 within skill cells. Card (2009a) finds even larger numbers, and Borjas, Grogger and Hanson (2012) and Ruist (2013) question whether they are imperfect substitutes at all. At the aggregate level (which is the relevant point here), differences in native and migrant skill compositions will also matter. These are thought to be similar in the US (at least in terms of college share: see e.g. Card, 2009a), though undercoverage bias may affect this conclusion if it is more severe among the low educated (as I suggest below).
post-following the approach Beaudry, Green and Sand (2012). In practice, it turns out that immigration does exert similar effects on native and migrant employment rates—which is consistent with the model’s assumptions. In a similar spirit, I do not account for skill heterogeneity here; but see Appendix A.3 for an exposition which does, and see Section 7 for associated empirical estimates.

In what follows, I first derive an estimable equation for conditional crowd-out, following identical steps to Amior (2019a). And new to this paper, I then solve for local employment growth and derive an unconditional crowd-out equation (which I subsequently estimate).

2.2 Local equilibrium conditional on population

Consider an economy with a single traded good, with price $P$, and a non-traded good (housing) with price $P^h_r$ in area $r$. If preferences are homothetic, I can define a unique local price index:

$$
P_r = Q \left( P, P^h_r \right)$$

Let $N_r$ and $L_r$ denote employment and population in area $r$. The standard Roback model assumes labor supply is fixed, so there is no meaningful difference between them. But I allow labor supply to be somewhat elastic to the real consumption wage:

$$
n_r = l_r + \epsilon^s (w_r - p_r) + z^s_r$$

where lower case variables denote logs, $w_r$ is the nominal wage, and $z^s_r$ is an area $r$ supply shifter. I write labor demand as:

$$
n_r = -\epsilon^d (w_r - p) + z^d_r$$

where $z^d_r$ is a demand shifter. In Appendix A.1, I set out equations for housing supply and demand. Together with (1)-(3), I can then solve for employment, wages and prices in terms of population $l_r$ alone.

Suppose indirect utility in area $r$ is given by:

$$v_r = w_r - p_r + a_r$$

where $w_r - p_r$ is the real consumption wage, and $a_r$ represents local amenities. Crucially, I can now substitute (2) for the real wage in (4). This allows me to use the local employment rate as a sufficient statistic for labor market conditions, conditional on the supply effect and amenity:

$$v_r = \frac{1}{\epsilon^s} (n_r - l_r - z^s_r) + a_r$$
Amior and Manning (2018) show this “sufficient statistic” result is robust to the inclusion of multiple traded and non-traded sectors (where migrants will generate their own local demand), agglomeration, endogenous amenities, labor market frictions; Amior and Manning (2019) argue it is robust to commuting across areas; and Amior (2019a) argues it is robust to heterogeneity in the consumer price indices of natives and migrants (as in Albert and Monras, 2018). Another concern is heterogeneous preferences for leisure: in line with Amior (2019a), I attempt to address this empirically by adjusting employment rates for demographic composition.

2.3 Population dynamics

Long run equilibrium is characterized by spatially invariant utility $v_r$, which fixes population $l_r$ in every area $r$. But I allow for sluggish adjustment of population; and like Amior (2019a), I distinguish between the contributions of internal and foreign migration to this adjustment:

$$dl_r = \lambda^I_r + \lambda^F_r$$

(6)

where $\lambda^I_r$ is the instantaneous rate of net internal inflows (i.e. from elsewhere the US) to area $r$, and $\lambda^F_r$ is the foreign inflow rate, relative to local population. (6) does not account for the impact of emigration, but I consider this later on when interpreting the empirical estimates. Amior (2019a) shows how $\lambda^I_r$ can be expressed as a linear function of utility $v_r$, using a logit model of residential choice:

$$\lambda^I_r = \gamma(n_r - l_r - z^*_r + \epsilon^*a_r)$$

(7)

where $\gamma \geq 0$ is the elasticity of net internal flows. I have not included a national intercept, but the supply effect $z^*_r$ may be redefined to include one. Mobility decisions in (7) depend only on current outcomes, so workers are implicitly myopic. However, Amior and Manning (2018) show that a model with forward-looking agents yields an equivalent expression, where the $\gamma$ parameter depends on both mobility and the local persistence of shocks.

In Amior (2019a), I also set out a parallel expression for the determination of foreign inflows, $\lambda^F_r$. This depends partly on what I call the “foreign supply”, i.e. the foreign inflow in the absence of local utility differentials. Crucially, the foreign supply varies geographically. In particular, it will depend on the presence of migrant enclaves, due to e.g. language or job market access. Since $\mu^F_r$ does not enter internal mobility (7) directly, this yields an exclusion restriction which can motivate the classic “enclave shift-share” instrument of Altonji and Card (1991) and Card (2001). But beyond this basic insight, I do not model $\lambda^F_r$ formally in this paper: my interest here is the evolution of $\lambda^I_r$ and local

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4I have simplified the notation of Amior (2019a); $\mu^I_t \gamma^I_t$ in my previous paper is now replaced by $\gamma$. 
population for a given $\lambda^F_r$.

2.4 Conditional crowd-out

To derive an empirical specification for conditional crowd-out, I first substitute (7) for the internal flows $\lambda^I_r$ in (6):

$$ dl_r = \lambda^F_r + \gamma (n_r - l_r - z^s_r + \epsilon^s a_r) $$

(8)

For estimation purposes, I require a discrete-time expression. Suppose the supply effect $z^s_r$, amenity effect $a_r$ and employment $n_r$ change at a constant rate within discrete intervals. For small employment shocks, Amior (2019a) then shows that:

$$ \lambda^I_{rt} \approx \left(1 - \frac{1 - e^{-\gamma}}{\gamma}\right) \left(\Delta n_{rt} - \lambda^F_{rt} - \Delta z^s_{rt} + \epsilon^s \Delta a_r\right) + \left(1 - e^{-\gamma}\right) (n_{rt-1} - l_{rt-1} - z^s_{rt-1} - \epsilon^s \Delta a_r) $$

(9)

where $\lambda^I_{rt} \equiv \int_{t-1}^{t} \lambda^I_r (\tau) d\tau$ is the discrete-time internal response over the unit interval, and $\lambda^F_{rt} \equiv \int_{t-1}^{t} \lambda^F_r (\tau) d\tau$ is the discrete foreign inflow. Conditional on employment growth $\Delta n_{rt}$ and the initial conditions (i.e. the lagged employment rate and supply shocks), equation (9) describes the impact of foreign inflows $\lambda^F_{rt}$ on net internal inflows $\lambda^I_{rt}$. This “conditional crowding out” effect increases from 0 to -1 as the internal population elasticity $\gamma$ increases from 0 to $\infty$.

Notice that (9) is implicitly an error correction model for local population growth: see Amior and Manning (2018) and Amior (2019a). This can be appreciated by adding the foreign inflow $\lambda^F_{rt}$ to both sides of (9). The dependent variable then becomes the log population change $\Delta l_{rt}$, which is a linear function of the log employment change $\Delta n_{rt}$ and the lagged log employment rate $n_{rt-1} - l_{rt-1}$ (which represents the initial steady-state deviation). As $\gamma$ becomes large, the local economy approaches full adjustment over decadal intervals: contemporaneous employment growth manifests one-for-one in population; and the coefficient on $n_{rt-1} - l_{rt-1}$ goes to -1, so any initial employment rate deviations are eliminated by population change in the subsequent interval.

In this analysis, I have focused exclusively on the labor market effects of immigration. But, “compositional disamenities” (i.e. native distaste for immigration) may also play an important role: see Card, Dustmann and Preston (2012), Saiz and Wachter (2011) and Fernandez-Huertas Moraga, Ferrer-i Carbonell and Saiz (2017). In the absence of such effects, equation (9) predicts an identical response of internal mobility to employment growth $\Delta n_{rt}$ and foreign inflows $\lambda^F_{rt}$. Intuitively, both have identical implications for the local employment rate (conditional on the other), which summarizes local welfare; and therefore, each should trigger an identical mobility response. As Amior (2019a) notes, a violation of this restriction would offer evidence in support of amenity effects.
2.5 Unconditional crowd-out

Equation (9) does not describe the unconditional impact of foreign inflows, because I am controlling for local employment growth $\Delta n_{rt}$. Labor demand, of course, may be a key margin of adjustment. To derive the unconditional effect (my point of departure from Amior, 2019a), I now reduce $\Delta n_{rt}$ to its various determinants.

This requires a specification of the housing market, as local prices shift labor supply (2) but not demand (3). Assuming individuals spend a fixed share of their income on housing (i.e. Cobb-Douglas utility) and abstracting from non-labor income, Appendix A.1 shows that changes in local prices $p_r$ can be specified as:

$$
\Delta (p_{rt} - p_t) = \frac{1}{\kappa} \left[ \frac{1}{e^s} (\Delta n_{rt} - \Delta l_{rt} - \Delta z^{s}_{rt}) + \Delta n_{rt} \right]
$$

where $\kappa > 0$ and goes to infinity with the elasticity of housing supply.\(^5\) Together with the labor supply and demand equations, this implies:

$$
\Delta n_{rt} = \eta (\Delta l_{rt} + \Delta z^{s}_{rt}) + (1 - \eta) \frac{\kappa}{\kappa + e^d} \Delta z^{d}_{rt}
$$

where

$$
\eta \equiv 1 - \left( 1 + \frac{\kappa + 1}{\kappa + e^d} \cdot \frac{e^d}{e^s} \right)^{-1}
$$

is the elasticity of employment with respect to population. This must lie between 0 and 1 if the labor demand elasticity is negative (i.e. $e^d > 0$) and the supply elasticity is positive ($e^s > 0$). As I show in Appendix A.2, replacing $\Delta n_{rt}$ in equation (9) with (11) yields the unconditional crowding out equation:

$$
\lambda^I_{rt} = \frac{(1 - \eta) \left( 1 - \frac{1 - e^{-\gamma}}{\gamma} \right)}{1 - \eta \left( 1 - \frac{1 - e^{-\gamma}}{\gamma} \right)} \left( \frac{\kappa}{\kappa + e^d} \Delta z^{d}_{rt} - \lambda^F_{rt} - \Delta z^{s}_{rt} + \frac{e^s}{1 - \eta} \Delta a_{rt} \right)
$$

$$
+ \frac{1 - e^{-\gamma}}{1 - \eta \left( 1 - \frac{1 - e^{-\gamma}}{\gamma} \right)} \left( n_{rt-1} - l_{rt-1} - z^{s}_{rt-1} + e^s a_{rt-1} \right)
$$

As before, the crowding out effect of $\lambda^F_{rt}$ goes to -1 as internal flows become perfectly elastic ($\gamma \to \infty$). In this scenario, the employment elasticity $\eta$ has no traction, because population adjusts so swiftly.

But for finite $\gamma$, the impact of $\lambda^F_{rt}$ is now moderated by an expansion of local labor demand (driven, in a more complete model, by capital mobility) and potentially also of housing supply. This is because, unlike in (9), I am no longer conditioning on current employment. To see this, notice the effect of $\lambda^F_{rt}$ in (13) is identical to (9) for $e^d = 0$ (and

\(^5\)Specifically, $\kappa \equiv \frac{1 - \nu + e^{hs}}{\nu}$, where $\nu$ is the (fixed) share of income spent on housing, and $e^{hs}$ is the housing supply elasticity.
\( \eta = 0 \), and it becomes smaller as \( \eta \) increases. Looking at (12), as \( \epsilon^d \) grows relative to \( \epsilon^s \), the employment elasticity \( \eta \) converges to 1: in the limit, adjustment is fully effected by changes in local employment rather than population (i.e. no crowding out). The effect of the housing supply elasticity (represented by \( \kappa \)), though, is theoretically ambiguous.\(^6\)

Of course, these claims are contingent on a negative labor demand elasticity. Suppose instead that the elasticity is positive; and further, suppose it exceeds the housing supply elasticity: i.e. \( \epsilon^d < -\kappa \). In terms of the comparative statics, an expansion of local population will then cause employment to fall (since lower wages now reduce demand). In terms of (12), \( \eta \) will become negative. Comparing the effect of \( \lambda^F_{rt} \) in (9) and (13), unconditional will now exceed conditional crowd-out. However, the dynamics rule out this possibility: if labor demand slopes up, local equilibrium becomes unstable; and consequently, a foreign inflow will trigger an employment–population “spiral”. Of course, some form of agglomeration is crucial to the existence of cities. But if these returns never peter out, population adjustment cannot bring about equilibrium; and the Roback (1982) equilibrium is unattainable.

3 Data

In the main analysis, I use decadal census observations between 1960 and 2010 across 722 CZs. Where possible, I use published county-level aggregates from the National Historical Geographic Information System (Manson et al., 2017). And where necessary, I supplement this with information from census microdata and (for the 2010 observation) pooled American Community Survey (ACS) samples of 2009-11, taken from the Integrated Public Use Microdata Series (Ruggles et al., 2017).

Data construction is identical to Amior (2019a): interested readers should consult that paper for details. But, I will briefly summarize the main points here.

The first challenge is to disaggregate the log population change \( \Delta L_{rt} \) into the contributions of foreign and internal mobility, i.e. \( \lambda^F_{rt} \) and \( \lambda^I_{rt} \). Since I only have discrete-time observations, I cannot precisely identify these components in the data. Based on the logic of Amior (2019a), I approximate them with \( \hat{\lambda}^F_{rt} \) and \( \hat{\lambda}^I_{rt} \) respectively:

\[
\hat{\lambda}^F_{rt} \equiv \log \left( \frac{L_{rt} + L^F_{rt}}{L_{rt-1}} \right) \tag{14}
\]

\[
\hat{\lambda}^I_{rt} \equiv \log \left( \frac{L_{rt} - L^F_{rt}}{L_{rt-1}} \right) \tag{15}
\]

where \( L^F_{rt} \) is the local foreign-born population at time \( t \) who arrived in the country in

\(^6\)The employment elasticity \( \eta \) (and therefore the crowding out effect) is increasing in \( \kappa \) (and hence in the elasticity of housing supply) if and only if \( \epsilon^d > 1 \). This condition ensures that the local wage bill (and therefore housing demand) expands in the face of foreign inflows.
the previous ten years (i.e. between $t - 1$ and $t$). Note that I have constructed $\hat{\lambda}_{rt}$ as a residual population change: this accounts for the entire contribution of natives and “old” migrants (who arrived in the US before $t - 1$), part of which is driven by “natural” growth and emigration (especially return migration by foreign-born individuals). It is not possible to identify emigration in this data; but in an effort to exclude it, I also study the native contribution to $\hat{\lambda}_{rt}$:

$$\hat{\lambda}_{rt}^{LN} \equiv \log \left( \frac{L_{rt} - 1 + \Delta L_{rt}^N}{L_{rt-1}} \right)$$

(16)

where $L_{rt}^N$ is the local native stock.

An alternative approach is to take first order approximations, i.e. $\lambda_{rt}^F \approx \frac{L_{rt}^F}{L_{rt-1}}$ and $\lambda_{rt}^I \approx \frac{\Delta L_{rt}^I - L_{rt-1}^F}{L_{rt-1}}$. But as Amior (2019a) shows, (14) and (15) offer more precision. While $\frac{L_{rt}^F}{L_{rt-1}}$ and $\frac{\Delta L_{rt}^I - L_{rt-1}^F}{L_{rt-1}}$ converge to $\lambda_{rt}^F$ and $\lambda_{rt}^I$ as they individually become small, convergence of (14) and (15) merely requires that their product becomes small.

Next, using the census and ACS microdata, I adjust all employment variables for local demographic variation, controlling for age, education (five categories), ethnicity, gender, foreign-born status, and years in the US, together with a rich set of interactions. This can be interpreted as purging any local variation in the supply shocks $z^s_{rt}$ which is driven by observable demographic composition. The aim is to reduce the demands on the various exclusion restrictions. Again, see Amior (2019a) for a more formal exposition and additional empirical details.

I identify changes in local demand using the pervasive Bartik (1991) industry shift-share, and foreign inflows using the enclave shift-share of Altonji and Card (1991) and Card (2001). The Bartik predicts change in local employment, based on initial industrial composition and national-level changes by industry:

$$b_{rt} = \sum_i \phi_{i,rt-1} \Delta n_{i,(-r)t}$$

(17)

where $\phi_{i,rt-1}$ is the fraction of area $r$ individuals working in a 2-digit industry $i$ (57 categories) in $t - 1$; and $\Delta n_{i,(-r)t}$ is industry $i$’s national log employment change, excluding area $r$.\footnote{Autor and Duggan (2003) and Goldsmith-Pinkham, Sorkin and Swift (2018) propose this exclusion to address concerns about endogeneity to local supply.}
consistency, I use the same functional form as (14):

\[ m_{rt} = \log \left( \frac{L_{rt-1} + \sum_o \phi_{o(i-r)t} L_{o(i-r)t}}{L_{rt-1}} \right) \]  

(18)

where \( \phi_{o(i-r)t} \) is the fraction of origin \( o \) migrants (77 countries) residing in area \( r \) at time \( t-1 \), and \( L_{o(i-r)t} \) is the stock of new origin \( o \) migrants (excluding area \( r \) residents) who arrived in the US between \( t-1 \) and \( t \). I construct both shift-share instruments using census microdata: see the Online Appendices of Amior (2019a) for details.

In all specifications, I control for a set of amenity effects identical to Amior and Manning (2018): (i) presence of coastline\(^8\) (ocean or Great Lakes); (ii) climate indicators, specifically maximum January temperature, maximum July temperature and mean July relative humidity; (iii) log population density in 1900; and (iv) an index of CZ isolation (log distance to closest CZ, measured between population-weighted centroids). To allow for time-varying effects, I interact each with a full set of year dummies. I do not include time-varying amenities (like crime), as these may be endogenous to the labor market (Diamond, 2016). Thus, the estimated effects of local shocks will account for both their direct (labor market) effect and any indirect effects (via amenity changes).

Table 1 offers descriptive statistics for key variables of interest: both means by decade, and percentiles of the full distribution. Since the 1960s, the mean foreign inflow \( \hat{\lambda}_{rt} \) has grown from 0.016 to 0.053. Unsurprisingly perhaps, the distribution of both foreign inflows and the enclave shift-share \( m_{rt} \) are very skewed. But I show that my results are robust to omitting outlying observations.

### 4 Estimates of crowding out

#### 4.1 Estimating equations

In line with (9), I begin by estimating the conditional crowding out equation of Amior (2019a):

\[ \hat{\lambda}_{rt} = \delta_{0t} + \delta_{1t} \hat{\lambda}_{rt} + \delta_{2t} \Delta n_{rt} + \delta_{3t} (n_{rt-1} - l_{rt-1}) + A_r \delta_{At} + \epsilon_{rt} \]  

(19)

where \( \hat{\lambda}_{rt} \) and \( \hat{\lambda}_{Ft} \) are the approximate internal (i.e. residual) and foreign contributions to local population growth, and the crowding-out effect is given by \( \delta_{1t} \). The “conditional” specification controls for (composition-adjusted) employment growth, \( \Delta n_{rt} \). The lagged (composition-adjusted) employment rate, \( n_{rt-1} - l_{rt-1} \), summarizes the initial conditions:

---

\(^8\)Coastline data is borrowed from Rappaport and Sachs (2003).
that is, any lingering impact of historical demand or migration shocks. I account for year effects in \( \delta_{0t} \), and the \( A_r \) vector contains amenity effects (i.e. observable components of \( \Delta a_{rt} \) and \( a_{rt-1} \) in (9)), which I interact with year effects (in \( \delta_{At} \)). Any unobserved amenity or supply effects fall into the error, \( \varepsilon_{rt} \). Looking at (9), there are no omitted demand shocks on the right hand side: this is a consequence of the sufficient statistic result (the employment variables fully summarize local welfare, conditional only on supply shocks). In the baseline estimates, I weight observations by the lagged local population share and cluster standard errors by state.

OLS estimates of (19) are not credible: foreign inflows \( \hat{\lambda}_F^{rt} \), employment growth \( \Delta n_{rt} \) and the lagged employment rate \( n_{rt-1} - l_{rt-1} \) are endogenous to omitted amenity and supply shocks (both current and lagged). Three instruments are required: I use the enclave shift-share (18) for \( \hat{\lambda}_F^{rt} \), the current Bartik \( b_{rt} \) in (17) for \( \Delta n_{rt} \), and the lagged Bartik \( b_{rt-1} \) for \( n_{rt-1} - l_{rt-1} \). In principle, the initial employment rate will depend on a distributed lag of Bartiks; but in practice, the first lag offers sufficient power.

Next, I turn to the unconditional estimates of crowding out, which are the focus of this paper. I base my empirical specification on (13):

\[
\hat{\lambda}_I^{rt} = \delta_{0t} + \delta_1 \hat{\lambda}_F^{rt} + \delta_2 b_{rt} + \delta_3 (n_{rt-1} - l_{rt-1}) + A_r \delta_{At} + \varepsilon_{rt} \tag{20}
\]

where I have replaced employment growth \( \Delta n_{rt} \) with its Bartik instrument \( b_{rt} \), as a proxy for local demand shocks. Looking at (13), the error \( \varepsilon_{rt} \) now contains any current unobserved demand innovations (historical demand and migration shocks are summarized by the lagged employment rate), as well as (current or lagged) unobserved supply and amenity shocks. There are now just two endogenous variables (\( \hat{\lambda}_F^{rt} \) and \( n_{rt-1} - l_{rt-1} \)), so I require only two instruments: as before, I use the enclave shift-share \( m_{rt} \) and the lagged Bartik \( b_{rt-1} \).

I have designed this specification for application to general settings: in this case, half a century of US history. This offers an advantage over natural experiments (such as Cohen-Goldner and Paserman, 2011; Monras, forthcoming; Edo, 2017), which are typically restricted to specific historical episodes. But it comes with empirical challenges, a crucial one being local dynamics. Jaeger, Ruist and Stuhler (2018) note that the local incidence of immigration is very persistent. Consequently, a regression of internal population flows on contemporaneous immigration may unintentionally pick up a sluggish response to historical foreign inflows - in which case, there is a risk of overestimating the crowding out effect. Jaeger, Ruist and Stuhler propose addressing this by controlling for historical foreign inflows. But, I take an alternative approach: as Amior (2019a) notes, the initial employment rate control \( (n_{rt-1} - l_{rt-1}) \) should in principle summarize the full history of immigration (and labor demand) shocks, a consequence of the sufficient statistic result. This offers a theoretical basis for Pischke and Velling’s (1997) suggestion
to control for the initial unemployment rate. In the analysis which follows, I probe the effectiveness of this approach in my setting.

### 4.2 Empirical estimates

Table 2 reports first stage estimates for the endogenous variables in (19) and (20), corresponding to the various columns of Table 3. Each instrument has large positive effects on its corresponding endogenous variable (marked in bold), with large Sanderson-Windmeijer (2016) F-statistics (which account for multiple endogenous variables).

Table 3 sets out estimates of (19) and (20). I focus here on the aggregate-level effects, but I consider heterogeneity by education in Section 5.2 below. The first two columns focus on conditional crowding out, i.e. (19): these replicate results from Table 8 of Amior (2019a). The OLS estimate of $\delta_1^C$ (i.e. the response to $\hat{\lambda}_{rt}$) is just under -0.9, and the IV estimate just over (in magnitude). Their similarity can be attributed to the employment control: in principle, there are no omitted demand shocks; so any OLS-IV disparity must be due to supply shocks alone. I also estimate large responses to employment growth and the lagged employment rate (especially in IV).

As I have noted above, the model in (9) predicts equal coefficients on the foreign inflow $\hat{\lambda}_{rt}$ and employment growth $\Delta n_{rt}$ (i.e. $\delta_1^C = \delta_2^C$). Intuitively, a given change in both $\hat{\lambda}_{rt}$ and $\Delta n_{rt}$ have identical implications for the local employment rate (which summarizes local welfare, conditional on supply and amenity effects) and should trigger identical mobility responses. However, at least in IV (column 2), $\delta_1^C$ exceeds $\delta_2^C$ - and significantly so. Amior (2019a) argues that this reflects compositional disamenities, i.e. native distaste for immigration.

The remaining columns study unconditional crowding out, i.e. (20), the focus of this paper. The OLS estimate of $\delta_1^U$ (-0.76) is somewhat smaller than $\delta_1^C$, but the IV estimate (-1.1) is larger. The IV results are certainly surprising: the employment response to immigration should, in theory, moderate any labor market impact and population response (assuming the labor demand elasticity is negative, i.e. $\varepsilon^d > 0$).

As I have noted above, one may be concerned that the estimates are distorted by local dynamics. However, the evidence suggests that the lagged employment rate control (which is intended to summarize initial conditions) is performing its function well. First, following the recommendation of Jaeger, Ruist and Stuhler (2018), I control in column 5 for the lagged enclave shift-share, $m_{rt-1}$. Notice that $m_{rt-1}$ negatively affects the initial employment rate in the first stage (column 6 of Table 2); but reassuringly, it takes a (reasonably precise) zero coefficient in the second stage (column 3 of Table 3). This is
consistent with the initial employment rate summarizing the full history of shocks. However, once I drop the initial employment rate in column 6 (and replace it with its lagged Bartik instrument), the lagged enclave shift-share now takes a large negative coefficient (about -0.4), and the coefficient on $\hat{\lambda}_F$ contracts commensurately. In words, though internal flows do respond sluggishly, the initial employment rate accounts successfully for these dynamics.

This point can be made more strongly by testing for pre-trends, as Peri (2016) recommends. In column 7, I replace the dependent variable with its lag, $\hat{\lambda}_{rt-1}$. Reassuringly, the current foreign contribution $\hat{\lambda}_F$ and Bartik $b_{rt}$ have no significant effect on $\hat{\lambda}_{rt-1}$; instead, the impact is fully absorbed by the lagged enclave shift-share and Bartik. This suggests that, using my data (which covers multiple decades), I can empirically tease apart the effects of current and historical shocks. While my focus here is the unconditional crowd-out specification, Amior (2019a) reaches a similar conclusion regarding the conditional specification.

Famously, Borjas (2006) finds less crowding out across states than metro areas. But in column 8, I cannot reject a $\delta_1$ of -1 using state-level data. As it happens, this data does not offer sufficient variation to identify the lagged employment rate (using the lagged Bartik instrument). So, my approach is to replace the lagged employment rate with the lagged Bartik and enclave shift-shares - as I do in column 6. Interestingly, the crowd-out effects (i.e. the coefficients on current foreign inflows $\hat{\lambda}_F$ and the lagged enclave shift-share $m_{t-1}$) are larger for the state-level data (than in column 6); but unsurprisingly, the standard errors are also larger.

Finally, in Appendix C, I show the effect is entirely driven by reductions of migratory inflows to the affected CZs, rather than increases in outflows - based on census respondents’ reported place of residence five years previously. This is consistent with evidence from Coen-Pirani (2010), Monras (2015), Dustmann, Schoenberg and Stuhler (2017) and Amior and Manning (2018), who document a disproportionate role for inflows in driving local population adjustment.

### 4.3 Robustness of estimates

In Table 4, I assess the sensitivity of my unconditional IV estimates (i.e. column 4 of Table 3) to the choice of controls and decadal sample. With no controls (except year effects), there is substantial variation in the $\delta_1^u$ estimates by decade: I find little crowd-out before 1990, but much more in the 1990s and 2000s. This reflects the findings of Card (2009a) and the concerns of Borjas, Freeman and Katz (1997), who emphasizes the instability of spatial correlations. This offers a motivation for pooling data over multiple decades. The average $\delta_1^u$ (column 6) increases from -0.53 to -0.75 when I control for the current Bartik and initial employment rate. And after including the amenity
effects (and especially climate, which is known to be an important determinant of regional population flows), I cannot statistically reject one-for-one unconditional crowd-out in any decade: see the penultimate row. These effects are intuitive: both natives and migrants disproportionately settle in places with strong labor market conditions and attractive climate, so omitting these variables should bias my $\delta^u_1$ estimates in a positive direction (i.e. towards zero). And as Borjas, Freeman and Katz (1997) emphasize, the extent of this bias will depend on the peculiarities of each individual decade.

In the final row, I replace the dependent variable $\hat{\lambda}_{rt}$ with the native contribution $\hat{\lambda}_{rt}^{I,N}$ to local population (i.e. excluding the contribution of old migrants), but continue to use the full set of controls. Comparing this row with the previous one, column 6 shows about two thirds of the $\delta^u_1$ effect is driven by natives rather than old migrants. But this result overlooks some important heterogeneity: exceptionally, in the 2000s, old migrants account for the entire crowding out effect. One possible explanation is large return migration of Mexicans in the 2000s (see e.g. Hanson, Liu and McIntosh, 2017), driven in part by the construction bust and Great Recession.

In Appendix Figure A1, I plot my baseline OLS and IV $\delta^u_1$ estimates graphically, conditional on the covariates: see Appendix D.1 for methodological details. The pictures show that the effect is not driven by outliers.

In Appendix D.2 (and Appendix Table A2), I show the crowding out effect is robust to numerous specification changes. I begin by considering the effect of weighting. The population weights applied in Table 3 ensure my estimates are driven predominantly by variation across larger CZs. But I show in the Appendix that dropping these weights makes little difference, as does dropping CZs with fewer than 100,000 individuals aged 16-64 in 1960. Given the skew in the spatial distribution of foreign inflows (Table 1), one may also be concerned that the estimates are driven by CZs facing unusually high inflows. But, excluding observations with enclave shift-share $m_{rt}$ above 0.1 (the maximum is 0.29) makes little difference. These results are consistent with the patterns visible in Appendix Figure A1.

My specification of the foreign and internal contributions is almost identical to that of Card and DiNardo (2000) and Card (2001), as recommended by Peri and Sparber (2011) and Card and Peri (2016). While they regress $\frac{\Delta L_{rt} - L_{rt-1}}{L_{rt-1}}$ on $\frac{L_{rt}}{L_{rt-1}}$, I am regressing $\log \left( \frac{L_{rt} - L_{rt-1}^F}{L_{rt-1}} \right)$ on $\log \left( \frac{L_{rt-1} + L_{rt}^F}{L_{rt-1}} \right)$, following the guidance of my model.9 The appendix shows this change has a negligible effect on the $\delta^u_1$ estimate. Also, basing the enclave

---

9My $\hat{\lambda}_{rt}^F$ specification in (14) shares with Peri and Sparber (2011) and Card and Peri (2016) the advantage of depending only on new foreign inflows - and not on changes in the population of longer term US residents (which might otherwise introduce a spurious correlation with $\hat{\lambda}_{rt}^I$).
shift-share in (18) on 1960 origin shares in all decades (rather than using lagged-once shares, following the example of Hunt, 2017) makes little difference.

My results are also robust to an alternative approach recommended by Wozniak and Murray (2012), which specifies the key variables in levels, i.e. regressing \((\Delta L_{rt} - L^F_{rt})\) on \(L^F_{rt}\), without normalizing by initial population. As Wright, Ellis and Reibel (1997) note, local population may be an important omitted variable in this specification; but in line with Wozniak and Murray, I address this by controlling for local fixed effects.10

More generally, even specifying the variables as I do in Table 3, I cannot reject one-for-one crowding out when I control for CZ fixed effects. This approach is similar in spirit to the double differencing methodology of Borjas, Freeman and Katz (1997) and is recommended by Hong and McLaren (2015). The idea here is to pick up time-invariant local supply or demand trends. The estimates do become less stable: this is a demanding specification, given the large persistence in the enclave shift-shares; and as Aydemir and Borjas (2011) note, measurement error may be a greater challenge in the presence of fixed effects. But precision does improve when I replace the lagged employment rate control with historical shocks (a lagged Bartik and enclave shift-share).

5 Labor market outcomes and heterogeneity

5.1 Aggregate-level effects

In the section above, I cannot reject one-for-one unconditional crowd-out. This would appear to imply full local labor market adjustment in response to immigration. But despite this, I identify small adverse effects of foreign inflows on local employment rates - consistent with Smith (2012), Edo and Rapoport (2017) and Gould (2019).

In Table 5, I re-estimate (20) using the same instruments as before, but replacing the dependent variable with changes in (composition-adjusted) employment rates. In my preferred specification (column 1), the elasticity of the native employment rate to foreign inflows is -0.21. The coefficient of -0.41 on the lagged employment rate suggests the effect is largely dissipated within two decades.

As before, my empirical model can successfully disentangle the impact of current and historical shocks. As in Table 3, the lagged enclave shift-share control \(m_{rt-1}\) in column 2 makes little difference, which suggests the lagged employment rate is successfully accounting for the initial conditions. Once I drop the lagged employment rate in column

10This approach can also address concerns that the Table 3 estimates are conflated with spurious correlation in local population, which appears in the denominator of both the dependent variable and regressor of interest. See Clemens and Hunt (2019).
3, I identify a larger initial impact of foreign inflows (-0.35), though the rate of adjustment implied by $m_{rt-1}$ (which now takes a positive effect, representing the recovery following the initial shock) is similar to before. Column 4 shows what happens when I replace the dependent variable with its lag. Reassuringly, as in Table 3, $m_{rt-1}$ picks up the entire (negative) effect on the lagged dependent, and the current inflow $\hat{\lambda}_{rt}$ becomes insignificant.

Column 5 re-estimates my preferred IV specification (column 1) for the migrant employment rate. The effect is similar to natives, which suggests there may be no great loss (at least in this context) from treating natives and migrants as perfect substitutes at the aggregate level - as I do in my model.

In principle, lower employment rates should be reflected in lower real consumption wages - based on the labor supply relationship in (2). Unfortunately, local wage deflators are notoriously difficult to construct (and typically rely on strong theoretical assumptions), especially for the detailed geographies and long time series in my data: see e.g. Koo, Phillips and Sigalla (2000), Albouy (2008) and Phillips and Daly (2010). Nevertheless, one can at least study the effects on nominal wages and housing costs separately.

I use mean residualized wages, purged of detailed demographics (age, education, ethnicity, gender, foreign-born status, and years in the US, plus a rich set of interactions), and residualized housing rents and prices, purged of observable housing characteristics: see Appendix B for further details. While I find no impact on natives’ residualized nominal wages, housing costs do respond positively (see also Saiz, 2007) - though the effect is statistically insignificant. Still, wage effects may be difficult to interpret in the context of declining employment rates, if it is the lowest paid natives who are leaving employment: see Card (2001) and Bratsberg and Raaum (2012).

### 5.2 Education-specific effects

I now study heterogeneity in the impact of foreign inflows across education groups. My strategy is to replace the dependent variable of (20) with various outcomes $\Delta y_{grt}$ specific to education groups $g$ (college graduates, non-graduates), but keeping the original aggregate-level immigration shock $\hat{\lambda}_{rt}^F$ and controls on the right hand side:

$$\Delta y_{grt} = \delta_{bg}^u + \delta_{1g}^u \hat{\lambda}_{rt}^F + \delta_{2g} u_{rt} + \delta_{3g} (n_{rt-1} - l_{rt-1}) + A_r \delta_{Agr}^u + \epsilon_{grt} \quad (21)$$

This is an application of the “total effects” approach recommended by Dustmann, Schoenberg and Stuhler (2016). I report estimates of $\delta_{1g}^u$ by outcome (across columns) and education group $g$ (rows) in Table 6. To adjust education-specific employment rates, wages and housing costs for local composition, I apply the same methodology described in Section 3 and Appendix B to the samples of each education group.
Column 1 reports the effects on education-specific population growth $\Delta l_{grt}$; and the next two columns disaggregate this change into foreign and residual contributions. I specify these analogously to (14) and (15):

$$
\hat{\lambda}_{grt}^F \equiv \log \left( \frac{L_{grt} - L_{grt-1} + L_{Fgrt}}{L_{grt-1}} \right)
$$

(22)

$$
\hat{\lambda}_{grt}^I \equiv \log \left( \frac{L_{grt} - L_{Fgrt}}{L_{grt-1}} \right)
$$

(23)

where $L_{Fgrt}$ is the stock of new migrants (arriving in the US since $t-1$) of education $g$ in area $r$. Column 2-3 show the crowding out effect is educationally “balanced”: the college share of both foreign inflows $\hat{\lambda}_{grt}^F$ and the residual population response $\hat{\lambda}_{grt}^I$ (elicited by the enclave shift-share $m_{rt}$) resemble the existing population. As a result, there is little change in the relative supply of college workers (column 1).

Despite this, the adverse effect of foreign inflows on native employment rates falls entirely on non-graduates (column 4). This suggests that column 2 is underestimating the labor market pressure on low educated natives. This may be a consequence of more severe undercoverage of low educated migrants, or alternatively occupational downgrading of better educated migrants (e.g. Dustmann, Schoenberg and Stuhler, 2016). Given the asymmetric employment rate effects, why then should the residual response be educationally balanced? One plausible explanation is educational differentials in geographical mobility.\textsuperscript{11} However, as I emphasize below, these responses will reflect not only geographical mobility but also changes in the characteristics of local birth cohorts.

The final four columns report the impact on residualized wages and housing costs. It turns out the aggregate-level effects in Table 5 mask some interesting heterogeneity: there is a small positive effect on the wages of graduate natives, but they also experience larger increases in housing expenditures (purged of local housing characteristics). Whether this reflects changes in unobserved housing consumption or prices is open to interpretation.\textsuperscript{12} Certainly, an analysis of the impact on real consumption wages is challenging - and not least because it is difficult to construct credible local wage deflators. This underscores the potential advantages of studying welfare effects using local employment rates, relying on the sufficient statistic result of Amior and Manning (2018).

\textsuperscript{11}See e.g. Bound and Holzer (2000); Wozniak (2010); Notowidigdo (2011); Amior (2019b). In particular, using the same data as this paper, Amior and Manning (2018) show the college graduate population adjusts fully to local employment shocks within one decade; and any sluggishness in local adjustment is due to non-graduates.

\textsuperscript{12}The price interpretation may be relevant if housing units are imperfect substitutes within CZs, e.g. due to unobserved characteristics or neighborhoods. For example, Albouy and Zabek (2016) have documented growing inequality in housing prices within cities, driven mostly by changes in the relative value of locations.
6 Accounting for undercoverage

6.1 Existing evidence on undercoverage

My estimate of one-for-one unconditional crowd-out is puzzling, even in the context of my own results. First, as I discuss in Section 2.5, economic theory suggests that the unconditional effect, $\delta_u$, should be smaller than the conditional effect, $\delta_c$; but I find the reverse. Second, one-for-one crowd-out is indicative of full labor market adjustment, but this is inconsistent with the adverse impact on local employment rates.

I argue that undercoverage of undocumented migrants in the US census and ACS may help account for this puzzle. Surprisingly perhaps, many undocumented migrants do respond to the census (Warren and Passel, 1987), but a significant fraction do not. The US Department of Homeland Security (2003) estimates that almost half the migrants who entered the US in the 1990s did not have legal status, and that the census understated the total 1990s foreign inflow by about 7 percent. The undercount was more severe in earlier years: see Card and Lewis (2007). For example, Marcelli and Ong (2002) find that 10-15 percent of undocumented Mexicans were missed by the 2000 census; Van Hook and Bean (1998) estimate that 30 percent were missed in 1990; and Borjas, Freeman and Lang (1991) estimate an undercount of 40 percent in 1980. However, these exercises are not necessarily informative about the undercoverage of foreign inflows elicited by the enclave instrument. Any such undercoverage will cause me to overstate the extent of geographical crowd-out.

6.2 Undercoverage bias in empirical specifications

The evidence above already offers some circumstantial support for this hypothesis. First, my estimates of unconditional crowd-out $\delta_u$ are much larger in the 1960s and 1970s (see Table 4): this is consistent with more severe undercoverage of undocumented migrants in those years. Second, it is reasonable to assume that undercoverage is more severe among low educated migrants, and this may help account for the more adverse employment rate effects on low educated natives (despite the apparently balanced local immigration shock): see the discussion in Section 5.2.

However, by imposing more structure, I can in principle quantify the bias more precisely. Under the assumptions of my theoretical model, I will show that controlling for employment growth (as in the conditional specification) should eliminate the bulk of this bias. And using an estimate of the employment elasticity, I can then impute the true unconditional crowding out effect.

Consider first the unconditional crowd-out specification, (20). Abstracting from the
initial employment rate and the other controls, the true model is:

$$\lambda_{rt}^I = \delta_1^u \lambda_{rt}^F$$  (24)

Now, suppose I do not observe the true foreign inflow $\lambda^F$, but rather $\tilde{\lambda}_{rt}^F = (1 - \pi) \lambda_{rt}^F$, where $\pi \in [0, 1]$ is the fraction of new migrants (arriving between $t - 1$ and $t$) who are not covered by the census. And suppose I estimate the model with $\tilde{\lambda}_{rt}^F$ on the right hand side. The relationship between $\lambda_{rt}^I$ and $\tilde{\lambda}_{rt}^F$ can be described by:

$$\lambda_{rt}^I = \tilde{\delta}_1^u \tilde{\lambda}_{rt}^F$$  (25)

where the coefficient $\tilde{\delta}_1^u$ exceeds the true effect $\delta_1^u$ by fraction $\pi$:

$$\frac{\tilde{\delta}_1^u - \delta_1^u}{\delta_1^u} = \pi$$  (26)

Now consider the conditional specification. Again abstracting from the various controls, the true model is:

$$\lambda_{rt}^I = \delta_1^c \lambda_{rt}^F + \delta_2^c \Delta n_{rt}$$  (27)

Recall that equation (9) in the model imposes that $\delta_1^c = \delta_2^c$. But the estimates in Table 3 reject this restriction (most likely a consequence of compositional disamenities), and I permit $\delta_1^c$ and $\delta_2^c$ to differ here. As before, I only observe $\tilde{\lambda}_{rt}^F = (1 - \pi) \lambda_{rt}^F$. But crucially, my data also understates the employment growth, $\Delta n_{rt}$. As in the model above, suppose that natives and migrants share the same local changes in (composition-adjusted) employment rates - which is consistent with the estimates of Table 5. Then, the observed employment growth will equal:

$$\Delta \tilde{n}_{rt} = \Delta (n_{rt} - l_{rt}) + \lambda_{rt}^I + \tilde{\lambda}_{rt}^F = \Delta n_{rt} - \pi \lambda_{rt}^F$$  (28)

Now, suppose I estimate the conditional specification using the observed (but mismeasured) $\tilde{\lambda}_{rt}^F$ and $\Delta \tilde{n}_{rt}$. This can be written as:

$$\lambda_{rt}^I = \tilde{\delta}_1^c \tilde{\lambda}_{rt}^F + \tilde{\delta}_2^c \Delta \tilde{n}_{rt}$$  (29)

where the coefficient estimators $\tilde{\delta}_1^c$ and $\tilde{\delta}_2^c$ are equal to:

$$\begin{pmatrix} \tilde{\delta}_1^c \\ \tilde{\delta}_2^c \end{pmatrix} = \begin{pmatrix} Var(\tilde{\lambda}_{rt}^F) & Cov(\Delta \tilde{n}_{rt}, \tilde{\lambda}_{rt}^F) \\ Cov(\Delta \tilde{n}_{rt}, \tilde{\lambda}_{rt}^F) & Var(\Delta \tilde{n}_{rt}) \end{pmatrix}^{-1} \begin{pmatrix} Cov(\tilde{\lambda}_{rt}^F, \lambda_{rt}^I) \\ Cov(\Delta \tilde{n}_{rt}, \lambda_{rt}^I) \end{pmatrix} = \begin{pmatrix} \delta_1^c + \frac{\pi}{1-\pi} (\delta_1^c + \delta_2^c) \\ \delta_2^c \end{pmatrix}$$  (30)

See Appendix E for more detailed steps. Notice that $\tilde{\delta}_2^c$, the coefficient on $\Delta \tilde{n}_{rt}$, identifies the true $\delta_2^c$. Intuitively, the bias in measured foreign inflows $\tilde{\lambda}_{rt}^F$ partials out the bias
in employment growth $\Delta n_{rt}$. Under the model’s assumption that $\delta^c_1 + \delta^c_2 = 0$ (i.e. in the absence of disamenity effects), $\tilde{\delta}^c_1$ will also be unbiased - for similar reasons. More generally though, using (30), the bias in $\tilde{\delta}^c_1$ can be written as:

$$\frac{\tilde{\delta}^c_1 - \delta^c_1}{\delta^c_1} = \pi \left( \frac{\tilde{\delta}^c_1 + \tilde{\delta}^c_2}{\delta^c_1} \right)$$

(31)

In practice, Table 3 (column 2) rejects the claim that $\tilde{\delta}^c_1 + \tilde{\delta}^c_2 = 0$, but the relative discrepancy is small: $\frac{\tilde{\delta}^c_1 + \tilde{\delta}^c_2}{\delta^c_1} = \frac{0.913 - 0.743}{0.913} = 0.19$. Based on (31), this suggests the bias in $\tilde{\delta}^c_1$ is just a fifth of the bias in $\tilde{\delta}^u_1$ in (26): i.e. undercoverage bias should have comparatively little effect on conditional estimates of crowd-out.

Table 7 offers some suggestive evidence in favor of this claim. The first row reports estimates of unconditional crowd-out by decade (these are identical to the penultimate row of Table 4). In the second row, I repeat this exercise for conditional crowd-out: i.e. I estimate (19) separately by decade. The estimates are no longer systematically larger in the 1960s and 1970s, when undercoverage was largest: this is consistent with lower bias in the conditional estimates.

### 6.3 Quantifying the undercoverage bias

By imposing some structure and an equation for employment growth, I can now use the *conditional* estimate of crowd-out to quantify the bias in the *unconditional* estimate. Using (11), and abstracting from local labor supply and demand shocks, employment growth can be written in terms of local population:

$$\Delta n_{rt} = \eta \Delta l_{rt}$$

(32)

where the elasticity $\eta$, defined in (12), will depend on both the labor and housing market elasticities. Substituting this for $\Delta n_{rt}$ in the true conditional model, (27), I can reduce the unconditional impact $\delta^u_1$ of foreign on internal inflows to$^{13}$:

$$\lambda^I_{rt} = \frac{\delta^c_1 + \eta \delta^c_2}{1 - \eta \delta^c_2} \lambda^F_{rt}$$

(33)

Note that theory predicts a negative $\delta^c_1$ and positive $\eta$ and $\delta^c_2$. Thus, a larger employment elasticity ($\eta$) and population response to employment ($\delta^c_2$) moderate the extent of crowd-out. Of course, I only observe the biased coefficient estimators $\tilde{\delta}^u_1$, $\tilde{\delta}^c_1$ and $\tilde{\delta}^c_2$. So, using

$^{13}$This expression is identical to the coefficient on $\lambda^F_{rt}$ in equation (13), under the model’s assumption that $\delta^c_2 = -\delta^c_1 = \left( 1 - \frac{1}{\eta \delta^c_2} \right)$. 

21
(26), (30) and (31), I now replace $\delta_1^u$, $\delta_1^c$ and $\delta_2^c$ in (33) with these estimators. Rearranging, I then have an expression for the undercoverage bias $\pi$ in terms of the three estimators and the employment elasticity $\eta$:

$$
\pi = 1 - \frac{(1 - \eta) \tilde{\delta}_2^c}{\tilde{\delta}_1^c + \tilde{\delta}_2^c - (1 - \eta \tilde{\delta}_2^c) \tilde{\delta}_1^u}
$$

(34)

Since $\tilde{\delta}_1^u$ is negative, (34) describes a positive relationship between $\pi$ and $\eta$, for given $\tilde{\delta}_1^u$, $\tilde{\delta}_1^c$ and $\tilde{\delta}_2^c$. Intuitively, a larger employment elasticity $\eta$ moderates the true unconditional crowd-out $\delta_1^u$ (see (33)), so I require more bias $\pi$ to account for a given estimate of $\tilde{\delta}_1^u$.

For illustration, I have plotted the implied relationship between $\pi$ and $\eta$ in Figure 1, based on my IV coefficient estimates from Table 3: $\tilde{\delta}_1^c = -0.913$ and $\tilde{\delta}_2^c = 0.743$ (column 2), and $\tilde{\delta}_1^u = 1.096$ (column 4). Assuming the employment elasticity $\eta$ is positive, the model imposes a lower bound on $\pi$ of 0.2: this is required for unconditional crowd-out $\delta_1^u$ not to exceed conditional crowd-out $\delta_1^c$. And as $\eta$ goes to 1 (implying employment takes the full burden of adjustment), $\pi$ must also go to 1 (to ensure the true unconditional response is 0).

### 6.4 Estimates of employment elasticity and implications for crowd-out

In order to identify the undercoverage bias $\pi$, I require a suitable estimate of the employment elasticity $\eta$ to population. We have estimated this parameter in earlier work (Amior and Manning, 2018), accounting also for sluggish adjustment of employment. Our IV estimate from that paper is 0.79, using maximum January temperature as an instrument for population growth (in line with Beaudry et al., 2014a; 2014b). Looking at Figure 1, this would imply an undercoverage bias $\pi$ of about 0.4.

In Appendix E.2, I extend the analysis of Amior and Manning (2018) by allowing for distinct employment responses to foreign and (net) internal inflows. To identify these effects separately, I use the enclave shift-share as an additional instrument. Based on this specification, I estimate elasticities of 0.61 and 0.78 to foreign and internal inflows respectively. Based on the assumption of my model that natives and migrants supply identical labor, these should be identical. But since they are identified using divergent sources of variation, it is perhaps unwise to over-interpret the gap between them.

In any case, since I am studying crowd-out in response to immigration (identified by the enclave shift-share), I choose to focus on the elasticity to foreign inflows (0.61). As I show in Appendix E.3, this number is not itself immune from undercoverage bias; but
the form of the bias can be derived. Together with the relationship in Figure 1, the model then implies a “true” employment elasticity $\eta$ of 0.44 and an undercoverage bias $\pi$ of 0.27. This lower value for $\pi$ seems more plausible in the context of the evidence described in Section 6.1. The implied “true” unconditional crowd-out would then be:

$$-1.096 \cdot (1 - 0.27) = 0.80.$$ 

One may be surprised that the crowding out effect is so large, in spite of the substantial employment elasticity $\eta$. But, the algebra shows the employment response may not be so salient if population itself is very elastic. This result is consistent with Blanchard and Katz (1992), Hornbeck (2012) and Amior and Manning (2018), who find that the burden of adjustment (following shocks to local labor demand) falls mostly on population rather than employment.

## 7 Within-area estimates of unconditional crowd-out

### 7.1 Empirical specification

Until now, I have studied crowding out at the aggregate CZ-level, in line with the “total effects” approach recommended by Dustmann, Schoenberg and Stuhler (2016). But the seminal work in the literature has typically exploited variation in migration shocks across skill groups within geographical areas. In this section, I emphasize (and demonstrate empirically) that aggregate-level and within-area specifications identify different objects: this offers a means to reconcile my results with what has come before.

In line with Peri and Sparber (2011), consider the following (unconditional) estimating equation for crowd-out:

$$\hat{\lambda}^F_{grt} = \delta^w_0 + \delta^w_1 \hat{\lambda}^F_{grt} + d_{rt} + d_{gt} + \varepsilon_{srt} \quad (35)$$

where $\hat{\lambda}^F_{grt}$ and $\hat{\lambda}^I_{grt}$ are the foreign and residual contributions to population in skill group $g$ in area $r$ (as defined by (22) and (23)), and the “$w$” superscript on the $\delta$ coefficients refers to the “within-group” specification. The $d_{rt}$ are area-time interacted fixed effects, which absorb local shocks common to all skill groups; and $d_{gt}$ are skill-time interacted effects, which account for national-level trends across skill groups.

The coefficient of interest, $\delta^w_1$, identifies the impact of skill-specific foreign inflows on local skill composition - or more precisely, on the contribution of existing US residents to skill composition. Comparable estimates of $\delta^w_1$ in the literature are typically small and sometimes positive (Card and DiNardo, 2000; Card, 2001, 2005; Cortes, 2008), though Borjas (2006) and Monras (forthcoming) offer alternative views.\(^{14}\) Either way, a small $\delta^w_1$...
is not necessarily inconsistent with large geographical crowd-out. This is for two reasons. First, changes in local skill composition reflect not only differential internal mobility, but also changes in the characteristics of local birth cohorts. And second, as Card (2001) and Dustmann, Schoenber and Stuhler (2016) point out, $\delta_1^w$ does not account for the labor market impact that new immigrant arrivals exert outside their own skill group $g$.

Regarding the latter point, it is useful to consider a simple example. Suppose production in area $r$, for the tradable good priced at $P_t$, has CES technology (as in e.g. Card, 2001) over skill-defined local labor inputs: $Y_{rt} = \psi_{rt} \left( \sum_g \theta_{grt} N_{grt}^{\sigma_g} \right)^{\frac{1}{\rho}}$, where $\psi_{rt}$ is an aggregate productivity shifter, $\frac{1}{1-\sigma}$ is the elasticity of substitution between labor inputs, and the exponent $\rho \leq 1$ allows for diminishing local returns. Assuming competitive labor markets, local wage growth for skill group $g$ can then be expressed as:

$$\Delta \left( w_{grt} - p_t \right) = \Delta \log \theta_{grt} - (1 - \sigma) \Delta n_{grt} + \frac{\sigma}{\rho} \Delta \log \psi_{rt} + \frac{\rho - \sigma}{\rho} \Delta y_{rt}$$  \hfill (36)

Consider a skill-specific expansion of local employment $\Delta n_{grt}$, driven by immigration. The area-time fixed effects $d_{rt}$ in (35) will absorb the local wage effect which is common to all skill groups, as encapsulated by $\Delta y_{rt}$ in (36). Conditional on the $d_{rt}$, the wage response is then the inverse of the elasticity of substitution, i.e. $1 - \sigma$. Intuitively, for larger $\sigma$, the impact on wages is more diffused across the various skill groups - and the same will be true of any mobility response.\footnote{Effectively, the $\sigma$ parameter plays an analogous role at the skill level as the employment elasticity $\eta$, in (12), at the aggregate level.} So even in the absence of cohort effects, $\delta_1^w$ will not in general identify an aggregate-level crowding-out effect akin to $\delta_1^u$ in (20). The single exception is the case of an additively separable production function (i.e. $\sigma = \rho$), which ensures no diffusion of wage effects. In Appendix A.3, I offer a more formal mapping of this multi-skill model onto the empirical specification (35), accounting for skill-specific population dynamics.

The purpose of this exercise is not to say that within-area estimates are uninteresting: the impact of skill-specific foreign inflows on local skill composition is certainly an important question. My point is merely that it is not the same as geographical crowd-out.

7.2 Estimates of $\delta_1^w$

I now offer evidence that cross-skill spillovers and local cohort effects make an important contribution to the within-area estimate $\delta_1^w$. In practice, we do not know the “true” skill delineation: this is ultimately a decision for the researcher. But in light of the discussion above, empirical estimates of $\delta_1^w$ are likely to be sensitive to this choice, as different skill delineations will artificially engender different elasticities of substitution. In Table 8, I one-for-one) using annual variation - in the year following the Mexican Peso crisis of 1995. But he estimates a small effect over a longer decadal interval.
present estimates of $\delta_i^w$ in (35) for four different education-based\textsuperscript{16} “skill” delineations: (i) college graduates / non-graduates; (ii) at least one year of college / no college (see e.g., Monras, forthcoming); (iii) high school dropouts / all others (Card, 2005; Cortes, 2008); (iv) four groups: dropouts, high school graduates, some college and college graduates (see Borjas, 2006).

To explore the role of cohort effects, I present estimates separately using both pooled census cross-sections and (to isolate an impact on internal mobility) a longitudinal dimension of the census: respondents were asked where they lived five years previously. This question, previously exploited by Card (2001) and Borjas (2006) in their analyses of crowding-out, is available in the 1980, 1990 and 2000 census extracts, yielding information on migratory flows over 1975-1980, 1985-1990 and 1995-2000.\textsuperscript{17} To preserve comparability, I restrict the pooled cross-section sample to the same three decades: the 1970s, 1980s and 1990s.

For the purposes of the longitudinal estimates, I continue to define $\hat{\lambda}_{grt}^I$ and $\hat{\lambda}_{grt}^F$ according to (22) and (23), but time intervals are now five years: so $L_{gtrt}^F$ is the stock of migrants who arrived in the US within the previous five years; and the initial population $L_{gtrt-1}$ is constructed using information on where current census respondents lived five years previously. As a result, the internal contribution $\hat{\lambda}_{grt}^I$ will not account for emigration from the US. But to the extent that emigration is a response to an individual’s local economic environment, my estimate should then understate any crowding out effect.

In an effort to exclude education-specific local demand shocks ($\theta_{gtrt}$ in the model), I instrument the foreign inflow $\hat{\lambda}_{grt}^F$ in (35) using an education-specific enclave shift-share - following the methodology of Card (2001). Building on equation (18) above:

$$m_{gtrt} = \log \left( \frac{L_{gtrt-1} + \sum_o \phi_{gtrt-1} L_{ogtr-1} F_{ogtr} \cdot r_{tr}}{L_{gtrt-1}} \right)$$

where new migrants of origin $o$ and education group $g$ are allocated proportionately according to the initial co-patriot geographical distribution. Again, for the longitudinal specification, the pre-period relates to five years previously, and $m_{gtrt}$ is constructed to predict the contribution of new migrants to the CZ-education cell over five years (rather

\textsuperscript{16}A potential drawback of the education classifications is occupational downgrading of migrants. Card (2001) addresses this concern by probabilistically assigning individuals to broad occupation groups (conditional on education and demographic characteristics), separately for natives and migrants. I offer estimates using these imputed occupation groups in Appendix F.

\textsuperscript{17}Previous residence is only classified by state in the 1970 census microdata, and the ACS (after 2000) only reports place of residence 12 months previously. I exploit this same census question to disaggregate the contributions of inflows and outflows to the aggregate-level crowding out effect in Appendix C.
than a decade). As is clear from columns 1 and 4 of Table 8, $m_{grt}$ is a strong instrument in all specifications.

The pooled cross-section estimates of $\delta^{w}_1$ are remarkably large, ranging from 1 to 1.5 for the full internal contribution in column 2 (accounting for both natives and old migrants). That is, each new immigrant in a given CZ-education cell attracts an additional 1-1.5 workers to the same cell (relative to other cells). A comparison with column 3 reveals that these positive effects are (more than) entirely driven by natives.

In contrast, the longitudinal estimates of $\delta^{w}_1$ in column 5 (which are not conflated with cohort effects) are universally negative. They also vary considerably in magnitude, ranging from -3.6 for the college graduate/non-graduate delineation to just -0.19 for the four-group delineation. In most cases, natives contribute substantially to these effects (column 6). The model offers a rationale for this variation: finer delineations (such as the four-group) should engender greater substitutability in production (i.e. larger $\sigma$) and consequently lower estimates of $\delta^{w}_1$. Also, if high school dropouts are close substitutes with other non-college workers (see e.g. Card, 2009a), the relatively low $\delta^{w}_1$ in the third row (-0.43) is perhaps understandable.

Using similar longitudinal data though (from the 1990 census), Card (2001) estimates a $\delta^{w}_1$ which is somewhat positive. In Appendix F, I find the divergence of our estimates is mostly explained by his fine delineation of skill groups (Card uses six imputed occupation groups) and choice of right hand side controls.\textsuperscript{18}

### 7.3 Cohort effects

The difference between the pooled cross-section and longitudinal estimates is suggestive of large cohort effects. And I now offer more direct evidence for this phenomenon, by exploiting census information on individuals’ state of birth. Specifically, using the same estimating equation (35), I show that foreign inflows to a given state exert a larger impact on the education composition of natives born in that state (i.e. the pure birth cohort effect) than on those residing in it (which accounts for both birth cohorts and mobility).

In the first two columns of Table 9, I first replicate the pooled cross-section specification - but now for state-level data. As with the CZ estimates in Table 8, the first stage (using the education-specific enclave shift-share instrument $m_{grt}$) has substantial power for all education delineations. And the IV estimates of the native-only response (column 2 of Table 9) look very similar to the comparable estimates for CZs (column 3 of Table 8).

\textsuperscript{18}Card controls for a range of demographic means at time $t-5$ within the skill-area cells (age, education, migrants' years in US), and these absorb much of the variation in the migration shock. Of course, these controls may be picking up important skill-specific shocks which I have neglected. The purpose of Appendix F is merely to show how our results can be reconciled.
Recall the dependent variable in column 2, $\lambda_{grt}$, is the contribution of natives to group $g$ population growth among state $r$ residents. In column 3, I now replace this with $\lambda_{BPgrt}$: the contribution of natives to group $g$ population growth among those born (rather than residing) in state $r$. The column 3 estimates should now proxy the contribution of cohort effects to state $r$’s education composition - though given that one third of individuals live outside their state of birth, it should understate any such effects. Remarkably, these numbers are all larger than the state of residence effects in column 2 - and for the first two delineations, substantially so. In other words, foreign inflows to a given state exert a larger impact on the education composition of natives born in that state than on those residing in it. This suggests any contribution of internal mobility to the $\delta_1$ estimate in column 2 is more than fully offset by cohort effects.

At first sight, these cohort effects may appear counterintuitive. Low-skilled immigration should raise the return to education and stimulate greater investment (see Hunt, 2017). But the effect could in principle go the other way: Llull (2017) argues a fall in wages may discourage labor market attachment and the accumulation of human capital.

8 Conclusion

Using census data since 1960, I estimate that new immigrants crowd out existing residents one-for-one (or 1.1 for one, in my preferred estimates) over decadal intervals. This effect is robust to numerous specification choices; and even without the instrument, I find substantial crowd-out of 0.76. I base my main estimates on CZs, but I find similar effects across states. The entire effect is driven by reduced internal inflows to the affected areas, rather than larger outflows. The crowding out result does conflict with some of the existing literature, but I show how these estimates can be reconciled.

The magnitude of the effect is puzzling for two reasons. First, the crowding out effect declines significantly (from 1.1 to 0.9) once I condition on local employment growth. But to the extent that employment responds to population, theory predicts the opposite. Second, it is difficult to reconcile perfect crowd-out (which should imply full local adjustment) with the adverse effects on local employment rates, especially among the low educated.

I argue that undercoverage of undocumented migrants can account for these inconsistencies. This view is consistent with the much larger estimates of unconditional (but importantly, not conditional) crowd-out in 1960s and 1970s, when coverage was much poorer. On imposing more structure, I attribute about 30 percent of the average crowding out effect to mismeasurement. The remaining effect is a consequence of both the labor
market impact and compositional disamenities. Though labor demand does respond, the burden of adjustment falls mostly on population.

These results have important methodological implications for the estimation and interpretation of the impact of immigration, both locally and nationally. First, local variation is often exploited to identify the national impact of immigration; but the interpretation of such estimates depends crucially on the response from internal mobility. Second, under-coverage of new immigrants will generate an upward bias in empirical estimates of their effect. It is also likely to distort estimates of the skill composition of migrants - which is the key determinant of the aggregate impact of immigration in structural models.

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A Derivations of equations in theoretical model

A.1 Housing market specification: Derivation of (10)

Suppose workers have Cobb-Douglas preferences over the traded good and housing, so
they spend a fixed fraction $\nu$ of their income on housing. This implies a simple linear
expression for the local price index:

$$ p_{rt} = \nu p^h_{rt} + (1 - \nu) p_t \tag{A1} $$

For simplicity, I assume non-employed individuals receive no income. Housing demand
in area $r$ can then be written as:

$$ H^d_{rt} = \nu \frac{W_{rt} N_{rt}}{P^h_{rt}} \tag{A2} $$

and in logarithms:

$$ h^d_{rt} = \log \nu + w_{rt} + n_{rt} - p^h_{rt} \tag{A3} $$
In turn, suppose housing supply can be written as:

\[ h_{rt}^s = \epsilon^{hs} (p_{rt}^h - p_t) \]  

(A4)

For simplicity, I have assumed in (A4) that housing production does not depend on local labor, but see the Online Appendices of Amior and Manning (2018) for such an extension. Equating supply and demand, and substituting (A1) for \( p_{rt}^h \), then gives:

\[ p_{rt} - p_t = \frac{\nu}{1 - \nu + \epsilon^{hs}} \left[ \log \nu + \frac{1}{\epsilon^s} (n_{rt} - l_{rt} - z_{rt}^s) + n_{rt} \right] \]  

(A5)

And taking first differences then yields equation (10) in the main text:

\[ \Delta (p_{rt} - p_t) = \frac{1}{\kappa} \left[ \frac{1}{\epsilon^s} (\Delta n_{rt} - \Delta l_{rt} - \Delta z_{rt}^s) + \Delta n_{rt} \right] \]  

(A6)

where

\[ \kappa \equiv \frac{1 - \nu + \epsilon^{hs}}{\nu} \]  

(A7)

is increasing in the elasticity of housing supply.

**A.2 Unconditional crowd-out: Derivation of (13)**

To move from the conditional crowding out specification (9) to the unconditional specification (13), I require a solution for local employment. Using the labor supply and demand curves, (2) and (3), local employment growth can be expressed as:

\[ \Delta n_{rt} = \frac{\epsilon^s}{\epsilon^s + \epsilon^d} \Delta z_{rt}^d + \frac{\epsilon^d}{\epsilon^s + \epsilon^d} (\Delta l_{rt} + \Delta z_{rt}^s) - \frac{\epsilon^s \epsilon^d}{\epsilon^s + \epsilon^d} \Delta (p_{rt} - p_t) \]  

(A8)

Replacing the local price deviation \( \Delta (p_{rt} - p_t) \) with (A6):

\[ \Delta n_{rt} = \eta (\Delta l_{rt} + \Delta z_{rt}^s) + (1 - \eta) \frac{\kappa}{\kappa + \epsilon^d} \Delta z_{rt}^d \]  

(A9)

and disaggregating local population growth \( \Delta l_{rt} \) into foreign and internal contributions:

\[ \Delta n_{rt} = \eta (\lambda_{rt}^F + \lambda_{rt}^I + \Delta z_{rt}^s) + (1 - \eta) \frac{\kappa}{\kappa + \epsilon^d} \Delta z_{rt}^d \]  

(A10)

where

\[ \eta \equiv 1 - \left( 1 + \frac{\kappa + 1}{\kappa + \epsilon^d} \cdot \frac{\epsilon^d}{\epsilon^s} \right)^{-1} \]  

(A11)

Equation (13) can then be derived by substituting (A10) for \( \Delta n_{rt} \) in (9).
A.3 Derivation of within-area empirical specification

In this appendix, I show how the multi-skill model described in Section 7.1 can be mapped onto the empirical specification (35), accounting for skill-specific population dynamics.

In line with (2) in Section 2, I first write an equation for labor supply to skill group \( g \):

\[
n_{gr} = l_{gr} + \epsilon^s (w_{gr} - p_r) + z^s_{gr} \tag{A12}
\]

And in line with (4), suppose that indirect utility for skill group \( g \) depends on a skill-specific amenity value \( a_{gr} \) and real consumption wage \( (w_{gr} - p_r) \), which itself can be replaced with the employment rate using (A12):

\[
v_{gr} = w_{gr} - p_r + a_{gr} = \frac{1}{\epsilon^s} (n_{gr} - l_{gr} - z^s_{gr}) + a_{gr} \tag{A13}
\]

Notice that local labor market conditions for skill group \( g \) can be fully summarized by the skill-specific employment rate \( (n_{gr} - l_{gr}) \): this is a skill-specific version of the sufficient statistic result in Section 2.

Group \( g \) subscripts can also be applied to the internal migratory response, equation (7). For simplicity, suppose the elasticity \( \gamma \) is common to all skill groups. So, the resident group \( g \) population adjusts (sluggishly) with elasticity \( \gamma \) to skill-specific differentials in local utility \( v_{gr} \).

\[
\lambda^I_{gr} = \gamma \left( n_{gr} - l_{gr} - z^s_{gr} + \epsilon^s a_{gr} \right) \tag{A14}
\]

By symmetry with the model in Section 2, these equations can be discretized to yield a skill-specific version of (9):

\[
\lambda^I_{grt} = \left( 1 - \frac{1 - e^{-\gamma}}{\gamma I} \right) \left( \Delta n_{grt} - \lambda^F_{grt} - \Delta z^s_{grt} + \epsilon^s \Delta a_{grt} \right) \tag{A15}
\]

where \( \lambda^F_{grt} \) is the skill-specific foreign inflow. To derive the unconditional crowding out effect, I require a solution for local skill-specific employment \( \Delta n_{grt} \). Given (A12) and the skill demand relationship in (36), this can be characterized as:

\[
\Delta n_{grt} = \frac{\epsilon^s}{1 + \epsilon^s (1 - \sigma)} \left( \Delta \log \theta_{grt} + \frac{\sigma}{\rho} \Delta \log \psi_{rt} + \frac{\rho - \sigma}{\rho} \Delta y_{rt} - \Delta p_{rt} + \Delta p_t \right) + \frac{1}{1 + \epsilon^s (1 - \sigma)} \left( \Delta l_{grt} + \Delta z^s_{grt} \right) \tag{A16}
\]
Substituting this for $\Delta n_{grt}$ in (A15), this yields:

$$
\lambda_{grt}^I = \frac{(1 - \eta^w)(1 - \frac{1-e^{-\gamma}}{\gamma})}{1 - \eta^w (1 - \frac{1-e^{-\gamma}}{\gamma})} \left( \frac{1}{1 - \sigma} \Delta \log \theta_{grt} - \lambda_{grt}^F - \Delta z_{grt}^s + \frac{\epsilon^s}{1 - \eta^w} \Delta a_{grt} \right) (A17)
$$

$$
+ \frac{(1 - \eta^w)(1 - \frac{1-e^{-\gamma}}{\gamma})}{1 - \eta^w (1 - \frac{1-e^{-\gamma}}{\gamma})} \cdot \frac{1}{1 - \sigma} \left( \frac{\sigma}{\rho} \Delta \log \psi_{rt} + \frac{\rho - \sigma}{\rho} \Delta y_{rt} - \Delta p_{rt} + \Delta p_t \right)
$$

$$
+ \frac{1 - e^{-\gamma}}{1 - \eta^w (1 - \frac{1-e^{-\gamma}}{\gamma})} (n_{grt-1} - l_{grt-1} - z_{grt-1}^s + \epsilon^s a_{grt-1})
$$

where

$$
\eta^w \equiv [\epsilon^s (1 - \sigma)]^{-1} \quad (A18)
$$

is the within-area elasticity of employment with respect to population, analogous to the aggregate-level $\eta$ in (A11).

Now consider how this maps onto the within-area empirical specification (35). The area-time fixed effects $d_{rt}$ will absorb the contents of the second line of (A17). The skill-time fixed effects $d_{gt}$ will absorb any skill-time varying components of: (1) the skill-specific demand shock $\Delta \log \theta_{grt}$, (2) the skill-specific supply shock $\Delta z_{grt}^s$, (3) the skill-specific amenity shock $\Delta a_{grt}$, and (4) the initial conditions on the final line of (A17). All remaining variation will fall into the error term $\epsilon_{grt}$, so the IV exclusion restriction requires that it is uncorrelated with the skill-specific enclave shift-share $m_{grt}$ in (37). Under these conditions (and the model’s various assumptions), the coefficient of interest $\delta^w_1$ will identify the coefficient on $\lambda_{grt}^F$ in (A17):

$$
\delta^w_1 = \frac{(1 - \eta^w)(1 - \frac{1-e^{-\gamma}}{\gamma})}{1 - \eta^w (1 - \frac{1-e^{-\gamma}}{\gamma})}
$$

(A19)

As I state in Section 7.1, $\delta^w_1$ is increasing in the internal mobility response $\gamma$, but decreasing in the elasticity of substitution $\sigma$ between skill groups in production.

B Residualization of wages and housing costs

In Section 5, I study the impact of immigration on local residualized wages, housing rents and housing prices. I construct these in the same way as Amior and Manning (2018), and I offer details here.

I compute hourly wages as the ratio of annual labor earnings to the product of weeks worked and usual hours per week in the census and ACS microdata. I restrict my wage sample to employees aged 16-64, excluding those in group quarters; and I also exclude wage observations below the 1st and above the 99th percentiles within each geographical unit in the microdata. For each census cross-section, I then regress log hourly wages
on a rich set of demographic controls\(^\text{19}\), and I compute the mean residual within each geographical unit (for the nativity group of interest). I then impute CZ-level wages by taking weighted averages across these units.

My housing sample consists of houses and apartments; I exclude farms, units with over 10 acres of land, and units with commercial use. To construct the rental index, I regress the monthly rents of privately rented units on a rich set of housing characteristics\(^\text{20}\) (restricting attention to prices between the 1st and 99th percentiles, within each geographical unit, from the sample), separately for each census cross-section. And I compute the local mean of the residuals within each geographical unit. I residualize local housing prices in the same way, though the sample is now restricted to owner-occupied units. As with wages, I impute CZ-level housing cost measures by taking weighted averages across the geographical units in each microdata sample.

C Contributions of inflows and outflows to crowd-out

It turns out that the geographical crowd-out is entirely driven by a reduction in migratory inflows to the affected CZ, rather than an increase in migratory outflows. I present the evidence in this appendix.

Similarly to Section 7, I exploit the longitudinal dimension of the census: respondents were asked where they were living five years previously. In practice, one can construct the relevant variables using microdata (as I do in Section 7.2). But since I do not need to disaggregate by education for this exercise, I instead use published statistics on gross migratory flows between all country pairs: these are based on larger samples and require no geographical imputation. I use data for the periods 1965-70, 1975-80, 1985-90 and 1995-2000, and I aggregate all flows to CZ level\(^\text{21}\).

My strategy is to re-estimate the crowding out equation (20), but replacing the decadal

\(^{19}\)These are the same controls I use for adjusting local employment rates: age, age squared, five education indicators, black/Asian/Hispanic indicators, gender, foreign-born status, and where available, years in US and its square for migrants, together with a rich set of interactions.

\(^{20}\)Specifically, number of rooms (9 indicators) and bedrooms (6 indicators); an interaction between number of rooms and bedrooms; building age (up to 9 indicators, depending on cross-section), presence of kitchen, complete plumbing and condominium status; I also control for a house/apartment dummy, together with interactions between this and all previously-mentioned variables.

foreign and internal contributions with 5-year flows. In particular, my specification is:

\[
\hat{\lambda}_{rt}^{F5} = \delta_{r} + \delta_{F} \hat{\lambda}_{rt}^{F5} + \delta_{b} b_{rt} + \delta_{u} (n_{rt-10} - I_{rt-10}) + A_{r} \delta_{A} + \varepsilon_{rt} \tag{A20}
\]

where the \( t \) subscript now designates years, rather than decades (as in the main text), and \( \hat{\lambda}_{rt}^{F5} \) and \( \hat{\lambda}_{rt}^{I5} \) are respectively the 5-year foreign and internal contributions to the change in log population. These are constructed in line with equations (14) and (15). Specifically, \( \hat{\lambda}_{rt}^{F5} \equiv \log\left( \frac{L_{rt} - 5 + L_{F5}}{L_{rt-5}} \right) \), where \( L_{rt}^{F5} \) is the 5-year flow into area \( r \) from abroad, and \( L_{rt-5} \) is the local population at time \( t - 5 \) (based on census respondents’ reported place of residence five years previously). And in turn, \( \hat{\lambda}_{rt}^{I} \equiv \log\left( \frac{L_{rt} - 5 + L_{Ii} - L_{Io}}{L_{rt-5}} \right) \), where \( L_{rt}^{Ii} \) and \( L_{rt}^{Io} \) are respectively the 5-year inflows and outflows to/from others parts of the US. Notice that, by construction, \( L_{rt} \equiv L_{rt-5} + L_{rt}^{F5} + L_{rt}^{Ii} - L_{rt}^{Io} \). Given that the flows are based on the reports of time \( t \) residents, individuals who emigrated from the US between \( t - 5 \) and \( t \) are excluded from this data.

I do not observe employment outcomes between census years (i.e. at 5 year intervals), so I choose to use the same right hand side variables as in equation (20): the decadal Bartik shift-share \( b_{rt} \) (which predicts employment growth between \( t - 10 \) and \( t \)), the employment rate lagged ten years, and the amenity controls. The mismatch in time periods is not ideal, and one should keep this in mind when interpreting the estimates.

I report OLS and IV estimates in Table A1. I instrument \( \hat{\lambda}_{rt}^{F5} \) using a 5-year migrant shift-share, constructed to predict the 5-year flow and based on migrant settlement patterns in \( t - 5 \). I construct these settlement patterns using migrants’ reported historical residence in the census microdata of year \( t \) (i.e. following a similar procedure to the longitudinal estimates of Section 7.2). I instrument the lagged employment rate using the lagged decadal Bartik shift-share.

The standard errors on the OLS estimates are too large to make definitive statements. But the IV estimates tell a much clearer story. Column 4 reports the basic \( \delta_{1} \) estimate, based on equation (A20). This points to a large crowding out effect (-1.6), somewhat in excess of one-for-one.\(^{22}\) In the next two columns, I disaggregate the effect into (approximate) contributions from internal inflows and outflows: column 5 replaces the dependent variable with \( \hat{\lambda}_{rt}^{Ii} \equiv \log\left( \frac{L_{rt} - 5 + L_{Ii}}{L_{rt-5}} \right) \), and column 6 replaces it with \( \hat{\lambda}_{rt}^{Io} \equiv \log\left( \frac{L_{rt} - 5 + L_{Io}}{L_{rt-5}} \right) \). The crowding out effect is entirely driven by variation in inflows rather than outflows. The effect on outflows is statistically insignificant.

\(^{22}\) This may reflect my inability to control for initial conditions at \( t - 5 \).
D Robustness of crowd-out estimates

D.1 Graphical illustration of unconditional crowd-out estimates

I now consider the robustness of my unconditional crowding out estimates in Section 4. One concern is that my estimates of the coefficient of interest, $\delta^u_1$, in equation (20) may be driven by outliers. To address this point, Figure A1 graphically illustrates the basic OLS and IV estimates of $\delta^u_1$, i.e. those of columns 3 and 4 of Table 3.

These plots follow the logic of the Frisch-Waugh theorem. For OLS, I compute residuals from regressions of both the residual and foreign contributions ($\hat{\lambda}^I_{rt}$ and $\hat{\lambda}^F_{rt}$ respectively) on the remaining controls: the lagged employment rate, the current Bartik shift-share, year effects and the amenity variables (interacted with year effects). And I then plot the $\hat{\lambda}^I_{rt}$ residuals against the $\hat{\lambda}^F_{rt}$ residuals.

For the IV plot, I apply the same logic to two-stage least squares. I begin by generating predictions of the two endogenous variables (the contribution of new migrants, $\hat{\lambda}^I_{rt}$, and the lagged employment rate, $n_{rt-1} - l_{rt-1}$), based on the first stage regressions (using the enclave shift-share $m_{rt}$ and lagged Bartik $b_{rt-1}$ instruments). I then compute residuals from regressions of both $\hat{\lambda}^I_{rt}$ and the predicted $\hat{\lambda}^F_{rt}$ on the remaining controls: the predicted lagged employment rate, the current Bartik shift-share, year effects and the amenity variables (interacted with year effects). And as before, I plot the $\hat{\lambda}^I_{rt}$ residuals against the $\hat{\lambda}^F_{rt}$ residuals.

The marker size in the plots correspond to the lagged population share weights. The (weighted) slopes of the fit lines are identical to the $\delta^u_1$ estimates in columns 3 and 4 in Table 3. Of course, the standard errors do not match: I do not account for state clustering in Figure A1; and for IV, the naive two-stage estimator does not account for sampling error in the first stage. In any case, the main take-away is that the $\delta^u_1$ estimates are not visibly driven by outliers.

D.2 Robustness to sample and specification

In this section, I study the robustness of my unconditional crowd-out estimates to sample and specification. One major concern is serial correlation in the enclave shift-share instrument (see Jaeger, Ruist and Stuhler, 2018), but I have attempted to address this by controlling for the initial conditions, as summarized by the initial employment rate. The evidence I present in the main text suggests this approach is effective. But there are other possible concerns, and I attempt to address some of these in Table A2.
Panel A offers estimates weighted by lagged population share, in line with the main text. Column 1 reproduces the basic IV unconditional crowding out estimate in column 4 of Table 3, based on equation (20). I instrument the foreign inflow \( \hat{\lambda}_{ft} \) using the enclave shift-share \( m_{rt} \) and the lagged employment rate using a lagged Bartik. Panel B offers unweighted estimates of \( \delta_{u1} \): the coefficient is not much different (-0.94 compared to -1.1), though the standard error is somewhat larger. This suggests the effects are not merely driven by large CZs, consistent with the patterns in Figure A1.

Alternatively, one may prefer to see estimates which exclude smaller CZs - in line with much of the literature, which restricts attention to metro areas. In column 2, I re-estimate the model for the sample of CZs which have at least 200,000 individuals aged 16-64 in the initial period. But this makes little difference to the results. Given the skew in the spatial distribution of foreign inflows, one may also be concerned that the estimates are driven by CZs facing unusually high inflows. In column 3, I exclude observations with values of the enclave shift-share \( m_{rt} \) above 0.1, which is the 98th percentile (the maximum value is 0.29: see Table 1). But again, this makes little difference.

Recall the enclave shift-share instrument \( m_{rt} \) is given by \( \log \left( \frac{L_{rt}-1+\lambda_{rt}}{L_{rt}-1} \right) \), where \( \lambda_{rt-1} \equiv \sum_o \phi_{or(\cdot)t} L_{Fo}(\cdot) \) is a shorthand for the predicted number of incoming migrants between \( t-1 \) and \( t \): see equation (18). Notice I am using the \( t-1 \) migrant settlement patterns (in \( \phi_{or(\cdot)t} \)) to predict foreign inflows in each subsequent decade. But other studies have taken a different approach: for example, Hunt (2017) predicts inflows in all decades from 1940 to 2010 using the 1940 settlement patterns. In column 4, I replace my instrument with \( \log \left( \frac{L_{rt}-1+\lambda_{F60}^{rt}}{L_{rt}-1} \right) \), where \( \lambda_{F60}^{rt} \equiv \sum_o \phi_{or60}^{rt} L_{Fo}(\cdot) \) predicts the migrant inflow based on 1960 settlement patterns, \( \phi_{or60}^{rt} \), for every decade. The weighted and unweighted estimates are now somewhat larger (-1.4 and -1.5 respectively), though they are not significantly different from -1.

In my basic crowding out specification (20), I approximate the foreign and internal contributions (to the change in log population) as \( \log \left( \frac{L_{rt}-1+L_{Frt}^{rt}}{L_{rt}-1} \right) \) and \( \log \left( \frac{L_{rt}-L_{Frt}^{rt}}{L_{rt}-1} \right) \) respectively. But much of the literature has taken a first order approximation, defining them as \( L_{Frt}^{rt} \) and \( \Delta L_{rt}^{rt} \): see e.g. Card (2001), Peri and Sparber (2011) and Card and Peri (2016). Column 5 re-estimates (20) using these definitions; and to maintain symmetry, I replace the instrument with \( \frac{\lambda_{F}^{rt}}{L_{rt}-1} \). But this makes little difference to the estimate.

Another possible concern is the predictive power of the instrument. Suppose the predicted number of incoming migrants, \( \lambda_{rt}^{F} \), is largely noise. Then variation in \( L_{rt-1} \) may generate artificial positive correlation between the endogenous variable and the instrument: see Clemens and Hunt (2019). This problem becomes worse if the \( L_{Frt}^{rt} \) component of the endogenous variable, \( \frac{L_{Frt}^{rt}}{L_{rt}-1} \), is itself also noisy. Indeed, Aydemir and Borjas (2011) argue that measurement error in the local migrant share can result in substantial atten-
uation bias, especially in the presence of fixed effects (which may absorb much of the meaningful variation). To address this concern, in column 6, I replace the instrument (which is expressed relative to the initial population) with the predicted inflow of new migrants in levels, $\Lambda_{rt}^F$. But again, this has little effect on the crowding out estimate or even its standard error.

An important reference in this context is Wozniak and Murray (2012), who estimate geographical crowd-out using a specification entirely expressed in levels. Building on equation (20), a specification in levels would be:

$$\Delta L_{rt} - L_{rt}^F = \delta_0^{uL} + \delta_1^{uL} L_{rt} + \delta_2^{uL} b_{rt} + \delta_3^{uL} (n_{rt-1} - l_{rt-1}) + A_t \delta_{At}^{uL} + \epsilon_{rt}$$

(A21)

where the dependent variable is the change in local population, less the stock of new immigrants; and the key regressor $L_{rt}^F$ is simply the number of new immigrants. I estimate $\delta_1^{uL}$ in column 7, yielding a coefficient on just -0.23 in Panel A. However, local population is an important omitted variable in this specification (Wright, Ellis and Reibel, 1997; Peri and Sparber, 2011; Wozniak and Murray, 2012): local population may be correlated with both the inflow of new migrants and subsequent population change. To address this concern, Wozniak and Murray recommend controlling for local fixed effects. Once I include CZ fixed effects (column 8), my estimate of $\delta_1^{uL}$ is again remarkably close to -1 irrespective of weighting. Notice that this specification is also immune to the criticism of Clemens and Hunt (2019), described in the previous paragraph, of spurious correlation in the population denominator.

In column 9, I apply CZ fixed effects directly to the basic specification in column 1. These effectively partial out CZ-specific linear trends in population. This approach is similar in spirit to the double differencing methodology (comparing changes before and after 1970) of Borjas, Freeman and Katz (1997) and is recommended by Hong and McLaren (2015). In terms of theory, the purpose of the fixed effects is to account for time-invariant unobserved components of the amenity, supply or demand effects in equation (20). However, their inclusion is empirically demanding in such a short panel, especially given the strong persistence in the enclave shift-share instrument $m_{rt}$. And as Aydemir and Borjas (2011) argue, measurement error may be more of a problem here. With population weights, I estimate a $\delta_1^{uL}$ of -0.63 with a very large standard error (0.61). In column 10, to ease the demands of the specification, I replace the lagged employment rate (i.e. the initial conditions) with historical shocks: a lagged Bartik $b_{rt-1}$ (originally used as an instrument) and a lagged enclave shift-share $m_{rt-1}$. I now estimate a much larger $\delta_1^{uL}$: -1.35, with a standard error of just 0.26. Without population weights, I attain perversely large estimates of $\delta_1^{uL}$ (in excess of -2) in both columns 9 and 10, though the standard errors are also large. However, the first stage F-statistics for the foreign inflow $\hat{\lambda}_{rt}^F$ are small in the unweighted specifications: about 6 in each case. I do not report fixed
effect estimates using the 1960-based enclave shift-share: this instrument has no power under fixed effects.

E Undercoverage bias: Supplementary theory and estimates

E.1 Bias in conditional crowd-out estimates: Equation (30)

In this section, I offer more complete steps for equation (30), where I derive expressions for the (biased) conditional crowd-out estimators, $\delta_1^c$ and $\delta_2^c$. From Section 6.2, I have the following expressions for the observed (biased) foreign inflow $\tilde{\lambda}_{rt}^F$ and employment growth $\Delta \tilde{n}_{rt}$:

$$\tilde{\lambda}_{rt}^F = (1 - \pi) \lambda_{rt}^F$$

$$\Delta \tilde{n}_{rt} = \Delta n_{rt} - \pi \lambda_{rt}^F$$

where $\lambda_{rt}^F$ and $\Delta n_{rt}$ are their true counterparts. And the equation for the (net) internal inflow is:

$$\lambda_{rt}^I = \delta_1^c \lambda_{rt}^F + \delta_2^c \Delta n_{rt}$$

Based on these:

$$Var \left( \tilde{\lambda}_{rt}^F \right) = (1 - \pi)^2 V^F$$

$$Var \left( \Delta \tilde{n}_{rt} \right) = V^n + \pi^2 V^F - 2\pi C^{Fn}$$

$$Cov \left( \Delta \tilde{n}_{rt}, \tilde{\lambda}_{rt}^F \right) = (1 - \pi) \left( C^{Fn} - \pi V^F \right)$$

$$Cov \left( \tilde{\lambda}_{rt}^F, \lambda_{rt}^I \right) = (1 - \pi) \left( \delta_1^c V^F + \delta_2^c C^{Fn} \right)$$

$$Cov \left( \Delta \tilde{n}_{rt}, \lambda_{rt}^I \right) = (\delta_1^c - \pi \delta_2^c) C^{Fn} + \delta_2^c V^n - \pi \delta_1^c V^F$$

where

$$V^F \equiv Var \left( \lambda_{rt}^F \right)$$

$$V^n \equiv Var \left( \Delta n_{rt} \right)$$

$$C^{Fn} \equiv Cov \left( \Delta n_{rt}, \lambda_{rt}^F \right)$$
Substituting (A25)-(A29) into equation (30) then gives:

\[
\begin{pmatrix}
\delta_1 c \\
\delta_2 c \\
\end{pmatrix} =
\begin{pmatrix}
V \text{ar}(\hat{\lambda}_F^c) & \text{Cov}(\Delta \hat{n}_{rt}, \hat{\lambda}_F^c) \\
\text{Cov}(\Delta \hat{n}_{rt}, \hat{\lambda}_F^c) & V \text{ar}(\Delta \hat{n}_{rt}) \\
\end{pmatrix}^{-1}
\begin{pmatrix}
\text{Cov}(\hat{\lambda}_F^c, \lambda_I^c) \\
\text{Cov}(\Delta \hat{n}_{rt}, \lambda_I^c) \\
\end{pmatrix}
\]

\[\text{(A33)}\]

\[
\frac{\delta_1 \pi V F}{\delta_2} \left( \frac{\delta_1 \delta_2}{\delta_1 + \delta_2} \right)
\]

\[
\frac{\delta_1 \pi V F}{\delta_2} \left( \frac{\delta_1 \delta_2}{\delta_1 + \delta_2} \right)
\]

E.2 Estimates of employment elasticity

In this Appendix, I offer estimates of the employment elasticity \( \eta \) to local population. For simplicity, the model in the main text assumes that employment adjusts instantaneously: equation (11) contains no dynamic terms. But Amior and Manning (2018) do find evidence of such dynamics, and their Online Appendices show how one can derive an estimating equation of the following form:

\[
\Delta n_{rt} = \eta + \eta_1 \Delta l_{rt} + \eta_2 (n_{rt-1} - l_{rt-1}) + \eta_3 b_{rt} + A_r \eta_A + \varepsilon_{rt} \quad \text{(A34)}
\]

The lagged employment rate summarizes the impact of historical shocks on contemporaneous changes in employment demand: firms should cut employment if labor supply is initially sparse and costly (i.e. if \( n_{rt-1} - l_{rt-1} \) is large). I also use the current Bartik \( b_{rt} \) as a control, to account for observable components of contemporaneous labor demand shocks.

New to this paper, I extend this specification by allowing for distinct effects of foreign and (net) internal inflows:

\[
\Delta n_{rt} = \eta + \eta_{1F} \hat{\lambda}_F + \eta_{1L} \hat{\lambda}_L + \eta_2 (n_{rt-1} - l_{rt-1}) + \eta_3 b_{rt} + A_r \eta_A + \varepsilon_{rt} \quad \text{(A35)}
\]

Clearly, OLS estimates of (A34) and (A35) cannot be interpreted causally. Omitted demand shocks in the errors will generate confounding positive correlation between employment on the left hand side and population on the right. The natural instrument for population growth \( \Delta l_{rt} \) is the enclave shift-share. However, as column 1 of Table A3 shows, this has no power: this reflects the one-for-one crowd-out. In column 2, like Beaudry et al. (2014a; 2014b) and Amior and Manning (2018), I use maximum January temperature as an instrument: Rappaport (2007) shows that Americans have been moving to places with milder winters. (In these specification, I exclude January temperature and its interactions with year effects from the \( A_r \) amenity vector on the right hand side.) This has a strong positive effect on population.

To identify the impact of \( \hat{\lambda}_F \) and \( \hat{\lambda}_L \) separately in (A35), I use both the enclave shift-share and January temperature as instruments. As expected, the shift-share has a large
positive effect on $\hat{\lambda}^F_{rt}$ (column 3 of Table A3) and a large negative effect on $\hat{\lambda}^I_{rt}$ (column 4); whereas January temperature has a large effect on $\hat{\lambda}^I_{rt}$, but matters little for $\hat{\lambda}^F_{rt}$. As before, I instrument the lagged employment rate with the lagged Bartik: the final three columns show a large positive effect.

I present OLS and IV estimates of (A34) and (A35) in Table A4. The OLS elasticity to $\Delta l_{rt}$ is essentially 1, and the effects of $\hat{\lambda}^F_{rt}$ and $\hat{\lambda}^I_{rt}$ are 0.87 and 0.99 respectively. Once I apply the instrument, I estimate smaller effects - as one might expect. Column 3, which instruments $\Delta l_{rt}$ with the enclave shift-share, has insufficient power to identify anything - for the reasons explained above. Once I use the January temperature instrument, I identify an elasticity to $\Delta l_{rt}$ of 0.74.\textsuperscript{23} And in column 4, the effects of $\hat{\lambda}^F_{rt}$ and $\hat{\lambda}^I_{rt}$ are now 0.61 and 0.78 respectively. Based on my model’s assumption that natives and migrants supply identical labor, these should be identical. But since they are identified using divergent sources of variation, it is perhaps unwise to over-interpret the gap between them.

E.3 Calibration of undercoverage bias

What do these $\eta$ estimates imply about the undercoverage bias, $\pi$? Given I am studying crowding out in response to immigration (identified by the enclave shift-share), I choose to focus on the estimated elasticity to $\hat{\lambda}^F_{rt}$, i.e. $\eta_{1F}$ in (A35).

But it should be emphasized that any undercoverage will bias my estimate of $\eta_{1F}$ upwards. To derive an expression for this bias, I follow identical steps to those of Section E.2. Abstracting from contemporaneous shocks and initial conditions in A35, the true model for employment can be written as:

$$\Delta n_{rt} = \eta_{1F} \hat{\lambda}^F_{rt} + \eta_{1I} \hat{\lambda}^I_{rt}$$  \hfill (A36)

But given we only observe $\Delta \tilde{n}_{rt}$ and $\hat{\lambda}^F_{rt}$ with error, I estimate:

$$\Delta \tilde{n}_{rt} = \tilde{\eta}_{1F} \hat{\lambda}^F_{rt} + \tilde{\eta}_{1I} \hat{\lambda}^I_{rt}$$  \hfill (A37)

where $\Delta \tilde{n}_{rt}$ and $\hat{\lambda}^F_{rt}$ are defined as in (A22) and (A23). Therefore, OLS estimates of $\tilde{\eta}_{1F}$

\textsuperscript{23}Amior and Manning (2018) estimate a slightly larger $\alpha_1$ of 0.79. This can be attributed to two differences: they use one more decade of data (their sample includes the 1950s), and they do not adjust employment for demographic composition.
and $\tilde{\eta}_I$ will identify:

$$\begin{pmatrix} \tilde{\eta}_F \\ \tilde{\eta}_I \end{pmatrix} = \begin{pmatrix} \text{Var}\left(\tilde{\lambda}_F^t\right) & \text{Cov}\left(\lambda_I^t, \tilde{\lambda}_F^t\right) \\ \text{Cov}\left(\lambda_I^t, \tilde{\lambda}_F^t\right) & \text{Var}\left(\lambda_I^t\right) \end{pmatrix}^{-1} \begin{pmatrix} \text{Cov}\left(\tilde{\lambda}_F^t, \Delta\tilde{n}_F^t\right) \\ \text{Cov}\left(\lambda_I^t, \Delta\tilde{n}_F^t\right) \end{pmatrix}$$

$$= \begin{pmatrix} (1-\pi)^2 V^F & (1-\pi) C^{FI} \\ (1-\pi) C^{FI} & V^I \end{pmatrix}^{-1} \begin{pmatrix} (1-\pi) \left(\eta_F V^F + \eta_I C^{FI}\right) \\ \eta_F C^{FI} + \eta_I V^I \end{pmatrix}$$

$$= \begin{pmatrix} \frac{1}{1-\pi} \eta_F \\ \eta_I \end{pmatrix} \quad (A38)$$

where $V^F \equiv \text{Var}\left(\lambda_F^t\right)$, $V^I \equiv \text{Var}\left(\lambda_I^t\right)$ and $C^{FI} \equiv \text{Cov}\left(\lambda_F^t, \lambda_I^t\right)$. It follows from (A38) that $\tilde{\eta}_I$ identifies $\eta_I$, while $\tilde{\eta}_F$ is upward biased:

$$\frac{\hat{\eta}_I - \eta_I}{\hat{\eta}_F} = \pi$$

Equation (A39) describes a negative relationship between $\eta$ and $\pi$, for a given estimate $\tilde{\eta}_F$ - which I calibrate to 0.61, based on column 4 of Table A4. Together with the positive $(\eta, \pi)$ relationship described in Figure 1 in the main text, this allows me to identify the two parameters: these take values of 0.44 and 0.27 respectively.

For completeness, I also calibrate the employment elasticity using the estimated response to $\tilde{\lambda}_I^t$, i.e. $\eta_I$ in (A35). This is equal to 0.78 and is (in principle) uncontaminated by undercoverage bias. Figure 1 would then imply a $\pi$ of 0.44. Though a $\pi$ of 0.27 (my preferred estimate) seems plausible in the context of the evidence described in Section 6.1, 0.44 does appear too large.

### Appendix F Reconciliation with Card (2001)

The seminal reference in the geographical crowding out literature is Card (2001). He offers within-area estimates of crowding out, i.e. $\delta^w_1$ in (35), but exploiting longitudinal residential information in the US census (respondents were asked where they lived five years previously: see Section 7.2). This approach should address concerns about cohort effects, but he still estimates a positive value for $\delta^w_1$ - with each new immigrant to an area-skill cell attracting (on net) 0.25 additional residents. This appears to conflict with my own longitudinal estimates in column 5 of Table 8 in the main text. In this appendix, I attempt to reconcile my results with his. It appears the divergence of our estimates is mostly explained by the delineation of skill groups (Card uses six imputed occupation groups) and choice of right hand side controls. The sample of geographical areas also plays a role.

I begin my attempting to replicate Card’s results. In line with his paper, I study
variation across the 175 largest MSAs in the 5 percent census extract of 1990. The sample is restricted to individuals aged 16 to 68 with more than one year of potential experience. In constructing his sample, Card uses all foreign-born individuals in the census extract and a 25 percent random sample of the native-born. I instead use the full sample of natives, and this may (at least partly) account for some small discrepancies between his estimates and my replication. Card delineates six skill groups by probabilistically assigning individuals into broad occupation categories (laborers and low skilled services; operative and craft; clerical; sales; managers; professional and technical), conditional on their education and demographic characteristics. This assignment is based on predictions from a multinomial logit model, estimated separately for native men, native women, migrant men and migrant women; and I follow the procedure set out in his appendix. This approach offers the advantage of accounting for any occupational downgrading of migrants (see e.g. Dustmann, Schoenberg and Stuhler, 2016).

Card estimates a specification very similar to (35), except he uses first order approximations of $\lambda^I_{grt}$ and $\lambda^F_{grt}$. Specifically:

$$\frac{(L_{gr,1990}^I - L_{gr,1990}^F)}{L_{gr,1985}^I} \approx \delta^w_0 + \delta^w_1 \frac{L_{gr,1990}^F}{L_{gr,1985}^I} + X_{gr} \delta^w_X + d_g + d_r + \varepsilon_{gr} \quad (A40)$$

where $L_{gr,1990}^I$ is the population of skill group $g$ in area $r$ in the census year (1990); $L_{gr,1985}^I$ is the local population five years previously, based on responses to the 1990 census; and $L_{gr,1990}^F$ is the number of immigrants in the skill-area cell in 1990 who were living abroad in 1985. Thus, the dependent variable is the contribution of natives and earlier (pre-1985) migrants to population growth (net of emigrants from the US, who do not appear in the sample), and the regressor $\frac{L_{gr,1990}^F}{L_{gr,1985}^I}$ is the contribution of immigration to that growth. To be more precise, Card actually uses the total (within-cell) population growth $\frac{L_{gr,1990} - L_{gr,1985}}{L_{gr,1985}}$ as the dependent variable, but this is a cosmetic difference: it simply adds a value of 1 to the $\delta^w_1$ coefficient (see Peri and Sparber, 2011). $X_{gr}$ is a vector of mean characteristics of individuals in the ($g$, $r$) cell: these consist of mean age, mean age squared, mean years of schooling and fraction black, separately for both natives and migrants in the cell, and (for migrants only) mean years in the US. Finally, $d_g$ and $d_r$ are full sets of skill group and area fixed effects respectively.

The instrument for $\frac{L_{gr,1990}^F}{L_{gr,1985}^I}$ is a first order approximation of (37) in the main text, specifically $\sum_o \phi^o_{r,1985} \frac{L_{gr,1990}^F}{L_{gr,1985}^o}$, where $\phi^o_{r,1985}$ is the share of origin $o$ migrants who lived in area $r$ in 1985, and $L_{gr,1990}^o$ is the number of new origin $o$ migrants who arrived in the US.

---

24 The 1990 census microdata includes sub-state geographical identifiers known as Public Use Microdata Areas (PUMAs), and a concordance between PUMAs and MSAs can be found at: https://usa.ipums.org/usa/volii/puma.shtml. A number of PUMAs straddle MSA boundaries; and following Card, I allocate the population of a given PUMA to an MSA if at least half that PUMA’s population resides in the MSA.
between 1985 and 1990. I use the 17 origin country groups described by Card.

In his baseline OLS specification (with 175 MSAs and observations weighted by cell population), Card estimates \( \delta^w \) as 0.25, with a standard error of 0.04: i.e. a “reverse” crowding out effect.\(^{25}\) Card’s IV estimate is also 0.25, but with a standard error of 0.05. I record these estimates in column 1 of Table A5.

I attempt to replicate these estimates in column 2 and achieve similar numbers for Card’s six-group occupation scheme (bottom row). In the remaining rows, I re-estimate the model for the four education delineations from Table 8 in the main text: (i) college graduates / non-graduates; (ii) at least one year of college / no college; (iii) high school dropouts / all others; (iv) four groups: dropouts, high school graduates, some college and college graduates. In the fifth row, I also study a classification with two imputed occupation groups: all those two-digit occupations with less than 40 percent college share in 1990, versus all those with more than 40 percent.\(^{26}\) I assign individuals probabilistically to these groups using the same multinomial logit procedure (conditioning on the same demographic characteristics) as for Card’s six group delineation in the final row. Looking at column 2, it appears that the choice of skill delineation makes no significant difference to the estimates. In column 3, I cluster the errors by state: the standard errors are now larger, but the difference is not dramatic.

Much of the action comes in column 4, when I exclude the mean demographic controls in \( X_{gr} \) from the right hand side. All the estimates of \( \delta^w \) are now negative, and they are statistically significant for the college graduate, college and two-group occupation schemes, with IV coefficients of -2.14, -0.45 and -0.47 respectively. The controls in \( X_{gr} \) absorb much of the (within-area) variation in the migration shock: a regression of the enclave instrument on the \( d_g \) and \( d_r \) fixed effects yields an R squared of 0.858, and including the controls raises the R squared to 0.928. Of course, these controls may be picking up important skill-specific shocks which I have neglected: the purpose of this exercise is merely to understand how our results can be reconciled.

Column 5 extends the geographical sample to all identifiable MSAs (raising the total from 175 to 320), and column 6 extends it to cover 49 additional regions consisting of the non-metro areas in each state\(^{27}\) (so 369 areas in total). The latter modification ensures the area sample is comprehensive of the US, similarly to the CZs I use in the main text. There may be good reason to exclude non-metro areas; but again, the purpose of this

\(^{25}\)Using his population growth dependent variable, this comes out as 1.25 - from which I subtract 1. See the final column of Table 4 of Card (2001).
\(^{26}\)As it happens, the occupational distribution in college share is strongly bipolar, and 40 percent is the natural dividing line.
\(^{27}\)Based on the allocation procedure described above, all of New Jersey is already classified as part of an MSA. The “49 additional regions” cover the remaining 49 states.
exercise is merely to reconcile our results. These sample extensions make the coefficients larger (more negative) for all skill delineations, and the IV estimates in column 6 are now statistically significant for all but the four-group education delineation.

In the final column, I replace the left and right hand side variables with $\hat{\lambda}_{gr,1990}^I$ and $\hat{\lambda}_{gr,1990}^F$ respectively, as defined by equations (22) and (23): i.e. log \( \frac{L_{gr,1990} - L_{gr,1990}^F}{L_{gr,1990}^I} \) and log \( \frac{L_{gr,1985} + L_{gr,1990}^I}{L_{gr,1985}} \). This makes a negligible difference to the results. The final column can now be compared to my longitudinal estimates in the main text (column 5 of Table 8): the results look similar. Just as with the education groups, moving to a finer occupation classification (i.e. from the penultimate to the final row) yields a smaller $\delta_1^w$ estimate: the discussion in Section 7.2 offers an intuition for this result.
### Tables and figures

#### Table 1: Descriptive statistics

<table>
<thead>
<tr>
<th></th>
<th>Means by decade</th>
<th>Percentiles</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1960s 1970s 1980s 1990s 2000s</td>
<td>5th 50th 95th Max</td>
</tr>
<tr>
<td>Foreign contrib: $\lambda_t$</td>
<td>0.020 0.033 0.047 0.065 0.060</td>
<td>0.000 0.007 0.057 0.211</td>
</tr>
<tr>
<td>Enclave shift-share, $m_{rt}$</td>
<td>0.016 0.025 0.038 0.056 0.053</td>
<td>0.000 0.007 0.054 0.289</td>
</tr>
<tr>
<td>Emp rate (end of decade)</td>
<td>0.624 0.659 0.707 0.694 0.665</td>
<td>0.553 0.648 0.752 0.810</td>
</tr>
<tr>
<td>Bartik shift-share, $b_{rt}$</td>
<td>0.173 0.227 0.142 0.095 0.056</td>
<td>-0.130 0.088 0.227 0.296</td>
</tr>
</tbody>
</table>

This table reports descriptive statistics for key variables of interest: population-weighted means by decade, and various percentiles of the full distribution. Employment rates are adjusted for local demographic composition.

#### Table 2: First stage for crowding out estimates

<table>
<thead>
<tr>
<th></th>
<th>Foreign contribution: $\lambda_t$</th>
<th>$\Delta \log \text{emp}$</th>
<th>Lagged log ER</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) (2) (3) (4) (5) (6) (7)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Current enclave, $m_{rt}$</td>
<td>0.919*** 1.229*** 1.173*** 1.069***</td>
<td>-0.233**</td>
<td>-0.022 0.475***</td>
</tr>
<tr>
<td>Lagged enclave, $m_{rt-1}$</td>
<td>-0.399*** -0.377*** (0.056) (0.053)</td>
<td>-0.640***</td>
<td>-0.139</td>
</tr>
<tr>
<td>Current Bartik, $b_{rt}$</td>
<td>0.092*** 0.078*** 0.121*** 0.097**</td>
<td>0.839***</td>
<td>-0.134* -0.156**</td>
</tr>
<tr>
<td>Lagged Bartik, $b_{rt-1}$</td>
<td>0.063*** 0.064*** 0.160*** 0.063</td>
<td>0.122*</td>
<td>0.371*** 0.373***</td>
</tr>
<tr>
<td>SW F-test: 2 endog vars</td>
<td>126.47 54.88 - - -</td>
<td>34.70 31.00</td>
<td></td>
</tr>
<tr>
<td>SW F-test: 3 endog vars</td>
<td>93.68 - - -</td>
<td>56.93 -</td>
<td></td>
</tr>
<tr>
<td>Amenity x yr controls</td>
<td>Yes Yes Yes Yes Yes Yes Yes Yes</td>
<td>Yes Yes Yes Yes</td>
<td></td>
</tr>
<tr>
<td>Geography</td>
<td>CZ CZ CZ State CZ CZ CZ CZ</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Year sample</td>
<td>60-10 60-10 70-10 60-10</td>
<td>60-10 60-10 60-10</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>3,610 3,610 2,888 245</td>
<td>3,610 3,610 3,610</td>
<td></td>
</tr>
</tbody>
</table>

This table reports first stage estimates corresponding to the crowding out specifications in Table 3. I report Sanderson-Windmeijer F-statistics which account for multiple endogenous variables, both for the unconditional crowding out specifications with two endogenous variables (i.e. $\lambda_t$ and the lagged employment rate) and the conditional specifications with three (as before, plus the current change in log employment). All specifications control for year effects and the amenity variables (interacted with year effects) described in Section 3. I have marked in bold the effect of each instrument on its corresponding endogenous variable, i.e., where one should theoretically expect to see positive effects. Errors are clustered by state, and robust standard errors are reported in parentheses. Each observation is weighted by the lagged local population share. *** p<0.01, ** p<0.05, * p<0.1.
Table 3: Estimates of crowding out across CZs and states

<table>
<thead>
<tr>
<th></th>
<th>Conditional</th>
<th>Unconditional</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$\lambda_{rt}$</td>
<td>$\lambda_{rt}$</td>
<td>$\lambda_{rt}$</td>
<td>$\lambda_{rt}$</td>
<td>$\lambda_{rt}$</td>
<td>$\lambda_{rt}$</td>
<td>$\lambda_{rt-1}$</td>
<td>$\lambda_{rt}$</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
<td>(8)</td>
</tr>
<tr>
<td>Foreign contrib:</td>
<td>$\hat{\lambda}_{Frt}$</td>
<td>-0.883***</td>
<td>-0.913***</td>
<td>-0.761***</td>
<td>-1.096***</td>
<td>-1.109***</td>
<td>-0.787***</td>
<td>-0.245</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.048)</td>
<td>(0.065)</td>
<td>(0.200)</td>
<td>(0.130)</td>
<td>(0.153)</td>
<td>(0.167)</td>
<td>(0.228)</td>
</tr>
<tr>
<td>$\Delta$ log emp</td>
<td>$\hat{\lambda}_{rt}$</td>
<td>0.882***</td>
<td>0.743***</td>
<td>0.520***</td>
<td>0.831***</td>
<td>0.833***</td>
<td>0.524***</td>
<td>-0.071</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.017)</td>
<td>(0.043)</td>
<td>(0.072)</td>
<td>(0.207)</td>
<td>(0.221)</td>
<td>(0.119)</td>
<td>(0.168)</td>
</tr>
<tr>
<td>Lagged log ER</td>
<td>$\hat{\lambda}_{rt}$</td>
<td>0.251***</td>
<td>0.556***</td>
<td>0.646***</td>
<td>0.677***</td>
<td>0.679***</td>
<td>0.524***</td>
<td>-0.071</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.021)</td>
<td>(0.105)</td>
<td>(0.109)</td>
<td>(0.099)</td>
<td>(0.096)</td>
<td>(0.119)</td>
<td>(0.168)</td>
</tr>
<tr>
<td>Current Bartik,</td>
<td>$\hat{\lambda}_{rt}$</td>
<td>0.290***</td>
<td>0.907***</td>
<td>0.300***</td>
<td>0.016</td>
<td>0.016</td>
<td>0.016</td>
<td>0.016</td>
</tr>
<tr>
<td>$b_{rt}$</td>
<td></td>
<td></td>
<td>(0.060)</td>
<td>(0.103)</td>
<td>(0.105)</td>
<td>(0.161)</td>
<td>(0.124)</td>
<td>(0.167)</td>
</tr>
<tr>
<td>Lagged Bartik,</td>
<td>$\hat{\lambda}_{rt}$</td>
<td>-0.388***</td>
<td>-0.984***</td>
<td>-0.469**</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$b_{rt-1}$</td>
<td></td>
<td></td>
<td>(0.161)</td>
<td>(0.124)</td>
<td>(0.167)</td>
<td>(0.223)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lagged enclave,</td>
<td>$\hat{\lambda}_{rt}$</td>
<td>0.016</td>
<td>-0.388***</td>
<td>-0.984***</td>
<td>-0.469**</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$m_{rt-1}$</td>
<td></td>
<td></td>
<td>(0.161)</td>
<td>(0.124)</td>
<td>(0.167)</td>
<td>(0.223)</td>
<td></td>
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</tr>
<tr>
<td>Specification</td>
<td>OLS</td>
<td>IV</td>
<td>OLS</td>
<td>IV</td>
<td>IV</td>
<td>IV</td>
<td>IV</td>
<td>IV</td>
</tr>
<tr>
<td>Instruments</td>
<td>$m_{rt}$, $b_{rt}$, $b_{rt-1}$</td>
<td>-</td>
<td>$m_{rt}$, $b_{rt-1}$</td>
<td>$m_{rt}$, $b_{rt-1}$</td>
<td>$m_{rt}$</td>
<td>$m_{rt}$</td>
<td>$m_{rt}$</td>
<td></td>
</tr>
<tr>
<td>Geography</td>
<td>CZ</td>
<td>CZ</td>
<td>CZ</td>
<td>CZ</td>
<td>CZ</td>
<td>CZ</td>
<td>State</td>
<td></td>
</tr>
<tr>
<td>Year sample</td>
<td>60-10</td>
<td>60-10</td>
<td>60-10</td>
<td>60-10</td>
<td>60-10</td>
<td>60-10</td>
<td>70-10</td>
<td>60-10</td>
</tr>
<tr>
<td>Observations</td>
<td>3,610</td>
<td>3,610</td>
<td>3,610</td>
<td>3,610</td>
<td>3,610</td>
<td>3,610</td>
<td>2,888</td>
<td>245</td>
</tr>
</tbody>
</table>

Columns 1-2 report OLS and IV estimates of the conditional crowding out specification (across CZs), (19), and columns 3-7 report the unconditional specification, (20). There are (up to) three endogenous variables: the foreign contribution to population growth, $\hat{\lambda}_{Frt}$, the log employment change, and the lagged log employment rate. The corresponding instruments are the enclave shift-share $\hat{\mu}_{Frt}$ and the current and lagged Bartiks. Column 7 replaces the dependent variable with its lag, so it omits the initial decade. Column 8 replicates column 6 for state-level data. Errors are clustered by state, and robust standard errors are in parentheses. Observations are weighted by lagged local population share. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. 

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Table 4: Robustness of unconditional IV crowd-out to controls and decadal sample

<table>
<thead>
<tr>
<th></th>
<th>1960s</th>
<th>1970s</th>
<th>1980s</th>
<th>1990s</th>
<th>2000s</th>
<th>All years</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>Controlling for year effects only</td>
<td>0.273</td>
<td>-0.726</td>
<td>-0.041</td>
<td>-0.943***</td>
<td>-0.538**</td>
<td>-0.526**</td>
</tr>
<tr>
<td></td>
<td>(0.944)</td>
<td>(0.635)</td>
<td>(0.250)</td>
<td>(0.225)</td>
<td>(0.252)</td>
<td>(0.246)</td>
</tr>
<tr>
<td>+ Current Bartik</td>
<td>-0.745</td>
<td>-0.268</td>
<td>-0.455</td>
<td>-0.921***</td>
<td>-0.572**</td>
<td>-0.689***</td>
</tr>
<tr>
<td></td>
<td>(1.134)</td>
<td>(0.466)</td>
<td>(0.350)</td>
<td>(0.260)</td>
<td>(0.251)</td>
<td>(0.217)</td>
</tr>
<tr>
<td>+ Lagged log ER (instrumented)</td>
<td>-0.709</td>
<td>-0.238</td>
<td>-0.744*</td>
<td>-0.327</td>
<td>-0.564**</td>
<td>-0.753***</td>
</tr>
<tr>
<td></td>
<td>(1.139)</td>
<td>(0.318)</td>
<td>(0.441)</td>
<td>(0.421)</td>
<td>(0.246)</td>
<td>(0.239)</td>
</tr>
<tr>
<td>+ Climate controls</td>
<td>-1.967**</td>
<td>-2.088***</td>
<td>-0.973***</td>
<td>-1.343***</td>
<td>-0.845***</td>
<td>-1.396***</td>
</tr>
<tr>
<td></td>
<td>(0.908)</td>
<td>(0.467)</td>
<td>(0.302)</td>
<td>(0.256)</td>
<td>(0.180)</td>
<td>(0.192)</td>
</tr>
<tr>
<td>+ Coastline dummy</td>
<td>-2.032**</td>
<td>-2.087***</td>
<td>-0.865**</td>
<td>-1.119***</td>
<td>-0.637***</td>
<td>-1.263***</td>
</tr>
<tr>
<td></td>
<td>(0.947)</td>
<td>(0.473)</td>
<td>(0.350)</td>
<td>(0.251)</td>
<td>(0.189)</td>
<td>(0.228)</td>
</tr>
<tr>
<td>+ Log pop density 1900</td>
<td>-1.657***</td>
<td>-1.797***</td>
<td>-0.726***</td>
<td>-1.100***</td>
<td>-0.558***</td>
<td>-1.107***</td>
</tr>
<tr>
<td></td>
<td>(0.610)</td>
<td>(0.220)</td>
<td>(0.201)</td>
<td>(0.276)</td>
<td>(0.215)</td>
<td>(0.256)</td>
</tr>
<tr>
<td>+ Log distance to closest CZ</td>
<td>-1.626**</td>
<td>-1.917***</td>
<td>-0.877***</td>
<td>-1.203***</td>
<td>-0.638***</td>
<td>-1.137***</td>
</tr>
<tr>
<td></td>
<td>(0.634)</td>
<td>(0.197)</td>
<td>(0.188)</td>
<td>(0.298)</td>
<td>(0.236)</td>
<td>(0.251)</td>
</tr>
<tr>
<td>+ Amenity×yr effects (i.e. all controls)</td>
<td>-1.626**</td>
<td>-1.917***</td>
<td>-0.877***</td>
<td>-1.203***</td>
<td>-0.638***</td>
<td>-1.096***</td>
</tr>
<tr>
<td></td>
<td>(0.634)</td>
<td>(0.197)</td>
<td>(0.188)</td>
<td>(0.298)</td>
<td>(0.236)</td>
<td>(0.130)</td>
</tr>
</tbody>
</table>

Native contribution only (all controls) | -0.926* | -1.873*** | -0.751*** | -0.870*** | 0.101 | -0.715*** |
|                  | (0.476) | (0.190) | (0.176) | (0.302) | (0.194) | (0.127) |

Observations | 722  | 722  | 722  | 722  | 722  | 3,610  |

This table tests the robustness of my IV unconditional crowding out estimate $\delta_{1}$ (in column 4 of Table 3) to the choice of controls and decadal sample. The first five columns estimate $\delta_{1}$ separately for each decade, and the final column pools all decades together. Moving down the rows of the table, I show how my $\delta_{1}$ estimate changes as progressively more controls are included. All specifications include the foreign contribution $\lambda_{F}$ (instrumented with the enclave shift-share, $m_{rt}$) and year effects. The second row controls additionally for a current Bartik, $b_{rt}$; the third row includes the (endogenous) lagged employment rate (together with its lagged Bartik instrument, $b_{rt-1}$); and the various amenities are then progressively added - until the penultimate row, which includes the full set of controls I use in Table 3. The final row replaces the dependent variable with the contribution of natives alone, $\lambda_{I,N}$, using the full set of controls. Errors are clustered by state, and robust standard errors are reported in parentheses. Each observation is weighted by the lagged local population share. *** p<0.01, ** p<0.05, * p<0.1.
Table 5: IV effects of foreign inflows on local labor market outcomes

<table>
<thead>
<tr>
<th></th>
<th>Employment rates</th>
<th>Wages</th>
<th>Housing costs</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Native Current (1)</td>
<td>Native Current (5)</td>
<td>Migrant Current (4)</td>
</tr>
<tr>
<td></td>
<td>(2)</td>
<td>(3)</td>
<td>(6)</td>
</tr>
<tr>
<td>Foreign contrib: $\hat{\lambda}_F$</td>
<td>-0.210***</td>
<td>-0.190**</td>
<td>-0.350***</td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.092)</td>
<td>(0.075)</td>
</tr>
<tr>
<td>Lagged log ER</td>
<td>-0.411***</td>
<td>-0.414***</td>
<td>-0.469**</td>
</tr>
<tr>
<td></td>
<td>(0.087)</td>
<td>(0.091)</td>
<td>(0.204)</td>
</tr>
<tr>
<td>Current Bartik</td>
<td>0.259***</td>
<td>0.255***</td>
<td>0.333***</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
<td>(0.034)</td>
<td>(0.038)</td>
</tr>
<tr>
<td>Lagged Bartik</td>
<td>-0.144***</td>
<td>0.069*</td>
<td>-0.177**</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.036)</td>
<td>(0.079)</td>
</tr>
<tr>
<td>Instruments</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Amenity×yr controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year sample</td>
<td>60-10</td>
<td>60-10</td>
<td>60-10</td>
</tr>
</tbody>
</table>

This table reports estimates of the unconditional crowding out equation (20), but with the dependent variable replaced with various labor market outcomes: the change in the log (composition-adjusted) employment rate and the mean residualized wage, separately for natives and migrants, and also residualized housing rents and prices. See the notes under Table 3 for further details about the empirical specification, and see Table 2 for the first stage estimates. All specifications control for year effects and the amenity variables (interacted with year effects) described in Section 3. The observation count is a little smaller in column 5: I am unable to compute composition-adjusted migrant employment rates for 11 small CZs in the 1960s. Errors are clustered by state, and robust standard errors are reported in parentheses. Each observation is weighted by the lagged local population share. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: IV effects of foreign inflows by education

<table>
<thead>
<tr>
<th></th>
<th>$\Delta$ log pop</th>
<th>Foreign contrib: $\hat{\lambda}_F$</th>
<th>Residual contrib: $\hat{\lambda}_{gF}$</th>
<th>Employment rates</th>
<th>Wages</th>
<th>Housing costs</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Residual contrib: $\hat{\lambda}_{gF}$</td>
<td>Residual contrib: $\hat{\lambda}_{gF}$</td>
<td>Residual contrib: $\hat{\lambda}_{gF}$</td>
<td>Residual contrib: $\hat{\lambda}_{gF}$</td>
<td>Residual contrib: $\hat{\lambda}_{gF}$</td>
<td>Residual contrib: $\hat{\lambda}_{gF}$</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>College graduates</td>
<td>-0.261*</td>
<td>0.816***</td>
<td>-0.977***</td>
<td>-0.023</td>
<td>-0.110**</td>
<td>0.167**</td>
</tr>
<tr>
<td></td>
<td>(0.159)</td>
<td>(0.041)</td>
<td>(0.184)</td>
<td>(0.020)</td>
<td>(0.047)</td>
<td>(0.077)</td>
</tr>
<tr>
<td>Non-graduates</td>
<td>-0.145</td>
<td>1.033***</td>
<td>-1.274***</td>
<td>-0.357***</td>
<td>-0.253***</td>
<td>-0.040</td>
</tr>
<tr>
<td></td>
<td>(0.135)</td>
<td>(0.031)</td>
<td>(0.136)</td>
<td>(0.070)</td>
<td>(0.076)</td>
<td>(0.130)</td>
</tr>
</tbody>
</table>

This table reports IV estimates of $\hat{\delta}_u$ in (21), i.e. the coefficient on CZ-level foreign inflows $\hat{\lambda}_F$ (instrumented with the enclave shift-share), estimated for a range of outcomes separately for college graduates and non-graduates. All specifications include 3,610 observations (722 CZs over five decadal periods) with the exception of some migrant-specific outcomes: in some small CZs, the sample of some census extracts does not include migrants in all education cells. The right hand side of the estimating equation is identical to that of column 2 (Panel A) of Table 3. Throughout, I control for a second endogenous variable: the lagged log employment rate, instrumented with the lagged Bartik. I also control for the current Bartik, year effects and the amenity variables (interacted with year effects) described in Section 3. Errors are clustered by state, and robust standard errors are reported in parentheses. Each observation is weighted by the lagged local population share. *** p<0.01, ** p<0.05, * p<0.1.
### Table 7: Conditional and unconditional crowd-out by decade

<table>
<thead>
<tr>
<th></th>
<th>1960s</th>
<th>1970s</th>
<th>1980s</th>
<th>1990s</th>
<th>2000s</th>
<th>All years</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td><strong>Unconditional crowd-out: (^\delta^u_1)</strong></td>
<td>-1.626**</td>
<td>-1.917***</td>
<td>-0.877***</td>
<td>-1.203***</td>
<td>-0.638***</td>
<td>-1.096**</td>
</tr>
<tr>
<td></td>
<td>(0.634)</td>
<td>(0.197)</td>
<td>(0.188)</td>
<td>(0.298)</td>
<td>(0.236)</td>
<td>(0.130)</td>
</tr>
<tr>
<td><strong>Conditional crowd-out: (^\delta^c_1)</strong></td>
<td>-0.631**</td>
<td>-1.158***</td>
<td>-0.689***</td>
<td>23.645</td>
<td>-0.807***</td>
<td>-0.913***</td>
</tr>
<tr>
<td></td>
<td>(0.261)</td>
<td>(0.128)</td>
<td>(0.241)</td>
<td>(1886.724)</td>
<td>(0.199)</td>
<td>(0.065)</td>
</tr>
<tr>
<td>Observations</td>
<td>722</td>
<td>722</td>
<td>722</td>
<td>722</td>
<td>722</td>
<td>3,610</td>
</tr>
</tbody>
</table>

The first row offers estimates of unconditional crowding out, i.e. equation (20), separately by decade. (This is identical to the penultimate row of Table 4.) The second row replicates this exercise for conditional crowding out, i.e. equation (19). Errors are clustered by state, and robust standard errors are reported in parentheses. Each observation is weighted by the lagged local population share. *** p<0.01, ** p<0.05, * p<0.1.

### Table 8: Within-CZ IV estimates of \(^\delta^w_1\)

<table>
<thead>
<tr>
<th></th>
<th>Pooled cross-sections</th>
<th>Longitudinal</th>
<th>Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>First stage coefficient</td>
<td>Full residual contrib: (^\hat{\lambda}_{grt})</td>
<td>Natives only: (^\hat{\lambda}_{grt}^N)</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>CG / &lt; CG</td>
<td>0.530***</td>
<td>1.502***</td>
<td>1.638***</td>
</tr>
<tr>
<td></td>
<td>(0.067)</td>
<td>(0.295)</td>
<td>(0.369)</td>
</tr>
<tr>
<td>Coll / &lt; Coll</td>
<td>0.662***</td>
<td>1.040***</td>
<td>1.046***</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.132)</td>
<td>(0.162)</td>
</tr>
<tr>
<td>HSD / &gt; HSD</td>
<td>0.785***</td>
<td>0.980***</td>
<td>1.410***</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.088)</td>
<td>(0.261)</td>
</tr>
<tr>
<td>4 edu groups</td>
<td>0.744***</td>
<td>1.330***</td>
<td>1.521***</td>
</tr>
<tr>
<td></td>
<td>(0.035)</td>
<td>(0.095)</td>
<td>(0.269)</td>
</tr>
</tbody>
</table>

This table reports within-area estimates of \(^\delta^w_1\) based on equation (35). The first three columns are based on pooled decadal cross-sections between 1970 and 2000, and columns 4-6 exploit longitudinal information on changes in residence over 1975-1980, 1985-1990 and 1995-2000. Columns 1 and 4 report the first stage coefficients on the education-specific enclave shift-share, \(^\hat{\mu}_{grt}\). And the remaining columns report IV estimates of \(^\delta^w_1\), both for the full internal contribution (natives and old migrants) and for natives only. The four rows offer estimates for different education-based skill delineations: (i) college graduates / no graduates, (ii) at least one year of college / no college, (iii) high school dropouts / all others, and (iv) four groups: high school dropouts, high school graduates, some college and college graduates. All specifications control for both CZ-year and education-year interacted fixed effects. Errors are clustered by state, and robust standard errors are reported in parentheses. Each observation is weighted by the lagged cell-specific population share. *** p<0.01, ** p<0.05, * p<0.1.
Table 9: Within-state pooled cross-section IV estimates of $\delta^p_1$: Cohort effects

<table>
<thead>
<tr>
<th></th>
<th>First stage coefficient</th>
<th>Native response</th>
<th>Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>CG / &lt; CG</td>
<td>0.596***</td>
<td>1.618**</td>
<td>2.245***</td>
</tr>
<tr>
<td></td>
<td>(0.102)</td>
<td>(0.663)</td>
<td>(0.423)</td>
</tr>
<tr>
<td>Coll / &lt; Coll</td>
<td>0.820***</td>
<td>1.212***</td>
<td>2.272***</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.146)</td>
<td>(0.206)</td>
</tr>
<tr>
<td>HSD / &gt; HSD</td>
<td>0.970***</td>
<td>1.435***</td>
<td>1.620***</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.344)</td>
<td>(0.248)</td>
</tr>
<tr>
<td>4 edu groups</td>
<td>0.932***</td>
<td>1.484***</td>
<td>1.768***</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.246)</td>
<td>(0.195)</td>
</tr>
</tbody>
</table>

This table explores the presence of cohort effects in the pooled cross-section IV estimates of $\delta^p_1$ in equation (35), using a range of education-based skill delineations. Columns 1 reproduces the first stage estimates of Table 8 (column 1) but for state-level data (more specifically, the 48 states of the continental US plus the District of Columbia). Column 2 estimates the IV effect of education-specific foreign inflows $\lambda^F_{grit}$ on the native contribution to education group $g$ population growth in state $r$: i.e., the state-level version of column 3 of Table 8. Column 3 replaces the dependent variable with the contribution of natives to education-specific population growth among those born (rather than residing) in state $r$. All specifications control for both state-year and education-year interacted fixed effects. Errors are clustered by state, and robust standard errors are reported in parentheses. Each observation is weighted by the lagged cell-specific population share. *** p<0.01, ** p<0.05, * p<0.1.

Table A1: Contribution of inflows and outflows to crowding out across CZs

<table>
<thead>
<tr>
<th></th>
<th>OLS</th>
<th>IV</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Net flow $\lambda^F_{grit}$</td>
<td>Inflow $\lambda^I_{grit}$</td>
</tr>
<tr>
<td>Foreign contrib</td>
<td>$\lambda^F_{grit}$</td>
<td>-0.500</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.319)</td>
</tr>
<tr>
<td>Log ER lagged 10 yrs</td>
<td>$\lambda^F_{grit}$</td>
<td>0.190***</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.047)</td>
</tr>
<tr>
<td>Current decadal Bartik</td>
<td>$\lambda^F_{grit}$</td>
<td>0.286***</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.126)</td>
</tr>
<tr>
<td>SW F-stat for foreign contrib</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>SW F-stat for lagged ER</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Observations</td>
<td>2,166</td>
<td>2,166</td>
</tr>
</tbody>
</table>

This table offers OLS and IV estimates of the 5-year unconditional crowding out effect, based on equation (A20), and disaggregates these into the (approximate) contributions from internal inflows and outflows. Variable definitions and data sources are given in Section C. The flow data covers the intervals 1965-70, 1975-80, 1985-90 and 1995-2000. The 5-year foreign contribution is instrumented with a 5-year enclave shift-share in the IV specification, based on settlement patterns five years previously. The log employment rate, lagged ten years (e.g. measured at 1960 for the 1965-70 flow interval), is instrumented using a lagged decadal Bartik. I also control for a current decadal Bartik, year effects and the amenity variables (interacted with year effects) described in Section 3. Errors are clustered by state, and robust standard errors are reported in parentheses. Each observation is weighted by the 5-year lagged local population share. *** p<0.01, ** p<0.05, * p<0.1.
Table A2: Robustness of crowd-out estimates to sample and specification

<table>
<thead>
<tr>
<th></th>
<th>( \lambda_{11} )</th>
<th>( \lambda_{12} )</th>
<th>( \lambda_{13} )</th>
<th>( \lambda_{14} )</th>
<th>( \lambda_{15} )</th>
<th>( \lambda_{16} )</th>
<th>( \lambda_{17} )</th>
<th>( \lambda_{18} )</th>
<th>( \lambda_{19} )</th>
<th>( \lambda_{20} )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Panel A: Weighted estimates</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \lambda_{11} )</td>
<td>-1.096*** (0.130)</td>
<td>-1.068*** (0.131)</td>
<td>-0.965*** (0.303)</td>
<td>-1.393*** (0.262)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \lambda_{12} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>-1.090*** (0.143)</td>
<td>-1.077*** (0.163)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \lambda_{13} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>-0.228*** (0.085)</td>
<td>-0.971*** (0.273)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \lambda_{14} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.831*** (0.207)</td>
<td>0.550*** (0.233)</td>
</tr>
<tr>
<td>( \lambda_{15} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.753*** (0.234)</td>
<td>0.943*** (0.172)</td>
</tr>
<tr>
<td>( \lambda_{16} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.860*** (0.227)</td>
<td>0.688*** (0.203)</td>
</tr>
<tr>
<td>( \lambda_{17} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>2.10^{10} (7\times10^5)</td>
<td>2.10^{10} (7\times10^5)</td>
</tr>
<tr>
<td>( \lambda_{18} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1.269*** (2\times10^5)</td>
<td>1.269*** (2\times10^5)</td>
</tr>
<tr>
<td>( \lambda_{19} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.615*** (0.086)</td>
<td>0.580*** (0.080)</td>
</tr>
<tr>
<td>( \lambda_{20} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.162*** (0.083)</td>
<td></td>
</tr>
<tr>
<td>SW F-stats for:</td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Foreign inflow</td>
<td>126.47</td>
<td>144.35</td>
<td>45.01</td>
<td>40.26</td>
<td>116.05</td>
<td>91.63</td>
<td>262.07</td>
<td>92.07</td>
<td>58.33</td>
<td>40.30</td>
</tr>
<tr>
<td>Lagged log ER</td>
<td>34.70</td>
<td>14.63</td>
<td>28.98</td>
<td>45.18</td>
<td>34.99</td>
<td>40.63</td>
<td>39.10</td>
<td>49.08</td>
<td>45.51</td>
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<tr>
<td>Panel B: Unweighted estimates</td>
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<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \lambda_{11} )</td>
<td>-0.940*** (0.266)</td>
<td>-0.776*** (0.251)</td>
<td>-0.820*** (0.463)</td>
<td>-1.538*** (0.458)</td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>( \lambda_{12} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>-1.005*** (0.285)</td>
<td>-1.210*** (0.293)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \lambda_{13} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>-0.147 (0.151)</td>
<td>-1.180*** (0.273)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \lambda_{14} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.578*** (0.183)</td>
<td>0.281*** (0.079)</td>
</tr>
<tr>
<td>( \lambda_{15} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.502*** (0.185)</td>
<td>0.664*** (0.188)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \lambda_{16} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.667*** (0.218)</td>
<td>0.697*** (0.227)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \lambda_{17} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>6\times10^5 (2\times10^5)</td>
<td>3\times10^4 (2\times10^5)</td>
<td>0.654*** (1\times10^4)</td>
<td></td>
</tr>
<tr>
<td>( \lambda_{18} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.409*** (0.074)</td>
<td>0.410*** (0.070)</td>
</tr>
<tr>
<td>( \lambda_{19} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.117*** (0.042)</td>
<td></td>
</tr>
<tr>
<td>( \lambda_{20} )</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.210 (0.042)</td>
<td>0.166 (0.042)</td>
</tr>
<tr>
<td>SW F-stats for:</td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Foreign inflow</td>
<td>51.13</td>
<td>64.01</td>
<td>43.11</td>
<td>50.09</td>
<td>42.80</td>
<td>14.59</td>
<td>121.72</td>
<td>42.95</td>
<td>6.27</td>
<td>5.61</td>
</tr>
<tr>
<td>Lagged log ER</td>
<td>25.77</td>
<td>34.57</td>
<td>21.04</td>
<td>32.27</td>
<td>26.03</td>
<td>25.61</td>
<td>30.32</td>
<td>52.55</td>
<td>53.13</td>
<td>-</td>
</tr>
<tr>
<td>Instruments</td>
<td>( m_{11}, b_{11-1} )</td>
<td>( m_{12}, b_{12-1} )</td>
<td>( m_{13}, b_{13-1} )</td>
<td>( m_{14}, b_{14-1} )</td>
<td>( \Lambda_{11} )</td>
<td>( \Lambda_{12} )</td>
<td>( \Lambda_{13} )</td>
<td>( \Lambda_{14} )</td>
<td>( m_{15}, b_{15-1} )</td>
<td>( m_{16}, b_{16-1} )</td>
</tr>
<tr>
<td>Amen\times yr controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>CZ fixed effects</td>
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<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Sample</td>
<td>Basic</td>
<td>Large CZs</td>
<td>Low ( m_{rt} )</td>
<td>Basic</td>
<td>Basic</td>
<td>Basic</td>
<td>Basic</td>
<td>Basic</td>
<td>Basic</td>
<td>Basic</td>
</tr>
</tbody>
</table>

This table offers alternative estimates of \( \delta \) in equation (20), implementing different IV strategies and variable specifications. Panel A reports estimates weighted by the lagged population share, and Panel B reports unweighted estimates. Columns 1-7 in each panel do not control for CZ fixed effects, while columns 8-10 do. The dependent variable in each specification is reported in the field above the column number. The instruments I use in each specification are reported at the bottom of the table. The Sandersen-Windmeijer (2016) F-statistics account for multiple endogenous variables. Column 2 restricts the sample to CZs with at least 100,000 individuals aged 16-64 in 1960, and column 3 restricts it to observations with enclave shift-share values below 0.1. All specifications control for the lagged employment rate (always instrumented with the lagged Bartik Bartik, year effects and the amenity variables (interacted with year effects) described in Section 3. Errors are clustered by state, and robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
### Table A3: First stage for employment response

<table>
<thead>
<tr>
<th></th>
<th>Log pop change: $\Delta \ln r$</th>
<th>Foreign contribution: $\hat{\lambda}^f_{it}$</th>
<th>Internal contribution: $\hat{\lambda}^i_{it}$</th>
<th>Lagged log ER</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Current enclave $m_{rt}$</td>
<td>-0.073</td>
<td>0.922***</td>
<td>-1.030***</td>
<td>-0.022</td>
</tr>
<tr>
<td></td>
<td>(0.139)</td>
<td>(0.087)</td>
<td>(0.174)</td>
<td>(0.120)</td>
</tr>
<tr>
<td>Max Jan temp</td>
<td>0.333***</td>
<td>0.038**</td>
<td>0.314***</td>
<td>-0.141***</td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.019)</td>
<td>(0.062)</td>
<td>(0.047)</td>
</tr>
<tr>
<td>Current Bartik $b_{rt}$</td>
<td>0.546***</td>
<td>0.084***</td>
<td>0.477***</td>
<td>-0.134*</td>
</tr>
<tr>
<td></td>
<td>(0.114)</td>
<td>(0.023)</td>
<td>(0.124)</td>
<td>-0.122*</td>
</tr>
<tr>
<td>Lagged Bartik $b_{t-1}$</td>
<td>0.266***</td>
<td>0.283***</td>
<td>0.237***</td>
<td>0.371***</td>
</tr>
<tr>
<td></td>
<td>(0.060)</td>
<td>(0.067)</td>
<td>(0.078)</td>
<td>(0.063)</td>
</tr>
</tbody>
</table>

This table reports first stage estimates corresponding to the employment growth specifications in Table A4. I report Sander-Windmeijer F-statistics which account for multiple endogenous variables. All specifications control for year effects and the amenity variables (interacted with year effects) described in Section 3. However, I only include the maximum January temperature control (and year interactions) in those specifications where it does not serve as an instrument. I have marked in bold the effect of each instrument on its corresponding endogenous variable, i.e. where one should theoretically expect to see positive effects. Errors are clustered by state, and robust standard errors are reported in parentheses. Each observation is weighted by the lagged local population share. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

### Table A4: Estimates of employment response

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<th></th>
<th>OLS</th>
<th>IV</th>
</tr>
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<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>$\Delta \text{log pop}$</td>
<td>1.025***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td></td>
</tr>
<tr>
<td>Foreign contrib: $\hat{\lambda}^f_{it}$</td>
<td>0.870***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td></td>
</tr>
<tr>
<td>Internal contrib: $\hat{\lambda}^i_{it}$</td>
<td>0.994***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td></td>
</tr>
<tr>
<td>Lagged log ER</td>
<td>-0.218***</td>
<td>-0.216***</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.023)</td>
</tr>
<tr>
<td>Current Bartik</td>
<td>0.143***</td>
<td>0.182***</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.020)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Instruments</th>
<th></th>
<th>$m_{rt}$, $b_{t-1}$</th>
<th>Jan temp, $b_{t-1}$</th>
<th>$m_{rt}$, Jan temp, $b_{t-1}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Amenity $\times$ yr controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Max temp $\times$ yr controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Year sample</td>
<td>60-10</td>
<td>60-10</td>
<td>60-10</td>
<td>60-10</td>
</tr>
<tr>
<td>Observations</td>
<td>3,610</td>
<td>3,610</td>
<td>3,610</td>
<td>3,610</td>
</tr>
</tbody>
</table>

This table reports OLS and IV estimates for models of local employment growth, i.e. equations (A34) and (A35). All specifications control for year effects and the amenity variables (interacted with year effects) described in Section 3. However, I only include the maximum January temperature control (and year interactions) in those specifications where it does not serve as an instrument. Errors are clustered by state, and robust standard errors are reported in parentheses. Each observation is weighted by the lagged local population share. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. 58
Table A5: Reconciliation with 1985-1990 within-area estimates from Card (2001)

<table>
<thead>
<tr>
<th>Panel A: OLS</th>
<th>Card (2001): 175 MSAs, weighted</th>
<th>Replication with errors clustered by state</th>
<th>... excluding demographic controls remaining full area sample with ( \hat{\lambda}<em>{I</em>{1990}} ) and RHS with ( \hat{\lambda}<em>{F</em>{1990}} )</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>CG / &lt; CG</td>
<td>-0.153</td>
<td>-0.153</td>
<td>-1.948***</td>
</tr>
<tr>
<td></td>
<td>(0.441)</td>
<td>(0.572)</td>
<td>(0.549)</td>
</tr>
<tr>
<td>Coll / &lt; Coll</td>
<td>0.212</td>
<td>0.212</td>
<td>-0.346***</td>
</tr>
<tr>
<td></td>
<td>(0.172)</td>
<td>(0.248)</td>
<td>(0.111)</td>
</tr>
<tr>
<td>HSD / &gt; HSD</td>
<td>0.118</td>
<td>0.118</td>
<td>-0.084</td>
</tr>
<tr>
<td></td>
<td>(0.101)</td>
<td>(0.161)</td>
<td>(0.090)</td>
</tr>
<tr>
<td>4 edu groups</td>
<td>-0.162</td>
<td>0.162</td>
<td>-0.117</td>
</tr>
<tr>
<td></td>
<td>(0.103)</td>
<td>(0.097)</td>
<td>(0.076)</td>
</tr>
<tr>
<td>2 occup groups</td>
<td>-0.106</td>
<td>0.106</td>
<td>-0.486***</td>
</tr>
<tr>
<td></td>
<td>(0.185)</td>
<td>(0.248)</td>
<td>(0.145)</td>
</tr>
<tr>
<td>6 occup groups</td>
<td>0.25***</td>
<td>0.214***</td>
<td>-0.071</td>
</tr>
<tr>
<td></td>
<td>(0.04)</td>
<td>(0.098)</td>
<td>(0.046)</td>
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Panel B: IV

<table>
<thead>
<tr>
<th></th>
<th>Card (2001): 175 MSAs, weighted</th>
<th>Replication with errors clustered by state</th>
<th>... excluding demographic controls remaining full area sample with ( \hat{\lambda}<em>{I</em>{1990}} ) and RHS with ( \hat{\lambda}<em>{F</em>{1990}} )</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>CG / &lt; CG</td>
<td>0.563</td>
<td>0.563</td>
<td>-2.143***</td>
</tr>
<tr>
<td></td>
<td>(1.290)</td>
<td>(2.912)</td>
<td>(0.750)</td>
</tr>
<tr>
<td>Coll / &lt; Coll</td>
<td>0.389***</td>
<td>0.389</td>
<td>-0.449***</td>
</tr>
<tr>
<td></td>
<td>(0.144)</td>
<td>(0.254)</td>
<td>(0.132)</td>
</tr>
<tr>
<td>HSD / &gt; HSD</td>
<td>0.297***</td>
<td>0.297***</td>
<td>-0.059</td>
</tr>
<tr>
<td></td>
<td>(0.080)</td>
<td>(0.133)</td>
<td>(0.073)</td>
</tr>
<tr>
<td>4 edu groups</td>
<td>0.484***</td>
<td>0.484***</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.117)</td>
<td>(0.118)</td>
<td>(0.077)</td>
</tr>
<tr>
<td>2 occup groups</td>
<td>0.244*</td>
<td>0.244</td>
<td>-0.469***</td>
</tr>
<tr>
<td></td>
<td>(0.139)</td>
<td>(0.298)</td>
<td>(0.100)</td>
</tr>
<tr>
<td>6 occup groups</td>
<td>0.25***</td>
<td>0.255***</td>
<td>-0.054</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.115)</td>
<td>(0.043)</td>
</tr>
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This table offers a reconciliation with Card’s (2001) within-area estimates of geographical crowd-out, based on equation (A40). Card’s OLS and IV estimates of \( \delta_w \) (for his six-group imputed occupation scheme) are presented in column 1. These are taken from Table 4 of his paper, based on the 175 largest MSAs of the 1990 census extract, with observations weighted by cell populations. (Card reports his estimates as the effect on aggregate population growth within the cell, but I subtract one from his numbers for comparability with my specification; see Peri and Sparber, 2011.) I attempt to replicate his results in column 2. In columns 3, 1 cluster standard errors by state. Column 4 excludes the demographic controls from the regression. Column 5 extends the geographical sample to all identifiable MSAs (raising the total to 320), and column 6 extends it to cover 49 additional regions consisting of the non-metro areas in each state (so 369 areas in total). Finally, column 7 replaces the left hand side variable with \( \hat{\lambda}_{I_{1990}} \) and the right hand side variable with \( \hat{\lambda}_{F_{1990}} \). I present estimates for both Card’s six-group occupation scheme and the other skill delineations described in Appendix F. *** p<0.01, ** p<0.05, * p<0.1.
Undercoverage bias: \( \pi \)

Employment elasticity: \( \eta \)

Figure 1: Implied relationship between \( \pi \) and \( \eta \)

This figure plots the relationship between \( \pi \) and \( \eta \) implied by (34). I calibrate this equation using estimates from Table 3 (columns 2 and 4): specifically, \( \hat{\delta}_c^1 = -0.913, \hat{\delta}_c^2 = 0.743 \) and \( \hat{\delta}_u^1 = 1.096. \)

Figure A1: Graphical illustration of crowding out estimates

This figure presents Frisch-Waugh type plots for the unconditional \( \delta_u^1 \) estimates in columns 3 and 4 of Table 3. See Appendix D.1. To restrict the range of the x-axis, I have excluded a small number of outlying data points: 9 observations in the OLS panel and 15 for IV.
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