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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. My estimate is precise and robust to numerous specifications, as well as accounting for local dynamics; and I show how it can be reconciled with apparently conflicting results in the literature. Exploiting my model's structure, I attribute 30% of the observed effect to mismeasurement, specifically undercoverage of immigrants. Based on a remarkably simple decomposition, I show that population mobility accounts for 90% of local adjustment, and labor demand the remainder. These results have important methodological implications for the estimation of immigration effects.

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

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1 Introduction

Recent years have seen growing interest in the geographical mobility of labor, as collapsing manufacturing employment exacerbates regional inequalities (Autor, Dorn and Hanson, 2013; Charles, Hurst and Notowidigdo, 2016). An important tension in this discussion is the apparent inconsistency between migratory responses to different shocks. On the one hand, economists mostly agree that local population responds strongly (though not instantaneously) to labor demand (e.g. Blanchard and Katz, 1992; Monras, 2015; Amior and Manning, 2018). But, it is commonly believed that local supply shocks driven by immigration elicit little such response: see e.g. Card (2001). As it happens, this latter result underpins much of the empirical immigration literature: many studies exploit local variation in the incidence of immigration to identify its effects (see Lewis and Peri, 2015, and Jaeger, Ruist and Stuhler, 2018, for excellent surveys); but this strategy is typically only feasible if mobility responds sluggishly (Borjas, Freeman and Katz, 1997).

This paper attempts to reconcile these results, using decadal US census data on 722 commuting zones (CZs) over 50 years. Like Amior and Manning (2018), I take a "semi-structural" approach to estimation which accounts explicitly for dynamic local adjustment. This allows me to study very general settings, without imposing heavy structural assumptions or relying on natural experiments (which restrict analysis to specific historic episodes). Identifying my model with shift-share instruments, I find that for each new foreign arrival to a CZ (over one decade), 1.1 existing residents leave on net contemporaneously, with a standard error of just 0.1. This effect is remarkably precise and robust to numerous specification choices, and even the OLS estimate is very large (0.76). It is also educationally "balanced": the college share of both foreign inflows and net outflows resemble the local population. And similarly to Dustmann, Schoenberg and Stuhler (2017), it is driven by a reduction in internal inflows rather than larger outflows (and hence my preference for the "crowd-out" terminology over the more typical "displacement"). I base my main estimates on CZs, but I also cannot reject perfect crowd-out across states.

Though one may expect a significant response, its sheer size is puzzling - for two reasons. First, it conflicts with much of the literature (as I describe below), and a key contribution of this paper will be to reconcile these results. Second, my estimates even lack *internal* consistency: I identify small but significant negative effects of foreign inflows on local employment rates, especially among the low educated. This suggests local adjustment is incomplete, which is difficult to reconcile with perfect crowd-out.

I attribute the latter inconsistency to mismeasurement - specifically, undercoverage of immigrants in the census. The structure of my model allows me to identify the extent of undercoverage. Purging the associated bias reduces my crowd-out estimate from 1.1 to 0.8. I attribute the bulk of the remaining 0.8 effect to the labor market. Though labor demand (itself mediated by the housing market) does cushion the impact of immigration,

I find that population mobility accounts for 90% of the recovery of local welfare. The dominant role of population mobility is consistent with what we know about local adjustment following labor *demand* shocks (as cited above). Methodologically, exploiting my model, I show how the contributions of internal mobility and labor demand to local adjustment can be quantified in a simple way, without any information on local wages or prices, and without having to impose any parameter values ex ante. This is useful because local wage deflators are notoriously difficult to construct and rely on heavy theoretical assumptions: see e.g. Koo, Phillips and Sigalla (2000), Albouy (2008) and Phillips and Daly (2010).

Turning to the existing literature, I am not the first to identify large crowd-out (see Filer, 1992; Frey, 1995; 1996; Borjas, Freeman and Katz, 1997; Hatton and Tani, 2005; Borjas, 2006, 2014); though Wright, Ellis and Reibel (1997) dispute Frey's methods, and Peri and Sparber (2011) and Card and Peri (2016) dispute Borjas'. Still, even Borjas' (2006) estimates are smaller than mine: he finds that each immigrant crowds out 0.6 natives from US metro areas, and 0.3 from states. In a study of the Mariel Boatlift, Monras (forthcoming) estimates that internal mobility accounts for about half of local adjustment - again, significantly less than my own estimate. Monras (2020) also estimates large crowd-out following an unanticipated surge of Mexican migration, but finds much less over the decadal census intervals which I study. Applying a structural model to local wage data, Colas (2018) finds that crowd-out reaches 0.5 ten years after a one-off immigration shock. Burstein et al. (2020) show that migrants crowd out natives from non-tradable jobs, though this is a within-CZ effect. Abramitzky et al. (2019) study the effect of *lost* foreign labor (following the 1920s imposition of border controls): in affected urban areas, this labor was fully replaced through population inflows; but in rural areas, it triggered large net outflows of native-born workers. Finally, Dustmann, Schoenberg and Stuhler (2017) find that Czech cross-border commuters crowded out German nationals one-for-one in local employment in the early 1990s, with a third of the effect (over three years) coming through internal migration.

Still, the US literature more typically reports small negative or even positive effects on native population: see Butcher and Card (1991), White and Imai (1994), Wright, Ellis and Reibel (1997), Card and DiNardo (2000), Card (2001, 2005, 2007), Card and Lewis (2007), Cortes (2008), Boustan, Fishback and Kantor (2010), Peri and Sparber (2011), Wozniak and Murray (2012), Hong and McLaren (2015), Edo and Rapoport (2019) and Piyapromdee (forthcoming); see also Pischke and Velling (1997) on Germany, and Sanchis-Guarner (2017) on Spain. These results are often rationalized by an elastic local demand for labor (which absorbs the new migrants), driven either by technological change or local consumption (Lewis, 2011; Dustmann and Glitz, 2015; Hong and McLaren, 2015). Consistent with these claims, I find employment is indeed very responsive to (total) local population. But since population *itself* adjusts so quickly, I find the labor demand

response has little traction over the path of adjustment - at least in this setting.

So, how can my results be reconciled with the literature? While I pool 50 years of data, a number of papers rely on much shorter horizons; and as Borjas, Freeman and Katz (1997) argue, such estimates are sensitive to the particular pattern of omitted shocks in any given decade. Indeed, with no controls, my estimates of crowd-out vary greatly by decade (consistent with the existing literature). But once I control for initial conditions and observable supply/demand shocks, the estimates are much more consistent over time - and I cannot reject perfect crowd-out in any decade. I also show the inclusion of arguably endogenous controls in previous studies may play a role.

Still, while I focus on cross-CZ variation, much of the literature exploits variation across skill groups *within* areas (e.g. Card and DiNardo, 2000; Card, 2001, 2005; Borjas, 2006; Cortes, 2008; Monras, 2020): i.e. they study the effect of skill-specific immigration on local skill composition. But small composition effects are not inconsistent with large crowd-out at the *aggregate* level - for two reasons. First, composition effects reflect not only differential internal mobility, but also changes in the character of local birth cohorts. Indeed, I show that cohort effects have historically offset the contribution of mobility (in the determination of local skill composition). And second, within-area estimates do not account for the economic impact that new migrants exert *outside* their own skill group (see Card, 2001; Dustmann, Schoenberg and Stuhler, 2016). This can be seen in the remarkable sensitivity of my within-area estimates to the delineation of these skill groups. These insights allow me to reconcile my results with this literature.

Finally, this paper highlights the importance of census undercoverage. Though there have been attempts to measure undercoverage of undocumented migrants (see e.g. Van Hook and Bean, 1998*b*, for an excellent survey), there has been little discussion of the implications (whether qualitative or quantitative) for estimating the *effects* of immigration. As I discuss below, neither reduced form nor structural estimates are immune from these concerns. Here, I offer a means to quantify the bias using the census data alone. Exploiting my model's structure, I show that controlling for employment growth (in the crowd-out equation) effectively partials out the bias in foreign inflows. An estimate of the employment elasticity is then sufficient to identify the undercount - which I place at about 30%. Variation across census years is consistent with these claims: I estimate double the crowd-out before 1980, when coverage was known to poorer. But as the model predicts, once I condition on employment, crowd-out varies little over time.

To summarize, this paper makes four contributions:

1. Estimation of an empirically robust one-for-one crowd-out effect, based on a model which accounts for sluggish adjustment and local dynamics.

2. Identification of significant undercoverage bias in US census data, which accounts for about 30% of the crowd-out effect.

3. A novel (and remarkably simple) approach to decomposing local adjustment into contributions from internal mobility (90%) and labor demand (10%), which requires no information on wages or prices, and does not impose any parameter values ex ante. The dominant role of population mobility is consistent with evidence on adjustment to local demand shocks.

4. Reconciliation of my crowd-out estimates with existing studies which exploit both cross-area and within-area (cross-skill) variation.

In Section 2, I set out my model and derive my estimating equations. Section 3 describes the data. Section 4 presents my main crowd-out estimates and assesses the credibility of my identification strategy. I also study the impact of immigration on employment rates, wages and housing costs. Section 5 assesses the implications of undercoverage for my results and the decomposition of local adjustment. And in Section 6, I subject my results to numerous robustness checks, and attempt to reconcile them with the existing literature. The Online Appendices contain theoretical derivations and numerous empirical sensitivity tests, as well as reconciliations with two classic studies of crowd-out in Card (2001) and Card (2007).

2 Model of local crowding out

2.1 Overview

Like Amior and Manning (2018), my theoretical framework consists of two components: (i) a Roback (1982) style model for local labor market equilibrium, conditional on population, and (ii) dynamic equations describing local population adjustment. In this paper, I develop this framework to explore the local impact of immigration.

This impact is moderated by two local adjustment mechanisms: (i) internal mobility and (ii) an expansion of labor demand. As I will show, the contributions of each can be quantified using estimates of "unconditional" and "conditional" crowd-out. Unconditional crowd-out is the impact of foreign inflows on net internal outflows, an object with long precedent in the literature (though I respecify it to account for dynamic adjustment). Conditional crowd-out also describes the impact on net internal outflows, but this time holding labor demand (identified by total employment) fixed. By comparing the two, I can identify the contribution of labor demand to local adjustment.

This paper is closely related to Amior (2020), but addresses a different set of questions. Here, I take *foreign inflows* to local labor markets as given, and I study the implications for local population, employment and welfare. In contrast, Amior (2020) takes local (demand-driven) *employment growth* as given, and studies the determination of new immigrants' location choices and their contribution to population adjustment (similar to

Borjas, 2001 and Cadena and Kovak, 2016). Both papers rely on the *conditional* crowd-out equation. But the *unconditional* equation is particular to this paper: by comparing the two, this paper seeks to unravel the direct (first order) effects of immigration shocks.

For simplicity, I assume in the model that native and migrant labor are identical and perfect substitutes. The existing literature does typically (though not unanimously) find they are close substitutes, both within skill cells and at the aggregate level.[1] But I do *not* impose perfect substitutability on my empirical specifications. Rather, I consider this assumption's validity ex post, following a strategy proposed by 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.4 for an exposition which does, and see Section 6.3 for associated empirical estimates. Another possible source of heterogeneity is monopsony: if firms enjoy greater market power over migrants than natives, they may exploit immigration by extracting more rents from native and migrant labor alike (Amior and Manning, 2020). For simplicity, I have chosen to assume a competitive market in this exposition; but I argue in Section 4.2 that this mechanism may help account for the large mobility response to immigration.

In what follows, I first derive an estimable equation for conditional crowd-out. I then solve for local employment growth and derive my unconditional crowd-out specification. And finally, I show how one can decompose the contributions of internal mobility and labor demand to local adjustment (following a given immigration shock), using estimated coefficients from the conditional and unconditional equations.

2.2 Local equilibrium conditional on population

Suppose there is a single traded good, with price P, and a non-traded good (housing) with price P_r^h in area r. If preferences are homothetic, I can define a unique local price index:

$$P_r = Q\left(P, P_r^h\right) \tag{1}$$

Next, let N_r and L_r denote local employment and population. The standard Roback (1982) model assumes labor supply is fixed, so there is no meaningful difference between

[1] Ottaviano and Peri (2012) estimate an elasticity of substitution between natives and migrants of 20 within skill cells. Card (2009) finds even larger numbers, and Borjas, Grogger and Hanson (2012) and Ruist (2013) cannot reject perfect substitutability. At the *aggregate* level (which is the relevant point here), differences in native and migrant skill composition will also matter. Migrants do have similar college shares to natives in the US (Card, 2009), but they are disproportionately represented among high school dropouts. The latter point is only pertinent if high school dropouts and graduates are imperfect substitutes: Card (2009) and Ottaviano and Peri (2012) cannot reject perfect substitutability, though this conclusion is disputed by Borjas, Grogger and Hanson (2012). Note also that these skill share estimates may be biased by undercoverage (see below).

them. But suppose labor supply is somewhat elastic to the real consumption wage:

$$n_r = l_r + \epsilon^s (w_r - p_r) + z_r^s \tag{2}$$

where lower case denotes logs. w_r is the log nominal wage, and z_r^s is a local supply shifter. I write labor demand as:

$$n_r = -\epsilon^d (w_r - p) + z_r^d \tag{3}$$

where z_r^d is a demand shifter. In Appendix A.2, I set out equations for housing supply and demand. Together with (1)-(3), I can then solve for employment, wages and prices as functions of population l_r alone. Finally, I write indirect utility as:

$$v_r = w_r - p_r + a_r \tag{4}$$

where $w_r - p_r$ is the real consumption wage, and a_r local amenities. Crucially, I can now replace the real wage in (4) with the the labor supply equation (2). This allows me to use the local employment rate as a sufficient statistic for local utility, for given supply and amenity effects:

$$v_r = \frac{1}{\epsilon^s} (n_r - l_r - z_r^s) + a_r \tag{5}$$

The elimination of real wages is useful because local wage deflators are notoriously difficult to construct (and rely on strong theoretical assumptions), especially for the detailed geographies and long time horizons I study (see Koo, Phillips and Sigalla, 2000; Albouy, 2008; Phillips and Daly, 2010); whereas employment rates are easily measured in the census data. 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 and labor market frictions; and it is also robust to heterogeneity in the consumer price indices of natives and migrants (as in Albert and Monras, 2018).[2] Another concern is heterogeneous preferences for leisure: I 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 determines population l_r in every area r. Like Amior and Manning (2018), I allow for population to adjust sluggishly to this equilibrium; but I distinguish between the contributions of internal and foreign migration:

$$dl_r = \lambda_r^I + \lambda_r^F \tag{6}$$

[2]Suppose natives and migrants weight local prices differently in utility. The labor supply of natives will depend on *their* price index, and similarly for migrants. But this means I can still replace both natives' and migrants' real consumption wages with the employment rate (in their respective utility functions), at least after adjusting employment rates for demographic composition.

where λ_r^I is the instantaneous rate of net internal inflows (i.e from elsewhere in the US), and λ_r^F is the foreign inflow rate, relative to local population. (6) does not account for emigration, but I consider this later when interpreting the estimates. Using a logit model of residential choice (see e.g. Appendix A of Amior, 2020), λ_r^I can be expressed as a linear function of utility v_r:

$$\lambda_r^I = \gamma \left(n_r - l_r - z_r^s + \epsilon^s a_r \right) \tag{7}$$

where $\gamma \geq 0$ is the elasticity of net internal flows. I have not included a national intercept, but the supply effect z_r^s 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 γ parameter depends on both mobility and the local persistence of shocks.

In Amior (2020), where I study the location choices of new immigrants, I set out a parallel expression for foreign inflows, λ_r^F. This depends partly on what I call the "foreign intensity", i.e. the foreign inflow in the absence of local utility differentials. Crucially, the foreign intensity varies regionally, partly due to the size of migrant enclaves (which offer e.g. language or job access benefits). Since it does not enter the internal mobility equation (7) directly, this yields an exclusion restriction which motivates the classic "enclave instrument" of Altonji and Card (1991) and Card (2001). But, I do not model λ_r^F formally in this paper: my interest here is the evolution of internal flows λ_r^I for *given* λ_r^F.

2.4 Conditional crowd-out

I now derive an estimable equation for conditional crowd-out, which describes the effect of foreign inflows λ_r^F on (net) internal flows λ_r^I, holding the level of labor demand fixed. I first substitute (7) for internal flows λ_r^I in (6):

$$dl_r = \lambda_r^F + \gamma \left(n_r - l_r - z_r^s + \epsilon^s a_r \right) \tag{8}$$

For estimation, I require a discrete-time expression. Suppose the foreign inflow λ_r^F is constant within decadal intervals; and suppose also the supply effect z_r^s, amenity a_r and employment n_r change at constant rates within them. I show in Appendix A.1 that:

$$\lambda_{rt}^I = \left(1 - \frac{1 - e^{-\gamma}}{\gamma} \right) \left(\Delta n_{rt} - \lambda_{rt}^F - \Delta z_{rt}^s + \epsilon^s \Delta a_{rt} \right) + \left(1 - e^{-\gamma} \right) \left(n_{rt-1} - l_{rt-1} - z_{rt-1}^s + \epsilon^s a_{rt-1} \right) \tag{9}$$

where $\lambda_{rt}^I \equiv \int_{t-1}^{t} \lambda_r^I(\tau)\, d\tau$ is the discrete-time internal response over the unit interval, and λ_{rt}^F is the discrete foreign inflow. Conditional on employment growth Δn_{rt} (which fixes labor demand), the initial conditions (the lagged employment rate) and amenity/supply shocks, (9) describes the impact of foreign inflows λ_{rt}^F on net internal inflows λ_{rt}^I. This

effect increases from 0 to -1 as the internal population elasticity γ increases from 0 to ∞. Notice the demand shifter Δz_{rt}^d does *not* appear in this equation: due to the sufficient statistic result, employment growth and the lagged rate account fully for the welfare effects of local labor market dynamics (conditional on the supply/amenity effects).

Equation (9) is implicitly an error correction model in population and employment. This can be seen by adding foreign inflows λ_{rt}^F to both sides. The dependent variable then becomes the log population change Δl_{rt}, which is a linear function of the log employment change Δn_{rt} and the lagged log employment rate $n_{rt-1} - l_{rt-1}$ (i.e. the initial steady-state deviation). As γ becomes large, we approach 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.

Notice the coefficients on λ_{rt}^F and Δn_{rt} are pure functions of γ, and identical up to their sign. This is because both λ_{rt}^F and Δn_{rt}, conditional on the other, mechanically feed one-for-one into local employment rates (and therefore into local utility v_{rt}); and γ represents the pure mobility response to local utility. In practice though, estimates of the λ_{rt}^F coefficient will exceed that of Δn_{rt} if there is native distaste for immigration (as in Card, Dustmann and Preston, 2012; Saiz and Wachter, 2011; Fernandez-Huertas Moraga, Ferrer-i Carbonell and Saiz, 2019): effectively, this would generate a negative correlation between λ_{rt}^F and an (unobserved) amenity change Δa_{rt}.

2.5 Unconditional crowd-out

Equation (9) does not describe the *unconditional* impact of foreign inflows, as I am controlling for employment growth Δn_{rt} (which fixed labor demand); and labor demand may be a key margin of adjustment. To derive the unconditional effect (where this paper and Amior, 2020, diverge), I now reduce Δn_{rt} to its determinants.

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

$$\Delta \left(p_{rt} - p_t \right) = \frac{1}{\kappa} \left[\frac{1}{\epsilon^s} \left(\Delta n_{rt} - \Delta l_{rt} - \Delta z_{rt}^s \right) + \Delta n_{rt} \right] \tag{10}$$

where the parameter $\kappa > 0$ goes to infinity with the elasticity of housing supply.[3] Given the labor supply and demand equations, employment growth can then be written as:

$$\Delta n_{rt} = \eta \left(\Delta l_{rt} + \Delta z_{rt}^s \right) + \left(1 - \eta \right) \frac{\kappa}{\kappa + \epsilon^d} \Delta z_{rt}^d \tag{11}$$

[3] $\kappa \equiv \frac{1 - \nu + \epsilon_r^{hs}}{\nu}$, where ν is the income share spent on housing, and ϵ_r^{hs} is the housing supply elasticity.

where

$$\eta \equiv 1 - \left(1 + \frac{\kappa + 1}{\kappa + \epsilon^d} \cdot \frac{\epsilon^d}{\epsilon^s}\right)^{-1} \tag{12}$$

is the elasticity of employment to local population. η is bounded by 0 and 1 if labor demand slopes down (i.e. $\epsilon^d > 0$) and labor supply up ($\epsilon^s > 0$). η captures the sum of all local adjustment mechanisms, conditional on population. In particular, η is increasing in the wage elasticity ϵ^d of labor demand (relative to labor supply ϵ^s): in a more complete model, ϵ^d will capture the extent of capital mobility and local demand for non-tradables.[4] Similarly, one would expect a larger employment response η to population if housing supply is more elastic (i.e. κ larger).[5] Intuitively, this should ensure local prices are less sensitive to foreign inflows; and this should moderate any negative effect on real wages, labor supply in (2), and labor demand in equilibrium. I have assumed here that the labor and housing market elasticities are fixed and constant: to the extent they are non-linear or vary regionally (as in e.g. Glaeser and Gyourko, 2005), my empirical estimates should be treated as average linearized effects.

As I show in Appendix A.3, substituting (11) for employment growth Δn_{rt} in the conditional crowd-out equation then yields the unconditional equation:

$$\begin{aligned} \lambda^I_{rt} &= \frac{(1-\eta)\left(1 - \frac{1-e^{-\gamma}}{\gamma}\right)}{1 - \eta\left(1 - \frac{1-e^{-\gamma}}{\gamma}\right)} \left(\frac{\kappa}{\kappa + \epsilon^d}\Delta z^d_{rt} - \lambda^F_{rt} - \Delta z^s_{rt} + \frac{\epsilon^s}{1-\eta}\Delta a_{rt}\right) \\ &+ \frac{1 - e^{-\gamma}}{1 - \eta\left(1 - \frac{1-e^{-\gamma}}{\gamma}\right)} \left(n_{rt-1} - l_{rt-1} - z^s_{rt-1} + \epsilon^s a_{rt-1}\right) \end{aligned} \tag{13}$$

Compared to the conditional equation (9), employment growth Δn_{rt} has been replaced with the exogenous demand shifter Δz^d_{rt}.

2.6 Relationship between crowd-out equations

To clarify how the crowd-out equations are related, notice the conditional crowd-out and employment equations, i.e. (9) and (11), form a two-equation system. This become clearer once I abstract from the lagged employment rate and amenity/supply controls[6]:

$$\lambda^I_{rt} = -\delta^c_1 \lambda^F_{rt} + \delta^c_2 \Delta n_{rt} \tag{14}$$

$$\Delta n_{rt} = \eta\left(\lambda^I_{rt} + \lambda^F_{rt}\right) + \eta_d \Delta z^d_{rt} \tag{15}$$

[4]Immigrants will of course support local demand through consumption (e.g. Hong and McLaren, 2015). Though I do not account for this effect above, it is observationally equivalent to a flatter labor demand curve; and it will return identical estimating equations: see Amior and Manning (2018).

[5]The employment elasticity η is increasing in κ (and therefore in the housing supply elasticity) if $\epsilon^d > 1$. This condition ensures that immigration causes the local wage bill (and therefore housing demand) to grow, in the absence of a mobility response.

[6]One may interpret the remaining variables as residuals, conditional on these controls.

There are two exogenous inputs (the foreign inflow λ^F_{rt} and local demand shifter Δz^d_{rt}) and two endogenous outputs (the net internal flow λ^I_{rt} and employment growth Δn_{rt}). Based on (9), $\delta^c_1 \to 1$ and $\delta^c_2 \to 1$ as the population elasticity $\gamma \to \infty$. Note the basic model predicts $\delta^c_1 = \delta^c_2$; but for the reasons discussed above, I do not impose this condition.

The unconditional crowd-out equation (13) is a "reduced form" specification:

$$\lambda^I_{rt} = -\delta^u_1 \lambda^F_{rt} + \delta^u_2 \Delta z^d_{rt} \tag{16}$$

which collapses λ^I_{rt} to the exogenous variables, λ^F_{rt} and z^d_{rt}. Given this, unconditional crowd-out δ^u_1 can be written as:

$$\delta^u_1 = \frac{\delta^c_1 - \eta\delta^c_2}{1 - \eta\delta^c_2} \tag{17}$$

which is smaller than the conditional effect δ^c_1, for employment elasticity $\eta > 0$. Intuitively, the conditional effect δ^c_1 describes the *counterfactual* mobility response to a foreign inflow λ^F_{rt}, in the absence of a labor demand response. If $\eta = 0$, the unconditional effect δ^u_1 will therefore equal δ^c_1. But as η expands, the employment response to immigration becomes increasingly positive: this will moderate any adverse welfare effects[7], and unconditional crowd-out δ^u_1 will contract. In the limit, as $\eta \to 1$, adjustment is fully effected by changes in local employment rather than population; so $\delta^u_1 \to 0$ (zero crowd-out).

Crucially though, as the population elasticity γ increases, the contribution of employment through η recedes. In the limit, as $\gamma \to \infty$ (so $\delta^c_1 \to 1$), internal mobility will account for the *entirety* of local adjustment, *irrespective* of the value of η: as (17) shows, unconditional crowd-out δ^u_1 will equal 1; so employment will remain static. Intuitively, even if employment is responsive to local population (due to the housing and labor market elasticities), this will have little traction if population *itself* adjusts very quickly.

2.7 Identification of mobility and labor demand responses

What is the impact of a one-off foreign inflow on local welfare? What are the contributions of internal mobility and labor demand to local adjustment? And how can we identify them? As I now show, the sufficient statistic result facilitates a remarkably simple and intuitive decomposition of the welfare effects of (i) the foreign inflow, (ii) the mobility response (via the γ elasticity) and (iii) the labor demand response (via the η elasticity, itself mediated by the housing market). This decomposition relies on estimates of the conditional and unconditional crowd-out equations alone: it requires no information on wages or prices, and imposes no ex ante values on housing or labor market elasticities.

[7]This moderating effect is contingent on labor demand sloping down (i.e. $\epsilon^d > 0$): see (12). Otherwise, η may turn negative, in which case population growth would perversely cause employment to *contract* in comparative statics (though equilibrium would be unstable). Of course, agglomeration is crucial to the existence of cities. But if these returns never peter out, population adjustment cannot return the system to equilibrium.

I focus for now on the contemporaneous welfare response, and return to the dynamics below. Given the sufficient statistic result, the overall (contemporaneous) welfare effect of an immigration shock (up to any amenity effect) can be summarized by the *employment rate*. This can be decomposed as:

$$\begin{aligned}\frac{d\left[\Delta n_{rt}-\Delta l_{rt}\right]}{d\lambda_{rt}^{F}} &= -\frac{d\lambda_{rt}^{F}}{d\lambda_{rt}^{F}}-\frac{d\lambda_{rt}^{I}}{d\lambda_{rt}^{F}}+\frac{d\Delta n_{rt}}{d\Delta l_{rt}}\cdot\frac{d\Delta l_{rt}}{d\lambda_{rt}^{F}} \\ &= -1+\delta_{1}^{u}+\eta\left(1-\delta_{1}^{u}\right) \\ &= \underbrace{-1}_{\text{Immig shock}}+\underbrace{\delta_{1}^{u}}_{\text{Pop response}}+\underbrace{\frac{\delta_{1}^{c}-\delta_{1}^{u}}{\delta_{2}^{c}}}_{\text{Emp response}} \end{aligned} \tag{18}$$

where the first term (the -1) represents the counterfactual impact of the immigration shock, in the absence of any mobility response (i.e. $\gamma = 0$) or any employment response (i.e. $\eta = 0$, such that labor demand is perfectly inelastic).[8] The second term (δ_1^u) describes the internal mobility response (i.e. unconditional crowd-out). And the final term describes the employment response, for given population. This can be identified by the difference between conditional and unconditional crowd-out ($\delta_1^c - \delta_1^u$): this is because δ_1^c holds labor demand fixed, while δ_1^u does not.

This is a remarkably simple and intuitive (and easily estimable) decomposition. In innovative work, Monras (forthcoming) attempts a similar exercise (applied to the Mariel Boatlift), but takes a different approach. He evaluates local welfare using wages; and to translate between the immigration shock (expressed in stocks) and prices, he parameterizes equations for labor demand, housing demand and housing supply. But since I write everything in stocks (by relying on the sufficient statistic result), I do not need to make assumptions about these parameters. In fact, my decomposition is overidentified: as (18) shows, I can predict the (unconditional) employment rate effect using estimates of the crowd-out equations; but I can also estimate the employment rate effect directly. This is a useful check on the model; and as I show below, I cannot reject its implicit restrictions.

Crucially, the contributions of mobility and employment to adjustment influence one another; and the crowd-out equation parameters allow me to characterize this interdependence. In the absence of any labor demand response (i.e. if $\eta = 0$), the counterfactual welfare impact is:

$$\frac{d\left[\Delta n_{rt}-\Delta l_{rt}\right]}{d\lambda_{rt}^{F}}|_{\eta=0} = \underbrace{-1}_{\text{Initial shock}}+\underbrace{\delta_{1}^{c}}_{\text{Pop response}} \tag{19}$$

where the mobility response is identified by δ_1^c (since the conditional equation holds labor demand fixed). This is larger than the actual response (equal to δ_1^u in (18)), because an elastic labor demand moderates the welfare impact of the shock along the adjustment

[8] Since the employment rate contracts by 1 point in this counterfactual, the real wage $w_r - p_r$ declines by $\frac{1}{\epsilon^s}$: see the labor supply equation (2).

path. Similarly, if there is no mobility response (i.e. if $\gamma = 0$), the counterfactual welfare impact would be:

$$\frac{d\left[\Delta n_{rt} - \Delta l_{rt}\right]}{d\lambda_{rt}^{F}}|_{\gamma=0} = \underbrace{-1}_{\text{Initial shock}} + \underbrace{\frac{\delta_1^c - \delta_1^u}{\delta_2^c\left(1-\delta_1^u\right)}}_{\text{Emp response}} \tag{20}$$

where the final term identifies the elasticity of employment to population, i.e. η. Again, this is larger than the actual employment response in (18), because the mobility response reduces the supply of labor (and hence employment growth) over the adjustment path.

In practice though, I will argue this simple decomposition is infeasible because of undercoverage of migrants in the data. Without knowing the size of the bias ex ante, I cannot identify *both* the employment response *and* the extent of undercoverage using the crowd-out equations alone. However, as I discuss in Section 5, identification is feasible if I additionally exploit the restrictions implied by the employment equation (15).

I have focused here on the contemporaneous adjustment to an immigration shock. But to the extent this adjustment is incomplete, the dynamics will also play a role. In the absence of new immigration shocks, population will decline over time (according to the coefficient on the lagged employment rate in (13)); and employment will contract along with population, commensurate with the employment elasticity, η.

3 Data

I study decadal census observations between 1960 and 2010 across 722 CZs. Where possible, I use published county-level aggregates from NHGIS (Manson et al., 2017). And where necessary, I supplement this with IPUMS census microdata and (for 2010) pooled American Community Survey (ACS) samples of 2009-11 (Ruggles et al., 2017). I describe the data more fully in Appendix B, but I summarize the main points here.

The first challenge is to disaggregate log population changes Δl_{rt} into the contributions of foreign and internal mobility, λ_{rt}^F and λ_{rt}^I. Since I only have discrete-time observations, I cannot precisely identify these components - though a close approximation is possible. To see this, notice that:

$$\Delta l_{rt} \equiv \log\left(\frac{L_{rt}}{L_{rt-1}}\right) \equiv \log\left(\frac{L_{rt-1}+L_{rt}^F}{L_{rt-1}}\right) + \log\left(\frac{L_{rt}-L_{rt}^F}{L_{rt-1}}\right) - \log\left(1+\frac{L_{rt}^F}{L_{rt}}\cdot\frac{\Delta L_{rt}-L_{rt}^F}{L_{rt-1}}\right) \tag{21}$$

where L_{rt} is the local population of 16-64s at time t; and L_{rt}^F is the local foreign-born population who immigrated in the last ten years (i.e. since $t-1$). Motivated by (21), I

approximate the foreign inflow λ^F_{rt} and net internal flow λ^I_{rt} as:

$$\lambda^F_{rt} \approx \log\left(\frac{L_{rt-1}+L^F_{rt}}{L_{rt-1}}\right) \tag{22}$$

$$\lambda^I_{rt} \approx \log\left(\frac{L_{rt}-L^F_{rt}}{L_{rt-1}}\right) \tag{23}$$

where the final term of (21) is the approximation error. An alternative approach is to take first order approximations, i.e. $\lambda^F_{rt} \approx \frac{L^F_{rt}}{L_{rt-1}}$ and $\lambda^I_{rt} \approx \frac{\Delta L_{rt}-L^F_{rt}}{L_{rt-1}}$. But (22) and (23) offer more precision.[9] Notice I have constructed λ^I_{rt} as a *residual* population change: this accounts for the entire contribution of natives and "old" migrants (who immigrated before $t-1$) to local population, part of which is driven by "natural" growth and emigration (especially of the foreign-born). It is not possible to identify emigration in this data; but I show in Section 6.1 that natives account for the bulk of local crowd-out.

My employment sample also consists of 16-64s. Using the microdata, I adjust all employment variables for local demographic composition, controlling for age, education, ethnicity, gender, foreign-born status, and years in the US, together with a rich set of interactions. In terms of the model, this purges any local variation in the supply shocks z^s_{rt} which is due to observable composition. The aim is to reduce the demands on the exclusion restrictions. See Appendix B.2 for methodological details.

I identify local demand changes using Bartik (1991) industry shift-shares, and foreign inflows using the enclave shift-share of Altonji and Card (1991) and Card (2001). I will describe the exclusion restrictions when I set out the estimating equations. The Bartik predicts local employment growth, for given initial industrial composition and national-level changes by industry:

$$b_{rt} = \sum_i \phi^i_{rt-1}\Delta n_{i(-r)t} \tag{24}$$

where ϕ^i_{rt-1} is the share of area r individuals working in a 2-digit industry i (57 categories) in $t-1$; and $\Delta n_{i(-r)t}$ is i's national log employment change, excluding area r.[10]

The enclave shift-share allocates new migrants to areas proportionally to the initial size of co-patriot communities. This proxies the "foreign intensity" (i.e. inflows in the absence of local utility differentials) which I describe in Section 2.3. Using the functional form of (22):

$$m_{rt} = \log\left(\frac{L_{rt-1}+\sum_o \phi^o_{rt-1}L^F_{o(-r)t}}{L_{rt-1}}\right) \tag{25}$$

[9] While $\frac{L^F_{rt}}{L_{rt-1}}$ and $\frac{\Delta L_{rt}-L^F_{rt}}{L_{rt-1}}$ converge to the true λ^F_{rt} and λ^I_{rt} as they individually become small, convergence of (22) and (23) merely requires their product become small. Both these specifications of λ^F_{rt} share the advantage of depending on foreign inflows and *not* on changes in the population of existing residents (which might otherwise introduce a spurious correlation with λ^I_{rt}): see Peri and Sparber (2011).

[10] Autor and Duggan (2003) and Goldsmith-Pinkham, Sorkin and Swift (2020) recommend this exclusion to address possible endogeneity to local supply.

where ϕ^o_{rt-1} is the fraction of origin o migrants (77 countries) residing in area r at time $t-1$, and $L^F_{o(-r)t}$ is the stock of new origin o migrants (excluding area r residents) who immigrated between $t-1$ and t. I construct both shift-share instruments using census microdata: see Appendix B.3.

Throughout, I control for a set of observable amenities: (i) presence of coastline[11]; (ii) climate, specifically maximum January/July temperatures 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. Since 1960, the mean foreign inflow λ^F_{rt} has grown from 0.02 to 0.06. Unsurprisingly perhaps, the distribution of both λ^F_{rt} and the enclave shift-share m_{rt} are very skewed. But I show my results are robust to omitting outlying observations.

4 Estimates of crowding out and local impact

4.1 Estimating equations and identification

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

$$\lambda^I_{rt} = \delta^c_{0t} - \delta^c_1 \lambda^F_{rt} + \delta^c_2 \Delta n_{rt} + \delta^c_3 \left(n_{rt-1} - l_{rt-1}\right) + A_r \delta^c_{At} + \varepsilon^c_{rt} \tag{26}$$

where δ^c_1 is the crowding-out effect (which I define in negative terms, for consistency with the notation in (14)). The (composition-adjusted) employment growth control, Δn_{rt}, holds the level of labor demand fixed; so δ^c_1 identifies the mobility response in the absence of local demand adjustment (i.e. under $\eta = 0$). The lagged (composition-adjusted) employment rate, $n_{rt-1} - l_{rt-1}$, summarizes the initial conditions: i.e. any lingering impact of historical demand or migration shocks. I account for year effects in δ^c_{0t}, and the A_r vector contains amenity effects (i.e. observable components of Δa_{rt} and a_{rt-1} in (9)), which I interact with year effects (in δ^c_{At}). Any unobserved amenity/supply effects fall into the error, ε^c_{rt}. Based on (9), this equation should contain *no* omitted demand shocks. This is a consequence of the sufficient statistic result: the employment variables fully summarize local welfare, conditional *only* on supply effects.

[11] Coastline data is borrowed from Rappaport and Sachs (2003).

OLS estimates of (26) are not credible: foreign inflows λ^F_{rt}, employment growth Δn_{rt} and the lagged employment rate are endogenous to omitted amenity/supply effects (both current and lagged), such as differences in school quality or transport infrastructure. Three instruments (which exclude these) are required: I use the enclave shift-share m_{rt} for λ^F_{rt}, the current Bartik b_{rt} for Δn_{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. As with all shift-share instruments, identification may be motivated by exogeneity of the initial local migrant/industry shares to the omitted shocks (Goldsmith-Pinkham, Sorkin and Swift, 2020), or by random aggregate-level shocks to migrant inflows or industries (Borusyak, Hull and Jaravel, 2018).

At least in this literature, it is unusual to include endogenous variables (suitably instrumented) as controls. The meaning of such controls is best understood in the context of a two stage estimator, where the endogenous variables in the second stage are replaced by their projections on the instruments. Consider the case of the employment control, Δn_{rt}: the second stage partials out its entire projection on *both* the Bartik b_{rt} *and* enclave m_{rt} instruments. Effectively, this allows me to turn off any effect of (enclave-driven) foreign inflows λ^F_{rt} which manifests *via* employment growth, Δn_{rt}.

Next, I turn to the unconditional estimates of crowding out. Whereas the conditional specification identifies the *counterfactual* mobility response in the absence of demand adjustment (i.e. holding employment growth Δn_{rt} fixed), the unconditional effect of λ^F_{rt} *is* mediated by the moderating effects of labor demand. I base my empirical specification on (13):

$$\lambda^I_{rt} = \delta^u_{0t} - \delta^u_1 \lambda^F_{rt} + \delta^u_2 b_{rt} + \delta^u_3 \left(n_{rt-1} - l_{rt-1}\right) + A_r \delta^u_{At} + \varepsilon^u_{rt} \tag{27}$$

Compared to (26), the employment growth control Δn_{rt} has been replaced with its Bartik instrument b_{rt}: the Bartik accounts for observable components of the exogenous labor demand shifter Δz^d_{rt} in (13). Since b_{rt} captures only *exogenous* components of local demand growth, δ^u_1 will be mediated by any *endogenous* demand adjustment to the foreign inflow. There are now just two endogenous variables (λ^F_{rt} and $n_{rt-1} - l_{rt-1}$), so I use two instruments: the enclave shift-share m_{rt} and the lagged Bartik b_{rt-1}. Looking at (13), the error ε^u_{rt} now contains *new* unobserved innovations in demand Δz^d_{rt}, as well as unobserved supply/amenity shocks. *Historical* demand and migration shocks are summarized by the initial employment rate. As a result, the threats to identification are much weaker than in traditional specifications which neglect dynamics.

In traditional specifications, these dynamics pose two particular challenges. First, foreign inflows are locally very persistent (Jaeger, Ruist and Stuhler, 2018); so a naive regression of internal population flows on *contemporaneous* immigration may pick up a sluggish response to *historical* foreign inflows. Second, the location of migrant enclaves (which underly the m_{rt} instrument) depends on *past* local demand shocks, which are

themselves highly predictive of *current* shocks (Amior, 2020). But given the sufficient statistic result, the initial employment rate ($n_{rt-1} - l_{rt-1}$) partials out the entire history of immigration *and* labor demand shocks. This offers a theoretical basis for Pischke and Velling's (1997) suggestion to control for the initial unemployment rate. And it offers a tractable structural approach to addressing the concerns of Goldsmith-Pinkham, Sorkin and Swift (2020) about the endogeneity of initial shares in shift-share instruments. Below, I probe the effectiveness of this strategy.

4.2 Estimates of conditional and unconditional crowd-out

Table 2 presents first stage estimates for (26) and (27). I weight observations by lagged local population share and cluster errors by state. Each instrument has large positive effects on its corresponding endogenous variable, with large Sanderson-Windmeijer (2016) F-statistics (accounting for multiple endogenous variables) reported in Table 3.

Table 3 sets out OLS and IV estimates of (26) and (27). The first two columns estimate the conditional crowd-out equation (26): these are identical to Table 7 (columns 1-2) of Amior (2020). The OLS coefficient on λ_{rt}^F (i.e. the negative of δ_1^c) is -0.88; and the IV coefficient -0.91, with a standard error of just 0.07. That is, holding changes in labor demand fixed, each new immigrant to a CZ contemporaneously crowds out almost one existing resident. The similarity between OLS and IV can be attributed to the employment control: in principle, there are no omitted demand shocks; so any OLS-IV disparity must be due to omitted supply shocks alone. I also estimate large responses to employment growth and the lagged employment rate. To see how the dynamics operate, consider a 1 log point foreign inflow. Given conditional crowd-out δ_1^c of 0.91 in IV, the employment rate contracts by 0.09 (holding employment fixed); and this will generate a net outflow of 0.05 ($= 0.56 \times 0.09$) in the subsequent decade (where 0.56 is the lagged employment rate coefficient).

As noted above, the model in (9) predicts equal (and opposite) coefficients on the foreign inflow λ_{rt}^F and employment growth Δn_{rt} (i.e. $\delta_1^c = \delta_2^c$). Intuitively, a given change in λ_{rt}^F or Δn_{rt} (conditional on the other) has identical implications for the local employment rate (and hence local welfare) and should trigger identical mobility responses. However, my IV estimate of δ_1^c significantly exceeds δ_2^c. As I have argued above (and see also Amior, 2020), one possible explanation is native distaste for immigration (as in e.g. Card, Dustmann and Preston, 2012). Another is monopsony: Amior and Manning (2020) argue that firms can exploit immigration by extracting larger rents from native and migrant labor alike. If so, one might expect immigration to have a more adverse effect on native wages and employment than in the competitive model outlined above.

I now turn to unconditional crowd-out, where this paper and Amior (2020) diverge. The OLS estimate of δ_1^u in (27) is itself very large: a foreign inflow equal to 1% of the initial local population is associated with a 0.76% net outflow. One would expect OLS to be biased towards zero, as omitted demand and amenity effects should draw both foreign and internal inflows. And indeed, the IV estimate in column 4 is even larger, reaching 1.1 (i.e. exceeding one-for-one crowd-out), with a standard error of just 0.13.

My IV estimate of δ_1^u is also larger than its conditional counterpart δ_1^c in column 2, and significantly so: the differential between them is 0.18, with a standard error of just 0.08.[12] This is puzzling: the employment response to immigration should *moderate* the impact on welfare and internal mobility (if labor demand slopes down, and the employment elasticity η is positive); and this should be reflected in a *smaller* unconditional effect. Based on (18), the employment response to foreign inflows can be quantified as $\frac{\delta_1^c - \delta_1^u}{\delta_2^c} = \frac{0.913 - 1.096}{0.743} = -0.25$ for each new immigrant (with a standard error of 0.10[13]), i.e. a perversely negative effect. Below, I will ascribe this result to undercoverage bias: adjusting for the bias, I find the true employment response is indeed positive (though small).

In Appendix C, I show the unconditional crowd-out effect δ_1^u is educationally "balanced": the college share of both the foreign inflow and net outflow (elicited by the enclave instrument) resemble the local population. This crowd-out is mostly driven by natives (Section 6.1); it is similarly large both within and across states (Appendix F.3), and robust to dropping population weights and excluding large or small CZs (Appendix F.2). In Appendix D, I show it is entirely driven by reductions of migratory *inflows* to affected CZs, rather than increases in *outflows*. This is consistent with Coen-Pirani (2010), Monras (2015), Dustmann, Schoenberg and Stuhler (2017) and Amior and Manning (2018), who find that inflows account for the bulk of local population adjustment.

4.3 Credibility of identification strategy

The enclave shift-share instrument is often criticized because of the endogeneity of the initial migrant shares (Goldsmith-Pinkham, Sorkin and Swift, 2020). If local demand or amenity shocks are persistent over time (as in e.g. Amior and Manning, 2018) and if migrants' location choices are very responsive to these shocks (e.g. Cadena and Kovak, 2016; Amior, 2020), initial migrant shares will *not* exclude these shocks: see Borjas, Freeman and Katz (1997). Furthermore, Jaeger, Ruist and Stuhler (2018) argue that if local labor markets respond sluggishly to immigration shocks, it may be difficult to

[12] This standard error accounts for statistical dependence between coefficient estimates across the conditional and unconditional equations. In practice, I account for this dependence by estimating a single model which nests both the conditional and unconditional equations: I use a dataset with every observation duplicated, where every right hand variable (and instrument) is interacted with dummy indicators for both the conditional and unconditional models. The standard error is relatively small because the δ_1^c and δ_1^u estimators covary positively.

[13] I compute this using the delta method, using the same nested model described in footnote 12.

empirically disentangle the effects of current and historical shocks.

Exploiting my model's structure, I address these concerns by controlling for the lagged employment rate (suitably instrumented), which partials out the *initial conditions* (and therefore the entire history of demand and migration shocks, whether observed or unobserved). This means I can estimate my model in very general contexts, without restricting attention to particular historical episodes where credible identification is more straightforward. To the extent there are any remaining omitted *contemporaneous* demand shocks (i.e. unobserved components of Δz^d_{rt}, which are orthogonal to the lagged employment rate and current Bartik control), these should bias my crowd-out estimates *positively* (as these will simultaneously draw foreign and internal inflows), i.e. towards zero.

Reassuringly, the evidence suggests the initial employment rate control is performing its function well. To see this, consider first what happens if I control for the lagged enclave shift-share, m_{rt-1}, following the recommendation of Jaeger, Ruist and Stuhler (2018). Notice that m_{rt-1} negatively affects the initial employment rate in the *first* stage (column 6 of Table 2); but reassuringly, it has no effect in the *second* stage (column 5 of Table 3). This is consistent with the employment rate summarizing the full history of shocks. However, once I drop the employment rate in column 6 (and replace it with its lagged Bartik instrument), m_{rt-1} now picks up much of the negative effect (this can be interpreted as the "reduced form" version of column 4, where the lagged employment rate is reduced to observable shocks). That is, though internal flows do respond sluggishly, the initial employment rate accounts successfully for these dynamics. Notice also the λ^F_{rt} coefficient in column 6 is now smaller: this likely reflects a positive correlation between the instrument m_{rt} and omitted historical *demand* shocks (which the employment rate partials out in column 5).

Peri (2016) and Goldsmith-Pinkham, Sorkin and Swift (2020) recommend testing for pre-trends. So in column 7, I replace the dependent variable with its lag, λ^I_{rt-1}. Reassuringly, the *current* foreign inflow λ^F_{rt} and Bartik b_{rt} have no significant effect on λ^I_{rt-1}: instead, the impact is fully absorbed by the *lagged* enclave shift-share and Bartik.

To summarize, this suggests my model can tease apart the effects of current and historical shocks; and the sufficient statistic control (i.e. initial employment rate) offers a tractable means to accomplish this. That is, the identification strategy appears to work well. So why do my estimates appear so different to what has come before? An important contribution of this paper is to address this question head-on. But so as not to interrupt the flow of the argument, I leave this discussion to Section 6: I attribute the deviation in results to empirical specification, a failure to condition on initial conditions and observable demand/supply shifters, and the inclusion of arguably endogenous controls.

4.4 Impact on employment rates, wages and housing costs

My result of perfect unconditional crowd-out appears to imply full local adjustment to immigration. But despite this, I now show that foreign inflows have small *adverse* effects on local employment rates (my sufficient statistic for local welfare, up to the local amenity), consistent with Smith (2012), Edo and Rapoport (2019), Gould (2019) and Monras (2020). Since total population is unaffected, this mechanically implies that total employment (as measured in the census) perversely *contracts* in response to foreign inflows. This is consistent with my unconditional crowd-out estimate δ_1^u exceeding conditional crowd-out δ_1^c: see Section 4.2 above.

In Table 4, I re-estimate the unconditional equation (27) using the same instruments, but replacing the dependent variable with changes in the log (composition-adjusted) employment rate. In column 1, the elasticity of the native employment rate to foreign inflows is -0.21. The coefficient of -0.41 on the lagged rate suggests the effect is largely dissipated within two decades. In Appendix C, I show these effects fall largely on non-graduates, despite educational balance in the foreign inflows and net population outflows.[14]

Again, my specification successfully disentangles the impact of current and historical shocks. As in Table 3, the lagged enclave shift-share m_{rt-1} in column 2 makes little difference, which suggests the lagged employment rate is indeed controlling for initial conditions. But once I drop the employment rate in column 3, m_{rt-1} now takes a positive effect (reflecting the recovery following the initial shock); and as before, the λ_{rt}^F coefficient becomes more negative (which likely reflects omitted demand shocks, correlated with the enclave instrument). Column 4 replaces 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 λ_{rt}^F becomes insignificant.

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

In principle, lower employment rates should be reflected in lower real consumption wages, given the labor supply relationship in (2). As I explain above, local wage deflators are notoriously difficult to construct (and rely on strong theoretical assumptions); and hence my preference for employment rates. But I can at least estimate the effects on nominal wages and housing costs separately. I use mean residualized wages, housing rents and prices, purged of observable demographic and housing characteristics respectively (see Appendix B.4). While I find no impact on nominal wages, this may be difficult

[14]This may be a consequence of more severe undercoverage among low-educated migrants (which understates the labor market pressure on low-educated natives): see Section 5.1.

to interpret in the context of declining employment rates, if it is the lowest paid who leave employment (Card, 2001; Bratsberg and Raaum, 2012). Housing costs do increase however (see also Saiz, 2007), though the standard errors are too large to deliver statistical significance. Taking together, there is weak evidence here of a small decline in real wages.

5 Accounting for undercoverage

5.1 Motivation

My finding of perfect crowd-out is puzzling, even in the context of my own results. First, theory suggests the unconditional effect, δ_1^u, should be smaller than the conditional, δ_1^c; but I find the reverse. Second, perfect crowd-out is indicative of full labor market adjustment, but this is inconsistent with the adverse employment rate effects.

Undercoverage of migrants in the census can help resolve this puzzle. Though the Census Bureau does itself produce estimates of the general undercount (disaggregated by race, though not by migrant status), these adjustments are not applied to census outputs or microdata weights (Clark and Moul, 2003). Among migrants, coverage is especially patchy for the undocumented (Bean et al., 2001). Surprisingly perhaps, many undocumented migrants do respond to the census (Warren and Passel, 1987), but a significant fraction do not.

The basic idea is the following: if foreign inflows are systematically underestimated, the measured impact of foreign inflows (in the unconditional crowd-out equation) will be upward biased. As an aside, note that undercoverage may also distort structural estimates of immigration effects: to the extent that undercounted migrants are low educated[15], the census will overstate the skill composition of migrants, an important input in most structural models (e.g. Borjas, 2003; Card, 2009; Ottaviano and Peri, 2012).

I begin this section by reviewing existing evidence on the extent of undercoverage. I then offer a simple model of undercoverage: this shows that controlling for employment growth (as in the conditional crowd-out equation) should mostly eliminate the bias. In Section 5.4, I offer supporting evidence for this claim, based on heterogeneity in crowd-out estimates over time. In Section 5.5, I show how I can identify the size of bias using the census data *alone*, exploiting direct estimates of the elasticity η of employment to local population. And I conclude in Section 5.6 by quantifying the contributions of mobility and employment to local adjustment, for different levels of undercoverage bias.

[15]For example, Borjas (2017) finds that undocumented migrants have half the college share of legal migrants; and undercoverage is understood to be more severe for the undocumented (Bean et al., 2001).

5.2 Existing evidence on undercoverage

Quantifying the extent of undercoverage is of course challenging. But the existing evidence suggests it is potentially very severe, at least in earlier census years. By comparing native and Mexican-born death rates, Borjas, Freeman and Lang (1991) conclude that 25% of Mexicans overall were missing from the 1980 census.[16] Based on their numbers, the undercount for Mexicans aged 16-64 is somewhat higher: about 32%.[17] But since I wish to estimate the effect of foreign *inflows*, what matters for my analysis is undercoverage among specifically *new* migrants (with up to 10 years in the US). If we are willing to assume that *all* uncounted Mexicans in 1980 arrived after 1970 (this is certainly true of the vast majority: see Borjas, Freeman and Lang, 1991), we can conclude that *more than half* of new Mexican migrants aged 16-64 may have been missing.[18] It is also worth emphasizing that uncounted Mexicans are disproportionately men (Bean, King and Passel, 1983; Van Hook and Bean, 1998*b*): this means they will have supplied more labor (and potentially generated more labor market competition and crowd-out) than the average migrant who did appear in the census.

Though undercoverage of new migrant inflows was potentially very severe in 1980, the Census Bureau has actively sought to reduce this problem over time (Clark and Moul, 2003). Consider, for example, the undercoverage rate among *undocumented* Mexicans (a particular focus of the literature): while Borjas, Freeman and Lang (1991) place this at 40% in 1980, Van Hook and Bean (1998*b*) find it shrank to 30% in 1990 (under comparable assumptions); and based on an external Los Angeles survey, Marcelli and Ong (2002) find an undercount of 10-15% in 2000 (Card and Lewis, 2007). At the same time, the amnesty of 1986 (under the Immigration Reform and Control Act) will have greatly improved coverage in 1990: Van Hook and Bean (1998*a*) estimate the undocumented share of Mexican population fell from 51% in 1980 to just 31% in 1990; but this share rebounded swiftly in the 1990s due to large undocumented immigration.

5.3 Model of undercoverage bias

I now extend my model to account for undercoverage. As in Section 2.6, I will abstract from the initial employment rate and supply/amenity controls in the crowd-out equations

[16] Others have arrived at similar numbers, based on specialized surveys of local populations or analysis of the Mexican census (Van Hook and Bean, 1998*b*). Among migrants, Mexicans are disproportionately likely to be undocumented. But what matters for my application is the undercount bias elicited by the *enclave shift-share* (rather than the national average); and foreign inflows to CZs with the largest enclaves (in California and Texas) are dominated by Mexicans.

[17] Based on their Table 2.2, there are 1.69m Mexicans aged 16-64 who appeared in the 1980 census, and 781,000 who were missing from the census. This implies an undercoverage rate of $\frac{781}{1690+781} = 32\%$.

[18] Among Mexican-born individuals aged 16-64 in the 1980 census microdata, 55% arrived in the last ten years. This group therefore numbers $55\% \times 1.69m = 930,000$ (see footnote 17). So if there are 781,000 missing from the census (see footnote 17), the undercoverage rate among new Mexican migrants (with up to ten years in the US) aged 16-64 is $\frac{930}{930+781} = 54\%$.

(one may interpret all variables in the analysis as residuals, conditional on these controls): this restricts attention to the contemporaneous response to immigration shocks, which accounts for the bulk of local adjustment. Consider first the simplified unconditional crowd-out equation in (16). Suppose I do not observe the true foreign inflow λ^F, but rather $\hat{\lambda}^F_{rt} = (1-\pi)\,\lambda^F_{rt}$, where $\pi \in [0,1]$ is the fraction of new migrants (arriving since $t-1$) who are missing in the census. And suppose I estimate the model with $\hat{\lambda}^F_{rt}$ on the right hand side:

$$\lambda^I_{rt} = -\hat{\delta}^u_1 \hat{\lambda}^F_{rt} + \hat{\delta}^u_2 \Delta z^d_{rt} \tag{28}$$

The biased crowd-out estimate $\hat{\delta}^u_1$ will exceed the true effect δ^u_1 by fraction π:

$$\hat{\delta}^u_1 - \delta^u_1 = \pi \hat{\delta}^u_1 \tag{29}$$

Now consider the simplified conditional equation in (14). As before, I only observe $\hat{\lambda}^F_{rt} = (1-\pi)\,\lambda^F_{rt}$. But crucially, my data will also understate employment growth, Δn_{rt}. In line with the model (and as Table 4 suggests), suppose natives and migrants face identical changes in (composition-adjusted) employment rates. Then, observed employment growth will be: $\Delta \hat{n}_{rt} = \Delta\left(n_{rt} - l_{rt}\right) + \lambda^I_{rt} + \hat{\lambda}^F_{rt} = \Delta n_{rt} - \pi \lambda^F_{rt}$. Now, suppose I estimate the conditional equation using the observed (but mismeasured) $\hat{\lambda}^F_{rt}$ and $\Delta \hat{n}_{rt}$:

$$\lambda^I_{rt} = -\hat{\delta}^c_1 \hat{\lambda}^F_{rt} + \hat{\delta}^c_2 \Delta \hat{n}_{rt} \tag{30}$$

As I show in Appendix E.1, the bias in the estimators $\hat{\delta}^c_1$ and $\hat{\delta}^c_2$ can be written as:

$$\hat{\delta}^c_1 - \delta^c_1 = \pi\left(\hat{\delta}^c_1 - \hat{\delta}^c_2\right) \tag{31}$$

$$\hat{\delta}^c_2 - \delta^c_2 = 0 \tag{32}$$

The coefficient $\hat{\delta}^c_2$ on measured employment $\Delta \hat{n}_{rt}$ in (30) is unbiased: intuitively, the bias in measured foreign inflows $\hat{\lambda}^F_{rt}$ partials out the bias in employment growth $\Delta \hat{n}_{rt}$. Under the baseline assumption that $\delta^c_1 = \delta^c_2$ (i.e. in the absence of disamenity effects: see Section 2.4), it must then be that $\hat{\delta}^c_1 = \hat{\delta}^c_2$; so given (31), the conditional crowd-out effect $\hat{\delta}^c_1$ will also be unbiased - for similar reasons. In practice, column 2 of Table 3 (i.e. the IV estimates) rejects the claim that $\hat{\delta}^c_1 = \hat{\delta}^c_2$, but the discrepancy is small. My estimates suggest the bias in conditional crowd-out $\hat{\delta}^c_1$ is just 20% of the bias (π) in unconditional crowd-out: $\frac{\hat{\delta}^c_1 - \delta^c_1}{\hat{\delta}^c_1} = \frac{\hat{\delta}^c_1 - \hat{\delta}^c_2}{\hat{\delta}^c_1}\pi = \frac{0.913-0.743}{0.913}\pi = 0.19\pi$. That is, conditioning on employment growth eliminates the bulk of the bias in the crowd-out equation.

5.4 Testing the undercoverage model: Crowd-out by decade

Given the large improvements in coverage documented above, the bias π should be significantly lower in more recent decades. Based on my model, one should then expect a large

decline in estimated *unconditional* crowd-out $\hat{\delta}_1^u$; but only a small decline in *conditional* crowd-out $\hat{\delta}_1^c$, where the employment control partials out most of the bias.

To test this claim, I estimate heterogeneity by decade in conditional and unconditional crowd-out. I present my results in Table 5, restricting attention to the coefficient on foreign inflow. Columns 1 and 4 are identical to the basic IV specifications of Table 3 (columns 2 and 4). In columns 2 and 5, I include interactions between the foreign inflow and decade effects (my additional instruments are interactions between the enclave shift-share m_{rt} and the same decade effects), with the 1960s interaction omitted. Column 2 reveals no clear time trend in conditional crowd-out; but unconditional crowd-out becomes visibly smaller from the 1980s. Note the 1980s inflows are measured in the 1990 census, following the amnesty and (temporary) steep decline in the undocumented share of migrants.

Having said that, the standard errors are too large to identify significant differences between the 1960s and any given decade. In columns 3 and 6, I include a single interaction between the foreign inflow and a dummy for the 1980-2010 period (which I also interact with the enclave instrument); and the effects are now statistically significant. Conditional crowd-out drops from 1.1 pre-1980 to 0.9 post-1980. But as the model predicts, the decline in unconditional crowd-out is much larger: from 1.8 to 1.0.

5.5 Identification of undercoverage bias π

Table 5 offers indirect evidence of undercoverage bias in my crowd-out estimates. But using the crowd-out estimates alone (i.e. $\hat{\delta}_1^u$, $\hat{\delta}_1^c$, $\hat{\delta}_2^c$), it is not possible to identify a value for π. This is because the difference between conditional and unconditional crowd-out can be attributed to *both* undercoverage bias π *and* the employment elasticity η; and without further information, it is not possible to disentangle the two. However, it is possible to identify η by directly estimating the employment response to population (i.e. equation (11)); and using this information I am able to point identify π.

To show the identification problem formally, replace the true δ_1^u, δ_1^c and δ_2^c in (17) with the biased estimators ($\hat{\delta}_1^u$, $\hat{\delta}_1^c$, $\hat{\delta}_2^c$), using equations (29), (31) and (32). Rearranging, this gives an expression for the bias π in terms of the crowd-out estimators and employment elasticity η:

$$\pi = 1 - \frac{(1-\eta)\,\hat{\delta}_2^c}{\hat{\delta}_2^c - \hat{\delta}_1^c + \left(1 - \eta\hat{\delta}_2^c\right)\hat{\delta}_1^u} \tag{33}$$

Equation (33) describes a positive relationship between π and η, for given $\hat{\delta}_1^u$, $\hat{\delta}_1^c$ and $\hat{\delta}_2^c$. Intuitively, a larger employment elasticity η moderates the *true* unconditional crowd-out δ_1^u (as in equation (17)), so I require more bias π to account for any given estimate of $\hat{\delta}_1^u$.

For illustration, I have plotted the implied relationship between π and η in Figure 1, based on my IV estimates from Table 3: $\hat{\delta}_1^c = 0.913$ and $\hat{\delta}_2^c = 0.743$ (column 2), and $\hat{\delta}_1^u = 1.096$ (column 4). The shaded area illustrates 95% confidence intervals (with standard errors computed by the delta method[19]), for given η. Assuming the employment elasticity η exceeds 0, the model imposes a lower bound on the undercoverage bias π of 0.2 (with a 0.07 standard error). This lower bound ensures the *true* unconditional crowd-out δ_1^u does not exceed conditional crowd-out δ_1^c, as is necessary for a positive employment response. And as η goes to 1 (implying employment takes the full burden of adjustment, so there is *no* mobility response), π must also go to 1 (to ensure the *true* unconditional response is indeed 0).

Based on (33), knowledge of the employment elasticity η (with respect to population) is sufficient to identify the undercoverage bias π. This elasticity can be estimated directly, using the employment equation (11). Amior and Manning (2018) estimate η as 0.79, using January temperature as an instrument for population growth (following Beaudry, Green and Sand, 2018). In Appendix E.2, I extend Amior and Manning's analysis by allowing for distinct employment responses to foreign and (net) internal inflows, using the enclave shift-share as an additional instrument. Since I am studying crowd-out in response to immigration, I focus on the elasticity to foreign inflows: this is 0.61.[20] As I show in Appendix E.3, this estimate is itself biased upwards by undercoverage. The form of the bias is known however, and I derive a "true" employment elasticity η of 0.44. Based on Figure 1, this would imply an undercoverage bias π of 0.27 (with a standard error of 0.05). This seems reasonable in the context of the evidence described in Section 5.2.

5.6 Quantification of local adjustment mechanisms

For given undercoverage bias π, I can now use equation (18) and my crowd-out estimates ($\hat{\delta}_1^u$, $\hat{\delta}_1^c$ and $\hat{\delta}_2^c$) to disentangle the contributions of internal mobility and labor demand to local adjustment, contemporaneous with a 1-point foreign inflow λ_{rt}^F. Given uncertainty over the true value of π, I estimate these contributions in Table 6 for a range of values: $\pi = 0$ (i.e. zero bias), $\pi = 0.197$ (the lower bound, commensurate with $\eta = 0$: see Figure 1), $\pi = 0.27$ (my preferred value, commensurate with $\eta = 0.44$), $\pi = 0.4$ (a high value), and $\pi = 1$ (the extreme case, for illustration only: zero coverage of new migrants). As before, standard errors are computed using the delta method: see the table notes.

[19]Standard errors account for dependence between coefficient estimates across the conditional and unconditional equations. See footnote 12 for details.

[20]I estimate employment elasticities of 0.61 to foreign inflows and 0.78 to (net) internal inflows. If natives and migrants supply identical labor, these should be identical. But since they are identified using divergent sources of variation (i.e. temperature and enclaves), it is perhaps unwise to over-interpret the gap between them.

Rows A and B report estimates of the true δ_1^c and δ_1^u, computed as $\delta_1^c = (1-\pi)\,\hat{\delta}_1^c + \pi\hat{\delta}_2^c$ and $\delta_1^u = (1-\pi)\,\hat{\delta}_1^u$ respectively: see (29) and (31). Recall that δ_1^c is the *counterfactual* mobility response (i.e. with labor demand held fixed, $\eta = 0$); whereas δ_1^u is the *actual* response (see equation (18)). For $\pi = 0$, the crowd-out effects are of course identical to those reported in columns 2 and 4 of Table 3. As π increases, the true effects decrease. Intuitively, to the extent I am underestimating the foreign inflow, the crowd out estimates will be upward biased. As I have discussed above, controlling for employment in the conditional equation removes the bulk of the bias: δ_1^c is much less sensitive to π than δ_1^u. As π goes to 1, δ_1^u goes to zero; whereas δ_1^c is bounded below at 0.74 (i.e. the $\hat{\delta}_2^c$ estimate).

For $\pi = 0$, δ_1^u is perversely larger than the counterfactual mobility response δ_1^c (and significantly so): see row C. This is reflected in the perversely negative employment response to foreign inflows in row D (which is equal to $\frac{\delta_1^c - \delta_1^u}{\delta_2^c}$, in line with (18)). But this effect is flipped for π above 0.2, at which point δ_1^c exceeds δ_1^u: thereafter, the employment response in row D turns positive, and converges to 1 with π. Intuitively, the differential between δ_1^c and δ_1^u may be attributed to either the employment elasticity η or undercoverage bias π; so as π increases, this must be offset by a more positive η.

Row E describes total local adjustment, contemporaneous with a 1-point foreign inflow. This is the sum of the mobility response (i.e. unconditional crowd-out δ_1^u, from Panel B) and the employment response (i.e. $\frac{\delta_1^c - \delta_1^u}{\delta_2^c}$, from Panel D). Overall, the positive slope in the employment response (with respect to π) dominates the negative slope in the mobility response; so total adjustment is increasing in π. Still, the effect of π is small: total adjustment is close to 0.9, for any value of π.

Row F describes the overall impact of foreign inflows on the employment rate. This is simply total adjustment minus 1: see equation (18). Since total adjustment is increasing in π, the employment rate effect is decreasing in magnitude. For my preferred π of 0.27, I predict a -0.11 effect (based on my crowd-out estimates), with a standard error of 0.04. This *predicted* effect can be compared to my *direct* estimates of the employment rate effect in Table 4 (column 1): this offers a testable overidentifying restriction. Taking my IV estimate of -0.21 (for natives), and multiplying this by $(1-\pi) = 0.73$ to correct for undercoverage bias, I have 0.15, which falls well within the confidence interval of Panel F (at $\pi = 0.27$). This offers further support for the model's fit.

Finally, Panel G reports the mobility response (Panel B) as a share of total adjustment (Panel E). At $\pi = 0.2$, population mobility accounts for 100% of the contemporaneous adjustment to foreign inflows; and this converges to zero as π goes to 1. But for reasonable values of π, the population share of adjustment is consistently high. For example, at my preferred π of 0.27, mobility accounts for 90% (with a standard error of 9%). One may be surprised the contribution of employment (i.e. 10%) is so small, despite the large

employment elasticity η (equal to 0.44 for $\pi = 0.27$). But as the algebra in (17) shows, the employment response may have little traction if population *itself* is very elastic. And indeed, the evidence shows the same is true of local adjustment to labor *demand* shocks: here also, the burden of adjustment falls mostly on population rather than employment (Blanchard and Katz, 1992; Hornbeck, 2012; Amior and Manning, 2018).

6 Robustness of unconditional crowd-out

Above, I have argued that population mobility accounts for the bulk of local adjustment to immigration shocks, consistent with the evidence on adjustment to labor demand shocks. In the presence of undercoverage, my estimates of the mobility response are upward biased. But for plausible levels of bias, mobility continues to play the dominant role. Ultimately, this is a consequence of my large unconditional crowd-out estimates.

But why should my crowd-out estimates differ from much of the existing literature? After all, Section 4.3 suggests my identification strategy works well. In this section, I attempt to address this question. I begin in Section 6.1 by studying the sensitivity of my δ_1^u estimates to various sample and specification choices found in other studies, relying on aggregate-level cross-CZ variation (as I have done until now). However, much of the literature exploits variation across skill groups *within* areas: I address this approach in Sections 6.2-6.4. In Appendix G and H, I attempt to reconcile my findings with one well-known aggregate-level study (Card, 2007) and a classic within-area study (Card, 2001). I conclude that empirical specification and choice of controls can help account for apparent discrepancies between my estimates and the broader literature.

6.1 Robustness of aggregate-level estimates

I begin by studying the robustness of my aggregate-level IV estimate of δ_1^u in Table 3 (column 4). In Section 4.3, I focused on local dynamics and tested for pre-trends. I now consider the implications of (i) controls and decadal sample, (ii) excluding old migrants, (iii) a total migrant shock, (iv) outliers, (v) CZ sample and weighting, (vi) cross/within-state variation, (vii) functional form of key variables, (viii) instrument specification (ix) a specification in levels, and (x) controlling for time-invariant local trends. I also consider the possible role of (xi) simultaneity bias in previous estimates. I leave several of these results to Appendix F and G: in these cases, I provide brief summaries here.

(i) Controls and decadal sample. Table 7 assesses the sensitivity of my IV δ_1^u estimate to the choice of controls (as Goldsmith-Pinkham, Sorkin and Swift, 2020, recommend) and decadal sample. With no controls (except year effects), the estimates vary greatly by decade (row A): I find little crowd-out in the 1960s and 1980s (consistent with Butcher and Card, 1991) and much more in other periods (consistent with Filer,

1992, who studies the 1970s). This discrepancy has previously been noted by Butcher and Card (1991), Borjas and Freeman (1991) and Wright, Ellis and Reibel (1997); and it speaks to the concerns of Borjas, Freeman and Katz (1997) about the instability of spatial correlations. My contribution is to show this instability can be resolved by including plausibly exogenous controls. The average effect (column 6) increases from -0.53 to -0.75 when I control for the current Bartik and initial employment rate (row C): i.e. even *without* the amenity controls, estimated crowd-out is substantial. And after including the exogenous amenities (especially climate, which is known to be a key determinant of regional migration: see Rappaport, 2007; Albouy, 2008), I cannot statistically reject (at least) one-for-one crowd-out in any decade: see row H.[21] Intuitively, both natives and migrants are attracted to places with strong labor market conditions and pleasant climate (see also Albouy, Cho and Shappo, forthcoming), so omitting these will bias my δ_1^u estimate towards zero. Crucially, the extent of the omitted variable bias will depend on the peculiarities of each individual decade: this can help explain why different papers (which study different decades) arrive at such different results.

(ii) Excluding old migrants. In row I of Table 7, I replace the dependent variable λ_{rt}^I with the *native* contribution to local population (i.e. excluding "old" migrants, who immigrated before $t-1$: see table notes). Compared to row H, column 6 shows two thirds of the average δ_1^u is driven by natives rather than old migrants. But this overlooks some important heterogeneity: exceptionally, in the 2000s, old migrants account for the entire effect. One possible explanation is large return migration to Mexico in the 2000s (see Hanson, Liu and McIntosh, 2017), driven in part by the construction bust and recession.

(iii) Total migrant shock. Though I summarize immigration shocks in terms of new foreign inflows (in line with my model), much of the literature studies the total change in the migrant stock (i.e. *all* foreign-born individuals). In the final row of Table 7, I replicate the exercise of row I (i.e. studying the *native* response, with the full set of controls) but replace the foreign inflow λ_{rt}^F with the total migrant contribution to local population growth (see table notes). This makes little difference to the results. Since I have now returned old migrants to the model (this time in the regressor), the average effect in column 6 is almost identical to my original specification (-1.1). And I continue to find large negative effects in almost all decades. The one outlier is of course the 2000s, where each new migrant crowds out one previous migrant (see row I); and therefore, exceptionally, the enclave shift-share instrument has no power in this decade.[22]

[21] Crowd-out in row H is especially large before 1980, consistent with my findings in Table 5: I attribute this to larger undercoverage bias.

[22] In the first stage regression for the total migrant contribution, the coefficient on the enclave shift-share m_{rt} in the 2000s is just 0.16 (with a standard error of 0.12).

(iv) Outliers (Appendix F.1). In Appendix Figure A1, I plot my δ_1^u estimates graphically, conditional on the covariates. The effects are visibly not driven by outliers.

(v) CZ sample and weighting (Appendix F.2). In Appendix Table A5, I show the crowding out effect is robust to different CZ samples and weighting choices. Much of the literature focuses on MSAs (to the exclusion of small towns and rural areas); but restricting my sample to the largest 100 CZs does not affect the results. Butcher and Card (1991) and Wright, Ellis and Reibel (1997) find some evidence of larger crowd-out in the top five migrant destinations (Los Angeles, New York, Chicago, Miami and San Francisco), but excluding these also makes little difference. Given the skew in the spatial distribution of foreign inflows (Table 1), one may be concerned the estimates are driven by CZs with unusually large inflows. But, excluding observations with enclave shift-share m_{rt} above 0.1 (the maximum is 0.29) changes little: this is consistent with the patterns in Appendix Figure A1. Finally, my main estimates are weighted by local population share, which ensures they are largely driven by variation across larger CZs; but removing these weights makes little difference.

(vi) Cross/within-state variation (Appendix F.3). Famously, Borjas (2006) finds less crowd-out across states than metro areas. But in Appendix Table A5, I cannot reject one-for-one crowd-out using state-level data (δ_1^u is 0.94). At the same time, controlling for state fixed effects in my CZ data makes little difference to my results. This suggests the CZ-level crowd-out is equally driven by variation across *and* within states.

(vii) Functional form (Appendix F.4). My specification of λ_{rt}^I and λ_{rt}^F is almost identical to Card and DiNardo (2000) and Card (2001), as recommended by Peri and Sparber (2011) and Card and Peri (2016). While they regress $\frac{\Delta L_{rt} - L_{rt}^F}{L_{rt-1}}$ on $\frac{L_{rt}^F}{L_{rt-1}}$, I am regressing $\log\left(\frac{L_{rt} - L_{rt}^F}{L_{rt-1}}\right)$ on $\log\left(\frac{L_{rt-1} + L_{rt}^F}{L_{rt-1}}\right)$, in line with my model. Appendix Table A6 shows this alternative specification makes little difference to my δ_1^u estimate.

(viii) Instrument specification (Appendix F.5). In my main estimates, I base the enclave shift-share in (25) on one-decade-lagged origin shares. But as I show in Appendix Table A6, applying 1960 origin shares to all decades (following the example of Hunt, 2017) makes little difference to the results.

(ix) Specification in levels (Appendix F.6). In line with my model (and also Card, 2001; Peri and Sparber, 2011), I have specified the key variables (i.e. foreign inflows and net internal outflows) relative to the existing population. But as I show in Appendix Table A6, my results are robust to a specification in levels, i.e. regressing $\left(\Delta L_{rt} - L_{rt}^F\right)$ on L_{rt}^F, without normalizing by initial population. As Wright, Ellis and Reibel (1997) note, local population may be an important omitted variable in this specification; but like Wozniak and Murray (2012), I address this by controlling for local fixed effects.[23]

[23]This levels specification can also address possible concerns that my main 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).

(x) Time-invariant local trends (Appendix F.7). As I show in Appendix Table A6, I also cannot reject perfect crowd-out when I control for CZ fixed effects in my main specification. This approach is similar to the double differencing methodology of Borjas, Freeman and Katz (1997) and is recommended by Hong and McLaren (2015). The idea is to pick up time-invariant local *trends* in omitted supply or demand. This is a demanding specification for my short panel, given large persistence in the enclave shift-share; and as Aydemir and Borjas (2011) note, measurement error may be a greater challenge in the presence of fixed effects. But precision does improve substantially when I replace the lagged employment rate control with lagged Bartik and enclave shift-shares.

(xi) Simultaneity bias (Appendix G). Table 7 demonstrates the importance of partialing out observable local conditions, whether related to labor demand or amenities. However, I have been careful to restrict attention to plausibly exogenous controls; and I have relied on instruments to deal with any remaining omitted effects. Understandably though, older studies (such as the seminal work of Wright, Ellis and Reibel, 1997) are more susceptible to endogeneity concerns.[24] A more subtle question is whether to control for lagged population, as in Card (2007). Wright, Ellis and Reibel argue that local population may be an important omitted variable: this is certainly true in their specification, which expresses native and migrant population changes in *absolute* terms (see part ix above). But it is not clear why it should apply to a specification like mine or Card (2007), which studies population flows *relative* to initial population (as recommended by Peri and Sparber, 2011). Indeed, in the presence of omitted demand or amenity shocks, there is reason to believe a lagged population control will generate a spurious negative correlation with the dependent variable $\lambda^I_{rt} = \log \frac{L_{rt}-L^F_{rt}}{L_{rt-1}}$ (where lagged population L_{rt-1} appears in the denominator). As it happens, controlling for lagged population makes no difference to my crowd-out estimate, conditional on my demand or amenity controls. But in the absence of these controls, it does have an important effect. I show in Appendix G that this observation can help reconcile my results with Card (2007).

6.2 Within-area crowd-out: Empirical specification

Above, I study crowd-out at the aggregate CZ-level, in line with the "total effects" approach recommended by Dustmann, Schoenberg and Stuhler (2016). But much of the literature exploits variation in immigration across skill groups *within* areas. In this section, I show theoretically (and demonstrate empirically) that aggregate-level and within-area specifications identify different objects: this offers a means to reconcile differing results.

[24] Wright, Ellis and Reibel (1997) does not instrument for foreign inflows, and rely instead on a timing argument (lagging foreign inflows by 5 years). But this approach cannot be credible if shocks to local demand are heavily persistent, as the evidence suggests (e.g. Blanchard and Katz, 1992; Amior and Manning, 2018). Wright, Ellis and Reibel (1997) and White and Imai (1994) also include arguably endogenous controls on the right-hand side, such as local unemployment, wages, and house prices: to the extent these are correlated with foreign inflows, they are likely to bias the crowd-out effect.

Consider a within-area (superscript w) specification for unconditional crowd-out:

$$\lambda^I_{jrt} = \delta^w_0 - \delta^w_1 \lambda^F_{jrt} + d_{rt} + d_{jt} + \varepsilon_{jrt} \tag{34}$$

where λ^F_{jrt} and λ^I_{jrt} are the foreign and residual contributions to local population growth among skill group j. These are constructed identically to (22) and (23), but using the skill j sample. The d_{rt} in (34) are area-time interacted fixed effects, which absorb local shocks common to all groups; and d_{jt} are skill-time interacted effects, which absorb group-specific national trends.

The coefficient of interest, δ^w_1, identifies the impact of skill-specific foreign inflows λ^F_{jrt} on (the contribution of existing US residents to) local skill composition. Existing estimates of the λ^F_{jrt} effect are typically small and sometimes negative (Card and DiNardo, 2000; Card, 2001, 2005; Cortes, 2008), though Borjas (2006) and Monras (2020) offer alternative views.[25] Either way, a small δ^w_1 is not necessarily inconsistent with large spatial crowd-out - 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, Schoenberg and Stuhler (2016) note, δ^w_1 does not account for the economic impact that new immigrants exert *outside* their own skill group j.

Regarding the latter point, consider a simple example. Suppose area r production of the traded good (priced at P_t) has CES technology (as in e.g. Card, 2001) over skill labor inputs N_{jrt}:

$$Y_{rt} = \left[Z^d_{rt} \left(\sum_j \theta_{jrt} N_{jrt}^{\frac{\sigma-1}{\sigma}} \right) \right]^{\frac{\sigma}{\sigma-1} \cdot \frac{\epsilon^d - 1}{\epsilon^d}} \tag{35}$$

where Z^d_r is an aggregate area r demand shifter, $\epsilon^d \geq 0$ is the aggregate-level elasticity of labor demand (which allows for locally diminishing returns), and $\sigma \geq 0$ is the elasticity of substitution between skill groups (within areas r). In Appendix A.4, I integrate the implied labor demand relationship into a model of local adjustment (akin to Section 2), with an internal population elasticity of γ and elasticity of labor supply ϵ^s common to all skill groups. Abstracting from local dynamics, I show the crowd-out effect δ^w_1 in equation (34) identifies:

$$\delta^w_1 = \frac{(1-\eta^w)\left(1 - \frac{1-e^{-\gamma}}{\gamma}\right)}{1 - \eta^w \left(1 - \frac{1-e^{-\gamma}}{\gamma}\right)} \tag{36}$$

where

$$\eta^w \equiv \frac{\sigma}{\sigma + \epsilon^s} \tag{37}$$

[25] Borjas' (2006) methodology however is disputed by Peri and Sparber (2011) and Card and Peri (2016). Monras (2020) estimates a large negative δ^w_1 following a sudden surge of Mexican immigration, but the effect is small over decadal census intervals.

The form of (36) is identical to aggregate-level crowd-out δ_1^u (compare the λ_{rt}^F coefficient in (13)), with the exception that the aggregate-level elasticity of employment to population, i.e. η, is now replaced by a *within-area* equivalent, η^w.

Comparing (12) to (37), η and η^w (and therefore δ_1^u and δ_1^w) are only equal under very special conditions: specifically if (i) $\sigma = \epsilon^d$ (i.e. the elasticity of substitution is equal to the aggregate-level elasticity of labor demand) and (ii) $\kappa \to \infty$ (i.e. housing is supplied perfectly elasticity). Intuitively, both these conditions eliminate the impact of foreign inflows to one group (in some area r) on other groups: $\sigma = \epsilon^d$ ensures the technology in (35) is additively separable, so there is no *labor market* interaction between skill groups; and $\kappa \to \infty$ ensures local prices are fixed, so there is no *housing market* interaction. Any such interactions will ensure the impact of foreign inflows is diffused across multiple skill groups - and therefore (to some extent) partialed out by the local fixed effects, d_{rt}. Note that removing these fixed effects will not solve the problem: the estimated δ_1^w would then be an awkward mixture between the cross-area and within-area effects (i.e. some function of both η and η^w), which will be even harder to interpret.

One important implication is that the δ_1^w estimate will be sensitive to exactly how the skill groups are defined. In practice, we do not know the "true" skill delineation: this is ultimately a choice the researcher makes. But a finer delineation of skill groups will artificially engender a larger elasticity of substitution σ; and as (36) and (37) show, this will ensure a larger within-area employment elasticity η^w and a smaller crowd-out effect δ_1^w. In the extreme case, as σ becomes very large, the *within-area* crowd-out effect δ_1^w will go to zero - entirely independently of the *aggregate-level* effect δ_1^u.

To summarize, δ_1^w will not in general identify aggregate-level crowd-out δ_1^u for two reasons. First, there may be important local birth cohort effects. And second, even in the absence of cohort effects, the impact of a skill-specific foreign inflow will be diffused across the local economy (through both labor and housing market interactions) and therefore not be fully captured by within-area estimates.

6.3 Estimates of within-area crowd-out δ_1^w

I now demonstrate empirically that *both* local economic interactions *and* local cohort effects make an important contribution to the within-area estimate δ_1^w. I present my estimates in Table 8. Of course, these will be sensitive to any skill-biased undercoverage of migrants - so the actual numbers should be treated with some caution.

To explore the role of local economic interactions, I study four different education-based[26] "skill" delineations (which will likely engender different σ elasticities): (i) college

[26]A potential drawback of education groupings is occupational downgrading of migrants. Card (2001)

graduates / non-graduates; (ii) at least one year of college / no college (as in Monras, 2020); (iii) high school dropouts / all others (Card, 2005; Cortes, 2008); (iv) four groups: dropouts, high school graduates, some college and college graduates (Borjas, 2006).

To explore the role of cohort effects, I compare estimates for (i) pooled census cross-sections (used by most of the literature) and (ii) a longitudinal dimension of the census which isolates the mobility response (used by Card, 2001; Borjas, 2006): respondents were asked where they lived five years ago. This question appears in the 1980, 1990 and 2000 census, yielding information on migratory flows over 1975-1980, 1985-1990 and 1995-2000.[27] For comparability, I restrict the pooled cross-section sample to the same three decades: the 1970s, 1980s and 1990s. Unlike the five-year intervals of my longitudinal data however, the pooled cross-sections consist of decadal (census) intervals.

To exclude education-specific local demand shocks (the θ_{jrt} in (35)), I instrument foreign inflows λ^F_{jrt} in (34) using an education-specific enclave shift-share m_{jrt}, following Card (2001). This is identical to (25) above, but is constructed using the skill j sample alone. As is clear from columns 1 and 4 of Table 8, m_{jrt} is a strong instrument.

In the pooled cross-section data, the effect of foreign inflows is remarkably large and *positive* (so δ^w_1 in (34) is negative), ranging from 1 to 1.5 for the total residual 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 in column 5 (which exclude cohort effects) are consistently *negative*. They also vary considerably in magnitude, from -3.6 for the college graduate/non-graduate delineation (though not significantly different to -1) 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 σ), greater within-area diffusion of labor market effects, and consequently lower estimates of δ^w_1. In particular, if high school dropouts are close substitutes with other non-college workers (Card, 2009), this can explain the relatively low effect in the third row (-0.43).

Using similar longitudinal data though, Card (2001) estimates a mildly positive effect of foreign inflows. In Appendix H, I attribute this to his fine delineation of skill groups (Card uses six imputed occupations, which will admit large productive substitutability

addresses this concern by probabilistically assigning individuals to occupations (based on demographics), separately for natives and migrants. I offer estimates for these imputed occupations in Appendix H.

[27] Previous residence is only classified by state in the 1970 microdata, and the ACS (after 2000) only reports place of residence 12 months previously. Since previous residence is constructed according to where current respondents previously lived, these flows 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 estimates should then understate the extent of crowd-out.

σ) and his choice of right hand side controls (which I argue may be problematic).[28]

6.4 Direct evidence of local cohort effects

The difference between the pooled cross-section and longitudinal estimates is suggestive of large cohort effects. But in Appendix F.8, I offer more direct evidence for this, by exploiting information on individuals' state of birth. Using the same estimating equation (34), 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).

As an example, consider a local inflow of low educated immigrants. Despite a large net outflow of low educated natives, the native college share will typically *contract* relative to elsewhere: the impact of spatial crowd-out on local skill composition is more than offset by a decline in the education levels of local birth cohorts.

Certainly, one may expect low-skilled immigration to raise the return to education and stimulate *greater* skills acquisition (see Hunt, 2017). But the effect can plausibly go the other way: Llull (2018) argues a fall in wages may discourage labor market attachment and the accumulation of human capital.

7 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 in OLS, I find substantial crowd-out of 0.76. It is also educationally "balanced": the college share of both foreign inflows and net outflows resemble the local population. I base my main estimates on CZs, but I find similar effects across states. And it is entirely driven by reduced internal inflows to the affected areas, rather than larger outflows. My results appear to conflict with much of the existing literature, but I show how these estimates can be reconciled.

The magnitude of the effect is puzzling, even in the context of my own results. First, theory predicts that conditional crowd-out (i.e. holding changes in employment fixed) should be smaller than unconditional; but I find the opposite. Second, perfect crowd-out is indicative of full labor market adjustment, but this is inconsistent with the adverse employment rate effects.

[28] Card controls for various demographic means within area-skill cells (age, education, migrants' years in US at baseline), which absorb much of the migration shock's variation. In principle, these controls may be picking up exogenous skill-specific shocks which I have neglected, though it is not clear (ex ante) what these might be. Furthermore, to the extent that the migration shocks and internal responses are persistent over time (as the evidence suggests), there is legitimate concern that they are endogenous to the dependent variable: foreign and internal mobility are liable to shift the demographic characteristics of the area-skill cells. See Appendix H.

I argue that census undercoverage of migrants can resolve these puzzles. This view is consistent with the much larger estimates of unconditional (but importantly, not conditional) crowd-out before 1980, when coverage was poorer. Exploiting my model's structure, I attribute 30% of observed crowd-out to mismeasurement. The remaining effect is largely a consequence of the labor market impact, though I argue disamenity effects and monopsony may also play a role. Though labor demand (itself mediated by the housing market) does cushion the impact of immigration, I show that population mobility accounts for 90% of local labor market adjustment. This result arises from a remarkably simple decomposition, which requires no information on wages or prices, and does not impose any parameter values ex ante. The dominant role of population mobility is consistent with what we know about local adjustment following labor *demand* shocks.

These findings have important methodological implications for the estimation of immigration effects. First, local variation is often exploited to identify the national impact of immigration; but the interpretation of such estimates requires some caution, in light of the near-perfect response from internal mobility. Second, undercoverage will bias upwards reduced form estimates of immigration effects; and it may also cause us to overstate the skill composition of migrants, a key input for structural estimates.

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Appendices

A Derivations of equations in theoretical model

A.1 Conditional crowd-out: Derivation of (9)

Let $x_r(\tau)$ denote the value of some variable x in area r at time τ. Equation (8) can then be written as:

$$\frac{\partial e^{\gamma\tau} l_r(\tau)}{\partial \tau}|_{\tau=t} = e^{\gamma\tau}\lambda_r^F(\tau) + \gamma e^{\gamma\tau}\left[n_r(\tau) - z_r^s(\tau) + \epsilon^s a_r(\tau)\right] \tag{A1}$$

Integrating both sides between $\tau = t-1$ and $\tau = t$:

$$e^{\gamma t} l_r(t) - e^{\gamma(t-1)} l_r(t-1) = \int_{t-1}^{t} e^{\gamma\tau}\left[\lambda_r^F(\tau) + \gamma n_r(\tau) - \gamma z_r^s(\tau) + \gamma\epsilon^s a_r(\tau)\right] d\tau \tag{A2}$$

Rearranging:

$$\begin{aligned} l_r(t) - l_r(t-1) &= \int_{t-1}^{t} e^{-\gamma(t-\tau)}\left[\lambda_r^F(\tau) + \gamma n_r(\tau) - \gamma n_r(t-1) - \gamma z_r^s(\tau) + \gamma\epsilon^s a_r(t)\right] d\tau \\ &\quad + \left(1 - e^{-\gamma}\right)\left[n_r(t-1) - l_r(t-1)\right] \end{aligned} \tag{A3}$$

and again:

$$\begin{aligned} l_r(t) - l_r(t-1) &= \int_{t-1}^{t} e^{-\gamma(t-\tau)}\lambda_r^F(\tau)\, d\tau + \left[n_r(t) - n_r(t-1)\right] \\ &\quad - \left[z_r^s(t) - z_r^s(t-1)\right] + \epsilon^s\left[a_r(t) - a_r(t-1)\right] \\ &\quad - \int_{t-1}^{t} e^{\gamma(\tau-t)}\left[\dot{n}_r(\tau) - \dot{z}_r^s(\tau) + \epsilon^s\dot{a}_r(\tau)\right] d\tau \\ &\quad + \left(1 - e^{-\gamma}\right)\left[n_r(t-1) - l_r(t-1) - z_r^s(t-1) + \epsilon^s a_r(t-1)\right] \end{aligned} \tag{A4}$$

Now, assume the foreign inflow $\lambda_r^F(\tau)$ is constant and equal to λ_{rt}^F within the $(t-1, t]$ unit interval. And assume also that employment n_r, the supply shifter z_r^s and the amenity effect a_r change at a constant rate over the interval. This implies:

$$\begin{aligned} l_r(t) - l_r(t-1) &= \left(\frac{1 - e^{-\gamma}}{\gamma}\right)\lambda_{rt}^F \\ &\quad + \left(1 - \frac{1 - e^{-\gamma}}{\gamma}\right)\left[n_r(t) - n_r(t-1) - z_r^s(t) + z_r^s(t-1) + \epsilon^s\left(a_r(t) - a_r(t-1)\right)\right] \\ &\quad + \left(1 - e^{-\gamma}\right)\left[n_r(t-1) - l_r(t-1) - z_r^s(t-1) + \epsilon^s a_r(t-1)\right] \end{aligned} \tag{A5}$$

Equation (9) then follows after subtracting the foreign inflow λ_{rt}^F on both sides.

A.2 Housing market specification: Derivation of (10)

Suppose workers have Cobb-Douglas preferences over the traded good and housing, so they spend a fixed fraction ν of their income on housing. This implies a simple linear

expression for the local price index:

$$p_{rt} = \nu p^h_{rt} + (1-\nu)\, p_t \tag{A6}$$

For simplicity, suppose there are no income transfers across areas. Then, housing demand in area r can be written as:

$$H^d_{rt} = \nu \frac{W_{rt} N_{rt}}{P^h_{rt}} \tag{A7}$$

and in logarithms:

$$h^d_{rt} = \log \nu + w_{rt} + n_{rt} - p^h_{rt} \tag{A8}$$

In turn, suppose housing supply can be written as:

$$h^s_{rt} = \epsilon^{hs} \left(p^h_{rt} - p_t \right) \tag{A9}$$

To ease the exposition, I have assumed in (A9) that housing production does not depend on local labor; but see the Online Appendices of Amior and Manning (2018) for an extension where it does. Equating supply and demand, and substituting (A6) for p^h_{rt}, gives:

$$p_{rt} - p_t = \frac{\nu}{1 - \nu + \epsilon^{hs}} \left[\log \nu + \frac{1}{\epsilon^s} \left(n_{rt} - l_{rt} - z^s_{rt} \right) + n_{rt} \right] \tag{A10}$$

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

$$\Delta \left(p_{rt} - p_t \right) = \frac{1}{\kappa} \left[\frac{1}{\epsilon^s} \left(\Delta n_{rt} - \Delta l_{rt} - \Delta z^s_{rt} \right) + \Delta n_{rt} \right] \tag{A11}$$

where

$$\kappa \equiv \frac{1 - \nu + \epsilon^{hs}_r}{\nu} \tag{A12}$$

is increasing in the elasticity of housing supply, ϵ^{hs}_r.

A.3 Unconditional crowd-out: Derivation of (13)

To move from the conditional crowd-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:

$$\Delta n_{rt} = \frac{\epsilon^s}{\epsilon^s + \epsilon^d} \Delta z^d_{rt} + \frac{\epsilon^d}{\epsilon^s + \epsilon^d} \left(\Delta l_{rt} + \Delta z^s_{rt} \right) - \frac{\epsilon^s \epsilon^d}{\epsilon^s + \epsilon^d} \Delta \left(p_{rt} - p_t \right) \tag{A13}$$

Replacing the local price deviation $\Delta \left(p_{rt} - p_t \right)$ with (A11):

$$\Delta n_{rt} = \eta \left(\Delta l_{rt} + \Delta z^s_{rt} \right) + (1 - \eta) \frac{\kappa}{\kappa + \epsilon^d} \Delta z^d_{rt} \tag{A14}$$

and disaggregating local population growth Δl_{rt} into foreign and internal contributions:

$$\Delta n_{rt} = \eta \left(\lambda_{rt}^{F} + \lambda_{rt}^{I} + \Delta z_{rt}^{s} \right) + (1 - \eta) \frac{\kappa}{\kappa + \epsilon^{d}} \Delta z_{rt}^{d} \tag{A15}$$

where

$$\eta \equiv 1 - \left(1 + \frac{\kappa + 1}{\kappa + \epsilon^{d}} \cdot \frac{\epsilon^{d}}{\epsilon^{s}} \right)^{-1} \tag{A16}$$

Equation (13) can then be derived by substituting (A15) for Δn_{rt} in equation (9).

A.4 Within-area crowd-out specification: Derivation of (36)

In this appendix, I set out a multi-skill labor model of local labor market adjustment, and I derive the expression for the within-area crowd-out estimate, δ_1^w, in equation (36). Assuming a competitive labor market, the CES technology in (35) implies the following skill-specific demand for local labor:

$$n_{jr} = \sigma \log \frac{\epsilon^{d} - 1}{\epsilon^{d}} - \sigma \left(w_{jr} - p \right) + \sigma \left(z_{r}^{d} + \log \theta_{jr} \right) + \frac{\epsilon^{d} - \sigma}{\epsilon^{d} - 1} y_{r} \tag{A17}$$

where n_{jr} is log local employment of skill j labor, w_{jr} is the skill-specific local wage, p is the (locally invariant) price of tradables, $z_r^d = \log Z_r^d$ is an aggregate-level local demand shifter, θ_{jr} is a skill-specific local demand shifter, and y_r is the log of total local output Y_r.

Parallel with (2) in Section 2, I also write an equation for skill j labor supply:

$$n_{jr} = l_{jr} + \epsilon^{s} \left(w_{jr} - p_{r} \right) + z_{jr}^{s} \tag{A18}$$

And parallel with (4), suppose that indirect utility for skill j depends on a skill-specific amenity a_{jr} and real consumption wage $(w_{jr} - p_r)$, which itself can be replaced with the employment rate using (A18):

$$\begin{aligned} v_{jr} &= w_{jr} - p_{r} + a_{jr} \\ &= \frac{1}{\epsilon^{s}} \left(n_{jr} - l_{jr} - z_{jr}^{s} \right) + a_{jr} \end{aligned} \tag{A19}$$

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

Skill j subscripts can also be applied to the internal migratory response, equation (7). For simplicity, suppose the elasticity γ is common to all skill groups. So, the resident skill j population adjusts (sluggishly) with elasticity γ to skill-specific differentials in local

utility v_{jr}.

$$\lambda_{jr}^{I} = \gamma \left(n_{jr} - l_{jr} - z_{jr}^{s} + \epsilon^{s} a_{jr} \right) \tag{A20}$$

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

$$\begin{aligned} \lambda_{jrt}^{I} &= \left(1 - \frac{1 - e^{-\gamma}}{\gamma^{I}} \right) \left(\Delta n_{jrt} - \lambda_{jrt}^{F} - \Delta z_{jrt}^{s} + \epsilon^{s} \Delta a_{jrt} \right) \\ &\quad + \left(1 - e^{-\gamma} \right) \left(n_{jrt-1} - l_{jrt-1} - z_{jrt-1}^{s} + \epsilon^{s} a_{jrt-1} \right) \end{aligned} \tag{A21}$$

where λ_{jrt}^{F} is the skill-specific foreign inflow. To derive the unconditional crowding out effect, I require a solution for local skill-specific employment Δn_{jrt}. Given (A18) and the skill demand relationship in (A17), this can be characterized as:

$$\begin{aligned} \Delta n_{jrt} &= \frac{\sigma \epsilon^{s}}{\sigma + \epsilon^{s}} \left(\Delta z_{rt}^{d} + \Delta \log \theta_{jrt} - \Delta p_{rt} + \Delta p_{t} \right) \\ &\quad + \frac{\sigma}{\sigma + \epsilon^{s}} \left(\Delta l_{jrt} + \Delta z_{jrt}^{s} \right) + \frac{\epsilon^{s}}{\sigma + \epsilon^{s}} \Delta y_{rt} \end{aligned} \tag{A22}$$

Substituting this for Δn_{jrt} in (A21) yields:

$$\begin{aligned} \lambda_{jrt}^{I} &= \frac{(1 - \eta^{w}) \left(1 - \frac{1 - e^{-\gamma}}{\gamma} \right)}{1 - \eta^{w} \left(1 - \frac{1 - e^{-\gamma}}{\gamma} \right)} \left[\sigma \Delta \log \theta_{jrt} - \lambda_{jrt}^{F} - \Delta z_{jrt}^{s} + (\sigma + \epsilon^{s}) \Delta a_{jrt} \right] \\ &\quad + \frac{(1 - \eta^{w}) \left(1 - \frac{1 - e^{-\gamma}}{\gamma} \right)}{1 - \eta^{w} \left(1 - \frac{1 - e^{-\gamma}}{\gamma} \right)} \left[\Delta y_{rt} + \sigma \left(\Delta z_{rt}^{d} - \Delta p_{rt} + \Delta p_{t} \right) \right] \\ &\quad + \frac{1 - e^{-\gamma}}{1 - \eta^{w} \left(1 - \frac{1 - e^{-\gamma}}{\gamma} \right)} \left(n_{jrt-1} - l_{jrt-1} - z_{jrt-1}^{s} + \epsilon^{s} a_{jrt-1} \right) \end{aligned} \tag{A23}$$

where

$$\eta^{w} \equiv \frac{\sigma}{\sigma + \epsilon^{s}} \tag{A24}$$

is the *within-area* elasticity of employment with respect to population, analogous to the aggregate-level η in (A16).

Now consider how this maps onto the within-area empirical specification (34). The area-time fixed effects d_{rt} will absorb the contents of the second line of (A23). The skill-time fixed effects d_{jt} will absorb any (national-level) skill-time varying components of: (i) the skill-specific demand shock $\Delta \log \theta_{jrt}$, (ii) the skill-specific supply shock Δz_{jrt}^{s}, (iii) the skill-specific amenity shock Δa_{jrt}, and (iv) the initial conditions on the final line of (A23). All remaining variation will fall into the error term ε_{jrt}, so the IV exclusion restriction requires that it is uncorrelated with the skill-specific enclave shift-share, m_{jrt}. Under these conditions, the coefficient of interest δ_{1}^{w} will identify the coefficient on λ_{jrt}^{F}

in (A23):

$$\delta_1^w = \frac{(1-\eta^w)\left(1-\frac{1-e^{-\gamma}}{\gamma}\right)}{1-\eta^w\left(1-\frac{1-e^{-\gamma}}{\gamma}\right)} \tag{A25}$$

which is equation (36).

B Data construction

B.1 Population

The data processing builds on the earlier work of Amior and Manning (2018). Local population counts for 16-64s are based on published county-level statistics, taken from the National Historical Geographic Information System (NHGIS: Manson et al., 2017). The precise tables references are given in the Online Appendices of Amior and Manning (2018). I group counties together to form Commuting Zones (CZs), following the scheme of Tolbert and Sizer (1996).[29]

I disaggregate these local population stocks into demographic components: "new" migrants (in the US for 10 years or less), "old" migrants, natives, and education-by-nativity groups. To this end, I rely on local population shares computed from the Integrated Public Use Microdata Series (IPUMS: Ruggles et al., 2017) microdata samples. For the 2010 cross-section, I use the American Community Surveys (ACS) of 2009, 2010 and 2011 (pooled together); I use the 5% census extracts for 2000, 1990, 1980 and 1960; and the (pooled) forms 1 and 2 metro 1% extracts of 1970. The main difficulty here is that the sub-state geographical identifiers in the IPUMS microdata do not precisely identify CZs, and these identifiers also vary across years. Following Autor and Dorn (2013) and Autor, Dorn and Hanson (2013), I impute CZ-level data by weighting estimates with population counts at the intersection of CZs and these identifiers. The sources for these intersection population counts can be found in the Online Appendices of Amior and Manning (2018).

B.2 Employment

Unlike Amior and Manning (2018), but similarly to Amior (2020), local employment stocks (for 16-64s) are adjusted for demographic composition. The first step is to run individual-level probit regressions of a binary employment variable on detailed demographic characteristics (listed below) and local fixed effects, separately for each microdata census cross-section (for 1960-2000) and the pooled ACS sample (of 2009-11). The local fixed effects correspond to the finest available geographical units available in each

[29] I make one minor modification to this scheme, to facilitate consistent definitions over time: I include La Paz County (AZ) in the same CZ as Yuma County (AZ).

cross-section; but where geographical units are subsumed within the same CZ, I aggregate them together (to reduce computational demands).

Separately for each cross-section, I then compute the mean predicted employment rate in each geographical unit, for a distribution of demographic characteristics identical to the full national sample:

$$EmpRateAdj_{rt} = \int_i \Omega\left(X_{it}\hat{\theta}_t + \hat{\theta}_{rt}\right) g\left(X_{it}\right) di \tag{A26}$$

where Ω is the normal c.d.f., $\hat{\theta}_{rt}$ are the local fixed effects, $\hat{\theta}_t$ is the vector of coefficients on the individual characteristics X_{it}, and $g\left(X_{it}\right)$ is the density of individuals with these characteristics. I aggregate to CZ-level by taking averages across the available geographical units, weighting by population counts at the intersections (as described in Section B.1).

In the empirical application, I identify $n_{rt} - l_{rt}$ with the log of the composition-adjusted employment rate, $EmpRateAdj_{rt}$. And I identify the log employment level n_{rt} with the sum of the log adjusted employment rate and log population: $\log EmpRateAdj_{rt} + l_{rt}$.

In the probit regressions, I control for the following characteristics: age, age squared, four education indicators[30] (each interacted with age and age squared), a gender dummy (interacted with all previously-mentioned variables), black/Asian/Hispanic indicators (interacted with all previously-mentioned variables), and a foreign-born indicator (also interacted with all previously-mentioned variables). In each cross-section, I also control for years in the US (among the foreign-born), again interacted with all previously mentioned variables. Information on years in the US is not consistently reported in each year, so I use different variables in each year:

ACS 2009-11: Years in US, years in US squared.

Census 2000: Years in US, years in US squared.

Census 1990: The census only reports years in US as a categorical variable. I take the mid-point of each category (and its square), and I also include a dummy for top-category cases.

Census 1980: Same as 1990. Except those who were citizens at birth do not report years in US: I code all these cases with a dummy variable.

Census 1970: Same as 1980. Except some respondents do not report years in US: I code all these non-response cases with a dummy variable. I also include an additional binary indicator for migrants who report living abroad five years previously (based on a different census question), which is available for the full sample. (I exclude foreign-born respondents in the form 2 sample, as they do not report years in US.)

[30] High school graduate (12 years of education), some college education (1 to 3 years of college), undergraduate degree (4 years of college) and postgraduate degree (more than 4 years of college). High school dropouts (less than 12 years of education) are the omitted category.

Census 1960: No information on years in US is available.

B.3 Shift-share instruments

In this section, I offer further details on the data underlying the Bartik industry and enclave shift-shares. The Bartik shift-share b_{rt} is based on a panel of CZ-by-industry employment. My sample for this exercise consists of employed individuals aged 16-64. For industries, I use the IPUMS consistent classification (based on the 1950 census scheme), aggregated to the 2-digit level[31] (with 57 codes). For each cross-section, I collapse the data to industry and the finest available sub-state geographical unit; and I then aggregate to CZ-level using the population weights described in Section B.1.

In turn, I construct the enclave shift-share m_{rt} using a panel of migrant population counts by CZ and 77 countries of origin. Again, I aggregate from the various sub-state geographical identifiers to CZ-level, using appropriate population weights. An important component of m_{rt} is the stock of new migrants (by origin o) who arrived in the US in the previous ten years (outside area r): i.e. $L^F_{o(-r)t}$ in equation (25). This can be constructed directly in all cross-sections from 1970 inclusive (which covers foreign inflows from the 1960s), since these cross-sections report year of arrival. But, for columns 5-8 of Table 3 (which condition on the lagged enclave shift-share), I also need to construct m_{rt} for 1960 (i.e. covering the 1950s inflow). My strategy is to impute the 1950s inflows using cohort changes: this is the difference between (i) the stock of origin o migrants in 1960 aged 16-64 (outside area r) and (ii) the stock of origin o migrants in 1950 aged 6-54 (again, outside r).

B.4 Wages and housing costs

In Section 4.4, I study the impact of immigration on local residualized wages, housing rents and housing prices, all of which I adjust for local (demographic or housing) composition.

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[32], and I compute the mean residual within each

[31] See *https://usa.ipums.org/usa/volii/occ_ind.shtml*. To address inconsistencies between census years, I group all wholesale sectors in a single category, and similarly for public administration and finance/insurance/real estate. I also drop individuals coded to "Not specified manufacturing industries".

[32] 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.

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[33] (restricting attention to prices between the 1st and 99th percentiles, within each geographical unit), 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 costs by taking weighted averages across the geographical units in each microdata sample.

C Heterogeneity in crowd-out and local effects by education

In this appendix, I study heterogeneity by education in the impact of foreign inflows. I replace the dependent variable of (27) with various outcomes Δy_{jrt} specific to education groups j (college graduates, non-graduates), but keep the same aggregate-level immigration shock λ^F_{rt} and controls on the right hand side:

$$\Delta y_{jrt} = \delta^u_{0jt} - \delta^u_{1j}\lambda^F_{rt} + \delta^u_{2j} b_{rt} + \delta^u_{3j}\left(n_{rt-1} - l_{rt-1}\right) + A_r \delta^u_{Ajt} + \varepsilon_{jrt} \tag{A27}$$

This follows the "total effects" approach recommended by Dustmann, Schoenberg and Stuhler (2016). I report IV estimates of δ^u_{1j} by outcome (across columns) and education group j (rows) in Table A1. To adjust education-specific employment rates, wages and housing costs for local composition, I apply the methods of Appendices B.2 and B.4 to the education samples.

Column 1 reports the effects on education-specific population growth Δl_{jrt}. The next two columns disaggregate this change into foreign and residual (i.e. native plus old migrant) contributions to local population: I specify these according to equations (22) and (23), but using the group j sample. Columns 2-3 show the crowd-out effect is educationally "balanced": the college share of both foreign inflows λ^F_{jrt} (elicited by the enclave shift-share) and the residual population response λ^I_{jrt} resemble the existing

[33]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.

population. As a result, there is little change in the relative supply of college graduates (column 1).

Despite this, the adverse effect of foreign inflows on native employment rates falls almost entirely on non-graduates (column 4). This suggests the (balanced) population changes in column 1 are understating the labor market pressure on low educated natives. This may be a consequence of more severe undercoverage of low educated migrants: see Section 5.1.

Finally, columns 6-9 reveal a small positive effect on the wages of graduate natives; but they also face larger growth in housing expenditures. Whether this reflects changes in unobserved housing consumption or prices is open to interpretation. The price interpretation may be relevant if housing units are imperfect substitutes within CZs: for example, Albouy and Zabek (2016) document growing house price dispersion within cities, driven mostly by changes in relative neighborhood values. Coupled with the difficulty of constructing credible local wage deflators, this underscores the advantages of studying welfare effects using local employment rates (as a sufficient statistic).

D Contributions of inflows and outflows to crowd-out

It turns out the crowd-out effect 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 6.3, I exploit the longitudinal dimension of the census: respondents were asked where they were living five years previously. One can in principle construct the relevant variables using microdata, as I do in Section 6.3. 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.[34]

My strategy is to re-estimate the unconditional crowd-out equation (27), but replacing the decadal foreign and residual contributions (to local population growth) with 5-year

[34]I thank Jack DeWaard for sharing the 1965-70 and 1975-80 flow data. I take the 1985-90 flow data from the Socioeconomic Data and Application Center at Columbia University: see the P1 STP-28 tables at *http://sedac.ciesin.columbia.edu/data/set/acrp_enhance-migration-1990*. I construct the 1995-2000 flows using the C2 A1 and B4 A1 tables on the Census 2000 Migration DVD (kindly made available by Kin Koerber); see *https://www.census.gov/population/www/cen2000/migration/mig_dvd.html*. Comparable data is not available for 2005-2010. Note the published flow data is available for individuals aged 15-64, rather than my usual 16-64 sample.

flows. My specification is:

$$\lambda_{rt}^{I5} = \delta_{0t}^{u} + \delta_{1}^{u}\lambda_{rt}^{F5} + \delta_{2}^{u}b_{rt} + \delta_{3}^{u}\left(n_{rt-10} - l_{rt-10}\right) + A_{r}\delta_{At}^{u} + \varepsilon_{rt} \tag{A28}$$

where the t subscript now designates years, rather than decades (as in the main text), and λ_{rt}^{F5} and λ_{rt}^{I5} are respectively the 5-year foreign and internal contributions to changes in log population. These are constructed in line with equations (22) and (23). Specifically, λ_{rt}^{F5} is approximated as $\log\left(\frac{L_{rt-5}+L_{rt}^{F5}}{L_{rt-5}}\right)$, where L_{rt}^{F5} is the 5-year inflow 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). In turn, λ_{rt}^{I} is approximated as $\log\left(\frac{L_{rt-5}+L_{rt}^{Ii5}-L_{rt}^{Io5}}{L_{rt-5}}\right)$, where L_{rt}^{Ii5} and L_{rt}^{Io5} are respectively the 5-year inflows and outflows to/from others parts of the US. Notice that, by construction, $L_{rt} \equiv L_{rt-5} + L_{rt}^{F5} + L_{rt}^{Ii5} - L_{rt}^{Io5}$. 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. But to the extent that emigration is a response to an individual's local economic environment, my estimates should then understate the extent of crowd-out.

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 (27): 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 A2. I instrument λ_{rt}^{F5} using a 5-year enclave 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 6.3). 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 δ_1^u estimate, based on equation (A28). This points to a large crowding out effect (-1.6), somewhat in excess of one-for-one (though not significantly different). In the next two columns, I disaggregate the effect into (approximate) contributions from internal inflows and outflows: column 5 replaces the dependent variable with λ_{rt}^{Ii5}, approximated as $\log\left(\frac{L_{rt-5}+L_{rt}^{Ii5}}{L_{rt-5}}\right)$; and column 6 replaces it with λ_{rt}^{Io5}, approximated as $\log\left(\frac{L_{rt-5}+L_{rt}^{Io5}}{L_{rt-5}}\right)$. The crowding out effect is entirely driven by variation in inflows rather than outflows.

E Undercoverage bias: Supplementary theory and estimates

E.1 Bias in conditional crowd-out estimates

In this section, I show how I derive equations (31) and (32), which describe the bias in the conditional crowd-out estimators, $\hat{\delta}_1^c$ and $\hat{\delta}_2^c$. From Section 5.3, I have the following expressions for the observed (biased) foreign inflow $\hat{\lambda}_{rt}^F$ and employment growth $\Delta\hat{n}_{rt}$:

$$\hat{\lambda}_{rt}^F = (1-\pi)\,\lambda_{rt}^F \tag{A29}$$

$$\Delta\hat{n}_{rt} = \Delta n_{rt} - \pi\lambda_{rt}^F \tag{A30}$$

where λ_{rt}^F and Δn_{rt} are their true counterparts. The true equation for the (net) internal inflow is:

$$\lambda_{rt}^I = -\delta_1^c\lambda_{rt}^F + \delta_2^c\Delta n_{rt} \tag{A31}$$

However, I am only able to estimate:

$$\lambda_{rt}^I = -\hat{\delta}_1^c\hat{\lambda}_{rt}^F + \hat{\delta}_2^c\Delta\hat{n}_{rt} \tag{A32}$$

The estimators $\hat{\delta}_1^c$ and $\hat{\delta}_2^c$ will identify:

$$\begin{pmatrix} -\hat{\delta}_1^c \\ \hat{\delta}_2^c \end{pmatrix} = \begin{pmatrix} Var\left(\hat{\lambda}_{rt}^F\right) & Cov\left(\Delta\hat{n}_{rt}, \hat{\lambda}_{rt}^F\right) \\ Cov\left(\Delta\hat{n}_{rt}, \hat{\lambda}_{rt}^F\right) & Var\left(\Delta\hat{n}_{rt}\right) \end{pmatrix}^{-1} \begin{pmatrix} Cov\left(\hat{\lambda}_{rt}^F, \lambda_{rt}^I\right) \\ Cov\left(\Delta\hat{n}_{rt}, \lambda_{rt}^I\right) \end{pmatrix} \tag{A33}$$

The variance and covariance terms in (A33) can be computed using equations (A29)-(A31):

$$Var\left(\hat{\lambda}_{rt}^F\right) = (1-\pi)^2\,V^F \tag{A34}$$

$$Var\left(\Delta\hat{n}_{rt}\right) = V^n + \pi^2 V^F - 2\pi C^{Fn} \tag{A35}$$

$$Cov\left(\Delta\hat{n}_{rt}, \hat{\lambda}_{rt}^F\right) = (1-\pi)\left(C^{Fn} - \pi V^F\right) \tag{A36}$$

$$Cov\left(\hat{\lambda}_{rt}^F, \lambda_{rt}^I\right) = (1-\pi)\left(-\delta_1^c V^F + \delta_2^c C^{Fn}\right) \tag{A37}$$

$$Cov\left(\Delta\hat{n}_{rt}, \lambda_{rt}^I\right) = \delta_2^c V^n + \pi\delta_1^c V^F - \left(\delta_1^c + \pi\delta_2^c\right) C^{Fn} \tag{A38}$$

where

$$V^F \equiv Var\left(\lambda_{rt}^F\right) \tag{A39}$$

$$V^n \equiv Var\left(\Delta n_{rt}\right) \tag{A40}$$

$$C^{Fn} \equiv Cov\left(\Delta n_{rt}, \lambda_{rt}^F\right) \tag{A41}$$

Substituting (A34)-(A38) into equation (A33) then gives:

$$\begin{pmatrix} -\hat{\delta}_1^c \\ \hat{\delta}_2^c \end{pmatrix} = \begin{pmatrix} (1-\pi)^2 V^F & (1-\pi)\left(C^{Fn} - \pi V^F\right) \\ (1-\pi)\left(C^{Fn} - \pi V^F\right) & V^n + \pi^2 V^F - 2\pi C^{Fn} \end{pmatrix}^{-1} \begin{pmatrix} (1-\pi)\left(-\delta_1^c V^F + \delta_2^c C^{Fn}\right) \\ \delta_2^c V^n + \pi\delta_1^c V^F - \left(\delta_1^c + \pi\delta_2^c\right) C^{Fn} \end{pmatrix}$$
$$= \begin{pmatrix} -\delta_1^c - \frac{\pi}{1-\pi}\left(\delta_1^c - \delta_2^c\right) \\ \delta_2^c \end{pmatrix} \tag{A42}$$

from which equations (31) and (32) follow.

E.2 Direct estimates of employment elasticity η

In this section, I offer direct estimates of the employment elasticity η 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:

$$\Delta n_{rt} = \eta_t + \eta_1 \Delta l_{rt} + \eta_2 \left(n_{rt-1} - l_{rt-1}\right) + \eta_3 b_{rt} + A_r \eta_{At} + \varepsilon_{rt} \tag{A43}$$

The lagged employment rate allows for sluggish adjustment of labor demand to historical shocks: firms should cut employment if labor supply is initially sparse and costly (i.e. if $n_{rt-1} - l_{rt-1}$ is large). The current Bartik b_{rt} on the right-hand side controls 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:

$$\Delta n_{rt} = \eta_t + \eta_{1F} \lambda_{rt}^F + \eta_{1I} \lambda_{rt}^I + \eta_2 \left(n_{rt-1} - l_{rt-1}\right) + \eta_3 b_{rt} + A_r \eta_{At} + \varepsilon_{rt} \tag{A44}$$

I present first stage estimates for both (A43) and (A44) in Table A3, and OLS and IV estimates in Table A4. Clearly, the OLS estimates cannot be interpreted causally. Omitted demand shocks in the errors will generate a confounding positive correlation between employment on the left hand side and population on the right. The natural instrument for population growth Δl_{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 identified in the main text. In column 2, like Beaudry, Green and Sand (2018) 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 specifications, 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 λ^F_{rt} and λ^I_{rt} separately in (A44), I use both the enclave shift-share and January temperature as instruments. As expected, the shift-share has a large positive effect on λ^F_{rt} (column 3 of Table A3) and a large negative effect on λ^I_{rt} (column 4). In contrast, January temperature has a large positive effect on λ^I_{rt}, but matters less for λ^F_{rt}. As before, I instrument the lagged employment rate with the lagged Bartik: columns 5-7 of Table A3 show a strong first stage.

I now turn to the OLS and IV estimates of (A43) and (A44) in Table A4. The OLS elasticity to Δl_{rt} is essentially 1 (column 1), and the effects of λ^F_{rt} and λ^I_{rt} are 0.87 and 0.99 respectively (column 2). Once I apply the instrument, I estimate smaller effects - as one might expect. Column 3, which instruments Δl_{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 Δl_{rt} of 0.74.[35] And in column 4, the effects of λ^F_{rt} and λ^I_{rt} are now 0.61 and 0.78 respectively. If natives and migrants supply identical labor, these should be identical. But since they are identified using divergent sources of variation (i.e. enclaves and temperature), it is perhaps unwise to over-interpret the gap between them.

E.3 Identification of true employment elasticity η and undercoverage bias π

Using my estimates of η from Table A4 , I now show how one can identify the undercoverage bias π. Given I am studying crowd-out in response to *immigration*, I choose to identify η with the elasticity of employment to λ^F_{rt}, i.e. η_{1F} in (A44).

The main challenge is that any undercoverage will bias my estimate of η_{1F} upwards; but the form of this bias is known. To derive an expression for the bias, I follow similar steps to those of Section E.1. Abstracting from contemporaneous shocks and initial conditions in (A44), the true model for employment can be written as:

$$\Delta n_{rt} = \eta_{1F}\lambda^F_{rt} + \eta_{1I}\lambda^I_{rt} \tag{A45}$$

But since I only observe Δn_{rt} and λ^F_{rt} with error, I estimate:

$$\Delta \hat{n}_{rt} = \hat{\eta}_{1F}\hat{\lambda}^F_{rt} + \hat{\eta}_{1I}\lambda^I_{rt} \tag{A46}$$

where $\Delta \hat{n}_{rt}$ and $\hat{\lambda}^F_{rt}$ are defined as in (A29) and (A30). The coefficients $\hat{\eta}_{1F}$ and $\hat{\eta}_{1I}$ will

[35] Amior and Manning (2018) estimate a slightly larger η_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.

identify:

$$\begin{pmatrix} \hat{\eta}_{1F} \\ \hat{\eta}_{1I} \end{pmatrix} = \begin{pmatrix} Var\left(\hat{\lambda}^F_{rt}\right) & Cov\left(\lambda^I_{rt}, \hat{\lambda}^F_{rt}\right) \\ Cov\left(\lambda^I_{rt}, \hat{\lambda}^F_{rt}\right) & Var\left(\lambda^I_{rt}\right) \end{pmatrix}^{-1} \begin{pmatrix} Cov\left(\hat{\lambda}^F_{rt}, \Delta\hat{n}_{rt}\right) \\ Cov\left(\lambda^I_{rt}, \Delta\hat{n}_{rt}\right) \end{pmatrix} \tag{A47}$$

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

$$= \begin{pmatrix} \frac{1}{1-\pi}\eta_{1F} \\ \eta_{1I} \end{pmatrix}$$

where $V^F \equiv Var\left(\lambda^F_{rt}\right)$, $V^I \equiv Var\left(\lambda^I_{rt}\right)$ and $C^{FI} \equiv Cov\left(\lambda^F_{rt}, \lambda^I_{rt}\right)$. It follows from (A47) that $\hat{\eta}_{1I}$ identifies η_{1I}, while $\hat{\eta}_{1F}$ is upward biased:

$$\hat{\eta}_{1F} - \eta_{1F} = \pi\hat{\eta}_{1F} \tag{A48}$$

Equation (A48) describes a negative relationship between the true η_{1F} and π, for a given $\hat{\eta}_{1F}$ estimate (which I set as 0.605, based on column 4 of Table A4). Together with the positive (η, π) relationship described in equation (33) and Figure 1 in the main text (and identifying η with η_{1F}), this yields a two-equation system:

$$\pi = \frac{\hat{\eta}_{1F} - \eta}{\hat{\eta}_{1F}} \tag{A49}$$

$$\pi = 1 - \frac{(1-\eta)\,\hat{\delta}^c_2}{\hat{\delta}^c_2 - \hat{\delta}^c_1 + \left(1 - \eta\hat{\delta}^c_2\right)\hat{\delta}^u_1} \tag{A50}$$

Using my preferred estimates of $\hat{\eta}_{1F} = 0.605$, $\hat{\delta}^c_1 = 0.913$, $\hat{\delta}^c_2 = 0.743$ and $\hat{\delta}^u_1 = 1.096$ (see Section 5.5), I identify the true employment elasticity η as 0.44 and undercoverage bias π as 0.27.

F Robustness of crowd-out: Supplementary estimates

In Section 6 in the main text, I subject my estimates of unconditional crowd-out, δ^u_1, to a broad range of robustness tests. In some cases, I only offer short summaries of these tests: this appendix presents the complete details and also regression tables. Sections F.1-F.7 deal with aggregate-level crowd-out (corresponding to Section 6.1 in the main text); and Section F.8 studies the role of cohort effects in within-area estimates (corresponding to Section 6.4).

F.1 Graphical illustration of crowd-out estimates

One concern is that my δ_1^u estimate may be driven by outliers. To address this question, Figure A1 illustrates the correlation underlying the basic OLS and IV estimates of δ_1^u (in columns 3 and 4 of Table 3).

For the OLS plot, I extract residuals from regressions of both the net internal flow λ_{rt}^I and foreign inflow λ_{rt}^F on the remaining controls: the initial employment rate, the current Bartik shift-share, year effects and the amenity variables (interacted with year effects). And I then plot the λ_{rt}^I residuals against those of λ_{rt}^F.

For IV, I apply the same Frisch-Waugh logic to two-stage least squares. I first generate predictions of the two endogenous variables (the foreign inflow, λ_{rt}^F, and the initial 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 extract residuals from regressions of both λ_{rt}^I and the *predicted* λ_{rt}^F on the remaining controls: the *predicted* initial employment rate, the current Bartik shift-share, year effects and the amenity variables (interacted with year effects). And as before, I then plot the λ_{rt}^I residuals against those of λ_{rt}^F.

The marker size in these figures is proportional to the initial population share weights. The fit lines' (weighted) slopes are identical to the δ_1^u estimates in columns 3 and 4 in Table 3. The standard errors are of course different: I do not cluster errors in Figure A1; and for IV, the naive two-step estimation does not account for sampling error in the first stage. In any case, the main take-away is that the δ_1^u estimates are not visibly driven by outliers.

F.2 CZ sample and weighting

In columns 1-5 of Table A5, I show the crowding out effect is robust to different CZ samples and weighting choices. For comparison, column 1 of Table A5 is identical to the basic IV unconditional crowd-out specification in Table 3 (column 4) in the main text.

My basic 722 CZ sample is comprehensive of the Continental US, and includes many rural areas and small towns. However, much of the literature focuses on metropolitan areas. In column 2, I restrict my sample to the largest 100 CZs (based on the population of 16-64s in 1960): the coefficient estimates are almost identical to column 1. The standard error on the crowd-out effect δ_1^u is also unchanged, but recall these are population-weighted estimates. Butcher and Card (1991) and Wright, Ellis and Reibel

(1997) find some evidence of larger crowd-out in the top five migrant destinations (Los Angeles, New York, Chicago, Miami and San Francisco): column 3 shows the coefficient estimate declines only marginally (to -0.87) when I omit these, though the standard error does now increase to 0.2.

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 4, 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.

Finally, my main estimates are weighted by initial population share, which ensures they are largely driven by variation across larger CZs. But when I remove these weights in column 5, the coefficient is remarkably similar (-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.

F.3 Cross-state and within-state variation

In the main text, I have focused on geographical crowd-out across CZs. Interestingly, using a within-area specification, Borjas (2006) finds the extent of crowd-out is smaller at higher-level geographical units, based on comparisons of estimates for census divisions, states and metropolitan areas. But using my aggregate-level specification, I show there is substantial crowd-out both across and within states: I cannot reject a one-for-one effect in either case.

As before, I base my estimates on equation (27) in the main text, though I replace the lagged employment rate control ($n_{rt-1} - l_{rt-1}$) with lagged Bartik and enclave shift-share controls, b_{rt-1} and m_{rt-1}. This is because I cannot successfully identify the lagged employment rate using the lagged Bartik instrument in the state-level data. For comparison, I include an equivalent CZ specification in column 6, which is identical to column 6 of Table 3 (in the main text). The crowd-out effect is slightly smaller in this specification (-0.79 compared to -1.1 in column 1): as I explain in Section 4.3, this is likely to reflect omitted historical demand shocks.

In column 7, I replicate the column 6 specification using state-level data. In line with my CZ estimates, I control for state-level amenity effects: a binary indicator for a coastal state (ocean or Great Lakes), maximum January temperature, maximum July temperature, mean July relative humidity, and log state-level population density in 1900 (see Section 3). I also control for year effects and a full set of interactions between the amenity variables and year effects. My sample consists of the 48 states of the continental US, with the District of Columbia merged into Maryland. The estimated crowd-out effect in column 7 is a little larger (reaching -0.94); though unsurprisingly, the standard error

increases also.

Column 7 shows there is large crowd-out *across* states. But do we see similar effects *within* states? To address this question, I replicate the column 6 specification (for CZ data) in column 8, but now controlling for state fixed effects. The crowd-out effect is similar to column 7. This suggests that CZ crowd-out is equally driven by variation across *and* within states.

F.4 Functional form

In Table A6, I explore the robustness of my unconditional crowd-out estimates to questions of empirical specification. For comparison, column 1 of Table A6 replicates the basic unconditional IV crowd-out specification in Table 3 (column 4) in the main text.

I begin in column 2 with the choice of functional form. In my basic specification (27), I approximate the foreign and internal contributions (to the change in log population) as $\log\left(\frac{L_{rt-1}+L^F_{rt}}{L_{rt-1}}\right)$ and $\log\left(\frac{L_{rt}-L^F_{rt}}{L_{rt-1}}\right)$ respectively. But much of the literature has taken a first order approximation, defining them as $\frac{L^F_{rt}}{L_{rt-1}}$ and $\frac{\Delta L_{rt}-L^F_{rt}}{L_{rt-1}}$: see e.g. Card (2001), Peri and Sparber (2011) and Card and Peri (2016). Column 2 re-estimates (27) using these definitions; and to maintain symmetry, I replace the instrument with $\frac{\Lambda^F_{rt}}{L_{rt-1}}$, where $\Lambda^F_{rt} \equiv \sum_o \phi^o_{rt-1} L^F_{o(-r)t}$ is the predicted number of incoming migrants between $t-1$ and t: see equation (25). But this makes little difference to the estimate.

F.5 Specification of instruments

In columns 3-4 of Table A6, I consider the specification of my enclave shift-share instrument. I begin with the shift-share's base year. The instrument in column 2 is $\frac{\Lambda^F_{rt}}{L_{rt-1}}$, where $\Lambda^F_{rt} \equiv \sum_o \phi^o_{rt-1} L^F_{o(-r)t}$ is the predicted number of incoming migrants between $t-1$ and t (see Appendix F.4). Notice that here and in the main text (see equation (25)), I use the $t-1$ migrant settlement patterns (in ϕ^o_{rt-1}) to predict foreign inflows in each subsequent decade. But others 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 3, I replace my instrument with $\frac{\Lambda^{F60}_{rt}}{L_{rt-1}}$, where $\Lambda^{F60}_{rt} \equiv \sum_o \phi^o_{r60} L^F_{o(-r)t}$ predicts the migrant inflow based on 1960 settlement patterns, ϕ^o_{r60}, for every decade. The crowd-out effect is now somewhat larger (-1.3), though it is not significantly different from -1. The robustness of my results to this modification should not be surprising: since I am already controlling for local conditions at $t-1$ (using the initial employment rate), an earlier base year should not offer any advantages in terms of exogeneity (to compensate for the loss of predictive power).

Another possible concern is the source of the instrument's predictive power. Suppose the predicted number of incoming migrants, Λ^F_{rt}, is largely noise. Then, variation in L_{rt-1} may generate an artificial positive correlation between the endogenous variable and the instrument: see Clemens and Hunt (2019). To address this concern, in column 4, I replace the instrument (which is expressed *relative* to the initial population) with the predicted inflow of new migrants in *levels*, i.e. Λ^F_{rt}. But again, this has little effect on the crowding out estimate or even its standard error.

F.6 Specification in levels

Related to the previous point, some papers have estimated crowd-out using a specification expressed entirely in levels (e.g. Wright, Ellis and Reibel, 1997; Wozniak and Murray, 2012). Building on equation (27), a specification in levels would be:

$$\Delta L_{rt} - L^F_{rt} = \delta^{uL}_{0t} - \delta^{uL}_1 L^F_{rt} + \delta^{uL}_2 b_{rt} + \delta^{uL}_3 \left(n_{rt-1} - l_{rt-1}\right) + A_r \delta^{uL}_{At} + \varepsilon_{rt} \tag{A51}$$

where the dependent variable is the change in local population, less the stock of new immigrants; and the key regressor L^F_{rt} is simply the number of new immigrants. I estimate (A51) in column 5 of Table A6, yielding a coefficient on L^F_{rt} of just -0.23. However, local population is an important omitted variable in this specification (Wright, Ellis and Reibel, 1997; Peri and Sparber, 2011; Wozniak and Murray, 2012): through a simple scale effect, local population will be correlated with *both* the inflow of new immigrants *and* subsequent (absolute) population change. To address this concern, Wozniak and Murray recommend controlling for local fixed effects. Once I include CZ fixed effects (column 6), the L^F_{rt} effect is again remarkably close to -1. Notice this specification is also immune to the criticism of Clemens and Hunt (2019), described in Appendix F.5, of spurious correlation in the population denominator.

F.7 Controlling for time-invariant local trends

In column 7 of Table A6, 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 principle, the fixed effects should remove time-invariant unobserved components of the amenity, supply or demand *changes* in equation (27). However, their inclusion is empirically demanding in such a short panel, especially given the strong persistence in the enclave 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 crowd-out effect of -0.63 with a very large standard error (0.61).

In column 8, 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} and lagged enclave shift-share m_{rt-1}, similarly to column 6 of Table 3. I now estimate much larger crowd-out of -1.35, with a standard error of just 0.26. This is remarkably precise, given the short length (just five decades) of the CZ panel.

F.8 Cohort effects in within-area estimates

The difference between the pooled cross-section and longitudinal estimates in Table 8 is suggestive of large cohort effects (see Section 6.3). In Table A7, I present more direct evidence for cohort effects (among natives), by exploiting information in the census on natives' place of birth.

I need to conduct this analysis at state level (rather than CZ), because the census only reports place of birth by state. My sample consists of the 48 states of the continental US (with the District of Columbia merged into Maryland) and three decadal observations (over 1970-2000, for comparability with the estimates in Table 8). In columns 1-3 of Table A7, I begin by estimating δ_1^w in equation (34) in a pooled cross-section of states, using the education-specific enclave instrument m_{jrt}. Reassuringly, the first stage and IV estimates are almost identical to the CZ-equivalent in column 1-3 of Table 8.

Recall the dependent variable in column 3, $\lambda_{jrt}^{I,N}$, is the contribution of natives to group j population growth among state r *residents*. In column 4, I now replace this with $\lambda_{BPjrt}^{I,N}$: the contribution of natives to skill j population growth among those *born* (rather than residing) in state r. The column 4 estimates will now describe the contribution of *cohort effects* to state r's education composition (though since a third of Americans live outside their birth state, it should understate any such effects). Remarkably, these numbers are all larger than the state of residence effects in columns 2-3; 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 (i.e. the pure birth cohort effect) than on those *residing* in it (which accounts for both birth cohorts and mobility). This suggests any contribution of internal mobility to the δ_1^w estimate in columns 2-3 is more than fully offset by cohort effects.

G Reconciliation with Card (2007)

G.1 Empirical specification

In this appendix, I attempt to reconcile my results with a well-known recent analysis of aggregate-level crowd-out in Card (2007). Card finds no significant evidence of crowd-out, and I attribute the difference in results to his choice of right hand side controls. He estimates a model of "unconditional crowd-out" (using my terminology) in the 1980s and 1990s, based on an empirical specification akin to (27):

$$\frac{\Delta L_{rt}^{N}}{L_{rt-1}} = \delta_{0t}^{u} - \delta_{1}^{u}\frac{\Delta L_{rt}^{M}}{L_{rt-1}} + \delta_{L}^{u}\log L_{rt-1} + \varepsilon_{rt}^{u} \tag{A52}$$

where the dependent variable is the contribution of natives, L_{rt}^{N}, to local population growth; and the regressor of interest is the contribution of migrants (both new and old), L_{rt}^{M}. To be precise, Card actually uses total population growth $\frac{\Delta L_{rt}}{L_{rt-1}}$ as the dependent variable, but the difference is just cosmetic: it raises the δ_{1}^{u} coefficient by 1 (see Peri and Sparber, 2011). Card also controls for initial log population, $\log L_{rt-1}$, on the right-hand side.

There are a number of differences between my empirical specification (27) and Card's, most of which are relatively trivial. While I study the effect of new immigrants on internal mobility, Card estimates the effect of the total migrant stock (both new and old) on the local native population; but as Table 7 (row J) shows, this makes little difference to the results. Also, Card uses a slightly different functional form for the variables of interest: $\frac{\Delta L_{rt}^{N}}{L_{rt-1}}$ and $\frac{\Delta L_{rt}^{M}}{L_{rt-1}}$, rather than $\log\left(\frac{L_{rt-1}+\Delta L_{rt}^{N}}{L_{rt-1}}\right)$ and $\log\left(\frac{L_{rt-1}+\Delta L_{rt}^{M}}{L_{rt-1}}\right)$; but as I discuss in Appendix F.4, this also makes little difference. In terms of geography, Card studies 100 large cities (rather than my comprehensive CZ sample); but as Appendix F.2 shows, my crowd-out estimates are stable across different CZ samples. Finally, Card instruments $\frac{\Delta L_{rt}^{M}}{L_{rt-1}}$ using the local migrant share in $t-1$, i.e. $\frac{L_{t-1}^{M}}{L_{t-1}}$: this is less targeted than my own instrument in (25), which is constructed within origin groups (as in Card, 2001); but the two are in practice closely correlated.

But unlike the differences listed above, the choice of controls *does* play an important role. There are two particular concerns. First, Card does not control for local demand and amenity shifters, which will draw both native and migrant inflows (and thereby bias the crowd-out effect towards zero). These shocks will not be excluded by the lagged migrant share instrument if they are very persistent (as in e.g. Amior and Manning, 2018) and exert large influence on immigrants' location choices (as in e.g. Amior, 2020).

Second, there are legitimate concerns over the the lagged population control ($\log L_{rt-1}$) which Card does include. Card justifies this control by reference to Wright, Ellis and Reibel (1997), who emphasize that local changes in native population are closely corre-

lated with city size. This is certainly a concern for Wright, Ellis and Reibel, since they specify native population changes in *absolute* terms (so population will generate a simple scale effect: see Appendix F.6); but it is not clear why it should apply to a specification like (A52) which studies native changes *relative* to the initial population level. Indeed, in the presence of omitted amenity or demand shocks, variation in lagged population L_{rt-1} may generate a spurious negative correlation between the $\log L_{rt-1}$ control and the left hand side variable $\frac{\Delta L^N_{rt}}{L_{rt-1}}$ (where population appears in the denominator). Since lagged population is correlated with the instrument (migrants are known to cluster in large cities: Albert and Monras, 2018), this endogenous control may bias the δ^u_1 estimate.

G.2 Estimates

To study this question further, I attempt a reconciliation in Table A8. Following Card, I estimate the model using decadal census data, separately for the 1980-90 period (Panel A) and 1990-2000 (Panel B). In practice, for both the 1980s and 1990s models, Card uses the 1980 migrant share as an instrument and the 1980 population as a control; and for consistency, I do the same.

Card estimates his model using a sample of the largest 100 MSAs and weights observations by 1980 population, with population stocks corresponding to 16-65s. I record his IV estimates (from his Table 3) in column 1 of each panel. For the 1980s, his coefficient on $\frac{\Delta L^M_{rt}}{L_{rt-1}}$ is 0.5 (with a 0.4 standard error), i.e. each new migrant attracts 0.5 additional natives. And for the 1990s, the coefficient is -0.8 (suggesting large crowd-out), though with a large standard error of 0.6 (so there is little information here).[36] Unfortunately, I have been unable to replicate his estimates for the MSA sample. This may be because the MSA definitions in the public microdata are inconsistent over time: Card may have adjusted for this, but I am not sure exactly how. Instead, in column 2, I have replicated his model using the top 100 CZs in my own data (for 16-64s). My estimates for both decades are closer to zero (0.2 and -0.2 respectively), with standard errors of just 0.2. Though the coefficients do differ somewhat from Card's MSA estimates, the difference is not statistically significant (given his large standard errors).

Card controls for 1980 population, but does not report the coefficient: hence the "?" in column 1. In column 2, I estimate this effect as -0.04 or -0.05, with a standard error of just 0.01. I have argued this negative effect may be driven by a spurious correlation between the 1980 population and the denominator of the $\frac{\Delta L^N_{rt}}{L_{rt-1}}$ dependent variable.[37]

[36] Using his population growth dependent variable, these come out as 1.5 and 0.2 respectively, from which I subtract 1. See the final column of Table 3 in Card (2007).

[37] Note the population control in the 1990s model is more accurately described as $\log L_{rt-2}$ rather than $\log L_{rt-1}$. But since population is heavily correlated over time, this makes little difference is practice.

Quantitatively, the estimated effect is consistent with this view. Holding ΔL_{rt}^N fixed, the derivative of $\frac{\Delta L_{rt}^N}{L_{rt-1}}$ with respect to $\log L_{rt-1}$ is simply the negative of $\frac{\Delta L_{rt}^N}{L_{rt-1}}$. And in my 100 CZ sample, $\frac{\Delta L_{rt}^N}{L_{rt-1}}$ has a mean of about 0.05 in the 1980s and 0.06 in the 1990s (which closely matches the estimated $\log L_{rt-1}$ coefficients).

This kind of spurious correlation becomes a problem in the presence of omitted amenity and demand shifters (which may generate sizeable variation in $\log L_{rt-1}$). To the extent these effects are persistent over time, one might account for them by controlling for lagged population *growth* (rather than the lagged *level*). To this end, I control in column 3 additionally for local population in 1970. The effect of 1980 population now takes a large positive coefficient (of about 0.7 in each decade), and the 1970 population takes an equal and opposite coefficient: this is clearly indicative of a lagged growth effect. At the same time, I now begin to see some evidence of crowd-out: the coefficient on $\frac{\Delta L_{rt}^M}{L_{rt-1}}$ drops to -0.5 in the 1980s and -1.1 in the 1990s (though with large standard errors). Of course, a lagged growth control is not an ideal solution: in a dynamic setting, one may continue to worry about endogeneity; and it does not deal with contemporaneous innovations to demand.

In column 4, I remove both population controls. This makes little difference to the crowd-out estimates, but the standard errors do contract (especially for the 1990s). In column 5, I control for a contemporaneous Bartik industry shift-share, which accounts for predictable shifts in local labor demand (given initial industrial composition). This causes the crowd-out effect to expand to one-for-one in the 1980s also. In column 6, I control for the amenity effects described in Section 3 (climate, coastline, historical density, CZ isolation): this does not affect the coefficients (which remain above -1), but reduces the standard errors considerably (to about 0.3). I do not control for the lagged employment rate in this exercise, because I do not have sufficient power to identify its effect (with the lagged Bartik instrument) in this smaller sample.

In column 7, I return the 1980 population control to the model: in the presence of the demand and amenity controls, this now takes a zero effect. This further supports my contention that the population control in column 2 is picking up a spurious correlation with the dependent variable's denominator, driven by omitted demand and amenity effects (which column 7 now holds fixed).

Finally, in column 8, I expand the sample to all 722 CZs. The coefficients are little affected, but the standard errors contract further. This suggests that the crowd-out effects do not depend on the particular CZ sample (consistent with the evidence in Appendix F.2), though a larger sample does improve precision.

H Reconciliation with Card (2001)

H.1 Overview

In this appendix, I attempt to reconcile my results with Card's (2001) seminal analysis of within-area crowd-out, i.e. δ_1^w in (34). As I have argued in Section 6.2, within-area estimates do *not* identify aggregate-level crowd-out, which is the focus of my paper. This is because they do not account for the local impact which immigrants exert *outside* their skill group; and hence, as I show in Section 6.3, they are remarkably sensitive to the chosen delineation of skill groups (which determines the degree of productive substitutability, σ).

Nevertheless, Card's (2001) within-area estimates do appear to conflict with my own in Table 8. When I exploit longitudinal residential information in the US census (which neutralizes the contribution of cohort effects), I do estimate large negative effects for at least some skill group delineations. However, using similar longitudinal data, Card estimates a small *positive* effect of foreign inflows, with each new immigrant to an area-skill cell *attracting* (on net) 0.25 additional residents.

In this appendix, I attempt to reconcile my results with his. The divergence of our estimates is mostly explained by two factors. First, Card uses a fine skill delineation with six imputed occupation groups (which will admit a large degree of productive substitutability σ). Indeed, as my model in Section 6.2 predicts, I show the crowd-out effects are considerably larger for more aggregated skill delineations. Second, Card controls for a range of cell-level demographic characteristics, which absorb much of the migration shock's variation. In principle, these controls may be picking up exogenous skill-specific shocks which I have neglected, though it is not clear (ex ante) what these might be. Furthermore, to the extent that the migration shocks and internal responses are persistent over time (as the evidence suggests), there is legitimate concern that they are endogenous to the dependent variable.

H.2 Data and empirical specification

In line with Card (2001), for this replication exercise, I study variation across the 175 largest MSAs in the 5% census extract of 1990.[38] 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% 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.

[38]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.

Card delineates six skill groups by probabilistically assigning individuals into 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 (34), except he uses first order approximations of λ^I_{jrt} and λ^F_{jrt}. Specifically:

$$\frac{\left(L_{jr,1990} - L^F_{jr,1990}\right) - L_{jr,1985}}{L_{jr,1985}} = \delta^w_0 - \delta^w_1 \frac{L^F_{jr,1990}}{L_{jr,1985}} + X_{jr,1985}\delta^w_X + d_j + d_r + \varepsilon_{jr} \qquad \text{(A53)}$$

where $L_{jr,1990}$ is the population of skill group j in area r in the census year (1990); $L_{jr,1985}$ is the local population five years previously, based on responses to the 1990 census; and $L^F_{jr,1990}$ 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 $\frac{L^F_{jr,1990}}{L_{jr,1985}}$ is the contribution of immigration to that growth. To be more precise, Card actually uses the total (within-cell) population growth $\frac{L_{jr,1990}-L_{jr,1985}}{L_{jr,1985}}$ as the dependent variable, but this is a cosmetic difference: it simply adds a value of 1 to the δ^w_1 coefficient (see Peri and Sparber, 2011). $X_{jr,1985}$ is a vector of mean characteristics of individuals in the (j, r) cell in 1985: 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_j and d_r are full sets of skill group and area fixed effects respectively.

The instrument for $\frac{L^F_{jr,1990}}{L_{jr,1985}}$ is a first order approximation of m_{jrt} in the main text, specifically $\frac{\sum_o \phi^o_{r,1985} L^F_{oj,1990}}{L_{jr,1985}}$, where $\phi^o_{r,1985}$ is the share of origin o migrants who lived in area r in 1985, and $L^F_{oj,1990}$ is the number of new origin o migrants who arrived in the US between 1985 and 1990. I use the same 17 origin country groups as Card.

H.3 Estimates

In his baseline OLS specification (with 175 MSAs and observations weighted by cell population), Card estimates the $\frac{L^F_{jr,1990}}{L_{jr,1985}}$ coefficient (i.e. the negative of δ^w_1) as 0.25, with a standard error of 0.04.[39] Card's IV estimate is also 0.25, but with a standard error of 0.05. That is, each new immigrant in a given MSA-skill cell attracts an additional 0.25

[39]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).

workers to the same cell (relative to other cells). I record these estimates in column 1 of Table A9.

I attempt to replicate these estimates in column 2 and achieve similar numbers for Card's six-group occupation scheme (top row). In the second row, I re-estimate the model using a classification with just two imputed occupation groups: the first group aggregates all those two-digit occupations with less than 40% college share in 1990; and the second all those with more than 40%.[40] 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 of each panel, I study a two-group education classification (college graduates and non-graduates), for comparison with Table 8. Looking at column 2, it appears that the choice of skill delineation makes no significant difference to the estimates. In column 3, I cluster errors by state, which expands the standard errors (as one might expect).

Much of the action comes in column 4, when I exclude the mean demographic controls in X_{jr} from the right hand side. All the estimates are now negative; and they are statistically significant for the two-group occupation and education schemes, with IV coefficients of -0.47 and -2.14 respectively. As my model in Section 6.2 predicts, Card's fine six-group delineation (which will admit greater productive substitutability) generates a smaller δ_1^w estimate. It is also not surprising that, among the two-group schemes, the education classification generates a larger δ_1^w: the imputed occupations essentially mix individuals from the education groups, so any effect will be more diluted.

Why do the X_{jr} controls make such a difference? Statistically, they absorb much of the (within-area) variation in the migration shock: a regression of the enclave instrument on the d_j and d_r fixed effects yields an R squared of 0.858, and including the controls raises the R squared to 0.928. In principle, these controls may be picking up exogenous skill-specific shocks which I have neglected, though it is not clear (ex ante) what these might be. Furthermore, to the extent that the migration shocks and internal responses are persistent over time (as the evidence suggests), there is legitimate concern that they are endogenous to the dependent variable: foreign and internal mobility are liable to shift the demographic characteristics of the area-skill cells.

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[41] (so 369 areas in total). The latter modification ensures

[40] The occupational distribution in college share is strongly bipolar, and 40% is the natural dividing line.

[41] 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.

the area sample is comprehensive of the US, similarly to the CZs I use in the main text. These sample extensions make the coefficients larger (more negative) for the occupation delineations (with the OLS and IV coefficients now consistently significant); but the education scheme is little affected in IV.

In the final column, I replace the left and right hand side variables with $\log\left(\frac{L_{jr,1990}-L^F_{jr,1990}}{L_{jr,1985}}\right)$ and $\log\left(\frac{L_{jr,1985}+L^F_{jr,1990}}{L_{jr,1985}}\right)$ respectively, using the functional form I apply elsewhere in the paper: see equations (22) and (23). 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 estimates for the graduate/non-graduate scheme look similar.

To summarize, Table 8 shows that Card's estimates are very sensitive to the skill delineation, in the direction my model predicts. This reflects the fact that within-area estimates do not identify aggregate-level crowd-out, as I explain in Section 6.2. Beyond this, his choice of controls (which I argue may be problematic) play an important role.

Tables and figures

Table 1: Descriptive statistics

	Means by decade					Percentiles			
	1960s	1970s	1980s	1990s	2000s	5th	50th	95th	Max
Foreign inflow: λ^F_{rt}	0.020	0.033	0.047	0.065	0.060	0.000	0.007	0.057	0.211
Enclave shift-share: m_{rt}	0.016	0.025	0.038	0.056	0.053	0.000	0.007	0.054	0.289
Emp rate (end of decade)	0.624	0.659	0.707	0.694	0.665	0.553	0.648	0.752	0.810
Bartik shift-share: b_{rt}	0.173	0.227	0.142	0.095	0.056	-0.130	0.088	0.227	0.296

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

Table 2: First stage for crowding out estimates

	Foreign inflow: λ^F_{rt}			Emp growth: Δn_{rt}	Lagged ER: $n_{rt-1} - l_{rt-1}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Current enclave: m_{rt}	0.919***	1.229***	1.173***	-0.233**	-0.022	0.475***
	(0.084)	(0.119)	(0.105)	(0.113)	(0.122)	(0.175)
Lagged enclave: m_{rt-1}		-0.399***	-0.377***			-0.640***
		(0.056)	(0.053)			(0.139)
Current Bartik: b_{rt}	0.092***	0.078***	0.121***	0.839***	-0.134*	-0.156**
	(0.025)	(0.026)	(0.034)	(0.124)	(0.069)	(0.067)
Lagged Bartik: b_{rt-1}	0.063***	0.064***	0.160***	0.122*	0.371***	0.373***
	(0.019)	(0.019)	(0.028)	(0.068)	(0.063)	(0.062)
Amenity×yr controls	Yes	Yes	Yes	Yes	Yes	Yes
Year sample	60-10	60-10	70-10	60-10	60-10	60-10
Observations	3,610	3,610	2,888	3,610	3,610	3,610

This table reports first stage estimates corresponding to the crowding out specifications in Table 3. All specifications control for year effects and the amenity variables (interacted with year effects). The sample consists of (up to) five decadal observations (from 1960 to 2010) across 722 CZs. Column 3 omits the 1970s, to correspond with the IV specification in column 7 of Table 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 3: Estimates of conditional and unconditional crowd-out

	Conditional		Unconditional				
	Net internal flows: λ^I_{rt}		Net internal flows: λ^I_{rt}				Lagged: λ^I_{rt-1}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Foreign inflow: λ^F_{rt}	-0.883***	-0.913***	-0.761***	-1.096***	-1.109***	-0.787***	-0.235
	(0.048)	(0.065)	(0.200)	(0.130)	(0.153)	(0.167)	(0.228)
Emp growth: Δn_{rt}	0.882***	0.743***					
	(0.017)	(0.043)					
Lagged ER: $n_{rt-1} - l_{rt-1}$	0.251***	0.556***	0.520***	0.831***	0.833***		
	(0.021)	(0.105)	(0.072)	(0.207)	(0.221)		
Current Bartik: b_{rt}			0.646***	0.677***	0.679***	0.524***	-0.071
			(0.109)	(0.099)	(0.096)	(0.119)	(0.168)
Lagged Bartik: b_{rt-1}						0.290***	0.907***
						(0.060)	(0.103)
Lagged enclave: m_{rt-1}					0.016	-0.388***	-0.984***
					(0.161)	(0.124)	(0.167)
Specification	OLS	IV	OLS	IV	IV	IV	IV
Instruments	-	m_{rt}, b_{rt}, b_{rt-1}	-	m_{rt}, b_{rt-1}	m_{rt}, b_{rt-1}	m_{rt}	m_{rt}
F-stat for λ^F_{rt}	-	93.68	-	126.47	54.88	106.79	124.92
F-stat for Δn_{rt}	-	84.09	-	-	-	-	-
F-stat for $n_{rt-1} - l_{rt-1}$	-	56.93	-	34.70	31.00	-	-
Amenity×yr controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year sample	60-10	60-10	60-10	60-10	60-10	60-10	70-10
Observations	3,610	3,610	3,610	3,610	3,610	3,610	2,888

Columns 1-2 report OLS and IV estimates of the conditional crowding out specification (26), and columns 3-7 report the unconditional specification (27). There are (up to) three endogenous variables: the foreign inflow, λ^F_{rt}, the log employment change, and the lagged log employment rate. The corresponding instruments are the enclave shift-share m_{rt} and the current and lagged Bartiks. The sample consists of (up to) five decadal observations (from 1960 to 2010) across 722 CZs. Column 7 replaces the dependent variable with its lag, so it omits the initial decade. I report Sanderson-Windmeijer F-statistics which account for multiple endogenous variables. 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.

Table 4: IV effects of foreign inflows on local labor market outcomes

	Employment rates					Nominal wages		Housing costs	
	Native				Migrant	Native	Migrant	Rents	Prices
	Current	Current	Current	Lagged	Current				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Foreign inflow: λ^F_{rt}	-0.210***	-0.190**	-0.350***	-0.022	-0.236***	0.034	0.020	0.105	0.319
	(0.057)	(0.092)	(0.075)	(0.061)	(0.055)	(0.124)	(0.077)	(0.114)	(0.284)
Lagged ER: $n_{rt-1} - l_{rt-1}$	-0.411***	-0.414***			-0.469**	-0.875***	-0.231	-0.420**	-0.690**
	(0.087)	(0.091)			(0.204)	(0.211)	(0.291)	(0.175)	(0.348)
Current Bartik: b_{rt}	0.259***	0.255***	0.333***	0.024	0.192**	0.111	0.175	0.474***	0.691***
	(0.036)	(0.034)	(0.038)	(0.058)	(0.081)	(0.111)	(0.107)	(0.098)	(0.204)
Lagged Bartik: b_{rt-1}			-0.144***	0.069*					
			(0.024)	(0.036)					
Lagged enclave: m_{rt-1}		-0.024	0.177**	-0.216***					
		(0.079)	(0.076)	(0.063)					
Instruments	m_{rt}, b_{rt-1}	m_{rt}, b_{rt-1}	m_{rt}	m_{rt}	m_{rt}, b_{rt-1}	m_{rt}, b_{rt-1}	m_{rt}, b_{rt-1}	m_{rt}, b_{rt-1}	m_{rt}, b_{rt-1}
Amenity×yr controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year sample	60-10	60-10	60-10	70-10	60-10	60-10	60-10	60-10	60-10
Observations	3,610	3,610	3,610	2,888	3,599	3,610	3,610	3,610	3,610

This table reports estimates of (27), but replacing the dependent variable with various local outcomes: changes in the log (composition-adjusted) employment rate and mean residualized wage, separately for natives and migrants, and residualized housing rents and prices. See notes under Table 3 for further details about the specification, and see Table 2 for the first stage. 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 5: Conditional and unconditional IV crowd-out by decade

	Conditional crowd-out			Unconditional crowd-out		
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign inflow λ^F_{rt}	-0.913***	-0.837***	-1.066***	-1.096***	-1.678***	-1.798***
	(0.065)	(0.175)	(0.098)	(0.130)	(0.647)	(0.219)
Foreign inflow λ^F_{rt} × 1970s		-0.346			-0.179	
		(0.259)			(0.674)	
Foreign inflow λ^F_{rt} × 1980s		-0.039			0.805	
		(0.170)			(0.744)	
Foreign inflow λ^F_{rt} × 1990s		-0.033			0.341	
		(0.193)			(0.761)	
Foreign inflow λ^F_{rt} × 2000s		-0.086			1.040	
		(0.219)			(0.836)	
Foreign inflow λ^F_{rt} × post-80			0.179**			0.803**
			(0.071)			(0.327)
Controls and instruments	As in column 2, Table 3			As in column 4, Table 3		
Observations	3,610	3,610	3,610	3,610	3,610	3,610

This table estimates heterogeneity in IV conditional and unconditional crowd-out effects over time. Columns 1 and 4 replicate the basic IV specifications of Table 3 (columns 2 and 4); but I report only the coefficient on foreign inflows. In columns 2 and 5, I include interactions between the foreign inflow and decade effects (my additional instruments are interactions between the enclave shift-share and the same decade effects), with the 1960s interaction omitted. In columns 3 and 6, I include a single interaction between the foreign inflow and a dummy for the 1980-2010 period (which I also interact with the enclave instrument). The specification is otherwise identical to columns 2 and 4 of Table 3: see notes under that table for further details. 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: Local adjustment to foreign inflows, for given π

		$\pi = 0$	$\pi = 0.197$	$\pi = 0.27$	$\pi = 0.4$	$\pi = 1$
		(1)	(2)	(3)	(4)	(5)
A. Conditional crowd-out	δ_1^c	0.913***	0.880***	0.868***	0.845***	0.743***
		(0.065)	(0.055)	(0.051)	(0.046)	(0.043)
B. Unconditional crowd-out	δ_1^u	1.096***	0.880***	0.800***	0.657***	0
		(0.130)	(0.104)	(0.095)	(0.078)	-
C. Crowd-out differential	$\delta_1^c - \delta_1^u$	-0.182**	0.000	0.068	0.188***	0.743***
		(0.082)	(0.061)	(0.053)	(0.041)	(0.043)
D. Employment response	$\frac{\delta_1^c - \delta_1^u}{\delta_2^c}$	-0.245**	0.000	0.091	0.253***	1
		(0.102)	(0.082)	(0.074)	(0.061)	-
E. Total adjustment	$\delta_1^u + \left(\frac{\delta_1^c - \delta_1^u}{\delta_2^c}\right)$	0.851***	0.880***	0.891***	0.910***	1
		(0.056)	(0.045)	(0.041)	(0.033)	-
F. Employment rate effect	$-1 + \delta_1^u + \left(\frac{\delta_1^c - \delta_1^u}{\delta_2^c}\right)$	-0.149***	-0.120***	-0.109***	-0.090***	0
		(0.056)	(0.045)	(0.041)	(0.033)	-
G. Pop share of adjustment	$\delta_1^u \left[\delta_1^u + \left(\frac{\delta_1^c - \delta_1^u}{\delta_2^c}\right)\right]^{-1}$	1.288***	1.000***	0.898***	0.722***	0
		(0.115)	(0.093)	(0.085)	(0.071)	-

This table quantifies a range of objects of interest, conditional on the (biased) coefficient estimates from the crowd-out equations ($\hat{\delta}_1^c$, $\hat{\delta}_2^c$ and $\hat{\delta}_2^u$) and an assumed level of undercoverage bias, π. Using equations (31) and (32), I can compute the "true" conditional crowd-out coefficients: $\delta_1^c = (1-\pi)\,\hat{\delta}_1^c + \pi\hat{\delta}_2^c$ and $\delta_2^c = \hat{\delta}_2^c$. And using equation (29), I can compute the "true" unconditional effect: $\delta_1^u = (1-\pi)\,\hat{\delta}_1^u$. The remaining objects are functions of the "true" δ_1^c, δ_2^c and δ_1^u, as specified in the table: see equation (18). Coefficient estimates of $\hat{\delta}_1^c$, $\hat{\delta}_2^c$ and $\hat{\delta}_2^u$ are based on the basic IV specifications of Table 3 (columns 2 and 4). Robust standard errors (in parentheses), clustered by state, are computed using the delta method and account for dependence between coefficient estimates across the conditional and unconditional crowd-out equations. In practice, I account for this dependence by estimating a single model which nests both the conditional and unconditional equations: I use a dataset with every observation duplicated, where every right hand variable (and instrument) is interacted with dummy indicators for both the conditional and unconditional models. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Robustness of unconditional IV crowd-out to controls and decade

		1960s	1970s	1980s	1990s	2000s	All years
		(1)	(2)	(3)	(4)	(5)	(6)
(A)	Controlling for year effects only	0.273	-0.726	-0.041	-0.943***	-0.538**	-0.526**
		(0.944)	(0.635)	(0.250)	(0.225)	(0.252)	(0.246)
(B)	+ Current Bartik	-0.745	-0.268	-0.455	-0.921***	-0.572**	-0.689***
		(1.134)	(0.466)	(0.350)	(0.260)	(0.251)	(0.217)
(C)	+ Lagged log ER (instrumented)	-0.709	-0.238	-0.744*	-0.327	-0.564**	-0.753***
		(1.139)	(0.318)	(0.441)	(0.421)	(0.246)	(0.239)
(D)	+ Climate controls	-1.967**	-2.088***	-0.973***	-1.343***	-0.845***	-1.396***
		(0.908)	(0.467)	(0.302)	(0.256)	(0.180)	(0.192)
(E)	+ Coastline dummy	-2.032**	-2.087***	-0.865**	-1.119***	-0.637***	-1.263***
		(0.947)	(0.473)	(0.350)	(0.251)	(0.189)	(0.228)
(F)	+ Log pop density 1900	-1.657***	-1.797***	-0.726***	-1.100***	-0.558***	-1.107***
		(0.610)	(0.220)	(0.201)	(0.276)	(0.215)	(0.256)
(G)	+ Log distance to closest CZ	-1.626**	-1.917***	-0.877***	-1.203***	-0.638***	-1.137***
		(0.634)	(0.197)	(0.188)	(0.298)	(0.236)	(0.251)
(H)	+ Amenity×yr effects (i.e. all controls)	-1.626**	-1.917***	-0.877***	-1.203***	-0.638***	-1.096***
		(0.634)	(0.197)	(0.188)	(0.298)	(0.236)	(0.130)
(I)	Native response 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)
(J)	Native response to total migrant stock	-3.353	-1.988***	-0.842***	-1.220***	0.315	-1.095***
		(3.937)	(0.253)	(0.209)	(0.438)	(0.592)	(0.204)
Observations		722	722	722	722	722	3,610

This table tests the robustness of my basic IV unconditional crowd-out effect (in column 4 of Table 3) to the choice of controls and decadal sample. The first five columns estimate the effect of the foreign inflow λ_{rt}^F separately by decade, and column 6 pools all decades. Moving down the rows of the table, I show how the crowd-out effect changes as progressively more controls are included. All specifications include the foreign inflow λ_{rt}^F (instrumented with the enclave shift-share, m_{rt}) and year effects. Row B controls additionally for a current Bartik, b_{rt}; row C includes the (endogenous) lagged employment rate (together with its lagged Bartik instrument, b_{rt-1}); and the various amenities are then progressively added - until, in row H, I have the full set of controls I use in Table 3. Row I replaces the dependent variable with the contribution of natives alone to local population growth, using the full set of controls. Row J also estimates the native response (with the full set of controls), but replaces the inflow of new migrants λ_{rt}^F with the contribution of *all* migrants (i.e. including old migrants) to local population growth. I write the native and (total) migrant contributions as $\log\left(\frac{L_{rt-1}+\Delta L_{rt}^N}{L_{rt-1}}\right)$ and $\log\left(\frac{L_{rt-1}+\Delta L_{rt}^M}{L_{rt-1}}\right)$ respectively (using the same functional form as (23)), where L_{rt}^N and L_{rt}^M are respectively the local populations of natives and all foreign-born individuals. 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 δ_1^w

	Pooled cross-sections			Longitudinal			Observations
	First stage coefficient	Total residual contrib: λ_{jrt}^{I}	Natives only: $\lambda_{jrt}^{I,N}$	First stage coefficient	Total residual contrib: λ_{jrt}^{I}	Natives only: $\lambda_{jrt}^{I,N}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CG / < CG	0.539***	1.502***	1.638***	0.475***	-3.587**	-2.798**	4,332
	(0.067)	(0.295)	(0.369)	(0.074)	(1.518)	(1.244)	
Coll / < Coll	0.662***	1.040***	1.046***	0.778***	-1.129***	-0.794***	4,332
	(0.044)	(0.132)	(0.162)	(0.056)	(0.094)	(0.140)	
HSD / > HSD	0.785***	0.980***	1.410***	0.841***	-0.425***	-0.252***	4,332
	(0.031)	(0.088)	(0.261)	(0.039)	(0.090)	(0.067)	
4 edu groups	0.744***	1.330***	1.521***	0.817***	-0.194*	-0.041	8,664
	(0.035)	(0.095)	(0.209)	(0.038)	(0.117)	(0.085)	

This table reports within-area estimates of crowd-out (i.e. the coefficient on λ_{jrt}^{F}, the negative of δ_1^w) using equation (34). Columns 1-3 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, m_{jrt}. And the remaining columns report IV estimates of δ_1^w, both for the total residual contribution (natives and old migrants) and for natives only. The four rows offer estimates for different education-based skill delineations: (i) college graduates / non-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 A1: IV effects of foreign inflows by education

	Pop growth: Δl_{jrt}	Foreign contrib: λ^F_{jrt}	Residual contrib: λ^I_{jrt}	Employment rates		Wages		Housing costs	
				Native	Migrant	Native	Migrant	Rents	Prices
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Coll grads	-0.261*	0.816***	-0.977***	-0.042*	-0.192***	0.185**	0.319**	0.259**	0.625**
	(0.159)	(0.041)	(0.184)	(0.025)	(0.057)	(0.080)	(0.157)	(0.109)	(0.278)
Non-grads	-0.145	1.033***	-1.274***	-0.276***	-0.258***	-0.030	-0.088	0.020	0.321
	(0.135)	(0.011)	(0.136)	(0.069)	(0.066)	(0.130)	(0.084)	(0.109)	(0.291)

This table reports IV effects of the aggregate-level foreign inflow λ^F_{rt} in equation (A27), estimated for various outcomes separately for college graduates and non-graduates. All specifications include 3,610 observations (722 CZs over five decadal periods), with the exception of column 5 (whose samples are 2,693 and 3,590 respectively), for the same reasons as discussed under Table 4. The right-hand side of the estimating equation is identical to column 4 of Table 3; see table notes for details. 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 A2: Contribution of inflows and outflows to crowding out across CZs

	OLS			IV		
	Net flow λ^{I5}_{rt}	Inflow λ^{Ii5}_{rt}	Outflow λ^{Io5}_{rt}	Net flows λ^{I5}_{rt}	Inflow λ^{Ii5}_{rt}	Outflow λ^{Io5}_{rt}
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign inflow: λ^{F5}_{rt}	-0.500	-0.296	0.147	-1.555***	-1.661***	-0.320
	(0.319)	(0.385)	(0.152)	(0.269)	(0.388)	(0.222)
Lagged 10yr ER: $n_{rt-10} - l_{rt-10}$	0.199***	0.162***	-0.016	0.556***	0.758***	0.285**
	(0.047)	(0.051)	(0.033)	(0.191)	(0.179)	(0.118)
Current decadal Bartik: b_{rt}	0.286**	0.400***	0.161***	0.442***	0.566***	0.190***
	(0.126)	(0.115)	(0.047)	(0.115)	(0.120)	(0.067)
SW F-stat for λ^{F5}_{rt}	-	-	-	88.92	88.92	88.92
SW F-stat for $n_{rt-10} - l_{rt-10}$	-	-	-	26.37	26.37	26.37
Observations	2,166	2,166	2,166	2,166	2,166	2,166

This table offers OLS and IV estimates of the 5-year unconditional crowding out effect, based on equation (A28), and disaggregates these into the (approximate) contributions from internal inflows and outflows. Variable definitions and data sources are given in Section D. The flow data covers the intervals 1965-70, 1975-80, 1985-90 and 1995-2000. The 5-year foreign inflow 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 A3: First stage for employment response

	Log pop change: Δl_{rt}		Foreign inflow: λ^F_{rt}	Net internal flows: λ^I_{rt}	Lagged ER: $n_{rt-1} - l_{rt-1}$		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Current enclave: m_{rt}	-0.073		0.922***	-1.030***	-0.022		-0.029
	(0.139)		(0.087)	(0.174)	(0.122)		(0.124)
Max Jan temp		0.333***	0.038**	0.314***		-0.141***	-0.139***
		(0.057)	(0.019)	(0.062)		(0.047)	(0.045)
Current Bartik: b_{rt}	0.546***	0.545***	0.083***	0.477***	-0.134*	-0.122*	-0.120*
	(0.114)	(0.114)	(0.023)	(0.124)	(0.069)	(0.069)	(0.066)
Lagged Bartik: b_{rt-1}	0.286***	0.283***	0.069***	0.237***	0.371***	0.359***	0.362***
	(0.060)	(0.067)	(0.020)	(0.078)	(0.063)	(0.062)	(0.062)
SW F-test	0.26	75.88	121.32	72.60	0.25	45.40	45.89
Amenity×yr controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Max temp jan×yr controls	Yes	No	No	No	Yes	No	No
Year sample	60-10	60-10	60-10	60-10	60-10	60-10	60-10
Observations	3,610	3,610	3,610	3,610	3,610	3,610	3,610

This table reports first stage estimates for the employment response equations (A43) and (A44); corresponding OLS and IV estimates are in Table A4. I report Sanderson-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. 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: OLS and IV estimates of employment response

	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Pop growth: Δl_{rt}	1.025***		4.000	0.735***	
	(0.013)		(6.533)	(0.066)	
Foreign inflow: λ^F_{rt}		0.870***			0.605***
		(0.030)			(0.057)
Net internal flows: λ^I_{rt}		0.994***			0.776***
		(0.015)			(0.055)
Lagged ER: $n_{rt-1} - l_{rt-1}$	-0.218***	-0.216***	-2.757	-0.280***	-0.273***
	(0.023)	(0.023)	(5.197)	(0.102)	(0.090)
Current Bartik: b_{rt}	0.143***	0.182***	-1.714	0.391***	0.388***
	(0.029)	(0.020)	(4.156)	(0.070)	(0.065)
Instruments	-	-	m_{rt}, b_{rt-1}	Jan temp, b_{rt-1}	m_{rt}, Jan temp, b_{rt-1}
Amenity×yr controls	Yes	Yes	Yes	Yes	Yes
Max temp jan×yr controls	Yes	Yes	Yes	No	Yes
Year sample	60-10	60-10	60-10	60-10	60-10
Observations	3,610	3,610	3,610	3,610	3,610

This table reports OLS and IV estimates for models of local employment growth, i.e. equations (A43) and (A44). Corresponding first stage estimates are in Table A3. 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 speciifcations 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.

Table A5: Robustness of crowd-out estimates to CZ sample and geographical units

	Basic specification					Without lagged emp rate control		
	Basic CZ sample	Top 100 CZs	Excluding top 5 destination CZs	Excluding $m_{rt} > 0.1$	Unweighted	Basic CZ sample	State data	CZ data + state FEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Foreign inflow: λ^F_{rt}	-1.096***	-1.018***	-0.866***	-0.989***	-0.940***	-0.787***	-0.936**	-0.931***
	(0.130)	(0.129)	(0.202)	(0.220)	(0.266)	(0.167)	(0.372)	(0.127)
Lagged ER: $n_{rt-1} - l_{rt-1}$	0.831***	0.887	0.667***	0.781***	0.578***			
	(0.207)	(0.572)	(0.168)	(0.220)	(0.183)			
Current Bartik: b_{rt}	0.677***	0.669***	0.770***	0.653***	0.604***	0.524***	0.104	0.699***
	(0.099)	(0.172)	(0.088)	(0.097)	(0.106)	(0.119)	(0.238)	(0.083)
Lagged Bartik: b_{rt-1}						0.290***	0.319***	0.228***
						(0.060)	(0.108)	(0.059)
Lagged enclave: m_{rt-1}						-0.388***	-0.482**	-0.063
						(0.124)	(0.221)	(0.080)
Instruments	m_{rt}, b_{rt-1}	m_{rt}, b_{rt-1}	m_{rt}, b_{rt-1}	m_{rt}, b_{rt-1}	m_{rt}, b_{rt-1}	m_{rt}	m_{rt}	m_{rt}
F-stat for λ^F_{rt}	126.47	99.91	127.68	62.51	51.13	106.79	299.94	40.72
F-stat for $n_{rt-1} - l_{rt-1}$	34.70	3.48	30.74	34.13	25.77	-	-	-
Amenity×yr controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geography	CZ	CZ	CZ	CZ	CZ	CZ	State	CZ
Observations	3,610	500	3,585	3,544	3,610	3,610	240	3,610

This table offers alternative IV estimates of unconditional crowd-out in equation (27), for different CZ samples, weighting choices, and also state-level data: see Appendix F.2 and F.3 for details. Columns 1 and 6 are identical to columns 4 and 6 respectively of Table 3. Column 2 restricts the sample to the 100 largest CZs (based on population of 16-64s in 1960). Column 3 excludes the five CZs with largest (absolute) migrant inflows, namely Los Angeles, New York, Chicago, Miami and San Francisco. Column 4 restricts the sample to observations with enclave shift-share m_{rt} values below 0.1. Column 5 reproduces the basic column 1 specification without population weights. Columns 6-8 replace the lagged employment rate on the right-hand side with the lagged Bartik and enclave shift-shares. Column 7 replicates column 6 for state-level data: the sample consists of the 48 states of the continental US, with the District of Columbia merged into Maryland. Column 8 controls for state fixed effects in the CZ-level specification of column 6. All specifications control for the current Bartik b_{rt}, 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 (with the exception of column 5). *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Robustness of crowd-out estimates to empirical specification

	λ^I_{rt}	$\frac{\Delta L_{rt}-L^F_{rt}}{L_{rt-1}}$	$\frac{\Delta L_{rt}-L^F_{rt}}{L_{rt-1}}$	$\frac{\Delta L_{rt}-L^F_{rt}}{L_{rt-1}}$	$\Delta L_{rt}-L^F_{rt}$	$\Delta L_{rt}-L^F_{rt}$	λ^I_{rt}	λ^I_{rt}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Foreign inflow: λ^F_{rt}	-1.096***						-0.631	-1.351***
	(0.130)						(0.611)	(0.262)
Foreign inflow: $\frac{L^F_{rt}}{L_{rt-1}}$		-1.090***	-1.332***	-1.077***				
		(0.143)	(0.289)	(0.163)				
Foreign inflow: L^F_{rt}					-0.228***	-0.971***		
					(0.085)	(0.273)		
Lagged ER: $n_{rt-1}-l_{rt-1}$	0.831***	0.893***	0.992***	0.888***	2×10^{6}***	4×10^{6}**	1.269**	
	(0.207)	(0.227)	(0.193)	(0.203)	(7×10^{5})	(2×10^{6})	(0.496)	
Current Bartik: b_{rt}	0.677***	0.791***	0.843***	0.788***	2×10^{5}	-2×10^{5}	0.615***	0.580***
	(0.099)	(0.116)	(0.123)	(0.124)	(3×10^{5})	(2×10^{5})	(0.086)	(0.080)
Lagged Bartik: b_{rt-1}								0.162***
								(0.063)
Lagged enclave: m_{rt-1}								0.078
								(0.119)
Instruments	m_{rt}, b_{rt-1}	$\frac{\Lambda^F_{rt}}{L_{rt-1}}$, b_{rt-1}	$\frac{\Lambda^{F60}_{rt}}{L_{rt-1}}$, b_{rt-1}	Λ^F_{rt}, b_{rt-1}	Λ^F_{rt}, b_{rt-1}	Λ^F_{rt}, b_{rt-1}	m_{rt}, b_{rt-1}	m_{rt}
F-stat for λ^F_{rt}	126.47	116.05	33.87	91.63	262.07	92.07	58.33	40.30
F-stat for $n_{rt-1}-l_{rt-1}$	34.70	34.99	45.13	40.63	39.10	49.08	45.51	-
Amenity×yr controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CZ fixed effects	No	No	No	No	No	Yes	Yes	Yes
Observations	3,610	3,610	3,610	3,610	3,610	3,610	3,610	3,610

This table offers alternative IV estimates of unconditional crowd-out in equation (27), for different empirical specifications: see Appendix F.4, F.5 and F.6 for details. 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. Column 1 is identical to columns 4 of Table 3 in the main text. In column 2, I replace the dependent variable with $\frac{\Delta L_{rt}-L^F_{rt}}{L_{rt-1}}$, and I recast the foreign inflow variable as $\frac{L^F_{rt}}{L_{rt-1}}$, and its instrument as $\frac{\Lambda^F_{rt}}{L_{rt-1}}$ (where $\Lambda^F_{rt} \equiv \sum_o \phi^o_{rt-1} L^F_{o(-r)t}$ is the predicted number of incoming migrants between $t-1$ and t). Column 3 then replaces the instrument with $\frac{\Lambda^{F60}_{rt}}{L_{rt-1}}$, where $\Lambda^{F60}_{rt} \equiv \sum_o \phi^o_{r60} L^F_{o(-r)t}$ predicts the migrant inflow based on 1960 settlement patterns, ϕ^o_{r60} (rather than $t-1$ patterns), for every decade. Column 4 replaces the instrument with Λ^F_{rt}, i.e. the predicted absolute (rather than relative) foreign inflow. Columns 5 and 6 are based on the levels specification in (A51). Columns 6-8 control for CZ fixed effects. Column 8 replaces the lagged employment rate on the right-hand side with the lagged Bartik and enclave shift-shares. The Sanderson-Windmeijer (2016) F-statistics account for multiple endogenous variables. All specifications control for the current Bartik b_{rt}, 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 A7: State-level within-area IV estimates of δ_1^w: Cohort effects

	First stage coefficient	Total response by residence: λ_{jrt}^{I}	Native response by:		Observations
			Residence: $\lambda_{jrt}^{I,N}$	Birthplace: $\lambda_{BPjrt}^{I,N}$	
	(1)	(2)	(3)	(4)	(5)
CG / < CG	0.604***	1.618***	1.616**	2.222***	288
	(0.104)	(0.357)	(0.665)	(0.425)	
Coll / < Coll	0.821***	1.230***	1.206***	2.265***	288
	(0.047)	(0.155)	(0.147)	(0.208)	
HSD / > HSD	0.970***	1.088***	1.434***	1.619***	288
	(0.033)	(0.130)	(0.344)	(0.249)	
4 edu groups	0.933***	1.328***	1.483***	1.763***	576
	(0.032)	(0.104)	(0.247)	(0.195)	

This table explores the presence of cohort effects in the pooled cross-section IV estimates of δ_1^w in equation (34), using state-level data and a range of education-based skill delineations. My sample consists of the 48 states of the continental US, with the District of Columbia merged into Maryland. Columns 1-3 are state-level equivalents to the CZ estimates in Table 8 (columns 1-3) in the main text, based on the three decadal periods between 1970 and 2000. Column 1 reports the first stage coefficient on the education-specific enclave shift-share, m_{jrt}. Column 2 reports the IV effect of skill-specific foreign inflows $\hat{\lambda}_{jrt}^{F}$ on the total residual contribution λ_{jrt}^{I} (natives and old migrants) to skill group g population growth in state r. Column 3 reports the contribution of natives only, i.e. $\lambda_{jrt}^{I,N}$. Column 5 then replaces the dependent variable with the contribution of natives to group-specific population growth among those *born* (rather than *residing*) in state r. All specifications control for both area-year and skill-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 A8: Reconciliation with aggregate-level estimates from Card (2007)

	Card (2009): top 100 MSAs	Replication						
		Top 100 CZs						All CZs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 1980s								
Migrant contribution: $\frac{\Delta L^M_{rt}}{L_{rt-1}}$	0.5	0.118	-0.533	-0.487	-1.145*	-1.055***	-1.115***	-1.034***
	(0.4)	(0.225)	(0.339)	(0.318)	(0.668)	(0.291)	(0.416)	(0.256)
Log population in 1980	?	-0.044***	0.751***				0.006	
	(?)	(0.011)	(0.161)				(0.019)	
Log population in 1970			-0.732***					
			(0.145)					
Current Bartik					1.850***	1.456***	1.433***	1.356***
					(0.538)	(0.290)	(0.272)	(0.211)
Panel A: 1990s								
Migrant contribution: $\frac{\Delta L^M_{rt}}{L_{rt-1}}$	-0.8	-0.197	-1.066*	-1.285***	-1.154***	-1.104***	-1.164*	-1.331***
	(0.6)	(0.234)	(0.636)	(0.371)	(0.398)	(0.321)	(0.630)	(0.239)
Log population in 1980	?	-0.049***	0.667***				0.004	
	(?)	(0.008)	(0.254)				(0.024)	
Log population in 1970			-0.662***					
			(0.236)					
Current Bartik					-0.578	0.590	0.577	0.770**
					(0.586)	(0.540)	(0.522)	(0.308)
Amenity controls	No	No	No	No	No	Yes	Yes	Yes
Observations	100	100	100	100	100	100	100	722

This table offers a reconciliation with Card's (2007) aggregate-level IV estimates of crowd-out, based on equation (A52). See Appendix G for details. The dependent variable is the contribution of *all* migrants (i.e. both new and old) to local population growth, i.e. $\frac{\Delta L^M_{rt}}{L_{rt-1}}$; and the regressor of interest is the contribution of natives, $\frac{\Delta L^N_{rt}}{L_{rt-1}}$. Card's IV estimates are presented in column 1, separately for the 1980s in Panel A and the 1990s in Panel B. These are taken from Table 3 of his paper, based on the 100 largest MSAs, with observations weighted by 1980 populations. (Card reports his estimates as the effect on aggregate population growth, but I substract 1 from his numbers for comparability with my specification; see Peri and Sparber, 2011.) Card controls for 1980 population, but does not report the coefficient: hence the "?". In column 2, I replicate his exercise using a sample of the largest 100 CZs. Columns 3-7 study the effects of including or excluding various control variables, with columns 6-7 including the amenity effects described in Section 3. Column 8 extends the sample to all 722 CZs. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Reconciliation with within-area estimates from Card (2001)

	Card (2001): 175 MSAs, weighted	Replication	... with errors clustered by state	... excluding demog controls	... with remaining MSAs	... with full area sample	... with alternative specification for flow variables
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS							
6 occup groups	0.25***	0.214***	0.214**	-0.071	-0.181***	-0.230***	-0.267***
	(0.04)	(0.045)	(0.098)	(0.046)	(0.055)	(0.067)	(0.072)
2 occup groups	-	0.106	0.106	-0.486***	-0.801***	-0.942***	-1.036***
		(0.185)	(0.248)	(0.145)	(0.244)	(0.287)	(0.299)
Coll grad / non-grad	-	-0.153	-0.153	-1.948***	-3.270***	-3.595***	-3.878***
		(0.441)	(0.572)	(0.549)	(1.124)	(1.099)	(1.145)
Panel B: IV							
6 occup groups	0.25***	0.255***	0.255**	-0.054	-0.123***	-0.169***	-0.192***
	(0.05)	(0.045)	(0.115)	(0.043)	(0.044)	(0.051)	(0.053)
2 occup groups	-	0.244*	0.244	-0.469***	-0.653***	-0.809***	-0.895***
		(0.139)	(0.298)	(0.100)	(0.109)	(0.138)	(0.141)
Coll grad / non-grad	-	0.563	0.563	-2.143***	-1.687*	-2.350**	-2.615***
		(1.280)	(2.912)	(0.750)	(0.867)	(0.927)	(0.949)

This table offers a reconciliation with Card's (2001) within-area estimates of crowd-out, based on equation (A53). See Appendix H for details. Card's OLS and IV estimates of within-area crowd-out (i.e. the coefficient on $\frac{L^F_{jr,1990}}{L_{jr,1985}}$), for his six-group imputed occupation scheme, are presented in the top row (in each panel) of 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 substract 1 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, I 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 and right hand side variables with $\log\left(\frac{L_{jr,1990}-L^F_{jr,1990}}{L_{jr,1985}}\right)$ and $\log\left(\frac{L_{jr,1985}+L^F_{jr,1990}}{L_{jr,1985}}\right)$ respectively, applying the functional form I use in the main text. I present estimates for both Card's six-group imputed occupation scheme (top row), a broader two-group imputed occupation scheme (which I have constructed), and a two-group education scheme (college graduates and non-graduates). *** p<0.01, ** p<0.05, * p<0.1.

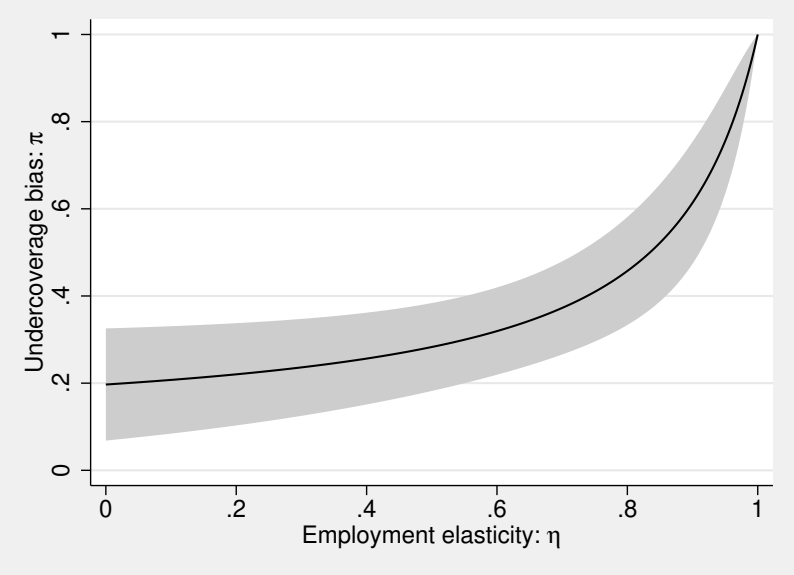

Figure 1: Implied relationship between π and η

This figure plots the relationship between undercoverage bias π and the employment elasticity η, implied by equation (33). I calibrate this equation using estimates from Table 3 (columns 2 and 4): specifically, $\hat{\delta}_1^c = 0.913$, $\hat{\delta}_2^c = 0.743$ and $\hat{\delta}_1^u = 1.096$. The shaded area illustrates 95% confidence intervals (for fixed values of η): standard errors are computed using the delta method, and account for dependence between coefficient estimates across the conditional and unconditional equations.

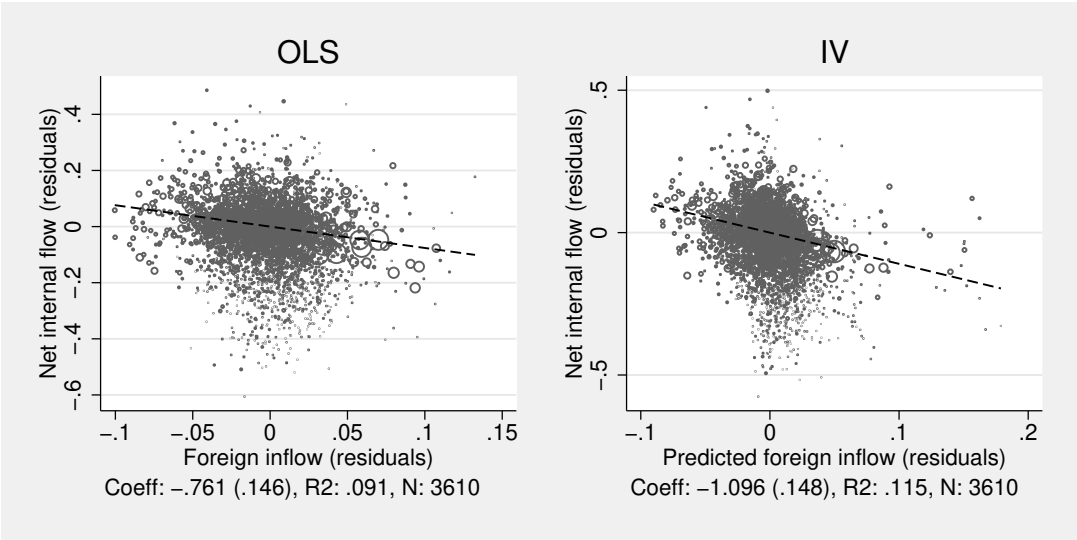

Figure A1: Graphical illustration of crowding out estimates

This figure presents Frisch-Waugh type plots for the unconditional crowd-out estimates in columns 3 and 4 of Table 3. See Appendix F.1 for details.

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