Monopsony and the Wage Effects of Migration

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Abstract

If labour markets are perfectly competitive, migration can only affect native wages by changing marginal products. But under imperfect competition, firms may also respond by imposing larger mark-downs – if they have greater monopsony power over migrants than natives, but cannot perfectly wage-discriminate between them. The marginal product effect will depend on how migration shifts relative labour supply *across* different skill cells, whereas the mark-down effect depends on migrant concentration *within* them. This insight can help account for empirical violations of canonical migration models in US data. Under imperfect competition, migration will increase aggregate native income significantly more (as firms capture rents from migrant labour). But the imposition of larger mark-downs also redistributes income from native workers to firms; and based on our estimates, native labour loses out overall. Crucially though, policies which constrain monopsony power over migrants can help eliminate these adverse wage effects.

1 Introduction

Much has been written on how migration affects native wages: see, for example, surveys by Borjas (2014), Card and Peri (2016) and Dustmann, Schoenberg and Stuhler (2016). These wage effects are difficult to identify empirically, as migration of one labour type j may affect the marginal products of all other labour types $k \neq j$, through potentially complicated patterns of substitutability and complementarity. To reduce the

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dimensionality of this problem, an influential strand of the literature, commonly known as the "structural approach", imposes an aggregate production function with nested CES technology:

$$Y = F\left(L_1, ..., L_J; K\right) \tag{1}$$

over labour skill types L_j and possibly capital K. See e.g. Borjas, Freeman and Katz (1997); Borjas (2003); Card (2009); Manacorda, Manning and Wadsworth (2012); Ottaviano and Peri (2012); Burstein et al. (2020); Monras (2020); Piyapromdee (2021). F can be parameterised by a relatively small set of estimable parameters, measuring relative productivities and elasticities of substitution. Armed with these estimates, the researcher can then simulate how a migration shock (which increases labour of particular skill types L_j) affects marginal products MP_j across the skill distribution:

$$MP_j = F_j\left(L_1, ..., L_J; K\right) \tag{2}$$

Assuming competitive labour markets, these changes in MP_j will then fully determine the impact on wages W_j. We refer to this as the "canonical model".

The canonical model severely restricts the wage effects of migration. Under perfect competition, immigration can *only* affect wages by changing the relative supplies of skill types j (or of factor inputs more generally); and conditional on these supplies, the *composition* of skill types (whether native or migrant) will make no difference. This has important implications for the aggregate effects of immigration: in the canonical model, an influx of migrants (holding their skill mix fixed) *must* increase the average native wage, in a long run scenario where capital K is elastically supplied (see Borjas, 1995; Dustmann, Frattini and Preston, 2012; Amior and Manning, 2024).[1] Although this is a theoretical result, it does have empirical implications: any structural model which imposes these assumptions will predict a surplus for native labour, whatever data is used for estimation.

In this paper however, we show that the composition of skill types does become relevant once the assumption of perfect competition is relaxed. Suppose the wage of skill group j can differ from its marginal product by a mark-down ϕ_j, as in Bound and Johnson (1992) or Katz and Autor (1999):

$$\log W_j = \log MP_j - \phi_j \tag{3}$$

[1]Borjas (1995) famously proved this result for a one-good economy with up to two labour types. But Amior and Manning (2024) show it is much more general: it holds for any number of labour types, and any number of (intermediate or final) goods, for any technology F with constant returns (CES or otherwise).

If this mark-down ϕ_j is fixed, the change in log wages is still equal to the change in log marginal products. However, we argue that firms may respond to immigration by *increasing* the mark-down ϕ_j – if firms have greater monopsony power over migrants than natives (something we argue is plausible), but are unable to perfectly discriminate in wage offers between them (as suggested by evidence from Arellano-Bover and San, 2020, Aslund et al., 2021, Dostie et al., 2023, and Amior and Stuhler, 2023). Under these conditions, a larger migrant concentration *within* a skill type j will make the labour market less competitive for natives and migrants alike – in violation of the canonical model.[2]

This potential malleability of mark-downs has important implications for empirical identification. We first show that the canonical framework is overidentified, even with standard wage and employment data, and even if natives and migrants function as distinct and imperfectly substitutable skill types (as in Manacorda, Manning and Wadsworth, 2012; Ottaviano and Peri, 2012). Intuitively, marginal products MP_j are fully determined by an aggregation of skill-specific employment stocks (across natives and migrants), as specified by the production technology. Therefore, conditional on this cell aggregator, any sensitivity of wages to cell composition (the migrant share) implies a rejection of the canonical model's restrictions. However, such effects are consistent with the mark-downs being affected by migration, in line with our monopsony framework.

We implement this test using variation across US skill cells, defined by education and experience (as in e.g. Borjas, 2003; Ottaviano and Peri, 2012; Llull, 2018*a*; Monras, 2020), and allowing natives and migrants to be imperfect substitutes within skill cells. To address concerns about endogeneity (as highlighted by e.g. Card and Peri, 2016, or Llull, 2018*a*), we devise new instruments for the allocation of natives and migrants to skill cells, driven by demographic shifts in the US and abroad. Using this variation, we reject the restrictions of the canonical model; and we show this rejection cannot plausibly be attributed to the specification of technology, the definition of skill cells, or the allocation of migrants to these cells. However, the results are consistent with a mark-down effect. Interpreted this way, our estimates suggest that a 1 pp increase in a cell's migrant share allows firms to mark down native wages by at least an additional 0.4-0.6%.

As further support for this interpretation, we explore heterogeneous effects by migrants' legal status. There is good reason to believe that firms have much greater market power over undocumented migrants, and we offer supporting evidence from job separation elasticities. One should therefore expect larger mark-down effects from undocumented

[2]This insight is closely related to Beaudry, Green and Sand (2012). They show that the wage bargain in a given job (conditional on its productivity) depends on the labour market's industrial composition, since this affects the set of outside options. In their story, it is the composition of industries which matters; while in ours, it is the composition of the labour force itself.

migration. To test this empirically, we disaggregate the migrant share into legal and undocumented components (and construct distinct instruments for each): as our model predicts, the impact of migrant share is mostly driven by undocumented migrants.

Quantitatively, the estimated mark-down effect is large: it more than offsets the small increase in average native wages which arises from changes in marginal products. However, our results also indicate that monopsony power significantly increases the *total* income gains of natives (including profits), which are typically small in competitive models (Borjas, 1995). This is because native-owned firms capture rents from migrants, even in a "long run" scenario where capital is elastically supplied. The finding that firms profit from migration is consistent with the observation that *individual* firms actively invest in foreign recruitment (whether through political lobbying on visa rules, payment of visa fees, or foreign employment agencies: see e.g. Rodriguez, 2004; Fellini, Ferro and Fullin, 2007; Facchini, Mayda and Mishra, 2011; Gibbons et al., 2019). But just as the *total* native surplus is larger, so too are the distributional effects: as mark-downs expand, rents are transferred from workers to firms; and the surplus of native labour turns negative.

These insights matter not only for the design of structural models, but also for policy. Any such mark-down effects may be offset through policies which constrain monopsony power over migrants (such as minimum wages, as in e.g. Edo and Rapoport, 2019, or regularisations, as in Monras, Vázquez-Grenno and Elias, forthcoming), rather than by restricting migration itself. In fact, these objectives may come into conflict: for example, limitations on access to permanent residency (designed to deter migration) may deliver more market power to firms, and native labour may ultimately suffer. We use our estimates to simulate the effects of a regularisation program: both native and migrant labour (and especially the low skilled) stand to benefit substantially, at the expense of firms. These results highlight the significance of our hypothesis: an understanding of the *origins* (and not just the magnitude) of wage effects is crucial for designing effective policy.

The paper proceeds as follows. Section 2 explains how immigration may reduce the wages of natives through a mark-down effect. Section 3 explores empirical identification in the canonical multi-skill model. In Section 4, we describe our empirical application and data; and Section 5 presents our basic estimates, which reject the canonical model. We argue that this rejection reflects the presence of mark-down effects: in support of this interpretation, we explore heterogeneous effects by migrants' legal status in Section 6. And finally, Section 7 quantifies the impact of an immigration shock and regularisation policy on wages and monopsony rents.

2 Modeling imperfect competition

In this section, we present a simple model to explain how immigration can increase the mark-downs ϕ_j on native wages. The idea is that if firms (i) have greater monopsony power over migrants than natives, but (ii) cannot perfectly wage discriminate between them, an influx of migrants can make the labour market less competitive.

There are a small number of papers which consider the impact of immigration in imperfect labour markets. Chassamboulli and Palivos (2013, 2014), Chassamboulli and Peri (2015), Battisti et al. (2017) and Albert (2021) study matching models with individual wage bargaining: if migrants have low reservation wages, they will stimulate job creation, potentially to the benefit of natives. These models assume free creation of vacancies (and hence zero profits in equilibrium), but this makes it hard to explain why employers actively lobby for more immigration. Additionally, there is growing evidence that firms find it difficult to pay different wages to employees doing identical work (as individual bargaining would imply), especially in low-skill markets (Caldwell and Harmon, 2019; Lachowska et al., 2022; Di Addario et al., 2023). This suggests that wage-posting, rather than bargaining, may often provide a more accurate description of wage-setting.

Malchow-Moller, Munch and Skaksen (2012), Naidu, Nyarko and Wang (2016), Amior and Stuhler (2023), Borjas and Edo (2023) and Gyetvay and Keita (2023) assume, like us, wage-posting by firms with some monopsony power.[3] In these models, firms benefit from immigration because marginal products exceed wages. Our key contribution is to relax the assumption of perfect wage discrimination between equally skilled natives and migrants: this is what causes market power over migrants to "spill over" to natives.

2.1 Labour supply to individual firms

Suppose there are many identical firms, which seek to hire labour of multiple skill types j. Workers of each skill type may be either natives or migrants. We assume that natives and migrants of skill j are productively identical (without loss of generality[4]), but they may differ in their labour supply to firms.

[3] Malchow-Moller, Munch and Skaksen (2012) find that migrant employees depress native wages within Danish firms, and they attribute this to differential outside options. Naidu, Nyarko and Wang (2016) study a UAE reform which relaxed restrictions on migrants' job mobility, and consider the implications for incumbent migrants. Building on insights from our paper, Amior and Stuhler (2023) explore effects across the firm pay distribution (rather than the aggregate-level impact, which is our focus here). Borjas and Edo (2023) study the efficiency implications of market power over migrants, and Gyetvay and Keita (2023) explore how segregation of migrants across firms can diminish the extent of wage competition.

[4] Any imperfect substitutability between natives and migrants can be characterised as reflecting differences in (possibly unobserved) skill j. Note this kind of skill segregation would limit wage competition between natives and migrants, at the aggregate level: we elaborate on this point in Section 3.1 and Appendix B.

The supply of native labour (of skill j) to an individual firm takes the form proposed by Card et al. (2018):

$$N_j(W) = N_{0j}(W - R_{Nj})^{\epsilon_N} \tag{4}$$

where N_{0j} will depend on the wages offered by other firms and the number of natives in the market. R_{Nj} is a reservation wage, below which natives will not work; and the supply curve is iso-elastic in wages W above R_{Nj}. Card et al. (2018) motivate this upward-sloping curve (the source of firms' market power) by idiosyncratic preferences over firms, but it might alternatively be rationalised by search frictions. The supply of migrants to an individual firm takes the same form, but with possibly different reservation wage R_{Mj} and elasticity ϵ_M:

$$M_j(W) = M_{0j}(W - R_{Mj})^{\epsilon_M} \tag{5}$$

There are several reasons why migrants' reservation wages might be lower than equally skilled natives', i.e. $R_{Mj} < R_{Nj}$. First, migrants may use wages in their origin country as a reference point (Constant et al., 2017; Akay, Bargain and Zimmermann, 2017), whether for psychological reasons or due to remittances (Albert and Monras, 2022; Dustmann, Ku and Surovtseva, 2024). They may also discount time in the host country more heavily, if they intend to return eventually (Amior, 2017; Adda, Dustmann and Görlach, 2022) or because of time-limited visas or deportation risk. And they may face restricted access to out-of-work benefits. Using a structural model, Nanos and Schluter (2014) conclude that migrants do indeed demand lower wages (for given productivity). Albert (2021) shows that undocumented migrants exit unemployment relatively quickly, which is consistent with lower reservation wages. Also, Aydemir and Skuterud (2008), De Matos (2017), Arellano-Bover and San (2020), Aslund et al. (2021), Dostie et al. (2023) and Amior and Stuhler (2023) find that migrant-native wage differentials are partly driven by firm effects, which is consistent with migrants accepting offers from lower-paying firms.

There are also several reasons why migrants might have a smaller elasticity parameter: $\epsilon_M < \epsilon_N$. They may be less efficient in job search, due to poor information, language barriers, exclusion from social networks, undocumented status (Kossoudji and Cobb-Clark, 2002; Orrenius and Zavodny, 2009; Brown, Hotchkiss and Quispe-Agnoli, 2013; Hotchkiss and Quispe-Agnoli, 2013), the E-Verify program (which compels employers to authenticate legal status: see e.g. Borjas and Cassidy, 2019, on wage effects), or visa-related restrictions on labour mobility (see e.g. Matloff, 2003; Depew, Norlander and Sørensen, 2017; Hunt and Xie, 2019; Wang, 2021; Doran, Gelber and Isen, 2022 on the H-1B and L-1; see Gibbons et al., 2019, on other US guest worker programs). Consistent with this, Hirsch and Jahn (2015) show that migrants' job separations in Germany are less sensitive to wages than natives'. In Appendix G, we offer similar evidence for the

US (building on Biblarsh and De-Shalit, 2021), and show further that these differentials are largely driven by undocumented migrants. Caldwell and Danieli (2024) also find that migrants suffer from lower-quality outside job options.[5]

If migrants have low reservation wages and supply labour to firms inelastically, this grants firms greater market power. We now explore how firms exercise this market power.

2.2 Optimal wage offers

Suppose firms produce a homogeneous output good, using the technology in (1). They set wages for natives and migrants of each skill type j (W_{Nj} and W_{Mj}) to maximise profit, subject to the labour supply curves (4) and (5). Profit is given by:

$$\max_{W_N, W_M} \pi\left(W_N, W_M\right) = F\left(L_1, ..., L_J; K\right) - rK - \sum_j \left[W_{Nj} N\left(W_{Nj}\right) + W_{Mj} M\left(W_{Mj}\right)\right] \quad (6)$$

where $W_N = \left(W_{N1}, ..., W_{NJ}\right)'$ and $W_M = \left(W_{M1}, ..., W_{MJ}\right)'$ are vectors of skill-specific wages, and r is the rental price of capital.[6] We will consider two wage-setting assumptions: (i) perfect wage discrimination, where the firm is free to set distinct native and migrant wages, and (ii) zero discrimination, where the firm must offer the same wage to all type j workers (i.e. $W_{Nj} = W_{Mj} = W_j$).

(i) Perfect wage discrimination

Assuming perfect discrimination, and given the labour supply curves (4) and (5), the marginal cost functions for native and migrant labour can be written as[7]:

$$MC_{Qj}\left(W\right) = W + \frac{W - R_{Qj}}{\epsilon_Q}, \quad Q = \{N, M\} \quad (7)$$

It is convenient to express marginal costs in (7) as functions of wages, rather than the more conventional quantities. But one form can be converted to the other using the relationship $MC_{Qj}\left(W\right) = \tilde{MC}_{Qj}\left(Q_j\left(W\right)\right)$, where $\tilde{MC}_{Qj}\left(Q_j\right)$ is the usual expression for marginal costs in terms of quantities. Since firms are identical, they will all choose the same wage; so the first order condition of individual firms also expresses the aggregate equilibrium.

[5]There are some reasons why one might expect the reverse. For example, foreign-born workers may be relatively mobile geographically (see e.g. Cadena and Kovak, 2016; Amior, forthcoming, on this question), though this speaks to the elasticity of labour supply to regions and not to individual employers (which is what matters for monopsony power).

[6]For simplicity, we abstract from the choice of capital here: this does not affect the basic argument.

[7]E.g. for natives: $MC_{Nj}\left(W\right) = W + N\left(W\right)/N'\left(W\right)$. Replacing N_j and $N_j'\left(W\right)$ with (4) yields (7).

We also assume that the reservation wage R_{Qj}, while exogenous to each individual firm, is proportional to the aggregate wage: i.e. the replacement ratio R_{Qj}/W_{Qj} is a fixed parameter r_Q. This is convenient for exposition, as it allows us to separate marginal product and mark-down effects. But it is also an attractive assumption, as it ensures that productivity growth does not mechanically make the labour market more competitive (by reducing the value of R_{Qj} relative to wages). Equation (7) can then be written as:

$$MC_{Qj}\left(W_{Qj}\right) = W_{Qj}\left(1 + \frac{1 - r_Q}{\epsilon_Q}\right) = W_{Qj}\left(1 + \frac{1}{\tilde{\epsilon}_Q}\right), \quad Q = \{N, M\} \tag{8}$$

where $\tilde{\epsilon}_Q = \epsilon_Q / \left(1 - r_Q\right)$ is an "adjusted" supply elasticity, which summarises the impact of ϵ_Q and r_Q (and is increasing in both). The marginal cost (8) is increasing in the wage W_j, and it is decreasing in the adjusted elasticity $\tilde{\epsilon}_Q$. For illustration, we plot the MC curves for natives and migrants against wages W in Figure 1, under the assumption that $\tilde{\epsilon}_M < \tilde{\epsilon}_N$. Notice MC_M lies above MC_N: since migrants supply labour less elastically (whether because of a lower r_M or ϵ_M), the cost of raising wages for the infra-marginals (with each new hire) is larger. Profit is maximised where these marginal costs equal the marginal product MP_j. After solving the first order conditions, the optimal native and migrant mark-downs, i.e. ϕ_{Nj} and ϕ_{Mj}, are given by:

$$\phi_{Qj} = \log \frac{MP_j}{W_{Qj}} = \log\left(1 + \frac{1}{\tilde{\epsilon}_Q}\right), \quad Q = \{N, M\} \tag{9}$$

For a given skill type j, natives and migrants share the same marginal product. But if employers have greater market power over migrants (i.e. if $\tilde{\epsilon}_M < \tilde{\epsilon}_N$, as illustrated), they will pay them less than natives: i.e. mark-downs will be larger for migrants ($\phi_{Mj} > \phi_{Nj}$). For simplicity, we illustrate this in Figure 1 for the case where the marginal product is constant; but the result does not depend on this.

Notice also that the marginal cost function in (8), for given wage W_j, does *not* depend on the level of migration; and hence, migration can *only* affect native wages through the marginal products (just as in the canonical model). Intuitively, the ability to perfectly discriminate segregates the native and migrant labour markets; so there is no direct competition between them.

To summarise, under perfect discrimination, native and migrant mark-downs may diverge (according to differences in market power), but these mark-downs will be insensitive to changes in market-level migrant share.

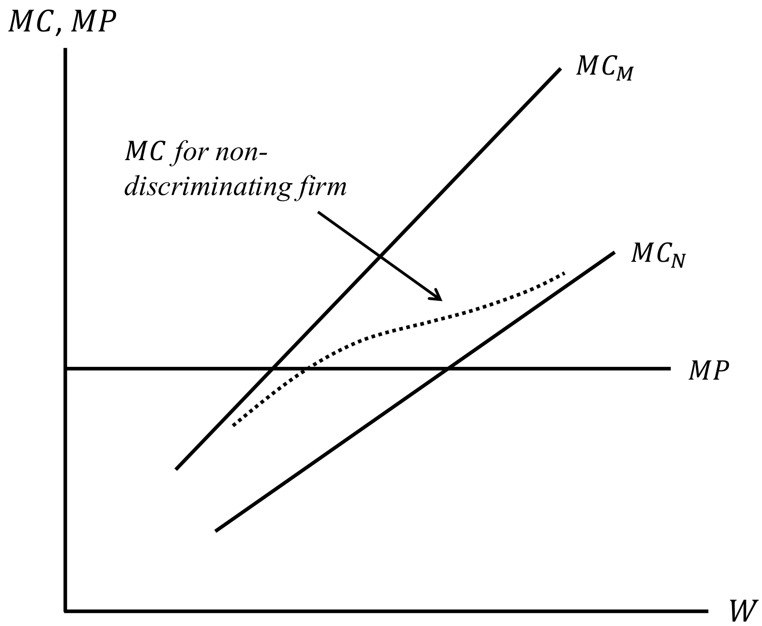

Figure 1: Optimal wages for discriminating and non-discriminating firms

This figure illustrates optimal wages for a firm employing workers of skill type j, assuming $\tilde{\epsilon}_M < \tilde{\epsilon}_N$. Skill j natives and migrants deliver the same marginal product (MP), which we take as given for the purposes of this diagram. For a discriminating firm (which can offer distinct wages to natives and migrants), the marginal cost of native and migrant labour are represented by MC_N and MC_M respectively; and the optimal wages will satisfy $MC_N = MP$ and $MC_M = MP$. For a non-discriminating firm, the marginal cost is represented by the dotted line; and the optimal wage will equate this dotted line with the marginal product.

(ii) Zero wage discrimination

In practice, firms may find it difficult to pay different wages to migrants and natives doing identical work. Several studies show empirically that similarly skilled natives and migrants benefit equally (or close to equally) from working in higher-paying firms: e.g. Arellano-Bover and San (2020); Aslund et al. (2021); Dostie et al. (2023); Amior and Stuhler (2023).

If firms cannot discriminate, natives and migrants will compete directly; and mark-downs *will* depend on the migrant share (as market power over one group "spills over" to the other). In terms of Figure 1, the firm now faces a marginal cost curve (the dotted line) which *mixes* natives and migrants (and lies between MC_N and MC_M). This curve tends towards MC_N as the wage rises (assuming natives supply labour more elastically, they will comprise an ever larger share of the firm's labour pool); and it tends towards MC_M as the wage declines. For a given marginal product, the optimal wage will lie between what a discriminating firm pays to natives and migrants.[8]

Crucially, the MC curve's position will depend on the migrant share: as this share increases, the curve shifts monotonically towards MC_M, and the optimal wage falls.

[8]Appendix A provides a formula for the equilibrium mark-down in the zero discrimination case.

Intuitively, if firms enjoy greater market power over migrant labour, immigration will make the labour market less competitive, allowing firms to extract greater rents from natives and migrants alike. In this way, differential market power over migrants can "spill over" to the wages of natives. If some firms can discriminate and others not, we will have a mixture of the two extreme cases discussed here.

2.3 Mark-down functions

Our model shows that differential market power over migrants can manifest in two different ways: (i) unequal native and migrant mark-downs (if at least some firms can discriminate) and/or (ii) sensitivity of mark-downs to migrant share (if at least some firms cannot). To allow for all possibilities, we define distinct native and migrant mark down functions, $\phi_{Nj}(m_j)$ and $\phi_{Mj}(m_j)$, which are permitted to vary monotonically with the skill-specific migrant share $m_j \equiv M_j/(N_j + M_j)$. The range of possibilities can then be summarised by two hypotheses, which will guide our empirical analysis below:

1. $H1$ **(Equal mark-downs)**: Natives face the same mark-downs as migrants, i.e. $\phi_{Nj}(m_j) = \phi_{Mj}(m_j)$.
2. $H2$ **(Independent mark-downs)**: Natives' mark-downs are independent of migrant share, i.e. $\phi'_{Nj}(m_j) = 0$.

As Table 1 shows, these hypotheses admit four possible cases. The joint hypothesis of $H1$ and $H2$ (top left) is of particular interest. This arises if $\tilde{\epsilon}_M = \tilde{\epsilon}_N$, i.e. firms have identical monopsony power over natives and migrants. As shown above, this ensures that the native and migrant mark-downs are identical and will not depend on the migrant share. Perfect competition (as in the "canonical model") is a special case of this joint hypothesis, with infinite elasticities and both mark-downs fixed at zero.

Differential monopsony power will violate this joint hypothesis. This violation can manifest in two different ways. Suppose for example that $\tilde{\epsilon}_M < \tilde{\epsilon}_N$, i.e. firms have greater market power over migrants. If at least some firms can discriminate, $H1$ will be violated: migrant mark-downs will be larger, i.e. $\phi_{Mj}(m_j) > \phi_{Nj}(m_j)$. And if at least some firms cannot discriminate, $H2$ will be violated: native mark-downs will be increasing in migrant share, i.e. $\phi'_{Nj}(m_j) > 0$.

The top-right quadrant of Table 1 represents the perfect discrimination case (i.e. fixed but unequal mark-downs). The bottom-left represents zero discrimination (equal mark-downs, which are increasing in m_j). The bottom-right represents a more general case, where some firms can discriminate and others not: both $H1$ and $H2$ are violated.

Table 1: Mark-down function hypotheses

		Equal mark-downs ($H1$): Yes	Equal mark-downs ($H1$): No
Independent mark-downs ($H2$)	Yes	$\tilde{\epsilon}_M = \tilde{\epsilon}_N$ (Perf comp. is special case)	$\tilde{\epsilon}_M \neq \tilde{\epsilon}_N$ + Perfect discrimination
	No	$\tilde{\epsilon}_M \neq \tilde{\epsilon}_N$ + Zero discrimination	$\tilde{\epsilon}_M \neq \tilde{\epsilon}_N$ + Partial discrimination

3 Empirical identification in the skill cell model

In this section, we integrate the mark-down functions into the canonical model, by specifying a production technology F. We then explore what can be identified with conventional wage and employment data. Once we allow for mark-down effects, it is no longer possible to *point identify* the technological parameters separately from the mark-down functions – if observably similar natives and migrants are imperfect substitutes. However, we are able to *set* identify key parameters, and to test the joint hypothesis of $H1$ and $H2$ (that native and migrant mark-downs are equal and independent of migrant share). A rejection of the joint hypothesis implies differential market power over natives and migrants.

To make progress, we first require some structure on F. We apply a nested CES technology to education-experience cells, a strategy first implemented in the migration literature by Borjas (2003), building on Card and Lemieux (2001), and later developed by Card (2009), Manacorda, Manning and Wadsworth (2012), Ottaviano and Peri (2012), Blau and Mackie (2017), Llull (2018*a*) and Monras (2020), among others.[9]

Though we mostly rely on a conventional CES structure, we will show below that our results are robust to alternative technological assumptions, and to alternative assignments of natives and migrants to skill cells. To address known concerns about endogeneity, we will develop new instruments for both native and migrant employment. We will mostly focus on national-level variation across skill cells: this makes sense if labour markets are reasonably well-integrated across space (as in e.g. Borjas, 2006; Monras, 2020; Amior, 2024), but we will also estimate specifications which exploit spatial variation.

[9]An alternative tradition advocates using spatial variation and relying on natural experiments: see Dustmann, Schoenberg and Stuhler (2016). Natural experiments can offer clean identification of the overall impact of particular migration events (which bring particular skill mixes and labour market competition effects); but without theoretical assumptions, one cannot extrapolate to other scenarios.

3.1 Production technology

Following Ottaviano and Peri (2012), we assume that aggregate output at time t is given by:

$$Y_t = AL_t^{\lambda} K_t^{1-\lambda} \tag{10}$$

where L_t is a CES labour aggregate of education-specific inputs, L_{et}:

$$L_t = \left(\sum_e \alpha_{et} L_{et}^{\sigma_E} \right)^{\frac{1}{\sigma_E}} \tag{11}$$

The α_{et} are education-specific productivity shifters (which may vary with time), and $1/\left(1-\sigma^E\right)$ is the elasticity of substitution between education groups.

In turn, the education inputs L_{et} depend on (education-specific) experience inputs L_{ext}:

$$L_{et} = \left(\sum_x \alpha_{ext} L_{ext}^{\sigma_X} \right)^{\frac{1}{\sigma_X}} \tag{12}$$

where the α_{ext} encapsulate the relative efficiency of the experience inputs x within each education group e.

Finally, we model the education-experience stocks L_{ext} as an aggregation $Z_{ext}\left(N_{ext}, M_{ext}\right)$ of cell-level native and migrant inputs, N_{ext} and M_{ext}, which are potentially imperfect substitutes: see e.g. Card (2009), Manacorda, Manning and Wadsworth (2012), Ottaviano and Peri (2012) or Piyapromdee (2021). For identification, we require only that Z_{ext} has constant returns (see Appendix F.1). But in practice, we show below that the conventional CES restriction (which all previous papers impose) provides a good fit to the data. Specifically, we parameterise Z_{ext} as:

$$L_{ext} = Z_{ext}\left(N_{ext}, M_{ext}\right) = \left(N_{ext}^{\sigma_Z} + \alpha_{Zext} M_{ext}^{\sigma_Z}\right)^{\frac{1}{\sigma_Z}} \tag{13}$$

where α_{Zext} is a migrant-specific productivity shifter (permitted to vary by cell and time), and $1/\left(1-\sigma^Z\right)$ is the elasticity of substitution between natives and migrants (within education-experience cells). Though we assume a fixed σ_Z here, we consider the possibility of cell-level heterogeneity in σ_Z in Section 5.6.

The conventional interpretation of (13) is that the native and migrant labour inputs, N_{ext} and M_{ext}, are distinct skill types which are imperfect substitutes. But if natives and migrants are indeed perfectly segregated by skill (within education-experience cells), firms should be able to perfectly wage discriminate between them (following the logic of Section 2.2). However, Appendix B shows that an alternative (and perhaps more realistic) interpretation is possible, where L_{ext} is an aggregation of many *unobservable* labour inputs

j, within which natives and migrants are perfect substitutes. In this representation, $N_{ext} = \sum_{j \in ex} N_j$ and $M_{ext} = \sum_{j \in ex} M_j$ are the *aggregate* cell-level native and migrant stocks (across unobservable skill types j); and the derivatives of Z_{ext} (with respect to N_{ext} and M_{ext}) identify the *average* cell-level marginal products. To the extent that natives and migrants are segregated by skill (across the unobservable types j), they will appear as imperfect substitutes to the researcher (at the observable cell level).[10] But since there is now *direct competition* between natives and migrants (within the unobservable markets j), mark-downs may plausibly be sensitive to migrant share (in line with Section 2.2). One interesting implication is that skill segregation shelters natives from these mark-down effects, in the same way as wage discrimination. See Appendix B for a more formal exposition.

3.2 Cell-level specification of native and migrant wages

Using the model above, we can now derive empirical specifications for native and migrant wages (at the level of education-experience cells), denoted W_{Next} and W_{Mext}. Applying the structure of equation (3), W_{Next} is equal to the cell-level marginal product (i.e. the derivative of output Y_t with respect to cell-level native employment N_{ext}) minus the cell-level native mark-down ϕ_{Next}:

$$\log W_{Next} = \log A_{ext} - (1 - \sigma_Z) \log N_{ext} - (\sigma_Z - \sigma_X) \log \left(N_{ext}^{\sigma_Z} + \alpha_{Zext} M_{ext}^{\sigma_Z}\right)^{\frac{1}{\sigma_Z}} - \phi_{Next}(m_{ext}) \tag{14}$$

where the native mark-down ϕ_{Next} is permitted to depend on the cell migrant share, m_{ext}. A_{ext} is a cell-level productivity shifter, which summarises the impact of all other labour market cells, as well as the general level of productivity and the influence of capital:

$$A_{ext} = \frac{\partial Y_t}{\partial L_t} \cdot \frac{\partial L_t}{\partial L_{et}} \alpha_{ext} L_{et}^{1-\sigma_X} \tag{15}$$

We can of course derive a parallel expression for the cell migrant wage W_{Mext}, but it is convenient to summarise this relative to the native wage W_{Next}:

$$\log \frac{W_{Mext}}{W_{Next}} = \log \alpha_{Zext} - (1 - \sigma_Z) \log \frac{M_{ext}}{N_{ext}} - \phi_{Mext}(m_{ext}) + \phi_{Next}(m_{ext}) \tag{16}$$

The relative wage depends on *both* the relative marginal product *and* any difference between the native and migrant mark-downs, ϕ_{Next} and ϕ_{Mext}. In line with Section 2, we permit ϕ_{Next} and ϕ_{Mext} to both differ from one another and to vary with cell migrant share m_{ext}. Their shape depends in principle on differential monopsony power, pay

[10] This is in the spirit of Peri and Sparber (2009), who argue that comparative advantage of natives over migrants in communication tasks (within observable skill cells) leads to imperfect substitutability.

discrimination and skill segregation within cells; but we do not prejudge these questions.

3.3 Identification of technology and mark-down functions

We now show that we *cannot* point identify (i) the technological parameters in the bottom nest Z and (ii) the mark-down functions (ϕ_N, ϕ_M), using standard wage and employment data. But the model is overidentified under the joint hypothesis of $H1$ and $H2$ (equal and independent mark-downs): this hypothesis, which implies equal monopsony power over natives and migrants, can therefore be tested.

The identification argument can be appreciated by inspection of equations (14) and (16). First, consider the equations' *intercepts*. In the native wage equation (14), a larger intercept can be attributed to the productivity shifter A or to an intercept in the mark-down function ϕ_N; and these cannot be disentangled.[11] Similarly, in the relative wage equation (16), a larger intercept may be attributed to either the migrant-native productivity differential α_{Zext} or unequal mark-downs (i.e. $\phi_{Next} \neq \phi_{Mext}$); and again, we cannot disentangle the two. This means we are unable to test $H1$ (equal mark-downs).

We face the same problem in interpreting the equations' *slopes*. Holding native employment N_{ext} fixed, variation in migrant employment M_{ext} (and therefore migrant share m_{ext}) may affect native and migrant wages in (14) and (16) in *three* different ways: through technological substitutability (via the σ_Z parameter), and also through the respective native and migrant mark-down slopes, $\phi'_{Next}(m_{ext})$ and $\phi'_{Mext}(m_{ext})$. But we only have *two* wage equations, so we cannot identify the mark-down slopes without further assumptions; and we therefore cannot test $H2$ (independent mark-downs).

However, we now show that the model is overidentified under the *joint* restriction of $H1$ (equal mark-downs) and $H2$ (independent mark-downs). Our empirical strategy consists of two steps:

Step 1: Conditional on $H1$, estimate the relative wage equation

Assuming equal mark-downs ($H1$), as Card (2009), Manacorda, Manning and Wadsworth (2012) and Ottaviano and Peri (2012) implicitly do, the ϕ terms disappear from the relative wage equation (16). We can then identify the technology parameters α_{Zext} and σ_Z by regressing $\log(W_{Mext}/W_{Next})$ on $\log(M_{ext}/N_{ext})$, exploiting variation across skill cells and over time.

[11] There may also be a price mark-up if the goods market is not competitive. Any such mark-up is unlikely to depend on the cell-level migrant share, so it will be subsumed in the mark-down intercept.

Step 2: Based on Step 1 coefficients, estimate the native wage equation

Rearranging (14), write the native wage equation as:

$$\log W_{Next} + (1 - \sigma_Z) \log N_{ext} = \log A_{ext} - (\sigma_Z - \sigma_X) \log \left(N_{ext}^{\sigma_Z} + \alpha_{Zext} M_{ext}^{\sigma_Z}\right)^{\frac{1}{\sigma_Z}} - \phi_{Next}(m_{ext}) \tag{17}$$

Using our $(\alpha_{Zext}, \sigma_Z)$ estimates from Step 1 (i.e. conditional on $H1$), we can compute (i) the left-hand side term (a linear combination of log native wages and employment[12]) and (ii) the cell "Armington" aggregator $(N_{ext}^{\sigma_Z} + \alpha_{Zext} M_{ext}^{\sigma_Z})^{1/\sigma_Z}$. We can then test $H2$ by regressing $[\log W_{Next} + (1 - \sigma_Z) \log N_{ext}]$ on $\log (N_{ext}^{\sigma_Z} + \alpha_{Zext} M_{ext}^{\sigma_Z})^{1/\sigma_Z}$ and m_{ext}. Conditional on $H1$, a significant effect of m_{ext} would imply a rejection of $\phi'_{Next}(m_{ext}) = 0$ (i.e. independent native mark-downs, $H2$), and therefore a rejection of the joint hypothesis of $H1$ and $H2$. Intuitively, the effect of immigration on the marginal products must enter through the cell aggregator; so conditional on this aggregator, the cell composition m_{ext} will pick up the mark-down effect.

We have framed our argument around a CES production technology, which is universally imposed in the literature. But in Section 5.6, we show that the joint hypothesis can be tested under the weaker assumption that Z has constant returns. Also, note that while the general model is not point identified, we can achieve *set* identification: for any given set of $(\alpha_{Zext}, \sigma_Z)$ values, the coefficient on m_{ext} in the second step identifies the linearised mark-down effect $\phi'_{Next}(m_{ext})$.[13]

4 Data

4.1 Samples and variable definitions

We construct our data in a similar way to Borjas (2003) and Ottaviano and Peri (2012), but extend the time horizon. We use IPUMS census extracts of 1960, 1970, 1980, 1990 and 2000, and American Community Survey (ACS) samples of 2010 and 2019 (Ruggles et al., 2023). Throughout, we exclude under-18s and those living in group quarters.

Like these earlier studies, we group individuals into four education groups in our main specifications: (i) high school dropouts, (ii) high school graduates, (iii) some college and (iv) college graduates[14]; and we divide each education group into eight categories of po-

[12]This type of measure has precedent in the technical change literature (Berman, Bound and Griliches, 1994). E.g. if Z is Cobb-Douglas (so $\sigma_Z = 0$), the left-hand side collapses to the log native wage bill.

[13]We show in Appendix E.4 that a linear approximation is both theoretically reasonable and empirically robust.

[14]Borjas (2014) further divides college graduates into undergraduate and postgraduate degree-holders. We choose not to account for this distinction, as there are very few postgraduates early in our sample.

tential labour market experience[15] (5-year intervals between 1 and 40 years). But we also study specifications with two education groups and four 10-year experience categories.

We identify employment stocks with hours worked, and wages with log weekly earnings of full-time civilian employees (at least 35 hours per week, and 40 weeks per year), weighted by weeks worked – though we study robustness to using hourly wages in Appendix E.3. Like Borjas (2003, 2014), we exclude enrolled students from the wage sample. And we also exclude the top and bottom 1% of wage observations in each cross-section.

In most specifications, we adjust cell-level wages for observable changes in demographic composition over time (separately for natives and migrants, and separately for each of our 32 education-experience cells). Using census and ACS microdata, we purge the effects of a quadratic in age, a postgraduate education indicator (for college graduate cells only), race effects (Hispanic, Asian, black), 12 global regions of origin (for migrants only), and an indicator for recent arrivals (also for migrants only): see Appendix D.1 for details on implementation. This adjustment can help address concerns raised by Ruist (2013) about cell-level changes in migrant workforce composition.

As explained above, we focus on national-level variation across skill cells. But in Section 5.5, we also estimate specifications which exploit spatial variation.

4.2 Instruments

One may be concerned that both native and migrant employment (by education-experience cell) are endogenous to wages. Unobserved cell-specific demand shocks may affect the human capital choices of both natives (Card and Peri, 2016; Hunt, 2017; Llull, 2018*b*) and foreign-born residents, as well as the skill mix of new migrants from abroad (Llull, 2018*a*; Monras, 2020). These shocks may also affect individuals' labour supply choices, even conditional on their education and experience. To address these concerns, we construct instruments (by demographic cell) for each of three worker types: (i) natives, (ii) "old" migrants (living in the US for more than ten years) and (iii) "new" migrants (up to ten years), which are intended to exclude cell-specific innovations to labour demand. Our strategy, which is new to the literature, is to predict the population of each cell using the mechanical aging of cohorts (by education) over time, both in the US and abroad. We discuss each of the three instruments in turn.

(i) Natives. The mechanical aging of native cohorts generates predictable changes in cell population stocks over time, as younger (and better educated) cohorts replace older ones (as in Card and Lemieux, 2001). For natives aged over 33, we predict cell populations using cohort sizes (by education) ten years previously, separately by single-

[15]To predict experience, we assume high school dropouts begin work at 17, high school graduates at 19, those with some college at 21, and college graduates at 23.

year age. For example, the stock of native college graduates aged 50 in 1980 is predicted using the population of 40-year-old native graduates in 1970. This is not feasible for 18-33s: given our assumptions on graduation dates, some of them will not have reached their final education status. In these cases, we allocate the *total* cohort population (by single-year age) to education groups using the same shares as the preceding cohort (i.e. from ten years earlier). Having constructed historical cohort population stocks (ten years before observation year t) by single-year age and education, we then aggregate to 5-year experience groups. We denote our instrument as $\tilde{N}_{ext}$, for each of 32 education-experience cells (e, x) and 7 observation years t (between 1960 and 2019).

(ii) Old migrants. We construct our instrument for "old" migrants $\tilde{M}^{old}_{ext}$ (with more than ten years in the US) in an identical way. Specifically, for over-33s, we use foreign-born cell populations within education cohorts ten years previously; and for 18-33s, we allocate total historical cohort populations to education groups according to the education choices of earlier cohorts.

(iii) New migrants. Analogously to our approach for existing US residents, we predict "new" migrant inflows using historical cohort sizes (by education), but this time in *origin countries*.[16] This is motivated by Hanson, Liu and McIntosh (2017), who relate the rise and fall of US low skilled immigration to changing fertility patterns in Latin America. For each education-experience cell (e, x) and year t, we predict the population of "new" immigrants (with up to ten years in the US) using a weighted aggregate of historical cohort sizes in origin countries (ten years before t), based on data from Barro and Lee (2013). The weights depend on origin-specific emigration propensities (since demographic shifts in certain global regions matter more for immigration to the US) and a time-invariant cell-specific index of geographical mobility (varying by education and experience).[17] See Appendix D.2 for further details. We denote the predicted new migrant stocks (aggregated to cell-level) as $\tilde{M}^{new}_{ext}$. Combining this with the old migrant instrument, we can now predict the *total* migrant stock as $\tilde{M}_{ext} = \tilde{M}^{old}_{ext} + \tilde{M}^{new}_{ext}$.

To summarise, our instruments for natives and older migrants are constructed by tracking *education cohorts* over time (note: they are not merely lags within education-experience cells); and for new immigrants, we exploit changing education cohort sizes *abroad*. The identifying assumption is that current cell-specific demand shocks are uncorrelated with historical demographic shifts and historical education choices, both in the US (for natives and old migrants) and abroad (for new immigrants). Reassuringly,

[16] Llull (2018*a*) and Monras (2020) offer alternative instruments for cell-specific inflows of new migrants: Monras exploits a natural experiment (the Mexican Peso crisis), while Llull bases his instrument on interactions of origin-specific push factors, distance and skill-cell dummies. But for consistency with our approach for existing residents, we instead exploit data on historical cohort sizes.

[17] In practice, our weights are the coefficient estimates from a regression of log population of new migrants (by origin, education, experience and time) on origin fixed effects and the mobility index.

we show in Appendix E.9 that conditioning on observable labour demand shocks (arising from sectoral shifts and automation) does not affect our results. Also, the instruments offer sufficient power to disentangle contemporaneous immigration shocks from those which occurred ten years earlier (Appendix E.8), and to disentangle variation in new and old migrant shares (Appendix E.6). These are demanding tests, which offer strong support for the instruments' validity.

4.3 Descriptive statistics

Table 2 sets out various statistics across our 32 education-experience cells. The average migrant employment share, m_{ext}, was just 5% in 1960 (Panel A), but reached 18% by 2019. This expansion was disproportionately driven by high school dropouts (Panel B). Note there may be some under-count of migrants in the census: this could cause us to over-estimate the effects of migrant share (see Amior, 2024); but we do include a rich set of fixed effects in our empirical models (see below), which will purge any measurement error along these dimensions.

The remaining panels report variation in wages, adjusted for changes in demographic composition. Panel C shows that native wages have declined most among the young and low educated (these changes are normalised to have mean zero across all groups). Panel D sets out the mean migrant-native wage differentials in each cell, averaged over all sample years. In almost all cells, migrants earn less than natives, with wage penalties varying from 0 to 15%, typically larger among high school workers and the middle-aged. In the context of our model, these penalties may reflect differences in within-cell marginal products or alternatively differential mark-downs.

5 Estimates of wage effects

We now turn to our empirical estimates. We begin in Section 5.1 by estimating the relative wage equation (16), which identifies the $(\alpha_{Zext}, \sigma_Z)$ parameters under $H1$. This allows us to construct the cell aggregator in Section 5.2, and to test the joint hypothesis of $H1$ and $H2$ using the native wage equation (17). We ultimately reject this hypothesis: interpreted through the lens of our model, this indicates that firms have greater market power over migrants than natives. Based on a set identification exercise in Section 5.3, we then conclude that firms exploit this differential market power to impose larger mark-downs on natives and migrants alike.

In Section 5.4, we clarify how (and why) our results differ from the existing literature. And finally, in Sections 5.5 and 5.6, we study the robustness of the estimated mark-down

Table 2: Descriptive statistics

	Experience groups							
	1-5	6-10	11-15	16-20	21-25	26-30	31-35	36-40
Panel A: Migrant share of employment hours, 1960								
HS dropouts	0.035	0.037	0.040	0.045	0.045	0.053	0.083	0.127
HS graduates	0.016	0.017	0.024	0.031	0.030	0.046	0.074	0.115
Some college	0.027	0.033	0.041	0.045	0.042	0.058	0.073	0.094
College graduates	0.031	0.038	0.045	0.048	0.058	0.064	0.092	0.111
Panel B: Change in migrant share of employment hours, 1960-2019								
HS dropouts	0.140	0.283	0.419	0.510	0.560	0.586	0.533	0.425
HS graduates	0.078	0.112	0.153	0.186	0.205	0.186	0.124	0.043
Some college	0.059	0.06	0.074	0.085	0.101	0.084	0.059	0.021
College graduates	0.083	0.119	0.139	0.156	0.144	0.119	0.079	0.039
Panel C: Change in log native wages, 1960-2019								
HS dropouts	-0.055	-0.113	-0.122	-0.136	-0.051	-0.066	-0.027	-0.015
HS graduates	-0.229	-0.217	-0.213	-0.133	-0.100	-0.044	-0.022	-0.007
Some college	-0.207	-0.178	-0.112	-0.049	0.013	0.068	0.106	0.132
College graduates	0.055	0.113	0.174	0.226	0.271	0.292	0.325	0.322
Panel D: Mean log migrant-native wage differential								
HS dropouts	-0.035	-0.114	-0.137	-0.134	-0.140	-0.141	-0.121	-0.088
HS graduates	-0.049	-0.112	-0.123	-0.144	-0.140	-0.144	-0.145	-0.130
Some college	-0.037	-0.075	-0.092	-0.095	-0.104	-0.121	-0.106	-0.079
College graduates	0.018	-0.037	-0.060	-0.074	-0.103	-0.132	-0.142	-0.133

Panel A reports migrant employment shares $m \equiv M/(M+N)$ in 1960, across the four education and eight experience groups; and Panel B reports changes in this share over 1960-2019. Panel C reports changes over 1960-2019 in composition-adjusted log native (weekly) wages, normalised to mean zero across all groups. Panel D reports the mean composition-adjusted log migrant-native wage differential, averaged over 1960-2019.

effects to numerous empirical choices and the specification of technology.

5.1 Estimates of relative wage equation under $H1$

We initially parameterise the relative migrant productivity α_{Zext} in the relative wage equation (16) as: $\log \alpha_{Zext} = \log \bar{\alpha}_Z + u_{ext}$, where $\log \bar{\alpha}_Z$ is the mean across education-experience cells, and the deviations u_{ext} have mean zero. Inserting this into (16) and

Table 3: Model for log relative migrant-native wages

	Basic estimates			FE: Edu*Exp, Year		First differences		FD + Year effects	
	OLS	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: OLS and IV estimates									
$\log \frac{M_{ext}}{N_{ext}}$	-0.026***	0.004	0.009	-0.015**	-0.029***	-0.042***	-0.045***	-0.005	-0.003
	(0.004)	(0.004)	(0.007)	(0.007)	(0.010)	(0.007)	(0.008)	(0.007)	(0.006)
Constant (or	-0.122***	-0.094***	-0.084***	-0.126***	-0.153***	-	-	-	-
mean intercept)	(0.012)	(0.013)	(0.019)	(0.013)	(0.020)				
Panel B: First stage estimates									
$\log \frac{\tilde{M}_{ext}}{N_{ext}}$	-	-	1.088***	-	1.102***	-	1.003***	-	1.047***
	-	-	(0.049)	-	(0.072)	-	(0.053)	-	(0.048)
Adjusted wages	N	Y	Y	Y	Y	Y	Y	Y	Y
Observations	224	224	224	224	224	192	192	192	192

Panel A reports estimates of equation (18), across 32 education-experience cells and 7 decadal observations (1960-2019). Wages in column 1 are based on raw means, but are adjusted for composition in remaining columns. Columns 1-3 include no fixed effects, while columns 4-5 control for interacted education-experience and year fixed effects ("FE"). The "constant" row in columns 4-5 reports the mean β_0 intercept, accounting for the fixed effects. Columns 6-9 are estimated in first differences ("FD"), with columns 8-9 controlling additionally for year effects. Panel B reports first stage estimates. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

imposing $H1$ (equal mark-downs) yields the following empirical specification:

$$\log \frac{W_{Mext}}{W_{Next}} = \beta_0 + \beta_1 \log \frac{M_{ext}}{N_{ext}} + u_{ext} \tag{18}$$

where W_{Mext}/W_{Next} is the relative migrant to native wage in the education-experience cell, and M_{ext}/N_{ext} is relative employment. Under $H1$, β_0 identifies $\log \bar{\alpha}_Z$; and β_1 identifies $-(1-\sigma_Z)$, the inverse elasticity of substitution.

We report estimates of (18) in Table 3. Like Ottaviano and Peri (2012), we cluster our standard errors by the 32 education-experience cells; but following the recommendation of Cameron and Miller (2015), we apply a small-sample correction to the cluster-robust standard errors. Note the relevant 95% critical value of the T distribution is 2.04.[18]

In column 1, we present OLS estimates for "raw" wages (i.e. not adjusted for changes in demographic composition): β_0 takes a value of -0.12, and β_1 is -0.026. These numbers are comparable to Ottaviano and Peri (2012).[19] Under H1 (equal mark-downs), β_0 identifies the mean within-cell productivity differential $\log \bar{\alpha}_Z$, and β_1 identifies $-(1-\sigma_Z)$,

[18]Specifically, we scale the standard errors by $\sqrt{[G/(G-1)] \cdot [(N-1)/(N-K)]}$ and use $T(G-1)$ critical values, where G is the number of clusters, and K the number of regressors and fixed effects. As Cameron and Miller (2015) note, this adjustment does not entirely eliminate the bias. But even when we use data with 16 clusters, bootstrapped estimates suggest the bias is small: see Appendix F.3.

[19]For full-time wages of men and women combined, with no fixed effects, Ottaviano and Peri estimate a β_1 of -0.044: see column 4 of their Table 2. The small difference is partly due to our extended year sample (we include 2010 and 2019) and restricted wage sample (like Borjas, 2003, we exclude students).

implying a large elasticity of substitution of $1/(1-\sigma_Z) = 38$ between natives and migrants. But in general, these parameters cannot be separately identified from differentials in the mark-downs: a negative β_0 may reflect larger migrant mark-downs ($\phi_M \geq \phi_N$), and a negative β_1 a greater sensitivity of migrant mark-downs to immigration ($\phi'_M \geq \phi'_N$).

Columns 2-9 report additional specifications, with composition-adjusted wages[20], fixed effects[21], first differencing[22] and IV. Our mean β_0 varies from -0.09 to -0.15, and β_1 from zero to -0.045. In some columns, β_1 is significantly different from zero (as in Ottaviano and Peri, 2012), and in others not (as in Borjas, Grogger and Hanson, 2012). But these differences are quantitatively small: under $H1$, our estimates suggest natives and migrants are either perfect substitutes within education-experience cells (if $\beta_1 = 0$) or very close substitutes; and as we show below, this makes little difference to our estimates of the native wage equation.

One may be concerned that relative migrant supply, M_{ext}/N_{ext}, is endogenous to demand shocks in the error, u_{ext}; though it is not possible to sign the bias.[23] In the IV columns, we instrument $\log(M_{ext}/N_{ext})$ with $\log\left(\tilde{M}_{ext}/\tilde{N}_{ext}\right)$, where $\tilde{M}_{ext} \equiv \tilde{M}_{ext}^{new} + \tilde{M}_{ext}^{old}$ is total predicted migrant employment (described above), and $\tilde{N}_{ext}$ is predicted native employment.[24] In each case, the first stage has considerable power (Panel B); but again, our estimates change little.

To summarise, under $H1$ (equal mark-downs), our estimates suggest that natives and migrants are close substitutes within cells, irrespective of empirical specification.

5.2 Native wage equation: Test of joint hypothesis

We now test the joint hypothesis of equal and independent mark-downs (i.e. $H1$ and $H2$), by estimating the native wage equation (17). We parameterise the cell-level productivity shifter A_{ext} in (15) as:

$$\log A_{ext} = d_{ex} + d_{et} + d_{xt} + v_{ext} \tag{19}$$

where the d_{ex} are education-experience interacted fixed effects, the d_{et} are education-year effects, and the d_{xt} experience-year effects. Comparing to (15), notice the d_{et} pick up

[20] Adjusting wages for composition in column 2 attenuates our β_1 estimate. This reflects shifts in the composition of the migrant workforce, as highlighted by Ruist (2013).

[21] Following Ottaviano and Peri (2012), in columns 4-5, we respecify α_{Zext} to include interacted education-experience and year fixed effects: $\alpha_{Zext} = \alpha_{Zex} + \alpha_{Zt} + u_{ext}$. Instead of a constant, we now report the mean β_0 intercept across all observations (averaging the fixed effects).

[22] In first-differenced specifications (columns 6-7), we regress $\Delta \log(W_{Mext}/W_{Next})$ on $\Delta \log(M_{ext}/N_{ext})$. Columns 8-9 control additionally for year effects.

[23] If cell-level relative employment responds positively to relative migrant demand, our OLS estimates may be be positively biased. However, if native and migrant labour supply elasticities differ (as our estimates in Section E.12 suggest), a balanced cell-level demand shock could generate a negative correlation between relative wages and employment – which would bias the OLS estimates negatively.

[24] In columns 7 and 9, the instrument is differenced – like the endogenous variable.

productivity shocks α_{et} and labour supply effects at the education nest level (i.e. L_{et}), as well as the influence of capital; and the d_{ex} and d_{xt} account for components of the education-specific experience effects α_{ext}. Any remaining variation in the α_{ext} (at the triple interaction) falls into the idiosyncratic v_{ext} term. Our native wage equation (17) can then be estimated linearly using the following specification:

$$[\log W_{Next} + (1-\sigma_Z)\log N_{ext}] = \gamma_0 + \gamma_1 \left[\log\left(N_{ext}^{\sigma_Z} + \alpha_{Zext} M_{ext}^{\sigma_Z}\right)^{\frac{1}{\sigma_Z}}\right] + \gamma_2 m_{ext} + d_{ex} + d_{et} + d_{xt} + v_{ext} \quad (20)$$

which regresses the augmented wage variable (bracketed) on the cell aggregator (also bracketed) and the migrant share m_{ext}, conditional on the fixed effects. We construct the two bracketed terms using our α_{Zext} and σ_Z estimates from Table 3 (under $H1$).[25] Based on (17), γ_1 identifies $\sigma_X - \sigma_Z$, where σ_X is the substitutability between experience groups, and σ_Z the substitutability between natives and migrants (within cells). And γ_2 identifies a linearised mark-down response to the migrant share, m_{ext}. The joint null of equal and independent mark-downs ($H1$ and $H2$) requires that $\gamma_2 = 0$.

The functional form of the cell aggregator is predicated on a CES lower nest; but in Appendix F.1, we show the CES restriction fits the data reasonably well. Similarly, Appendix E.4 shows the linear approximation of the mark-down function (with m_{ext} on the right-hand side) is both theoretically reasonable and empirically robust.

The two right-hand side variables in (20) rely on different sources of variation: native employment N_{ext} increases the aggregator $\log\left(N_{ext}^{\sigma_Z} + \alpha_{Zext} M_{ext}^{\sigma_Z}\right)^{1/\sigma_Z}$ but diminishes the migrant share $m_{ext} \equiv M_{ext}/\left(N_{ext} + M_{ext}\right)$; whereas migrant employment M_{ext} increases both. However, there are endogeneity concerns. First, omitted demand shocks at the interaction of education, experience and time (in v_{ext} in (19)) may generate unwanted selection: through selective immigration (Llull, 2018*a*; Monras, 2020), the human capital choices of existing residents (Hunt, 2017; Llull, 2018*b*), or labour supply choices. Second, native employment N_{ext} appears on both the left and right-hand sides; so any measurement error in N_{ext} or misspecification of the technology will mechanically threaten identification. The direction of the bias is unclear: measurement error or misspecification should bias OLS estimates of γ_1 positively and γ_2 negatively, but we cannot sign the implications of omitted demand shocks (it depends whether native or migrant employment is more responsive). To address these challenges, we construct instruments for the two right-hand side variables using our predicted native and migrant stocks, $\tilde{N}_{ext}$ and $\tilde{M}_{ext}$. We instrument m_{ext} using $\tilde{m}_{ext} \equiv \tilde{M}_{ext}/\left(\tilde{N}_{ext} + \tilde{M}_{ext}\right)$, i.e. the predicted migrant share. And we instrument $\log\left(N_{ext}^{\sigma_Z} + \alpha_{Zext} M_{ext}^{\sigma_Z}\right)^{1/\sigma_Z}$ using $\log\left(\tilde{N}_{ext} + \tilde{M}_{ext}\right)$, i.e. the log

[25] We use our β_1 estimate from column 5 of Table 3, which implies $\sigma_Z = 1 - 0.029$; and we back out the α_{Zext} in each labour market cell as the residual, i.e. $\log \alpha_{Zext} = \log\left(W_{Mext}/W_{Next}\right) - \beta_1 \log\left(M_{ext}/N_{ext}\right)$.

Table 4: Model for native wages: First stage

	Fixed effects		First differences	
	Cell aggregator	Mig share m_{ext}	Cell aggregator	Mig share m_{ext}
	(1)	(2)	(3)	(4)
$\log\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$	1.103***	0.029	0.876***	0.045***
	(0.073)	(0.018)	(0.092)	(0.015)
Predicted share $\tilde{m}_{ext}$	0.698**	1.090***	1.098***	0.995***
	(0.283)	(0.074)	(0.335)	(0.111)
SW F-stat	228.65	193.87	97.61	79.87
Observations	224	224	192	192

This table presents first stage estimates for the native wage equation (20), across 32 education-experience cells and 7 decadal observations (1960-2019). There are two endogenous variables: the cell aggregator $\log\left(N_{ext}^{\sigma_Z}+\alpha_{Zext}M_{ext}^{\sigma_Z}\right)^{1/\sigma_Z}$ and migrant share $m_{ext}\equiv M_{ext}/\left(N_{ext}+M_{ext}\right)$. We identify α_{Zext} and σ_Z using the estimates from column 5 of Table 3 (i.e. assuming equal mark-downs: $H1$). Columns 1-2 control for interacted education-year, experience-year and education-experience fixed effects. In columns 3-4, all variables (and instruments) are differenced and the education-experience effects eliminated. Sanderson-Windmeijer F-statistics account for multiple endogenous variables. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

of predicted cell-level employment.[26]

Table 4 presents our first stage estimates for equation (20), imposing equal mark-downs ($H1$). Each instrument drives its corresponding endogenous variable with considerable power: the Sanderson and Windmeijer (2016) conditional F-statistics, which account for multiple endogenous variables, range from 80 to 230.[27]

In Panel A of Table 5, we present our second stage results (we return to Panel B below). Throughout, we rely on composition-adjusted wages; and we consider different combinations of right-hand side variables. Our estimates of γ_1 are mostly positive (which would imply $\sigma_X > \sigma_Z$) but close to zero. If σ_Z is close to 1 (as Table 3 suggests, at least under $H1$), these γ_1 estimates would then imply $\sigma_X \approx 1$, i.e. experience groups are very close substitutes within education nests. This appears to contradict the prevailing view in the literature; but as we show below, our estimates closely match the seminal work of Card and Lemieux (2001) when we use broader education groups.

The effect of migrant share, γ_2, is universally negative. Its statistical significance in the full specification (columns 3, 6, 7 and 8) leads us to reject the null of independent native mark-downs ($H2$), conditional on equal mark-downs ($H1$). The IV estimates are close

[26] Note these instruments are not functions of the estimated σ_Z or α_{Zext} parameters, so the IV estimates will be immune to any sampling issues arising from our two-step estimation procedure.

[27] These can be assessed against standard Stock and Yogo (2005) weak instrument critical values.

Table 5: Model for native wages: OLS and IV

	Fixed effects						First differences	
	OLS	OLS	OLS	IV	IV	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Imposing equal mark-downs ($H1$)								
Cell aggregator	0.086***		0.058***	0.115***		0.055***	0.037***	0.052***
	(0.021)		(0.011)	(0.031)		(0.014)	(0.010)	(0.018)
Mig share m_{ext}		-0.628***	-0.558***		-0.704***	-0.599***	-0.422***	-0.471***
		(0.058)	(0.037)		(0.062)	(0.062)	(0.043)	(0.079)
Panel B: Imposing $\alpha_{Zext} = \sigma_Z = 1$								
Cell aggregator	0.055***		0.030**	0.082**		0.026*	0.011	0.025
	(0.020)		(0.011)	(0.030)		(0.014)	(0.010)	(0.018)
Mig share m_{ext}		-0.542***	-0.508***		-0.599***	-0.550***	-0.371***	-0.418***
		(0.049)	(0.037)		(0.057)	(0.062)	(0.043)	(0.078)
Observations	224	224	224	224	224	224	192	192

This table presents estimates of the native wage equation (20), across 32 education-experience cells and 7 decadal observations (1960-2019). The dependent variable is $[\log W_{Next} + (1 - \sigma_Z) \log N_{ext}]$. We use composition-adjusted wages throughout. The two regressors are the cell aggregator $\log \left(N_{ext}^{\sigma_Z} + \alpha_{Zext} M_{ext}^{\sigma_Z}\right)^{1/\sigma_Z}$ and migrant share $m_{ext} \equiv M_{ext} / \left(N_{ext} + M_{ext}\right)$. In Panel A, we identify α_{Zext} and σ_Z using the estimates from column 5 of Table 3 (i.e. assuming equal mark-downs: $H1$); and in Panel B, we impose that $\alpha_{Zext} = \sigma_Z = 1$, so the dependent variable collapses to the log native wage, and the cell aggregator to total employment, $\log \left(N_{ext} + M_{ext}\right)$. Columns 1-6 control for interacted education-year, experience-year and education-experience fixed effects. In columns 7-8, all variables (and instruments) are differenced and the education-experience effects eliminated. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

to OLS, which suggests selection is not a significant problem here.[28] For illustration, our IV estimate of γ_2 is -0.60 (column 6 of Panel A), with a standard error of just 0.06. That is, conditional on $H1$, a 1 pp expansion of the migrant share allows firms to mark down native wages by 0.6% more. The first differenced estimates are similar: the equivalent specification yields a γ_2 of -0.47 (in column 8), with a similar standard error.

To summarise, interpreted through the lens of our model, the significant deviation of γ_2 from zero allows us to reject the null of equal and independent mark-downs (i.e. the joint hypothesis of $H1$ and $H2$). This rejected null represents the case of *equal* monopsony power over natives and migrants: see Table 1. If we are willing to assume $H1$ (equal mark-downs), the negative γ_2 additionally indicates that a larger migrant share *increases* the native mark-down. This is consistent with firms enjoying greater market power over migrants, but being unable to perfectly wage discriminate; so that immigration makes the native labour market less competitive.

[28] Llull's (2018*a*) IV estimate of the migrant share effect is more than twice his OLS estimate, though he uses a different instrument.

5.3 Set identification of key parameters

The estimates of γ_2 in Panel A are predicated on $H1$ (equal native and migrant mark-downs). However, we cannot test $H1$ in isolation. If it is not satisfied, the true mark-down effect may be entirely different: conceivably, even its sign may be incorrect.

Though the full model is not identified, it does imply restrictions on sets of parameters; and this allows us to explore the robustness of our conclusions. For any given α_Z and σ_Z, we can use the native wage equation (20) to point identify the mark-down effect. Our strategy is therefore to study how the estimated mark-down effect γ_2 varies across a broad range of α_Z and σ_Z values. This offers a form of set identification, in the sense that only some *combinations* of parameters are consistent with the data.

We begin with a specification where natives and migrants are productively identical within education-experience cells (as in Borjas, 2003): i.e. $\alpha_{Zext} = \sigma_Z = 1$. This case can be interpreted as an opposite extreme to $H1$. While the $H1$ case attributes deviations of β_0 and β_1 from zero (in the relative wage equation) *entirely* to the technology parameters (α_{Zext} and σ_Z), the alternative "equal productivity" case attributes them entirely to native-migrant differentials in mark-downs. In the latter case, the left-hand side of the native wage equation (20) collapses to the log native wage, and the cell aggregator collapses to log total employment, i.e. $\log(N_{ext} + M_{ext})$. We offer estimates for this specification in Panel B of Table 5. As it happens, the results are very similar to Panel A: this is because the α_{Zext} and σ_Z values implied by $H1$ are themselves close to 1.

In Figure 2, we now study how our estimated mark-down effect γ_2 varies across a broader range of (α_Z, σ_Z) values.[29] In Panel A, we focus on the IV fixed effect specification (comparable with column 6 of Table 5), and Panel B repeats the exercise for first differences (comparable with column 8). We offer more complete regression tables for a selection of (α_Z, σ_Z) values in Appendix Table E1.

Compared with other (α_Z, σ_Z) values, our γ_2 estimates in Table 5 (which hover around -0.5) represent a lower bound. As σ_Z decreases from 1, the mark-down effect becomes larger. Intuitively, for lower σ_Z, we are treating natives and migrants as more complementary in technology. This would imply that immigration is more beneficial for native marginal products; and consequently, to rationalise the observable wage variation, we require a more adverse mark-down effect. Notice also the effect of σ_Z diminishes as α_Z declines: if migrants contribute little to output, they will have less influence on native marginal products, so the value of σ_Z becomes moot. In the limit, when α_Z reaches zero, the cell aggregator collapses to the native stock; so σ_Z has no influence.

[29] Note these calibrations permit the native-migrant mark-down differential (within cells) to have the opposite sign to the productivity differential. In this exercise, we impose equal α_Z values in every labour market cell.

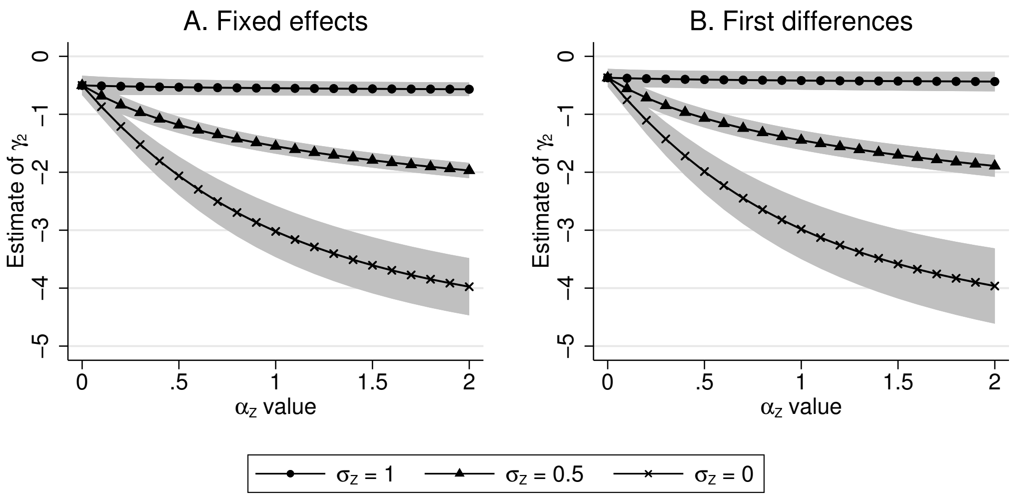

Figure 2: Native mark-down effect γ_2 for different (α_Z, σ_Z)

This figure reports IV estimates of the mark-down effect γ_2 in the native wage equation (20), for a range of (α_Z, σ_Z) values. The estimates for $\alpha_{Zext} = \sigma_Z = 1$ are identical to columns 6 and 8 of Table 5 (Panel B). The shaded areas are 95% confidence intervals. See Appendix Table E1 for corresponding regression tables.

To summarise, these results suggest we can reject $H2$ (independent mark-downs) for a broad range of parameter values. In particular, we find evidence that the native mark-down is *increasing* in the cell migrant share. In terms of the model, this suggests that: (i) firms have greater monopsony power over migrants than natives (whether because migrants have lower reservation wages or supply labour less elastically to firms); *and* (ii) firms cannot perfectly discriminate in their wage offers.

5.4 Comparison with existing empirical literature

We are not the first to estimate a native wage equation across education-experience cells. The novelty here is to control simultaneously for *both* the cell aggregator (accounting for imperfect substitutability) *and* cell composition (the migrant share); whereas other studies just include one or the other. Empirically, we are able to include both because the native stock N_{ext} and migrant stock M_{ext} (both suitably instrumented) provide two alternative sources of variation: as we explain above, M_{ext} enters both the aggregator and migrant share positively, but N_{ext} affects them in opposite directions.

Borjas (2003; 2014) and Ottaviano and Peri (2012) study a specification with the cell aggregator alone, to estimate the substitutability σ_X between experience groups within education nests (building on Card and Lemieux, 2001). Borjas (2003) estimates a coefficient γ_1 of -0.29 on the cell aggregator (implying an elasticity of substitution of 3.4, assuming $\sigma_Z = 1$), and Ottaviano and Peri's preferred estimate is -0.16; whereas our estimates of γ_1 are close to zero. Both Borjas and Ottaviano and Peri instrument the cell aggregator using total migrant labour hours: this instrument will violate the exclusion restriction if, as our model suggests, migrant composition enters wages independently (through the mark-down effect). In contrast, we identify distinct effects of the cell aggregator and cell composition, using two distinct instruments.

Borjas (2003) also estimates a version of equation (20) which excludes the cell aggregator, implicitly imposing $\gamma_1 = 0$. His motivation is to generate descriptive estimates (i.e. without imposing theoretical structure) of the effect of immigration. The effect of migrant share varies from -0.5 or -0.6, similar to our own estimates of γ_2. Card and Peri (2016) argue that Borjas is picking up the effect of the cell aggregator (which is omitted); but this is not true of our specification, which directly conditions on the aggregator. Peri and Sparber (2011) and Card and Peri (2016) also critique Borjas' specification on the grounds of endogeneity: since native employment N_{ext} appears in the denominator of the migrant share $M_{ext}/\left(N_{ext} + M_{ext}\right)$, unobserved cell-specific demand shocks (which raise wages and draw in natives) may generate a spurious negative relationship between wages and migrant share. We address this concern by using instruments.

To summarise, relative to this empirical literature, we identify distinct effects of (i) the cell aggregator and (ii) cell mix, using new instruments. And we offer a novel interpretation of these distinct effects: while the aggregator captures the impact of marginal products, cell mix captures the mark-down effects. Though we apply this insight to national skill cell variation, it has implications for the interpretation of structural migration models more generally. It is also closely related to Beaudry, Green and Sand (2012): they show that the wage bargain in a given job (*conditional* on its productivity) depends on the labour market's industrial composition, since this affects the set of outside options. In their story, it is the composition of industries which matters; while in ours, it is the composition of the labour force itself.

5.5 Robustness of migrant share effect

We now consider the robustness of the migrant share effect, γ_2, in the native wage equation (20). We briefly summarise our tests here, and offer greater detail and formal estimates in the appendices. For simplicity, we impose $\alpha_{Zext} = \sigma_Z = 1$ throughout; so the dependent variable in (20) collapses to the log native wage, and the cell aggregator collapses to log total employment: as Table 5 shows, this makes little difference to the results.

(i) Outliers (Appendix E.2). Figure E1 shows scatter plots of our γ_2 estimates (OLS and IV), after partialing out controls. The effects are visibly not driven by outliers.

(ii) Wage definition and weighting (Appendix E.3). In Table E2, we estimate the effects of migrant share on male and female wages separately, and on hourly instead of full-time weekly wages. Like Ottaviano and Peri (2012), we also try weighting observations by total cell employment. In each case, the γ_2 estimates are little affected.

(iii) Functional form of mark-down function (Appendix E.4). We show that a linear approximation of the mark-down function (which we implement above) is theoretically reasonable, and we explore this empirically in Table E3. Replacing the linear approximation m_{ext} in (20) with an alternative functional form, the log relative migrant supply $\log\left(m_{ext}/\left(1-m_{ext}\right)\right)$, yields qualitatively similar results. But in a horse-race between the two, the migrant share m_{ext} picks up the entire effect, just as our model predicts.

(iv) Instrument specification (Appendix E.5). One may worry the power of the migrant share instrument $\tilde{m}_{ext} \equiv \tilde{M}_{ext}/\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$ comes from the native stock predictor $\tilde{N}_{ext}$ (in the denominator of $\tilde{m}_{ext}$) rather than the migrant stock predictor $\tilde{M}_{ext}$. See Clemens and Hunt (2019) for a related criticism. Reassuringly though, when we use $\tilde{M}_{ext}$ as an instrument instead of $\tilde{m}_{ext}$, our γ_2 estimates remain large and significant.

(v) New and old migrant instruments (Appendix E.6). Recall that our migrant share instrument $\tilde{m}_{ext}$ aggregates distinct components for new migrants (up to ten years in the US) and old migrants (more than ten). Reassuringly, it turns out each component does individually elicit the migrants we intend; and we have sufficient power to identify the wage effects of each group separately (at least conditional on fixed effects), after breaking the instrument into two. As it happens, both new and old migrants have large and negative effects on native wages.

(vi) Heterogeneity by education and experience (Appendix E.7). One may wonder whether the migrant share effects are driven exclusively by certain parts of the skill distribution. But in most specifications, we do not find significant evidence of heterogeneous effects by education or experience groups.

(vii) Dynamics (Appendix E.8). Another possible issue is serial correlation in the migrant share. If wages adjust sluggishly, the lagged migrant share will be an omitted variable; and in the presence of serial correlation, our γ_2 estimate may be biased (Jaeger, Ruist and Stuhler, 2018). However, our instruments have sufficient power to disentangle the effects of contemporaneous and lagged immigration shocks; and at least in IV, these dynamics are statistically insignificant (i.e. past shocks have no influence on current wages). This supports our contention that we are identifying the long run effects of immigration.

(viii) Robustness to observable demand shocks (Appendix E.9). In principle, our instruments should exclude shifts in labour demand, conditional on the fixed effects. In support of this restriction, we show that the native wage effects are robust to controlling for a cell-specific Bartik industry shift-share (which captures the predictable effects of the decline in manufacturing) and to initial cell-level routine and offshorable occupation shares (which capture vulnerability to technological change).

(ix) Robustness to minimum wage effects (Appendix E.10). The native wage effects are also robust to controlling for changes in the real value of the minimum wage. We implement this test by defining a cell-specific shift-share, which accounts for differential exposure to changes in minimum wages.

(x) Spatial variation (Appendix E.11). Above, we study national-level skill cell variation. But we find similar effects if we additionally exploit *cross-state* variation in immigration intensity, within education-experience cells. Interestingly though, if we include the original *national-level* shock in the same regression, this (and not the state-specific shock) picks up the entire wage effect. This supports the view that, at least over decadal intervals, labour markets are reasonably well-integrated nationally (e.g. Borjas, 2006; Monras, 2020; Amior, 2024).

(xi) Labour supply responses (Appendix E.12). Our estimates do not assume labour supply is inelastic, since we use employment (suitably instrumented) and not population in our regressions. For completeness though, we estimate labour supply responses in Table E13. We find that an a larger migrant share does reduce native employment rates, as in e.g. Borjas (2003) or Monras (2020). Since these are responses to migrant share (and not to total cell employment), we interpret them as arising from changes in *mark-downs* rather than marginal products.

Given these labour supply effects, one might worry that our wage estimates are conflated with unobserved *compositional* changes (Bratsberg and Raaum, 2012; Borjas and Edo, forthcoming), e.g. if the low-paid disproportionately exit employment. But it appears this is not a major concern here. As Table E13 shows, immigration only reduces the employment rates of native *women* (consistent with French evidence from Borjas and Edo, forthcoming); but despite this, the wage effects for women are very similar to men (Table E2).

5.6 Sensitivity to specification of technology

Above, we reject the overidentifying restrictions of the canonical model, and we offer a theory (of imperfect competition) which can account for this rejection. However, this rejection may in principle also reflect a misspecification of the production technology. In this section, we study the sensitivity of our estimates to various features of the production technology. As above, we discuss each point briefly here, and offer greater detail and regression tables in the marked appendices.

We focus our analysis on the specification of the education-experience cells, as this is what matters for the estimates in Table 5. The influence of restrictions *above* these cells (e.g. on the substitutability between education or experience groups) are absorbed by the fixed effects in equation (20), so they do not matter for this test – though such restrictions will be important for the simulation exercises in Section 7 below.

(i) Assumption of CES technology (Appendix F.1). To estimate the native wage equation (20), we need to construct a cell-level aggregator $Z(N, M)$ over native and migrant employment. Following the literature, we have assumed Z has CES form. But our identification strategy can be generalised to any Z with constant returns. Under constant returns, Appendix F.1 shows that the relative wage W_{Mext}/W_{Next} (of migrants to natives) can always be reduced to some function of the relative supply M_{ext}/N_{ext}, which can in principle be estimated (allowing us to construct Z). But this more flexible approach is ultimately redundant. As equation (16) shows, CES implies this relationship is log-linear; and we show graphically in Figure F1 that this restriction does indeed appear reasonable.

(ii) Cross-cell heterogeneity in σ_Z (Appendix F.2). In the relative wage equation (18), we implicitly assume that σ_Z (the within-cell substitutability between natives and migrants) is identical across cells. This restriction matters for the construction of the cell aggregator, on the right-hand side of the native wage equation (20). But in Table F1, we show there is little heterogeneity in the relative wage effect across college/non-college cells and high/low experience cells.

(iii) Broad education groups (Appendix F.3). Our results are also robust to a specification with two education groups (college and high school "equivalents"), instead of four.[30] Similar to Table 5, we estimate the native wage equation both under the assumption of equal mark-downs ($H1$) and under $\alpha_{Zext} = \sigma_Z = 1$. The γ_2 estimates (on migrant share) in the native wage equation are larger than before, exceeding -1 under fixed effects, and ranging from -0.6 to -1.3 in first differences (Table F2). Interestingly, γ_1 (the elasticity to total cell employment) is now consistently negative in the $\alpha_{Zext} = \sigma_Z = 1$ specification, taking a value of -0.1 under fixed effects. This implies an elasticity of substitution between experience groups of 10, which matches Card and Lemieux (2001).[31]

(iv) Broad experience groups (Appendix F.3). We also re-estimate our model using four 10-year experience groups (rather than eight 5-year groups). This makes little difference to our coefficient estimates and standard errors.[32] Teulings (2000) makes the point that elasticities of substitution will be biased if skill types are too aggregated, and

[30] As Card (2009) notes, a four-group scheme implicitly constrains the elasticity of substitution between any two groups to be identical; but there is evidence that high-school graduates and dropouts are closer substitutes with each other than with college graduates.

[31] In their main specification, Card and Lemieux estimate an elasticity of substitution of 5 across age (rather than experience) groups; but they also offer experience-based estimates which are similar to ours.

[32] The fact that the standard errors are unaffected is important. One may be concerned that statistical dependence between the 5-year experience (by education) clusters in the baseline specification could bias the standard errors, but the robustness of the standard errors to the 10-year grouping should reassure.

proposes an alternative based on continuous skill types: this would be an interesting extension, but very different from our approach here and in the broader literature.

(v) Allocation of migrants to native cells (Appendix F.4). Above, we allocate migrants to native cells according to their education and experience. But if migrants "downgrade" occupation and compete with natives of lower education or experience, this would generate measurement error in the cell migrant stocks (Dustmann, Schoenberg and Stuhler, 2016). In Appendix F.4, in the spirit of Card (2001) and Sharpe and Bollinger (2020), we probabilistically allocate migrants (of given education and experience) to native cells according to their occupational distribution. Again, we continue to see large effects of migrant share, in both the $H1$ and $\alpha_{Zext} = \sigma_Z = 1$ specifications.

6 Heterogeneity by migrants' legal status

Above, we have shown that cell mix affects natives wages independently of the cell aggregator (even accounting for imperfect substitutability within cells), and we have argued this is consistent with immigration making native labour markets less competitive. Our model attributes this to differential market power over migrants, coupled with an inability to perfectly wage discriminate. This differential power may be rationalised in various ways (see Section 2.1), but the common thread is a migrant labour force which lacks credible outside options. This is especially true of the undocumented, due to deportation risk and legal risk of firms: see e.g. Kossoudji and Cobb-Clark (2002); Orrenius and Zavodny (2009); Brown, Hotchkiss and Quispe-Agnoli (2013); Borjas and Cassidy (2019); Albert (2021).

Motivated by this insight, this section explores heterogeneity in wage effects by migrants' legal status. We first offer evidence that firms have significantly more market power over undocumented migrants, based on separation elasticities. We should then expect undocumented migrants to have larger effects on native mark-downs, at least if firms cannot wage discriminate disproportionately against them (i.e. pay them differently for identical work, within the same workplace). We are not aware of evidence on differential discrimination against undocumented migrants within firms. But in practice, we do find that undocumented migrants generate larger mark-down effects for natives, suggesting a limited ability to wage discriminate.

6.1 Heterogeneity in separation elasticities

In Appendix G, similar to Hotchkiss and Quispe-Agnoli (2013), Hirsch and Jahn (2015) and Biblarsh and De-Shalit (2021), we estimate separation elasticities (to initial wages) for natives and migrants, using the Survey of Income and Program Participation (which tracks individual workers over time). Separation elasticities offer a useful (and easily estimable) indicator of the elasticity of labour supply to a firm and hence monopsony power: see Manning (2003). Though there may be biases in the estimated level of the separations elasticity, the comparisons *between* demographic groups can still be informative.

Notably, the elasticities are significantly smaller for migrants, suggesting that firms have greater wage-setting power over migrant labour. The differential is mostly driven by non-college migrants, whose elasticity is just one third of natives. And for those low-educated migrants without permanent residency (which, unusually, the SIPP reports), many of whom are likely undocumented (Hall, Greenman and Farkas, 2010), the elasticity is close to zero. Based on our model, we should then expect that undocumented migrants generate larger mark-down effects for natives (subject to the wage discrimination caveat above); and we now test this claim, using variation across education-experience cells.

6.2 Imputation of cell-level undocumented migrant shares

The first challenge is to measure undocumented migrant employment by education-experience cell and year. Legal status is unobserved in the census, and our approach is to probabilistically assign it by country of origin. The US Department of Homeland Security (2003) produces "residual" estimates of the undocumented population by origin, by subtracting official counts of legal migrants from census estimates of the foreign-born population. Using these estimates, we impute the undocumented employment stock in cell (e, x) at time t as: $M^{undoc}_{ext} = \sum_o \psi_o M_{oext}$, where ψ_o is the estimated undocumented share of origin o migrants (from the DHS), and M_{oext} is the origin o migrant stock in the cell at time t (computed in the census). We rely on a time-invariant measure of ψ_o (using estimates for 2000), but these shares are very stable over time.[33]

Table 6 reports (imputed) undocumented shares of total migrant employment. On average, between 1960 and 2019, 21% of migrant employment was undocumented. They are heavily over-represented at low levels of education: this reflects the skill distribution of Mexicans and other Latin Americans, who dominate the undocumented population. Our estimates of undocumented shares by education are almost identical to Passel and Cohn (2016) and Borjas (2017*b*), despite methodological differences (see Appendix D.4). Table 6 also reports wage differentials (within education-experience cells) relative to natives: these are about twice as large for undocumented migrants. Again, these within-cell wage

[33]Consider the undocumented share of Mexican-born migrants, who account for about half of undocumented migrants (Passel and Cohn, 2016). In 1980, 48.6% of Mexican migrants were undocumented (see Table 2 of Warren and Passel, 1987). This rises to 52.4% in 2000 (Table 2 of Department of Homeland Security, 2003), and falls back to 48% in 2014 (Passel and Cohn, 2016).

Table 6: Employment and wages of legal and undocumented migrants

	HS dropouts	HS grads	Some coll	Coll grads	Average
Undocumented share of migrant employment	0.334	0.230	0.174	0.107	0.207
Within-cell wage differentials:					
Legal migrants v natives	-0.094	-0.111	-0.062	-0.013	-0.067
Undocumented migrants v natives	-0.182	-0.172	-0.152	-0.154	-0.165

Migrants are probabilistically assigned legal status by country of origin, using Department of Homeland Security (2003) estimates. The top row reports the imputed undocumented share of migrant employment. The second reports differentials in log wages between undocumented migrants and natives, and the third repeats for legal migrants. Wage differentials are computed within education-experience cells, and then averaged across groups. Wages are adjusted for cell-level changes in demographic composition, separately for natives, legal migrants and undocumented migrants, according to the procedure of Appendix D.1. All statistics are averages over 1960-2019.

differentials are almost identical to Borjas (2017*a*): see his Table 2.

6.3 Heterogeneity in native wage effects

We now re-estimate the native wage equation (20), but decomposing the overall migrant employment share m_{ext} into (i) the cell's legal migrant share $m_{ext}^{legal} \equiv M_{ext}^{legal} / (N_{ext} + M_{ext})$ and (ii) the undocumented share $m_{ext}^{undoc} \equiv M_{ext}^{undoc} / (N_{ext} + M_{ext})$. For IV, we require distinct instruments for each. Since Mexicans account for half the undocumented population (Passel and Cohn, 2016), our chosen instruments are the predicted cell-level shares of (i) old Mexican migrants and (ii) old non-Mexican migrants.[34] We exclude new migrants from our instruments: in practice, they dilute the instruments' power. Table 7 shows the instruments perform well: the Mexican instrument positively affects both the legal and undocumented shares, but the non-Mexican instrument only positively affects the former. This makes sense: non-Mexican migrants mostly have legal status. The F-statistics range from 20 to 40.

In Table 8, we turn to the wage equation itself. For simplicity, we impose $\alpha_{Zext} = \sigma_Z = 1$ throughout, so the dependent variable is simply the log native wage. In OLS and IV, the undocumented share captures the entire effect, with coefficients between -0.6 and -1.2 (and standard errors of 0.3 or 0.4). The dominant role of undocumented migrants is remarkable, given they account for just 21% of migrant employment in our sample (Table 6). One might worry about what the (multiple) instruments are doing, so we also report reduced form estimates. Again, we see the same story: the instrument for old Mexicans (which drives the undocumented share) picks up the entire effect (now with much smaller standard errors), and the non-Mexican instrument is insignificant. These results are consistent with Edo (2015), who finds that the wage effects of migration in

[34]We construct these by following the procedure for old migrants (i.e. with more than ten years in the US) described in Section 4.2, but tracking cohorts of Mexicans and non-Mexicans respectively.

Table 7: Effects of legal and undocumented migrants: First stage

	Fixed effects			First differences		
	$\log(N_{ext}+M_{ext})$	m_{ext}^{legal}	m_{ext}^{undoc}	$\log(N_{ext}+M_{ext})$	m_{ext}^{legal}	m_{ext}^{undoc}
	(1)	(2)	(3)	(4)	(5)	(6)
$\log\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$	0.817***	-0.013*	0.006	0.658***	-0.007	0.009*
	(0.087)	(0.007)	(0.007)	(0.101)	(0.007)	(0.005)
$\tilde{m}_{ext}^{old,nonmex}$	-4.674***	0.394**	-0.327**	-3.651***	0.708***	-0.048
	(1.120)	(0.148)	(0.154)	(1.051)	(0.143)	(0.117)
$\tilde{m}_{ext}^{old,mex}$	0.068	0.636***	0.641***	1.145**	0.668***	0.687***
	(0.423)	(0.044)	(0.054)	(0.432)	(0.069)	(0.056)
SW F-stat	24.89	20.10	20.27	21.60	22.80	36.85
Observations	224	224	224	192	192	192

This table presents first stage estimates for the native wage equation (20), but accounting separately for the cell employment shares of legal migrants $m_{ext}^{legal} \equiv M_{ext}^{legal}/(N_{ext}+M_{ext})$ and undocumented migrants $m_{ext}^{undoc} \equiv M_{ext}^{undoc}/(N_{ext}+M_{ext})$. We asssume $\alpha_{Zext} = \sigma_Z = 1$, so the cell aggregator (the third endogenous variable) collapses to $\log(N_{ext}+M_{ext})$. Our instruments are (i) log total predicted employment, (ii) the predicted employment share of old Mexican migrants $\tilde{m}_{ext}^{old,mex} \equiv \tilde{M}_{ext}^{old,mex}/\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$, and (iii) the predicted share of old non-Mexicans $\tilde{m}_{ext}^{old,nonmex} \equiv \tilde{M}_{ext}^{old,nonmex}/\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$. Columns 1-3 control for interacted education-year, experience-year and education-experience fixed effects. In columns 4-6, all variables (and instruments) are differenced and the education-experience effects eliminated. Sanderson-Windmeijer F-statistics account for multiple endogenous variables. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Effects of legal and undocumented migrants: OLS, IV and reduced form

	Fixed effects			First differences		
	OLS	IV	RF	OLS	IV	RF
	(1)	(2)	(3)	(4)	(5)	(6)
$\log(N_{ext}+M_{ext})$	0.057***	0.057*		0.022	0.056	
	(0.017)	(0.029)		(0.017)	(0.033)	
m_{ext}^{legal}	0.048	0.062		-0.135	0.142	
	(0.268)	(0.426)		(0.240)	(0.371)	
m_{ext}^{undoc}	-1.081***	-1.193***		-0.638**	-1.061**	
	(0.303)	(0.401)		(0.293)	(0.403)	
$\log\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$			0.038**			0.027*
			(0.018)			(0.016)
$\tilde{m}_{ext}^{old,nonmex}$			0.151			-0.055
			(0.198)			(0.160)
$\tilde{m}_{ext}^{old,mex}$			-0.721***			-0.569***
			(0.072)			(0.094)
Observations	224	224	224	192	192	192

This table presents OLS, IV and reduced form estimates of the native wage equation (20), but accounting separately for the cell employment shares of legal and undocumented migrants. We assume $\alpha_{Zext} = \sigma_Z = 1$, so the dependent variable collapses to the log native wage, and the cell aggregator on the right-hand side collapses to $\log(N_{ext}+M_{ext})$. The reduced form specification replaces the endogenous variables with their three instruments. Columns 1-3 control for interacted education-year, experience-year and education-experience fixed effects. In columns 4-6, all variables (and instruments) are differenced and the education-experience effects eliminated. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

France are driven by non-citizens (and not by naturalised migrants).

To summarise, our estimates of separation elasticities suggest that firms have significantly more market power over undocumented migrants. Based on our model, we should then expect that undocumented migrants generate larger mark-down effects; and this is confirmed by Table 8.

7 Quantifying the immigration surplus

Under perfect competition, Borjas (1995) famously shows that immigration generates a surplus for native workers (in a single-good economy with up to two labour types); and Amior and Manning (2024) generalise this result to any number of labour types and (intermediate or final) goods. But this surplus is commonly believed to be small: see e.g. Borjas (1995) or Ottaviano and Peri (2012). We now show how the introduction of monopsony affects wages and profits, based on our estimates above.

We consider two counterfactuals. The first is an immigration shock equal to 1% of total employment in 2019, holding migrants' skill mix fixed. And the second, motivated by the discussion above, is a "regularisation" policy which transforms a portion of undocumented migrants (equal to 1% of total employment, or 25% of undocumented employment) to legal migrants, within education-experience cells.[35]

We simulate these counterfactuals in a "long run" scenario (where capital inputs are supplied elastically) and assuming that workers supply labour to the market inelastically (so the welfare effects can be summarised by changes in wages). This exercise requires a calibration of the entire nested CES production technology. We restrict attention to our baseline structure, with four education groups and eight experience groups. Our estimates above focus only on the lowest nest, at the level of education-experience cells. For comparability, we calibrate the upper nests using Ottaviano and Peri's (2012) estimates (based on their "Model A"): we set σ_E (the substitutability between composite education inputs, L_e) in equation (11) to 0.7, and σ_X (between experience inputs, L_{ex}) to 0.84.[36] We explain exactly how we compute the counterfactuals in Appendix C. For simplicity, for the immigration shock counterfactual, we ignore any differences between legal and undocumented migrants, and rely instead on our baseline IV mark-down effects from Table 5.

[35] Our predictions can only be interpreted as first-order approximations, as they rely on linearised estimates of mark-down effects. Hence we prefer to focus on small 1% shocks.

[36] Blau and Mackie (2017) report a similar exercise for several different scenarios reflecting different assumptions about the elasticity of substitution (under perfect competition): see e.g. footnote 37 below. But since the focus of our paper is the implications of monopsony power, we restrict attention to one set of upper-nest elasticities. Importantly, the mark-down effects are independent of these assumptions.

7.1 Immigration shock: Perfect competition

We begin with the 1% immigration shock: Table 9 presents our results. Column 1 reports estimates under perfect competition, the conventional case. Given zero mark-downs, the within-cell substitutability between natives and migrants (σ_Z) and relative migrant productivity (α_{Zex}) are identified by the relative wage equation (18). Using estimates of (18) from Table 3 (column 5), we predict the change in native and migrant wages (Panels A and B) and the change in output and its distribution (Panel C) following the immigration shock. Appendix C.1 describes how these effects are computed: they account for the effect of immigration in each cell on every other cell.

As the immigration surplus theorem requires, the average native wage rises in response to the shock. The effect is small (0.04%), but this hides large distributional effects. The wage of native high-school dropouts declines by 0.48% (as this is where migrants are concentrated), but this is offset by wage increases in other education groups.[37] Migrant wages contract in all groups (and especially among dropouts), because natives and migrants are treated here as imperfect substitutes within cells.

Panel C predicts the % change in long run output (net of the costs of elastic capital inputs) and its distribution. Net output rises because the labour force expands; but the increase is a little less than 1%, due to diminishing returns to individual factors and migrants' over-representation in low-wage cells. Given perfect competition and constant returns, net output is fully exhausted by wage income. Total migrant wage income rises, but by less than proportionally to the 1% immigration shock (as their wages fall). And total native wage income expands because their wages grow on average.

7.2 Immigration shock: Equal monopsony power

Column 2 introduces monopsony power. We begin by assuming equal market power over natives and migrants. Hence, native and migrant mark-downs are equal ($H1$), and do not depend on cell migrant share ($H2$). A crucial parameter here is the baseline mark-down level. As we explain above, this is not identified by our model; and there is no commonly accepted estimate in the literature. For illustrative purposes, we calibrate this to 10% (i.e. $\phi_N = 0.1$). This seems reasonable, perhaps on the conservative side: e.g. Lamadon, Mogstad and Setzler (2022) estimate an average US mark-down of 15%, and Kroft et al. (2020) find mark-downs of 20% in the construction sector. Since native and migrant

[37] Card (2009), Ottaviano and Peri (2012) and Blau and Mackie (2017) emphasise that these distributional effects are much smaller if high school dropouts and graduates are treated as close substitutes. In this case, wage effects will only materialise to the extent that natives and migrants differ in college share; but differences in college share are known to be small. Our purpose in this paper is not to revisit this debate, but rather to study the implications of monopsony power.

Table 9: Simulation of 1% immigration shock

	Perfect competition	Equal monopsony power	Differential monopsony power	
	(1)	(2)	(3)	(4)
Impose equal mark-downs ($H1$)?	Yes	Yes	Yes	No
Impose $\alpha_{Zex} = \sigma_Z = 1$?	No	No	No	Yes
Baseline native mark-down	0	0.1	0.1	0.1
Native mark-down response to migrant share	0	0	0.6	0.6
Panel A: Native wages (% changes)				
HS dropouts	-0.479	-0.479	-1.253	-1.358
HS graduates	0.035	0.035	-0.460	-0.506
Some college	0.114	0.114	-0.243	-0.264
College graduates	0.022	0.022	-0.459	-0.477
Average	0.038	0.038	-0.418	-0.444
Panel B: Migrant wages (% changes)				
HS dropouts	-0.681	-0.681	-1.480	-1.596
HS graduates	-0.134	-0.134	-0.654	-0.703
Some college	-0.050	-0.050	-0.415	-0.437
College graduates	-0.144	-0.144	-0.636	-0.654
Average	-0.185	-0.185	-0.693	-0.727
Panel C: Net long run output and distribution of gains				
% change in net output	0.963	0.963	0.963	0.991
Decomposition:				
(i) Δ Migrant wage income (% net output)	0.931	0.843	0.764	0.754
(ii) Δ Native wage income (% net output)	0.032	0.029	-0.313	-0.330
(iii) Δ Monopsony rents (% net output)	0	0.092	0.513	0.568
Total native surplus (% net output) = (ii) + (iii)	0.032	0.120	0.199	0.238
Δ (Monopsony rents / net output)	0	0	0.359	0.398

This table quantifies the impact of an immigration shock equal to 1% of total employment in 2019, holding migrants' skill mix fixed. Column 1 describes the perfect competition case, with zero mark-downs. Column 2 imposes a fixed mark-down of 0.1 for all workers, natives and migrants alike. And columns 3-4 allow for differential market power over natives and migrants: mark-downs are permitted to respond to migrant share, in line with our estimates in Table 5. Panels A and B predict changes in native and migrant wages, in % terms. Panel C predicts the % change in long run output (net of the costs of elastic inputs) and its distribution. The native surplus is the sum of changes in native wage income and monopsony rents (i.e. assuming that all monopsony rents go to native-owned firms), as a % of net output. The final row reports changes in the ratio of monopsony rents to net output.

mark-downs are equal in this specification, the σ_Z and α_{Zex} technology parameters are again identified by the relative wage equation.

Column 2 shows the wage effects are identical to the competitive case: since the mark-down is fixed, immigration only affects wages via the marginal products (which adjust in the same way as in column 1). Similarly, the output response is identical, since this depends only on the technological interaction between natives and migrants. However, immigration now increases monopsony rents (commensurate with the baseline mark-down level), as firms take a cut from the new migrants' marginal product. If we follow the convention that capital and firms are owned by natives[38] (e.g. Borjas, 1995), the total native surplus then expands to 0.12% of long-run output, most of which goes to employers as monopsony rents. But as the final row shows, the share of monopsony rents in output is unaffected (since rents are fixed at 10%).

7.3 Immigration shock: Differential monopsony power

In Section 5, we empirically rejected the joint hypothesis of $H1$ and $H2$ (equal and independent mark-downs). The direction of the rejection implies differential market power over migrants relative to natives. We now explore the implications for the immigration surplus, based on our estimates above.

In column 3, we allow mark-downs to vary with cell migrant share (i.e. relaxing $H2$), but we continue to assume they are the same for natives and migrants (i.e. $H1$). We calibrate the native mark-down response to 0.6 (based on column 6 of Table 5), while maintaining a 10% share of monopsony rents at baseline. We now see universally negative effects on native wages, averaging -0.4%. The mark-down effect is larger in cells with larger migrant shares at baseline (so dropouts suffer especially). Overall, column 3 suggests the negative mark-down effects on native wages dominate the small positive contribution from marginal products. This has important distributional implications: while workers are worse off, the flip-side is larger growth of monopsony rents (0.5% of net output). The total native surplus (0.2%) is larger than in column 2, because firms are capturing even more rents from migrant labour.

In column 4, we allow native and migrant mark-downs to differ (a violation of $H1$). Specifically, we impose $\alpha_{Zex} = \sigma_Z = 1$ (so natives and migrants are productively identical within cells); and we allow the relative wage equation to identify the differential mark-down effects. The output response is now somewhat larger, since migrants are no longer less productive than natives (within cells). Native wages decline more, since migrants are now more productive ($\alpha_{Zex} = 1$) and are perfect substitutes ($\sigma_Z = 1$); though the

[38] Clearly, some firms are migrant-owned, so one would expect some of the increase in profits to go to migrants. But since we lack data on this, we do not explore it further.

difference is small. Notice that monopsony rents expand more than in column 3 (even as a share of output), because the new migrants are employed at larger mark-downs.

Overall, our results suggest that monopsony power has important implications for the immigration surplus. On the one hand, it significantly expands the total surplus going to natives: native-owned firms take a cut from new migrants' marginal products and capture additional rents from the existing migrant workforce. But the distributional effects are also larger, with employers gaining and native labour losing (due to increasing mark-downs). Indeed, our estimates suggest the entire surplus goes to firms, even in a "long run" scenario with elastic capital; and this can explain why individual firms often lobby for immigration.

7.4 Regularisation counterfactual

The results above suggest the adverse effects of immigration on native labour are mostly a consequence of firms' greater market power over migrants (coupled with an inability to perfectly wage discriminate). Therefore, policies which directly target this market power may help protect native workers from these effects. With this in mind, we now turn to the "regularisation" counterfactual, which transforms a portion of undocumented migrants (equal to 1% of total employment, or 25% of undocumented employment) into legal migrants, within education-experience cells. Undocumented workers in every cell are transformed with equal probability.

We assume here that all workers (natives, legal migrants and undocumented migrants) are perfect substitutes within cells (i.e. $\sigma_Z = 1$).[39] However, we permit productive differences between these workers. Specifically, we write the cell-level input as: $L_{ex} = N_{ex} + \alpha_{Zex}^{legal} M_{ex}^{legal} + \alpha_{Zex}^{undoc} M_{ex}^{undoc}$, where M_{ex}^{legal} is the employment stock of legal migrants, M_{ex}^{undoc} is the stock of undocumented migrants, and α_{Zex}^{legal} and α_{Zex}^{undoc} are the relative efficiencies of each migrant type (compared to natives).

We present our results in Table 10. We begin in column 1 by assuming the labour market is fully competitive: mark-downs are fixed at zero for all workers. Any wage differentials between natives, legal migrants and undocumented migrants (within education-experience cells) are attributed entirely to productive differences (in the α_{Zex}^{legal} and α_{Zex}^{undoct} parameters); so the economic impact of the regularisation policy derives purely from an increase in the productivity of formerly undocumented workers.[40] This generates a 0.08% increase in net output, the bulk of which goes to the migrants themselves. Natives do

[39]Recall from Table 9 that the limited amount of imperfect substitutability suggested by the data makes little difference to the broad conclusions.

[40]We assume here that the wage differential between legal and undocumented migrants represents the *causal* impact of regularisation on productivity. If instead it reflects unobserved heterogeneity, the policy would have no effect on productivity – and therefore no economic effect at all under perfect competition.

Table 10: Simulation of regularisation counterfactual

	Perfect competition	Differential monopsony power	
	(1)	(2)	(3)
Impose equal mark-downs ($H1$)?	Yes	Yes	No
Impose $\alpha_{Zex}^{legal} = \alpha_{Zex}^{undoc} = 1$?	No	No	Yes
Baseline native mark-down	0	0.1	0.1
Mark-down response to undocumented share	0	1.193	1.193
Panel A: Native wages (% changes)			
HS dropouts	-0.099	5.817	5.916
HS graduates	0.006	1.497	1.491
Some college	0.008	0.757	0.749
College graduates	0.001	0.627	0.626
Average	0.002	0.917	0.916
Panel B: Migrant wages (% changes)			
HS dropouts	0.715	7.300	7.409
HS graduates	0.362	1.954	1.949
Some college	0.454	1.218	1.210
College graduates	0.424	1.066	1.065
Average	0.450	1.902	1.911
Panel C: Net long run output and distribution of gains			
% change in net output	0.079	0.079	0
Decomposition:			
(i) Δ Migrant wage income (% net output)	0.077	0.295	0.295
(ii) Δ Native wage income (% net output)	0.002	0.688	0.682
(iii) Δ Monopsony rents (% net output)	0	-0.904	-0.976
Total native surplus (% net output) = (ii) + (iii)	0.002	-0.217	-0.295
Δ (Monopsony rents / output)	0	-0.909	-0.976

This table quantifies the impact of a "regularisation" policy which transforms a portion of the undocumented workforce (1% of total employment in 2019) into legal migrants, within education-experience cells. Column 1 describes the perfect competition case, with zero mark-downs. Column 2 allows for differential monopsony power over undocumented migrants: we impose a baseline native mark-down of 0.1, and permits mark-downs to respond to the undocumented (but not legal migrant) cell employment share, in line with our Table 8 estimates. Column 3 maintains the mark-down response, but attributes within-cell wage differentials entirely to unequal mark-downs (rather than productivity). Panels A and B predict changes in native and migrant wages. Panel C predicts the % change in net output and its distribution. The native surplus is the sum of changes in native wage income and monopsony rents (i.e. we assume that all monopsony rents go to native-owned firms), as a % of net output. The final row reports changes in the ratio of monopsony rents to net output. See Appendix C.2 for computational details.

benefit on average (the effect is very small), but there are distributional effects: the wages of native dropouts contract by 0.1%, because this is where the newly regularised migrants are concentrated (and where the increase in quality-adjusted labour supply is largest).

In column 2, we introduce differential monopsony power over undocumented migrants. In line with our Section 6 estimates, we allow mark-downs to respond to the cell share of undocumented workers (but not to the share of legal migrants). We set this effect to 1.193, based on column 2 of Table 8. We continue to assume equal mark-downs for all workers, so within-cell wage differentials are again attributed to α_{Zex}^{legal} and α_{Zex}^{undoc}. The policy now has two effects: a change in migrant productivity (as in column 1) and also a reduction in monopsony rents (as firms have less market power following regularisation). Comparing columns 1 and 2, it is clear that the latter effect dominates in the wage response. Native wages now increase substantially across all cells, especially among dropouts. Since the mark-downs do not matter for net output, these wage increases are absorbed by a contraction of monopsony rents (equal to 0.9% of net output).

Finally, in column 3, we maintain the mark-down response of column 2; but we now attribute within-cell wage differentials to unequal mark-downs rather than productivity (i.e. we assume $\alpha_{Zex}^{legal} = \alpha_{Zex}^{undoc} = 1$). There is no output effect here, since productivity is unchanged; but the distributional effects are similar to column 2. Migrants still benefit disproportionately, but now through access to lower mark-downs rather than higher productivity. To summarise, these results suggest that a regularisation policy can benefit both native and migrant labour substantially (at firms' expense), due to a reduction in monopsony power.

8 Conclusion

Under perfect competition, migration can only affect native wages by changing marginal products. This assumption is routinely applied in structural models of migration. But it severely restricts the set of possible outcomes, as migration can only affect marginal products by shifting the relative supply of labour *across* skill types (or across factor inputs more generally).

Once we allow for imperfect competition though, the *composition* of skill types (whether native or migrant) also becomes relevant. If firms have greater monopsony power over migrants than natives (a claim supported by a burgeoning empirical literature), but cannot perfectly wage discriminate between them (as recent evidence suggests), this differential power over migrants can "spill over" to the wages of natives. In this way, the labour market becomes less competitive, allowing firms to impose larger mark-downs on both native and migrant wages (for any given change in marginal products).

This insight can help account for empirical violations of the canonical labour market model in US census data, namely that wages depend on migrant share independently of the cell aggregator (even accounting for imperfect substitutability between observably similar natives and migrants). To identify these migrant share effects, we develop new instruments for native and migrant employment, driven by demographic shifts in the US and abroad. Our estimates cannot plausibly be attributed to the specification of technology, the delineation of skill cells, or the allocation of migrants to these cells. Instead, we argue that they reflect an increase in mark-downs. Consistent with this interpretation, we show that the effects are mostly driven by undocumented migrants.

Based on our estimates, the expansion of native mark-downs (in response to migration) dominates the aggregate gains in marginal products. As a result, the average native wage declines, even in a "long run" setting with elastic capital – in violation of the classic "immigration surplus" result. Though aggregate native income grows (due to the transfer of migrants' rents), more than 100% of these gains go to profits, as the increased mark-downs redistribute income from workers to firms.

However, one cannot conclude that immigration is generally harmful for native workers. Adverse mark-down effects may be ameliorated through policies which constrain monopsony power over migrants, such as minimum wages or regularisations. In particular, our estimates suggest that a regularisation policy may substantially benefit both native and migrant labour, at the expense of firms. More generally, variation in labour market institutions may help account for disparate empirical findings on the wage effects of immigration; and the role of these institutions may be a fruitful topic for further investigation.

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Appendices: For Online Publication

A Optimal wage-setting of non-discriminating firm

A non-discriminating firm chooses a wage W_j for skill type j to maximise profits, under the constraint that $W_{Nj} = W_{Mj} = W_j$. Its marginal cost of labour is given by:

$$\begin{aligned} MC(W_j) &= W_j + \frac{N(W) + M(W)}{N'(W) + M'(W)} \\ &= W_j \left\{ 1 + \left[\frac{N(W_j)}{N(W_j) + M(W_j)} \tilde{\epsilon}_N + \frac{M(W_j)}{N(W_j) + M(W_j)} \tilde{\epsilon}_M \right]^{-1} \right\} \end{aligned} \quad \text{(A1)}$$

where $N(W_j)$ and $M(W_j)$ are respectively the supply of native and migrant labour to the firm, as defined by (4) and (5). As illustrated by Figure 1, this marginal cost curve (the dotted line) will lie between the native and migrant MC curves of the discriminating firm. The optimal wage will equate the marginal cost with the marginal product MP_j, so $MC(W_j) = MP_j$. Imposing this condition, we have:

$$\phi_j = \log \left\{ 1 + \left[(1 - m_j) \tilde{\epsilon}_N + m_j \tilde{\epsilon}_M \right]^{-1} \right\} \quad \text{(A2)}$$

where $\phi_j = \log(MP_j / W_j)$ is the mark-down, and $m_j \equiv M_j / (N_j + M_j)$ is the market j migrant share. Equation (A2) implies that if $\tilde{\epsilon}_M < \tilde{\epsilon}_N$ (so firms have greater market power over migrants), the mark-down ϕ_j will be increasing in migrant share m_j.

B Cell-level aggregation of native and migrant wages

Equation (13) in Section 3.1 shows the production nest $Z(N_{ext}, M_{ext})$ at the level of education-experience cells. The conventional interpretation of (13) is that native and migrant labour (i.e. N_{ext} and M_{ext}) are distinct skill types which are imperfect substitutes. But this makes it difficult to understand why employers cannot wage discriminate between them (a key premise of our story). In this appendix, we offer an alternative interpretation of education-experience cells: we treat L_{ext} as an aggregation of many *unobserved* skill inputs L_j, within which natives and migrants are perfect substitutes (i.e. $L_j = N_j + M_j$). The set-up is illustrated in the left panel of Figure B1. In what follows, we show how native and migrant wages can be aggregated (across skill types j) to the level of education-experience (subscript ex) cells, as illustrated by the right panel. We begin by describing the aggregation of marginal products, and then the aggregation of mark-downs.

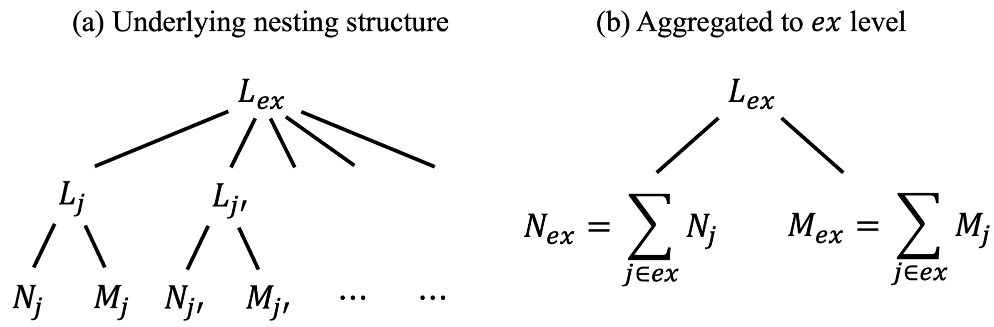


Figure B1: Aggregation of education-experience cells

B.1 Aggregating marginal products across skill types j

Suppose the education-experience composite L_{ex} in equation (13) is a HoD1 function of potentially many (unobserved) skill inputs L_j:

$$L_{ex} = \tilde{Z}\left(L_1, .., L_J\right) \tag{B1}$$

Within each skill type j, natives and migrants are perfect substitutes: i.e. $L_j = N_j + M_j$.

Suppose there are N_{ex} natives (within a given education-experience cell), of whom a fraction $\eta_j \equiv N_j/N_{ex}$ have skill j; and there are M_{ex} migrants, of whom a fraction $\mu_j \equiv M_j/M_{ex}$ have skill j. We can then rewrite (B1) as:

$$L_{ex} = \tilde{Z}\left(\eta_1 N_{ex} + \mu_1 M_{ex}, .., \eta_J N_{ex} + \mu_J M_{ex}\right) = Z\left(N_{ex}, M_{ex}\right)$$

where Z is a HoD1 function of the *total* cell-level native and migrant stocks (i.e. $N_{ex} = \sum_{j \in ex} N_j$ and $M_{ex} = \sum_{j \in ex} M_j$), which subsumes the native and migrant skill allocations (η_j and μ_j).

The partial derivative of Z with respect to N_{ex} is:

$$\frac{\partial Z\left(N_{ex}, M_{ex}\right)}{\partial N_{ex}} = \sum_j \eta_j \frac{\partial \tilde{Z}\left(L_1, .., L_J\right)}{\partial L_j} \tag{B2}$$

which is equal to the *average* marginal product of natives. Similarly, the partial derivative with respect to M_{ex} is:

$$\frac{\partial Z\left(N_{ex}, M_{ex}\right)}{\partial M_{ex}} = \sum_j \mu_j \frac{\partial \tilde{Z}\left(L_1, .., L_J\right)}{\partial L_j} \tag{B3}$$

which is equal to the *average* marginal product of migrants. In this way, we have reduced L_{ex} to an aggregated production function over two composite inputs (N_{ex} and M_{ex}), whose marginal products are equal to those of the average native and migrant (in the education-experience cell). Our approach here implements an aggregation trick from Amior and Manning (2024), which builds on a long-standing literature on the aggregation of production functions (Houthakker, 1955; Levhari, 1968; Jones, 2005; Growiec, 2008). This literature offers a range of methods to achieve this where the two inputs are capital and labour, rather than natives and migrants. Levhari (1968) in particular shows how one can construct an underlying $\tilde{Z}$ from a desired Z, using as an example the case where Z is CES.

If natives and migrants are allocated differently across the unobserved labour markets j (i.e. $\eta_j \neq \mu_j$), they will function as imperfect substitutes at the level of education-experience cells: i.e. $\sigma < 1$ in equation (13).[41] Conversely, as the skill allocations of natives and migrants become more similar, they behave as closer substitutes at the cell-level: i.e. σ goes to 1.

B.2 Aggregating mark-downs across skill types j

In this section, we derive expressions for the aggregated cell-level mark-downs, $\phi_{Nex}(m_{ex})$ and $\phi_{Mex}(m_{ex})$, which are functions of the cell-level migrant share $m_{ex} \equiv M_{ex}/(N_{ex} + M_{ex})$. For the purposes of this exercise, we assume that firms cannot wage discriminate between natives and migrants of the same skill type j. As a result, as equation (A2) shows, natives and migrants will face an identical mark-down $\phi_j = \phi(m_j)$ within each skill type j. But if natives and migrants are distributed differently across skill types j (as in Section B.1), their *average* mark-downs ϕ_{Nex} and ϕ_{Mex} (at the *ex* cell level) may still differ.

Note first that the market j wage (for both natives and migrants) can be written as:

$$W_j = MP_j \exp(-\phi(m_j)) \tag{B4}$$

where MP_j is the marginal product. The aggregate native wage W_{Nex} (at the education-experience cell level) is the average of these W_js, weighted by the native market shares $\eta_j \equiv N_j/N_{ex}$. Its log can be written as:

$$\log W_{Nex} = \log \sum_{j \in ex} \eta_j MP_j - \phi_{Nex}(m_{ex}) \tag{B5}$$

where ϕ_{Nex} is the *aggregate* cell-level native mark-down, a function of the aggregate

[41] This is in the spirit of Peri and Sparber (2009), who argue that comparative advantage of natives over migrants in communication tasks (within observable skill cells) leads to imperfect substitutability.

migrant share m_{ex}:

$$\phi_{Nex}(m_{ex}) = \log \frac{\sum_{j\in ex} \eta_j MP_j}{\sum_{j\in ex} \eta_j MP_j \exp\left(-\phi\left(\frac{\mu_j m_{ex}}{\eta_j(1-m_{ex})+\mu_j m_{ex}}\right)\right)} \tag{B6}$$

which uses the fact that:

$$m_j = \frac{\mu_j m_{ex}}{\eta_j(1-m_{ex})+\mu_j m_{ex}} \tag{B7}$$

for markets $j \in ex$. By symmetry, we can write a similar expression for the aggregate migrant mark-down:

$$\phi_{Mex}(m_{ex}) = \log \frac{\sum_{j\in ex} \mu_j MP_j}{\sum_{j\in ex} \mu_j MP_j \exp\left(-\phi\left(\frac{\mu_j m_{ex}}{\eta_j(1-m_{ex})+\mu_j m_{ex}}\right)\right)} \tag{B8}$$

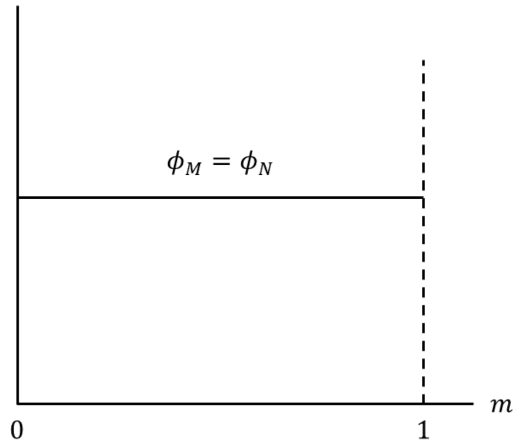
It is worth briefly commenting on the role of marginal products in these mark-down equations. In the main text, we permit the cell-level mark-downs (i.e. ϕ_{Nex} and ϕ_{Mex}) to depend on the cell-level migrant share m_{ex}, but not on the cell-level marginal products. Now, equations (B6) and (B8) show that the cell-level mark-downs *do* depend on the marginal products of the underlying skill types j (within the cell); but these only enter the mark-down formulae in the role of *weights*. Therefore, a change in the *aggregate cell-level* marginal product will only affect the cell-level mark-down if this change is accompanied by shifts in the *relative* marginal products of skill types j within the cell. In applying H1 (the hypothesis of equal native and migrant mark-downs) to the level of aggregated education-experience cells in the empirical model (in Section 3), we implicitly rule out such changes in the these relative (within-cell) marginal products.

B.3 Skill segregation and the properties of aggregate cell-level mark-downs

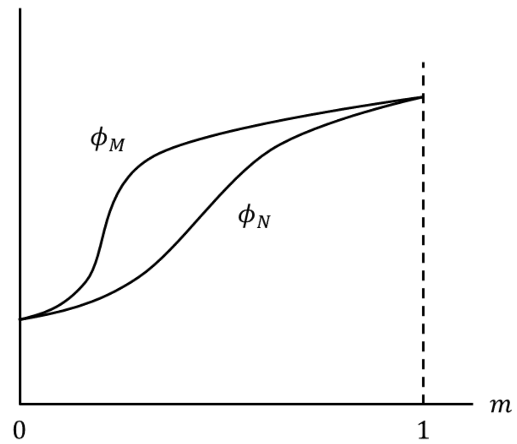
We now explore the properties of the aggregate cell-level mark-down functions, $\phi_{Nex}(m_{ex})$ and $\phi_{Mex}(m_{ex})$. To ease notation, we will suppress the *ex* (education-experience cell) subscripts in the discussion which follows. The extent of skill segregation (within cells) will be crucial to the shape of these functions. We begin by considering the perfect segregation case, and then turn to imperfect skill segregation.

(i) Perfect skill segregation

First, consider the special case where natives and migrants are perfectly segregated: i.e. each skill type j consists of only natives or migrants, so $\mu_j \eta_j = 0$ for all j. Based on (A2), this implies that $\phi_j = \log(1 + 1/\tilde{\epsilon}_N)$ in all native skill types (where $\eta_j > 0$); so



(a) $\tilde{\epsilon}_M = \tilde{\epsilon}_N$



(b) $\tilde{\epsilon}_M < \tilde{\epsilon}_N$

Figure B2: Aggregate mark-down functions

the aggregate native mark-down $\phi_N(m)$ depends *only* on the adjusted supply elasticity $\tilde{\epsilon}_N$. Similarly, perfect segregation implies that $\phi_j = \log(1 + 1/\tilde{\epsilon}_M)$ in all migrant skill types (where $\mu_j > 0$), so the migrant mark-down $\phi_M(m)$ depends only on the migrant replacement ratio, r_M, and elasticity $\tilde{\epsilon}_M$. Thus, the mark-downs are identical to the perfect discrimination case, described by equation (9).

(ii) Imperfect skill segregation

Now suppose that natives and migrants overlap across skill types j. The aggregate mark-downs may now depend on migrant share, but only if firms have differential market power over natives and migrants. If they have equal monopsony power (i.e. $\tilde{\epsilon}_M = \tilde{\epsilon}_N$), equation (A2) shows that the skill j mark-downs ϕ_j will be independent of migrant share m and invariant with skill j; so the aggregate mark-downs are again equal ($\phi_N = \phi_M$) and independent of migrant share. In Figure B2a, we illustrate the $\phi_N(m)$ and $\phi_M(m)$ functions for this equal monopsony power case.

In Figure B2b, we consider the case where firms have greater market power over migrants, i.e. $\tilde{\epsilon}_M < \tilde{\epsilon}_N$. Skill-specific (and hence aggregate) mark-downs are now increasing in migrant share m; and since migrants are necessarily concentrated in skill types j with larger migrant shares, they must face larger aggregate mark-downs for any given m: i.e. $\phi_M(m) \geq \phi_N(m)$. However, as (A2) shows, ϕ_M and ϕ_N must converge to equality as $m \to 0$ or $m \to 1$. Intuitively, as the labour force becomes exclusively native or migrant, the marginal cost curve facing firms converges to the pure native or migrant one (identical to those of the discriminating case), in which case all workers will face the same mark-down. Given the symmetry of the model, these results will be reversed if monopsony power is greater over natives (i.e. $\tilde{\epsilon}_M > \tilde{\epsilon}_N$).

Differential between mark-down functions

To conclude, we now derive a more formal expression for the differential between the aggregate migrant and native mark-downs, ϕ_M and ϕ_N. Define $\tilde{\eta}_j \equiv \eta_j MP_j / \sum_j \eta_j MP_j$ and $\tilde{\mu}_j \equiv \mu_j MP_j / \sum_j \mu_j MP_j$. From (B6) and (B8), we then have:

$$\begin{aligned}
\exp(-\phi_M) - \exp(-\phi_N) &= \sum_j \tilde{\mu}_j \exp(-\phi_j) - \sum_j \tilde{\eta}_j \exp(-\phi_j) \quad \text{(B9)} \\
&= \sum_j \tilde{\eta}_j \left(\frac{\tilde{\mu}_j}{\tilde{\eta}_j}\right) \exp(-\phi_j) - \sum_j \tilde{\eta}_j \exp(-\phi_j) \\
&= E_\eta \left[\frac{\tilde{\mu}_j}{\tilde{\eta}_j} \exp(-\phi_j)\right] - E_\eta \left[\frac{\tilde{\mu}_j}{\tilde{\eta}_j}\right] E_\eta [\exp(-\phi_j)] \\
&= Cov_\eta \left[\frac{\tilde{\mu}_j}{\tilde{\eta}_j}, \exp(-\phi_j)\right]
\end{aligned}$$

where the expectation E_η is weighted by the native shares $\tilde{\eta}_j$, and we are using the fact that $E_\eta [\tilde{\mu}_j / \tilde{\eta}_j] = 1$. If $\tilde{\epsilon}_M < \tilde{\epsilon}_N$ (i.e. greater monopsony power over migrants), the skill j mark-down $\phi_j = \phi(\mu_j M / \eta_j N)$ is an increasing function of the ratio $\tilde{\mu}_j / \tilde{\eta}_j$. As a result, the covariance in the final line of (B9) will be negative, and the aggregate mark-down will be larger for migrants. Intuitively, migrants will be disproportionately located in migrant-intensive skill types (which are less competitive and have larger mark-downs).

C Computation of counterfactual effects

Here, we describe how we compute the impact of the immigration shock and regularisation counterfactuals in Section 7. We begin with the immigration shock in Section C.1 (ignoring the distinction between legal and undocumented migrants). And in Section C.2, we describe what is different in the regularisation counterfactual.

C.1 Immigration shock

We begin by setting out the wage equations. Using (14) and (16), and expanding the productivity shifter A_{ex} with (15), cell-level native and migrant wages can be written as:

$$\begin{aligned}
\log W_{Nex} &= \log(\alpha_e \alpha_{ex}) + (1 - \sigma_E) \log \tilde{Y} + (\sigma_E - \sigma_X) \log L_e \quad \text{(C1)} \\
&\quad + (\sigma_X - \sigma_Z) \log L_{ex} + (\sigma_Z - 1) \log N_{ex} - \phi_{Nex} \\
\log W_{Mex} &= \log(\alpha_e \alpha_{ex} \alpha_{Zex}) + (1 - \sigma_E) \log \tilde{Y} + (\sigma_E - \sigma_X) \log L_e \quad \text{(C2)} \\
&\quad + (\sigma_X - \sigma_Z) \log L_{ex} + (\sigma_Z - 1) \log M_{ex} - \phi_{Mex}
\end{aligned}$$

where $\tilde{Y}$ is long run output, net of the costs of the elastic capital inputs. Consider an immigration shock equal to 1% of total employment, holding the skill mix of migrants fixed. To assess the overall impact of this shock, we must consider the effect of migrant inflows in any given cell (e, x) on the wages of every other cell (e', x'): this yields a 32×32 matrix. For $e' \neq e$ pairs, the entire effect comes through the net output term, $\log \tilde{Y}$, in (C1) and (C2). For pairs with $e' = e$ and $x' \neq x$, we must also consider the impact on the education aggregator, $\log L_e$. For same-cell pairs (i.e. $e' = e$ and $x' = x$), we must also consider the impact on the education-experience aggregator, $\log L_{ex}$; and for migrant wages in the same cell, we must also consider the effect via the $\log M_{ex}$ term in (C2). Finally, workers in the same cell ($e' = e$ and $x' = x$) will be subject to mark-down effects via ϕ_{Nex} and ϕ_{Mex}.

Effect on wage equation components

How does the immigration shock affect the various components of (C1) and (C2)? Let $N \equiv \sum_{e,x} N_{ex}$ denote the aggregate native stock, and $M \equiv \sum_{e,x} M_{ex}$ the aggregate migrant stock. Notice first that, holding the native stock and migrant skill mixed fixed, a 1% increase in the aggregate migrant stock M (relative to total employment, $M + N$), i.e. $dM/(M+N)$, will cause the log migrant stock M_{ex} in each cell (e, x) to expand by:

$$d \log M_{ex} = 0.01 \cdot \frac{N+M}{M} \tag{C3}$$

For a given change in M_{ex}, the education-experience aggregator L_{ex} will increase by:

$$\frac{d \log L_{ex}}{d \log M_{ex}} = \frac{\alpha_{Zex} M_{ex}^{\sigma_Z}}{N_{ex}^{\sigma_Z} + \alpha_{Zex} M_{ex}^{\sigma_Z}} = \frac{\tilde{F}_{Mex} M_{ex}}{\tilde{F}_{Mex} M_{ex} + \tilde{F}_{Nex} N_{ex}} \tag{C4}$$

where the second equality follows from (13), and where:

$$\tilde{F}_{Nex} = \exp(\phi_{Nex}) W_{Nex} \tag{C5}$$

$$\tilde{F}_{Mex} = \exp(\phi_{Mex}) W_{Mex} \tag{C6}$$

are the (long run) cell-specific marginal products of native and migrant labour respectively. Notice that, under perfect competition (i.e. $\phi_{Nex} = \phi_{Mex} = 0$), $\frac{\tilde{F}_{Mex} M_{ex}}{\tilde{F}_{Mex} M_{ex} + \tilde{F}_{Nex} N_{ex}}$ will equal the migrant wage bill share (within the labour market cell).

For a given change in L_{ex} in experience group x, the education aggregator L_e increases by:

$$\frac{d \log L_e}{d \log L_{ex}} = \frac{\alpha_{ex} L_{ex}}{\sum_{x'} \alpha_{ex'} L_{ex'}} = \frac{\tilde{F}_{Mex} M_{ex} + \tilde{F}_{Nex} N_{ex}}{\sum_{x'} \left(\tilde{F}_{Mex'} M_{ex'} + \tilde{F}_{Nex'} N_{ex'} \right)} \tag{C7}$$

where the second equality follows from (12), and where $\frac{\tilde{F}_{Mex}M_{ex}+\tilde{F}_{Nex}N_{ex}}{\sum_{x'}\left(\tilde{F}_{Mex'}M_{ex'}+\tilde{F}_{Nex'}N_{ex'}\right)}$ will equal the wage bill share of experience group x (within education group e) under perfect competition.

And finally, for a given change in L_e in some education group e, net output $\tilde{Y}$ increases by:

$$\frac{d\log\tilde{Y}}{d\log L_e}=\frac{\alpha_e L_e}{\sum_{e'}\alpha_{e'}L_{e'}}=\frac{\sum_{x'}\left(\tilde{F}_{Mex'}M_{ex'}+\tilde{F}_{Nex'}N_{ex'}\right)}{\sum_{e',x'}\left(\tilde{F}_{Me'x'}M_{e'x'}+\tilde{F}_{Ne'x'}N_{e'x'}\right)} \tag{C8}$$

where the second equality follows from (11), and where $\frac{\sum_{x'}\left(\tilde{F}_{Mex'}M_{ex'}+\tilde{F}_{Nex'}N_{ex'}\right)}{\sum_{e',x'}\left(\tilde{F}_{Me'x'}M_{e'x'}+\tilde{F}_{Ne'x'}N_{e'x'}\right)}$ will equal the wage bill share of education group e under perfect competition.

Mark-down effects

Next, consider the mark-down effects, which fall on workers in the same cell (i.e. $e' = e$ and $x' = x$). The change in the native mark-down ϕ_{Nex} is given by $\gamma_2 \cdot d\left(M_{ex}/\left(M_{ex}+N_{ex}\right)\right)$, where γ_2 is the estimated mark-down effect from equation (20), and $d\left(M_{ex}/\left(M_{ex}+N_{ex}\right)\right)$ is the change in migrant share. As a response to $\log M_{ex}$ (holding native employment fixed), this is equal to:

$$\frac{d\phi_{Nex}}{d\log M_{ex}}=\frac{\gamma_2\cdot d\left(\frac{M_{ex}}{M_{ex}+N_{ex}}\right)}{d\log M_{ex}}=\gamma_2\frac{N_{ex}M_{ex}}{\left(N_{ex}+M_{ex}\right)^2} \tag{C9}$$

The change in the migrant mark-down ϕ_{Mex} depends on our identifying assumption. Under equal mark-downs ($H1$), we simply have:

$$\frac{d\phi_{Mex}}{d\log M_{ex}}=\frac{d\phi_{Nex}}{d\log M_{ex}} \tag{C10}$$

But under equal productivity ($\alpha_{Zex}=\sigma_Z=1$):

$$\frac{d\phi_{Mex}}{d\log M_{ex}}=\frac{d\phi_{Nex}}{d\log M_{ex}}+\beta_1 \tag{C11}$$

where β_1 is the coefficient on $\log\left(M_{ext}/N_{ext}\right)$ in the relative wage equation (18). Intuitively, if we assume $\alpha_{Zex}=\sigma_Z=1$, we must attribute any change in relative wages (as picked up by β_1) to differential (migrant/native) mark-down effects.

Aggregation of wage change components

The equations above describe the effect of immigration in any given cell (e, x) on the various aggregators (L_{ex}, L_e and $\tilde{Y}$), as well as the mark-downs (ϕ_{Nex} and ϕ_{Mex}). To compute the overall response of wages in some education-experience cell, accounting for immigration $d\log M_{ex}$ across the full distribution of cells (e,x), we simply aggregate over all these effects. Using (C1) and (C2), we have:

$$\begin{aligned} d\log W_{Nex} &= (1-\sigma_E)\sum_{e',x'} \frac{d\log \tilde{Y}}{d\log L_{e'}} \cdot \frac{d\log L_{e'}}{d\log L_{e'x'}} \cdot \frac{d\log L_{e'x'}}{d\log M_{e'x'}} \cdot d\log M_{e'x'} \\ &\quad + (\sigma_E-\sigma_X)\sum_{e,x'} \frac{d\log L_e}{d\log L_{ex'}} \cdot \frac{d\log L_{ex'}}{d\log M_{ex'}} \cdot d\log M_{ex'} \\ &\quad + (\sigma_X-\sigma_Z)\frac{d\log L_{ex}}{d\log M_{ex}} \cdot d\log M_{ex} - d\phi_{Nex} \end{aligned} \tag{C12}$$

and

$$\begin{aligned} d\log W_{Mex} &= (1-\sigma_E)\sum_{e',x'} \frac{d\log \tilde{Y}}{d\log L_{e'}} \cdot \frac{d\log L_{e'}}{d\log L_{e'x'}} \cdot \frac{d\log L_{e'x'}}{d\log M_{e'x'}} \cdot d\log M_{e'x'} \\ &\quad + (\sigma_E-\sigma_X)\sum_{e,x'} \frac{d\log L_e}{d\log L_{ex'}} \cdot \frac{d\log L_{ex'}}{d\log M_{ex'}} \cdot d\log M_{ex'} \\ &\quad + (\sigma_X-\sigma_Z)\frac{d\log L_{ex}}{d\log M_{ex}} \cdot d\log M_{ex} - (1-\sigma_Z)\, d\log M_{ex} - d\phi_{Mex} \end{aligned} \tag{C13}$$

Distributional effects and surplus

We now turn to Panel C of Table 9. The first row of Panel C reports the impact on total migrant wage income, relative to net output. To derive this, we first compute the change in migrant wage income in each labour market cell (e,x):

$$d\left(W_{Mex}M_{ex}\right) = W_{Mex}M_{ex}\left(d\log W_{Mex} + d\log M_{ex}\right) \tag{C14}$$

Similarly, the change in native wage income in cell (e,x) can be written as:

$$d\left(W_{Nex}N_{ex}\right) = W_{Mex}N_{ex} \cdot d\log W_{Nex} \tag{C15}$$

To compute the total change in migrant and native wage bills, we sum (C14) and (C15) over cells (e,x). And we express these changes relative to net output $\tilde{Y}$, where $\tilde{Y}$ can be written as:

$$\tilde{Y} = \sum_{e,x}\left(\tilde{F}_{Mex}M_{ex} + \tilde{F}_{Nex}N_{ex}\right) \tag{C16}$$

given our assumption that production has constant returns. The change in monopsony rents R (relative to $\tilde{Y}$) can be expressed as a residual, after subtracting changes in total wage income from total income growth:

$$\frac{dR}{\tilde{Y}} = d\log\tilde{Y} - \sum_{e,x}\frac{d\left(W_{Nex}N_{ex}\right)}{\tilde{Y}} - \sum_{e,x}\frac{d\left(W_{Mex}M_{ex}\right)}{\tilde{Y}} \tag{C17}$$

Finally, if we assume that all monopsony rents go to natives, we can write the "immigration surplus" S (relative to net output) as:

$$\frac{S}{\tilde{Y}} = \frac{dR}{\tilde{Y}} + \sum_{e,x}\frac{d\left(W_{Nex}N_{ex}\right)}{\tilde{Y}} \tag{C18}$$

C.2 Regularisation counterfactual

The procedure for the regularisation counterfactual is similar to the immigration shock, but not identical. We begin by disaggregating the cell-specific migrant employment stock, M_{ex}, into legal and undocumented components:

$$M_{ex} \equiv M_{ex}^{legal} + M_{ex}^{undoc} \tag{C19}$$

Let π_{ex} be the fraction of migrant employment in cell (e, x) which is undocumented:

$$\pi_{ex} \equiv \frac{M_{ex}^{undoc}}{M_{ex}} \tag{C20}$$

In the regularisation counterfactual, we consider the impact of transforming a small (and proportionally equal) number of undocumented migrants in every education-experience cell to legal migrants. That is, we consider a small (and equal) decrease in $\log\pi_{ex}$ in every cell. For a policy which regularises 1% of total employment, $\log\pi_{ex}$ decreases in every (e, x) cell by:

$$d\log\pi_{ex} = 0.01\cdot\frac{N+M}{M^{undoc}} \tag{C21}$$

We assume here that all workers (natives, legal migrants and undocumented migrants) within education-experience cells are perfect substitutes: i.e. $\sigma_Z = 1$. However, we permit productive differences between these workers. In place of (13), we therefore write the cell-level input L_{ex} as:

$$L_{ex} = N_{ex} + \alpha_{Zex}^{legal}M_{ex}^{legal} + \alpha_{Zex}^{undoc}M_{ex}^{undoc} \tag{C22}$$

where α_{Zex}^{legal} and α_{Zex}^{undoc} are the relative efficiencies of each migrant type. The production technology at higher nests is identical to before: i.e. (11) and (12) are unchanged.

In place of (C2), we now have distinct wage equations for legal and undocumented migrants:

$$\log W_{Mex}^{legal} = \log\left(\alpha_e\alpha_{ex}\alpha_{Zex}^{legal}\right) + (1-\sigma_E)\log\tilde{Y} + (\sigma_E-\sigma_X)\log L_e - (1-\sigma_Z)\log L_{ex} - \phi_{Mex}^{legal} \quad \text{(C23)}$$

$$\log W_{Mex}^{undoc} = \log\left(\alpha_e\alpha_{ex}\alpha_{Zex}^{undoc}\right) + (1-\sigma_E)\log\tilde{Y} + (\sigma_E-\sigma_X)\log L_e - (1-\sigma_Z)\log L_{ex} - \phi_{Mex}^{undoc} \quad \text{(C24)}$$

where ϕ_{Mex}^{legal} and ϕ_{Mex}^{undoc} are their respective cell-specific mark-downs.

Effect on wage equation components

Using (C22), we begin by consider the impact of a change in $d\log\pi_{ex}$ on $\log L_{ex}$ in a given cell, in place of equation (C4) above:

$$\frac{d\log L_{ex}}{d\log\pi_{ex}} = \frac{\left(\tilde{F}_{Mex}^{undoc} - \tilde{F}_{Mex}^{legal}\right)M_{ex}^{undoc}}{\tilde{F}_{Mex}^{legal}M_{ex}^{legal} + \tilde{F}_{Mex}^{undoc}M_{ex}^{undoc} + \tilde{F}_{Nex}N_{ex}} \quad \text{(C25)}$$

where

$$\tilde{F}_{Nex} = \exp\left(\phi_{Nex}\right)W_{Nex} \quad \text{(C26)}$$

$$\tilde{F}_{Mex}^{legal} = \exp\left(\phi_{Mex}^{legal}\right)W_{Mex}^{legal} \quad \text{(C27)}$$

$$\tilde{F}_{Mex}^{noncit} = \exp\left(\phi_{Mex}^{noncit}\right)W_{Mex}^{undoc} \quad \text{(C28)}$$

are the cell marginal products. Similarly to (C7) and (C8) above, we can then derive changes in the education-level aggregator $d\log L_e$ and net output $d\log\tilde{Y}$, but simply replacing all occurrences of $\left\{\tilde{F}_{Mex}M_{ex}\right\}$ with $\left\{\tilde{F}_{Mex}^{legal}M_{ex}^{legal} + \tilde{F}_{Mex}^{undoc}M_{ex}^{undoc}\right\}$.

Mark-down effects

We now turn to the mark-down responses. Motivated by our empirical estimates, we suppose the mark-downs respond only to the *undocumented* share of cell-specific employment (and not to the share of legal migrants). For simplicity, we also assume this response is identical (within cells) for natives, legal migrants and undocumented migrants alike. The change in the mark-downs is given by $\gamma_2^{undoc}\cdot d\left(M_{ex}^{undoc}/\left(M_{ex}+N_{ex}\right)\right)$, where γ_2^{undoc} is the estimated effect of the undocumented share in Table 8. As a response to $\log M_{ex}$ (holding native and total migrant employment fixed), this can be expressed as:

$$\frac{d\phi_{Nex}}{d\log\pi_{ex}} = \frac{d\phi_{Mex}^{legal}}{d\log\pi_{ex}} = \frac{d\phi_{Mex}^{undoc}}{d\log\pi_{ex}} = \frac{\gamma_2^{undoc}\cdot d\left(\frac{M_{ex}^{undoc}}{M_{ex}+N_{ex}}\right)}{d\log\pi_{ex}} = \gamma_2^{undoc}\frac{M_{ex}^{undoc}}{M_{ex}+N_{ex}} \quad \text{(C29)}$$

Aggregation of wage change components

Using the various equations above, we can then aggregate over the components of the wage equation, to derive wage changes in every education-experience cell, just as we do in equations (C12) and (C13) for the immigration shock. Since we assume the mark-down response is identical for all workers within cells, and that all workers are perfect substitutes within cells, the wage response is identical across natives and migrants. Specifically:

$$\begin{aligned} d\log W_{ex} &= (1-\sigma_E)\sum_{e',x'}\frac{d\log\tilde{Y}}{d\log L_{e'}}\cdot\frac{d\log L_{e'}}{d\log L_{e'x'}}\cdot\frac{d\log L_{e'x'}}{d\log \pi_{e'x'}}\cdot d\log\pi_{e'x'} \\ &\quad +(\sigma_E-\sigma_X)\sum_{e,x'}\frac{d\log L_e}{d\log L_{ex'}}\cdot\frac{d\log L_{ex'}}{d\log\pi_{ex'}}\cdot d\log\pi_{ex'} \\ &\quad +(\sigma_X-\sigma_Z)\frac{d\log L_{ex}}{d\log M_{ex}}\cdot d\log\pi_{ex}-d\phi_{Nex} \end{aligned} \tag{C30}$$

Distributional effects and surplus

Using (C30), we can then compute the distributional effects in the same way as before. The only difference is the cell-specific migrant wage bill change in equation (C14), which we now replace with:

$$\begin{aligned} d\left(W_{Mex}M_{ex}\right) &= d\left(W_{Mex}^{legal}M_{ex}^{legal}+W_{Mex}^{undoc}M_{ex}^{undoc}\right) \\ &= \left(W_{Mex}^{undoc}-W_{Mex}^{legal}\right)M_{ex}^{undoc}d\log\pi_{ex} \\ &\quad +\left(W_{Mex}^{legal}M_{ex}^{legal}+W_{Mex}^{undoc}M_{ex}^{undoc}\right)d\log W_{ex} \end{aligned} \tag{C31}$$

D Further details on data

D.1 Adjusting cell-level wages for changes in composition

In most specifications, we adjust native and migrant wages for observable changes in demographic composition over time. In this section, we explain how we implement this adjustment.

For natives, we proceed as follows. We begin by pooling census and ACS microdata from all observation years. Separately for each of our 32 education-experience cells, and separately for native men and women, we regress log wages on a quadratic in age, a postgraduate education indicator (for college graduate cells only), race effects (Hispanic, Asian, black), and a full set of year effects.[42] We then predict the mean male and female

[42] We have experimented with residualizing also by state, industry and occupation, but this makes negligible difference to our results.

wage for each year, for a distribution of worker characteristics identical to the multi-year pooled sample (within education-experience cells). And we then compute a composition-adjusted native wage in each cell-year by taking weighted averages of the predicted male and female wages (using the gender ratios in the pooled sample as weights).

We follow identical steps for migrants, but replacing the race indicators with dummies covering 12 global regions of origin[43], and also including an indicator for recent arrivals (up to five years in the US[44]).

D.2 Instrument for new immigrant stocks

In this section, we describe in greater detail how we construct the instrument for new immigrant stocks, $\tilde{M}_{ext}^{new}$. As we explain in Section 4.2 in the main text, this is a weighted aggregate of historical cohort sizes in origin countries. We construct this weighted average using the coefficient estimates of the following linear regression:

$$\log M_{oext}^{new} = \lambda_0^{Mnew} + \lambda_1^{Mnew} \log HistoricalCohortSize_{oext} + \lambda_2^{Mnew} Mobility_{ex} + Region_o + \varepsilon_{oext}^{Mnew} \tag{D1}$$

where M_{oext}^{new} the US population of new migrants (up to ten years in the US) at each observation year t, for each of 164 origin countries o and 32 education-experience cells (e, x). We take this information from our ACS and census samples. $HistoricalCohortSize_{oext}$ is the historical size of the relevant education cohort at origin o, ten years before t, which we take from Barro and Lee (2013) and the World Population Prospects database (United Nations, 2024).[45] Of course, we cannot observe the historical sizes of education cohorts aged 18-33 in year t, since many of them will not have reached their final education status ten years previously: we assign these individuals to education groups in the same way as we do for US natives (as described in Section 4.2), based on the education choices of the previous cohort (in the relevant origin country). Conditional on cohort size, one might expect more emigration to the US from more mobile demographic groups – especially the young. To account for this, we control in (D1) for a time-invariant index of cell-specific residential mobility, $Mobility_{ex}$, which we describe in the following section

[43]Specifically: North America, Mexico, Other Central America, South America, Western Europe, Eastern Europe and former USSR, Middle East and North Africa, Sub-Saharan Africa, South Asia, Southeast Asia, East Asia, Oceania.

[44]This category is available in all census years.

[45]The Barro-Lee data offer population counts by country, education and 5-year age category for individuals aged 15 or over. We identify Barro and Lee's "complete tertiary" education category with college graduates, "incomplete tertiary" with some college, "secondary complete" with high school graduates, and all remaining categories with high school dropouts. We impute single-year age counts by dividing the 5-year stocks equally across their single-year components. To predict historical cohort sizes of the youngest groups, we also require counts of under-15s; and we take this information from the World Population Prospects database.

(Appendix D.3). And finally, we control for a set of 12 region of origin effects[46], $Region_o$, which account for the fact that demographic shifts in certain global regions matter more for emigration to the US. As it turns out, origin cohort size delivers substantial predictive power: we estimate a λ_1^{Mnew} of 0.49, with a standard error of 0.03 (clustered by education-experience cells). The coefficient λ_2^{Mnew} on the mobility index is 1.21, with a 0.06 standard error.

Using our estimates of (D1), we then predict $\log M_{oext}^{new}$ for every origin o, education-experience cell (e, x) and observation year t. And to generate our instrument $\tilde{M}_{ext}^{new}$ for the cell-level (e, x) stock of new migrants, we sum the predicted M_{oext}^{new} over origins o:

$$\tilde{M}_{ext}^{new} = \sum_{o} \exp\left(\hat{\lambda}_0^{Mnew} + \hat{\lambda}_2^{Mnew} Mobility_{ex} + Region_o\right) \cdot HistoricalCohortSize_{oext}^{\hat{\lambda}_1^{Mnew}} \tag{D2}$$

Effectively, this is a weighted aggregate of historical cohort sizes in origin countries (ten years before t), where the weights depend on time-invariant origin-specific migration propensities (as picked up by the $Region_o$ effects) and cell mobility (as picked up by the $Mobility_{ex}$ index). Notice we do not rely on e, x or t fixed effects in our predictive regression (D1), as these may pick up migratory responses to unobserved cell-level demand shocks; and the entire purpose of this instrument is to exclude such variation.

D.3 Mobility index for predicting new immigrant stocks

In this section, we describe our education-experience index of residential mobility $Mobility_{ex}$, which we use to predict new migrant stocks in equation (D1). One might choose to measure mobility using cell-level (e, x) shares of new immigrants in the US population. But of course, this may pick up demand effects at the education-experience cell level, which we are attempting to exclude (as US cells with stronger demand may attract more immigrants). Instead, we proxy mobility with cross-state migration *within* the US rather than *international* migration.

More specifically, we use the log rate of cross-state migration, based on the 1960 census. We use the log rate to match the log migrant inflow on the left-hand side of (D1). The census reports place of residence *five* years previously. But our dependent variable is the stock of new immigrants who arrived in the last *ten* years. These differences may matter, given we are studying mobility within fine 5-year experience cells. We address this inconsistency in two steps. First, we compute internal mobility shares (i.e. the probability of living in a different state five years previously) by education and 5-year experience cell

[46]Specifically: North America, Mexico, Other Central America, South America, Western Europe, Eastern Europe and former USSR, Middle East and North Africa, Sub-Saharan Africa, South Asia, Southeast Asia, East Asia, Oceania.

Table D1: Residential mobility index

	Experience groups							
	1-5	6-10	11-15	16-20	21-25	26-30	31-35	36-40
HS dropouts	-2.422	-2.064	-2.092	-2.399	-2.686	-2.938	-3.140	-3.263
HS graduates	-2.137	-1.842	-2.007	-2.315	-2.520	-2.730	-2.888	-2.974
Some college	-1.709	-1.480	-1.718	-1.985	-2.191	-2.466	-2.704	-2.843
College graduates	-1.260	-1.138	-1.448	-1.764	-2.073	-2.392	-2.625	-2.752

This table sets out values of the residential mobility index, $Mobility_{ex}$, described in Appendix D.3. This index is essentially the log rate of cross-state mobility (within the US), based on the 1960 census.

(denote these shares as $ShareDiffState5yr_{ex}$). And for each education-experience cell (e, x), we then compute the mobility index as:

$$Mobility_{ex} = \log\left[\frac{1}{2}\left(ShareDiffState5yr_{ex} + ShareDiffState5yr_{ex-1}\right)\right] \qquad \text{(D3)}$$

i.e. the log average of internal mobility shares of cells (e, x) and $(e, x-1)$, where the latter predicts the mobility of the same education cohort five years previously. For example, the mobility index of college graduates in experience group 8 (i.e. with 36-40 years of experience) is computed as the log average of graduates' 5-year mobility shares in experience groups 8 (36-40 years of experience) and 7 (31-35 years).

The computation of $Mobility_{ex}$ for experience group 1 (1-5 years of experience) is more challenging: we require a value of $ShareDiffState5yr_{ex}$ for a hypothetical pre-career experience group "0" (between -4 and 0 years of experience). We apply the following strategy. For college graduates (who we assume leave school at age 23), we compute $ShareDiffState5yr_{ex}$ for experience group "0" as the migration probability of students aged 19-23. Similarly, for the "some college" group in experience group 0, we use the migration probability of students aged 17-21. For high school graduates, we use students aged 15-19s; and for high school dropouts, we use students aged 13-17.

We set out the resulting mobility index $Mobility_{ex}$ in Table D1. As is well known, cross-state mobility is highest among the young and highly educated.

D.4 Cross-validation of undocumented migrant stocks

As we explain in Section 6.3, we probabilistically assign migrants to legal status (in every education-experience cell and year) using origin-specific undocumented shares from the Department of Homeland Security (2003). In this appendix, we cross-validate our estimates of undocumented migrant stocks, by education, against alternative methodologies.

Since the 1980s, demographers have been estimating the undocumented population as a residual, taking the difference between (i) the foreign-born population which appears in surveys (corrected for undercoverage) and (ii) official statistics of the legal migrant population: see Warren and Passel (1987). This is the source of the DHS origin-specific estimates which we apply in our analysis. More recently, Jeffrey Passel and colleagues at the Pew Research Center (e.g. Passel, 2006) have attempted to identify "likely" undocumented migrants in the Current Population Survey (CPS), to facilitate richer demographic analysis. The first step is to identify migrants who are likely legal residents, based on e.g. their country of origin, year of immigration, occupation, household composition, and naturalisation status (though recent migrants and Central Americans are excluded from this calculation, due to a known tendency to heavily over-report naturalisation). In the second step, the remaining migrants are probabilistically assigned to undocumented status, to match external "residual" estimates of the undocumented population by US geography, country of origin, and other dimensions. This second step is complex and difficult to replicate. However, Borjas (2017*b*) shows he can identify a population of undocumented migrants in the CPS (based again on origin, year of immigration, occupation and naturalisation), which closely matches the population from the Pew files (in terms of size and characteristics), but without any probabilistic imputation.

Using the CPS-ASEC samples of 2012 and 2013, Borjas (2017*b*) describes the characteristics of natives, legal and undocumented migrants, aged 20-64, based on the Pew undocumented identifier (to which he was given access) and his own. As Borjas shows in his Table 1, these are very similar. In Panels A and B of Table D2, we reproduce Borjas' estimates of population size and education composition (for the two methodologies). And in Panel C, we replicate this same exercise using our methodology (using CPS data from IPUMS: Flood et al., 2018), where legal status is imputed probabilistically using country of origin alone. Reassuringly, the estimates are very similar to both the Pew files and Borjas' reconstruction.

D.5 Disaggregation of 1960 migrant stocks by year of arrival

The 1960 census does not report migrants' year of arrival, but we require this information for the analysis in Appendix E.6. In particular, we need to know the employment stocks of "old" migrants (i.e. in the US for more than ten years) by education-experience cell. We impute these stocks using migrant cohort sizes ten years later. There are three steps:

1. For each education-experience stock of old migrants in 1960 (more than ten years in US), predict the size of the same cohort in 1970 (i.e. migrants with more than *twenty* years). We assume here that prospective high school graduates leave education at

Table D2: Cross-validation of education shares by legal status: CPS-ASEC 2012-3

	% of total population	Education shares (%)			
		HS dropout	HS grad	Some coll	Coll grad
Panel A: Pew CPS files					
Natives	82.9	7.1	29.3	32.7	30.9
Legal migrants	12.4	19.2	24.0	21.0	35.8
Undocumented migrants	5.4	42.0	28.8	13.2	16.0
Panel B: Borjas (2017b) reconstruction					
Natives	82.9	7.1	29.3	32.7	30.9
Legal migrants	11.5	19.9	25.2	21.2	33.8
Undocumented migrants	5.7	39.5	26.9	13.5	20.1
Panel C: Imputed by country of origin					
Natives	82.8	7.1	29.3	32.7	30.9
Legal migrants	13.6	21.0	25.0	20.0	34.1
Undocumented migrants	4.8	43.4	28.0	14.4	14.2

This table reports estimated population and education composition of natives, legal migrants and undocumented migrants, aged 20-64, in the Current Population Survey ASEC files of 2012 and 2013, based on various methodologies. The education shares are percentages of the native or legal/undocumented migrant population. Panels A and B reproduce Table 1 of Borjas (2017b), and Panel C is based on our own methodology (imputing legal status by country of origin).

19, those with some college at 21, and college graduates at 23. These assumptions allow us to assign every immigrant in 1970 to a 1960 labour market cell.

2. Account for emigration. If foreign-born residents leave the US, the cohort size in 1970 should be smaller than in 1960. To account for this, we repeat step (1) for the 1970 and 1980 census years; and we regress the log actual cohort size (in 1970) on the log predicted size (based on 1980 census data). We then use the regression estimates to predict the 1960 stocks of old migrants, based on the 1970 cohort size. The regression coefficient is 1.11, which suggests about 10% of immigrants leave the country each decade, which is consistent with Ahmed and Robinson (1994).

3. Convert from population to employment. Step (2) yields estimates of the old migrant population in every education-experience cell in 1960. For our analysis though, we require employment stocks. Our approach is to compute employment rates for the total migrant population in each 1960 education-experience cell, and then to apply these rates to the old migrant stocks. (Note the population and employment stocks of "new" migrants can be computed as the residual, after subtracting old migrants from the total migrant stock.)

Table E1: IV estimates of native wage equation for selection of (α_Z, σ_Z) values

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Fixed effects (224 observations)									
$\log Z\,(N_{ext}, M_{ext})$	0.027*	0.026*	0.026*	0.527***	0.542***	0.560***	1.027***	1.103***	1.020***
	(0.016)	(0.014)	(0.015)	(0.016)	(0.018)	(0.022)	(0.016)	(0.062)	(0.098)
Mig share m_{ext}	-0.502***	-0.550***	-0.568***	-0.502***	-1.551***	-1.970***	-0.502***	-3.022***	-3.976***
	(0.082)	(0.062)	(0.058)	(0.082)	(0.064)	(0.065)	(0.082)	(0.218)	(0.242)
Panel B: First differences (192 observations)									
$\log Z\,(N_{ext}, M_{ext})$	0.026	0.025	0.025	0.526***	0.544***	0.568***	1.026***	1.139***	1.072***
	(0.020)	(0.018)	(0.019)	(0.020)	(0.023)	(0.028)	(0.020)	(0.070)	(0.084)
Mig share m_{ext}	-0.369***	-0.418***	-0.435***	-0.369***	-1.448***	-1.891***	-0.369***	-2.982***	-3.964***
	(0.076)	(0.078)	(0.083)	(0.076)	(0.083)	(0.093)	(0.076)	(0.254)	(0.319)
σ_Z	1	1	1	0.5	0.5	0.5	0	0	0
α_Z	0	1	2	0	1	2	0	1	2

This table offers complete regression estimates (i.e. IV estimates of the native wage equation (20)) corresponding to a selection of (α_Z, σ_Z) values in Figure 2. These replicate the exercises of columns 6 and 8 of Table 5 (with the same instruments), but for different (α_Z, σ_Z) values. *** p<0.01, ** p<0.05, * p<0.1.

E Robustness of migrant share effect

E.1 Regression tables corresponding to Figure 2

In Table E1, we set out IV estimates of the native wage equation (20), corresponding to a selection of (α_Z, σ_Z) values in Figure 2. Notice that column 2 (with $\alpha_Z = \sigma_Z = 1$) is identical to columns 6 and 8 of Table 5 (Panel B).

E.2 Graphical illustration of mark-down effects

One may be concerned that our γ_2 estimates are driven by outliers. To address this, Figure E1 graphically illustrates our OLS and IV estimates of γ_2, both for fixed effects and first differences, based on columns 3, 6, 7 and 8 of Table 5 (Panel B). These plots partial out the effects of the controls (i.e. log total employment and the various fixed effects) from both native wages (on the y-axis) and migrant share (on the x-axis).[47] By inspection of the plots, it is clear the slope coefficients (which identify the γ_2 estimates of Table 5) are not driven by outliers.

E.3 Robustness to wage definition and weighting

In Table E2, we confirm that our IV estimates of the native wage equation (20) are robust to the choice of wage variable and weighting.

[47]For IV, we first replace both (i) log total employment and (ii) migrant share with their linear projections on the instruments and fixed effects; and we then follow the same procedure as for OLS.

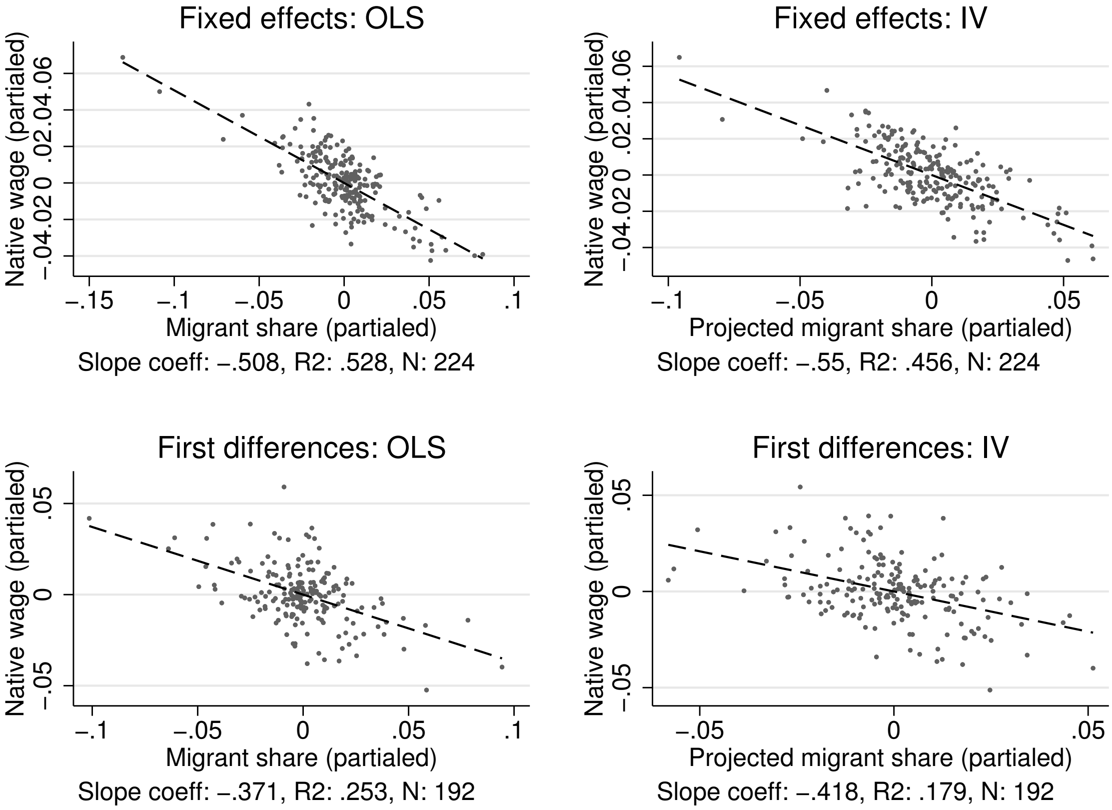


Figure E1: Visualisation of native wage responses to migrant share

This figure graphically illustrates the OLS and IV effects of migrant employment share, m_{ext}, on native composition-adjusted wages, based on columns 3, 6, 7 and 8 of Table 5 (Panel B).

In each specification, the right hand side is identical to columns 6 and 8 of Table 5 (Panel B), and we also use the same instruments. The only difference is the left hand side variable and the choice of weighting. Odd columns study the wages of native men, and even columns those of native women. Columns 1-2 and 5-6 study weekly wages of full-time workers (as in the main text), and the remaining columns hourly wages of all workers. All wage variables are adjusted for changes in demographic composition, in line with the method described in Appendix D.1. The estimates in Panel A are unweighted (as in Table 5); while in Panel B, we weight observations by total cell employment. It turns out the estimates are similar across specifications.

E.4 Functional form of mark-down function

In the native wage equation (20), we implement a linear approximation of the mark-down function, i.e. summarizing the mark-down effect with the migrant share m_{ext} on

Table E2: Robustness of native IV estimates to wage variable and weighting

	Fixed effects				First differences			
	FT weekly wages		Hourly wages		FT weekly wages		Hourly wages	
	Men	Women	Men	Women	Men	Women	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Unweighted estimates								
$\log(N_{ext} + M_{ext})$	0.021	0.045**	0.018	0.024	0.005	0.050***	0.005	0.043**
	(0.013)	(0.016)	(0.013)	(0.022)	(0.022)	(0.018)	(0.024)	(0.020)
Mig share m_{ext}	-0.460***	-0.589***	-0.420***	-0.584***	-0.398***	-0.381***	-0.353***	-0.369***
	(0.054)	(0.081)	(0.060)	(0.084)	(0.087)	(0.109)	(0.076)	(0.086)
Panel B: Weighted by cell employment								
$\log(N_{ext} + M_{ext})$	0.025	0.057**	0.020	0.034	-0.013	0.051*	-0.012	0.042
	(0.018)	(0.022)	(0.018)	(0.027)	(0.030)	(0.028)	(0.033)	(0.026)
Mig share m_{ext}	-0.467***	-0.543***	-0.416***	-0.549***	-0.463***	-0.360**	-0.404***	-0.339***
	(0.054)	(0.100)	(0.064)	(0.100)	(0.116)	(0.144)	(0.108)	(0.115)
Observations	224	224	224	224	192	192	192	192

This table assesses the robustness of our IV estimates of the native wage equation (20) to wage definition and choice of weighting. The right-hand side is identical to columns 6 and 8 of Table 5 (Panel B), and we also use the same instruments. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

the right-hand side. In this appendix, we show this linear approximation is theoretically attractive and empirically robust. We contrast it against an alternative specification, the log relative migrant supply $\log(M_{ext}/N_{ext})$, which appears elsewhere in our model (in the relative wage equation). Though the $\log(M_{ext}/N_{ext})$ specification yield qualitatively similar empirical results, the migrant share m_{ext} picks up the entire mark-down effect in a horse-race regression, just as our model predicts.

Theoretical validity of linear approximation

We begin by showing that the skill j mark-down ϕ_j is much better approximated as a linear function of the migrant share m_j than of the log relative migrant supply, $\log(M_j/N_j)$, in a model with non-discriminating firms. The derivative of the mark-down ϕ_j in equation (A2), with respect to the migrant share $m_j \equiv M_j/(N_j + M_j)$, is then:

$$\frac{d\phi_j}{dm_j} = \frac{\left(e^{\phi_j} - 1\right)^2}{e^{\phi_j}} \cdot (\tilde{\epsilon}_N - \tilde{\epsilon}_M) \tag{E1}$$

Notice the migrant share m_j has no effect on the mark-down if $\tilde{\epsilon}_N = \tilde{\epsilon}_M$, but increases the mark-down if migrants supply labour less elastically ($\tilde{\epsilon}_M < \tilde{\epsilon}_N$) even if the migrant share is small.

However, this is not the case for the relationship between ϕ_j and the log relative supply of migrants, i.e. $\log(M_j/N_j)$ or equivalently $\log(m_j/(1-m_j))$. The derivative can be written as:

$$\frac{d\phi_j}{d\log\left(\frac{M_j}{N_j}\right)} = \frac{d\phi_j}{dm_j} \cdot \frac{dm_j}{d\log\left(\frac{M_j}{N_j}\right)} = \frac{\left(e^{\phi_j}-1\right)^2}{e^{\phi_j}} \cdot m_j\left(1-m_j\right)\left(\tilde{\epsilon}_N - \tilde{\epsilon}_M\right) \qquad \text{(E2)}$$

This goes to zero as the migrant share m_j becomes small, even if $\tilde{\epsilon}_N \neq \tilde{\epsilon}_M$. Intuitively, if m_j is initially small, a very small increase in m_j (which has little effect on ϕ_j) will mechanically generate a large increase in $\log(M_j/N_j)$. Given this, a linear relationship between ϕ_j and $\log(M_j/N_j)$ will offer a relatively poor approximation of the true relationship, especially for small migrant share m_j.

Empirical performance of linear approximation

In Table E3, we compare the empirical performance of the m_j and $\log(M_j/N_j)$ approximations in the native wage equation (20). To construct an instrument for the log relative supply, we apply this functional form to the predicted stocks: i.e. $\log\left(\tilde{M}_{ext}/\tilde{N}_{ext}\right)$. Column 1 replicates our baseline estimates (in column 6 of Table 5), using the m_{ext} specification. Column 2 shows we get qualitatively similar results when we replace m_{ext} with $\log(M_{ext}/N_{ext})$: both take large negative coefficients. However, in a horse-race between the two, column 3 shows the mark-down effect is entirely picked up by the migrant share m_{ext}: this is consistent with our model's predictions. Notice the F-statistics are universally large in this column: i.e. our instruments offer sufficient power to disentangle the effects of m_{ext} and $\log(M_{ext}/N_{ext})$. Columns 4-6 repeat this exercise in first differences: the results are very similar.

E.5 Alternative specification for instrument

One may be concerned that our predictor for the migrant stock, $\tilde{M}_{ext}$, is largely noise; and that the first stage of our native wage equation is driven instead by the correlation between native employment N_{ext} and its predictor $\tilde{N}_{ext}$ – which appear in the denominators of the migrant share, $m_{ext} \equiv M_{ext}/(N_{ext}+M_{ext})$, and its instrument, $\tilde{m}_{ext} \equiv \tilde{M}_{ext}/\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$. See Clemens and Hunt (2019) for a related criticism.

However, Table E4 shows the IV estimates are robust to replacing the migrant share instrument $\tilde{m}_{ext}$ with its numerator $\tilde{M}_{ext}$. We scale $\tilde{M}_{ext}$ by 10^{-6} to make the coefficients visible in the table.

The instruments take the correct sign in the first stage: the migrant share is decreasing in $\log\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$ but increasing in $\tilde{M}_{ext}$; and the associated F-statistics are large, especially in first differences. Comparing the second stage estimates to Table 5, the

Table E3: Robustness of IV estimates to functional form of mark-down effect

	Fixed effects			First differences		
	(1)	(2)	(3)	(4)	(5)	(6)
$\log(N_{ext}+M_{ext})$	0.026*	0.006	0.023	0.025	0.010	0.034
	(0.014)	(0.025)	(0.031)	(0.018)	(0.019)	(0.027)
$m_{ext} \equiv \frac{M_{ext}}{N_{ext}+M_{ext}}$	-0.550***		-0.518**	-0.418***		-0.514***
	(0.062)		(0.195)	(0.078)		(0.154)
$\log \frac{M_{ext}}{N_{ext}}$		-0.092***	-0.007		-0.055***	0.021
		(0.026)	(0.052)		(0.019)	(0.034)
Instruments	$\log(\tilde{N}_{ext}+\tilde{M}_{ext})$ $\frac{\tilde{M}_{ext}}{\tilde{N}_{ext}+\tilde{M}_{ext}}$	$\log(\tilde{N}_{ext}+\tilde{M}_{ext})$ $\log\frac{\tilde{M}_{ext}}{\tilde{N}_{ext}}$	$\log(\tilde{N}_{ext}+\tilde{M}_{ext})$ $\frac{\tilde{M}_{ext}}{\tilde{N}_{ext}+\tilde{M}_{ext}}$, $\log\frac{\tilde{M}_{ext}}{\tilde{N}_{ext}}$	$\log(\tilde{N}_{ext}+\tilde{M}_{ext})$ $\frac{\tilde{M}_{ext}}{\tilde{N}_{ext}+\tilde{M}_{ext}}$	$\log(\tilde{N}_{ext}+\tilde{M}_{ext})$ $\log\frac{\tilde{M}_{ext}}{\tilde{N}_{ext}}$	$\log(\tilde{N}_{ext}+\tilde{M}_{ext})$ $\frac{\tilde{M}_{ext}}{\tilde{N}_{ext}+\tilde{M}_{ext}}$, $\log\frac{\tilde{M}_{ext}}{\tilde{N}_{ext}}$
F-stat: $\log(N_{ext}+M_{ext})$	251.14	120.49	110.42	97.10	88.90	49.29
F-stat: m_{ext}	202.13		64.83	80.04		75.67
F-stat $\log\frac{M_{ext}}{N_{ext}}$		54.35	57.45		74.69	48.67
Observations	224	224	224	192	192	192

In this table, we study the robustness of our IV estimates of the native wage equation (20) to the specification of the mark-down effect. Columns 1 and 4 replicates the baseline specifications of Table 5 (columns 6 and 8). Columns 2 and 5 replace the migrant share, m_{ext}, with the log relative migrant supply $\log(M_{ext}/N_{ext})$; and columns 3 and 6 include both simultanoeusly. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table E4: Alternative instrument specification for native wage equation

	First stage				Second stage	
	Fixed effects (FE)		First differences (FD)		FE	FD
	$\log(N_{ext}+M_{ext})$	m_{ext}	$\log(N_{ext}+M_{ext})$	m_{ext}	$\log W_{Next}$	$\log W_{Next}$
	(1)	(2)	(3)	(4)	(5)	(6)
$\log\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$	1.001***	-0.070**	0.790***	-0.047***		
	(0.063)	(0.027)	(0.083)	(0.013)		
$\tilde{M}_{ext}\times 10^{-6}$	-0.179*	0.240***	-0.257*	0.211***		
	(0.089)	(0.049)	(0.129)	(0.024)		
$\log(N_{ext}+M_{ext})$					0.043*	0.027
					(0.022)	(0.025)
Mig share m_{ext}					-0.386***	-0.387***
					(0.111)	(0.126)
SW F-stat	41.64	21.67	65.54	56.92	-	-
Observations	224	224	192	192	224	192

This table replicates the first and second stage estimates of the native wage equation (20) in Tables 4 and 5, but using an alternative instrument for migrant share. In the main text, our two instruments are $\log\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$ and $\tilde{m}_{ext}\equiv \tilde{M}_{ext}/\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$; but here, we replace $\tilde{m}_{ext}$ with $\tilde{M}_{ext}\times 10^{-6}$, the predicted migrant employment *level* (which we have scaled to make the coefficients visible). *** p<0.01, ** p<0.05, * p<0.1.

Table E5: Impact of new and old migrant shares: First stage

	Fixed effects			First differences		
	$\log\left(N_{ext}+M_{ext}\right)$	$\frac{M_{ext}^{old}}{N_{ext}+M_{ext}}$	$\frac{M_{ext}^{new}}{N_{ext}+M_{ext}}$	$\log\left(N_{ext}+M_{ext}\right)$	$\frac{M_{ext}^{old}}{N_{ext}+M_{ext}}$	$\frac{M_{ext}^{new}}{N_{ext}+M_{ext}}$
	(1)	(2)	(3)	(4)	(5)	(6)
$\log\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$	1.032***	0.004	0.047***	0.888***	-0.009	0.052***
	(0.073)	(0.011)	(0.014)	(0.089)	(0.008)	(0.014)
Predicted old mig share: $\frac{\tilde{M}_{ext}^{old}}{\tilde{N}_{ext}+\tilde{M}_{ext}}$	-0.006	1.387***	-0.102	0.403	1.089***	0.151
	(0.264)	(0.079)	(0.078)	(0.484)	(0.092)	(0.098)
Predicted new mig share: $\frac{\tilde{M}_{ext}^{new}}{\tilde{N}_{ext}+\tilde{M}_{ext}}$	2.239***	0.094	0.643***	2.400***	0.120	0.433**
	(0.437)	(0.110)	(0.194)	(0.573)	(0.139)	(0.180)
SW F-stat	94.00	435.22	16.63	4.01	8.34	2.40
Observations	224	224	224	192	192	192

This table presents first stage estimates for the native wage equation (20) (as in Table 4), but now accounting separately for the effect of the new migrant share $M_{ext}^{new}/\left(N_{ext}+M_{ext}\right)$, i.e. up to ten years in the US, and the old migrant share $M_{ext}^{old}/\left(N_{ext}+M_{ext}\right)$, i.e. more than ten years. Sanderson-Windmeijer F-statistics account for multiple endogenous variables. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

migrant share coefficients are somewhat smaller: both the fixed effect and first differenced estimates are -0.39 (down from -0.55 and -0.42 respectively). The standard errors are unsurprisingly larger, but both estimates remain statistically significant.

E.6 Heterogeneous effects of new and old migrants

Here, we study whether mark-downs respond differently to "new" migrants (up to ten years in the US) and "old" migrants (more than ten years).[48] Our approach is to control separately for the shares of new migrants $M_{ext}^{new}/\left(N_{ext}+M_{ext}\right)$ and old migrants $M_{ext}^{old}/\left(N_{ext}+M_{ext}\right)$ in the native wage equation (20). We construct distinct instruments for each, i.e. $\tilde{M}_{ext}^{new}/\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$ and $\tilde{M}_{ext}^{old}/\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$. Table E5 reports first stage estimates: our instruments perform very well in fixed effects, but offer limited power in first differences (F-statistics are all below 10).

Table E6 presents our OLS and IV estimates. Both the new and old migrant shares command large and negative effects. In OLS, the effects of old migrants are somewhat larger (columns 1 and 3); but they are similar in the IV fixed effect specification (column 2). In the first differenced IV specification, the standard error on the new immigrant share is too large to make definitive claims.

[48] We cannot construct employment stocks of new and old migrants in the 1960 census (which does not report year of arrival); so we impute the 1960 stocks using information on the same migrant cohorts, by year of arrival, 10 years later. See Appendix D.5 for further details.

Table E6: Impact of new and old migrants: OLS and IV

	Fixed effects		First differences	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
$\log(N_{ext} + M_{ext})$	0.011	0.028	0.005	0.032
	(0.013)	(0.021)	(0.010)	(0.044)
Old mig share: $\frac{M_{ext}^{old}}{N_{ext}+M_{ext}}$	-0.544***	-0.546***	-0.429***	-0.389***
	(0.039)	(0.048)	(0.053)	(0.116)
New mig share: $\frac{M_{ext}^{new}}{N_{ext}+M_{ext}}$	-0.304**	-0.579**	-0.273**	-0.530
	(0.111)	(0.282)	(0.110)	(0.647)
Observations	224	224	192	192

This table presents OLS and IV estimates of the native wage equation (20), but now accounting separately for the effect of the new migrant share $M_{ext}^{new}/(N_{ext}+M_{ext})$, i.e. up to ten years in the US, and the old migrant share $M_{ext}^{old}/(N_{ext}+M_{ext})$, i.e. more than ten years. The fixed effect and first differenced specifications are otherwise identical to Table 5. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

E.7 Heterogeneous wage effects by education and experience

Another pertinent question is whether the mark-down effects differ across labour market cells. To study this heterogeneity, we alternately interact the migrant share in equation (20) with a college dummy (taking 1 for cells with any college education) and a high-experience dummy (for 20+ years). These interactions require additional instruments: we use the interactions between the predicted migrant share and the college/experience dummies.

We report OLS and IV estimates in Table E7. Looking at the Sanderson-Windmeijer F-statistics (which account for multiple endogenous variables), the instruments appear to perform well. We find no significant evidence of heterogeneity by education or experience in IV, though the college interactions are imprecisely estimated.

In Section 6 in the main text, we show that the migrant share effect is driven by undocumented workers. Motivated by this insight, we now repeat the exercise of Table E7, but replacing the overall migrant share m_{ext} with the employment share of undocumented migrants m_{ext}^{undoc}. As instruments for the undocumented migrant share and its interactions, we use (i) the share of old Mexican migrants and (ii) the share of new Mexican migrants, each interacted with a college or high-experience dummy: see Section 6.3 for an explanation of these instruments. We report our estimates in Table E8. As before, the F-statistics suggest the instruments perform well. Similar to Table E7, we find no significant evidence of heterogeneous effects by education – though again, the college interactions are imprecisely estimated. However, we do find that wage effects are more negative in high-experience cells – though only in the first differenced specifications.

Table E7: Heterogeneous native wage effects by education and experience

	Fixed effects				First differences			
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\log(N_{ext} + M_{ext})$	0.034***	0.026*	0.026**	0.026	0.013	0.018	0.004	0.024
	(0.011)	(0.013)	(0.012)	(0.016)	(0.010)	(0.015)	(0.010)	(0.026)
Mig share m_{ext}	-0.487***	-0.553***	-0.491***	-0.550***	-0.367***	-0.422***	-0.312***	-0.414***
	(0.034)	(0.072)	(0.070)	(0.092)	(0.042)	(0.081)	(0.060)	(0.138)
$m_{ext} \times$ College	0.242	-0.031			0.093	-0.188		
	(0.220)	(0.299)			(0.127)	(0.241)		
$m_{ext} \times (\text{Exp} \geq 20)$			-0.023	0.001			-0.114**	-0.006
			(0.054)	(0.062)			(0.055)	(0.116)
F-Stat: $\log(N_{ext} + M_{ext})$		119.11		147.56		70.81		32.19
F-Stat: m_{ext}		100.89		171.18		74.68		47.13
F-Stat: $m_{ext} \times$ College		29.80				41.07		
F-Stat: $m_{ext} \times (\text{Exp} \geq 20)$				85.18				33.03
Observations	224	224	224	224	192	192	192	192

This table presents OLS and IV estimates of the native wage equation (20), but now accounting for differential effects of migrant share across college/non-college and high/low-experience cells. We implement this by interacting both the migrant share and (in IV specifications) its instrument with either a college or high-experience (more than 20 years) dummy. The fixed effect and first differenced specifications are otherwise identical to Table 5. Sanderson-Windmeijer F-statistics account for multiple endogenous variables. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table E8: Heterogeneous wage effects of undocumented migration

	Fixed effects				First differences			
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\log(N_{ext} + M_{ext})$	0.050***	0.052***	0.051***	0.056***	0.027**	0.036*	0.018	0.027
	(0.009)	(0.012)	(0.013)	(0.016)	(0.010)	(0.018)	(0.011)	(0.021)
Undoc mig share m_{ext}^{undoc}	-1.103***	-1.152***	-1.002***	-1.160***	-0.795***	-1.018***	-0.614***	-0.674***
	(0.096)	(0.107)	(0.138)	(0.120)	(0.115)	(0.161)	(0.125)	(0.165)
$m_{ext}^{undoc} \times$ College	-1.653	-0.615			-0.563	-1.990		
	(1.515)	(1.789)			(0.954)	(1.269)		
$m_{ext}^{undoc} \times (\text{Exp} \geq 20)$			-0.049	0.027			-0.339***	-0.291**
			(0.098)	(0.093)			(0.094)	(0.133)
F-Stat: $\log(N_{ext} + M_{ext})$		38.12		100.43		49.95		24.58
F-Stat: m_{ext}^{undoc}		42.48		110.65		36.36		15.72
F-Stat: $m_{ext}^{undoc} \times$ College		20.74				29.22		
F-Stat: $m_{ext}^{undoc} \times (\text{Exp} \geq 20)$				157.37				27.80
Observations	224	224	224	224	192	192	192	192

This table replicates the specifications of Table E7, except we now replace the overall migrant share m_{ext} with the undocumented migrant share m_{ext}^{undoc}. As instruments for the undocumented migrant share and its interactions, we use (i) the share of old Mexican migrants and (ii) the share of new Mexican migrants, each interacted with a college or high-experience dummy: see Section 6.3 for an explanation of these instruments. Sanderson-Windmeijer F-statistics account for multiple endogenous variables. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table E9: Robustness of native wage effects to dynamics: First stage

	Fixed effects				First differences			
	$\log(N_{ext}+M_{ext})$		Mig share m_{ext}		$\log(N_{ext}+M_{ext})$		Mig share m_{ext}	
	Current	Lagged	Current	Lagged	Current	Lagged	Current	Lagged
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\log\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$	0.890***	0.278***	0.023	0.021	0.784***	0.445***	0.038**	0.005
	(0.101)	(0.093)	(0.017)	(0.013)	(0.094)	(0.104)	(0.017)	(0.012)
$\log\left(\tilde{N}_{ext}+\tilde{M}_{ext}\right)$: Lagged	0.128*	0.850***	-0.025	0.018**	0.098	0.880***	0.029	0.014
	(0.074)	(0.096)	(0.018)	(0.008)	(0.086)	(0.099)	(0.022)	(0.009)
Predicted share $\tilde{m}_{ext}$	-0.539	-1.886***	0.973***	0.084	0.784**	-0.452	0.966***	-0.006
	(0.617)	(0.581)	(0.112)	(0.076)	(0.356)	(0.424)	(0.130)	(0.065)
$\tilde{m}_{ext}$: Lagged	2.292***	1.143*	0.156	0.808***	2.335***	0.371	0.407**	0.855***
	(0.602)	(0.641)	(0.117)	(0.081)	(0.594)	(0.434)	(0.166)	(0.079)
SW F-stat	58.67	43.16	17.94	29.76	29.36	108.91	43.45	28.73
Observations	192	192	192	192	160	160	160	160

This table presents first stage estimates for the native wage equation (20) (as in Table 4), but this time controlling additionally for the one-period lagged cell aggregator and migrant share. Since we include lagged observations, we lose one period of data. As additional instruments, and we use lags of our existing instruments. Sanderson-Windmeijer F-statistics account for multiple endogenous variables. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

E.8 Dynamic wage adjustment

One possible concern is serial correlation in the migrant share, conditional on the various fixed effects. If wages adjust sluggishly to immigration, the lagged migrant share will be an omitted variable; and in the presence of serial correlation, our γ_2 estimate in the native wage equation (20) may be biased (Jaeger, Ruist and Stuhler, 2018). However, as we now show, our instruments have sufficient power to disentangle the effect of contemporaneous and lagged shocks (despite serial correlation); and at least in IV, we find these dynamics are statistically insignificant (i.e. past shocks have no influence on current wages).

We take the native wage equation (20) as a point of departure, but now control additionally for the one-period lagged cell aggregator (in this case, total employment) and migrant share. The lag is 10 years at all observation years except for 2019 (where the lagged outcome corresponds to 2010). For IV, this requires two additional instruments: we use the lags of our existing instruments.

We present our first stage estimates in Table E9. Each instrument has a large positive effect on its corresponding endogenous variable, whether contemporaneous or lagged; and the F-statistics are universally large. This suggests the instruments offer sufficient power to disentangle the effects of contemporaneous and lagged immigration shocks.

Table E10 reports OLS and IV estimates. Since we include lagged observations, we lose one period of data; so for comparison, we report estimates of the basic specification (without lags) using the shorter sample. These look very similar to the full-sample estimates in Table 5. Turning to the dynamic specification, the lagged migrant share picks up about half the negative wage effect in OLS (columns 2 and 6): this might suggest there

Table E10: Robustness of native wage effects to dynamics: OLS and IV

	Fixed effects				First differences			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\log(N_{ext} + M_{ext})$	0.035**	0.043***	0.026*	0.026	0.010	0.029**	0.022	0.027
	(0.013)	(0.012)	(0.014)	(0.019)	(0.012)	(0.012)	(0.018)	(0.019)
$\log(N_{ext} + M_{ext})$: Lagged		0.016		0.017		0.030**		0.032**
		(0.010)		(0.015)		(0.012)		(0.014)
Mig share m_{ext}	-0.495***	-0.356***	-0.551***	-0.443***	-0.379***	-0.323***	-0.455***	-0.359***
	(0.029)	(0.053)	(0.035)	(0.075)	(0.043)	(0.060)	(0.061)	(0.075)
Mig share m_{ext}: Lagged		-0.213**		-0.112		-0.210*		-0.193
		(0.100)		(0.100)		(0.120)		(0.131)
Observations	192	192	192	192	160	160	160	160

This table presents OLS and IV estimates of the native wage equation (20), but controlling additionally for the one-period lagged cell aggregator and migrant share. Since we include lagged observations, we lose one period of data. Odd-numberd columns report estimates of the basic specification (without lags), for this shorter sample. Even-numbered columns focus on the dynamic specification. For IV, this requires two additional instruments: we use the lags of our existing instruments. The fixed effect and first differenced specifications are otherwise identical to Table 5. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

is large serial correlation (even conditional on the fixed effects) and sluggish adjustment to immigration shocks. However, once we apply the instruments in columns 4 and 8, the lagged shocks become comparatively smaller and statistically insignificant. That is, once we rely on variation which is more plausibly exogenous to cell-specific demand, we find no significant evidence of sluggish wage adjustment.

E.9 Robustness to observable demand shocks

In this paper, we have sought to construct our instrument using plausibly exogenous variation, which can exclude shifts in labour demand (conditional on the fixed effects). As supporting evidence for this restriction, we show in Table E11 that the native wage effects are robust to controlling for observable demand shocks. As our point of departure, we use the first differenced IV specification in Panel B of Table 5: this specification is most appropriate for this exercise, as our controls mostly predict *changes* in demand (rather than levels).

We begin in column 1 by replicating the relevant first differenced IV specification from Table 5: specifically column 8 of Panel B. In the remaining columns, we control progressively for a range of cell-specific shocks.

In column 2, we condition on a Bartik industry shift-share. This predicts differential changes in sectoral labour demand, by weighting decadal industry-level employment changes (the "shifters") by cell-level industry shares at the beginning of the decade (the "shares"). This shift-share will capture how labour market cells would be expected to

Table E11: Robustness of native wage effects (first differenced IV) to observable shocks

	(1)	(2)	(3)	(4)	(5)
$\log\left(N_{ext}+M_{ext}\right)$	0.025	0.020	0.024	0.022	0.023
	(0.018)	(0.017)	(0.016)	(0.017)	(0.017)
Mig share m_{ext}	-0.418***	-0.362***	-0.360***	-0.333***	-0.315***
	(0.078)	(0.073)	(0.074)	(0.079)	(0.084)
Bartik industry shift-share		0.420**	0.377**	0.488***	0.517***
		(0.159)	(0.158)	(0.161)	(0.158)
Initial routine share			0.107	0.053	0.061
			(0.079)	(0.105)	(0.120)
Initial offshorable share				0.104	0.105
				(0.085)	(0.096)
Minimum wage shift-share					0.398
					(0.425)
Observations	192	192	192	192	192

Column 1 replicates the baseline first-differenced IV specification of Table 5 (column 8 of Panel B), for native wages. The remaining columns test sensitivity to conditioning on predictors of various cell-level shocks. Specifications are otherwise identical to Panel B of Table 5. *** p<0.01, ** p<0.05, * p<0.1.

be affected by e.g. the decline in manufacturing.[49] The Bartik has a significant positive effect on wages, and the coefficient on the migrant share does contract (compared to column 1); but the difference is small. Thus, our conclusions are robust to conditioning on predictable industry shocks.

In column 3, we control additionally for initial cell-level routine share. The idea is to capture the potential for automation. This is the share of workers in each education-experience cell (at the beginning of the decade) who work in occupations with top-tercile routine task content (based on the index of Autor and Dorn, 2013). Conditional on our fixed effects, the effect of the immigration shock is robust to this control.

In column 4, we repeat the same exercise for initial cell-level offshorability share. This is the share of workers in each education-experience cell (at the beginning of the decade) who work in occupations with top-tercile offshorable task content (again, based on the index of Autor and Dorn, 2013). This also makes little difference.

E.10 Robustness to minimum wage effects

Another possible challenge is changes in the minimum wage, which may differentially affect workers across education-experience cells (according to its bite). To control for these effects, we construct a shift-share variable which predicts changes in the share of

[49]This speaks to Bohn and Sanders (2007), who raise concerns about correlation between cell-level immigration shocks and sectoral shifts.

workers (within each cell) who are paid the minimum wage. Our strategy is the following: for each decadal interval, we hold the native wage distribution fixed at the initial year (within education-experience cells), and then predict how changes in the state minimum wage (deflated by national wage growth) affect the share of natives (in our wage sample) earning the minimum wage or less. We borrow minimum wage data from Neumark (2020). Looking at column 5 of Table E11, this shift-share has a large positive effect on wages, though not statistically significant. The coefficient on migrant share does contract – but again, the difference is small.

E.11 Spatial variation

In the main text, we study national-level skill cell variation, following long precedent in the literature. But in this appendix, we additionally exploit *cross-state* variation in immigration intensity, within education-experience cells. Building on the native wage equation (20), and imposing $\alpha_{Zext} = \sigma_Z = 1$ for simplicity, consider the following empirical specification:

$$\log W_{Nsext} = \gamma_0 + \gamma_1 \log (N_{sext} + M_{sext}) + \gamma_2 m_{sext} + d_{sex} + d_{set} + d_{sxt} + v_{sext} \tag{E3}$$

where all variables now have an additional subscript s for state. State-level cell-specific native wages W_{Nsext} are adjusted for demographic composition, following a similar procedure to that of Appendix D.1.[50] We also interact all the original fixed effects from (20) with a full set of state s dummies.

To construct our instruments, we again exploit historical cohort sizes in the US and abroad, in a similar way to before. To predict the native and old migrant stocks by state and cell (i.e. $\tilde{N}_{sext}$ and $\tilde{M}^{old}_{sext}$), we apply the procedure described in Section 4.2 (exploiting mechanical aging of cohorts, within education groups), but now using state-specific samples. For new immigrants, we use a shift-share which takes the form:

$$\tilde{M}^{new}_{sext} = \sum_o \phi^s_{ot-1} \tilde{M}^{new}_{oext} \tag{E4}$$

Our "shifters" are the predicted origin/cell-specific inflows from our main analysis (which

[50] We take the following approach. Separately for each of our 32 education-experience cells, and separately for native men and women, we regress log wages on a quadratic in age, a postgraduate education indicator (for college graduate cells only), and race effects (Hispanic, Asian, black). But we now also include a full set of state-year interacted fixed effects. We then predict the mean male and female wage for each state-year combination, for a distribution of worker characteristics identical to the multi-year pooled sample (within education-experience cells). And we then compute a composition-adjusted native wage in each cell-state-year by taking weighted averages of the predicted male and female wages.

Table E12: State-level native wage effects

	Fixed effects		First differences	
	(1)	(2)	(3)	(4)
$\log(N_{sext} + M_{sext})$: State-level	0.046***	-0.002	0.038***	0.003
	(0.007)	(0.006)	(0.006)	(0.007)
Mig share m_{sext}: State-level	-0.469***	-0.026	-0.261***	0.033
	(0.025)	(0.045)	(0.024)	(0.038)
$\log(N_{ext} + M_{ext})$: National-level		0.032***		0.022***
		(0.006)		(0.007)
Mig share m_{ext}: National-level		-0.537***		-0.433***
		(0.034)		(0.025)
SW F-stats for:				
$\log(N_{sext} + M_{sext})$: State	1222.98	79.69	690.71	59.12
Mig share m_{sext}: State	245.30	90.75	128.89	54.95
$\log(N_{ext} + M_{ext})$: National		310.12		75.11
Mig share m_{ext}: National		73.12		27.00
Observations	11,194	11,194	9,590	9,590

This table presents estimates of equation (E3), across 32 education-experience cells, 7 decadal observations (1960-2019), and 50 states (with Washington D.C. merged into Maryland). In principle, this should yield 11,200 observations in the basic fixed effect specification; but 6 small cells are unrepresented in the sample. We include up to four endogenous variables (the cell aggregator and cell migrant share, measured at state-level and national-level respectively); and each has a corresponding instrument, described in Section E.11. The fixed effect specifications in columns 1-2 include interacted state-education-experience, state-education-year and state-experience-year fixed effects. In columns 3-4, all variables and instruments are first differenced, and we condition on interacted state-education-year and state-experience-year fixed effects. All observations are weighted by total state employment; and robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

exploit demographic shifts in the source country), denoted $\tilde{M}^{new}_{oext}$ for origin group o. We allocate these shifters to states s, according to the initial spatial distribution of each origin group o, where ϕ^{s}_{ot-1} is the share of origin o immigrants living in state s at time $t-1$. This exploits the preference of new immigrants to cluster in co-patriot enclaves (as in Altonji and Card, 1991; Card, 2001). As (E4) shows, we then sum these predicted allocations across origin groups o.

Table E12 reports IV estimates of (E3). We instrument the state-level cell-specific migrant share m_{sext} using $\tilde{m}_{sext} \equiv \tilde{M}_{sext} / \left(\tilde{N}_{sext} + \tilde{M}_{sext}\right)$, where $\tilde{M}_{sext} = \tilde{M}^{old}_{sext} + \tilde{M}^{new}_{sext}$ is the total predicted migrant stock; and we instrument the cell aggregator $\log(N_{sext} + M_{sext})$ using $\log\left(\tilde{N}_{sext} + \tilde{M}_{sext}\right)$. Throughout, we cluster errors by state, and weight each observation by the total state employment.[51] Column 1 shows the basic fixed effect specification: the estimates look similar to the equivalent national-level specification (column 6 of Table 5), with the migrant share m_{sext} taking a coefficient of -0.5.

[51]Within states, we are therefore weighting each education-experience cell at period t equally: this reflects our approach in the main empirical analysis.

Table E13: IV labour supply responses

	Log of average native hours				Log of average migrant hours			
	Men		Women		Men		Women	
	FE	FD	FE	FD	FE	FD	FE	FD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\log(N_{ext} + M_{ext})$	0.034**	0.034	0.035	0.065*	0.002	0.016	-0.126**	-0.057
	(0.016)	(0.021)	(0.028)	(0.033)	(0.016)	(0.017)	(0.048)	(0.040)
Mig share m_{ext}	-0.074	-0.085	-1.123***	-0.749***	-0.074	0.016	0.022	-0.533**
	(0.151)	(0.116)	(0.174)	(0.199)	(0.067)	(0.088)	(0.301)	(0.217)
Observations	224	192	224	192	224	192	224	192

This table re-estimates the native wage equation (20), but replacing the dependent variable with the log of average annual employment hours (including individuals with zero hours) for various subgroups, adjusted for observable changes in composition. The right-hand side of all specifications are identical to columns 6 and 8 of Table 5 (Panel B), for both fixed effect (FE) and first differenced (FD) specifications. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A number of papers, e.g. Borjas (2006), Monras (2020) or Amior (2024), have argued that labour markets are well-integrated nationally, at least over the decadal intervals we study. This claim often serves as motivation for using national-level variation, which we exploit in the main analysis. To address this idea empirically, column 2 controls additionally for the *national-level* cell aggregator and migrant share (within education-experience cells), i.e. $\log(N_{ext} + M_{ext})$ and m_{ext}. This requires two additional instruments: we simply use the national-level predictors, $\tilde{m}_{ext}$ and $\log\left(\tilde{N}_{ext} + \tilde{M}_{ext}\right)$. These instruments offer substantial power: see the F-statistics at the bottom of the table. Interestingly, the national cell migrant share m_{ext} picks up the entire wage effect: this suggests that labour markets are indeed well integrated nationally.

Finally, in columns 3 and 4, we estimate the same equations in first differences. In doing so, we retain the interacted d_{set} and d_{sxt} fixed effects, but drop the d_{sex} effects. At least qualitatively, these estimates show similar patterns before.

E.12 Labour supply responses

In Table E13, we replace the left-hand side of the native wage equation with the log of average annual employment hours (across all individuals, including those with zero hours). Just as in our wage sample (and like Borjas, 2003), we exclude enrolled students when computing annual hours; and we also adjust for changes in demographic composition[52] (as we do for wages). We report IV estimates for both fixed effect and first differenced

[52]Our motivation for adjusting annual hours is the same as for wages: changes in either outcome may be conflated with observable demographic shifts (within education-experience cells). We follow identical steps to those described in Appendix D.1; but this time, we estimate linear regressions for annual employment hours (including zeroes for individuals who do not work) rather than log wages.

specifications, separately for the annual hours of men and women, and separately for natives and migrants.

Consistent with Borjas (2003) and Monras (2020), who study similar skill-cell variation, we find that migrant share (suitably instrumented) does indeed reduce native employment hours. It turns out this response is entirely driven by native women, which matches the findings of Borjas and Edo (forthcoming) in France.

F Sensitivity to specification of technology

F.1 Robustness of CES assumption

To estimate the native wage equation (20), we need to construct a cell-level aggregator $Z(N, M)$ over native and migrant employment. Following the literature, we have assumed Z has CES form. But our identification strategy can be generalised to any Z with constant returns. This is because, assuming constant returns, the relative wage W_{Mext}/W_{Next} (of migrants to natives) can always be reduced to some function of the relative supply M_{ext}/N_{ext}, which can in principle be estimated (allowing us to construct Z). To see why, notice that any constant returns Z can be expressed as:

$$Z(N, M) = Nz\left(\frac{M}{N}\right) \tag{F1}$$

for some single-argument function z. Using (11)-(13), and accounting for the mark-downs, the native and migrant wages can then be expressed as:

$$\log W_{Next} = \log A_{ext} - (1 - \sigma_X) \log N_{ext} + \log \left[\frac{z_{ext}\left(\frac{M_{ext}}{N_{ext}}\right) - \frac{M_{ext}}{N_{ext}} z'_{ext}\left(\frac{M_{ext}}{N_{ext}}\right)}{z_{ext}\left(\frac{M_{ext}}{N_{ext}}\right)^{1-\sigma_X}} \right] - \phi_N (m_{ext}) \tag{F2}$$

$$\log W_{Mext} = \log A_{ext} - (1 - \sigma_X) \log N_{ext} + \log \left[\frac{z'_{ext}\left(\frac{M_{ext}}{N_{ext}}\right)}{z_{ext}\left(\frac{M_{ext}}{N_{ext}}\right)^{1-\sigma_X}} \right] - \phi_M (m_{ext}) \tag{F3}$$

Under equal mark-downs ($H1$), the relative wage can then be written as:

$$\log \frac{W_{Mext}}{W_{Next}} = \log \left[\frac{z'_{ext}\left(\frac{M_{ext}}{N_{ext}}\right)}{z_{ext}\left(\frac{M_{ext}}{N_{ext}}\right) - \frac{M_{ext}}{N_{ext}} z'_{ext}\left(\frac{M_{ext}}{N_{ext}}\right)} \right] \tag{F4}$$

which is a function of the relative supply M_{ext}/N_{ext} alone. In principle, this function can be estimated non-parametrically, allowing us to construct Z. We can then replace the (bracketed) CES aggregator in the native wage equation (20) with this more flexible Z,

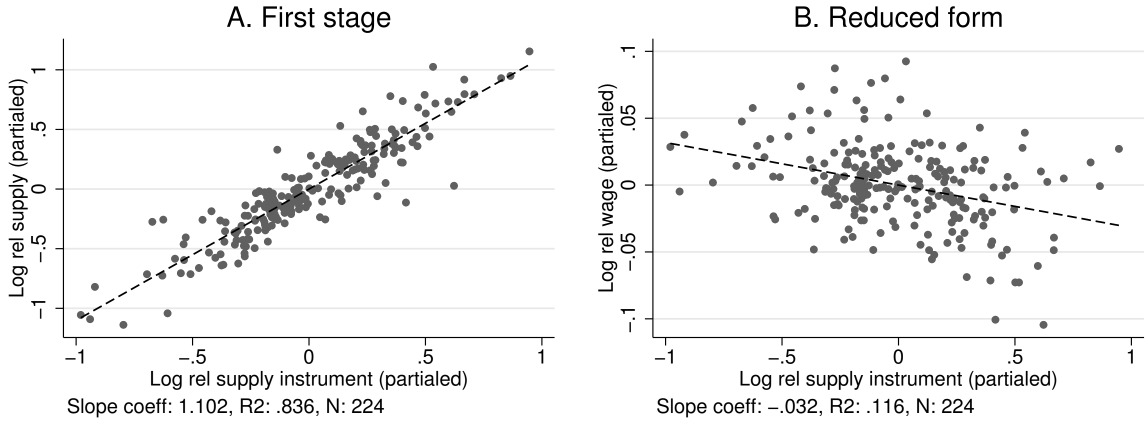


Figure F1: Visualisation of relative wage equation estimates

This figure graphically illustrates our preferred IV specification of the relative wage equation (18), i.e. the log relative migrant-native wage $\log\left(w_{Mext}/w_{Next}\right)$ on log relative supply $\log\left(M_{ext}/N_{ext}\right)$. We focus on the specification of column 5 of Table 3, which controls for education-experience effects and year effects. In Panel A, we plot the first stage relationship, i.e. the log relative supply on its instrument $\log\left(\tilde{M}_{ext}/\tilde{N}_{ext}\right)$, after partialing the fixed effects from both the left and right-hand side variables. Panel B does the same for the reduced form, i.e. the log relative wage directly on the instrument $\log\left(\tilde{M}_{ext}/\tilde{N}_{ext}\right)$.

and estimate equation (20) linearly as before. A rejection of $\gamma_2 = 0$ (i.e. a significant effect of migrant share) would then imply rejection of the joint hypothesis of $H1$ and $H2$ (equal and independent mark-downs).

However, this more flexible approach is ultimately redundant. As equation (16) shows, CES implies the relationship between relative wages W_M/W_N and relative supply M_{ext}/N_{ext} is log-linear; and it turns out that log-linearity offers a reasonable description of the data. To demonstrate this, we present scatter-plots which illustrate our preferred IV specification of the relative wage equation (18): i.e. column 5 of Table 3 (which controls for education-experience effects and year effects). In Panel A of Figure F1, we plot the first stage relationship corresponding to this specification (i.e. the log relative supply M_{ext}/N_{ext} on its instrument), after partialing the fixed effects from both the left and right-hand side variables. And in Panel B, we do the same for the reduced form (i.e. the log relative wage W_M/W_N directly on the instrument). In each case, by inspection, linearity appears a reasonable description of the data.

F.2 Cross-cell heterogeneity in σ_Z

In our relative wage model (equation (18)), we implicitly assume that σ_Z (the within-cell substitutability between natives and migrants) is identical across education-experience cells. But one may be concerned about heterogeneity in σ_Z: this would imply the Z aggregator should be constructed differently (on the right-hand side of the native wage

Table F1: Heterogeneity in relative wage estimates

	Basic estimates		Fixed effects: Edu*Exp, Yr		First differences		First diff + Yr effects	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\log \frac{M_{ext}}{N_{ext}}$	0.009	0.000	-0.029***	-0.010	-0.045***	-0.057***	-0.003	-0.004
	(0.007)	(0.006)	(0.010)	(0.011)	(0.008)	(0.010)	(0.006)	(0.009)
$\log \frac{M_{ext}}{N_{ext}} \times$ College		-0.010**		0.046**		0.015		0.045**
		(0.004)		(0.018)		(0.014)		(0.018)
$\log \frac{M_{ext}}{N_{ext}} \times (\text{Exp} \geq 20)$		0.017***		-0.004		0.015		-0.001
		(0.004)		(0.006)		(0.010)		(0.009)
F-Stat: $\log \frac{M_{ext}}{N_{ext}}$	493.81	1129.76	233.61	68.21	354.48	223.85	470.36	251.36
F-Stat: $\log \frac{M_{ext}}{N_{ext}} \times$ College		2654.62		146.22		1753.95		1088.10
F-Stat: $\log \frac{M_{ext}}{N_{ext}} \times (\text{Exp} \geq 20)$		5445.25		3060.78		830.03		549.35
Observations	224	224	224	224	192	192	192	192

This table tests for heterogeneity in our IV estimates of the relative wage equation (18), across college/non-college cells and high/low experience cells. In odd-numbered columns, we report estimates without heterogeneity: these replicate the baseline estimates of Table 3. In even-numbered columns, we include interactions between log relative employment and (i) a college dummy and (ii) a high-experience (more than 20 years) dummy. Our instruments are the interactions between the predicted log relative employment and these dummies. Throughout, wages are adjusted for changes in observable demographic composition. Sanderson-Windmeijer F-statistics account for multiple endogenous variables. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

equation), and this may cause us to incorrectly estimate the mark-down effect.

In Table F1, we test for heterogeneity in IV estimates of the relative wage equations, across college/non-college cells and high/low experience cells. In odd-numbered columns, we report estimates without heterogeneity: these replicate the baseline estimates of Table 3. In even-numbered columns, we include interactions between log relative employment and (i) a college dummy and (ii) a high-experience (more than 20 years) dummy. Our instruments are the interactions between the predicted log relative employment and these dummies. The F-statistics show the first stage is strong in each case. However, in the second stage, the interactions are quantitatively small. This suggests that heterogeneity in σ_Z across education-experience cells will not affect our conclusions.

F.3 Broad education and experience groups

We next study alternative specifications with two (instead of four) education groups, and four (instead of eight) experience groups. For the two-group education specification, we divide workers into "college-equivalents" (i.e. all college graduates, plus 0.8 times half of the some-college stock) and "high-school equivalents" (high school graduates, plus 0.7 times the dropout stock, plus 1.2 times half of the some-college stock): the weights, borrowed from Card (2009), have an efficiency unit interpretation. This leaves us with just 16 clusters (since we cluster by labour market cell); but at least in this data, the bias to the standard errors appears to be small.[53]

[53]For example, consider the OLS effect of m_{ext} in column 1 of Table F2. Since we have 16 clusters, we apply the 95% critical value of the $T(15)$ distribution (as recommended by Cameron and Miller, 2015), which is 2.13. The standard error in column 1 of Panel B then implies a confidence interval of $[-1.360, -0.656]$. But the wild bootstrap recommended by Cameron, Gelbach and Miller (2008), which

Table F2: Broad education and experience groups: OLS and IV

	2 education groups				4 experience groups			
	Fixed effects		First differences		Fixed effects		First differences	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Imposing equal mark-downs ($H1$)								
Cell aggregator	0.004	-0.011	0.038	0.047	0.051***	0.056***	0.042***	0.070***
	(0.034)	(0.050)	(0.028)	(0.057)	(0.013)	(0.017)	(0.014)	(0.023)
Mig share m_{ext}	-1.123***	-1.621***	-0.754***	-1.348*	-0.547***	-0.565***	-0.497***	-0.589***
	(0.162)	(0.460)	(0.148)	(0.634)	(0.044)	(0.079)	(0.079)	(0.096)
F-Stat: Cell aggregator	-	142.02	-	8.48	-	76.13	-	72.86
F-Stat: Mig share m_{ext}	-	24.66	-	14.83	-	77.67	-	23.58
Panel B: Imposing $\alpha_{Zext} = \sigma_Z = 1$								
Cell aggregator	-0.088**	-0.103*	-0.050*	-0.046	0.020	0.025	0.014	0.040*
	(0.033)	(0.048)	(0.028)	(0.056)	(0.014)	(0.017)	(0.014)	(0.023)
Mig share m_{ext}	-1.008***	-1.511***	-0.635***	-1.236*	-0.491***	-0.510***	-0.443***	-0.530***
	(0.165)	(0.462)	(0.148)	(0.630)	(0.044)	(0.079)	(0.077)	(0.096)
F-Stat: Cell aggregator	-	141.91	-	8.85	-	85.93	-	75.12
F-Stat: Mig share m_{ext}	-	24.51	-	14.59	-	82.04	-	22.91
Observations	112	112	96	96	112	112	96	96

This table presents OLS and IV estimates of the native wage equation (20), but this time across broader labour market cells. In columns 1-4, we use 2 broad education groups (college and high school equivalents) and the 8 original experience groups. And in columns 5-8, we use the original 4 education groups, but this time 4 broad experience groups (1-10, 11-20, 21-30, 31-40 years). Specifications are otherwise identical to Table 5. Sanderson-Windmeijer F-statistics account for multiple endogenous variables. Robust standard errors, clustered by 16 education-experience cells, are in parentheses. We apply the same small-sample corrections as detailed in Table 3. The relevant 95% critical value for the T distribution (with $G-1=15$ degrees of freedom, where G is the number of clusters) is 2.13. *** p<0.01, ** p<0.05, * p<0.1.

Similar to Table 5, we consider alternative specifications of the native wage equation under equal mark-downs ($H1$) and equal productivities ($\alpha_{Zext} = \sigma_Z = 1$). In the former case, we impose a σ_Z of 0.907. This is estimated from an IV relative wage equation with education-experience and year fixed effects (which we do not report in full here).[54]

We report OLS and IV estimates in columns 1-4 of Table F2, together with F-statistics. Notice that γ_1 (the elasticity to total cell employment) is now consistently negative in the $\alpha_{Zext} = \sigma_Z = 1$ specification. Under fixed effects, γ_1 is about -0.1, which implies an elasticity of substitution between experience groups (within education nests) of 10: this is similar to Card and Lemieux (2001), who use an equivalent two-group education classification.[55] The migrant share effect γ_2 now exceeds -1 under fixed effects (columns 1-2). In first differences (columns 3-4), it ranges from -0.6 to -1.3.

we implement with Roodman et al.'s (2019) "boottest" command, delivers a very similar interval of $[-1.317, -0.605]$.

[54]The β_1 estimate in (18) is -0.093, with a standard error of 0.037.

[55]In their main specification, they estimate an elasticity of substitution of 5 across age (rather than experience) groups. But they also offer estimates across experience groups, and these are quantitatively similar to ours.

In columns 5-8 of Table F2, we also re-estimate our model using four 10-year experience groups (rather than eight 5-year groups), while keeping the original four-group education classification. This makes little difference to our baseline estimates in Table 5. This result can also help address concerns over the independence of the detailed 5-year education-experience clusters in the baseline specification: Table F2 shows the estimates (and standard errors) are little affected after aggregating to larger 10-year groups.

F.4 Occupation-imputed migrant stocks

In this paper, we allocate migrants to native labour market cells according to their education and experience, following the example of Borjas (2003), Ottaviano and Peri (2012) and others. However, one important concern is that migrants may "downgrade" occupation and compete with natives of lower education or experience. If so, the migrant stocks in native cells may be measured with error (Dustmann, Schoenberg and Stuhler, 2016).

To address this concern, we now probabilistically allocate migrants (of given education and experience) to native cells according to their occupational distribution. Our strategy is similar in spirit to Card (2001) and Sharpe and Bollinger (2020). Suppose there are O occupations, denoted o, and EX education-experience cells, denoted ex. Let $\mathbf{\Pi}^M_{O\times EX}$ be a matrix, with O rows and EX columns, which allocates migrant education-experience cells to occupations, where the (o, ex) element is the share of education-experience ex migrant labour which is employed in occupation o (so the columns of $\mathbf{\Pi}^M_{O\times EX}$ sum to 1). We base these shares on averages across all sample years. Similarly, let $\mathbf{\Pi}^N_{EX\times O}$ be an $EX \times O$ matrix which allocates occupations to native education-experience cells, where the (ex, o) element is the share of occupation o native labour which has education-experience ex (so the columns of $\mathbf{\Pi}^N_{EX\times O}$ sum to 1). Using these matrices, we can probabilistically allocate migrant education-experience stocks to native cells, according to their occupational distribution:

$$\mathbf{M}^{occ}_{EX\times T} = \mathbf{\Pi}^N_{EX\times O}\mathbf{\Pi}^M_{O\times EX}\mathbf{M}_{EX\times T} \tag{F5}$$

where $\mathbf{M}_{EX\times T}$ is the matrix of *actual* migrant employment stocks by education-experience cell and time, and $\mathbf{M}^{occ}_{EX\times T}$ is the *imputed* allocation of migrants to native cells (based on the occupational distributions). We use an identical strategy to construct instruments for the occupation-imputed migrant stock:

$$\tilde{\mathbf{M}}^{occ}_{EX\times T} = \mathbf{\Pi}^N_{EX\times O}\mathbf{\Pi}^M_{O\times EX}\tilde{\mathbf{M}}_{EX\times T} \tag{F6}$$

where $\tilde{\mathbf{M}}^{occ}_{EX\times T}$ are our instruments for immigrant stocks by education and experience, as described in Section 4.2. In practice, we rely on the time-consistent IPUMS classification

Table F3: Occupation-imputed migrant stocks: OLS and IV

	Fixed effects				First differences			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Imposing equal mark-downs ($H1$)								
Cell aggregator	0.033		-0.089		0.013		-0.017	
	(0.023)		(0.069)		(0.015)		(0.051)	
Mig share: m_{ext}^{occ}	-0.569***	-0.764***	-1.494***	-0.971***	-0.361***	-0.436***	-0.843***	-0.742***
	(0.170)	(0.145)	(0.484)	(0.163)	(0.111)	(0.078)	(0.300)	(0.117)
F-Stat: Cell aggregator	-	-	21.35	-	-	-	10.93	-
F-Stat: Mig share m_{ext}^{occ}	-	-	20.22	88.10	-	-	12.89	96.38
Panel B: Imposing $\alpha_{Zext} = \sigma_Z = 1$								
Cell aggregator	0.012		-0.104		-0.001		-0.035	
	(0.022)		(0.066)		(0.014)		(0.050)	
Mig share: m_{ext}^{occ}	-0.542***	-0.613***	-1.426***	-0.819***	-0.297**	-0.292***	-0.799***	-0.589***
	(0.175)	(0.143)	(0.459)	(0.164)	(0.111)	(0.082)	(0.291)	(0.123)
F-Stat: Cell aggregator	-	-	25.52	-	-	-	11.34	-
F-Stat: Mig share m_{ext}^{occ}	-	-	23.32	88.10	-	-	14.40	96.38
Observations	224	224	224	224	192	192	192	192

This table presents OLS and IV estimates of the native wage equation (20), but this time replacing education-experience migrant stocks, M_{ext}, with occupation-imputed stocks, M_{ext}^{occ}, when constructing the cell aggregator and migrant share: these are now equal to $\log\left(N_{ext}^{\sigma_Z} + \alpha_{Zext} M_{ext}^{occ\,\sigma_Z}\right)^{1/\sigma_Z}$ and $m_{ext}^{occ} \equiv M_{ext}^{occ} / \left(N_{ext} + M_{ext}^{occ}\right)$ respectively. Similarly, when constructing our instruments, we replace predicted migrant stock, $\tilde{M}_{ext}$, with occupation-imputed equivalents, $\tilde{M}_{ext}^{occ}$. Specifications are otherwise identical to Table 5. Sanderson-Windmeijer F-statistics account for multiple endogenous variables. Robust standard errors, clustered by 32 education-experience cells, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

of occupations (based on the 1990 census scheme), aggregated to 81 groups.

We now re-estimate the native wage equation (20), but replacing education-experience migrant stocks M_{ext} with occupation-imputed stocks M_{ext}^{occ} when constructing the regression variables (and similarly for the instruments). Thus, the cell aggregator is now $\log\left(N_{ext}^{\sigma_Z} + \alpha_{Zext} M_{ext}^{occ\,\sigma_Z}\right)^{1/\sigma_Z}$, the migrant share is $m_{ext}^{occ} \equiv M_{ext}^{occ} / \left(N_{ext} + M_{ext}^{occ}\right)$, and the instruments are $\log\left(\tilde{N}_{ext} + \tilde{M}_{ext}^{occ}\right)$ and $\tilde{m}_{ext}^{occ} \equiv \tilde{M}_{ext}^{occ} / \left(\tilde{N}_{ext} + \tilde{M}_{ext}^{occ}\right)$ respectively. As in Table 5, we report estimates both under equal mark-downs ($H1$) and under equal productivities ($\alpha_{Zext} = \sigma_Z = 1$). In the former case, we impose a σ_Z of 0.979. This is estimated from an IV relative wage equation with education-experience and year fixed effects (which we do not report in full here), based on the occupation-imputed stocks.[56]

We present OLS and IV estimates in Table F3. The OLS effects of migrant share (columns 1 and 5) are similar to our baseline specifications in the main text (compare to Table 5). The IV effects are much larger, with migrant share effects around -1.5 in fixed effects and -0.8 in first differences. However, the standard errors are also large: about

[56]The β_1 estimate in (18) is -0.021, with a standard error of 0.006.

0.5 in fixed effects and 0.3 in first differences. This appears to stem from a collinearity problem: once we drop the cell aggregator (whose coefficient is always insignificant) in columns 4 and 8, the IV effects of migrant share are smaller (ranging from -0.6 to -1) and more precise (with standard errors between 0.1 and 0.2). This suggests our estimates are robust to concerns about occupational downgrading.

G Separation elasticities

In this appendix, we present estimates of job separation elasticities across workers with different migrant status and education. We rely on data from the Survey of Income and Program Participation (SIPP), which we access from National Bureau of Economic Research (2019). Separation elasticities offer a useful (and easily estimable) proxy for the elasticity of labour supply to a firm. Since the flow of separations from a firm must equal the flow of recruits in equilibrium, the overall elasticity of labour supply to the firm should be double the separation elasticity (Manning, 2003). We are not the first to compare separation elasticities by migrant status: see Hotchkiss and Quispe-Agnoli (2013), Hirsch and Jahn (2015) and Biblarsh and De-Shalit (2021).

The SIPP is a longitudinal dataset with large samples and frequent waves, just four months apart. We rely on SIPP panels beginning 1996, 2001, 2004 and 2008 (which cover the period 1996-2013). Our sample consists of individuals aged at least 18, with 1-40 years of potential labour market experience and no business income. Unusually, the SIPP records whether migrants have legal permanent residency (i.e. a green card). For an individual in employment at the end of wave $t-1$, a separation occurs (by our definition) if that individual leaves their "primary" job[57] by the end of wave t. We use hourly wages of civilian employees aged 18 or over, with 1-40 years of experience, and who work at least 15 hours per week; and we exclude the top and bottom 1% of wage observations. Wages are deflated by CPI, using data from Bureau of Labor Statistics (2022).

Like Manning (2003), we estimate the elasticities using a complementary log-log model. Suppose the instantaneous separation rate (denoted s_{it}) for individual i is fixed within the time interval $t-1$ to t. The probability of separating within this interval is then:

$$\Pr\left(Sep_{it} = 1\right) = 1 - \exp\left(-s_{it}\right) \tag{G1}$$

where Sep_{it} is a binary variable taking 1 if the individual separates between $t-1$ and t.

[57]Respondents report up to two jobs in each wave. If an individual reports two jobs in $t-1$, the primary job is the one which occupies the most weekly hours. Where both jobs have the same hours, we define the primary job as the first one reported in the survey.

Table G1: Separation elasticities

	Full sample	College	Non-college				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lagged log wage	-0.501***	-0.555***	-0.394***	-0.395***	-0.377***	-0.368***	-0.218***
	(0.015)	(0.018)	(0.026)	(0.026)	(0.029)	(0.028)	(0.027)
Lagged log wage * Migrant	0.211***	0.156***	0.267***	0.212***	0.206***	0.216***	0.186***
	(0.031)	(0.041)	(0.056)	(0.060)	(0.061)	(0.060)	(0.059)
Lagged log wage * Non-permanent				0.306**	0.281*	0.306**	0.257*
				(0.143)	(0.143)	(0.144)	(0.140)
Immigrant	-0.332***	-0.182	-0.460***	-0.347**	-0.349**	-0.386***	-0.267*
	(0.082)	(0.116)	(0.136)	(0.147)	(0.148)	(0.147)	(0.144)
Non-permanent status				-0.633*	-0.593*	-0.646*	-0.513
				(0.331)	(0.330)	(0.333)	(0.324)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Initial occupation controls	No	No	No	No	Yes	No	No
Initial industry controls	No	No	No	No	No	Yes	No
Initial tenure controls	No	No	No	No	No	No	Yes
Observations	889,210	566,313	322,897	322,897	322,357	322,826	322,896

This table reports estimates of the elasticity of job separation to initial wages, based on the complementary log-log specification in equation (G2). We rely on SIPP panels beginning 1996, 2001, 2004 and 2008 (which cover the period 1996-2013), whose waves are four months apart. All specifications control for education, experience, gender, and various interactions. Column 1 is estimated for the full sample, columns 2 for individuals with at least some college education, and columns 3-6 for those without. Robust standard errors, clustered by individual, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

This motivates our complementary log-log model:

$$\Pr\left(Sep_{it}=1\right)=1-\exp\left(-\exp\left(\beta_W\log W_{it-1}+\beta_X'X_{it}+\beta_t\right)\right) \tag{G2}$$

where we write s_{it} as a function of the initial wage W_{it-1}, human capital and demographic indicators[58] X_{it}, and a full set of wave effects β_t. The coefficient of interest β_W (which we expect to be negative) can then be interpreted as the elasticity of the instantaneous separation rate with respect to W_{it-1}. Assuming a constant hazard, this interpretation is independent of the time interval between waves.

The purpose of the X_{it} controls is to purge, as much as possible, variation across individuals in productivity. Ideally, this should allow us to interpret β_W as the separation elasticity with respect to W_{it-1} for individuals of fixed marginal product: that is, the elasticity of the separation rate to wage mark-downs. Of course, unobservable heterogeneity in individual productivity may confound this interpretation (and bias our elasticity estimates towards zero); but comparisons of separation elasticities across demographic groups can still be informative.

Table G1 presents our estimates of the separation elasticity β_W by immigration status. All variables which are interacted with the lag logged wage are included individually on

[58]Specifically: experience and experience squared; four education indicators (high school graduate, some college, undergraduate and postgraduate), each interacted with a quadratic in experience; and a gender dummy, interacted with education and the experience quadratic.

the right-hand side (among the demographic controls). In column 1, we include both the lagged wage and an interaction with a foreign-born dummy: on average, migrants have significantly lower separation elasticities than natives: -0.29 compared to -0.50. Biblarsh and De-Shalit (2021) reach similar conclusions using the US Current Population Survey. In columns 2 and 3, we re-estimate this equation for college and non-college samples. Notice first that the native separation elasticities vary little by education. However, the native-migrant differential in separation elasticities is mostly driven by the low-educated: in column 3, the migrant elasticity is just one third of the native elasticity.

In column 4, restricting to the low-educated sample, we also interact the lagged wage with a dummy for non-permanent status: many low-educated migrants in this category are likely undocumented (Hall, Greenman and Farkas, 2010). The elasticity for this group is close to zero (to see this, sum the first three coefficients in column 4), and significantly smaller than the elasticity of migrants with permanent residency. Of course, given the evidence from Passel, Clark and Fix (1997) on self-reported naturalisation, one might expect that some migrants without green cards will misreport their residency status. If so, the true differential (in separation elasticities) between permanent and non-permanent migrants may be even larger than column 4 suggests.

One possible concern is that migrants sort into less stable jobs, and this may explain the differences in the elasticities. In an attempt to control for this, we condition in column 5 on fixed effects for initial occupation (in column 5: 321 categories) and initial industry (column 6: 229 categories), in period $t-1$: reassuringly, this makes little difference to the results. In column 7, we then condition on initial job tenure (fixed effects for each single-year category). As Manning (2003) notes, initial tenure may be a “bad control”: longer tenure is expected to be an *outcome* of higher pay (and therefore part of the effect we wish to estimate). Unsurprisingly then, the separation elasticity (in the top row) becomes smaller in this specification; but despite this, the native-migrant differentials (i.e. the interaction effects) are largely preserved.