The effect of low-skill immigration restrictions on US firms and workers: Evidence from a randomized lottery

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Abstract

The U.S. limits work visas for low-skill jobs outside of agriculture, with a binding quota that firms access via a randomized lottery. We evaluate the marginal impact of the quota on firms entering the 2021 H-2B visa lottery using a novel survey and pre-analysis plan. Firms exogenously authorized to employ more immigrants significantly increase production (elasticity +0.16) with no decrease or an increase in U.S. employment (elasticity +0.10, statistically imprecise) across several pre-registered subsamples. The results imply very low substitutability of native for foreign labor in the policy-relevant occupations. Forensic analysis suggests similarly low substitutability of black-market labor.

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1 Introduction

The effect of immigration on natives hinges on how the economy adjusts. Researchers and policymakers mostly agree that immigration for high-skill work—requiring higher education and specialized skill—ultimately causes adjustment with net benefits to natives. There is less consensus about immigration for low-skill work (Dustmann et al. 2016a; Blau et al. 2017, 267; Blau and Hunt 2019, 174; Edo et al. 2020). Despite the great economic and political importance of restrictions on low-skill immigration, estimates of their effects range widely depending on the assumptions used to approximate causal identification (Card 1990; Borjas 2003; Ottaviano and Peri 2012; Dustmann et al. 2016b).

Here we study the economic effects of a large-scale experiment in the United States: nationwide, firm-level, natural randomization of restrictions on the employment of immigrants for low-skill jobs. The United States has one principal work visa for low-skill labor in the nonfarm economy—the H-2B visa. U.S. employers' access to that visa is limited by a quota and allocated in part via a randomized lottery conducted by the federal government. This exogenous variation in restrictions on immigrant employment allows unusually transparent, policy-relevant estimates of how U.S. firms and workers adjust. After publicly committing to our hypothesis tests and predicted treatment effects with a pre-analysis plan, we collected data from both winners and losers of the 2021 H-2B visa lottery in a novel firm survey. This allows prespecified tests of basic theoretical predictions about the magnitude and hetergeneity of the effect of low-skill immigration restrictions. It furthermore allows estimation of the firm-level, immigrant-native "combined" elasticity of substitution (Hicks 1936).

We find that exogenous permission to employ immigrants for low-skill work causes the marginal firm to expand production. Put differently, exogenous *restrictions* on employing the profit-maximizing number of immigrants for low-skill work cause the marginal firm to contract. These restrictions cause a large and statistically significant decrease in revenue and investment. The restrictions cause no increase, or a decrease, in the employment of low-skill native workers and the rate of profit. Losing the lottery reduces firms' employment of low-skill immigrants by 56%. This decrease causes firms to contract, reducing operations with an elasticity of +0.164 for revenue and +1.03 for investment (statistically distinguishable from zero at conventional levels),

and with an elasticity of +0.102 for low-skill U.S. employment, and +0.100 for the profit rate (statistically indistinguishable from zero at conventional levels).

This evidence is consistent with a substantial negative effect of low-skill immigration restrictions on economic activity inside and outside treated firms. It is consistent with immigrant employment in low-skill jobs crowding-in native employment in low-skill jobs to a minor degree, but also consistent with null effects of immigrant employment on native employment. The U.S. employment effects imply that low-skill immigrant workers at the policy-marginal firm are very poor substitutes for low-skill U.S. workers. We estimate the firm-level, low-skill foreign-native effective elasticity of substitution in the range 0.8–2.1, somewhat lower than previous estimates.

These effects are consistent with a simple model of a monopolistically-competitive firm in which the immigrant-native elasticity of substitution is low relative to the price elasticity of output demand. The same model predicts that the treatment effect on revenue should be greater for firms that are small relative to the output market—and thus face greater competition—and the treatment effect on U.S. employment should be larger in rural areas where complementary native workers have less attractive alternatives. These predictions motivated our prespecified tests for heterogeneous effects in both dimensions. We confirm the sign of our prespecified predictions: The magnitude of the treatment effect on revenue more than doubles for small firms facing high competition in the output market, and the treatment effect on U.S. low-skill employment is greater (and becomes statistically significant at conventional levels) in rural areas.

The treatment effects we measure are robust to several prespecified changes, including alternative definitions of the instrumental variable, control for the familywise error rate, and tests for global and item nonresponse. They are likewise robust to a range of changes that were not prespecified, including treatment of zeros in the data, sensitivity to influential observations, and randomization inference. Most notably, we test the validity of the core analysis from 2021 by partially replicating it using data from the similar, independent lottery of 2020—with quantitatively similar results. Finally, we forensically test for and fail to find evidence of large unobserved changes in black-market employment, given that *observed* treatment effects on revenue are almost fully accounted for by *observed* effects on foreign and native employment. This corroborates prior evidence, both observational (Hotchkiss et al. 2015) and quasi-experimental (Zhu et

al. 2020), that black-market foreign workers are very poor substitutes for other workers in basic low-skill jobs within U.S. firms.

This research contributes to the literature in three ways. First, it contributes to work on the economic effects of low-skill immigration by exploring its effects at the firm level. Within-firm adjustment is a core driver of overall economic adjustment to immigration (Card and Lewis 2007; Lewis and Peri 2015; Dustmann et al. 2015; Peri 2016) Thus a growing literature seeks to estimate the effects of immigration restrictions at the firm level. Recent work in this area has focused on *high-skill* immigration, especially its effects on innovation and entrepreneurship (Kerr and Lincoln 2010; Hunt and Gauthier-Loiselle 2010; Hunt 2011; Hornung 2014; Kerr et al. 2015; Mayda et al. 2018; Bound et al. 2017; Mayda et al. 2020; Bahar et al. 2020; Khanna and Lee 2019; Glennon 2020; Glennon et al. 2021; Raux 2021; Azoulay et al. 2022). This work contributes complementary tests for low-skill immigration.

Traditional approaches that rely on variation in immigration not across firms but across aggregates—by skill-cell, geographic area, or both—rest on axiomatically ruling out specialization across firms within the aggregates (Card 2009, 2). Results from this approach can be highly sensitive to the definition of the aggregates, embodying assumptions about within- and cross-cell substitution (Boustan 2009; Dustmann et al. 2016a, 33). Firm-level studies can clarify the mechanism of adjustment to immigration, such as the relative importance of shifts in production techniques within firms and shifts in the size distribution of firms (Dustmann and Glitz 2015; Foged and Peri 2016).

A second contribution of this work lies in its transparent causal identification. The variation in exposure to immigrant employment that we study is exogenous by design. This is desirable relative to what is by far the most common approach to causal identification in the literature on low-skill immigration: constructing 'shift-share' instrumental variables based on lagged patterns of immigrant presence across geographic areas (Card 1990; Altonji and Card 1991; Burchardi et al. 2018; Monras 2020; Piyapromdee 2020; Kim et al. 2022) or across firms (Lewis 2011; Olney 2013; Dustmann and Glitz 2015; Mitaritonna et al. 2017; Burstein et al. 2020; Gray et al. 2020; Imbert et al. 2022; Mahajan 2022)—either alone or in combination with shocks at the migrant origin. One limitation of this approach is well recognized: Some of the same unobserved traits of geographic areas that attracted immigrants in the past can persist, producing confounding variation in the

outcome of interest at present. This complicates the internal validity and interpretation of such studies (Jaeger et al. 2018; Adão et al. 2019; Goldsmith-Pinkham et al. 2020; Borusyak et al. 2021). Our identification strategy contributes more generally to the literature on how firms adjust to shocks, such as shocks to local input costs (Baqaee and Farhi 2019; Bilbiie and Melitz 2021; Butters et al. 2022; Guerrieri et al. 2022; Kumar et al. 2022), offering a rare setting in which large shocks are randomized across firms.

A third contribution lies in the fact that the variation in exposure to immigrant employment that we study is driven directly by a change in *policy*, unlike in standard 'shift-share' studies. This limitation of the standard research design is less recognized. The Local Average Treatment Effect (LATE) estimated from instrumental variables based on migrants' social networks, even when it is internally valid, may be substantially biased as an estimate of the Policy-Relevant Treatment Effect (PRTE; Heckman and Vytlacil 2001; Heckman and Vytlacil 2005; Carneiro et al. 2011). Intuitively, the LATE of varying the supply of immigrants regardless of current demand for their labor—as the standard instrumentation strategy does by construction—need not equal the LATE from policy-induced restrictions on current, realized demand for their labor. More credible estimates of the PRTE arise from exogenous variation in policy itself. A small, recent literature has sought causal identification not from lagged settlement patterns or overseas shocks but from sudden changes to immigration policy restrictions, primarily affecting either high-skill (Beerli et al. 2021) or low-skill immigrant labor (Dustmann et al. 2016b; Clemens et al. 2018; Ayromloo et al. 2020; Ifft and Jodlowski 2022; Luo and Kostandini 2022; San 2022; East et al. 2022; Abramitzky et al. 2022).

An important strand of this research uses natural randomization of policy restrictions to transparently identify the causal effect of the supply of immigrant workers. But none of these exploit firm-level randomization of labor for low-skill jobs. When these studies consider low-skill immigration, some rely on randomized refugee placement across geographic areas (Glitz 2012; Couttenier et al. 2019; Olney and Pozzoli 2021). Others exploit randomized visa allocation across individuals to study the effects of migration *on migrants* and their families (Gibson et al. 2011; Mergo 2016; Mobarak et al. 2020; Buechel et al. 2021). The handful of studies exploiting randomized supply of immigrants across firms focus exclusively on high-skill workers (Clemens 2013;

Doran et al. 2022; Dimmock et al. 2022).[1]

The paper proceeds as follows. In Section 2 we build a basic model of the effect of restrictions on hiring low-skill immigrants affects a monopolistically competitive firm. We describe the United States' low-skill nonfarm work visa in Section 3 and explain the process of firm-level randomization in Section 4. Section 5 describes the Pre-Analysis Plan and novel firm survey. The core results for 2021 are presented in Section 6, with robustness checks including the partial replication for 2020 in Section 7. Section 8 then discusses several issues of interpreting the treatment effects: prespecified tests for heterogeneous effects predicted by theory, estimating the foreign-native elasticity of substitution, aggregation of firm-level impacts, and forensic tests for bias from black-market employment. A final Section 9 concludes.

2 Firm-level effects of low-skill immigration policy

We begin with a straightforward theory of firm-level production. This allows us to structurally interpret the observed reduced-form effects of an exogenous change in low-skill employment. The theory predicts positive causal effects of low-skill immigrant employment on firm revenue. It predicts crowding out of low-skill native workers by immigrants when the the immigrant-native elasticity of substitution in production is sufficiently high relative to the price elasticity of output demand—and crowding in of low-skill natives otherwise.[2] It likewise predicts positive effects on investment and absolute profits, but not necessarily the rate of profit.

Consider a firm in monopolistic competition that maximizes profits as it produces output by combining low-skill immigrant labor (I), low-skill native labor (N), and capital (K) in a homogeneous production technology. It also has positive fixed costs ($\mathcal{F}$) of operation, which includes permanent employment, H, as well as fees associated with hiring immigrants.[3] The firm is a

[1] A limitation of that work is that in the United States, natural randomization of high-skill work visa petitions occurs at the level of the individual foreign worker, not at the level of the firm. This can only produce substantial random variation in the immigrant share of employment across *small* firms with few petitions; the more petitions a firm files, the more likely it is to receive a uniform, fixed share of those workers (Peri et al. 2015). In the low-skill visa we study, randomization occurs not at the individual level but at the firm level (with nuances discussed below).

[2] In the related model of Burstein et al. (2020), crowding out of native workers occurs only in nontradable activities where the price elasticity of output demand would be lower than in tradable activities.

[3] Because we impose that permanent employment does not respond to short-term variation in seasonal employment below, we treat it as a fixed cost. We will comment further on this below.

price taker in the market for inputs (at factor prices w_I, w_N and r, respectively), but faces a downward-sloping demand for its product

$$Q(p) = Dp^{-\eta}, \tag{1}$$

where Q is output p is price, $\eta > 1$ is the demand elasticity and D is a constant.

The firm pays a fee to enter a lottery to become authorized to freely hire immigrant workers. If they "win" the lottery, they hire the profit-maximizing quantity of immigrant labor at the wage w_I. If not, they may be authorized to hire up to $\bar{I}$ workers at this wage.

2.1 Effects on revenue

The first result from this setup is that relaxing the hiring constraint on low-skill immigrants unambiguously increases the scale of the firm. Intuitively, relaxing a constraint must weakly increase the firm's profits; the Appendix offers a proof. Output and revenue must also rise as a consequence of homogeneity. Winning the lottery will not necessarily increase profit *rates*, however, because while immigration-induced scale increases will help defray a firm's fixed costs, adding immigrants will also undermine the revenue product of other immigrant workers, leading to the ambiguous result. That is,

> **Proposition 1.** *Greater immigrant employment (weakly) causes higher output, revenue, and profit. The magnitude of the effect on revenue, in proportional terms, is increasing in the firm's output demand elasticity η. The sign of the impact on profit rates (profits/revenue) is indeterminate.*

Additional notation can help illustrate why winning the lottery must cause revenue to rise. Let I_w represent the number of immigrant hires the firm makes when unconstrained—"winning" the lottery—and $I_\ell \leq \bar{I}$ when losing. Use analogous notation for capital (K_w and K_ℓ) and low-skill native-born employment (N_w and N_ℓ). The impact of winning can be linearly approximated as

$$\ln \frac{R_w}{R_\ell} \approx s_I \ln \frac{I_w}{I_\ell} + s_N \ln \frac{N_w}{N_\ell} + s_K \ln \frac{K_w}{K_\ell}, \tag{2}$$

where R_w and R_ℓ are revenues without and with the constraint, respectively, and s_I, s_N, and s_K are immigrant labor, native labor, and capital's share in revenue, respectively. The partial effect of increasing immigrant labor on revenues is thus positive. While the adjustment of other factor inputs that may substitute for I can lessen this effect, the total effect is always (weakly) positive.

We can further derive expressions for the adjustments of other inputs that will allow us to solve for $\ln \frac{R_w}{R_\ell}$ as a function of $\ln \frac{I_w}{I_\ell}$ alone. For this, we will use a more concrete example of the sort of production function typically used in the immigration literature, namely a nested constant elasticity of substitution (CES) form (e.g. Ottaviano and Peri 2012). Let

$$Q = zH^\gamma K^\beta \left(\alpha I^{\frac{\sigma-1}{\sigma}} + (1-\alpha)N^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}(1-\beta-\gamma)}, \tag{3}$$

where $\sigma > 1$ is the elasticity of substitution between immigrant and native low-skill labor, H is high-skill native labor, α, β, and γ are share parameters, and z is a productivity shifter. For initial intuition, it is useful to consider a further simplified version of (3) that does not include capital or high-skill labor (imposing $\beta = \gamma = 0$), which implies

$$\ln \frac{R_w}{R_\ell} \approx s_I \cdot \frac{\eta - 1}{(\eta-1)(1-s_N) + (\sigma-1)s_N} \cdot \ln \frac{I_w}{I_\ell}. \tag{4}$$

Intuitively, firms facing a higher price elasticity of output η can expand production more in response to a positive immigrant labor shock without causing a large fall in the output price.

2.2 Effects on investment

The more general expression for the revenue effect of immigration following from (3)—allowing for arbitrary β and γ—is better understood after first seeing how other inputs adjust. First, and most simply, under equation (3) capital's share of revenue is fixed at $\beta\frac{\eta-1}{\eta}$, which means it responds (in proportional terms) exactly as revenues do to winning the lottery:

> **Lemma 1.** *Under equation (3), greater immigrant employment causes greater capital stock. The magnitude of this effect is increasing in the firm's output demand elasticity η.*

That capital increases with labor is not a surprise under these assumptions, as capital occupies

a fixed share of revenue, which rises with immigrant hires under Proposition 1. Another implication advances us toward finding the response of revenue after the adjustment of all factors. Capital's fixed share implies revenue and capital grow *pari passu*. Thus we can shorten the revenue expression (2) by utilizing the fact that capital's response proportionately increases the response of revenue to other inputs,[4] yielding

$$\ln \frac{R_w}{R_\ell} \approx \frac{s_I}{1 - s_K} \ln \frac{I_w}{I_\ell} + \frac{s_N}{1 - s_K} \ln \frac{N_w}{N_\ell}. \tag{5}$$

In the special case of little change in native employment, for example, revenue's elasticity to immigrant hires is simply $\frac{s_I}{1-s_K}$. While this is a potentially useful simplification, it would not hold under more general production setups than (3) in which capital instead substitutes for low-skill labor. This has has been found in manufacturing and agriculture (e.g. Lewis 2011; Hornbeck and Naidu 2014; Clemens et al. 2018; Lafortune et al. 2019; Coluccia and Spadavecchia 2021), where such substitution helps account for a smaller-than-expected labor market impact of immigration. If this alternative specification applies here as well, capital stocks would instead *fall* in response to additional immigrant employment. The true effect is an empirical question.

2.3 Effects on native employment

The response of native-born employment is not *a priori* obvious either. A conventional story is that firms will prefer to hire "cheap" immigrant labor and displace natives. However, this story ignores the scale response in Proposition 1. Depending on how substitutable immigrants are for natives, relative to this scale response, restrictions on employing immigrants may either raise or lower the employment of natives (Friedberg and Hunt 1995).[5] This gives:

> **Proposition 2.** *The effect of greater immigrant employment on native employment has indeterminate sign.*

To see this, again consider an intuitive version of (3) that ignores other inputs, before turning to the more general version. Under (3), but imposing $\gamma = \beta = 0$, the native employment response

[4] Substitute $\ln \frac{K_w}{K_\ell} = \ln \frac{R_w}{R_\ell}$ into equation (2) and rearrange.

[5] Immigrant wages are fixed by regulation in the empirical setting we study below.

to an exogenous increase in immigrant employment is given by:

$$\ln \frac{N_w}{N_\ell} \approx s_I \cdot \frac{\eta - \sigma}{(\eta - 1)(1 - s_N) + (\sigma - 1)s_N} \cdot \ln \frac{I_w}{I_\ell}, \tag{6}$$

where the long expression in the denominator is necessarily positive. That is, without allowing for the adjustment of capital, the effect of low-skill immigrant employment on low-skill native employment depends positively on η and negatively on σ.

We can then increase the realism of the model by bringing back capital and high-skill labor, allowing $\beta, \gamma > 0$.[6] Under (3) with fixed H, the native employment response to greater immigrant employment is given by a refined version of equation (6),

$$\ln \frac{N_w}{N_\ell} \approx s_I \cdot \frac{(\eta - 1)(1 - \beta - \gamma\sigma) - (\sigma - 1)}{\Theta} \cdot \ln \frac{I_w}{I_\ell} \tag{7}$$

where $\Theta > 0$.[7] This implies a necessary and sufficient condition for immigrant employment to crowd in native employment:

$$\frac{\ln(N_w/N_\ell)}{\ln(I_w/I_\ell)} > 0 \iff \frac{\eta - 1}{\sigma - 1} > \frac{1}{1 - \beta - \gamma\sigma}. \tag{8}$$

We then have:

> **Lemma 2.** *The amount by which the lottery increases native employment is rising in the output demand elasticity η and falling in the immigrant-native elasticity of substitution σ. If η is sufficiently high (low) relative to σ, immigrant employment crowds in (out) native employment.*

We can now derive the revenue effect of immigrant employment in the general case, with capital

[6] We will impose that high-skill permanent employment H is unaffected by a shock to immigrant employment, which is realistic in the short-term setting we study. We do not expect (and do not observe) adjustment of high-skill permanent employment on such a short timescale. This has implications for external validity: permanent employment would be expected to respond to increases in the number of H-2B visas available (or the chance of being authorized to use them), likely producing response that is larger than what we will obtain from the impact of winning a single lottery under fixed conditions.

[7] The ungainly but strictly positive quantity $\Theta \equiv \big((1 - \beta - \gamma)[(\eta - 1)(1 - s_N) + (\sigma - 1)s_N] + (\beta + \gamma)s_N\eta(\sigma - 1)\big) \times (1 - s_K) - (\eta - 1)(1 - \beta - \gamma)\sigma s_K s_N > 0$, proven in the Appendix.

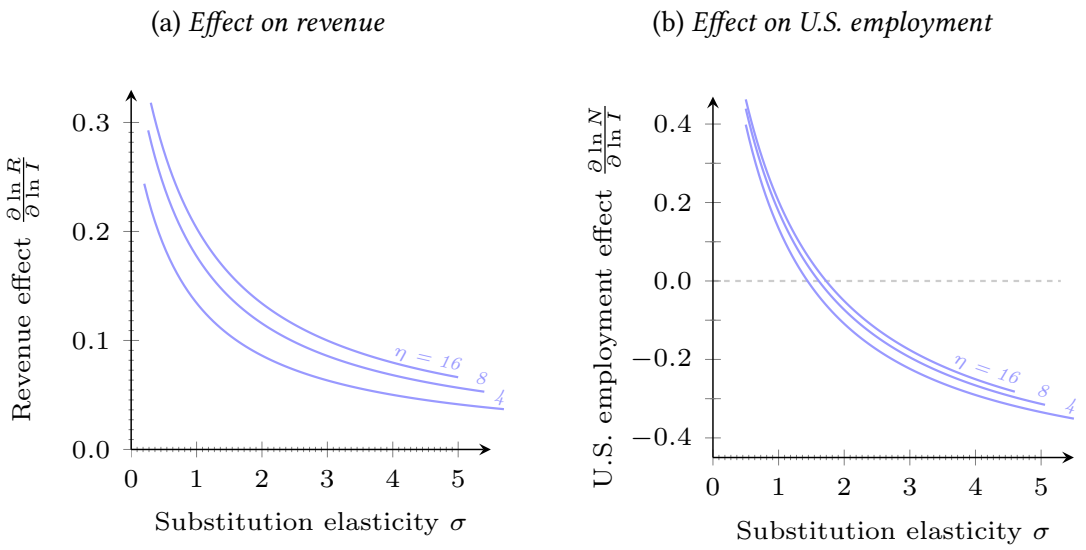
adjustment. Substituting (7) into (5) gives the response of revenues:

$$\ln \frac{R_w}{R_\ell} \approx \left[\frac{s_I}{1-s_K} + \frac{s_N}{1-s_K} s_I \left(\frac{(\eta-1)(1-\beta-\gamma\sigma)-(\sigma-1)}{\Theta} \right) \right] \cdot \ln \frac{I_w}{I_\ell}. \tag{9}$$

The first term in square braces is the direct effect of adding immigrant labor (and capital), always positive. The second term in square braces is the indirect effect working through induced changes in native employment, positive or negative according to condition (8).

A graph of the revenue effect (9) is presented in Figure 1a, and the native employment effect (7) in Figure 1b, using parameter values from the empirical analysis to follow. These confirm the key results above: The revenue effect of immigrant employment is nonnegative (Figure 1a). Both revenue and native employment responses are falling in the substitution elasticity and rising in the output demand elasticity (Figures 1a and 1b). And there is a cutoff value of the substitution elasticity, relative to the output demand elasticity: Immigrants displace natives above that cutoff (8), and crowd in natives below it (Figure 1b).

Figure 1: Effects of immigrant employment on revenue and U.S. employment in theory



Uses empirical estimates of other model parameters from the core firm sample: $\beta = 0.35$, $\gamma = 0.349$, and native share of inner labor nest 0.648. Details in Appendix.

These findings are related to, but not the same as the Marshall-Hicks laws of derived demand (Marshall 1890, 434; Hicks 1932, 242–244). Marshall's laws describe the sensitivity of *own* labor demand elasticities to product demand (second law) and substitution (first law) elasticities. The impact of removing hiring restrictions on immigrant labor on demand for native labor is instead more closely related to a cross-elasticity of native demand with respect to immigrant wages. Nevertheless, the results are analogous.[8]

3 The United States' low-skill, nonfarm work visa program

We estimate the model using natural, firm-level randomization of access to the principal work visa for low-skill labor in the U.S. nonfarm economy, the "H-2B" visa. It is the only employment-based visa available to foreign workers without a college education working outside agriculture, with immaterial exceptions.[9] 98% of all H-2B jobs do not even require a high school education; the mean months of experience required by employers is 1.2.[10] Haaland and Roth (2020) use political support for expanding the H-2B visa as a proxy measure of support for low-skill immigration. This visa has undergone relatively little study despite its importance. Observational data reval that U.S. county-years where employers petition for more H-2B workers do not exhibit higher unemployment or lower wages on average for natives in related low-skill service occupations (Amuedo-Dorantes et al. 2021), but a strictly causal interpretation of these patterns

[8] A more detailed examination of how these theoretical results vary with more general demand structures than (1) is beyond the scope of this paper, as we lack empirical counterparts for them. In the empirical sections below, we will employ various proxies for the demand elasticity. If the demand elasticity itself is falling in scale (relative to the market) as asserted in the second Marshall-Hicks Law of Derived Demand, then the response of both revenues and employment to winning the lottery may be smaller at larger firms.

[9] Essentially all employment-based immigrant visas require a college degree. An immaterial exception is the EW3 subclass of the third-preference employment-based green card, for newly-arriving "needed unskilled workers", totaling 818 in fiscal year 2019 and 744 in fiscal year 2020 (Dept. of Homeland Security, *Yearbook of Immigration Statistics* 2019:22 and 2022:21). Likewise, almost all *nonimmigrant* visas for temporary work in the nonfarm economy require a college education, such as the H-1B visa. A few subcategories of nonimmigrant worker in small niches of the nonfarm economy—such as au pairs under the J-1 'cultural exchange' visa and the L-1A 'intracompany transfer' visa—do not formally require a college education, but the overwhelming majority are given to workers with a college education. These visas thus increase the supply of high-skill relative to low-skill labor in the United States. The Diversity Visa is an immigrant visa that does not require family sponsorship and is available to workers with a high school education only, but 1) it is not an employment-based visa, since no firm is required to express demand for the visa recipient, and 2) 50% of adult Diversity Visa recipients have a bachelor's degree or higher, well above the U.S. native fraction of 32% (Gelatt 2018). Moreover roughly one quarter of Diversity Visa recipients are children, and many go on to complete a college education after arrival (Imoagene 2017). This implies that the Diversity Visa, too, reduces the supply of low-skill workers relative to high-skill workers in the United States.

[10] Dept. of Labor classification of certified positions in FY2021 disclosure data.

is difficult.

The legal origin of the visa is the Immigration and Nationality Act of 1952. It created a low-skill nonimmigrant work visa for both farm and nonfarm work—named 'H-2' after the Act's relevant paragraph (66 Stat 168 § 101(a)(15)(H)(ii)). A separate 'H-2B' visa for *nonfarm* low-skill work was created by the Immigration Reform and Control Act of 1986 (Pickral 2007). An H-2B worker is defined by law as "having a residence in a foreign country which he has no intention of abandoning who is coming temporarily to the United States to perform . . . temporary [non-agricultural] service or labor if unemployed persons capable of performing such service or labor cannot be found in this country." Wages for H-2B workers are fixed by the federal government at the prevailing wage, "the mean wage for the occupation in the pertinent geographic area derived from the Bureau of Labor Statistics Occupational Employment Statistics survey".[11]

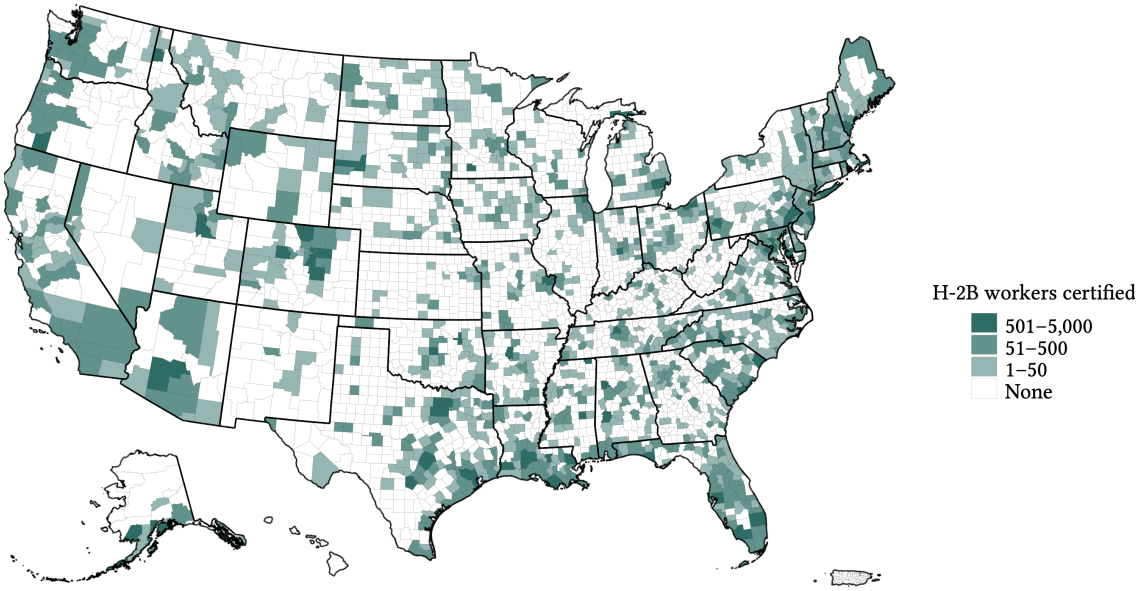
Foreign workers received an average of 84,383 H-2B visas per year during the five fiscal years ending in 2021. 87% are male.[12] The leading industries employing H-2B workers are Administrative and Support Services (especially groundskeeping/landscaping); Hospitality (including restaurants); Arts, Entertainment, and Recreation; Forestry, Fishing and Hunting; Construction; and Food, Beverage, Textile, and Apparel Manufacturing (Barnes 2020, 12). These roughly correspond to the low-skill service industries with the highest prevalence of immigrant workers in the United States, led by landscaping (Cortés 2008, 387). The large majority of workers are citizens of Mexico (75% in FY2021); most of the rest are citizens of Jamaica, Guatemala, Ukraine, Honduras, Serbia, the Philippines, and El Salvador.

Employers can in principle employ any given H-2B worker indefinitely provided that they apply successfully to extend the visa once a year, and that the worker applies to renew the visa once every three years by departing the United States for three months. But in practice the Department of Labor is unlikely to approve long-term rehiring as satisfying a temporary need. H-2B jobs must offer full-time employment, defined as at least 35 hours per week and at least 75% of the workdays in each 12-week period. Workers' spouses and minor children can accompany

[11] "Wage Methodology for the Temporary Non-Agricultural Employment H-2B Program", *Federal Register* 80 FR 24145.

[12] In fiscal 2019, DHS reports 129,120 entries (I-94 only) on H-2B visas, of which 16,620 women: DHS, "Nonimmigrant Admissions by Selected Classes of Admission and Sex and Age", FY2019 data the most recent available when updated July 6, 2022.

Figure 2: Worksites of H-2B visa petitions by U.S. county, 2021 (universe)



Full universe of cap-subject petitions certified by the Dept. of Labor, fiscal year 2021. County of worksite, not necessarily of the petitioning firm. There were petitions from all 50 states and Puerto Rico. (The three from Hawaii were not certified.)

them into the country, but may not work (and do not count against the visa cap). The migrants' worksites are widely distributed across the country, in 49 of the 50 states and spanning both rural and urban areas (Figure 2).

To hire an H-2B worker, employers must successfully petition two federal government agencies, in order: the Department or Labor (DOL), and the Department of Homeland Security (DHS). DOL must certify that the H-2B job complies with labor law; DHS must authorize issuance of a work visa. For each employer's petition, DOL certifies that hiring the foreign worker will not adversely affect the wages or employment of U.S. workers, and that the hiring need is 'intermittent', 'peak load', 'one-time occurrence', or 'seasonal'.[13] On the average petition in FY2021, 88.1% of requested workers were certified. For DOL-certified workers, employers must then petition DHS, which decides whether there are sufficient visas for the petition and whether anything disqualifies each worker from receiving a visa. A firm hiring a group of workers to provide the

[13]This roughly four-month administrative process is detailed by Bruno (2018), Barnes (2020) and Bier (2021).

same service at the same location can list up to 25 workers on the same petition.[14]

This regulatory process was created to address lawmakers' enduring suspicions of negative labor-market effects from low-skill work visas. Between 1885 and 1952, the contract hiring of low-skill foreign workers banned outright by the Foran Act,[15] because hiring of this type was considered harmful to the employment prospects of low-skill U.S. workers (Orth 1907). The same 1952 law that reversed this ban, creating the H-2 visa, required DOL to certify that there were insufficient U.S. workers "able, willing, and qualified" to perform each individual job for which a foreign worker was to be contracted (Wasem 2003).

The efficacy of that certification process has been frequently questioned since then, notably by the influential Hesburgh (1981, 226) Commission. Its recommendations culminated in the 1990 law tightly restricting all visas based on low-skill, nonfarm employment—capping the H-2B visa (Schuck 1992, 53; Chishti and Yale-Loehr 2016) as well as all but eliminating employment-based green cards for low-skill work (Aragones 1991, 125; Adler and Jarrett 1992, 791). Whether or not those restrictions achieved their explicit objective—to raise employment for U.S. workers relative to the counterfactual—does not appear to have undergone systematic empirical tests.

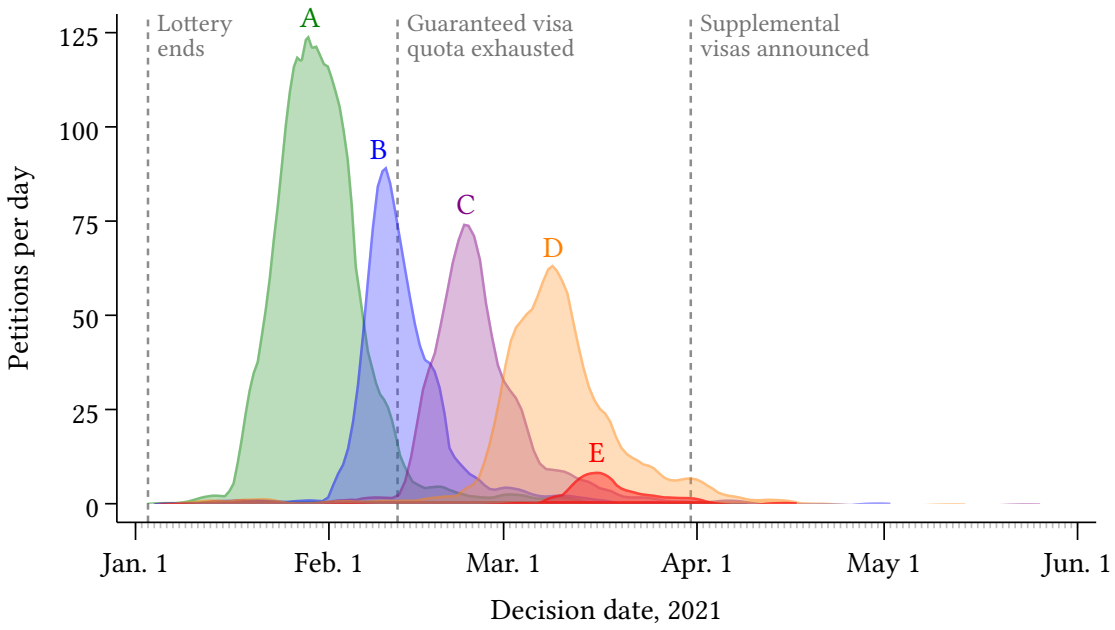
4 Firm-level randomization of low-skill immigrant employment

Two key features of the institutional process for H-2B visa allocation create the natural experiment that we study.

First, the H-2B visa is subject to a statutory cap of 66,000 per year, comprising 33,000 for the first half of each fiscal year (October–March) and 33,000 for the second half (April–September). This cap was written into law by the Immigration Act of 1990, and remains in force (8 USC § 1184(g)(1)(B)). The cap was set without any quantitative empirical evidence of its effects on the U.S. labor market. The writers of the law set the annual cap arbitrarily at triple the number of visas being used at the time (Leibowitz 1991, 313), because it was foreseen that demand for H-2B visas would rise (GAO 1992, 73), and a high cap would allow ample room for reasonably

[14] 8 CFR 214.2(h)(2)(ii).
[15] 23 Stat. 332, 48th Congress, Sess. II, Chap. 164.

Figure 3: Foreign worker petition decision dates by lottery result, universe of firms, second half of fiscal year 2021



The unit of observation is petitions. Shown is the universe of firms entering the lottery for work to be performed in the second half of the fiscal year (April 1–September 30). Epanechnikov kernel densities, bandwidth 2 days. 'Decision date' is the date of the Department of Labor's decision on whether or not to certify each petition, a necessary condition of proceeding to petition USCIS for a visa. The lottery was conducted for DOL petitions received January 1–2, 2021. The statutory quota of 33,000 guaranteed visas for the second half of the fiscal year was reached on Feb. 12, 2021.

foreseeable demand.[16] But as years went on, demand came to vastly exceed the statutory cap, due primarily to changing economic conditions, especially low unemployment (Orrenius and Zavodny 2020). By 2022 the statutory cap was oversubscribed by a factor of four: For the 33,000 visas in the statutory quota for the second half of FY2022, employers petitioned DOL for 136,555 workers.[17]

Second, a naturally randomized lottery constrains firms' access to H-2B visas under the statu-

[16] Personal communication with Bruce Morrison, former Chair of the House Immigration Subcommittee and a principal author of the Act, July 25, 2022. During the Congressional reconciliation process in mid-1990 that led to the final Immigration Act, the most recent available *Statistical Yearbook* of the Immigration and Naturalization Service was the 1988 edition, which reported the latest annual number of H-2B nonimmigrants admitted as 22,115.

[17] *Federal Register* May 18, 2022, 87 *FR* 30334. The splitting of the 66,000 annual cap into two half-year caps of 33,000 occurred after a legal reform in 2005 (Bruno 2018). Note that the visa quota very tightly binds not just demand but supply: There is little constraint on labor supply given that H-2B jobs commonly offer migrant workers over 1,000% of their home-country reservation wage (Brodbeck et al. 2018).

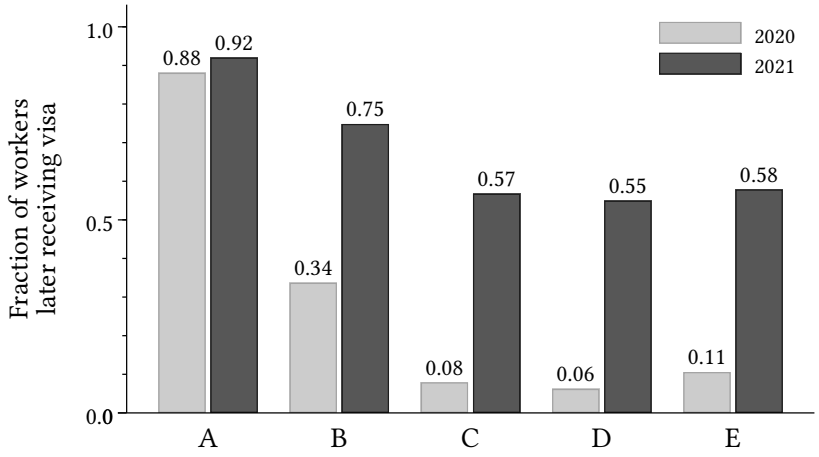
tory cap. Because DOL certification is required before firms can petition DHS for a visa, and because the demand for visas at DHS greatly exceeds the supply, firms' ability to obtain a visa at DHS is highly dependent on how quickly they can complete processing at DOL. Knowing this, and to ensure equitable access to visas across firms, DOL begins processing firms' petitions in randomized order. It began doing this after an unprecedented number of petitions were received for the second half of fiscal year 2019, causing the DOL server to crash and making it impossible to determine the order in which petitions had been filed.[18] DOL randomly assigns each petition one of five letters, *A* through *E*. It begins processing the *A* petitions first, and starts the *B* petitions when staff become available but there are no new *A* petitions left to begin. It then proceeds to the *C*, *D*, and finally *E* petitions in order (Figure 3). Petitions receiving an *A* result are highly likely to emerge from DOL processing before the visa cap is reached; petitions with all other results are not. The result is that there is a large random component to the order in which firms get past the required DOL administrative step, and thus their ability to petition DHS for visas before the statutory quota of visas is exhausted.

In this natural experiment, we consider 'treatment' as each U.S. firm's employment of low-skill immigrant workers on H-2B visas. Randomization into treatment at the firm level is continuous and fuzzy. It is continuous because randomization is at the petition level, not *necessarily* the firm level. For most firms, this does equate to randomization at the firm level, because the large majority of firms file a single petition for a group of workers (median 11 workers per petition). But groups of workers performing different occupations at different worksites can be requested on multiple separate petitions by the same firm. Since larger firms are more likely to file multiple petitions, we measure treatment by the *fraction* of workers petitioned for by each firm—across one or multiple petitions—that receive timely DOL certification. That fraction is randomly assigned at the firm level. Randomization occurs across the universe of H-2B employers nationwide, obviating site selection bias (Allcott 2015).

And treatment is fuzzy because there are ways for some firms to hire H-2B workers regardless of their DOL lottery result—that is, there are some 'always-taker' firms (Angrist et al. 1996). First, the workers on a petition are exempt from the visa cap if they are already present in the United States (12.7% of workers in the lottery). For them the randomized timing of DOL

[18] *Federal Register*, March 4, 2019, 84 *FR* 7403.

Figure 4: Worker petition success rates by lottery letter



The unit of observation is petitions, in the sampling universe. The vertical axis shows the number of workers for whom the visa approval process was successfully completed at DHS, in the average petition in each lottery-group and year, as a fraction of the number of workers for whom the each firm originally petitioned DOL.

processing does not affect their access to visas. Second, firms that are "capped out"—that is, firms that receive DOL certification after the 33,000 visa quota for that semester is exhausted—can sometimes obtain an H-2B visa from a "supplemental" visa allocation created in the middle of the relevant semester. By the time such supplemental allocations are announced, almost all firms have completed DOL processing, so their access to any supplemental visas is not legally restricted by the DOL lottery. But the lottery result nevertheless strongly influences firms' H-2B hiring. This is because 1) it is ex ante uncertain whether a supplemental allocation will occur at all, and if there is one, 2) it is ex ante uncertain how large any supplemental allocation will be, but 3) supplemental allocations are generally far lower than employer demand.[19]

[19]DHS has discretion under law to approve supplemental H-2B visas. It interprets this legal authority to allow issuance of a maximum of 64,716 supplemental visas per year, to reflect "the needs of American business" (*Federal Register*, May 25, 2021, 86 FR 28205). But in practice, the number of supplemental visas approved is far less than the maximum allowed and far less than the number requested by employers—if any supplemental visas are approved at all. For the second half of fiscal year 2019, DOL received petitions for over 96,400 workers under the 33,000 visa cap (Apr. 1–Sep. 30), and in late May began approving a supplemental allotment of 30,000 additional visas (*Federal Register*, May 8, 2019, 84 FR 20005). In the second half of fiscal year 2020, DOL received petitions for 87,298 workers under the 33,000 cap, and approved *no* supplemental visas. In the second half of fiscal year 2021, DOL received petitions for 96,641 workers under the 33,000 cap, and in early June began approving 22,000 supplemental visas (*Federal Register*, May 25, 2021, 86 FR 28205). By law, any visas unused from the 33,000 quota for the *first* half of the fiscal year can be made available as additional visas in the second half of the year, but the first-half quota was exhausted in all recent years (2019, 2020, and 2021). A few additional, minor classes of H-2B employers are exempt from the visa cap, such as fish roe processors and most citizens of Canada (Geer 2021).

Thus firms receiving any lottery result other than *A* on their petition(s) understand in January that there is a high probability they will be unable to hire new H-2B workers during April–September, despite the possibility of a supplemental visa allotment. They plan production for that year accordingly. This is seen in the rates of DHS processing completion by DOL lottery letter (Figure 4). In the second half of fiscal year 2020, when no supplemental visas were approved by DHS, employers whose petition(s) received poor lottery result were unlikely to access H-2B visas at all. Those they could hire were generally 'cap-exempt' workers already present in the first half of the year. In 2021, thanks to the supplemental visas, employers with a poor lottery result were able to hire roughly half of the workers they demanded, in spite of the supplemental visas. There are also a small number of workers for whom firms *could* petition DHS due to their lottery result *A* from DOL, but they choose not to as business plans evolve—that is, there is a minor number of 'never-takers' (8% in 2021).

5 Novel survey of U.S. firms and Pre-Analysis Plan

We gathered data on firm outcomes in the second half of fiscal year 2021 (April to September) with a novel survey of firms that entered the January 2021 lottery for H-2B petition processing conducted by the Department of Labor. The information requested in the survey, and the tests we performed, were specified in a pre-analysis plan posted before data collection began. That plan specified the primary outcomes (revenue and employment), regression specifications (reduced form and 2SLS), and tests for heterogeneous effects (by level of output market competition and by rural/urban location) that follow.[20]

5.1 Data collection

We asked four industry associations of U.S. firms that hire workers on H-2B visas to send an online survey to all of their members, asking a knowledgeable representative of each firm to complete it. These associations are the National Association of Landscape Professionals, the Outdoor Amusement Business Association, the Seasonal Employment Alliance, and the American Seafood Jobs Alliance. These associations claim as members roughly 2,500 firms out of the

[20]Pre-analysis plan registered on October 21, 2021, the morning that the survey was first disseminated and before any responses had been received, at https://osf.io/zdyun.

4,406 firms that entered the January 2021 lottery (57%). They sent the survey to their members seven weeks after the end of the second half of fiscal year 2021, on October 21, 2021, and followed up with email reminders to their members on November 1, 12, and 30. We received responses from October 21, 2021 through January 26, 2022. We closed the survey to further responses on February 8, 2022.

The title of the survey was "*Survey of US businesses after the H-2B visa lottery*". It stated its purpose to respondents as, "*We are economists studying how the H-2B visa lottery in January 2021 affected American businesses that entered that lottery. We want to hear from you whether or not you were able to hire any H-2B workers this year.*" The survey instrument then asked nine factual questions about how many H-2B workers they petitioned for; which lottery letters they received; how many of different types of workers they employed between April and September; their revenue and investment during the same period; and a few questions about business conditions including the degree of competition they faced, recent changes in their costs, and their geographic location. The survey questionnaire is reproduced in the Appendix.[21] Respondents were told that "U.S. worker" includes both citizens and lawful permanent residents. The survey respondents were well aware of their randomization outcome. One advantage of the purely online administration of the survey is that the enumeration experience is identical for all respondents, without regard to randomization status. There was no face-to-face contact that could in principle convey enumerator expectations of different responses by lottery winners versus lottery losers.

The survey measures the degree of competition faced by each firm in two different, pre-specified ways. The first, following Nickell (1996), is simply to ask each firm to report the absolute number of direct competitors it faces in the market it serves. The second, following Tang (2006), is to ask the firm to subjectively rate, on a four-step ordered scale, "how easy it would be for one of your business's competitors to steal your clients simply by underpricing you?"

The survey measures profits indirectly, due to the well-known reluctance of firms to directly

[21] Firms were then given an opportunity to identify themselves by firm name and postal code if they wished, though the survey instrument prominently indicated that this question was optional; 73% of firms chose to do so. Both DOL and DHS already make public the names of every firm that petitions for H-2B workers and the details of those petitions, so it was unsurprising that most firms felt comfortable identifying themselves in this survey.

report profits on surveys (e.g. Iarossi 2006, 53). The survey asks a prespecified question about its year-on-year change in *operating costs*, which combined with information about the change in revenues, yields a proxy measure of the change in profits (specifically: Earnings Before Interest, Taxes, Depreciation, and Amortization, EBITDA).

When the survey closed we had received survey forms from 371 respondents. 54 of these (14.6%) were dropped because they were too incomplete for analysis. In most cases, this was because the respondent had answered questions about the H-2B lottery only, and had not answered any of the questions about business outcomes such as revenue. Another 15 responses (4.0%) were dropped because the firm reported petitioning for zero H-2B workers for the period April–September 2021, despite the instruction that the survey was intended only for 2021 H-2B lottery entrants. Another 13 responses (3.5%) were dropped because two different people from the same firm had sent separate responses.[22] This left a final survey sample of 289 firms that had answered most questions about 2021. The core sample used in most regressions to follow, 251 firms, comprises those that also provided full pre-lottery baseline data from 2020. Summary statistics are presented in the Appendix.

5.2 Survey response

A first-order concern in a survey of this kind is bias from global nonresponse. The H-2B petitions reported by survey respondents represent 8.9% of the universe of petitions in the lottery (477 out of 5,376). We test for nonresponse bias in three complementary ways.

First, we compare the distribution of lottery results in the survey sample to the distribution in the universe (Table 1). The two distributions match closely. The proportions of each lottery letter in the sample and universe are pairwise statistically indistinguishable, by a wide margin, as well as indistinguishable across all five letters collectively. This is inconsistent with theories that firms with good lottery results might be more likely to respond (e.g. because they are more likely to employ H-2B workers and thus consider the survey relevant) or less likely to respond (e.g.

[22] For one of these, only one of the respondents had completed a substantial portion of the survey, so the other response from that firm was dropped. For the other twelve, roughly the same amount of information was provided by both respondents from each firm, so a random number generator was used to choose which of the two responses for each firm was kept.

Table 1: Lottery results in sampling universe vs. survey sample, 2021

	Frequency		Proportion		
Result	*Universe*	*Sample*	*Universe*	*Sample*	p-val.
A	2,029	186	0.377	0.390	0.589
B	1,046	97	0.195	0.203	0.643
C	1,065	97	0.198	0.203	0.783
D	1,125	86	0.209	0.180	0.134
E	111	11	0.021	0.023	0.724
Total petitions	5,376	477	1.000	1.000	*0.651**

The unit of observation is petitions. The p-value is for a two-sample test of the null hypothesis that the fraction of petitions receiving each lottery letter in the survey sample is equal to that fraction in the universe of petitions. *The final row gives the p-value of Fisher's exact test of the null hypothesis that the lottery-result distribution across all five letters is equal in the sample and the universe.

because other firms use the survey to express perceived harm from their own lottery result).[23]

Second, we test for randomization balance—whether or not firms' baseline (pre-lottery) characteristics in the survey sample exhibit spurious correlation with the lottery results of firms in the survey sample. This test could in principle reject the null of no correlation for two independent and singly sufficient reasons. First, firms with certain baseline traits (e.g. relatively large firms) could be differentially affected by the lottery result, and those differential effects could make them more or less likely to respond to the survey. Second, this test could reject the null if there were irregularities in the randomization process carried out by DOL. When we regress both measures of the lottery outcome used below on the baseline traits of firms in the survey sample, however, there is no sign of spurious correlation. The p-values on the baseline traits range from 0.40 to 0.89, and the largest R^2 is 0.006 (reported in the Appendix). That is, beyond being representative of the universe of lottery outcomes, the responding firms were also representative of firms' baseline size and employment patterns within lottery outcome groups. This is inconsistent with substantial nonresponse bias correlated with treatment status, or with substantial administrative irregularities in randomization.

Finally, we test whether the results below vary according to the amount of time elapsed between respondents' first receipt of the survey and their submission of a response. This is a common

[23]The results of this comparison are substantially the same when the sample of survey-reported petitions is restricted to the 328 petitions that were filed by the 251 firms in the core regression sample for the analysis to follow. The p-value for Fisher's exact test of the sample-universe equality of proportions is 0.501. Full results in the Appendix.

proxy test for nonresponse bias in the literature (e.g. Behaghel et al. 2015; Heffetz and Reeves 2019). The results, reported below, exhibit no significant heterogeneity across a wide range of response delay.

The survey sample describes 9,341 H-2B workers employed in the second half of fiscal year 2021 to perform a variety of basic tasks providing low-skill, nontradable services to several different industries. The most common industry for an H-2B worker requested on a petition in the survey sample is groundskeeping and outdoor maintenance workers (35.5%), which typically include workers in landscaping, irrigation, gardening, maintenance vehicle driving, tree care, removal of debris/mud/snow, brush clearing for electrical-line rights-of-way, and hanging holiday décor. The next most common is basic workers in hospitality (20.2%), which typically include housekeepers, clerks, porters, waiters, cooks, dishwashers, baristas, parking attendants, lifeguards, and janitors. These are followed by workers in forestry (16.2%), seafood processing (6.1%), golf courses and country clubs (5.2%), restaurants (1.2%), carnivals (0.8%), and construction (0.5%), along with workers in various other industries (14.2%).

This industry breakdown for requested workers in the survey sample is broadly representative of the sampling universe. The ranking of industries in the sample and universe are similar, with the exception of forestry and construction: Forestry is overrepresented in the sample (ranked third in the sample, seventh in the universe) and construction is underrepresented in the sample (ranked eighth in the sample, third in the universe). In the sample relative to the universe, workers in hospitality and forestry are overrepresented by roughly 10 percentage points, while workers in groundskeeping/outdoor maintenance are underrepresented by 18 percentage points and workers in construction are underrepresented by 8 percentage points.

The geographic locations of the firms in the sample and universe are likewise similar. 31.0% of workers in the sample were requested by firms whose employer lists a rural (non-metropolitan) address, compared to 27.4% in the universe. The Appendix reports a detailed comparison of these basic traits for firms in the sample versus the universe.

The survey has three important limitations. The first is sample size, which limits statistical power and limits opportunities for subgroup analysis. The second is that it cannot measure

long-run effects on firms, which are observed at one moment in time during the period 3–9 months following the lottery. The third is that it only measures effects on firms that existed both at the beginning of 2021 and near the end of 2021, and thus cannot estimate any effects on exit or entry. We do note, however, that the results in Table 1 are incompatible with any large effects of the lottery on firm exit in the short run: Firms with poor lottery outcomes were not substantially less likely to exist 10–11 months after the lottery, and thus complete the survey, than firms with better lottery outcomes.

5.3 Defining the instrumental variable

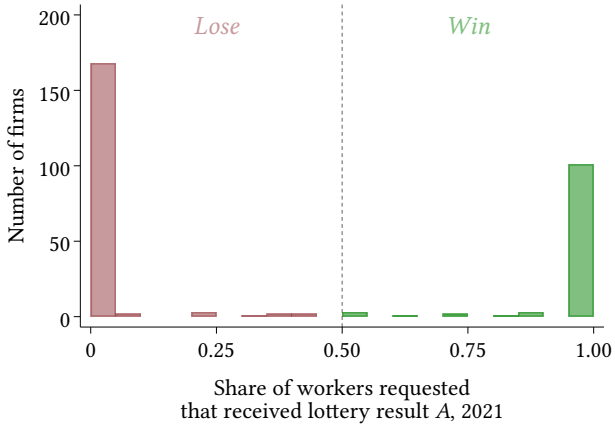
The lottery allows us to define an instrumental variable for employment of low-skill immigrant workers by firms in the survey sample. The lottery result is exogenous and only affects the firm via its effect on the firm's access to those workers. We use two different specifications, both of which were described in our pre-analysis plan.

Lottery win instrument: The first specification of the instrument is dichotomous and intuitive: Based on Figure 4, we simply define a *petition* as 'winning' the lottery if it receives letter A, and 'losing' otherwise. In the survey sample, the firm-level share of requested workers on winning petitions (s_i^A) is nearly dichotomous, but not quite, because some firms file multiple petitions (Figure 5). We then define a *firm* as winning the lottery (value = 1) if and only if the share s_i^A of all its requested H-2B workers on winning petitions exceeds 0.5. The 'lottery win' instrument for firm i is

$$z_i \equiv \begin{cases} 1 & \text{if } s_i^A > 0.5 \\ 0 & \text{otherwise.} \end{cases} \tag{10}$$

Expected share instrument: The second specification of the instrument is continuous and somewhat less transparent: it is defined as the *share* of H-2B workers originally entered into the lottery that each firm i can expect to receive permission from both DOL and DHS to employ, based on the lottery result. It uses the rates of success from Figure 4: the rate of success for a worker on an A petition in the second half of fiscal year 2021 was $\rho^A = 0.920$, the rate of success for a worker on a B petition was $\rho^B = 0.748$, and similarly $\rho^C = 0.568$, $\rho^D = 0.550$, and $\rho^E = 0.579$. Denoting by s_i^ℓ the share of each firm's requested workers receiving lottery letter $\ell \in \{A, B, C, D, E\}$, the

Figure 5: Defining a lottery 'win' at the firm level, 2021 lottery



The unit of observation is firms in the survey sample, second half of fiscal year 2021. Frequency histogram with bin width 0.05.

'expected share' instrument is

$$z_i' \equiv \sum_{\ell \in \{A \ldots E\}} \rho^{\ell} s_i^{\ell}. \tag{11}$$

This specification avoids bias that would arise from alternative specifications of the instrument, such as the absolute predicted number of H-2B hires or the probability of at least one winning petition, which would correlate with firm size.

6 Results

The above instruments, randomized across firms, allow us to estimate the reduced-form effect of winning the lottery and the two-stage-least-squares effect of foreign worker employment, for the firms whose foreign hiring was altered by the lottery outcome. We focus on the primary outcomes specified in the pre-analysis plan: revenue and U.S. employment. The same results can be interpreted as the inverse effect of *losing* the lottery, and thus reducing immigrant hires from the profit-maximizing level to the restricted level.

In most of the analysis we use the simple linear regression specification of

$$y_{i,t} = \zeta + \theta I_{i,t} + \mathbf{X}'_{\mathbf{i,t-1}}\mathbf{\Phi} + \varepsilon_{i,t}, \tag{12}$$

where $y_{i,t}$ is the outcome for firm i in the current period t; $I_{i,t}$ is foreign temporary worker employment in the current period; $\mathbf{X}'_{\mathbf{i,t-1}}$ is a vector of firms' predetermined traits; θ and the vector $\mathbf{\Phi}$ are coefficients to be estimated; $\varepsilon_{i,t}$ is an error term; and ζ a constant. In two-stage least squares specifications, $I_{i,t}$ is instrumented by the randomized $Z_{i,t}$, either the 'lottery win' or the 'expected share' instrument described above. We also consider the reduced-form specification

$$y_{i,t} = \zeta + \mu Z_{i,t} + \mathbf{X}'_{\mathbf{i,t-1}}\mathbf{\Phi} + \varepsilon_{i,t}. \tag{13}$$

We use logarithmic transformations of variables such as revenue, in order to yield coefficient estimates interpretable as elasticities. Because the variables for employment and investment often take values of zero, we use the inverse hyperbolic sine (IHS) rather than the logarithmic transformation of these variables (Burbidge et al. 1988). The resulting coefficients are likewise interpretable as elasticities at the magnitudes of the untransformed variables encountered here; that is, the untransformed means well exceed 10 (Bellemare and Wichman 2020). Moreover, the results are robust to alternative specifications that do not use the IHS transformation (below in Section 7).

The first step is to verify that the lottery outcome substantially alters foreign-worker employment at the firm level, as suggested by Figure 4. In the first-stage regressions of the form in equation (13) with foreign employment as the outcome, the randomized instruments have an effect on foreign-worker employment that is large and statistically significant. Winning the lottery causes firms to employ 2.3 times as many foreign workers as lottery-losing firms, corresponding to a first-stage semielasticity $\hat{\mu} = 0.822$. The first stage regressions are presented in the Appendix. This large effect across firms allows estimation of the various firm-level impacts to follow.

6.1 Effect of low-skill foreign employment on revenue

Table 2 presents preregistered tests of the effects on firm revenue from the lottery and from foreign employment in the second half of fiscal year 2021. These regressions estimate the coef-

Table 2: Effect of foreign worker employment on firm revenue, 2021

Dep. var:	*Revenue 2021 (ln)*									
Estimator:	OLS				2SLS		OLS		2SLS	
Instrument:					*Lottery win*				*Expected share*	
Foreign employed 2021 (IHS)	0.113	0.127			0.183	0.207			0.144	0.164
	(0.030)	(0.029)			(0.087)	(0.089)			(0.071)	(0.073)
Anderson-Rubin p-*val.*	—	—			*0.0452*	*0.0262*			*0.0457*	*0.0288*
Lottery win 2021			0.157	0.170						
			(0.078)	(0.076)						
Expected share 2021							0.490	0.531		
							(0.244)	(0.241)		
Revenue 2020 (ln)	0.781	0.728	0.839	0.775	0.745	0.697	0.841	0.777	0.765	0.714
	(0.065)	(0.084)	(0.060)	(0.086)	(0.078)	(0.090)	(0.060)	(0.086)	(0.071)	(0.086)
Full baseline controls	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*
Number of firms	251	251	251	251	251	251	251	251	251	251
R^2	0.807	0.816	0.792	0.800	0.800	0.808	0.792	0.800	0.805	0.814

The unit of observation is firms. All regressions include constant term. Robust standard errors in parentheses. The dichotomous 'Lottery win' instrumental variable is an indicator variable for winning the lottery, that is, receiving 'A' on petitions totaling at least 50% of workers requested. The continuous 'Expected share' instrumental variable is the share of overall workers petitioned for that the firm could expect to be certified according to the certification rates in the sampling universe for each lottery letter. IHS is inverse hyperbolic sine. *Full baseline controls* are the 2020 values of revenue, number of U.S. year-round workers, number of U.S. temporary workers, and number of foreign temporary workers.

ficient modeled in equation (9). In this and other tables the firm sample is held constant across columns: All regressions include only those firms that reported full baseline data.

The first two columns of Table 2 show estimates from a simple OLS regression of revenue on foreign workers. In the first column the only predetermined control variable is 2020 revenue. In the second column, the predetermined controls are expanded to include 2020 foreign worker employment, 2020 temporary U.S. employment, and 2020 year-round U.S. employment. Revenue and foreign employment are correlated across firms with elasticity 0.127, controlling for the full baseline data.

The rest of the table reveals a large and statistically significant positive effect of foreign-worker employment on firm revenue. Columns 3 and 4 of Table 2 present reduced-form regressions of revenue on the 'lottery win' instrument (10), using regression specification (13). Winning the lottery causes firm revenue to grow by 18.5% (corresponding to a semielasticity of 0.17). Here again, and hereafter, the first column of the pair controls only for baseline revenue while the second controls for the full set of observed baseline traits. Columns 5 and 6 show the second stage of a 2SLS regression of revenue on foreign employment, with foreign employment instrumented by the 'lottery win' instrument. With this randomized instrument, the causal effect of foreign employment on firm revenue is to raise it with elasticity 0.207, among firms whose foreign employment was altered by the visa cap.

All 2SLS regressions we report are just-identified, reducing concerns about statistical inference distorted by weak instruments (Angrist and Kolesár 2021). We nevertheless report the p-value of the Anderson-Rubin test, which is fully robust to weak instrumentation (Andrews et al. 2019) that might invalidate traditional t-ratio inference (Lee et al. 2021), below each estimate of the coefficient of interest. This p-value on foreign employment in the 2SLS 'lottery win' regression with full baseline controls is 0.026, suggesting a high degree of statistical significance.

Columns 9–10 of Table 2 repeat the exercise of columns 5–6, using the alternative 'expected share' instrument (11). The results are similar using this alternative, randomly-assigned instrument. In the eighth column, exogenously shifting a firm from the minimum to the maximum fraction of requested foreign workers who could be employed (from 0.55 to 0.92) caused firm

revenue to rise by 21.7%, corresponding to a semielasticity of $0.531 \times (0.92 - 0.55) = 0.196$. This is close to the semielasticity of 0.170 estimated as the reduced form effect of the dichotomous 'lottery win' instrument in column 4. In the final column, the causal effect of foreign employment on firm revenue using the 'expected share' instrument has an estimated elasticity of 0.164, with an Anderson-Rubin p-value of 0.029. This too is close to the magnitude of the estimate in column 6 relying on the 'lottery win' instrument.

6.2 Effect of low-skill foreign employment on U.S. employment

Table 3 presents preregistered tests of the effect of employing foreign temporary workers on firms' employment of U.S. workers in the second half of fiscal year 2021. It is very similar to Table 2, with two differences. First, the outcome variable is firm-level employment of U.S. temporary workers during the second half of fiscal year 2021. Second, in the specifications that control for a single baseline trait, that trait is not baseline revenue but baseline employment of U.S. workers. (The set of 'full' baseline traits is unchanged from Table 2.) These regressions estimate the coefficient modeled in equation (7).

In the first two columns of Table 3, U.S. temporary worker employment and foreign temporary worker employment are correlated across firms with elasticity 0.208, controlling for the full baseline traits. In columns 3–4, the causal semielasticity of U.S. temporary employment to winning the lottery is 0.146, an estimate that is not statistically significantly different from zero at conventional levels (p=0.31). In columns 5–6, the causal elasticity of U.S. temporary employment to foreign temporary employment is 0.178, though again we cannot reject the null hypothesis that this effect is zero for the average firm whose foreign employment was determined by the lottery outcome. The last four columns yield very similar estimates and inference using the 'expected share' instrument.

Figure 6 presents a graphic representation of the reduced-form effects of the 'lottery win' instrument on revenue and U.S. temporary employment, from Tables 2 and 3, column 4. The kernel density plots show the residual after regressing each outcome on the predetermined (2020) traits, for lottery-winning versus lottery-losing firms. Figure 6a simply verifies the strength of the instrument: It shows the large effect of the lottery on foreign-worker employment conditional

Table 3: Effect of foreign worker employment on U.S. employment, 2021

Dep. var:	*U.S. temporary workers 2021 (IHS)*									
Estimator:	OLS				2SLS		OLS		2SLS	
Instrument:					*Lottery win*				*Expected share*	
Foreign employed 2021 (IHS)	0.121	0.208			0.138	0.178			0.058	0.102
	(0.047)	(0.057)			(0.166)	(0.167)			(0.133)	(0.129)
Anderson-Rubin p-*val.*	—	—			*0.412*	*0.305*			*0.670*	*0.445*
Lottery win 2021			0.124	0.146						
			(0.151)	(0.142)						
Expected share 2021							0.194	0.331		
							(0.456)	(0.432)		
U.S. temporary workers 2020 (IHS)	0.776	0.784	0.781	0.770	0.775	0.781	0.780	0.770	0.777	0.776
	(0.040)	(0.038)	(0.039)	(0.039)	(0.040)	(0.038)	(0.039)	(0.039)	(0.039)	(0.039)
Full baseline controls	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*
Number of firms	251	251	251	251	251	251	251	251	251	251
R^2	0.652	0.703	0.642	0.680	0.652	0.702	0.641	0.680	0.649	0.697

The unit of observation is firms. All regressions include constant term. Robust standard errors in parentheses. The dichotomous 'Lottery win' instrumental variable is an indicator variable for winning the lottery, that is, receiving 'A' on petitions totaling at least 50% of workers requested. The continuous 'Expected share' instrumental variable is the share of overall workers petitioned for that the firm could expect to be certified according to the certification rates in the sampling universe for each lottery letter. IHS is inverse hyperbolic sine. *Full baseline controls* are the 2020 values of revenue, number of U.S. year-round workers, number of U.S. temporary workers, and number of foreign temporary workers.

on baseline traits. A nonparametric Kolmogorov-Smirnov test rejects the hypothesis that the residual distributions are equal between lottery-winning and lottery-losing firms at the 0.012% level.

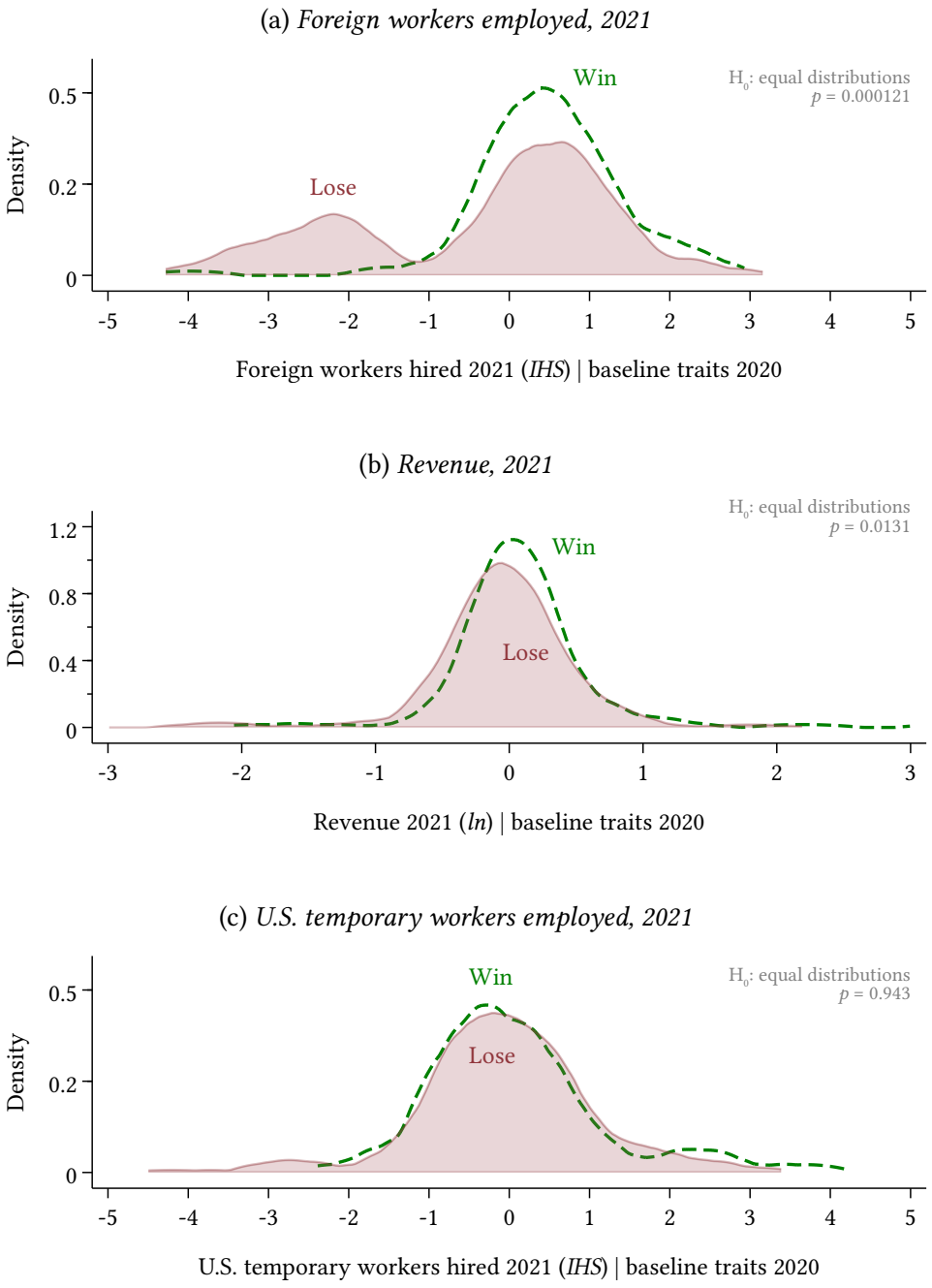
Figure 6b shows the reduced-form effect of winning the lottery across the full distribution of firm-level revenue conditional on baseline traits including 2020 revenue. The null hypothesis of equal distributions is rejected at the 1.3% level. Two features of the graph are notable. First, the effects estimated in Table 2 are not driven by a small number of influential observations. The lottery outcome causes a visible shift in revenue across the distribution. Second, firms whose 2021 revenue was much larger than 2020 revenue (toward the right of the graph) appear less affected by the lottery outcome than firms whose 2021 revenue was much smaller than 2020 revenue (toward the left). In other words, the most rapidly growing firms appear to find a way to produce regardless of access to these foreign workers; firms whose revenue would otherwise have been more stable experience sharp declines in revenue caused by a bad lottery outcome.

Finally, Figure 6c shows the reduced-form effect of the lottery result across the full distribution of U.S. temporary employment, conditional on baseline traits including 2020 U.S. employment. At the center of mass, the distribution for lottery-winning firms is very close to the distribution for lottery-losing firms. Here the Kolmogorov-Smirnov test fails to reject the null hypothesis of equal distributions, by far ($p = 0.94$). Differences between the distributions are only visually apparent at the tails: Firms that experienced high growth in U.S. employment from 2020 to 2021 (toward the right of the graph) appear to raise U.S. employment even more when they win the lottery to employ foreign temporary workers; firms with declining U.S. employment from 2020 to 2021 (toward the left) appear to mitigate that decline when they win the lottery.

6.3 Effect of low-skill foreign employment on investment and profit

We now consider firm outcomes labeled as secondary in the pre-analysis plan. Table 4 presents preregistered tests of the effect of low-skill foreign temporary worker employment on investment by firms in the second half of fiscal year 2021. 'Investment' is the dollar value reported in response to the question, '*How much did your business spend on large, occasional investments in equipment or real estate this year ($)?*' Except for the outcome variable, the regression specifica-

Figure 6: Reduced-form effects of the 2021 lottery



The unit of analysis is firms. 'Win' is defined as a firm receiving randomized lottery letter '*A*' for petitions exceeding half of the total workers requested; all other results are defined as 'lose'. Graphs show Epanechnikov kernel density estimates with a bandwidth of 0.15 inverse hyperbolic sine (IHS) points (*a* and *c*) or 0.5 ln points (*b*). Exact *p*-values are from nonparametric two-sample Kolmogorov-Smirnov tests of the null hypothesis of the equality of the 'win' and 'lose' distributions in each pane of the figure. Residuals are estimated controlling for the full set of baseline traits, corresponding to column 4 in Tables 2 and 3.

tions in the table are identical to those in Table 2.

In the first two columns of Table 4, investment is correlated with foreign temporary worker employment across firms with an elasticity of 0.580. In columns 3–4. winning the lottery causes investment to rise by a factor of 2.12, corresponding to a semielasticity of 1.137. In columns 5–6, foreign employment causes greater investment with an elasticity of 1.331 (Anderson-Rubin p-value 0.044) using the 'lottery win' instrument. The same test using the 'expected share' instrument, in column 10, yields a causal elasticity of 1.031 (Anderson-Rubin p-value 0.068). Winning the lottery causes firms' investment to triple (in col. 4, $e^{1.137} = 3.12$). This evidence is consistent with a large, positive, short-run effect of the ability to hire low-skill foreign workers on firms' purchases of equipment, vehicles, structures, and land.

Assessing the effect of the lottery on firms' profits was complicated by the fact, as discussed in Section 5, firms are known to be reluctant to respond to direct questions about profits. Thus we sought to indirectly estimate the *change* in the rate of profit $0 < \pi < 1$ from year 0 to year 1, by asking about the *level* of revenue in each year (R, in dollars) and the *change* in operating costs between years (C, in dollars). Profit is specified as EBITDA (Earnings Before Interest, Taxes, Depreciation, and Amortization).[24] Firms report the year-on-year percentage change in dollar-value operating costs, or

$$\Delta C \equiv \frac{R_1(1-\pi_1)}{R_0(1-\pi_0)} - 1. \tag{14}$$

This identity implies $\ln \frac{1-\pi_1}{1-\pi_0} = \ln(1+\Delta C) - \ln \frac{R_1}{R_0}$. Since $\ln \frac{1-\pi_1}{1-\pi_0} \approx -\ln \frac{\pi_1}{\pi_0}$ for any small (π_0, π_1), the year-on-year percentage change in profits can be estimated using only information reported on the survey (R_0, R_1, and ΔC):

$$\ln \frac{\pi_1}{\pi_0} \approx \ln \frac{R_1}{R_0} - \ln\big(1+\Delta C\big). \tag{15}$$

This year-on-year change in the *rate* of profit is the outcome variable in the regressions to follow.

Table 5 presents preregistered tests of the effect of foreign worker employment on the growth of

[24] The survey question reads, "*We'll ask now about any changes in your month-to-month operating costs since last year. By 'operating costs' we mean all the expenditures it takes to keep your business running in a typical month: cost of goods sold, marketing, recruiting, wages, and overhead—that is, all expenditures by your business excluding occasional large purchases of equipment or real estate. By what percentage would you say your normal monthly operating costs this year (2021) have been higher or lower than the same period of last year (2020)?*"

Table 4: Effect of foreign worker employment on firm investment, 2021

Dep. var:	*Investment 2021 (IHS)*									
Estimator:	OLS				2SLS		OLS		2SLS	
Instrument:					*Lottery win*				*Expected share*	
Foreign employed 2021 (IHS)	0.572	0.580			1.358	1.331			1.064	1.031
	(0.224)	(0.240)			(0.648)	(0.669)			(0.532)	(0.557)
Anderson-Rubin p-*val.*	—	—			*0.0322*	*0.0435*			*0.0467*	*0.0676*
Lottery win 2021			1.212	1.137						
			(0.563)	(0.560)						
Expected share 2021							3.712	3.432		
							(1.857)	(1.869)		
Revenue 2020 (ln)	0.634	0.922	0.921	1.105	0.227	0.645	0.933	1.119	0.379	0.756
	(0.268)	(0.316)	(0.244)	(0.303)	(0.385)	(0.389)	(0.245)	(0.306)	(0.358)	(0.371)
Full baseline controls	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*
Number of firms	249	249	249	249	249	249	249	249	249	249
R^2	0.105	0.119	0.087	0.102	0.042	0.068	0.086	0.101	0.081	0.101

The unit of observation is firms. All regressions include constant term. Robust standard errors in parentheses. The dichotomous 'Lottery win' instrumental variable is an indicator variable for winning the lottery, that is, receiving 'A' on petitions totaling at least 50% of workers requested. The continuous 'Expected share' instrumental variable is the share of overall workers petitioned for that the firm could expect to be certified according to the certification rates in the sampling universe for each lottery letter. IHS is inverse hyperbolic sine. *Full baseline controls* are the 2020 values of revenue, number of U.S. year-round workers, number of U.S. temporary workers, and number of foreign temporary workers.

Table 5: Effect of foreign worker employment on change in the rate of profit, 2021

Dep. var:	*Change in profit rate 2020–2021*									
Estimator:	OLS				2SLS		OLS		2SLS	
Instrument:					*Lottery win*				*Expected share*	
Foreign employed 2021 (IHS)	0.096	0.103			0.120	0.129			0.094	0.100
	(0.028)	(0.026)			(0.079)	(0.080)			(0.066)	(0.067)
Anderson-Rubin p-*val.*	—	—			*0.142*	*0.123*			*0.165*	*0.147*
Lottery win 2021			0.109	0.109						
			(0.074)	(0.070)						
Expected share 2021							0.314	0.317		
							(0.225)	(0.218)		
Revenue 2020 (ln)	−0.233	−0.297	−0.185	−0.259	−0.245	−0.306	−0.184	−0.259	−0.232	−0.296
	(0.061)	(0.081)	(0.056)	(0.082)	(0.074)	(0.086)	(0.056)	(0.083)	(0.069)	(0.085)
Full baseline controls	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*
Number of firms	238	238	238	238	238	238	238	238	238	238
R^2	0.234	0.275	0.182	0.221	0.230	0.271	0.181	0.220	0.234	0.274

The unit of observation is firms. All regressions include constant term. Robust standard errors in parentheses. The dichotomous 'Lottery win' instrumental variable is an indicator variable for winning the lottery, that is, receiving 'A' on petitions totaling at least 50% of workers requested. The continuous 'Expected share' instrumental variable is the share of overall workers petitioned for that the firm could expect to be certified according to the certification rates in the sampling universe for each lottery letter. IHS is inverse hyperbolic sine. *Full baseline controls* are the 2020 values of revenue, number of U.S. year-round workers, number of U.S. temporary workers, and number of foreign temporary workers.

firms' profits, in the second half of fiscal year 2021. But for the outcome variable, the regressions in each column are identical to those in Table 2.

In the first two columns of Table 5, the growth in profit rate is positively correlated across firms with low-skill foreign employment, with an elasticity of 0.103. In the next two columns, winning the lottery has a positive reduced-form effect on growth in the profit rate, with a causal semielasticity of 0.109, that is not quite statistically distinguishable from zero at conventional levels ($p = 0.123$). In the rest of the table, depending on the instrument used, foreign employment has a positive effect on growth in the profit rate, with a positive causal elasticity of 0.100–0.129 (Anderson-Rubin p value 0.123–0.147).

This cannot be translated into a dollar value because the *levels* of profit and profit rate are unobserved in the survey. But the magnitude of the coefficient estimate using the 'expected share' instrument implies that a doubling of foreign-worker employment would raise dollar-value profits by 28%, because the 10.0% increase in the rate of profit as a fraction of revenue (Table 5) is augmented by the 16.4% increase in revenue (Table 2, col. 10).[25] This estimate must be interpreted with caution since the coefficient estimate from the profit regressions is not quite statistically precise.

7 Robustness

In the above analysis the core outcomes, regressions, instruments, and baseline controls were prespecified and immutable. Here we report the robustness of the results to a series of tests, some of which were likewise prespecified, and some that were not.

7.1 Prespecified robustness checks

The preanalysis plan specified that we would test the results for heterogeneity by response delay, as a proxy test for nonresponse bias. This is a common test for nonresponse bias in the literature (e.g. Behaghel et al. 2015; Heffetz and Reeves 2019), based on a model of nonresponse in which

[25] Dollar-value profits for lottery-losing firms are $R_\ell \cdot \pi_\ell$, which would rise by a factor of $\frac{1.164R_\ell \cdot 1.10\pi_\ell}{R_\ell \pi_\ell} = 1.28$.

the same latent variable that causes delayed responses to the survey causes a substantial portion of complete nonresponses. For example, firms with fewer full-time staff, who are busier, or who have less interest in research might be both less likely to respond immediately and less likely to respond at all. The elapsed time between survey receipt and survey completion thus can serve as an imperfect proxy for the latent traits of global nonresponders. The coefficients estimated in the core regressions, however, do not vary with the response delay to a statistically or economically significant degree (reported in the Appendix). This is inconsistent with any strong bias in the core results arising from a plausible model of nonresponse behavior.

Another prespecified robustness check was to test for heterogeneity of the core results according to item nonresponse. The most important form of item nonresponse was the firms that did not voluntarily provide the firm name and postal code (11% of responses and 8% of the core sample, that is, 20 of the core sample of 251). Such firms could not be assigned to a 'rural' or 'urban' environment, and could not be linked to their 2020 lottery results. The core results of Tables 2–4 are qualitatively and quantitatively robust to truncating these item-nonresponders from the 2021 sample (reported in the Appendix).

The core results are furthermore robust to the prespecified robustness check of adjusting statistical inference for multiple hypothesis testing by (asymptotically) controlling for the familywise error rate with the method of List et al. (2019, Thm. 3.1). This method is suitable for the dichotomous 'lottery win' treatment. We thereby reconsider the p-values in the reduced-form regressions of the three core prespecified outcomes: revenue, U.S. employment, and investment (col. 4 in Tables 2, 3, and 4). For revenue, controlling for the familywise error rate shifts the p-value from 0.026 to 0.063. For U.S. employment, the p-value shifts from 0.305 to 0.309. For investment, the p-value shifts from 0.044 to 0.084. This correction does not alter the broad pattern of statistical inference above, as might be expected in a study with a small number of prespecified outcomes.[26]

[26]This test across three outcomes is conservative, in the sense that the pre-analysis plan contained only two 'principal outcomes'—revenue and U.S. temporary employment; investment was classified as a 'secondary outcome'.

7.2 Partial replication in 2020

We partially replicate the 2021 natural experiment in fiscal year 2020. This analysis was not prespecified because we did not anticipate that it would be possible. Although the Department of Labor conducted a very similar, independent lottery on January 1, 2020 for the second half of fiscal year 2020, our survey did not ask about firms' lottery-letter result from 2020. It asked for firms' traits in 2020, such as revenue and employment, only to be used as baseline controls for analysis of the 2021 lottery.

But to our surprise, 89.3% of respondents chose voluntarily to identify their firm by name. This might have been foreseeable, given that most of the information requested on the survey is already published by the government along with detailed firm-by-firm identifiers, but we did not expect the rate of self-identification to be so high.

The firms that did self-identify could be easily matched to public records of their 2020 lottery-letter result, allowing the replication exercise for 2020. This exercise has advantages and disadvantages. One reason to expect greater statistical power in 2020 is that the lottery was a stronger determinant of access to H-2B workers in 2020 than in 2021, because in 2020 no supplemental visas were issued by DHS (Figure 4). On the other hand, a reason to expect lower statistical power in 2020 is that the sample size is reduced, since only self-identifying firms can by included in the 2020 analysis. Another disadvantage is that the prior-year baseline traits used in the 2021 analysis are unobserved in the 2020 analysis. (The survey did not ask about revenue or employment in 2019.) Instead, in the 2020 analysis we control for the only observed, time-varying firm trait that is predetermined in 2019: the number of H-2B workers requested from DOL in the 2020 lottery, which was fixed by December 31, 2019. This predetermined trait is informative because it is correlated with the size of the firm, but is a more imperfect control for baseline size than (unobserved) baseline revenue.[27] For this reason the 2020 replication is partial rather than exact.

Table 6 presents the results of the 2020 replication exercise for the revenue and U.S. employment outcomes, corresponding to the 2021 results in Tables 2 and 3 above. The magnitudes of the coefficient estimates are strikingly similar in this independent experiment.

[27] The regressions with investment as an outcome cannot be done in this setting because the survey does not ask

Table 6: Robustness: The 2020 Lottery

Dep. var:	*Revenue 2020 (ln)*					*U.S. temporary workers 2020 (IHS)*				
Estimator:	OLS		2SLS	OLS	2SLS	OLS		2SLS	OLS	2SLS
Instrument:			*Lottery win*		*Expected share*			*Lottery win*		*Expected share*
Foreign employed 2020 (IHS)	0.082		0.151		0.146	0.108		0.071		0.166
	(0.050)		(0.103)		(0.091)	(0.083)		(0.170)		(0.150)
Anderson-Rubin p-*val.*	—		*0.152*		*0.111*	—		*0.680*		*0.262*
Lottery win 2020		0.223					0.100			
		(0.155)					(0.242)			
Expected share 2020				0.348					0.371	
				(0.217)					(0.330)	
Baseline control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Number of firms	191	191	191	191	191	212	212	212	212	212
R^2	0.304	0.300	0.296	0.303	0.297	0.126	0.118	0.125	0.122	0.124

The unit of observation is firms. Robust standard errors in parentheses. All regressions control for the only predetermined measure of firm scale available for 2020: the number of H-2B workers *requested* by the firm in 2020 (IHS), a number that is well correlated with revenue and was chosen by each firm in 2019. Other baseline controls for this lottery are not observed. All regressions include constant term. The dichotomous 'Lottery win' instrumental variable is an indicator variable for winning the lottery, that is, receiving 'A' on petitions totaling at least 50% of workers requested. The continuous 'Expected share' instrumental variable is the share of overall workers petitioned for that the firm could expect to be certified according to the certification rates in the sampling universe for each lottery letter. IHS is inverse hyperbolic sine.

For example, the reduced-form regression of revenue on 'lottery win' yields an estimate of 0.223 in 2020 (Table 6, col. 2), compared to an estimate of 0.170 from 2021 (Table 2, col. 4). The reduced-form regression of revenue on 'expected share' yields an estimate of 0.348 in 2020 (Table 6, col. 4), compared to an estimate of 0.531 from 2021 (Table 2, col. 8). The analogous comparison of the reduced-form coefficients in the U.S. temporary workers regressions shows a coefficient on 'lottery win' of 0.100 in 2020 (Table 6, col. 7) versus 0.146 in 2021 (Table 3, col. 4); and a coefficient on 'expected share' of 0.371 in 2020 (Table 6, col. 9) versus 0.331 in 2021 (Table 3, col. 8).

In isolation, the reduced sample of firms whose lottery result is observed in 2020 does not yield estimates with statistical precision at conventional levels. The revenue effect of foreign worker employment in 2020 using the 'expected share' instrument, for example, yields a coefficient of 0.146 that is not statistically significant at the 10% level (Table 6, col. 5; p-val. 0.111). But the 2020 replication is more informative when considered in conjunction with the results from 2021—an independently randomized natural experiment—where the corresponding coefficient estimate takes the similar magnitude of 0.164 (Table 2, col. 10; p-val. 0.0288). The chance that two independent experiments would yield coefficients that are both positive and similar magnitude is much smaller than the p-values presented in the two tables separately. The probability that both results are positive due to random sampling error can be approximated as the product of the p-values: $0.111 \times 0.0288 = 0.00320$. The same comparison for the effect of foreign employment on U.S. worker employment (in 2020, Table 6, col. 10, coefficient 0.166 with p-val. 0.262; in 2021, Table 3, col. 10, coefficient 0.102 with p-val. 0.445) yields a joint probability of Type I error for the null hypothesis of zero effect: $0.262 \times 0.445 = 0.117$.

The 2020 replication serves as a check not just on internal validity but on external validity. The US labor market was very tight during the second half of fiscal year 2021, the period of focus in this paper. The same was not true in the second half of fiscal 2020 (Domash and Summers 2022; Duval et al. 2022). The seaonally-adjusted Job Openings rate estimated by the Bureau of Labor Statistics was similar in the second half of fiscal 2020 to what it had been in the years before the Covid-19 pandemic. It nearly doubled by mid-2021.[28] The average national unemployment rate in the second half of fiscal 2020 was 10.9%; in 2021 it was 5.5%.[29] The similar magnitude of the

about investment in 2020.

[28] Bureau of Labor Statistics *Job Openings and Labor Turnover Survey*, May 2022 release.

[29] Bureau of Labor Statistics "(Seas) Unemployment Rate, 16 and over", series LNS14000000, extracted Aug. 5, 2022.

point estimates in Tables 2, 3, and 6 is inconsistent with any crucial dependency of the results on the tight labor market conditions of mid-2021.

7.3 Alternative specifications

The core results for 2021 in Section 6 are furthemore robust to a wide range of alternative empirical methods that were not prespecified.

First, the results are robust to using randomization inference. Young (2018) notes that the data produced by randomized treatment may not yield standard errors with the asymptotic properties assumed by standard statistical inference, and urges the use of Fisher's randomization inference in these settings. The core results above are essentially invariant to the use of randomization inference (presented in the Appendix).

Second, the results are very similar in regression specifications robust to influential 'outlier' observations, as suggested by the full-distribution comparisons of Figure 6. Repeating the analysis of Tables 2 and 3 using quantile regressions (p50), both standard and IV specifications, yields qualitatively similar results for both the 2021 lottery and the 2020 replication (presented in the Appendix). The causal elasticity of *median* revenue to foreign employment is 0.090 in 2021 (p-val. 0.0243) and 0.134 in 2020 (p-val. 0.218); the causal elasticity of *median* U.S. temporary worker employment is statistically indistinguishable from zero in both years.

Third, the results are robust to specifications that do not use the inverse hyperbolic sine (IHS) transformation. When the untransformed mean of a variable falls below roughly 10, the IHS transformation can yield coefficient estimates that vary with an irrelevant affine transformation of the underlying variable (Bellemare and Wichman 2020, see also Aihounton and Henningsen 2020). There is low risk of such bias in the present setting, where where the untransformed mean of H-2B hires per firm is 28.9 and of U.S. seasonal hires is 48.5 across firms in 2021. Nevertheless, we repeat the core 2021 analysis using entirely untransformed variables with the Poisson pseudo-maximum-likelihood estimator due to Silva and Tenreyro (2006), presented in the Appendix. The qualitative and quantitative conclusions of Tables 2 and 3 are only strengthened by this exercise. The reduced-form effect of the 'lottery win' instrument and the 'expected share' instrument is

positive and statistically significant on both revenue and U.S. hiring, with a causal semielasticity exceeding 0.4 on both outcomes.

Finally, we test the results for robustness to the elimination of the most common industry in the survey sample: groundskeeping and landscaping. While this is the most common industry for low-skill male immigrant workers in the United States (Cortés 2008), due to its importance in the survey sample it is important to understand whether the effects of H-2B workers in that industry differ fundamentally from other industries. But the qualitative conclusions of the core analysis are robust to truncating all groundskeeping/landscaping firms from the sample (presented in the Appendix). The estimated causal elasticities of firms' revenue, U.S. worker employment, and investment to foreign worker employment all rise in magnitude when groundskeeping/landscaping firms are truncated.

8 Interpretation: foreign-native substitution and heterogeneous effects

The results above are directly interpretable as estimates of the Policy-Relevant Treatment Effect (PRTE, Heckman and Vytlacil 2001). The source of variation is variation in the application of a specific policy—firm-level access to the principal U.S. visa for low-skill labor. Under the assumptions of the model in Section 2, additional and indirect interpretation of the results is possible. Here we use the model to consider the foreign-native elasticity of substitution, heterogeneity in the treatment effect, treatment effect aggregation, and black-market employment.

8.1 Components of the foreign-native elasticity of substitution

Before we derive estimates of the foreign-native elasticity of substitution and place them in the context of the literature, we must consider the information contained in various estimates of this parameter. In standard labor-market analysis of immigration at the aggregate level, across geographic areas or statistical cells, the estimated immigrant-native elasticity of substitution comprises three independent effects.

First, the typically-estimated immigrant-native elasticity of substitution measures a process *within* firms: purely technical substitution within a firm's current or available production technology.

Second, the elasticity measures a process *between* firms: factor-price and output-price-induced shifts in demand from immigrant-intensive to native-intensive goods and services, known as Rybczynski effects. When the elasticity of substitution was invented by Hicks (1932, 120) and Robinson (1933, 256), Hicks specified that it measured some mix of these two processes, a mix that he called the "community level" elasticity that included effects of "commodity substitution" Hicks (1936, 8); Knoblach and Stöckl (2020) call this the "aggregate" elasticity.

But third, as Hicks (1936) soon clarified, the elasticity is furthermore shaped by imperfect competition in output markets or in factor markets (see e.g. Freeman and Medoff 1982). Including such features of the institutional environment yields what Knoblach and Stöckl (2020) call the "effective elasticity of substitution". For example, if immigration increased employers' monopsony power, immigration could reduce the immigrant-native "effective elasticity of substitution" for reasons unrelated to production technique or Rybczynski effects (Amior and Manning 2020). Standard estimates of the immigrant-native elasticity of substitution in the literature combine all three interpretations.

Our parameter σ is measured at the firm level. It omits Hicks's "community level" substitution of demand between firms (Rybczynski effects), but includes the influence of both purely technical substitution and institutional imperfections in factor markets faced by the firm. It is most comparable to other elasticities of substitution measured at the firm level.

This specific elasticity is highly informative and merits estimation, for three reasons. First, the literature has generally found that between-firm adjustment is limited, and that the principal channels of economic adjustment to immigration shocks occur within firms (Card and Lewis 2007; Dustmann and Glitz 2015). This lends some priority to pursuing unbiased estimates of firm-level substitution. Second, the *exclusion* of Rybczynski effects is desirable in the present setting because it allows us to exploit randomized variation in immigrant employment across firms. This is extremely rare across aggregates, resulting in estimates of aggregate elasticities that are less transparent and vary widely (Dustmann et al. 2016a). Third, the *inclusion* of institutional

features is also desirable since we seek the Policy-Relevant Treatment Effect—as Hicks urged. All policy occurs within an institutional setting, and our estimates include the influence of the precise institutional setting in which a marginal change in policy would occur. "Concentration upon technical substitution alone would certainly be misleading," wrote Hicks (1936, 10), for the purpose of "interpreting facts."[30]

8.2 Heterogeneity in the treatment effect

This understanding of the elasticity of substitution allows us to interpret the pre-specified tests for heterogeneous treatment effects to follow. The pre-analysis plan specified these tests explicitly to explore imperfect competition in output markets and factor markets—both of which would tend to shape the observed treatment effect.

Imperfect competition in the output market is built into the simple model in Section 2. The model and pre-analysis plan predicted that firms with less output market power—and thus a high price elasticity of output demand η—will exhibit larger effects of foreign employment on revenue (Proposition 1), on U.S. employment (Lemma 2), and on investment (Lemma 1). Intuitively, monopolistically competitive firms employing added labor and facing relatively high output price elasticity will expand production relatively more, because by doing so they will drive down output prices relatively less. We expect to observe this in firms that are small relative to their market.

Imperfect competition in factor markets is not built into the simple model above, so we extend it here. The pre-analysis plan predicted a less negative or more positive effect of immigrant employment on native employment in smaller labor markets, such as rural areas. To see why, suppose that native labor supply to the firm is upward sloping with constant elasticity e_N. This could arise from "modern monopsony" labor market frictions or "classical monopsony" forces

[30] Relatively few empirical papers attempt to separate institutional determinants of the foreign-native elasticity of substitution from the others, by modeling and specifying native labor supply; these include Card (2001, 26) and Amior and Manning (2020). In the model of Amior and Manning (2020), immigration itself alters the effective elasticity of foreign-native substitution by reducing other immigrants' wage-bargaining power. In the setting we study, as discussed above, the immigrant wage is centrally set by the federal government at the level prevailing for similar U.S. workers in the same industry and geographic area. It is fixed before the (random, unpredictable) immigrant employment shock occurs for each firm. We thus expect the firm-level shocks we study, per se, to have negligible effects on the elasticity of substitution.

such as native heterogeneity in natives' preferences over firms (Card et al. 2018; Manning 2021). Natives wages are then marked down from the marginal revenue product: $w_N = \left(1 + \frac{1}{e_N}\right)^{-1} \frac{\partial R}{\partial N}$. In the simple, illustrative case that ignores capital and high-skill labor ($\beta = \gamma = 0$), the effect of low-skill immigrant employment on low-skill native employment in equation (6) becomes

$$\ln \frac{N_w}{N_\ell} \approx s_I \cdot \frac{\eta - \sigma}{(\eta - 1)(1 - s_N) + (\sigma - 1)s_N + \frac{\sigma}{e_N}(\eta - 1)} \cdot \ln \frac{I_w}{I_\ell}, \tag{16}$$

derived in the Appendix. That is, the native employment response to immigration is increasing in the native labor supply elasticity, converging to equation (6) as $e_N \to \infty$.

The pre-analysis plan's prediction of larger treatment effects (16) in rural areas rested on the prediction of a higher elasticity e_N in rural than in urban areas. Understanding this requires a subtle distinction between e_N as defined here and the supply elasticity typically estimated in the monopsony literature.

A key driver of "modern" monopsony power in rural areas is their geographic remoteness from thick urban labor markets, implying frictions on physical movement and information transmission between those markets, as highighted by Pigou (1920, 508–513) and Robinson (1933, 256). This would tend to reduce rural workers' separation and recruitment elasticities, and thus their labor supply elasticity, to an alternative employer *in a distant urban area.* A consequence is relatively greater wage markdowns in rural areas (e.g. Azar et al. 2022; Bassier et al. 2022).

But the same frictions would tend to *raise* rural workers' supply elasticity to a nearby alternative employer *within the isolated district.* This is the supply elasticity e_N above. Intuitively, an alternative employer within the rural district experiencing a positive productivity shock—such as from receiving government permission to hire immigrant workers—would find it easier to recruit complementary rural native workers whose local wages were held further below their marginal product, such as by frictions in the nationwide labor market. The same employer experiencing the same shock in an urban area, where natives are paid closer to their marginal product, would have more difficulty recruiting natives away from their superior alternatives.[31] Beyond this, the greater diversity of workers and firms in urban relative to rural areas would

[31] This result is derived more formally with a simple Hotelling duopsony model in the Appendix.

tend to create relatively more "classical" monopsony power for urban employers. The Appendix presents a minimal formal spatial duopsony model of this intuitive distinction.

In sum, the model and pre-analysis plan predicted relatively larger treatment effects on revenue, U.S. employment, and investment for firms facing greater competition in the output market. It predicted relatively more positive treatment effects on U.S. employment in rural areas than urban areas.

Figure 7 graphically presents empirical tests of these predictions, in the first three columns of panels (a) through (c). The vertical axis in the three panels shows the effect of foreign employment on each outcome: the 2SLS regression coefficient on foreign temporary employment using the 'lottery win' instrument, corresponding to column 6 in each of Tables 2, 3, and 4. (Full regression results are in the Appendix.)

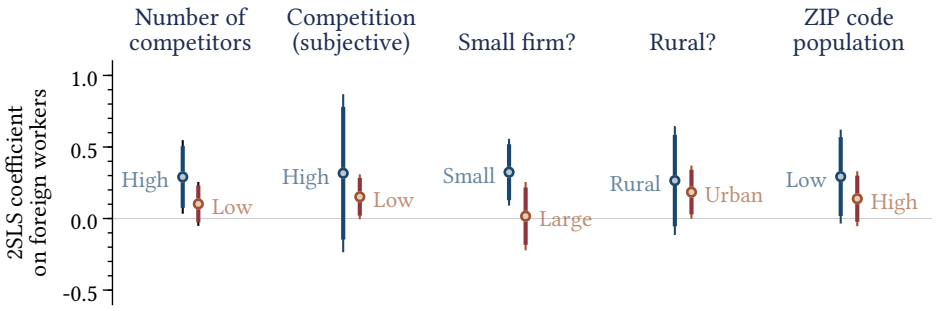
These tests support the theoretical prediction of heterogeneous effects on revenue by competitive environment (Figure 7a, cols. 1–3). In these tests, a firm is considered to face a 'high' *number of competitors* if it reports more than the median number, and 'low' otherwise. A firm is considered to face 'high' *subjective competition* if it reports that it would be 'very easy' (4 on a 4-point scale of ease) for competitors to steal its customers by underpricing. *Firm size* is considered 'small' if it had less than median revenue at baseline (in 2020).

Firms that face greater objective or subjective competition, and firms that are smaller, exhibit much larger effects of foreign employment on revenue. The revenue effect is three times higher for firms facing high competition relative to low competition, and is an order of magnitude higher for small businesses relative to large ones. This suggests that firms with greater output market power are less affected by exogenous changes in low-skill foreign employment, as predicted.

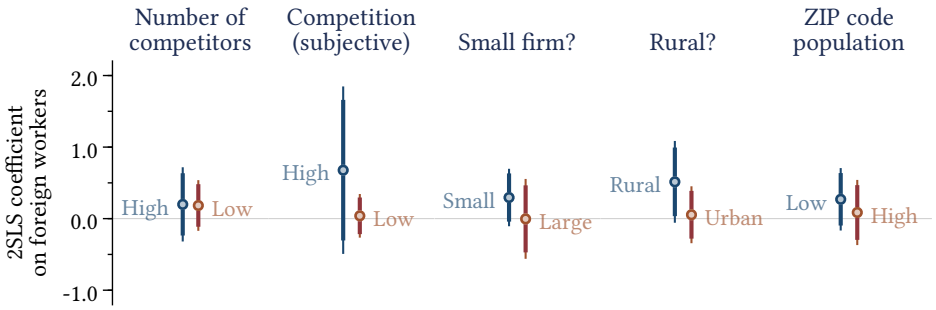
The tests also corroborate the predicted heterogeneity of the effect on employment by competitive environment, but less definitively (Figure 7b, cols. 1–3). For small businesses, as predicted, the U.S. employment effect is much larger in magnitude than for large ones, and more statistically precise ($p = 0.15$). The coefficient estimates for firms facing high subjective competition are much higher than for firms facing low subjective competition, but the coefficient estimates for

Figure 7: Heterogeneous effects of foreign workers employed, 2021

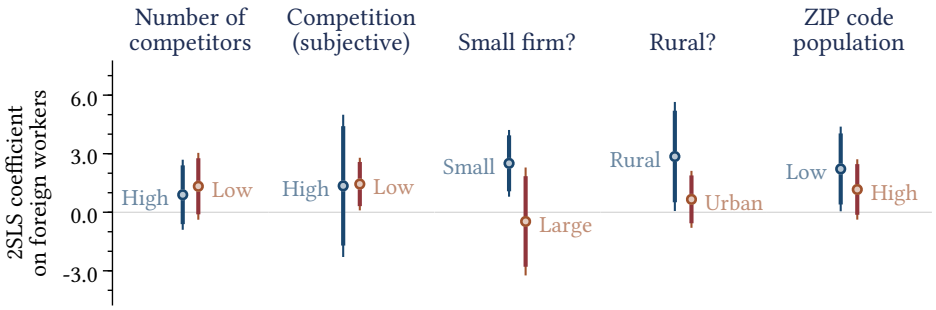
(a) *Effect on revenue*



(b) *Effect on U.S. temporary workers employed*



(c) *Effect on investment*



The unit of analysis is firms. The vertical axis in each pane shows the 2SLS coefficient on foreign workers employed (2021, IHS) in a regression with full baseline controls, corresponding to the specification in column 6 of Tables 2, 3, and 4. Thin vertical line shows 95% confidence interval, thick line shows 90% confidence interval. Each column shows contrasting mutually exclusive, collectively exhaustive sample restrictions according to some firm trait. "High" number of competitors means greater than the median response (25). "High" subjective competition means the business self-reported that it would be "very easy" (4 on a 4-point scale of ease) for competitors to steal their customers by underpricing them. "Small" firms are those with less than median revenue at baseline (in 2020). "Rural" firms are those whose ZIP code is classified by the Census Bureau as anything other than "Metropolitan Area, Core" (RUCA code 1). "Low" population means the firm's ZIP code has less than the median population among all ZIP codes (20,459 residents) in the 2010 full-count census. Full regression results in the Appendix.

firms facing high objective competition are similar in magnitutde to estimates for firms facing low objective competition.

The results are mixed for the investment outcome (Figure 7c, cols. 1–3). As predicted, the effect of foreign employment on investment is much greater—in fact, is only detectable—for small businesses. For them the causal elasticity is 2.51, implying that doubling foreign employment raises investment by an order of magnitude. No such pattern is clear in the heterogeneous results by objective or subjective degree of competition.

Second, the tests support the theoretical prediction of heterogeneous treatment effects on U.S. employment by rural/urban location. In the tests below, *firm location* is 'rural' if its postal code is classified by the U.S. Census Bureau as anything other than 'Metropolitan Area, Core' (RUCA code 1). As an alternate measure of rurality, firms' *local population* is 'low' if its postal code has less than the median population for all postal districts (<20,459 residents) in the 2010 full-count census.

These tests are graphically presented in the last two columns of Figure 7, panels (a) through (c), columns 4–5. The magnitude of the revenue effect is 55% greater in rural areas relative to urban areas, and 110% greater in low-population postal areas relative to high-population areas. The magnitude of the U.S. employment effect is 9.5 times greater in rural areas than urban areas—and nears statistical significance at conventional levels (Anderson-Rubin $p = 0.087$)—and is 3.1 times greater in low-population postal areas. The investment effect is 4.3 times higher in rural areas—and statistically significant (Anderson-Rubin $p = 0.016$)—and is 1.9 times greater in low-population postal areas.

In sum, the core results in this paper exhibit substantial heterogeneity across different predetermined firm types. The treatment effect on revenue is substantially larger for firms that are small relative to their output market (face greater competition) or small in absolute terms, consistent with the model and the prespecified predictions. The treatment effect on U.S. employment is substantially larger in rural areas than urban areas, consistent with the model and prespecified predictions, and consistent with greater native wage markdowns in isolated rural areas. The investment results are consistent with small firms and rural firms facing more binding capital

constraints, which tend to magnify the effects of shocks (Ghosal and Loungani 2000).

These results furthermore suggest high robustness of the core findings in Tables 2–4. The sign of the effect measured in the core results, for example, does not change in 28 of the 30 of the prespecified subsamples in Figure 7.

8.3 Estimates of the foreign-native elasticity of substitution

The regression results in Section 6 can yield estimates of the foreign-native *effective elasticity of substitution* at the firm level, as defined in Section 8.1. We interpret the regression coefficients in Table 3 as estimates of the expression in equation (7), which can be solved for σ.[32] This requires empirical estimates of the other parameters: the price elasticity of output demand η, the capital elasticity of output β, the high-skill labor elasticity of output γ, and the native share of the low-skill labor nest $1 - \alpha$.

Output demand elasticity: First, we estimate the output price elasticity $\eta \approx 8$ for the industries that principally employ H-2B workers, where concentration is typically low. This is based on markup estimates for related low-skill service industries in the United States. A firm maximizing profits by the Lerner (1934) Rule sets markup $m = \frac{\eta}{\eta-1}$. Thus the estimates by De Loecker et al. (2020, Appendix p. 23) of low-skill service sector markups 1.12 for "accommodation and food services", 1.12 for "wholesale trade", and 1.16 for "construction", imply demand elasticity η = 7.3–9.3. Likewise Christopoulou and Vermeulen's (2012, 74–75) markup estimates of 1.12 for "food and beverages" and 1.15 for "hotels and restaurants" in the U.S. imply η in the range of 7.7–9.3. Concentration is generally low in the landscaping, seafood preparation, and forestry services industries.[33]

[32] The elasticity we estimate here is *not* the purely technical substitution elasticity σ in equation (16), but the elasticity including both purely technical substitution and institutional features that Hicks, 10 described as necessary "to begin interpreting facts".

[33] Details in the Appendix. The exception among typical H-2B employers is Amusement Parks, an industry where concentration is generally high, but these are not represented in the survey sample here, where 2% of respondents report their industry as *temporary outdoor carnivals*—where concentration is much lower than large, fixed amusement parks. Average U.S. workers in similar markets and similar occupations to H-2B workers face low rates of concentration and monopsony power, in the relevant worker-weighted estimates of Gibbons et al. (2019, Fig. 2, col. 4).

Capital elasticity of output: Second, we estimate the capital elasticity of output $\beta \approx 0.35$. Detailed profiles of the industries employing the vast majority of H-2B workers yield estimates of the capital share of revenue at 0.292–0.310 in the most common industries employing H-2B workers (landscaping/groundskeeping and hospitality), which corresponds to $\beta = 0.3 \cdot \frac{\eta}{\eta-1} \approx 0.35$ under $\eta = 8$. The other relevant industries' capital shares fall between the extremes 0.24–0.45, corresponding to $\beta \approx 0.27$–0.51.[34]

Labor shares: Finally, the share of year-round native employees in total employment in the core firm survey sample is 0.470 (std. err. 0.0659, $N = 251$), implying $\gamma = 0.470 \cdot (1 - s_K) \cdot \frac{\eta}{\eta-1} = 0.349$ at $s_K = 0.35$ and $\eta = 8$. The share of native workers in the inner (low-skill) labor nest in the survey sample is $1 - \alpha = 0.648$ (std. err. 0.0572, $N = 251$).

These parameters allow us to estimate the value of σ implied by the regressions in Section 6. The results are presented in Table 7. Our preferred estimate uses the parameter estimates in the middle of the plausible ranges above: $\eta = 8$, $\gamma = 0.35$, and $\beta = 0.35$. These yield the estimate $\sigma = 1.22$, with a 95% confidence interval (0.0789, 2.366), in the center of the table. The remainder of the table shows how this estimate changes under a wide range of different assumptions on the base parameters.

Regardless of these varying assumptions on the other parameters of the model, the empirical estimates in Table 3 imply values of σ that never fall outside the range 0.8–2.2. This suggests that low-skill foreign workers and low-skill U.S. workers are very poor substitutes at the marginal firm. In other words, though influential studies have rested on the assumption of perfect substitutability between low-skill immigrants and natives ($\sigma \equiv \infty$, reviewed by Card and Peri 2016, 1345), the tests presented here strongly reject interpretation of such studies as informative about the magnitude or sign of the Policy-Relevant Treatment Effect (PRTE) from a marginal expansion of low-skill work visas.

It is useful to compare these firm-level estimates of the PRTE to other, inherently different estimates of the foreign-native elasticity. Our preferred estimate of $\sigma = 1.2$ is somewhat lower than prior, already-low estimates measured in the aggregate rather than at the firm level. Cortés

[34] Details in the Appendix. Capital share is specified as depreciation, amortization, rent, and net income as a share of gross profit, that is, revenue minus cost of goods sold (less taxes and insurance).

Table 7: Estimates of the foreign-native effective elasticity of substitution σ

	$\eta = 4$			$\eta = 8$			$\eta = 16$		
$\gamma =$	0.25	0.35	0.45	0.25	0.35	0.45	0.25	0.35	0.45
$\beta = 0.25$	1.465	1.222	1.031	1.863	1.439	1.146	2.172	1.586	1.218
	(0.549)	(0.525)	(0.522)	(0.596)	(0.556)	(0.544)	(0.608)	(0.568)	(0.555)
$\beta = 0.35$	1.294	1.070	0.896	1.595	1.222	0.967	1.825	1.324	1.011
	(0.564)	(0.545)	(0.542)	(0.627)	(0.583)	(0.565)	(0.663)	(0.606)	(0.579)
$\beta = 0.45$	1.123	0.922	0.766	1.336	1.016	0.799	1.496	1.078	0.819
	(0.576)	(0.559)	(0.554)	(0.643)	(0.595)	(0.570)	(0.689)	(0.619)	(0.580)

Delta-method standard errors in parentheses. Derived from solving equation (7) for σ. Uses $\beta = 0.35$ and native share of the inner labor nest 0.648, estimated from the U.S. employment regression in Table 3, col. 10, using the core firm sample $N = 251$. Details in Appendix.

(2008, 411) estimates this elasticity at around 4, while other estimates fall in the range 4–10 (Peri and Sparber 2009; Peri 2011, 8; Ottaviano et al. 2013). These estimates include substitution of demand between firms, what Hicks called "commodity substitution" at the "community level", what is more recently known as Rybczynski effects. We can reject the hypothesis that the firm-level elasticity for H-2B visa employers that we estimate takes any of these values, given that the highest upper bound on any 95% confidence interval implied by Table 7 is 3.36.[35] But the relatively minor difference between our estimates that exclude Rybczynski effects, and other estimates that include them, corroborate the limited importance of Rybczynski effects that has been found in the literature.

8.4 Aggregation of firm-level estimates

The firm-level analysis in Section 6 need not imply aggregate effects of equal magnitude. GDP need not rise by a summation of the firm-level revenue effect; overall U.S. employment need not rise by a summation of the firm-level employment effects. As discussed above, these results are only strictly comparable with other firm-level analyses.

[35] Our estimate for the nonfarm economy is close to the very low foreign-native elasticity of 2.1 estimated for otherwise similar low-skill jobs in the farm-sector, where the H-2A visa offers analogous opportunities for farm work by foreign workers (Wei et al. 2019; Clemens 2022). Our estimate is similar to estimates of the very limited substitutability between all high- and low-skill workers in the U.S. economy, an elasticity estimated at 1.4 (Katz and Murphy 1992).

That said, this firm-level analysis contains some information about aggregate effects. Prima facie, we would expect adding more of a factor that did not exist before to raise GDP by some amount in any plausible national production function. An increase in GDP would be difficult to observe without observing a substantial increase in GDP at the most-affected firms. And it is difficult to posit a theoretical mechanism for substantial crowding out of native employment in the aggregate if we observe no crowding out at the firm level—and even crowing in of native employment in rural areas (Figure 7b).

The precise aggregate effect, however, depends on the mechanism of the treatment effect at the firm level. Two mechanisms are possible in principle. If firms that win the lottery produce more, but firms that lose the lottery produce no less, the firm-level treatment effect is a reliable first-order estimate of the effect in aggregate. On the other hand, if firms that win the lottery simply take over market share from firms that lose the lottery, a large firm-level effect on production is compatible with a smaller effect on aggregate production. That is, the positive effect of the lottery on revenue in 2021 (Table 2) could in principle arise from an expansion of economic activity overall, or from a reallocation of business from lottery-losers to lottery-winners that is neutral with respect to aggregate revenue.

We conduct three tests to address this question, none of which were pre-registered because aggregate effects were not considered in the pre-analysis plan.

First, we test for an effect of the lottery outcome on the competitive environment reported by firms. If the large magnitude of the effect on output were driven by lottery-losing firms losing market share to lottery-winning competitors, we would expect to observe lottery-losing firms reporting—after the work season is over—that it is relatively easier for competitors to steal their customers. But the treatment effect of losing the lottery on subjective competition is very small in magnitude and statistically indistinguishable from zero (results reported in the Appendix).

Second, a different test arises from the partial replication in 2020 discussed in Section 7.2. If aggregate-revenue-neutral reallocation were the principal mechanism for the treatment effect, we would expect to observe much larger revenue effects in 2020 than in 2021. This is because in 2020, losing firms greatly outnumbered winning firms (Figure 4). In 2021, winning firms

outnumbered winning firms, implying that there was far less 'business to steal' from lottery-losers. The magnitude of the revenue effect of winning the lottery is broadly similar between the two years, suggesting that zero-sum reallocation of a fixed amount of business activity is not a primary driver of the firm-level effects.

Finally, suppose that the firm-level treatment effect on revenue consisted entirely of shifting a fixed amount of output demand from other firms to lottery-winning firms, without increasing aggregate production. In this case we might expect larger firm-level effects on revenue in urban areas, where there is more 'business to steal' from other firms in a larger output market. But the observed revenue effect is somewhat smaller in urban areas (Figure 7a).

Indeed, the results suggest the possibility that the aggregate effect exceeds the firm-level effect. The large, positive treatment effect of immigrant employment on firms' investment expenditures indirectly implies a substantial multiplier effect in the aggregate. These expenditures typically represent additional purchases of equipment, tools, vehicles, and structures, raising production in other firms and industries. A range of models imply that firm-level scale effects should be considered a lower bound on aggregate effects (e.g. di Giovanni et al. 2015; Mahajan 2022).

8.5 Black-market employment: A rough forensic test

We do not observe whether or not the firms in the survey sample employ unauthorized workers, either directly or through subcontractors. It is possible in principle that lottery-losing firms substitute unobserved black-market immigrant workers for the authorized immigrant workers they are barred from hiring. There are theoretical and empirical reasons to expect low bias, however, from unobserved black-market labor. First, profit-maximizing employers willing and able to hire substitutes for lost H-2B workers on the black market would have little incentive to pay the lottery-entry fees, fixed wages, travel costs, and administrative fees imposed on H-2B hiring by regulation but absent from the black market.[36] Empirically, Orrenius and Zavodny (2020) test for relationships between several different measures of immigration enforcement and

[36] Firms typically pay recruitment companies around US$4,000 up front fixed cost to petition for any H-2B workers, plus around $1,200 per worker for the first 20–30 workers, with scale discounts for larger petitions. The median of 11 workers per petition translates to roughly $17,000 paid per petition by the median firm. Beyond this, wages paid to H-2B workers are fixed by DOL at a rate well above the minimum wage (e.g. Read 2006, 450).

firms' demand for H-2B visas, finding no systematic relationship. More generally, and within U.S. firms, Hotchkiss et al. (2015) and Zhu et al. (2020) find extremely low substitution between black market employment and native workers in very basic low-skill jobs.

Beyond this, the estimated treatment effects in Section 6 are incompatible with a high degree of substitutability between black-market employment and H-2B employment. If firms had access to unauthorized workers that were perfect substitutes for authorized workers, basic theory would predict zero effect of losing the lottery on firm revenue, investment, and profit. Thus Tables 2, 4, and 5 can be interpreted as testing, and strongly rejecting, the hypothesis of perfect substitution between observed, authorized immigrants and unobserved, unauthorized immigrants.

We can go further, however, to construct a forensic test for the degree of bias to the estimated treatment effect from unobserved black-market employment. The key is that we observe all other inputs to production, and we observe firm revenue—which is a consequence of both observed and unobserved inputs. This allows us to roughly estimate the plausible contribution of unobserved inputs. Suppose that the treatment effect of winning the lottery on revenue is given by equation (5). But now, the true total employment of immigrant workers by lottery-losing firms is greater than the observed employment of authorized immigrants: $I_\ell^* \equiv (1+\phi)I_\ell$ such that $\phi > 0$ is the number of unobserved immigrant employees as a fraction of the number of observed immigrant employees. Solving for the unobserved ϕ gives

$$\phi \approx \underbrace{\ln \frac{I_w}{I_\ell}}_{\sim 0.822} + \underbrace{\frac{s_N}{s_I} \ln \frac{N_w}{N_\ell}}_{\sim 0.269} - \underbrace{\frac{1-s_K}{s_I} \ln \frac{R_w}{R_\ell}}_{\sim 1.042} = 0.049, \tag{17}$$

where the values on the right hand side are filled in from the empirics above.[37] This estimate of ϕ is not statistically precise. But its small magnitude illustrates that the fall in revenue is not just nonzero for lottery-losing firms. The fall in revenue is so large as to require the almost the entire resulting fall in *observed* immigrant and U.S. employment to explain it. The same analysis would apply regardless of whether unauthorized immigrants are direct employees unreported by survey respondents or indirect employees concealed within subcontractors. Note that if the

[37] The estimate of $\ln \frac{I_w}{I_\ell} = 0.822$ is from the first-stage regressions in the Appendix; the estimate of $\ln \frac{N_w}{N_\ell} = 0.146$ is from Table 3, col. 4; the estimate of $\ln \frac{R_w}{R_\ell} = 0.17$ is from Table 2 col. 4; $s_K = s_H = 0.35$ and $\frac{s_N}{s_I} = 0.648/(1-0.648) = 1.84$, and thus $s_I = (1-0.648)\times(1-s_K-s_H) = 0.106$ are from Section 8.3; and $s_I \approx (1-0.648)\times(1-0.7)\frac{\eta-1}{\eta} = 0.121$.

treatment effect on U.S. employment is actually zero in equation (17), a hypothesis that cannot be rejected at conventional levels in Table 3, the evidence would become incompatible with any positive value of ϕ. In sum, the empirical estimates offer forensic evidence incompatible with a substantial shift by lottery-losing firms into black-market employment.

9 Conclusion

The U.S. has a long history of limiting contract foreign labor for low-skill work.[38] In this tradition, H-2B visas are quota restricted, by law, to avoid "adversely affect the wages and working conditions of similarly-employed U.S. workers."[39] While plausible, these concerns run counter to employers' plausible counterclaims that the survival of their businesses depends on access to foreign workers for low-skill jobs (e.g. Casanova and McDaniel 2005, 64; Blinn et al. 2021, 3). Neither claim has been subjected to sufficient scrutiny.

The effecitvely randomized allocation of H-2B visas to firms in recent years provides a strong basis for such an evaluation. Our novel survey of a sample of the firms who participated in the 2021 lottery reveals little benefit, and substantial costs, due to restricting firms' access to these visas. Comparing firms that were able to hire more workers on these visas to those that were able to hire fewer—by random chance—we find that gaining access to immigrant hires raises firm revenues (elasticity with respect to immigrant hires of +0.16) and also weakly raises, rather than lowers, their employment of U.S. workers (elasticity +0.10). This is a robust result that holds in several pre-registered subsamples. It is larger at both rural firms (consistent with native labor supply being elastic in such markets) and at firms facing more competition (consistent with the finding of Burstein et al. [2020] that the labor market impact of U.S. immigration is more positive for firms facing more price-elastic output demand). It also holds, with slightly less strength, in the similar 2020 visa lottery.

Why are the effects so uniformly positive despite widespread priors of a harm to natives? Our model and additional evidence suggest that it is because there are simply few substitutes for the

[38] The U.S. is certainly not alone in this practice, however. Indeed, a striking feature of international migration is the importance of the emigration of *high-skill* immigrants from low-skill countries to high-skill countries (e.g. Artuç et al. 2015).

[39] *Federal Register* May 25, 2021, 86 *FR* 28203

labor provided by legally authorized low-skill workers. First, pushing our estimates (of either the employment or revenue response) through a standard model of the labor market used in the immigration literature, we find that U.S. workers do not substantially substitute for foreign workers on H-2B visas. Second, unlike in other low-skill industries like agriculture (e.g. Clemens et al. 2018; San 2022) or manufacturing (e.g. Lewis 2011) there appears to be little potential to simply "automate away" labor shortages. Indeed, we find that H-2B hires are associated with an increase in capital investment (elasticity +1.03), suggesting that capital is a complement, rather than a substitute for H-2B workers. Finally, a simple forensic analysis shows little sign that lottery losing firms turn to unauthorized labor, suggesting that the unauthorized are not a viable substitute for legally hired workers, either.[40]

What do these findings imply about the likely impact of increasing the H-2B visa quota? There is some potential for our estimates overstate the aggregate impact of H-2B visas, as "winning" firms may be, to some extent, stealing some business from "losing" firms. On the other hand, the group that is likely the largest beneficiary of the program—immigrant workers and their families (Gibson and McKenzie 2014; Bossavie et al. 2022)—is not subject to this concern. There are also compelling reasons to think that there are benefits of increasing the H-2B visas quota that our short-run estimates under a fixed quota do not fully capture.[41] Unlike a one-time lottery, from a firm's point of view a quota increase is tantamount to a permanent increase in the chances of being allocated an H-2B visa. This would reduce uncertainty and thus likely lead to larger responses (Ghosal and Loungani 2000). For example, a permanent increase seems likely to induce a greater response of investment and (likely) the hiring of year-round employees (we found no response), both of which likely complement the hiring of U.S. seasonal workers.

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[40] Informal comments given to us by those who work in the industry, outside of our survey (which studiously avoided directly asking about unauthorized hires) suggest some reasons for this. First, firms suggest there is a substantial business risk to hiring unauthorized labor. Second, they suggest that there may be severe limitations on what unauthorized workers are able to do in many locations; for example, in 32 of the 50 U.S. states they cannot legally drive vehicles. Finally, unauthorized labor may simply be unavailable in many of the locations where these firms operate.

[41] Even within this environment, there are likely increases in native employment at supplier firms we do not measure, induced by the investment response.

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Online Appendix

"The effect of low-skill immigration restrictions on US firms and workers: Evidence from a randomized lottery"

Michael A. Clemens and Ethan G. Lewis — December 2022

In this Appendix we present derivations of the model in the main text, a discussion of monopsony power in rural labor markets, summary statistics for the firm sample (with comparisons of selected traits to the firm universe), and numerous extensions of the empirical analysis, some prespecified and others not.

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A1 Derivations

While we will ultimately execute derivations for CES production function shown in (3), let us begin with the general setup underlying proposition 1. Inverting the demand function (1) as $p = D^{\frac{1}{\eta}}Q^{-\frac{1}{\eta}}$, we have that revenues, $R = Q(p)p = D^{\frac{1}{\eta}}Q^{\frac{\eta-1}{\eta}}$. The firm's problem is to maximize profits

$$\Pi(I, N, K) = D^{\frac{1}{\eta}}Q^{\frac{\eta-1}{\eta}} - w_I I - w_N N - rK - \mathcal{F}$$

subject to $I \leq \bar{I}$ (if it faces the hiring constraint). In summary, they maximize the objective function:

$$\mathcal{L}(I, N, K) = D^{\frac{1}{\eta}}Q^{-\frac{\eta-1}{\eta}} - w_I I - w_N N - rK - \mathcal{F} + \lambda(I - \bar{I})$$

where $\lambda = 0$ for an unconstrained firm. This produces the following first order conditions:

$$\frac{\eta-1}{\eta}D^{\frac{1}{\eta}}Q^{-\frac{1}{\eta}}\frac{\partial Q}{\partial I} = w_I + \lambda \tag{A.1}$$

$$\frac{\eta-1}{\eta}D^{\frac{1}{\eta}}Q^{-\frac{1}{\eta}}\frac{\partial Q}{\partial N} = w_N \tag{A.2}$$

$$\frac{\eta-1}{\eta}D^{\frac{1}{\eta}}Q^{-\frac{1}{\eta}}\frac{\partial Q}{\partial K} = r \tag{A.3}$$

...and $I \leq \bar{I}$ for a constrained firm. Notice that λ represents a positive wedge between a firm's marginal revenue product of immigrant labor and immigrant wages for constrained firms.

In light of this, we can compute optimal total costs as follows:

$$\begin{aligned} C^*(I, N, K, \mathcal{F}) &= w_I I + w_N N + rK + \mathcal{F} \\ &= \frac{\eta-1}{\eta}D^{\frac{1}{\eta}}Q^{-\frac{1}{\eta}}\left(\frac{\partial Q}{\partial I}I + \frac{\partial Q}{\partial N}N + \frac{\partial Q}{\partial K}K\right) + \mathcal{F} - \lambda I \\ &= \frac{\eta-1}{\eta}D^{\frac{1}{\eta}}Q^{\frac{\eta-1}{\eta}} + \mathcal{F} - \lambda I \end{aligned}$$

where the last step follows from homogeneity. Optimal profits are then given by

$$\Pi^* = R^* - C^* = D^{\frac{1}{\eta}}Q^{\frac{\eta-1}{\eta}} - \left(\frac{\eta-1}{\eta}D^{\frac{1}{\eta}}Q^{\frac{\eta-1}{\eta}} + \mathcal{F} - \lambda I\right) \tag{A.4}$$

$$= \frac{1}{\eta}D^{\frac{1}{\eta}}Q^{\frac{\eta-1}{\eta}} - \mathcal{F} + \lambda I \tag{A.5}$$

Now let us contrast constrained and unconstrained firms. Since unconstrained firms can freely choose I – and, in particularly, could choose $I_u \leq \bar{I}$, it must be that unconstrained firms have profits that are at least as large as constrained firms, and therefore $\frac{1}{\eta}D^{\frac{1}{\eta}}Q_u^{\frac{\eta-1}{\eta}} \geq \frac{1}{\eta}D^{\frac{1}{\eta}}Q_c^{\frac{\eta-1}{\eta}} + \lambda I_c$. But this implies that unconstrained revenues are weakly higher $D^{\frac{1}{\eta}}Q_u^{\frac{\eta-1}{\eta}} \geq D^{\frac{1}{\eta}}Q_c^{\frac{\eta-1}{\eta}}$, and therefore that unconstrained output is weakly higher $Q_u \geq Q_c$. Rearranging, the proportional revenue increase induced by a relaxation of the

hiring constraint is larger the larger is the demand elasticity:

$$\ln(R_u/R_c) = \frac{\eta-1}{\eta}\ln(Q_u/Q_c)$$

As proposition 1 says, however, profit rates are not necessarily higher in the unconstrained firms:

$$\Pi_u/R_u - \Pi_c/R_c = \mathcal{F}D^{-\frac{1}{\eta}}\left(Q_c^{-\frac{\eta-1}{\eta}} - Q_u^{-\frac{\eta-1}{\eta}}\right) - \lambda I_c D^{-\frac{1}{\eta}} Q_c^{-\frac{\eta-1}{\eta}}$$

The first term of the expression is positive, but the second one is negative, so the impact on profit rates is ambiguous. This is because while relaxing the hiring constraint allows output and profits to increase (first term), it also reduces the wedge between the marginal revenue product of immigrant labor and wages (second term), reducing revenues and profits. The more important fixed costs are, the more the first term dominates, and the likely the impact on profit rates is to be positive. The impact on profit rates is also more likely to be positive at higher demand elasticities.

A1.1 Nested CES

We proceed to the CES production function (3) in steps. Returning to the full version in the next section, let us first consider a simpler version without permanent labor ($\gamma = 0$), which implies that revenue

$$R = D^{\frac{1}{\eta}} z^{\frac{\eta-1}{\eta}} K^{\beta\frac{\eta-1}{\eta}} \left(\alpha I^{\frac{\sigma-1}{\sigma}} + (1-\alpha)N^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)} \tag{A.6}$$

In this case, the first order conditions become:

$$\frac{\eta-1}{\eta}(1-\beta)D^{\frac{1}{\eta}} z^{\frac{\eta-1}{\eta}} K^{\beta\frac{\eta-1}{\eta}} \left(\alpha I^{\frac{\sigma-1}{\sigma}} + (1-\alpha)N^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)-1} (1-\alpha)I^{-\frac{1}{\sigma}} = w_I(+\lambda) \tag{A.7}$$

$$\frac{\eta-1}{\eta}(1-\beta)D^{\frac{1}{\eta}} z^{\frac{\eta-1}{\eta}} K^{\beta\frac{\eta-1}{\eta}} \left(\alpha I^{\frac{\sigma-1}{\sigma}} + (1-\alpha)N^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)-1} (1-\alpha)N^{-\frac{1}{\sigma}} = w_N \tag{A.8}$$

$$\beta\frac{\eta-1}{\eta}D^{\frac{1}{\eta}} z^{\frac{\eta-1}{\eta}} K^{\beta\frac{\eta-1}{\eta}-1} \left(\alpha I^{\frac{\sigma-1}{\sigma}} + (1-\alpha)N^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)} = r \tag{A.9}$$

Solving for the total impact on factor demand of relaxing the immigrant hiring constraint uses the fact that these first order conditions hold in both constrained and unconstrained cases, and factor prices remain the same. For example, there is the well-known fact the Cobb-Douglass outer nest implies that capital's share is a constant:

$$\frac{rK_w}{R} = \frac{rK_\ell}{R} = \beta\frac{\eta-1}{\eta} = s_K \tag{A.10}$$

which implies $\ln(K_w/K_\ell) = \ln(R_w/R_\ell)$, per the lemma. Recall that substituting this into (2) also delivers (5), repeated here:

$$\ln(R_w/R_\ell) = \ln(K_w/K_\ell) \approx \frac{s_I}{1-s_K}\ln(I_w/I_\ell) + \frac{s_N}{1-s_K}\ln(N_w/N_\ell) \tag{A.11}$$

For native employment, we can use the equality of (A.8) at different factor mixes:

$$\frac{\eta-1}{\eta}(1-\beta)D^{\frac{1}{\eta}}z^{\frac{\eta-1}{\eta}}K_w^{\beta\frac{\eta-1}{\eta}}\left(\alpha I_w^{\frac{\sigma-1}{\sigma}}+(1-\alpha)N_w^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)-1}(1-\alpha)N_w^{-\frac{1}{\sigma}}=w_N$$
$$=\frac{\eta-1}{\eta}(1-\beta)D^{\frac{1}{\eta}}z^{\frac{\eta-1}{\eta}}K_\ell^{\beta\frac{\eta-1}{\eta}}\left(\alpha I_\ell^{\frac{\sigma-1}{\sigma}}+(1-\alpha)N_\ell^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)-1}(1-\alpha)N_\ell^{-\frac{1}{\sigma}}$$

to get that

$$-\frac{1}{\sigma}\ln(N_w/N_\ell)=-s_K\ln(K_w/K_\ell)-\left[\frac{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)-1}{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)}\right]\ln\left(\frac{\alpha I_w^{\frac{\sigma-1}{\sigma}}+(1-\alpha)N_w^{\frac{\sigma-1}{\sigma}}}{\alpha I_\ell^{\frac{\sigma-1}{\sigma}}+(1-\alpha)N_\ell^{\frac{\sigma-1}{\sigma}}}\right)^{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)} \tag{A.12}$$

where the ugly ratio of parameters in front of the second term on right hand side is to get the second term back into the form it was in the revenue function, (A.6). This allows us to construct the approximation:[42]

$$\ln(N_w/N_\ell)\approx\sigma s_K\ln(K_w/K_\ell)+\sigma\left[\frac{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)-1}{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)}\right][s_I\ln(I_w/I_\ell)+s_N\ln(N_w/N_\ell)] \tag{A.13}$$

After collecting the $\ln(N_w/N_\ell)$ terms, we have that

$$\ln(N_w/N_\ell)\approx\frac{\sigma s_K}{c_1}\ln(K_w/K_\ell)+\frac{\sigma s_I}{c_1}\left[\frac{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)-1}{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)}\right]\ln(I_w/I_\ell) \tag{A.14}$$
$$=\frac{\sigma s_K}{c_1}\ln(K_w/K_\ell)+\frac{s_I}{c_1}\left[\frac{\sigma(\eta-1)(1-\beta)-\eta(\sigma-1)}{(\eta-1)(1-\beta)}\right]\ln(I_w/I_\ell) \tag{A.15}$$

where

$$c_1=1-\sigma s_N\left[\frac{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)-1}{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)}\right]$$
$$=\frac{(1-\beta)[(\eta-1)(1-s_N)+(\sigma-1)s_N]+\beta s_N\eta(\sigma-1)}{(\eta-1)(1-\beta)}>0$$

That c_1 is larger than zero comes from the fact that the numerator is a weighted average of positive parameters $(\eta-1,\sigma-1)$ and the denominator is also positive for a similar reason.

Before fully solving this, (A.15) reveals the results for intuitive the two factor case (in which we also impose $\beta=0$ so $s_K=0$) that was given in the lemma:

$$\ln(N_w/N_\ell)\approx s_I\frac{\eta-\sigma}{\eta(1-s_N)+\sigma s_N-1}\ln(I_w/I_\ell)$$

...which is positive whenever $\eta>\sigma$.

[42] This comes from applying (2) to (A.6), taking out the (ln separable) part assigned to capital.

To include the adjustment of capital, we substitute the expression for capital, (A.11), into (A.15) :

$$\begin{aligned}
\ln(N_w/N_\ell) &\approx \frac{\sigma s_K}{c_1}\left[\frac{s_I}{1-s_K}\ln(I_w/I_\ell)+\frac{s_N}{1-s_K}\ln(N_w/N_\ell)\right]+\frac{s_I}{c_1}\left[\frac{\sigma(\eta-1)(1-\beta)-\eta(\sigma-1)}{(\eta-1)(1-\beta)}\right]\ln(I_w/I_\ell)\\
&=\left[1-\frac{\sigma s_K}{c_1}\frac{s_N}{1-s_K}\right]^{-1}\left[\frac{\sigma s_K}{c_1}\frac{s_I}{1-s_K}+\frac{s_I}{c_1}\frac{\sigma(\eta-1)(1-\beta)-\eta(\sigma-1)}{(\eta-1)(1-\beta)}\right]\ln(I_w/I_\ell)\\
&=\left[\frac{c_1(1-s_K)}{c_1(1-s_K)-\sigma s_K s_N}\right]\left[\frac{\sigma s_K}{c_1}\frac{s_I}{1-s_K}+\frac{s_I}{c_1}\frac{(1-s_K)}{(1-s_K)}\frac{\sigma(\eta-1)(1-\beta)-\eta(\sigma-1)}{(\eta-1)(1-\beta)}\right]\ln(I_w/I_\ell)\\
&=s_I\left[\frac{\sigma s_K(\eta-1)(1-\beta)+(1-s_K)\left[\sigma(\eta-1)(1-\beta)-\eta(\sigma-1)\right]}{(\eta-1)(1-\beta)[c_1(1-s_K)-\sigma s_K s_N]}\right]\ln(I_w/I_\ell)
\end{aligned}$$

Some algebra, plus the fact that $s_K \times \eta = \beta(\eta-1)$ from (A.10), simplifies the numerator of this to:

$$\sigma s_K(\eta-1)(1-\beta)+(1-s_K)\left[\sigma(\eta-1)(1-\beta)-\eta(\sigma-1)\right]=(\eta-1)(1-\beta)-(\sigma-1) \tag{A.16}$$

We can now we can write a simpler expression for $\ln(N_w/N_\ell)$:

$$\ln(N_w/N_\ell)=s_I\left[\frac{(\eta-1)(1-\beta)-(\sigma-1)}{c_2}\right]\ln(I_w/I_\ell), \tag{A.17}$$

where $c_2 \equiv (\eta-1)(1-\beta)[c_1(1-s_K)-\sigma s_K s_N]$. The sign of c_2 is unfortunately not straightforward to determine, but it is positive in admitted ranges of parameter values. It can be rewritten further by defining the numerator of c_1 as $c_1^{num} = c_1 \times (\eta-1)(1-\beta)$ which gives

$$c_2 = c_1^{num}(1-s_K)-\sigma s_N s_K(\eta-1)(1-\beta). \tag{A.18}$$

A1.2 Including Permanent Labor

To carry out accurate simulations of the model, we need to account for the substantial role permanent employees appear to take in production (see summary statistics in Table A1), even if their employment is not adjusting to seasonal fluctuations in the employment of other factors. So now using (3) from above, that is,

$$Q = zH^\gamma K^\beta\left(\alpha I^{\frac{\sigma-1}{\sigma}}+(1-\alpha)N^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}(1-\beta-\gamma)}, \tag{A.19}$$

we have the revenue function:

$$R = D^{\frac{1}{\eta}} z^{\frac{\eta-1}{\eta}} H^{\gamma\frac{\eta-1}{\eta}} K^{\beta\frac{\eta-1}{\eta}}\left(\alpha I^{\frac{\sigma-1}{\sigma}}+(1-\alpha)N^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta-\gamma)}. \tag{A.20}$$

We assume that permanent labor does not adjust to winning and losing the lottery, but rather stays at its optimal level for the *expected* mix of other inputs. (One might imagine that there is a cost of recruiting or firing permanent employees that make such adjustments not cost effective within a season.) A brief aside on this: larger changes not being considered in this model – like changes in visa quota, or permanent changes to demand conditions – could still impact on permanent employment. An expansion of the number of H2-B visas available might have a different – and likely larger – impact on revenue and native-born season employment at the average firm than simply "winning" a single year's lottery.

Because H is fixed, the expressions above largely hold – for example the revenue growth identity stays the same (2) – but the factor shares need to be adjusted in some cases. (A.11), describing the responses of

revenues and capital to N and I, holds as is. (A.15), describing N's response, requires adjustment to

$$\ln(N_w/N_\ell) \approx \frac{\sigma s_K}{c_3}\ln(K_w/K_\ell) + \frac{s_I}{c_3}\left[\frac{\sigma(\eta-1)(1-\beta-\gamma)-\eta(\sigma-1)}{(\eta-1)(1-\beta-\gamma)}\right]\ln(I_w/I_\ell) \tag{A.21}$$

where

$$\begin{aligned} c_3 &= 1-\sigma s_N\left[\frac{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta-\gamma)-1}{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta-\gamma)}\right] \\ &= \frac{(1-\beta-\gamma)[(\eta-1)(1-s_N)+(\sigma-1)s_N]+(\beta+\gamma)s_N\eta(\sigma-1)}{(\eta-1)(1-\beta-\gamma)} > 0 \end{aligned}$$

Carrying this through to the expression for (N_w/N_ℓ)

$$\ln(N_w/N_\ell) \approx \left[\frac{c_3(1-s_K)}{c_3(1-s_K)-\sigma s_K s_N}\right]\left[\frac{\sigma s_K}{c_3}\frac{s_I}{1-s_K}+\frac{s_I}{c_3}\frac{(1-s_K)}{(1-s_K)}\frac{\sigma(\eta-1)(1-\beta-\gamma)-\eta(\sigma-1)}{(\eta-1)(1-\beta-\gamma)}\right]\ln(I_w/I_\ell) \tag{A.22}$$

$$= s_I\left[\frac{\sigma s_K(\eta-1)(1-\beta-\gamma)+(1-s_K)\left[\sigma(\eta-1)(1-\beta-\gamma)-\eta(\sigma-1)\right]}{(\eta-1)(1-\beta-\gamma)[c_3(1-s_K)-\sigma s_K s_N]}\right]\ln(I_w/I_\ell) \tag{A.23}$$

$$= s_I\left[\frac{(\eta-1)(1-\beta-\gamma\sigma)-(\sigma-1)}{c_4}\right]\ln(I_w/I_\ell) \tag{A.24}$$

where $c_4 = c_3^{num}(1-s_K)-(\eta-1)(1-\beta-\gamma)\sigma s_K s_N > 0$ and c_3^{num} is the numerator of c_3 (when written out in long form), shown above. This implies that the response of native employment is positive if $(\eta-1) > \frac{(\sigma-1)}{1-\beta-\gamma\sigma}$, as was asserted in the lemma.

For revenues, we go back to (A.11) to obtain

$$\ln(R_w/R_\ell) \approx \frac{s_I}{1-s_K}\ln(I_w/I_\ell)+\frac{s_N}{1-s_K}\ln(N_w/N_\ell) \tag{A.25}$$

$$= \frac{s_I}{1-s_K}\left(1+s_N\left[\frac{(\eta-1)(1-\beta-\gamma\sigma)-(\sigma-1)}{c_4}\right]\right)\ln(I_w/I_\ell). \tag{A.26}$$

A1.3 Labor Supply

Now suppose that native labor supply to the firm is upward sloping, due to "modern monopsony" labor market frictions (Manning 2021) or "classical monopsony" heterogeneity in natives' preferences over firms (Card et al. 2018), with constant elasticity e_N. The first order condition then produces the well-known result that wages are marked down from the marginal revenue product R_N:

$$w_N = \left(1+\frac{1}{e_N}\right)^{-1} R_N, \tag{A.27}$$

where $w_N = a_N N^{\frac{1}{e_N}}$, and $a_N > 0$ is a constant. This leads to a modification of the expressions above. Ignoring capital and permanent labor for simplicity, notice that this alters (A.12) as follows:

$$\left(\frac{1}{e_N}+\frac{1}{\sigma}\right)\ln(N_w/N_\ell) = \left[\frac{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}-1}{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}}\right]\ln\left(\frac{\alpha I_w^{\frac{\sigma-1}{\sigma}}+(1-\alpha)N_w^{\frac{\sigma-1}{\sigma}}}{\alpha I_\ell^{\frac{\sigma-1}{\sigma}}+(1-\alpha)N_\ell^{\frac{\sigma-1}{\sigma}}}\right)^{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)} \tag{A.28}$$

Therefore we now have that:

$$\ln(N_w/N_\ell) \approx \frac{\sigma e_N}{\sigma + e_N} \left[\frac{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta} - 1}{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}} \right] [s_I \ln(I_w/I_\ell) + s_N \ln(N_w/N_\ell)] \tag{A.29}$$

As $\left[\frac{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}-1}{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}} \right] = \left[\frac{\eta-\sigma}{\sigma(\eta-1)} \right]$, after collecting terms this expression simplifies to:

$$\ln(N_w/N_\ell) \approx \frac{s_I \frac{\sigma e_N}{\sigma+e_N} \left[\frac{\eta-\sigma}{\sigma(\eta-1)} \right]}{1 - s_N \frac{\sigma e_N}{\sigma+e_N} \left[\frac{\eta-\sigma}{\sigma(\eta-1)} \right]} \ln(I_w/I_\ell) \tag{A.30}$$

$$= s_I \frac{e_N(\eta-\sigma)}{(\sigma+e_N)(\eta-1) - s_N e_N(\eta-\sigma)} \ln(I_w/I_\ell) \tag{A.31}$$

$$= s_I \frac{e_N(\eta-\sigma)}{e_N[(\eta-1)(1-s_N) + (\sigma-1)s_N] + \sigma(\eta-1)} \ln(I_w/I_\ell) \tag{A.32}$$

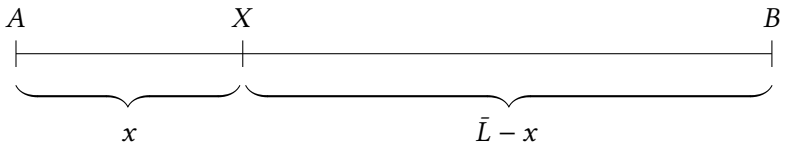
This is a modified version of the expression from Lemma 2, and shows that the native employment response to immigration is increasing in magnitude in the native labor supply elasticity. The response is zero when native supply is inelastic ($e_N = 0$), and converges to the expression in Lemma 2 as the elasticity increases.

A2 Imperfect competition and rural labor markets

The main text explained intuitively why the pre-analysis plan predicted less negative or more positive treatment effects of immigrant employment on native employment in rural areas relative to urban areas. This is a consequence of monopsony power in rural labor markets created by frictions in the national labor market between thin rural labor markets and thick urban labor markets. A consequence of those frictions is that the best alternative wage for two workers of identical marginal product can be lower in rural relative to urban areas. This would tend to make it easier for an alternative employer *within the rural area* to recruit "exploited" workers (Pigou's term) in rural areas away from their best local alternative. That is, the elasticity of firm-level labor supply to an alternative employer within the rural area—and thus not isolated from rural residents by transportation costs or information costs—would tend to be higher than in an urban area.

This can be seen somewhat more formally in a simple Hotelling duopsony model, following Monte and Pinheiro (2021). Consider two firms producing a single tradable product in perfect competition, firm A in a small, remote rural area and firm B in a large, densely populated urban area. Workers are identical except for their location. They are distributed evenly—by travel cost or information cost—on a segment between the two firms (Figure A1). The total labor supply is $\bar{L}$ and workers choose to supply labor to firm A or firm B. To work at either firm the worker incurs a cost κ per unit distance (transportation or information).

Figure A1: A rural-urban Hotelling model of labor-market duopsony



At location X, the marginal worker is indifferent between working for firm A at wage w_A or for firm B at wage w_B: $w_A - \kappa x = w_B - \kappa\left(\bar{L} - x\right)$. The optimum size of the rural labor supply is

$$x = \frac{\kappa\bar{L} + w_A - w_B}{2\kappa}.$$

That is, a necessary and sufficient condition for rural wages to be lower than urban wages ($w_A < w_B$) is for the rural labor market to be smaller than the urban labor market $\left(x < \frac{\bar{L}}{2}\right)$. Recalling that all workers have identical marginal revenue product, this implies that wage markdowns are greater in the rural area.

Consider now two different attempts by a third employer to recruit workers currently employed by firm A in the rural area. First, suppose a firm *in the urban area* tries to recruit those workers, by offering just above the going rate in the urban area, $w_b + \varepsilon$. The marginal supply of rural labor to that urban firm barely rises, by $\frac{\varepsilon}{2\kappa}$. That supply elasticity is lower as the friction κ increases. This is the finite-elasticity labor supply that produces the wage markdown in the rural area, induced by the frictions associated with firm A's remoteness.

Second, suppose a third firm *in the rural area* tries to recruit the same workers, those employed in the rural area. It offers just above the going rate in the rural area, $w_A + \varepsilon$. The marginal supply of rural labor to that *rural* firm, in this model, is infinitely elastic. All workers in the rural area (and a few just to the right of point X) would instantly supply their labor to the third firm.

Why, then, would the urban firm B not experience similarly infinitely-elastic labor supply within the urban area? To take an extreme case, suppose that the third firm's technology is such that the marginal revenue product of labor lies between w_A and w_B. Because the profit-maximizing firm cannot pay more than the marginal revenue product, at the margin the elasticity of labor supply to that firm would be zero if it were located in the urban area; it would be infinite if it were located in the rural area. In other words, workers in urban areas surrounded by high-productivity firms have better reserve options, reducing their elasticity of labor supply to the third firm.

A less extreme case of the same tendency, extending beyond the toy model above, is simply that of "classical monopsony" power originating from the existence of a range of firms with different productivity and different amenities, and a range of workers with different preferences (Card et al. 2018). The variation of firms and workers in a large, relatively diverse urban area would generally exceed the variation in small, more homogeneous rural area. This would create a greater tendency for less-than-infinite labor supply elasticities in urban areas than in rural areas, for reasons unrelated to spatial frictions in the worker's location choice.

A3 Summary statistics

Table A1 shows summary statistics across firms in the survey sample. The top panel shows firms time-varying traits in the second half of fiscal year 2021. The middle panel shows firms' time-varying traits in the second half of fiscal year 2020. The bottom panel shows firms' time-invariant traits.

A4 Compare sample to universe

Table A2 repeats the table in the main text comparing lottery results in the firm survey sample to lottery results in the sampling universe, this time restricting the survey sample to petitions by firms included in the core regression sample of 251 firms. The results are highly similar to the table in the main text. Fisher's exact test fails to reject the hypothesis that proportions of each lottery result in the core-sample petitions and the universe of petitions are identical, with $p = 0.501$.

Table A3 compares the industry breakdown of the number of requested workers in the sampling universe to the industry breakdown in the survey sample, for H-2B petitions entering the Dept. of Labor lottery for the second half of fiscal year 2021. The proportions of the industries among petitioned-for workers is broadly similar in the sample and universe. The ranking of industries in the sample and universe are similar, with the exception of forestry and construction: Forestry is overrepresented in the sample (ranked third in the sample, seventh in the universe) and construction is underrepresented in the sample (ranked eighth in the sample, third in the universe). In the sample relative to the universe, workers in hospitality and forestry are overrepresented by roughly 10 percentage points, while workers in groundskeeping/outdoor maintenance are underrepresented by 18 percentage points and workers in construction are underrepresented by 8 percentage points.

Table A4 displays the corresponding comparison by rural/urban location of the employer. The geographic distribution of firms in the sample corresponds closely to the distribution in the universe.

A5 First-stage regressions

Table A5 presents the first-stage regressions underlying the 2SLS estimates of Tables 2–5 in the main text. Both the 'lottery win' instrument and the 'expected share' instrument cause large and highly statistically significant increases in immigrant employment, conditional on predetermined firm traits. Losing the lottery reduces firms' employment of low-skill immigrants by $1 - e^{-0.822} = 56\%$.

A6 Industry-level parameter assumptions

Table A6 shows the sources and estimates of capital share for several of the leading industries for H-2B employment estimated by IBISWorld, a global research consultancy founded in 1971 in Australia, that compiles industry- and country-specific data including firms' typical costs structure. We include in the capital share: depreciation, amortization, rent, and net income (that is, operating profit minus insurance and taxes). A typical capital share in these industries is 0.3 (implying $\beta = s_K \cdot \frac{\eta}{\eta-1} \approx 0.35$ for $\eta \approx 8$), with a range of roughly 0.25 to 0.45 in plausible values ($\beta \approx 0.29$–0.51 for $\eta \approx 8$).

This leaves γ and α to be estimated for the firms in the core survey sample. The average firm's year-round U.S. employment as a fraction of total employment is 0.470 (std. err. 0.0659, $N = 251$). This implies $\gamma = 0.470 \cdot (1 - s_K) \cdot \frac{\eta}{\eta-1} = 0.349$ for $\eta = 8$. The average firm's share of foreign workers in all temporary employment is 0.544, implying a native share of the inner labor nest of 0.648 (std. err. 0.0572, $N = 251$).

IBISWorld rates concentration in each industry on a three-point scale. For all of the industries in Table A6 except 'amusement parks', it assesses concentration as 'low'. It describes the 'landscaping' industry in the

Appendix Table A1: Summary statistics

	mean	*sd*	*min*	*max*	*count*
2021 firm traits:					
Revenue ($)	6.6*e*+06	1.8*e*+07	5000.000	2.3*e*+08	258
Foreign temp. workers employed	28.882	65.138	0.000	630.000	289
U.S. temp. workers employed	48.460	173.163	0.000	1821.000	289
U.S. perm. workers employed	63.235	260.283	0.000	3600.000	289
Investment ($)	4.8*e*+05	2.1*e*+06	0.000	3.0*e*+07	285
ln Revenue	14.786	1.329	8.517	19.232	258
ihs Foreign temp. workers employed	2.818	1.718	0.000	7.139	289
ihs Foreign temp. workers requested	3.731	1.122	1.444	7.244	289
Share of requested workers with '*A*'	0.383	0.474	0.000	1.000	289
Share of requested workers with '*B*'	0.198	0.387	0.000	1.000	289
Share of requested workers with '*C*'	0.180	0.366	0.000	1.000	289
Share of requested workers with '*D*'	0.212	0.398	0.000	1.000	289
Share of requested workers with '*E*'	0.026	0.155	0.000	1.000	289
ihs U.S. temp. workers employed	2.717	1.944	0.000	8.200	289
ihs U.S. perm. workers employed	3.477	1.531	0.000	8.882	289
ihs Investment	9.949	5.346	0.000	17.910	285
Change in profit rate, year-on-year	0.067	0.601	−2.357	4.321	238
Lottery win	0.384	0.487	0.000	1.000	289
Expected share of workers	0.735	0.157	0.550	0.920	289
2020 firm traits:					
Revenue ($)	5.8*e*+06	1.6*e*+07	5500.000	2.2*e*+08	251
Foreign temp. workers employed	21.720	61.894	0.000	500.000	289
U.S. temp. workers employed	40.239	149.878	0.000	1780.000	289
U.S. perm. workers employed	54.820	191.775	0.000	2000.000	289
ln Revenue	14.620	1.375	8.613	19.186	251
ihs Foreign temp. workers employed	1.950	1.961	0.000	6.908	289
ihs Foreign temp. workers requested	2.592	1.733	0.000	6.620	289
Share of requested workers with '*A*'	0.381	0.466	0.000	1.000	212
Share of requested workers with '*B*'	0.179	0.369	0.000	1.000	212
Share of requested workers with '*C*'	0.155	0.342	0.000	1.000	212
Share of requested workers with '*D*'	0.232	0.414	0.000	1.000	212
Share of requested workers with '*E*'	0.053	0.217	0.000	1.000	212
ihs U.S. temp. workers employed	2.400	1.990	0.000	8.178	289
ihs U.S. perm. workers employed	3.335	1.619	0.000	8.294	289
Lottery win	0.373	0.485	0.000	1.000	212
Expected share of workers	0.429	0.353	0.062	0.881	212
Fixed firm traits:					
Competitors (number)	468.037	6076.605	0.000	1.0*e*+05	271
Competition on price (subjective)	3.125	0.807	1.000	4.000	279
Rural (non-metropolitan)	0.255	0.436	0.000	1.000	275
Population of ZIP code	2.2*e*+04	1.6*e*+04	48.000	6.4*e*+04	277
Industry: Landscaping	0.599	0.491	0.000	1.000	289
Industry: Food, hotel, entertainment	0.156	0.363	0.000	1.000	289
Industry: Other	0.246	0.431	0.000	1.000	289
Region: Northeast	0.197	0.399	0.000	1.000	289
Region: Midwest	0.291	0.455	0.000	1.000	289
Region: South	0.349	0.478	0.000	1.000	289
Region: West	0.121	0.327	0.000	1.000	289

Note: '*ihs*' is inverse hyperbolic sine.

Appendix Table A2: Lottery results in sampling universe vs. survey sample, 2021: Restricted to petitions by firms in the core regression sample

	Frequency		Proportion		
Result	*Universe*	*Sample*	*Universe*	*Sample*	*p*-val.
A	2,029	128	0.377	0.390	0.642
B	1,046	69	0.195	0.210	0.484
C	1,065	65	0.198	0.198	0.998
D	1,125	57	0.209	0.174	0.124
E	111	9	0.021	0.027	0.405
Total petitions	5,376	328	1.000	1.000	*0.501**

The unit of observation is petitions. The *p*-value is for a two-sample test of the null hypothesis that the fraction of petitions receiving each lottery letter in the survey sample is equal to that fraction in the universe of petitions. *The final row gives the *p*-value of Fisher's exact test of the null hypothesis that the lottery-result distribution across all five letters is equal in the sample and the universe.

United States with the follow passage, typical of the other low-concentration industries: "*The Landscaping Services industry has a low level of market share concentration. …The industry is characterized by a large number of small operators. According to the latest Economic Census, 94.0% of establishments employ fewer than 20 workers. Several companies have the resources to operate on a national scale and are typically integrated with landscape architecture departments, which enables them to bid for lucrative design-build-installation projects for commercial clients such as hotels and resorts. Nevertheless, the sheer volume of small-scale, low-value work conducted by nonemployers and small companies in the single-family housing market prevents these larger companies from capturing a substantial portion of revenue*" (Dmitry Diment, *IBISWorld Industry Report 56173: Landscaping Services in the US*, June 2022, p. 24).

A7 Check for nonresponse bias and/or randomization irregularities

Table A7 tests both for nonresponse bias and/or randomization irregularities by running a placebo test for spurious explanatory power of firm-level lottery results by firms' baseline (pre-lottery) traits in the survey sample. The tests reveal no economically or statistically significant explanatory power of the lottery results by baseline traits. The is inconsistent with substantial nonresponse bias that is correlated with treatment status and relevant observed baseline traits. It is also inconsistent with any randomization irregularities favoring firms with certain observed traits, such as larger firms or firms that employ more U.S. workers. These results are compatible with genuine randomization.

A8 Robustness to inverse hyperbolic sine transformation

Table A8 reports core regression results without using any inverse hyperbolic sine or log transformations, using the Poisson pseudo-maximum-likelihood (PPML) estimator due to Silva and Tenreyro (2006).

The coefficient on foreign employment is interpreted as a semi-elasticity. That is, the observed cross-firm correlation between H-2B employment and revenue in 2021, controlling for baseline traits, is such that the employment of one additional H-2B worker is associated with 1.72% greater revenue (col. 1). The semielasticity of revenue to winning the lottery (col. 2) and the expected share variable (col. 3) are both larger than their magnitudes in Table 2 of the main text.

Appendix Table A3: Workers requested by industry in sampling universe vs. survey sample, 2021

	Frequency		Proportion	
Industry	*Universe*	*Sample*	*Universe*	*Sample*
Grounds keeping/outdoor maintenance	52,738	4,022	0.540	0.355
Hospitality	9,604	2,288	0.098	0.202
Forestry	3,766	1,832	0.039	0.162
Seafood processing	4,457	695	0.046	0.061
Golf courses/country clubs	7,567	588	0.077	0.052
Restaurants	2,386	141	0.024	0.012
Carnivals	2,363	89	0.024	0.008
Construction	8,863	57	0.091	0.005
Other	5,910	1,613	0.061	0.142
Total workers	97,654	11,325	1.000	1.000

The unit of observation is workers requested on DOL petitions entered into the DOL lottery for H-2B visas for the second half of fiscal year 2021. For all industries the difference in proportions between universe and sample is statistically significant: the p-value is less than 0.0001 for two-sample tests of the null hypothesis that the fraction of workers is equal in the sample and universe for all rows of the table. Includes only workers on petitions in the universe and sample for which firms reported a NAICS code for the employer.

Across firms the one additional H-2B worker employed is associated with a 1.65% increase in the number of U.S. temporary workers employed, controlling for baseline traits. The semielasticity of U,S. temporary employment to winning the lottery (col. 2) and the expected share variable (col. 3) are, again, both larger than their magnitudes in Table 3 of the main text. These results are statistically significant at the 0.054 level and 0.014 level, respectively.

In sum, the qualitative findinds of the core regressions using the inverse hyperbolic sine transformation are robust to alternative specifications that do not use that transformation. The only substantial difference in the results from the PPML specification is that the effect of an advantageous lottery outcome on firms' employment of U.S. temporary workers is both positive and statistically significant at or below the ~0.05 level in these specifications; the corresponding estimates in the main text using the IHS transformation are positive but smaller in magnitude and less statistically precise.

A9 Robustness to industry composition

Firms with NAICS two-digit industry code 56 (groundskeeping and landscaping) represent the largest share of employers in the sample and universe. It is thus of interest to know if the core results are driven by treatment effects on that specific industry. Figure A6 tests the heterogeneity of the core results according to whether or not a respondent firm's industry is groundskeeping and landscaping. The 2SLS point estimates on foreign worker employment are higher for non-landscaping firms than for non-landscaping firms in the revenue, U.S. employment, and investment regressions. This suggests that if anything, the local average treatment effect estimated for the firm sample is lower than it would be if groundskeeping/landscaping firms were less prevalent.

A10 Robustness to influential observations

Table A13 repeats the core regression analysis with quantile regressions (p50) that are robust to influential observations. The IV quantile regressions are executed with the smoothed estimating equations method

Appendix Table A4: Workers requested by rural/urban employer in sampling universe vs. survey sample, 2021

	Frequency		Proportion	
Employer address	*Universe*	*Sample*	*Universe*	*Sample*
Rural	26,549	3,397	0.274	0.310
Urban	70,290	7,573	0.726	0.690
Total workers	96,839	10,970	1.000	1.000

The unit of observation is workers requested on DOL petitions entered into the DOL lottery for H-2B visas for the second half of fiscal year 2021. For both the rural and urban rows, the difference in proportions between universe and sample is statistically significant: the p-value is less than 0.0001 for two-sample tests of the null hypothesis that the fraction of workers is equal in sample and universe. Includes only workers on petitions in the universe and sample for which firms reported a postal code for the employer.

of Kaplan and Sun (2017) and Kaplan (2022). The qualitative pattern of results is similar to the results in the core regressions, which is incompatible with substantial sensitivity to a small number of influential observations.

A11 Robustness to randomization inference

Young (2018) notes that some data obtained from randomized controlled trials do not meet the conditions necessary to rely on the asymptotic properties of conventional standard errors. Table A14 shows the core results of the reduced-form regressions using the 'lottery win' instrument using Fisher's randomization inference as implemented by Heß (2017). The first column is an OLS regression of 2021 H-2B employment on the instrument, controlling for the standard baseline (2020) traits. Columns 2–4 are randomization-inference versions of the reduced-form regressions in column 4 of Table 2, Table 3, and Table 4 respectively. The qualitative pattern of inference is identical to that in core regressions of the main text using conventional standard errors.

A12 Full regression results from tests for heterogeneous treatment effects

Table A12 reports the full regression results underlying the coefficient plots in Figure 7.

A13 Item nonresponse

The most important form of item nonresponse in the survey was firms that declined to identify themselves. Table A9 tests the sensitivity of the core results to restricting the sample to firms that did self-identify. The core results in Tables 2–4 are qualitatively and quantitatively similar in the subsample that *did* provide the firm name. The coefficient of principal interest in the subsample that *did not* provide a firm name (20 out of 251) is higher to a statistically significant degree for the revenue outcome. The coefficient is not statistically significantly different in the unidentified subgroup for the other two core outcomes.

A14 Effect on U.S. year-round employment

The preanalysis plan contained specified reporting tests of the effect of employing foreign temporary low-skill workers on an additional secondary outcome: employment of year-round, generally higher-

Appendix Table A5: First stage regressions, 2021

Dep. var.:	Foreign hired 2021 (IHS)	
Lottery win 2021	0.822	
	(0.162)	
Expected share 2021		3.232
		(0.559)
U.S. temporary hired 2020 (IHS)	−0.062	−0.060
	(0.049)	(0.048)
Revenue 2020 (ln)	0.376	0.384
	(0.087)	(0.090)
Foreign hired 2020 (IHS)	0.290	0.284
	(0.054)	(0.054)
U.S. year-round hired 2020 (IHS)	−0.005	0.001
	(0.068)	(0.070)
Constant	−3.396	−5.595
	(1.124)	(1.236)
N	251	251

Presents the first-stage regressions from the rightmost columns of Tables 2–5.

skill U.S. workers. Table A10 reports these tests, analogous to the core outcomes of interest in the main text. Although firms with similar baseline traits but greater hiring of foreign workers in 2021 exhibit 3.8% higher employment of higher skill year-round U.S. workers (col. 2), this relationship could arise from unobserved confounders. In the corresponding 2SLS specifications (cols. 6 and 10) the *effect* of foreign hiring on year-round U.S. employment is statistically indistinguishable from zero by a wide margin. The present research design only measures effects in the short term—within the same same half-year (the second half of fiscal 2021).

A15 Testing for a competition channel for treatment effects

Table A11 presents tests of the reduced-form effect of the lottery result on the competitive environment faced by lottery-entrant firms. These tests were not pre-registered.

The first column presents a regression of an indicator variable for high subjective competition (the firm reports that it would be "very easy" for a competitor to steal its customers) on an indicator for winning the 2021 lottery, controlling for the standard set of predetermined baseline traits. The coefficient estimate is negative and far from statistically significant. A negative coefficient estimate implies that firms that lose the lottery are less likely to report facing conditions of high subjective competition. The second column repeats the exercise using the expected share instrument as the regressor. Again the coefficient is negative, implying less subjective competition faced by firms that exogenously hire a lower share of their desired H-2B workers, but not statistically distinguishable from zero. The final two columns repeat the regressions of the first two, but using as the dependent variable the raw score for subjective competition reported by the firm (on a 1–4 scale, where a higher number means that it is easier for competitors to

Appendix Table A6: Capital share estimates from typical industry cost structures

Industry	Year	NAICS	Wages	Net inc.	Deprec.	Rent	K share	Source
Landscaping	2022	56173	32.6	8.8	3.7	1.8	0.305	(1)
Hotels	2021	72111	32.2	3.2	8.1	2.0	0.292	(2)
Golf courses	2022	71391	39.0	1.1	9.2	7.2	0.310	(3)
Amusement parks	2022	71311	41.6	9.9	8.4	5.7	0.366	(4)
Seafood preparation	2022	31171	11.8	2.1	1.1	0.6	0.244	(5)
Forest support serv.	2021	11531	29.2	13.6	3.6	6.6	0.449	(6)

Sources: 1. Dmitry Diment, IBISWorld *Industry Report 56173: Landscaping Services in the US*, June 2022; 2. Jared Ristoff, *IBISWorld Industry Report 72111: Hotels & Motels in the US*, September 2021; 3. Brigette Thomas, *IBISWorld Industry Report 71391: Golf Courses & Country Clubs in the US*, June 2022; 4. Thi Le, *IBISWorld Industry Report 71311: Amusement Parks in the US*, July 2022; 5. Dmitry Diment, *IBISWorld Industry Report 31171: Seafood Preparation in the US*, July 2022; 6. John Madigan, *IBISWorld Industry Report 11531: Forest Support Services in the US*, November 2021.

steal customers). The coefficient of interest in these regressions is either negative or very close to zero. Collectively these regressions fail to detect evidence of a substantial effect of the lottery outcome on firms' subjective competitive environment.

A16 The 2020 lottery

Figure A3 reports the DOL decision dates for the 2020 lottery. The pattern is highly similar to the pattern in the corresponding decision dates for 2021, with the exception that no supplemental visas were issued in 2020.

Figure A4 shows the distribution of firm-level share of petitions receiving lottery result A in the 2020 lottery. The pattern is highly similar to the pattern in the 2021 lottery.

A17 Kernel density plots of 2021 outcomes by lottery result without baseline controls

Figure A2 displays kernel density plots of the core firm outcomes by lottery result, in 2021, without controlling for predetermined baseline traits.

A18 Survey questionnaire

Figure A7 reproduces the online survey exactly as respondents saw it, on 11 separate click-through screens. Respondents reached the survey form by clicking on a link named "http://visalotterystudy.org" in an email from an industry association of which their firm was a paying member. We estimate that it took the average respondent 15 minutes to complete.

Appendix Table A7: Placebo test for spurious explanatory power of lottery result by baseline traits in the survey sample

Dep. var.:	*Lottery win*	*Expected share*
Estimator:	OLS	OLS
Revenue 2020 (ln)	0.015	0.001
	(0.030)	(0.010)
Foreign hired 2020 (IHS)	0.007	0.004
	(0.017)	(0.006)
U.S. year-round hired 2020 (IHS)	-0.013	-0.005
	(0.026)	(0.008)
U.S. temporary hired 2020 (IHS)	-0.014	-0.004
	(0.016)	(0.005)
Number of firms	251	251
R^2	0.005	0.006

Robust standard errors in parentheses. *IHS* is inverse hyperbolic sine.

Appendix Table A8: Core results in PPML specification, all variables in levels without inverse hyperbolic sine or log transformations

Estimator:	PPML					
Dep. var. (*levels*):	Revenue 2021			U.S. temporary hired 2021		
Foreign employed 2021 (*level*)	0.0172			0.0165		
	(0.0014)			(0.0032)		
Lottery win (*indicator* {0,1})		0.422			0.431	
		(0.233)			(0.224)	
Expected share (0–1)			1.183			1.678
			(0.697)			(0.682)
Full baseline controls (*levels*)	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Number of firms	251	251	251	251	251	251
R^2	0.893	0.683	0.678	0.886	0.875	0.877

Robust standard errors in parentheses. All regressions include constant term. All variables in levels (without log or IHS transformation). All columns include the same baseline variables as in the corresponding regressions of the main text, but in levels (untransformed): the 2020 values of revenue, number of U.S. year-round workers, number of U.S. temporary workers, and number of foreign temporary workers.

Appendix Table A9: Tests for sensitivity of the results to item nonresponse for firm name, 2021

Dep. var.:	Revenue 2021 (ln)	U.S. hired 2021 (IHS)	Investment 2021 (IHS)
Specification:	2SLS	2SLS	2SLS
Foreign hired 2021 (IHS)	0.156	0.093	1.119
	(0.072)	(0.127)	(0.548)
Foreign hired 2021 (IHS) × unidentified	0.062	0.070	−0.696
	(0.027)	(0.098)	(0.421)
Full baseline controls	*Yes*	*Yes*	*Yes*
Number of firms	251	251	249
R^2	0.816	0.696	0.103

'Unidentified' is an indicator variable taking the value one if the respondent left the firm name blank, 0 otherwise. All regressions are 2SLS with two endogenous variables and two instruments. The endogenous variables are 'Foreign hired 2021' and its interaction with 'unidentified'. The instruments are the 'expected shared' instrument and its interaction with 'unidentified'.

Appendix Table A10: Effect of foreign worker employment on higher-skill, year-round U.S. employment, 2021

Dep. var:	*U.S. year-round workers 2021 (IHS)*									
Estimator:	OLS				2SLS		OLS		2SLS	
Instrument:					*Lottery win*				*Expected share*	
Foreign temp. employed 2021 (IHS)	0.216	0.038			−0.052	−0.025			−0.105	−0.013
	(0.060)	(0.017)			(0.221)	(0.073)			(0.184)	(0.056)
Anderson-Rubin p-*val.*	—	—			*0.812*	*0.729*			*0.557*	*0.823*
Lottery win 2021			−0.047	−0.021						
			(0.195)	(0.060)						
Expected share 2021							−0.354	−0.041		
							(0.602)	(0.183)		
U.S. year-round workers 2020 (IHS)	0.908	0.858	0.917	0.857	0.922	0.857	0.917	0.857	0.921	0.857
	(0.042)	(0.062)	(0.040)	(0.063)	(0.035)	(0.062)	(0.040)	(0.063)	(0.036)	(0.062)
Full baseline controls	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*
Number of firms	251	251	251	251	251	251	251	251	251	251
R^2	0.889	0.900	0.887	0.898	0.885	0.896	0.887	0.898	0.885	0.897

The unit of observation is firms. All regressions include constant term. Robust standard errors in parentheses. The dichotomous 'Lottery win' instrumental variable is an indicator variable for winning the lottery, that is, receiving 'A' on petitions totaling at least 50% of workers requested. The continuous 'Expected share' instrumental variable is the share of overall workers petitioned for that the firm could expect to be certified according to the certification rates in the sampling universe for each lottery letter. IHS is inverse hyperbolic sine. *Full baseline controls* are the 2020 values of revenue, number of U.S. year-round workers, number of U.S. temporary workers, and number of foreign temporary workers.

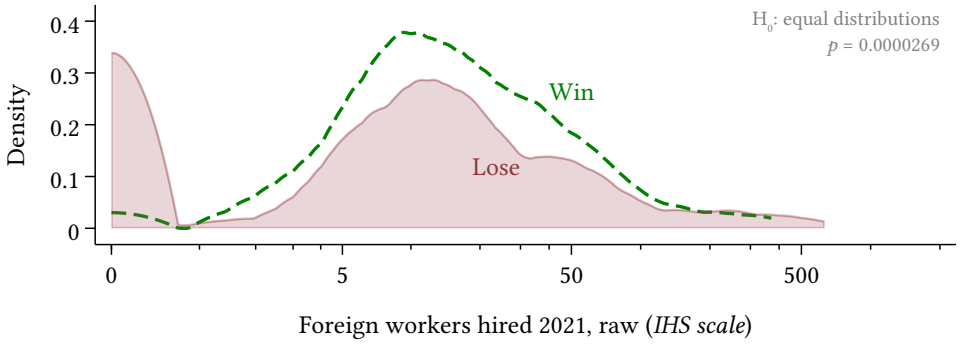
Appendix Table A11: Effects on Competitive Environment, 2021

Dep. var.:	High subjective competition, indicator (0,1)		Subjective competition, raw score (1–4)	
Lottery win 2021	−0.038		0.004	
	(0.062)		(0.102)	
Expected share 2021		−0.187		−0.079
		(0.199)		(0.329)
Observed baseline controls	*Yes*	*Yes*	*Yes*	*Yes*
Number of firms	245	245	245	245
R^2	0.018	0.020	0.021	0.022

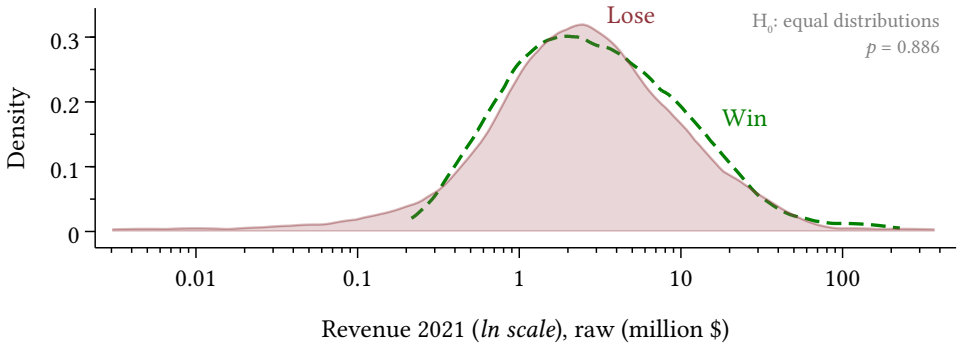
OLS regressions with robust standard errors in parentheses. "High" subjective competition means the business self-reported that it would be "very easy" (4 on a 4-point scale of ease) for competitors to steal their customers. The raw score is the number reported by each firm, where 1 means it would be very difficult for competitors to steal their customers. These questions were asked of firms at the end of the hiring season, starting in October 2021, referring to *current* (not retrospective) conditions. *Observed baseline controls* are the 2020 values of revenue, number of U.S. year-round workers, number of U.S. temporary workers, and number of foreign temporary workers.

Figure A2: Reduced-form effects of the 2021 lottery, without baseline controls

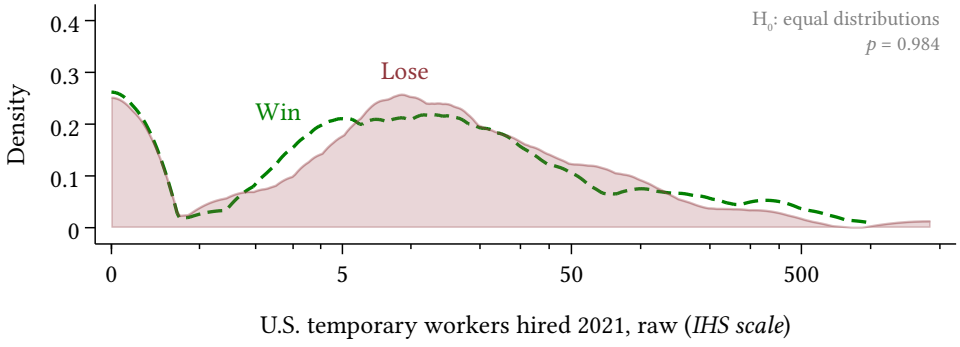
(a) *Foreign workers employed, 2021*



(b) *Revenue, 2021*

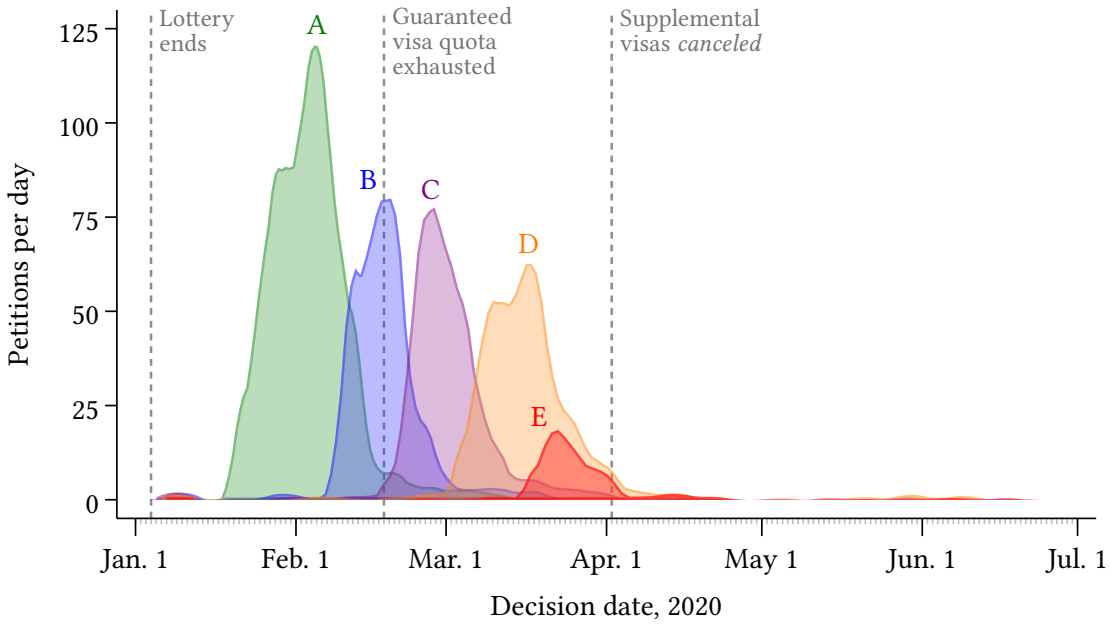


(c) *U.S. temporary workers employed, 2021*



The unit of analysis is firms. 'Win' is defined as a firm receiving randomized lottery letter '*A*' for petitions exceeding half of the total workers requested; all other results are defined as 'lose'. Graphs show Epanechnikov kernel density estimates with a bandwidth of 0.15 inverse hyperbolic sine (IHS) points (*a* and *c*) or 0.5 ln points (*b*). Exact *p*-values are from nonparametric two-sample Kolmogorov-Smirnov tests of the null hypothesis of the equality of the 'win' and 'lose' distributions in each pane of the figure.

Figure A3: Foreign worker petition decision dates by lottery result, universe of firms, second half of fiscal year 2020



Shown is the universe of firms entering each lottery. Epanechnikov kernel densities, bandwidth 2 days. 'Decision date' is the date of the Department of Labor's decision on whether or not to certify each petition, a necessary condition of proceeding to petition USCIS for a visa. The 2020 lottery was conducted for DOL petitions received January 2–4, 2020. The statutory quota of 33,000 guaranteed visas for the second half of the fiscal year was reached on Feb. 18, 2020.

Figure A4: Defining a lottery 'win' at the firm level, 2020 lottery



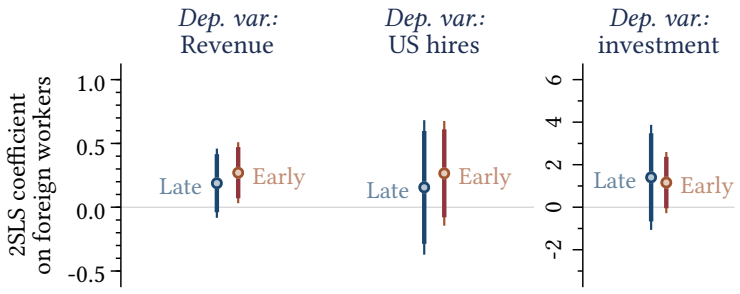
Only includes firms that voluntarily self-identified in the survey, allowing them to be matched to public records of their 2020 lottery result.

Appendix Table A12: Heterogeneous effects by competition, size, and location

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Number of competitors		Competition (subjective)		Small firm		Rural		Low population	
	High	*Low*	*High*	*Low*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*
Dep. var.: Revenue 2021 (ln)										
Foreign employed 2021 (IHS)	0.291	0.102	0.317	0.152	0.324	0.017	0.265	0.185	0.293	0.139
	(0.131)	(0.078)	(0.253)	(0.081)	(0.119)	(0.122)	(0.194)	(0.094)	(0.167)	(0.098)
And.-Rubin p-*val.*	0.0366	0.2339	0.2932	0.0669	0.0061	0.8961	0.2173	0.0592	0.0890	0.1838
N	134	117	92	159	123	128	66	185	119	132
Dep. var.: U.S. temporary workers employed (IHS)										
Foreign employed 2021 (IHS)	0.200	0.184	0.677	0.039	0.295	−0.003	0.514	0.054	0.270	0.086
	(0.264)	(0.181)	(0.597)	(0.156)	(0.205)	(0.285)	(0.291)	(0.203)	(0.223)	(0.232)
And.-Rubin p-*val.*	0.4720	0.3488	0.2428	0.8065	0.1765	0.9910	0.0868	0.7943	0.2408	0.7217
N	134	117	92	159	123	128	66	185	119	132
Dep. var.: Investment (IHS)										
Foreign employed 2021 (IHS)	0.896	1.330	1.352	1.444	2.506	−0.473	2.857	0.661	2.218	1.167
	(0.915)	(0.874)	(1.859)	(0.688)	(0.872)	(1.411)	(1.426)	(0.745)	(1.107)	(0.789)
And.-Rubin p-*val.*	0.3340	0.1185	0.4998	0.0281	0.0012	0.7368	0.0161	0.3884	0.0347	0.1397
N	134	115	91	158	122	127	65	184	118	131

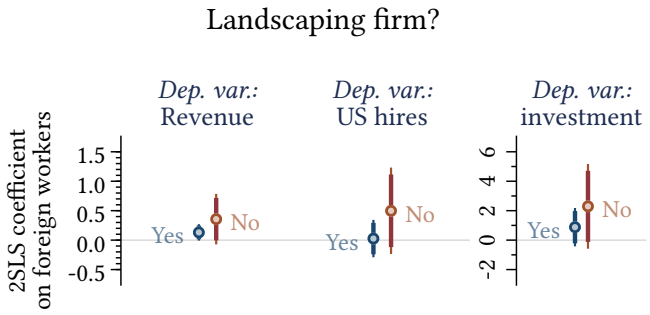
The unit of observation is firms. All regressions include constant term. All regressions are two-stage least squares with 'Foreign employed 2021 (IHS)' as the endogenous regressor, in a regression with full baseline controls, corresponding to the specification in column 6 of Tables 2, 3, and 4. The dichotomous instrumental variable is an indicator variable for winning the lottery, that is, receiving 'A' on petitions totaling at least 50% of workers requested. IHS is inverse hyperbolic sine. All regressions include the full set of predetermined baseline control variables: ln 2020 revenue and the inverse hyperbolic sine (IHS) of 2021 *requested* H-2B workers, 2020 *employed* H-2B workers, 2020 U.S. year-round workers, and 2020 U.S. seasonal workers. "High" number of competitors means greater than the median response (25). "High" subjective competition means the business self-reported that it would be "very easy" (4 on a 4-point scale of ease) for competitors to steal their customers by underpricing them. "Small" firms are those with less than median revenue at baseline (in 2020). "Rural" firms are those whose ZIP code is classified by the Census Bureau as anything other than "Metropolitan Area, Core" (RUCA code 1). "Low" population means the firm's ZIP code has less than the median population among all ZIP codes (20,459 residents) in the 2010 full-count census.

Figure A5: Test for nonresponse bias



The unit of analysis is firms. 'Late' responses are those that took more than the median time to complete the survey after it was first emailed. The first response was received on October 21, 2021 at 9:36am Eastern time; the median response was received on October 25, 2021 at 1:14pm Eastern time. The vertical axis in each pane shows the 2SLS coefficient on foreign workers employed (2021, IHS) in a regression with full baseline controls, corresponding to the specification in column 6 of Tables 2, 3, and 4. The coefficients can be interpreted as elasticities. Thin vertical line shows 95% confidence interval, thick line shows 90% confidence interval.

Figure A6: Test for bias from sampling overweight on groundskeeping/landscaping



The unit of analysis is firms. All regressions include constant term. 'Yes' indicates the firm's industry is groundskeeping and landscaping (two-digit NAICS industry code 56); 'no' indicates any other industry. The vertical axis in each pane shows the 2SLS coefficient on foreign workers employed (2021, IHS) in a regression with full baseline controls, corresponding to the specification in column 6 of Tables 2, 3, and 4. The coefficients can be interpreted as elasticities. Thin vertical line shows 95% confidence interval, thick line shows 90% confidence interval.

Appendix Table A13: Robustness: Quantile regressions (p50), 2021

Year:	2021				2020			
Dep. var:	Revenue (ln)		U.S. temporary workers employed (IHS)		Revenue (ln)		U.S. temporary workers employed (IHS)	
Estimator:	Quantile	IV quantile	Quantile	IV quantile	Quantile	IV quantile	Quantile	IV quantile
Foreign employed (IHS)	0.080	0.090	0.060	−0.001	0.077	0.138	0.312	0.357
	(0.012)	(0.040)	(0.029)	(0.105)	(0.048)	(0.112)	(0.150)	(0.556)
Revenue 2020 (ln)	0.893	0.873	0.000	0.024				
	(0.017)	(0.046)	(0.042)	(0.036)				
Observed baseline controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Number of firms	251	251	251	251	191	191	191	191
Pseudo R^2	0.693	—	0.570	—	0.204	—	0.076	—

The unit of observation is firms. Quantile IV estimator due to Kaplan and Sun (2017) and Kaplan (2022). All regressions include constant term. Standard errors in parentheses. The dichotomous 'Lottery win' instrumental variable is an indicator variable for winning the lottery, that is, receiving 'A' on petitions totaling at least 50% of workers requested. IHS is inverse hyperbolic sine. All regressions include the same baseline variables as in the linear regressions in the main text: For 2021, the baseline controls are: numbers of U.S. year-round workers, U.S. temporary workers, and foreign temporary workers in 2020. For 2020 the only observed basline trait is the number of H-2B workers requested (IHS).

Appendix Table A14: Robustness: Randomization inference, 2021

Dep. var:	Foreign temp. workers employed (IHS)	Revenue (ln)	U.S. temp. workers employed (IHS)	Investment (IHS)
Lottery win	0.8216	0.1700	0.1462	1.1372
	(0.1624)	(0.0760)	(0.1422)	(0.5604)
Rand. inference p-val.	<0.001	0.025	0.300	0.071
Full baseline controls	*Yes*	*Yes*	*Yes*	*Yes*
Number of firms	251	251	251	249
R^2	0.339	0.800	0.680	0.102

Robust standard errors in parentheses. Uses Fisher's randomization inference implemented by Heß (2017).

Figure A7: THE FIRM SURVEY QUESTIONNAIRE AS RESPONDENTS SAW IT ON 11 SCREENS

