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THE EFFECT OF LOW-SKILL IMMIGRATION RESTRICTIONS ON US FIRMS AND WORKERS: EVIDENCE FROM A RANDOMIZED LOTTERY

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ABSTRACT

U.S. firms face a binding quota on visas to employ foreign workers in low-skill occupations outside of agriculture. The government allocates this quota to firms in part through a randomized lottery. We evaluate the marginal impact of the quota on firms entering this lottery in 2021 and 2022, using a novel survey and pre-analysis plan. Firms exogenously authorized to employ more immigrants in low-skill jobs significantly increase production (elasticity 0.20–0.22), investment (1.5–2.1), and the rate of profit (0.15). Because the foreign-native elasticity of substitution in production is very low in the policy-relevant occupations (0.8–2.2), the effect on native employment is zero or positive overall, and positive in rural areas. 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 low-skill immigration using a novel, 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, predicted treatment effects, and subgroups with a pre-analysis plan, we conducted a survey of 472 firms comprising both winners and losers of the H-2B visa lotteries for mid-2021 and mid-2022. This allows pre-specified tests of basic theoretical predictions about the magnitude and heterogeneity 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 its operations. Put differently, exogenous *restrictions* on employing the profit-maximizing number of immigrants for low-skill work cause the marginal firm to contract. Losing the lottery reduces firms' employment of low-skill immigrants by about half (−0.62 log points). This exogenous decrease in employment of low-skill foreign workers causes firms to reduce revenue with elasticity 0.20–0.22, reduce investment with elasticity 1.5–2.1, and reduce the rate of profit with elasticity 0.15 (all statistically precise at conventional levels). Across all firms collectively this restriction either does not affect or causes a reduction in employment of low-skill U.S. workers, with elasticity 0.06–0.19 (statistically imprecise in some specifications). In pre-registered subsamples of firms located in rural areas, this reduced employment of low-skill foreign workers causes reduced employment of low-skill U.S. workers, with elasticity 0.61 (statistically precise).

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 substantially crowding-in native employment for low-skill jobs in rural areas; for the nation as a whole, it is consistent with either modest crowding-in of native employment or null effects on native employment. It is not consistent with substantial crowding out of native employment, for firms collectively or in any preregistered subgroup. 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 0.8–2.2, 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 in rural, less-populated areas of the country.

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 sensitivity to influential observations and randomization inference. Finally, we forensically test for and rule out substantial changes in unobserved black-market employment. This corroborates prior evidence, both observational (Hotchkiss et al. 2015; Orrenius and Zavodny 2020) 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. The first lies in its transparent causal identification. We are not aware of a prior study on the effects of low-skill immigration that uses randomization as its research 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 2024)—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.

Second, this paper 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 considers immigration for low-skill work, complementing ongoing work by Amuedo-Dorantes et al. (2023).[1]

Traditional approaches that rely on variation in exposure to 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 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 2023; East et al. 2023; Abramitzky et al. 2023).

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 ex-

[1] Amuedo-Dorantes et al. likewise study the effects of H-2B visa access on firms, using administrative data on the universe of H-2B visa sponsoring firms in 2018, but without information on firms' lottery outcomes. The instead address causal identification using an unexpected shift in the availability of visas, finding that expanded visa access causes large increases in firm revenue with limited crowd-out of non-H-2B employment.

ploit 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; Brinatti et al. 2023).[2]

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 are presented in Section 6, with robustness checks 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 a forensic test 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.[3] It likewise predicts positive effects on investment and absolute profits, but not necessarily the rate of profit.

[2] 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).

[3] 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.

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.[4] The firm is a 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

[4] 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.

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 with the Euler equation,

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

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,[5] 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).[6] This gives:

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

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

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 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 $\beta, \gamma > 0$.[7] 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 $\Theta > 0$.[8] 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:

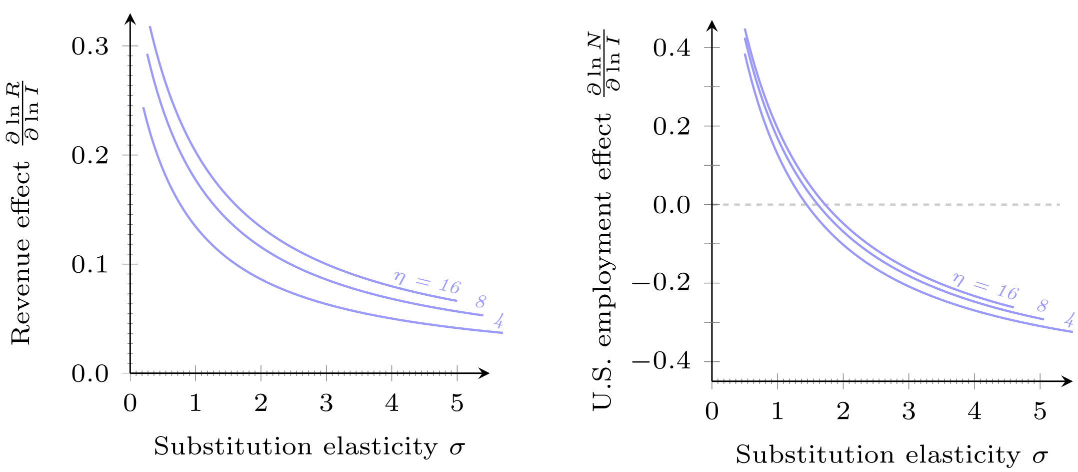
Lemma 2. *The amount by which the lottery increases native employment is rising in the output demand elasticity η and falling in the immigrant-native elasticity of substitution σ. If η is sufficiently high (low) relative to σ, immigrant employment crowds in (out) native*

[7] 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.

[8] 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.

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

(a) *Effect on revenue* (b) *Effect on U.S. employment*



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 0.668. Details in Appendix.

employment.

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

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.[9]

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.[10] 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.[11] 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 reveal that U.S. county-years where employers petition for more H-2B workers do not ex-

[9] 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.

[10] 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.

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

hibit 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 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".[12]

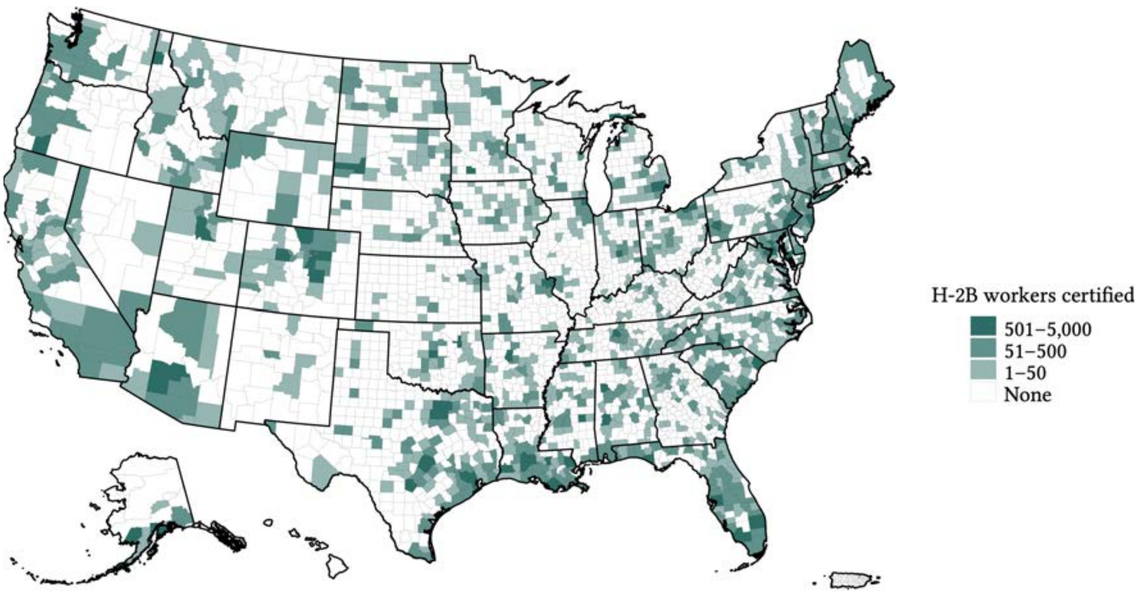
Foreign workers received an average of 84,383 H-2B visas per year during the five fiscal years ending in 2021. 88% are male.[13] The leading industries employing H-2B workers are Administrative and Support Services (especially groundskeeping/landscaping); Hospitality (including restaurants); Arts, Entertainment, 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

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

[13] In fiscal 2021, the latest data released at the time of writing, DHS reports 123,046 entries (I-94 only) on H-2B visas, of which 15,374 women: DHS, "Nonimmigrant Admissions by Selected Classes of Admission and Sex and Age".

Figure 2: Worksites of H-2B visa petitions by U.S. county, 2021–2022 (universe)



Full universe of cap-subject petitions certified by the Dept. of Labor, fiscal years 2021 and 2022. County of worksite, not necessarily of the petitioning firm. There were certified petitions from all 50 states plus the District of Columbia and Puerto Rico.

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 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 all 50 states plus the District of Columbia, 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'.[14] On the average petition, 88.1% of requested workers were certified in fiscal year 2021 and 78.8% in fiscal year 2022. For DOL-certified workers, employers must then petition DHS, which decides whether there are sufficient visas for the petition

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

and whether anything disqualifies each worker from receiving a visa. A firm hiring a group of workers to provide the same service at the same location can list up to 25 workers on the same petition.[15]

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,[16] 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

[15] 8 CFR 214.2(h)(2)(ii).

[16] 23 Stat. 332, 48th Congress, Sess. II, Chap. 164.

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 foreseeable demand.[17] 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.[18]

Second, a naturally randomized lottery constrains firms' access to H-2B visas under the statutory 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.[19] DOL randomly assigns each petition a letter: *A* through *E* in 2021, *A* through *G* in 2022. 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 petitions with letter *C*, *D*, *E* and so forth 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.

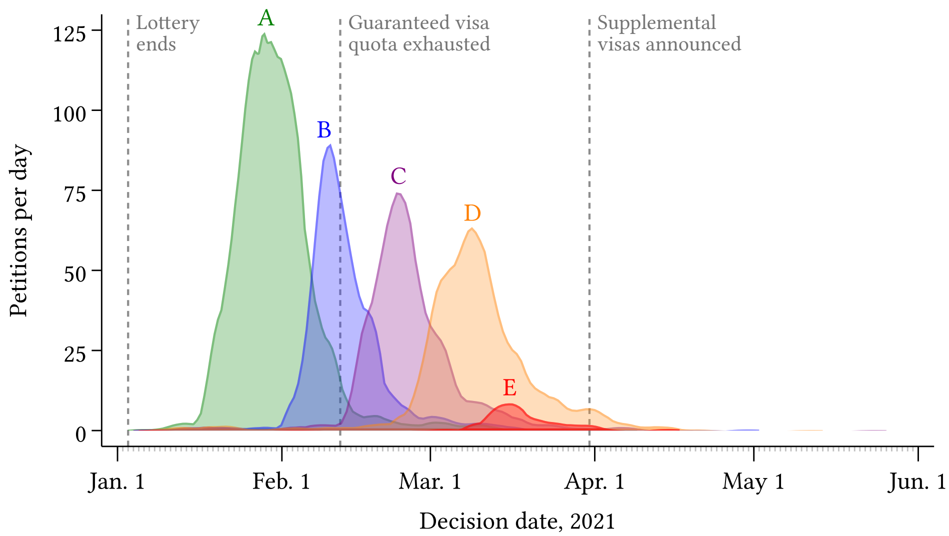
[17] Personal communication with Bruce Morrison, former Chair of the House Immigration Subcommittee and a principal author of the Act, July 25, 2022. During the Congressional reconciliation process in mid-1990 that led to the final Immigration Act, the most recent available *Statistical Yearbook* of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

[18] *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).

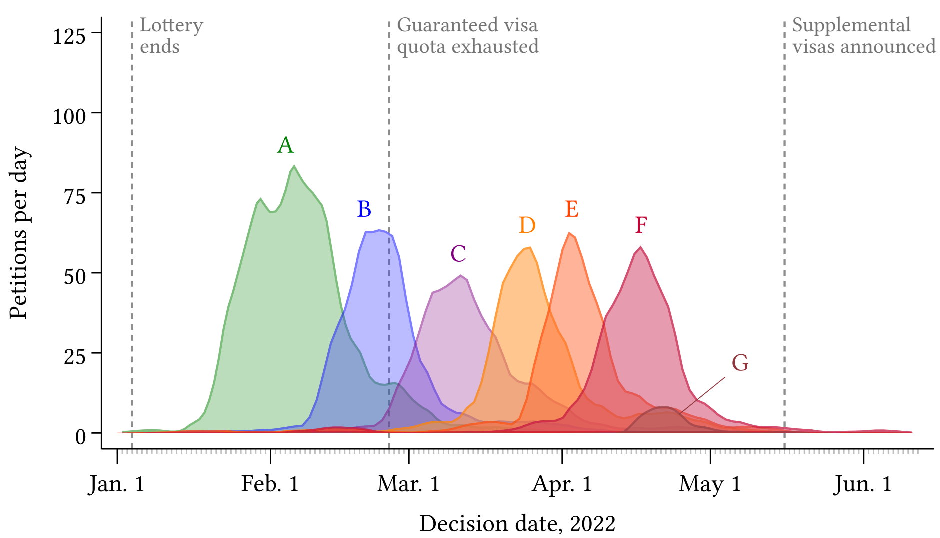
[19] *Federal Register*, March 4, 2019, 84 *FR* 7403.

Figure 3: Foreign worker petition decision dates by lottery result, universe of firms

(a) *Fiscal year 2021, second half*



(b) *Fiscal year 2022, second half*



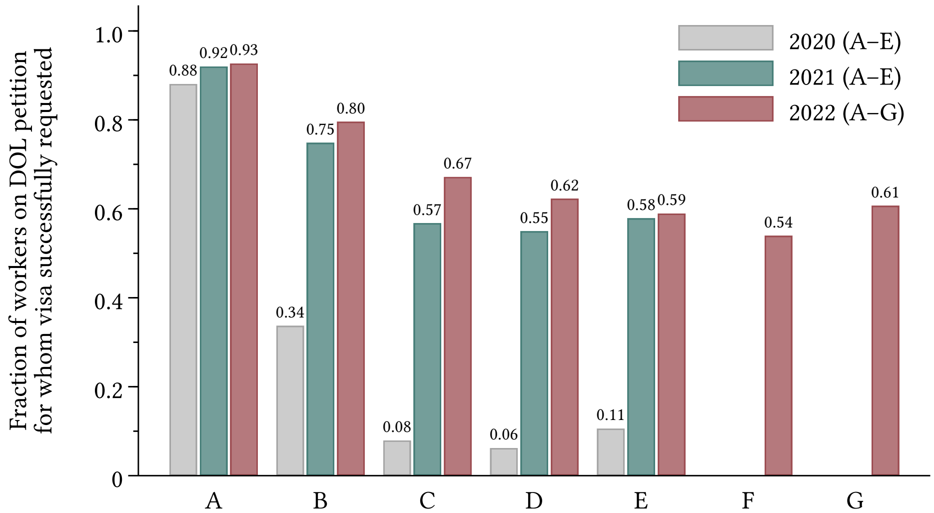
The unit of observation is petitions. Shown is the universe of firms entering the January lottery for work to be performed in the second half of each fiscal year (April 1–September 30). Epanechnikov kernel densities, bandwidth 2 days. 'Decision date' is the date of the Department of Labor's decision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

Treatment 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).

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 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.[20]

[20]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). In the second half of fiscal year 2022, DOL received petitions for 136,555 workers under the 33,000 cap, and in early June began approving 35,000 supplemental visas (*Federal Register*, May 18, 2022, 87 FR 30335–30337). 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 (2019–2022). 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).

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. In the 2020 and 2021 lotteries petitions were given a letter A–E; in the 2022 lottery petitions were given a letter A–G.

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 and 2022, due to the supplemental visas, even employers with a poor lottery result were able to hire roughly half of the workers they demanded. There is 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 limited fraction of 'never-takers' (roughly 8%).

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

We conducted a novel firm survey to gather data on employers that entered the lotteries to employ low-skill foreign workers in mid-2021 and mid-2022, the second half of each fiscal year (April to September). 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.[21]

We began this study by collecting data only for mid-2021. We released an initial analysis on the 2021 data alone, whose survey yielded a sample of 251 firms (Clemens and Lewis 2022). To increase the statistical precision of the study we repeated an identical survey in 2022, raising the eventual pooled firm sample to 472. While our Pre-Analysis Plan did state the intent to collect data only in 2021, we do not believe that repeating an identical survey the following year substantively departs from that plan.[22] Moreover, as will be discussed below, the general pattern and magnitude of the point estimates did not substantially shift between the 2021-only analysis and the pooled 2021–2022 analysis that we focus on here.

5.1 Data collection

In 2021, 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 claimed as members roughly 2,500 firms out of the 4,406 firms that entered the January 2021 lottery (57%). They sent the 2021 survey to their members seven weeks after the end of the second half of fiscal year 2021, on October 21, 2021,

[21] 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.

[22] Our assessment is that the intent and value of Pre-Analysis Plans lies primarily in revealing or preventing specification searching and data mining, not in preventing the collection of more data with an identical survey instrument and research design.

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 2021 survey to further responses on February 8, 2022. We conducted the 2022 survey in a nearly identical manner, first disseminating the survey form on March 10, 2023 and closing the survey on April 25, 2023.[23]

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 [or 2022] 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.[24] 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?"

[23]The only substantive differences between the implementation of the 2022 survey and the 2021 survey were that 1) in addition to the four industry associations mentioned above, we also asked two labor recruiters that work with numerous H-2B employers—Practial Employee Solutions Inc. and másLabor LLC—to send the survey link to their H-2B employer clients, and 2) we conducted the survey somewhat later relative to the end of the work season in 2022 than in 2021. The 2021 survey asked during October 2021–February 2022 about the period ending September 2021; the 2022 survey asked during February 2023–April 2023 about the period ending the September 2022.

[24]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.

The survey measures profits indirectly, due to the well-known reluctance of firms to directly 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 2021 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.[25] This left a final 2021 survey sample of 289 firms that had answered most questions about 2021. The core 2021 sample used in most regressions to follow, 251 firms, comprises those that also provided full pre-lottery baseline data from 2020.

When the 2022 survey closed we had received forms from 297 respondents. Ten of these (3.4%) were dropped because they were duplicate responses; in all cases the response kept was the one that contained responses to more questions. Two responses (0.7%) were dropped because the respondent firms appeared to cease operations in 2022 with near-zero revenue. This left a final 2022 survey sample of 285 firms that had answered most questions about 2022. The core 2022 sample used in most regressions to follow, 221 firms, comprises those that also provided full pre-lottery baseline data from 2021.

The pooled 2021 and 2022 core sample is thus 472 firms, that is, 251 from 2021 and 221 from 2002. Summary statistics are presented in the Appendix.

[25] 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

(a) *2021 survey*

	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**

(b) *2022 survey*

	Frequency		Proportion		
Result	*Universe*	*Sample*	*Universe*	*Sample*	*p*-val.
A	2,082	88	0.264	0.246	0.455
B	1,229	55	0.156	0.154	0.920
C	1,151	39	0.146	0.109	0.053
D	1,136	61	0.144	0.170	0.162
E	1,075	53	0.136	0.148	0.519
F	1,094	55	0.139	0.154	0.418
G	110	6	0.014	0.017	0.656
Total petitions	7,877	357	1.000	1.000	*0.337**

The unit of observation is petitions, not firms. 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 for that year. *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 (2021) or all seven letters (2022) is equal in the sample and the universe.

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 6.3% of the universe of petitions in the lottery (834 petitions out of 13,253, see Table 1). 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, in both years. In 2021, 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. In 2022, petitions receiving lottery letter C are slightly underrepresented in the sample, but there is no generalized pattern of underrepresentation for petitions with better lottery results or worse lottery results. The results in Table 1 are inconsistent with concerns that firms with good lottery results might be generally 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. because other firms use the survey to express perceived harm from their own lottery result).[26]

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 0.32 to 0.88 (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 baseline employment patterns within lottery outcome groups. This is inconsistent with substantial nonresponse bias correlated with treatment status, and inconsistent with substantial administrative irregularities in the randomization procedure of the Department of Labor.

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 proxy test for nonresponse bias in the literature (e.g. Behaghel et al. 2015; Heffetz and Reeves 2019), resting on the assumption that the same latent firm traits associated with late response are likely to be associated with nonresponse. The results, reported below, exhibit no significant heterogeneity across a wide range of response delay.

The survey sample describes firms employing 13,739 H-2B workers collectively in the second half of each of two fiscal years; 8,347 in mid-2021 and 5,392 in mid-2022. These workers are hired

[26] The results of this comparison are substantially the same when the sample of survey-reported petitions is restricted to the petitions that filed by the 472 firms in the core regression sample for the analysis to follow.

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 (46.2%), 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 are basic workers in forestry (15.5%); seafood processing (10.2%); hospitality (7.0%), which typically include housekeepers, clerks, porters, waiters, cooks, dishwashers, baristas, parking attendants, lifeguards, and janitors. These are followed by workers in carnivals (5.0%), golf courses and country clubs (3.8%), construction (3.0%), and restaurants (0.5%), along with workers in various other industries (8.8%).

This industry breakdown for requested workers in the survey sample is roughly but imperfectly representative of the sampling universe, incompatible with severe heterogeneity in sampling by industry. Groundskeeping and landscaping is the most common industry in both the sample (46.2% of workers) and the universe (39.5% of workers), though the sample somewhat overrepresents it. Considering the next two most common industries for H-2B workers covered by the survey sample, forestry is somewhat overrepresented (15.5% of sample, 8.9% of universe) as is seafood processing (10.2% of sample, 8.9% of universe). The remaining industries collectively are somewhat underrepresented in the sample (28.1%) versus the universe (42.8%), though each of the top eight industries is substantially covered by the sample. The geographic locations of the firms in the sample and universe are likewise similar. 34% of workers in the sample were requested by firms whose employer lists a rural (non-metropolitan) address, compared to 32% 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 firm outcomes, 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 time of the lottery and continued to exist roughly a year later when we contacted them. We 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 roughly a year 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

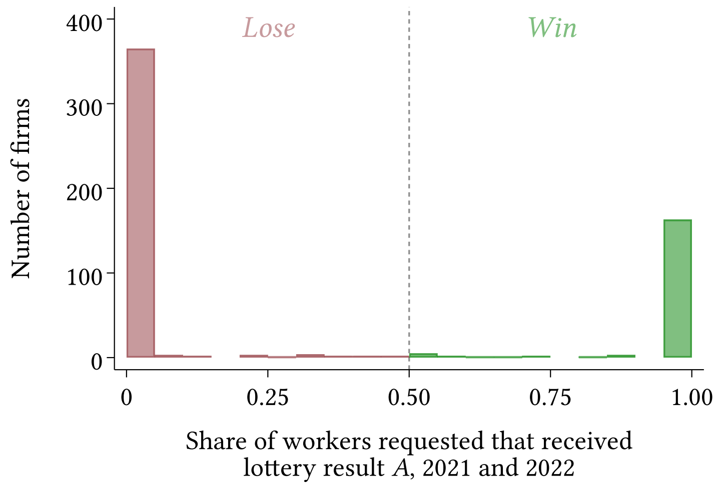
$$z_i \equiv \begin{cases} 1 & \text{if } s_i^A > 0.5 \\ 0 & \text{otherwise.} \end{cases} \tag{10}$$

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. For example, the rate of success for a worker on an A petition in the January 2021 lottery was $\rho^A = 0.920$, the rate of success for a worker on an F petition in the January 2022 lottery was $\rho^F = 0.54$. Denote by s_i^ℓ the share of each firm's requested workers receiving lottery letter $\ell \in \mathbb{L}$ in each year, where $\mathbb{L} = \{A, \ldots, E\}$ in 2021 and $\mathbb{L} = \{A, \ldots, G\}$ in 2022. The 'expected share' instrument is

$$z_i' \equiv \sum_{\ell \in \mathbb{L}} \rho^\ell s_i^\ell. \tag{11}$$

This specification avoids bias that would arise from alternative specifications of the instrument,

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



The unit of observation is firms in the survey sample, pooled 2021 and 2022 data. Frequency histogram with bin width 0.05.

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

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

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, δ and the vector Φ are coefficients to be estimated; $\mathbb{1}_t$ is a year dummy (for 2022, base year 2021); $\varepsilon_{i,t}$ 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

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

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, see also Aihounton and Henningsen 2020). That said, because the literature recommends care in interpreting coefficients estimated after IHS transformation (Chen and Roth 2024), we consider 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 86 percent more foreign workers as lottery-losing firms, corresponding to a first-stage semielasticity $\hat{\mu} = 0.618$. 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 outcome (Intent-to-Treat) and from foreign employment (Treatment-on-Treated), in 2021 and 2022. These regressions estimate the coefficient 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

Table 2: Effect of foreign worker employment on firm revenue

Dep. var:	*Revenue (ln)*									
Estimator:	OLS				2SLS		OLS		2SLS	
Instrument:					*Lottery win*				*Expected share*	
Foreign employed (IHS)	0.096	0.115			0.202	0.218			0.184	0.198
	(0.022)	(0.022)			(0.082)	(0.080)			(0.069)	(0.069)
Anderson-Rubin p-*val.*	—	—			*0.015*	*0.008*			*0.006*	*0.004*
Lottery win			0.124	0.135						
			(0.051)	(0.051)						
Expected share							0.419	0.443		
							(0.154)	(0.154)		
Revenue, baseline (ln)	0.852	0.817	0.901	0.856	0.798	0.780	0.901	0.858	0.807	0.787
	(0.043)	(0.057)	(0.038)	(0.056)	(0.059)	(0.065)	(0.038)	(0.056)	(0.052)	(0.062)
Full baseline controls	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*
Number of firms	472	472	472	472	472	472	472	472	472	472

Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term and a dummy for the year 2022 (omitted year 2021). 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 values—in the year before the lottery—of revenue, number of U.S. year-round workers, number of U.S. temporary workers, and number of foreign temporary workers.

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 baseline revenue. In the second column, the predetermined controls are expanded to include baseline foreign worker employment, baseline temporary U.S. employment, and baseline year-round U.S. employment. Revenue and foreign employment are correlated across firms with elasticity 0.115, controlling for the full baseline data. This simple correlation might understate the causal effect of foreign employment on revenue if, for example, firms with less revenue relative to baseline are more likely to employ H-2B workers.

The rest of the table considers the causal effect of foreign-worker employment on revenue. It reveals a large and statistically significant positive effect. 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 13.2% (corresponding to a semielasticity of 0.124). 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 0.218, 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 2024). We nevertheless report the p-value of the Anderson-Rubin χ^2 test, as recommended by Moreira (2009), which is fully robust to weak instrumentation (Andrews et al. 2019) that might invalidate traditional t-ratio inference (Lee et al. 2022). This is reported in italics 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 0.008, 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, prespecified, randomly-assigned instrument: The causal elasticity of revenue to an increase in foreign employment is 0.20 in column 10, compared to the estimate of 0.22 using the lottery-win instrument in column 6. The Anderson-Rubin p-value of 0.004 suggests high statistical significance of this result as well. This result suggests that the results are robust to relaxing the simplifying assumptions made in construction of 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. It is very similar to Table 2, with two differences. First, the outcome variable is firm-level employment of U.S. temporary workers. 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 0.225, controlling for the full baseline traits. In columns 3–4, the causal semielasticity of U.S. temporary employment to winning the lottery is 0.116, an estimate that is not statistically significantly different from zero at conventional levels. In columns 5–6, the causal elasticity of U.S. temporary employment to foreign temporary employment is 0.188, 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 lower point estimates using the 'expected share' instrument, also statistically indistinguishable from zero for the average firm.

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 baseline 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 on baseline traits.

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

Dep. var:	*U.S. temporary workers (IHS)*									
Estimator:	OLS				2SLS		OLS		2SLS	
Instrument:					*Lottery win*				*Expected share*	
Foreign employed (IHS)	0.140	0.225			0.171	0.188			0.034	0.061
	(0.039)	(0.047)			(0.176)	(0.149)			(0.139)	(0.125)
Anderson-Rubin p-*val.*	—	—			*0.335*	*0.219*			*0.808*	*0.630*
Lottery win			0.098	0.116						
			(0.102)	(0.095)						
Expected share							0.073	0.136		
							(0.303)	(0.285)		
U.S. temporary workers, baseline (IHS)	0.812	0.810	0.822	0.804	0.810	0.809	0.821	0.803	0.819	0.805
	(0.029)	(0.028)	(0.028)	(0.029)	(0.031)	(0.028)	(0.028)	(0.029)	(0.029)	(0.029)
Full baseline controls	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*
Number of firms	472	472	472	472	472	472	472	472	472	472

Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term and a dummy for the year 2022 (omitted year 2021). 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 values—in the year before the lottery—of revenue, number of U.S. year-round workers, number of U.S. temporary workers, and number of foreign temporary workers.

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. 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 revenue was much larger than baseline revenue (toward the right of the graph) appear less affected by the lottery outcome than firms whose revenue was much smaller than baseline 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 baseline U.S. employment. At the center of mass, the distribution for lottery-winning firms is very close to the distribution for lottery-losing firms. Differences between the distributions are only visually apparent at the tails: Firms that experienced high growth in U.S. employment relative to baseline (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 relative to baseline (toward the left) appear to mitigate that decline when they win the lottery.

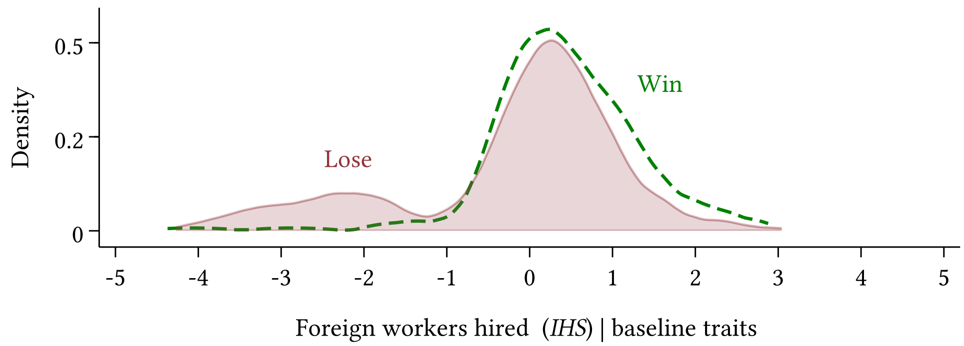
6.3 Effect of low-skill foreign employment on investment and profit

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. '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 specifications in the table are identical to those in Table 2.

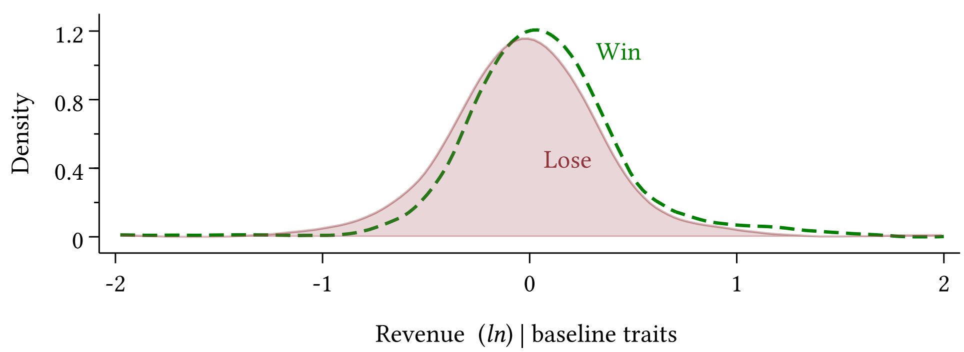
In the first two columns of Table 4, investment is correlated with foreign temporary worker employment across firms with an elasticity of 0.403. In columns 3–4. winning the lottery causes investment to rise by a factor of 3.8, corresponding to a semielasticity of 1.325. In columns 5–

Figure 6: Reduced-form effects of the lottery

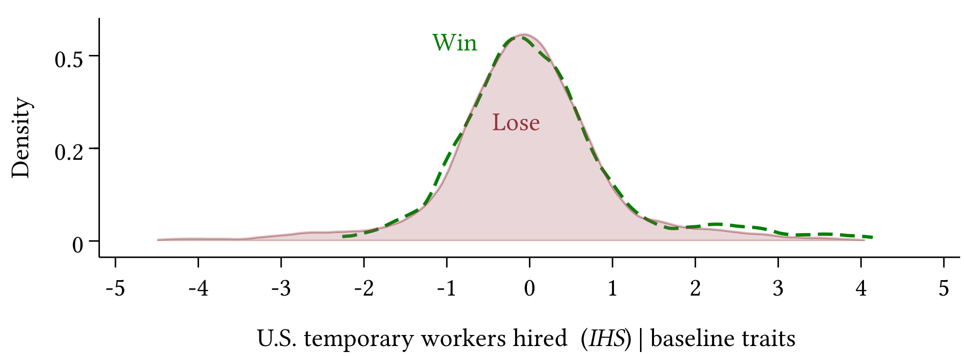
(a) *Foreign workers employed*



(b) *Revenue*



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



The unit of analysis is firms, pooled 2021 and 2022 samples. '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*). Residuals are estimated controlling for the full set of baseline traits, corresponding to column 4 in Tables 2 and 3, measured in the year prior to the lottery.

Table 4: Effect of foreign worker employment on firm investment

Dep. var:	*Investment (IHS)*									
Estimator:	OLS				2SLS		OLS		2SLS	
Instrument:					*Lottery win*				*Expected share*	
Foreign employed (IHS)	0.419	0.403			2.067	2.072			1.434	1.466
	(0.175)	(0.197)			(0.736)	(0.721)			(0.604)	(0.610)
Anderson-Rubin p*-val.*	—	—			*0.002*	*0.001*			*0.013*	*0.012*
Lottery win			1.314	1.325						
			(0.418)	(0.415)						
Expected share							3.371	3.390		
							(1.366)	(1.366)		
Revenue, baseline (ln)	0.526	0.611	0.746	0.740	−0.282	0.045	0.745	0.751	0.028	0.250
	(0.209)	(0.266)	(0.193)	(0.258)	(0.399)	(0.367)	(0.193)	(0.261)	(0.347)	(0.338)
Full baseline controls	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*
Number of firms	456	456	456	456	456	456	456	456	456	456

Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term and a dummy for the year 2022 (omitted year 2021). 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 values—in the year before the lottery—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

Dep. var:	*Change in profit rate, year-on-year*									
Estimator:	OLS				2SLS		OLS		2SLS	
Instrument:					*Lottery win*				*Expected share*	
Foreign employed (IHS)	0.077	0.090			0.141	0.152			0.146	0.151
	(0.022)	(0.021)			(0.075)	(0.075)			(0.068)	(0.067)
Anderson-Rubin p-*val.*	—	—			*0.065*	*0.047*			*0.029*	*0.024*
Lottery win			0.090	0.094						
			(0.049)	(0.048)						
Expected share							0.321	0.327		
							(0.147)	(0.146)		
Revenue, baseline (ln)	−0.151	−0.195	−0.113	−0.166	−0.182	−0.215	−0.113	−0.166	−0.184	−0.214
	(0.043)	(0.061)	(0.037)	(0.058)	(0.056)	(0.066)	(0.037)	(0.059)	(0.052)	(0.064)
Full baseline controls	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*
Number of firms	441	441	441	441	441	441	441	441	441	441

Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term and a dummy for the year 2022. 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.

6, foreign employment causes greater investment with an elasticity of 2.072 (Anderson-Rubin p-value 0.001) using the 'lottery win' instrument. The same test using the 'expected share' instrument, in column 10, yields a causal elasticity of 1.466 (Anderson-Rubin p-value 0.012). 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 in general 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).[27] 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 $\%\Delta C$):

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

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 year-on-year growth of firms' profits for the current year. 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 0.090. In the next two columns, winning

[27] 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 or 2022] have been higher or lower than the same period of last year [2021 or 2020]?*"

the lottery has a positive reduced-form effect on growth in the profit rate, with a causal semielasticity of 0.094 that is statistically distinguishable from zero at conventional levels. In columns 6 and 10, regardless of which instrument is used, foreign employment has a positive effect on growth in the profit rate, with a causal elasticity of 0.15 (Anderson-Rubin p value 0.024–0.047).

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 implies that a doubling of foreign-worker employment would raise dollar-value profits by 40%, because the 15.2% increase in the rate of profit as a fraction of revenue (Table 5, col. 6) is augmented by the 21.8% increase in revenue (Table 2, col. 6).[28] This large positive effect on the profitability of the firm is consistent with the large positive effect on capital investment noted above.

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. The first set of these tests was prespecified. A second set was 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 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, do not differ to a statistically significant degree between early responders (less than median response time) and late responders (more than median response time), in tests

[28] Dollar-value profits for lottery-losing firms are $R_\ell \cdot \pi_\ell$, which would rise by a factor of $\frac{1.218 R_\ell \cdot 1.152 \pi_\ell}{R_\ell \pi_\ell} = 1.403$.

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 provide their postal code (11.8% of responses and 2.3% of the core sample). Such firms could not be assigned to a 'rural' or 'urban' environment, preventing their inclusion in our prespecified tests for heterogeneous effects by rural/urban location. The core results of Tables 2–4 are statistically indistinguishable in subsamples of firms that did or did not provide a postal code (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). The corresponding p-values shift to 0.023, 0.23, and 0.002. That is, this correction does not alter the qualitative statistical inference above, as might be expected in a study with a small number of prespecified outcomes.[29]

7.2 Alternative specifications

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

First, we test whether the results for one of our two pre-specified primary outcomes—the employment of US workers—are robust to regression specifications that do not use the inverse hyperbolic sine (IHS) transformation. (The other primary outcome, revenue, always exceeds zero.) Table 6 reestimates the reduced-form regressions in columns 4 and 8 of Table 3 using the Poisson Pseudo-Maximum-Likelihood (PPML) estimator due to Silva and Tenreyro (2006),

[29]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'.

Table 6: Effect of foreign worker employment on U.S. employment: Robustness to specifications without the IHS transformation

Dep. var:	U.S. temporary workers	
Estimator:	PPML	PPML
Lottery win	0.292	
	(0.188)	
Expected share		1.157
		(0.573)
p-*val.*	*0.122*	*0.043*
Full baseline controls	*Yes*	*Yes*
Number of firms	472	472

PPML is the Poisson Pseudo-Maximum-Likelihood estimator due to Silva and Tenreyro (2006). The dependent variable is the untransformed value of current-year US temporary worker employment. Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term and a dummy for the year 2022 (omitted year 2021). *Full baseline controls* are the *untransformed* values—in the year before the lottery—of revenue, number of U.S. year-round workers, number of U.S. temporary workers, and number of foreign temporary workers. 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.

as recommended by Chen and Roth (2024), using exclusively untransformed values of the regressors and regressand. This exercise yields a larger and more statistically precise estimate of the effect of the lottery result on firms' employment of low-skill US workers. Using the 'Lottery win' instrument in column 1, the causal semielasticity of winning on US worker employment is 0.292, with a p-value of 0.122 on the test of the null hypothesis of no effect, compared to the corresponding estimate of 0.116 with a p-value of 0.223 from Table 3. Using the 'Expected share' instrument in column 2, the coefficient estimate is 1.157 with a p-value of 0.043 on the test of the null hypothesis of no effect, compared to the corresponding estimate of 0.136 with a p-value of 0.633 from Table 3. The two coefficient estimates in Table 6 agree in magnitude: Given the average difference in 'expected share' between 'winning' and 'losing' firms, the coefficient estimate on 'expected share' in column 2 implies that the causal semielasticity of winning is 0.321, close to the estimate of 0.292 in column 1.[30]

[30] In the core firm sample the average difference in 'expected share' for winners is 0.914 and for losers is 0.636, thus $(0.914 - 0.636) \times 1.157 = 0.321$.

Second, 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 substantively invariant to the use of randomization inference (presented in the Appendix).

Third, 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 (presented in the Appendix). The causal elasticity of *median* revenue to foreign employment is 0.11 (s.e. 0.040); the causal elasticity of *median* U.S. temporary worker employment is statistically indistinguishable from zero.

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."[31]

8.2 Heterogeneity in the treatment effect: Theory

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). Intu-

[31] 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.

itively, 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 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 $e_N \longrightarrow \infty$.

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.[32] Beyond this, the greater diversity of workers and firms in urban relative to rural areas would tend to create relatively more "classical" monopsony power for urban employers. The Appendix presents a minimal formal spatial duopsony model of this intuitive distinction.

8.3 Heterogeneity in the treatment effect: Empirics

In sum, the model and thus the pre-analysis plan predicted relatively more positive 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.

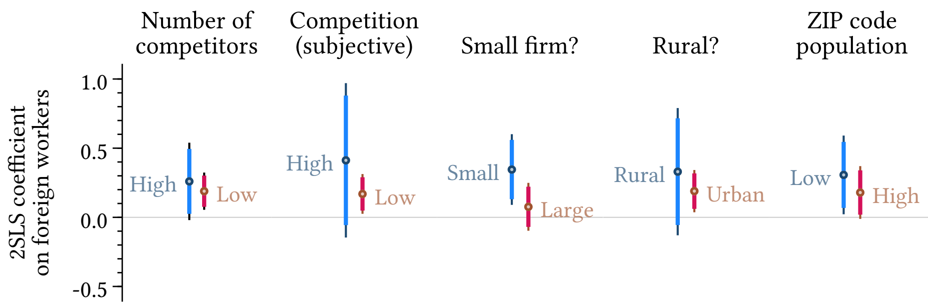
Figure 7 graphically presents empirical tests of these predictions, in the first three columns of panels (a) through (c). The vertical axis in the three panels shows the effect of foreign employment on each outcome: the 2SLS regression coefficient on foreign temporary employment using the 'lottery win' 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.

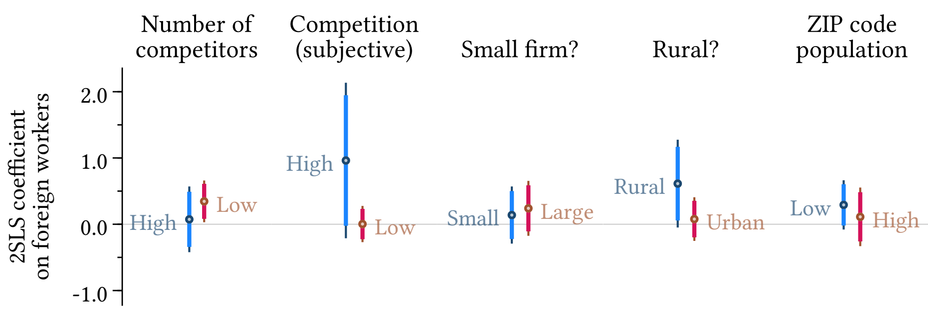
[32] This result is derived more formally with a simple Hotelling duopsony model in the Appendix.

Figure 7: Heterogeneous effects of foreign workers employed

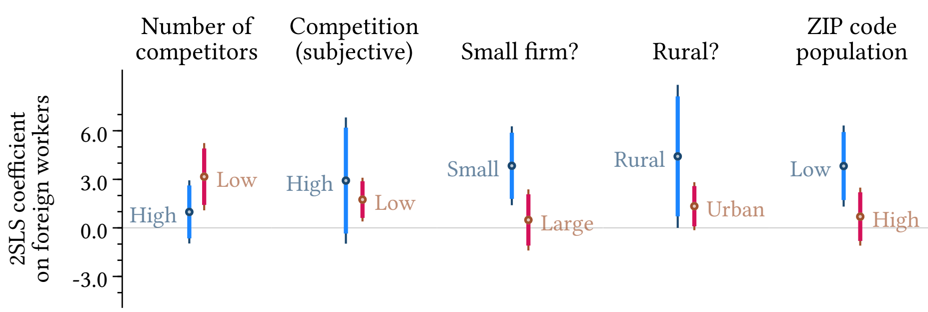
(a) *Effect on revenue*



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



(c) *Effect on investment*



The unit of analysis is firms. Pooled 2021 and 2022 data. The vertical axis in each pane shows the 2SLS coefficient on foreign workers employed (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. "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. "Rural" firms are those whose postal 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 that face greater objective or subjective competition, and firms that are smaller, exhibit larger effects of foreign employment on revenue. The revenue effect is more than double in magnitude for firms facing high subjective competition relative to low competition, and is roughly triple 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.

In the tests for predicted heterogeneity of the effect on employment by competitive environment, the results are more mixed (Figure 7b, cols. 1–3). The coefficient estimates for firms facing high subjective competition are much higher than for firms facing low subjective competition, but the coefficient estimates for small firms are similar in magnitude to the coefficients for large firms, and the coefficient estimate for firms facing higher numbers of competitors are lower in magnitude than for those facing fewer competitors.

For the investment outcome (Figure 7c, cols. 1–3), the theoretical prediction is corroborated for two of the three outcomes. As predicted, the effect of foreign employment on investment is much greater for small businesses. Firms facing higher subjective competition have exhibit a more positive elasticity as predicted, but firms facing greater numbers of competitors exhibit a lower coefficient—against the prediction.

Second, the tests uniformly 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). The magnitude of the revenue effect is 83% greater in rural areas relative to urban areas, and 71% greater in low-population postal areas relative to high-population areas. The magnitude of the U.S. employment effect is 7.9 times greater in rural areas than urban areas. In the prespecified subsample of rural firms, the U.S. employment effect is statistically significantly different from zero at the level of Anderson-Rubin $p = 0.053$. The investment effect is 3.3 times higher in rural 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 diverge from that of the core results in any of the 30 of the prespecified subsamples in Figure 7.

8.4 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.[33] 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

[33] 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 (1936, 10) described as necessary "to begin interpreting facts".

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.[34]

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.[35]

Labor shares: Finally, the share of year-round native employees in total employment in the core firm survey sample is 0.421 (std. err. 0.013, $N = 470$), implying $\gamma = 0.470 \cdot (1 - s_K) \cdot \frac{\eta}{\eta-1} = 0.313$ 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 $1 - \alpha = 0.668$ (std. err. 0.012, $N = 470$).

These parameters allow us to estimate the value of σ implied by the regressions in Section 6, specifically the more conservative casual elasticity result using the expected-share instrument in the final column of Table 3. The results are presented in Table 7. Our preferred estimate uses the parameter estimates in the middle of the plausible ranges above: $\eta \approx 8$, $\gamma \approx 0.35$, and $\beta \approx 0.35$. These yield the estimate $\sigma = 1.26$, with a 95% confidence interval (0.12, 2.39), 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 es-

[34] 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).

[35] 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).

timates 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 $\sigma \approx 1.3$ is somewhat lower than prior, already-low estimates measured in the aggregate rather than at the firm level. Cortés (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 3.37.[36] 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.

8.5 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.

That said, this firm-level analysis contains some information about aggregate effects. Prima

[36] 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).

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
$\beta = 0.25$	1.50	1.25	1.06	1.90	1.47	1.18	2.22	1.62	1.25
	(0.55)	(0.52)	(0.52)	(0.59)	(0.55)	(0.54)	(0.59)	(0.56)	(0.55)
$\beta = 0.35$	1.33	1.10	0.93	1.63	1.26	1.00	1.87	1.36	1.04
	(0.56)	(0.54)	(0.54)	(0.62)	(0.58)	(0.56)	(0.65)	(0.60)	(0.58)
$\beta = 0.45$	1.16	0.95	0.79	1.37	1.05	0.83	1.54	1.11	0.85
	(0.58)	(0.56)	(0.56)	(0.64)	(0.59)	(0.57)	(0.68)	(0.62)	(0.58)

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 0.668, estimated from the U.S. employment regression in Table 3, col. 10, using the core firm sample $N = 472$. Details in Appendix.

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 production 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 and 2022 (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 A15. 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 2021/2022. This is because in 2020, losing firms greatly outnumbered winning firms (Figure 4). In 2021, winning firms outnumbered losing 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 in all three 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 2024).

8.6 Black-market employment: A rough forensic analysis

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 by regulation on H-2B hiring but absent from the black market.[37] Empirically, Orrenius and Zavodny (2020) test for relationships between several different measures of immigration enforcement and 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.

Moreover, 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 than this. Beyond the fact that the treatment effects of losing the lottery are nonzero, the magnitude of those effects is informative about possible substitution into unobserved black-market employment by lottery-losing firms. Consider the Euler equation (5), which relates marginal changes in *observed* inputs to a marginal change in *observed* revenue. But now, the true total employment of immigrant workers by lottery-losing firms is greater than

[37] 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).

the observed employment of authorized immigrants: $I_\ell^* \equiv (1+\phi)I_\ell$ such that $\phi > 0$ is the number of unobserved (black market) immigrant employees as a fraction of the number of observed immigrant employees. Solving for the unobserved black-market fraction ϕ gives

$$\phi \approx \underbrace{\ln\frac{I_w}{I_\ell}}_{\sim 0.618} + \underbrace{\frac{s_N}{s_I}\ln\frac{N_w}{N_\ell}}_{\sim 0.219} - \underbrace{\frac{1-s_K}{s_I}\ln\frac{R_w}{R_\ell}}_{\sim 0.828} = 0.009, \tag{17}$$

where the values on the right hand side are filled in from the empirics above.[38] 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 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.[39] 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."[40] While plausible, these concerns run counter to employers' frequent 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.

[38]The estimate of $\ln\frac{I_w}{I_\ell} = 0.618$ is from the first-stage regressions in the Appendix; the estimate of $\ln\frac{N_w}{N_\ell} = 0.116$ is from Table 3, col. 4; the estimate of $\ln\frac{R_w}{R_\ell} = 0.135$ is from Table 2 col. 4; $s_K = s_H = 0.35$ and $\frac{s_N}{s_I} = 0.668/(1-0.668) = 1.84$, and thus $s_I = (1-0.668)\times(1-s_K-s_H) = 0.106$ from Section 8.4.

[39]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).

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

The effectively 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 and 2022 lotteries 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 +0.20–22). It also does not reduce, and may raise their employment of U.S. workers overall (elasticity +0.06–0.19, statistically imprecise). In a pre-specified subsample of rural firms, as the model predicts, we find a statistically significant positive effect of foreign worker employment on native employment (elasticity +0.61, Anderson-Rubin p=0.05).

These results are robust across several pre-registered subsamples. Scale effects are generally 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). Notably, we released analysis of the 2021 lottery results prior to collecting data on the 2022 lottery results, while retaining identical survey instruments and regression specifications for both lotteries—another dimension in which the analysis is pre-specified and unusally transparent.

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 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 2023) 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 +1.5–2.1), 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.[41]

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.[42] 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 find a positive but statistically imprecise response), both of which likely complement the hiring of U.S. seasonal workers.

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[41] 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.

[42] 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 — May 2024

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.

Contents

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 $I \le \bar{I}$ (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 $I \le \bar{I}$ 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 $I_u \le \bar{I}$, it must be that unconstrained firms have profits that are at least as large as constrained firms, and therefore $\frac{1}{\eta}D^{\frac{1}{\eta}}Q_u^{\frac{\eta-1}{\eta}} \ge \frac{1}{\eta}D^{\frac{1}{\eta}}Q_c^{\frac{\eta-1}{\eta}} + \lambda I_c$. But this implies that unconstrained revenues are weakly higher $D^{\frac{1}{\eta}}Q_u^{\frac{\eta-1}{\eta}} \ge D^{\frac{1}{\eta}}Q_c^{\frac{\eta-1}{\eta}}$, and therefore that unconstrained output is weakly higher $Q_u \ge Q_c$. 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:[43]

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

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 $\eta > \sigma$.

[43]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

$$\begin{aligned} \ln(R_w/R_\ell) &\approx \frac{s_I}{1-s_K}\ln(I_w/I_\ell)+\frac{s_N}{1-s_K}\ln(N_w/N_\ell) &\text{(A.25)} \\ &= \frac{s_I}{1-s_K}\left(1+s_N\left[\frac{(\eta-1)(1-\beta-\gamma\sigma)-(\sigma-1)}{c_4}\right]\right)\ln(I_w/I_\ell). &\text{(A.26)} \end{aligned}$$

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:

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

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

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

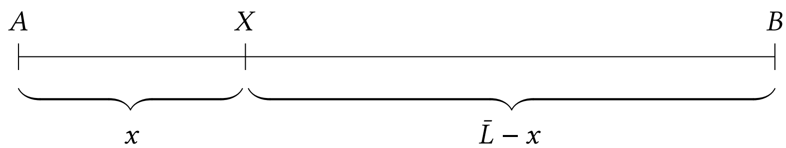
This is a modified version of the expression from Lemma 2, and shows that the native employment response to immigration is increasing in magnitude in the native labor supply elasticity. The response is zero when native supply is inelastic ($e_N = 0$), and converges to the expression in Lemma 2 as the 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, pooled 2021 and 2022.

Appendix Table A1: Summary statistics

	mean	*sd*	*min*	*max*	*count*
Revenue ($)_curr	8.7*e*+06	5.3*e*+07	5000.000	1.0*e*+09	472
Foreign temp. workers employed	22.570	44.817	0.000	412.000	472
U.S. temp. workers employed	31.867	130.207	0.000	1821.000	472
U.S. perm. workers employed	50.227	215.423	0.000	3600.000	471
Investment ($)	4.2*e*+05	1.7*e*+06	0.000	3.0*e*+07	456
ln Revenue	14.729	1.289	8.517	20.723	472
ihs Foreign temp. workers employed	2.855	1.501	0.000	6.714	472
ihs Foreign temp. workers requested	3.556	1.053	1.444	7.281	472
ihs U.S. temp. workers employed	2.415	1.828	0.000	8.200	472
ihs U.S. perm. workers employed	3.270	1.498	0.000	8.882	471
ihs Investment	10.835	4.672	0.000	17.910	456
Change in profit rate, year-on-year	0.025	0.488	−2.357	4.321	441
Lottery win (IV)	0.314	0.464	0.000	1.000	472
Expected share of workers (IV)	0.723	0.149	0.539	0.927	472
Competitors (number)	371.470	4878.795	0.000	1.0*e*+05	447
Competition on price (subjective)	3.087	0.789	1.000	4.000	461
Rural (non-metropolitan)	0.260	0.439	0.000	1.000	461
Population of ZIP code	2.2*e*+04	1.6*e*+04	23.000	6.5*e*+04	462
Region: Northeast	0.206	0.405	0.000	1.000	472
Region: Midwest	0.324	0.469	0.000	1.000	472
Region: South	0.341	0.475	0.000	1.000	472
Region: West	0.108	0.311	0.000	1.000	472

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

A4 Compare sample to universe

Table A2 compares the number of H-2B workers by industry in the sampling universe to the number employed by survey-respondent firms. Groundskeeping and landscaping is the most common industry in both the universe (39.5% of workers) and the sample (46.2% of workers). The survey sample somewhat overrepresents forestry and seafood processing workers; it somewhat underrepresents workers in hospitality, construction, restaurants, carnivals, and golf courses/country clubs.

Table A3 displays the corresponding comparison by rural/urban location of the employer. The geographic distribution of firms in the sample (34% rural) is close to the distribution in the universe (32% rural).

A5 First-stage regressions

Table A4 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 causes firms' employment of low-skill immigrants to fall by $1 - e^{-0.618} = 46\%$.

Appendix Table A2: Compare industry breakdown of H-2B workers among survey respondents with industry breakdown in sampling universe, 2021 and 2022 pooled

Industry	Universe		Sample	
	Workers	*Frac.*	*Workers*	*Frac.*
Landscaping	187,016	0.395	5,874	0.462
Golf courses/country clubs	46,536	0.098	478	0.038
Hospitality	43,349	0.091	894	0.070
Forestry	42,146	0.089	1,978	0.155
Seafood processing	42,100	0.089	1,292	0.102
Construction	38,928	0.082	382	0.030
Restaurants	11,856	0.025	66	0.005
Carnivals	10,534	0.022	637	0.050
Other	51,532	0.109	1,123	0.088

The unit of observation is H-2B workers employed by firms that entered the January 2021 and January 2022 lotteries. The number in the universe is the number petitioned for, whether or not the petition was successful. The number in the sample is the number reported actually employed by survey-responding firms.

Appendix Table A3: Workers requested by rural/urban employer in sampling universe vs. survey sample

	Frequency		Proportion	
Employer address	*Universe*	*Sample*	*Universe*	*Sample*
Rural	148,686	3,515	0.316	0.340
Urban	322,242	6,812	0.684	0.660

Years 2021 and 2022 pooled. The unit of observation is workers requested on DOL petitions entered into the DOL lottery for H-2B visas for the second half of each fiscal year (universe) and H-2B workers employed by survey-respondent firms (sample). Includes only workers on petitions in the universe and sample for which firms reported a postal code for the employer.

A6 Industry-level parameter assumptions

Table A5 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 0.421 (std. err. 0.013, $N = 470$). This implies $\gamma = 0.470 \cdot (1 - s_K) \cdot \frac{\eta}{\eta-1} = 0.313$ for $\eta = 8$. The average firm's share of foreign workers in all temporary employment is 0.572, implying a native share of the inner labor nest of 0.668 (std. err. 0.012 $N = 470$).

IBISWorld rates concentration in each industry on a three-point scale. For all of the industries in Table A5 except 'amusement parks', it assesses concentration as 'low'. It describes the 'landscaping' industry in the United States with the follow passage, typical of the other low-concentration industries: "*The Landscap-*

Appendix Table A4: First stage regressions, pooled 2021 and 2022

Dep. var.:	Foreign hired (IHS)	
Lottery win	0.618	
	(0.112)	
Expected share		2.233
		(0.374)
U.S. temporary hired, baseline (IHS)	−0.027	−0.025
	(0.035)	(0.035)
Revenue, baseline (ln)	0.348	0.354
	(0.066)	(0.067)
Foreign hired, baseline (IHS)	0.313	0.310
	(0.039)	(0.040)
U.S. year-round hired, baseline (IHS)	0.009	0.004
	(0.049)	(0.049)
Number of firms	472	472

Presents the first-stage regressions from the rightmost columns of Tables 2–5. 'Baseline' is 2020 for the 2021 lottery, and 2021 for the 2022 lottery. Includes constant term and a year dummy (for 2022). Robust standard errors in parentheses. 'IHS' is inverse hyperbolic sine.

ing 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 A6 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.

Appendix Table A5: 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.

A8 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 A5 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.

A9 Robustness to influential observations

Table A12 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 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.

A10 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 A13 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 H-2B employment on the instrument, controlling for the standard baseline 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.

Appendix Table A6: 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)	0.004	-0.002
	(0.021)	(0.007)
Foreign hired 2020 (IHS)	-0.002	0.001
	(0.013)	(0.004)
U.S. year-round hired 2020 (IHS)	-0.018	-0.002
	(0.018)	(0.006)
U.S. temporary hired 2020 (IHS)	-0.011	-0.004
	(0.012)	(0.004)
Number of firms	472	472
R^2	0.033	0.017

Robust standard errors in parentheses. *IHS* is inverse hyperbolic sine. Pooled 2021 and 2022 sample. All regressions include constant term and dummy variable for 2022.

A11 Full regression results from tests for heterogeneous treatment effects

Table A11 reports the full regression results underlying the coefficient plots in Figure 7.

A12 Item nonresponse

The most important form of item nonresponse in the survey was firms that declined to give thir postal code, preventing us from including them in our prespecified tests for heterogeneous effects by rural location. Table A7 tests the sensitivity of the core results to restricting the sample to firms that did give a postal code. The core results in Tables 2–4 are substantially the same for the subgroups that did or did not provide a ZIP code. The coefficient is not statistically significantly different in the 'No ZIP' subgroup for any of the three outcomes.

A13 Effect on U.S. year-round employment

The preanalysis plan specified reporting tests of the effect of employing foreign temporary low-skill workers on an additional secondary outcome: employment of year-round, generally higher-skill U.S. workers. Table A8 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 exhibit 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) any positive *effect* of foreign hiring on year-round U.S. employment (elasticity 0.07–0.09) is statistically indistinguishable from zero. The present research design only measures effects in the short term, that is, within the same half-year as the change in foreign-worker hiring occurred.

Appendix Table A7: Tests for sensitivity of the results to item nonresponse for firm postal code

Dep. var.:	Revenue (ln)	U.S. hired (IHS)	Investment (IHS)
Specification:	2SLS	2SLS	2SLS
Foreign hired (IHS)	0.209	0.165	2.093
	(0.082)	(0.151)	(0.732)
Foreign hired (IHS) × No ZIP	0.360	0.884	−0.096
	(0.453)	(1.105)	(8.192)
No ZIP	−1.031	−2.470	−1.799
	(1.485)	(3.513)	(24.699)
Full baseline controls	*Yes*	*Yes*	*Yes*
Number of firms	472	472	456

Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term, full baseline controls, and a dummy for the year 2022. 'No ZIP' is an indicator variable taking the value one if the respondent left the response for their postal code blank, 0 otherwise. All regressions are 2SLS with two endogenous variables and two instruments. The endogenous variables are 'Foreign hired' and its interaction with 'No ZIP'. The instruments are the 'lottery win' instrument and its interaction with 'No ZIP'.

A14 Testing for a competition channel for treatment effects

Table A9 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 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 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.

A15 Partial replication in 2020

We partially replicate the 2021/2022 natural experiment in fiscal year 2020. This analysis was not pre-specified 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,

Appendix Table A8: Effect of foreign worker employment on higher-skill, year-round U.S. employment

Dep. var:	*U.S. year-round workers (IHS)*									
Estimator:	OLS				2SLS		OLS		2SLS	
Instrument:					*Lottery win*				*Expected share*	
Foreign temp. employed (IHS)	0.042	0.043			0.071	0.068			0.084	0.092
	(0.015)	(0.014)			(0.079)	(0.074)			(0.067)	(0.061)
Anderson-Rubin p-*val.*	—	—			*0.369*	*0.360*			*0.206*	*0.128*
Lottery win			0.043	0.042						
			(0.048)	(0.046)						
Expected share							0.187	0.205		
							(0.148)	(0.136)		
U.S. year-round workers, baseline (IHS)	0.926	0.875	0.938	0.876	0.919	0.876	0.938	0.876	0.915	0.876
	(0.027)	(0.041)	(0.025)	(0.042)	(0.028)	(0.041)	(0.025)	(0.042)	(0.028)	(0.041)
Full baseline controls	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*
Number of firms	471	471	471	471	471	471	471	471	471	471

Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term and a dummy for the year 2022. 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 A9: Effects on Competitive Environment

Dep. var.:	High subjective competition, indicator (0,1)		Subjective competition, raw score (1–4)	
Lottery win	−0.031		−0.000	
	(0.048)		(0.080)	
Expected share		−0.140		−0.049
		(0.150)		(0.253)
Observed baseline controls	*Yes*	*Yes*	*Yes*	*Yes*
Number of firms	461	461	461	461

Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term and a dummy for the year 2022. 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 a few months after the end of the hiring season, referring to *current* (not retrospective) conditions. *Observed baseline controls* are the previous-year values of revenue, number of U.S. year-round workers, number of U.S. temporary workers, and number of foreign temporary workers.

our survey did not ask about firms' lottery-letter result from 2020. The 2021 survey did ask 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 2021/2022, 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 2021/2022 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.[44] For this reason the 2020 replication is partial rather than exact.

Figure A2 reports the DOL decision dates for the 2020 lottery. The pattern is highly similar to the pattern in the corresponding decision dates for 2021/2022, with the exception that no supplemental visas were issued in 2020.

[44]The regressions with investment as an outcome cannot be done in this setting because the survey does not ask about investment in 2020.

Figure A3 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 2021/2022 lotteries.

Table A10 presents the results of the 2020 replication exercise for the revenue and U.S. employment outcomes, corresponding to the 2021/2022 results in Tables 2 and 3 above. The magnitudes of the coefficient estimates are broadly similar in this independent experiment.

For example, the reduced-form regression of revenue on 'lottery win' yields an estimate of 0.223 in 2020 (Table A10, col. 2), compared to an estimate of 0.135 from 2021/2022 (Table 2, col. 4). The reduced-form regression of revenue on 'expected share' yields an estimate of 0.348 in 2020 (Table A10, col. 4), compared to an estimate of 0.443 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 A10, col. 7) versus 0.116 in 2021 (Table 3, col. 4); and a coefficient on 'expected share' of 0.371 in 2020 (Table A10, col. 9) versus 0.136 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 A10, col. 5; p-val. 0.111). But the 2020 replication is more informative when considered in conjunction with the results from 2021/2022—an independently randomized natural experiment—where the corresponding coefficient estimate takes the similar magnitude of 0.198 (Table 2, col. 10; p-val. 0.004). 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 same comparison for the effect of foreign employment on U.S. worker employment (in 2020, Table A10, col. 10, coefficient 0.166 with p-val. 0.262; in 2021/2022, Table 3, col. 6, coefficient 0.188 with p-val. 0.219) again shows striking similarity.

The 2020 replication serves as a check not just on internal validity but on external validity. The US labor market was very tight during 2021/2022, 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 seasonally-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.[45] The average national unemployment rate in the second half of fiscal 2020 was 10.9%; in 2021 it was 5.5%.[46] The similar magnitude of the point estimates in Tables 2, 3, and A10 is inconsistent with any crucial dependency of the results on the tighter labor market conditions of 2021/2022.

A16 Survey questionnaire

Figure A6 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.

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

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

Appendix Table A10: Robustness: The 2020 Lottery

Dep. var:	*Revenue 2020 (ln)*					*U.S. temporary workers 2020 (IHS)*				
Estimator:	OLS	OLS	2SLS	OLS	2SLS	OLS	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.

Appendix Table A11: 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 (ln)										
Foreign employed (IHS)	0.260	0.189	0.412	0.169	0.345	0.076	0.330	0.189	0.307	0.179
	(0.143)	(0.069)	(0.285)	(0.073)	(0.130)	(0.088)	(0.235)	(0.078)	(0.145)	(0.097)
And.-Rubin p-*val.*	0.074	0.006	0.117	0.024	0.006	0.408	0.150	0.018	0.032	0.069
N	272	200	169	303	226	246	131	341	226	246
Dep. var.: U.S. temporary workers employed (IHS)										
Foreign employed (IHS)	0.075	0.346	0.962	0.004	0.139	0.240	0.613	0.078	0.292	0.112
	(0.253)	(0.161)	(0.599)	(0.139)	(0.220)	(0.211)	(0.338)	(0.168)	(0.190)	(0.226)
And.-Rubin p-*val.*	0.771	0.036	0.044	0.980	0.538	0.260	0.053	0.646	0.131	0.623
N	272	200	169	303	226	246	131	341	226	246
Dep. var.: Investment (IHS)										
Foreign employed (IHS)	0.986	3.161	2.922	1.748	3.837	0.493	4.418	1.339	3.820	0.695
	(0.995)	(1.060)	(1.988)	(0.689)	(1.243)	(0.963)	(2.253)	(0.756)	(1.278)	(0.913)
And.-Rubin p-*val.*	0.309	0.000	0.108	0.005	0.000	0.609	0.001	0.067	0.000	0.441
N	262	194	160	296	217	239	121	335	219	237

Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term and a dummy for the year 2022. All regressions are two-stage least squares with 'Foreign employed (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. "High" number of competitors means greater than the median response. "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.

Appendix Table A12: Robustness: Quantile regressions (p50)

Dep. var:	Revenue (ln)		U.S. temporary workers (IHS)	
Estimator:	Quantile	IV quantile	Quantile	IV quantile
Foreign employed (IHS)	0.078	0.112	0.047	0.007
	(0.008)	(0.040)	(0.010)	(0.032)
Number of firms	472	472	472	456

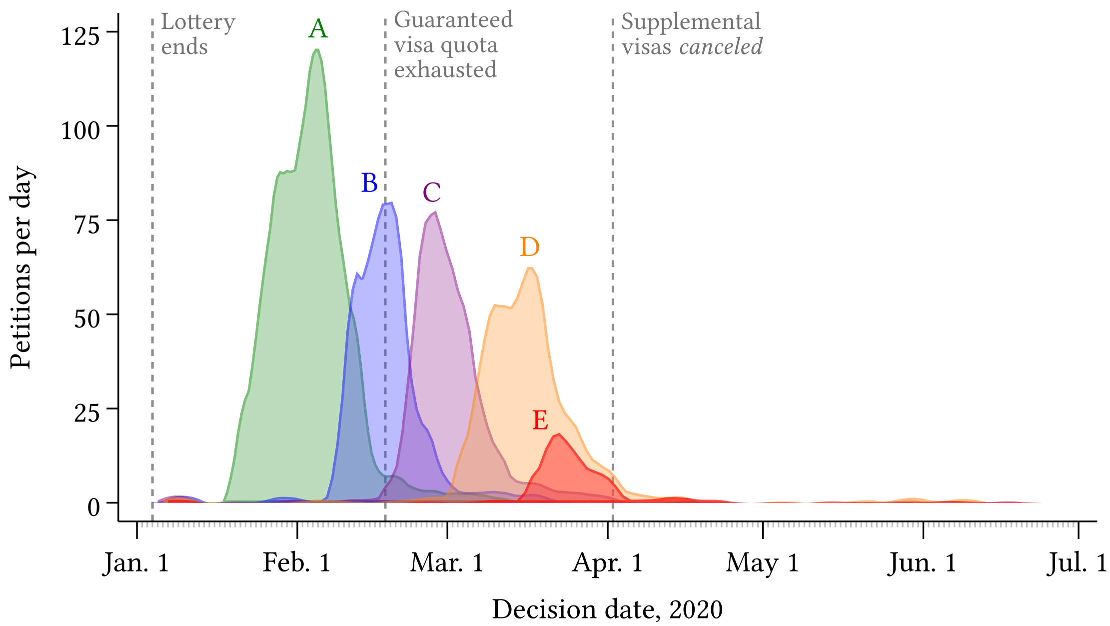
Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term, baseline controls, and a dummy for the year 2022. Quantile IV estimator due to Kaplan and Sun (2017) and Kaplan (2022). All regressions include constant term. Standard 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.

Appendix Table A13: Robustness: Randomization inference

Dep. var:	Foreign temp. workers employed (IHS)	Revenue (ln)	U.S. temp. workers employed (IHS)	Investment (IHS)
Lottery win	0.6176	0.1347	0.1160	1.3251
	(0.1121)	(0.0508)	(0.0952)	(0.4155)
Rand. inference p-val.	<0.001	0.005	0.235	0.003
Full baseline controls	*Yes*	*Yes*	*Yes*	*Yes*
Number of firms	472	472	472	456

Robust standard errors in parentheses. Uses Fisher's randomization inference implemented by Heß (2017). Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term and a dummy for the year 2022.

Figure A2: Foreign worker petition decision dates by lottery result, universe of firms, second half of fiscal year 2020

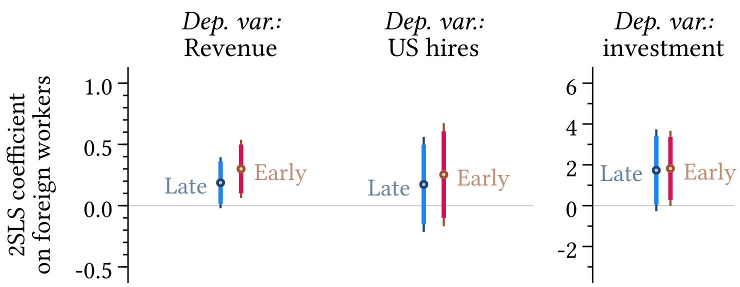


Shown is the universe of firms entering each lottery. Epanechnikov kernel densities, bandwidth 2 days. 'Decision date' is the date of the Department of Labor's decision on whether or not to certify each petition, a necessary condition of proceeding to petition USCIS for a visa. The 2020 lottery was conducted 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 A3: Defining a lottery 'win' at the firm level, 2020 lottery

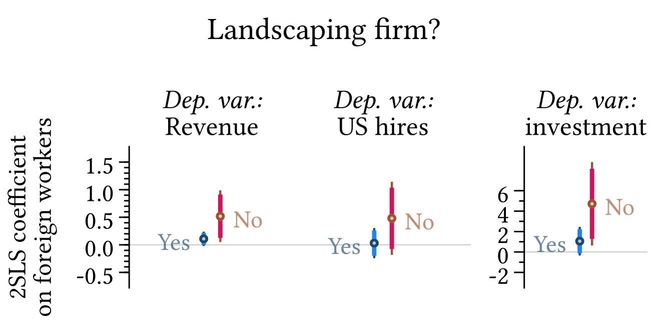
Only includes firms that voluntarily self-identified in the survey, allowing them to be matched to public records of their 2020 lottery result.

Figure A4: Test for nonresponse bias



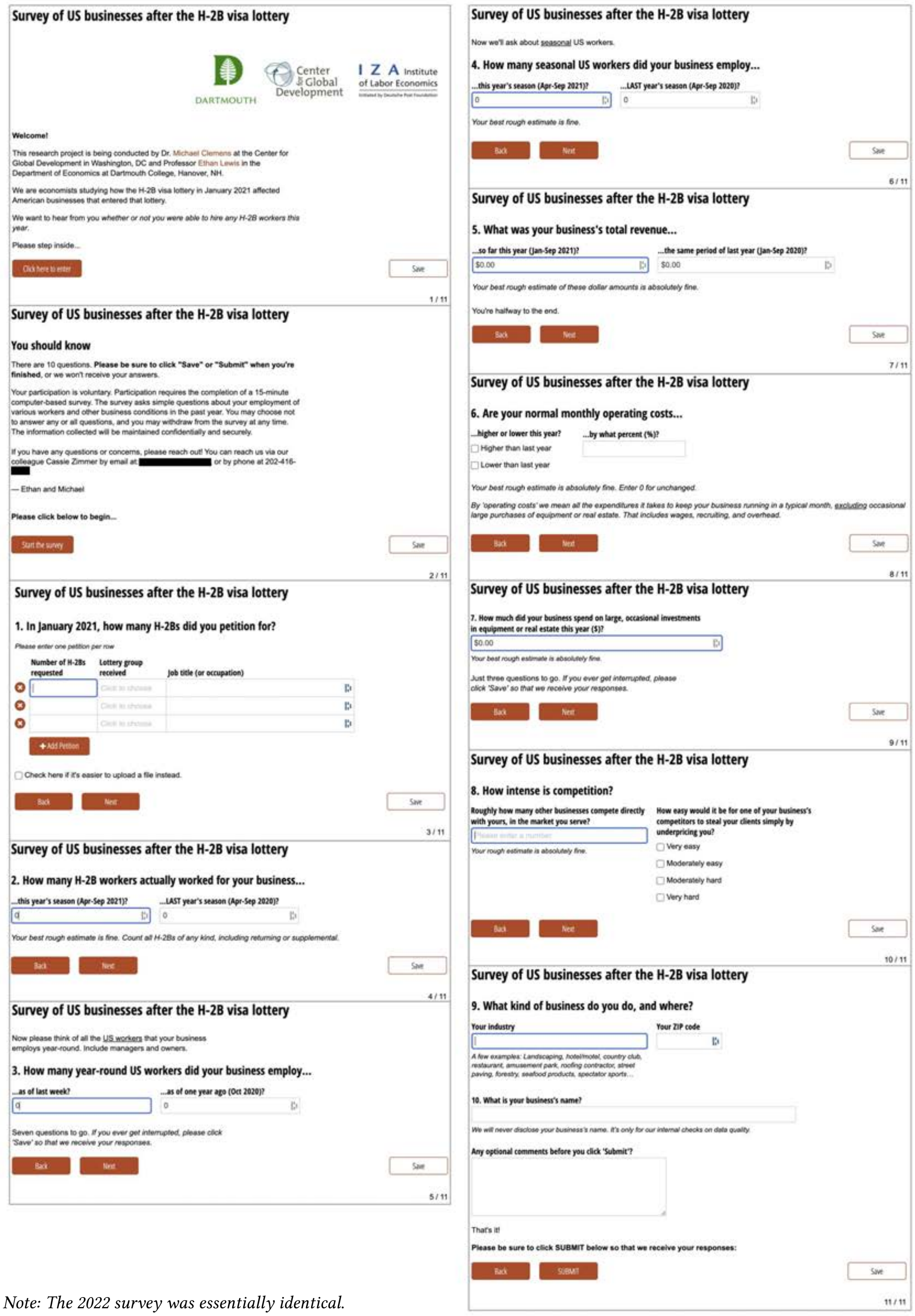
Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term and a dummy for the year 2022. 'Late' responses are those that took more than the median time (in that year's survey) to complete the survey after the first firm completed it. For the survey on 2021 operations, 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. For the survey on 2022 operations, the first response was received on March 10, 2023 at 2:05pm Eastern time; the median response was received April 7, 2023 at 3:38pm Eastern time. The vertical axis in each pane shows the 2SLS coefficient on foreign workers employed (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 A5: Test for bias from sampling overweight on groundskeeping/landscaping



Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term and a dummy for the year 2022. '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 (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: The 2021 firm survey questionnaire as respondents saw it on 11 screens



Survey of US businesses after the H-2B visa lottery

DARTMOUTH · Center for Global Development · IZA Institute of Labor Economics

Welcome!

This research project is being conducted by Dr. Michael Clemens at the Center for Global Development in Washington, DC and Professor Ethan Lewis in the Department of Economics at Dartmouth College, Hanover, NH.

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.*

Please step inside...

Click here to enter · Save

1 / 11

Survey of US businesses after the H-2B visa lottery

You should know

There are 10 questions. **Please be sure to click "Save" or "Submit" when you're finished,** or we won't receive your answers.

Your participation is voluntary. Participation requires the completion of a 15-minute computer-based survey. The survey asks simple questions about your employment of various workers and other business conditions in the past year. You may choose not to answer any or all questions, and you may withdraw from the survey at any time. The information collected will be maintained confidentially and securely.

If you have any questions or concerns, please reach out! You can reach us via our colleague Cassie Zimmer by email at [redacted] or by phone at 202-416-[redacted]

— Ethan and Michael

Please click below to begin...

Start the survey · Save

2 / 11

Survey of US businesses after the H-2B visa lottery

1. In January 2021, how many H-2Bs did you petition for?

Please enter one petition per row

Number of H-2Bs requested	Lottery group received	Job title (or occupation)
	Click to choose	
	Click to choose	
	Click to choose	

+ Add Petition

☐ Check here if it's easier to upload a file instead.

Back · Next · Save

3 / 11

Survey of US businesses after the H-2B visa lottery

2. How many H-2B workers actually worked for your business...

...this year's season (Apr-Sep 2021)? 0 · ...LAST year's season (Apr-Sep 2020)? 0

Your best rough estimate is fine. Count all H-2Bs of any kind, including returning or supplemental.

Back · Next · Save

4 / 11

Survey of US businesses after the H-2B visa lottery

Now please think of all the US workers that your business employs year-round. Include managers and owners.

3. How many year-round US workers did your business employ...

...as of last week? 0 · ...as of one year ago (Oct 2020)? 0

Seven questions to go. *If you ever get interrupted, please click 'Save' so that we receive your responses.*

Back · Next · Save

5 / 11

Survey of US businesses after the H-2B visa lottery

Now we'll ask about seasonal US workers.

4. How many seasonal US workers did your business employ...

...this year's season (Apr-Sep 2021)? 0 · ...LAST year's season (Apr-Sep 2020)? 0

Your best rough estimate is fine.

Back · Next · Save

6 / 11

Survey of US businesses after the H-2B visa lottery

5. What was your business's total revenue...

...so far this year (Jan-Sep 2021)? $0.00 · ...the same period of last year (Jan-Sep 2020)? $0.00

Your best rough estimate of these dollar amounts is absolutely fine.

You're halfway to the end.

Back · Next · Save

7 / 11

Survey of US businesses after the H-2B visa lottery

6. Are your normal monthly operating costs...

...higher or lower this year? ☐ Higher than last year ☐ Lower than last year · ...by what percent (%)?

Your best rough estimate is absolutely fine. Enter 0 for unchanged.

By 'operating costs' we mean all the expenditures it takes to keep your business running in a typical month, excluding occasional large purchases of equipment or real estate. That includes wages, recruiting, and overhead.

Back · Next · Save

8 / 11

Survey of US businesses after the H-2B visa lottery

7. How much did your business spend on large, occasional investments in equipment or real estate this year ($)? $0.00

Your best rough estimate is absolutely fine.

Just three questions to go. *If you ever get interrupted, please click 'Save' so that we receive your responses.*

Back · Next · Save

9 / 11

Survey of US businesses after the H-2B visa lottery

8. How intense is competition?

Roughly how many other businesses compete directly with yours, in the market you serve? Please enter a number

Your rough estimate is absolutely fine.

How easy would it be for one of your business's competitors to steal your clients simply by underpricing you?

☐ Very easy
☐ Moderately easy
☐ Moderately hard
☐ Very hard

Back · Next · Save

10 / 11

Survey of US businesses after the H-2B visa lottery

9. What kind of business do you do, and where?

Your industry · Your ZIP code

A few examples: Landscaping, hotel/motel, country club, restaurant, amusement park, roofing contractor, street paving, forestry, seafood products, spectator sports...

10. What is your business's name?

We will never disclose your business's name. It's only for our internal checks on data quality.

Any optional comments before you click 'Submit'?

That's it!

Please be sure to click SUBMIT below so that we receive your responses:

Back · SUBMIT · Save

11 / 11

Note: The 2022 survey was essentially identical.