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26-11 The Effect of Low-Skill Immigration Restrictions on US Firms and Workers: Evidence from a Randomized Lottery

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ABSTRACT

US firms hiring foreign workers in low-skill nonfarm jobs face a binding quota on the H-2B visa, allocated in part through a randomized lottery. We evaluate the quota's marginal impact using the lottery, a novel firm survey, and a preanalysis plan. Firms exogenously employing more H-2B workers in low-skill jobs increase production (elasticity 0.20–0.22), investment (1.5–2.1), and profits (0.15). The elasticity of substitution between H-2B and US workers is very low (0.8–2.0). Thus the effect on US employment is zero or positive overall, and positive in rural areas. Forensic analysis suggests similarly low substitutability of black-market labor.

JEL codes: F22, J61, D22

Keywords: low-skill immigration, US firms, randomized lottery, H-2B visa, US employment, low-skill jobs, elasticity of substitution

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Note: Firm survey approved by the Dartmouth College Committee for the Protection of Human Subjects #STUDY00032360. Pre-analysis plan irreversibly registered before data collection, at https://osf.io/zdyun; AEA RCT Registry: https://www.socialscienceregistry.org/trials/16896, data and code available at Clemens and Lewis (2025). We benefited from interactions with Suresh Naidu, Thomas Chaney, Melanie Morten, Ran Abramitzky, Olivier Blanchard, Jeff Kling, Dean Yang, Leah Boustan, Chris Walters, Joan Llull, William Collins, Paolo Falco, Chad Sparber, Jeremy Weinstein, Todd Schoellman, Nicolas Morales, Anna Maria Mayda, Joan Monras, Giovanni Peri, Muly San, Parag Mahajan, Sharat Ganapati, Quy-Toan Do, Marta Prato, Britta Glennon, Jonathan Dingel, Nels Lind, Stefano Carattini, Federico Mandelman, Hyunju Lee, Justin Sandefur, Greg Auclair, and seminar participants at the NBER Summer Institute, the NBER Conference on Immigrants in the US Economy, Stanford University Department of Economics, the CESifo Venice Summer Institute, the Vanderbilt University Department of Economics, University of Delaware Department of Economics, the Federal Reserve Bank of Richmond, the Bank of Canada Workshop on Macroeconomic Implications of Migration, the Federal Reserve Bank of Atlanta, the Congressional Budget Office, George Mason University, the Peterson Institute for International Economics, and the University of Pittsburgh. US Citizenship and Immigration Services and the US Department of Labor provided public data on the certification lottery. The firm survey was distributed by the National Association of Landscape Professionals, the Outdoor Amusement Business Association, the Seasonal Employment Alliance, and the American Seafood Jobs Alliance. We acknowledge support from Open Philanthropy and we thank Reva Resstack for research assistance. Any views expressed herein are those of the authors alone and do not represent any organization.

1 Introduction

The effect of immigration on U.S. workers remains controversial. It hinges crucially on the degree to which foreign workers complement or substitute for U.S. workers in production. The literature generally finds that immigration for high-skill work—requiring higher education and specialized knowledge—broadly complements native labor. 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 its effect 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 policy experiment in the United States: nationwide, firm-level, natural randomization of restrictions on the employment of immigrants in 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 allows unusually transparent, policy-relevant estimates of how low-skill immigration restrictions affect U.S. firms in the short run.

We first pre-registered the hypothesis tests, predicted treatment effects, and subgroup heterogeneity tests implied by a simple model of a monopolistically-competitive firm facing a potentially imperfectly-competitive input market for foreign and U.S. workers. We then collected survey data on 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 policy-relevant estimates of the firm-level "combined" elasticity of substitution (Hicks 1936) between H-2B and U.S. workers.[1]

We find that exogenous increases in H-2B worker employment for low-skill work cause the

[1] We follow the industry-standard definition of "U.S. workers" to comprise U.S. citizens or lawful permanent residents (green-card holders).

marginal firm to expand operations. Put differently, *restrictions* on H-2B employment cause the marginal firm to contract. Losing the lottery reduces firms' employment of low-skill immigrants by about half (−0.62 log points). At the marginal firm, this causes reduced revenue with elasticity 0.20–0.22, reduced investment with elasticity 1.5–2.1, and a reduced 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 H-2B workers causes reduced employment of low-skill U.S. workers, with elasticity 0.61 (statistically precise). We estimate that the firm-level, policy-relevant effective elasticity of substitution between foreign H-2B labor and U.S. labor in low-skill work is very low, in the range 0.8–2.0.

Overall, this evidence is consistent with substantial negative effects of low-skill immigration restrictions on economic activity inside and outside restricted firms—especially small firms and those in rural areas. These effects 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 several changes that were not prespecified, including sensitivity to influential observations, randomization inference, and forensic tests for unobserved black-market employment.

We contribute to the literature in three dimensions. First, we are not aware of prior work leveraging policy-relevant randomization to transparently identify the effects of restrictions on firms' demand for low-skill immigrant labor.[2] The standard approach in the literature is to construct 'shift-share' instrumental variables assuming that persistence in immigrant presence acts to exogenously allocate more recent immigration across regions or firms.[3] But unobserved factors

[2]Other studies have used naturally-randomized refugee placement to study exogenous variation in supply, but not variation in restrictions on demand (Glitz 2012; Couttenier et al. 2019; Olney and Pozzoli 2021). Some have exploited 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 naturally randomized restrictions on firms' demand for immigrant labor focus exclusively on high-skill workers (Clemens 2013; Doran et al. 2022; Dimmock et al. 2022; Brinatti et al. 2023). 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]Using regions: Card (1990); Altonji and Card (1991); Burchardi et al. (2018); Monras (2020); Piyapromdee (2020);

that shape economic outcomes can also persist, with potential for biased estimation (Jaeger et al. 2018; Adão et al. 2019; Goldsmith-Pinkham et al. 2020; Borusyak et al. 2021). Our use of natural randomization furthermore addresses the literature on how firms respond to shocks more generally (Baqaee and Farhi 2019; Bilbiie and Melitz 2021; Butters et al. 2022; Guerrieri et al. 2022; Kumar et al. 2022).

Second, we explore effects at the firm level. Within-firm responses are central to the overall economic effect of immigration (Card and Lewis 2007; Lewis and Peri 2015; Dustmann et al. 2015; Peri 2016). Recent study of firm-level effects focuses on *high-skill* immigration, especially its effects on innovation and entrepreneurship.[4] This work considers immigration for low-skill work, complementing ongoing research by Amuedo-Dorantes et al. (2024).[5] An advantage of firm-level research is that it does not require the strong assumption of ruling out specialization across firms within the aggregates—such as skill-cells or geographic areas—that are typically the unit of analysis (Card 2009, 2).[6]

Third, we contribute estimates based on exogenous changes in *policy.* The Local Average Treatment Effect (LATE) estimated from standard 'shift-share' instrumental variables, even when 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 'supply push' LATE of varying the supply of immigrants regardless of current demand for their labor—as the 'shift-share' approach is designed to do—need not equal the LATE from policy-induced restrictions on current, realized demand for their labor. A relatively small and recent literature focuses instead on exogenous changes in policy restrictions.[7]

Kim et al. (2022). Using 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).

[4] 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. (2023); Bahar et al. (2020); Khanna and Lee (2019); Glennon (2020); Glennon et al. (2021); Raux (2021); Azoulay et al. (2022).

[5] 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 with a fundamentally different identification strategy that leverages a sudden change in the availability of visas. We compare the two studies' results below.

[6] Results from more aggregate approaches can be 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 also 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).

[7] On high-skill immigrants: Beerli et al. (2021); on low-skill immigrants: 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).

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

We study 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. 98% of all H-2B jobs do not require a high school education; the mean months of experience required by employers is 1.2.[8] Haaland and Roth (2020) use political support for expanding the H-2B visa as a proxy measure of support for low-skill immigration in general. This visa has undergone relatively little study despite its importance. U.S. county-years where employers petition for more H-2B workers do not exhibit higher unemployment or lower wages on average for U.S. workers in related low-skill service occupations (Amuedo-Dorantes et al. 2021), but further investigation of that observed correlation is warranted.

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*".[9]

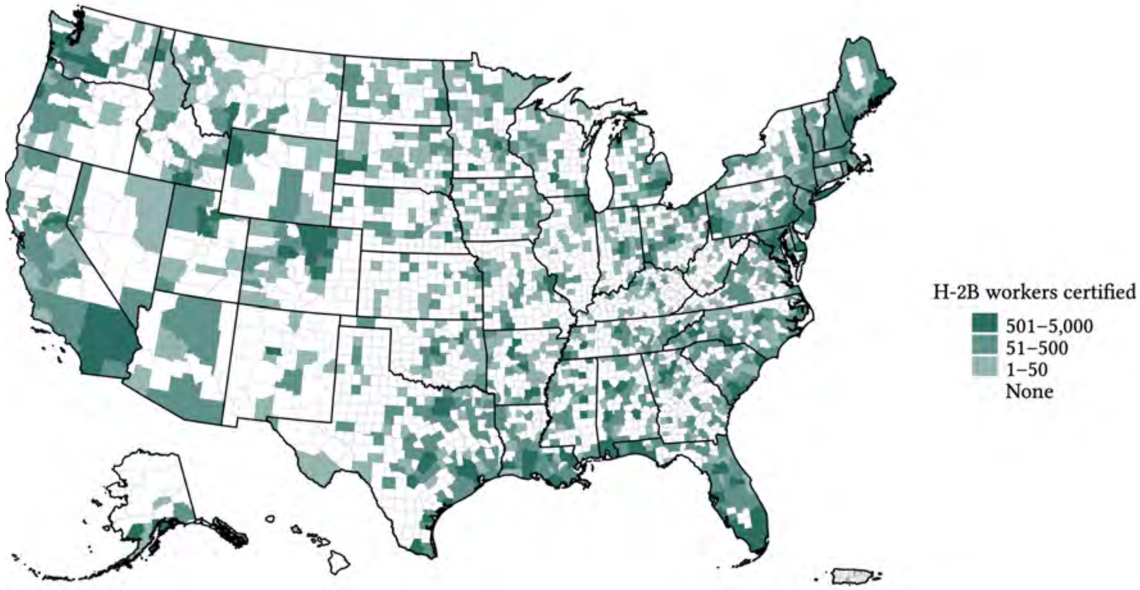
Foreign workers received an average of 84,383 H-2B visas per year during the five fiscal years ending in 2021. 88% are male.[10] The leading industries employing H-2B workers are Administrative and Support Services (especially groundskeeping/landscaping); Hospitality (including restaurants); Arts, Entertainment, and Recreation; Forestry, Fishing and Hunting; Construction;

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

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

[10]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 1: 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 (DOL 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. Boundary coordinates from US Census Bureau (2016).

and Food, Beverage, Textile, and Apparel Manufacturing (Barnes 2020, 12). These roughly correspond to the low-skill service industries with the highest prevalence of immigrant workers in the United States, led by landscaping (Cortés 2008, 387). The large majority of workers are citizens of Mexico (75% in FY2021); most of the rest are citizens of Jamaica, Guatemala, Ukraine, Honduras, Serbia, the Philippines, and El Salvador.

Employers can in principle employ any given H-2B worker indefinitely provided that they apply successfully to extend the visa once a year, and that the worker applies to renew the visa once every three years by departing the United States for three months. But in practice the Department of Labor is unlikely to approve long-term rehiring as satisfying a temporary need. H-2B jobs must offer full-time employment, defined as at least 35 hours per week and at least 75% of the workdays in each 12-week period. Workers' spouses and minor children can accompany 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 1).

To hire an H-2B worker, employers must successfully petition two federal government agencies, in order: the Department of 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'.[11] 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 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.[12]

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 was banned outright by the Foran Act,[13] 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.

[11] This roughly four-month administrative process is detailed by Bruno (2018), Barnes (2020) and Bier (2021).
[12] 8 CFR 214.2(h)(2)(ii).
[13] 23 Stat. 332, 48th Congress, Sess. II, Chap. 164.

3 Firm-level randomization of low-skill immigrant employment

Two 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. This cap was written into law by the Immigration Act of 1990, and remains in force (8 USC § 1184(g)(1)(B)). In 2005 it was split into a limit of 33,000 visas for the first half of each fiscal year (October–March) and 33,000 for the second half (April–September) (Bruno 2018). The cap was set without any quantitative empirical evidence of its effects on the U.S. labor market. By 2022 the statutory cap was oversubscribed by several hundred percent: For the 33,000 visas in the statutory quota for the second half of FY2022, employers petitioned DOL for 136,555 workers.[14]

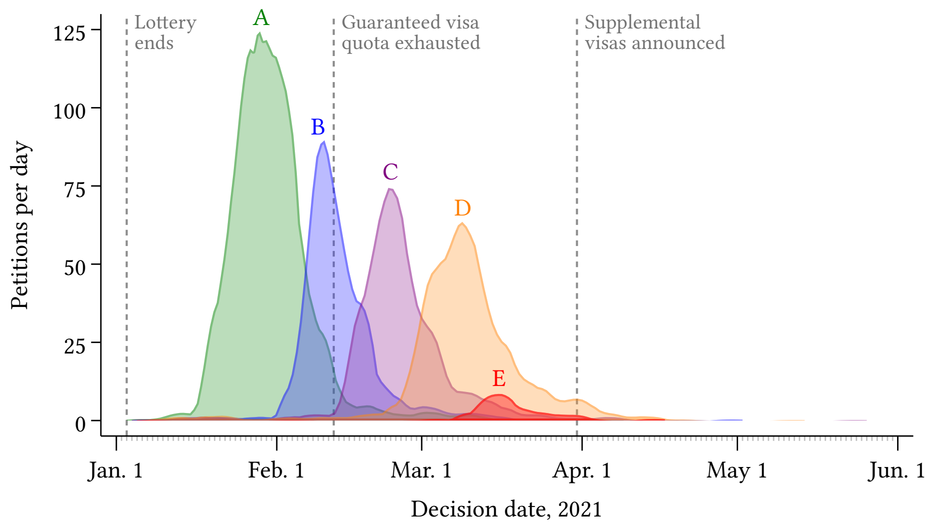
Second, a naturally randomized lottery constrains firms' access to H-2B visas under the statutory cap. Because DOL certification is required before firms can petition DHS for a visa, and because the demand for visas at DHS greatly exceeds the supply, firms' ability to obtain a visa at DHS is highly dependent 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.[15] 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 (but other staff may still be finishing some *A* petitions). It then proceeds to petitions with letter *C*, *D*, *E* and so forth in order (Figure 2). 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.

[14] *Federal Register* May 18, 2022, 87 *FR* 30334. 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).

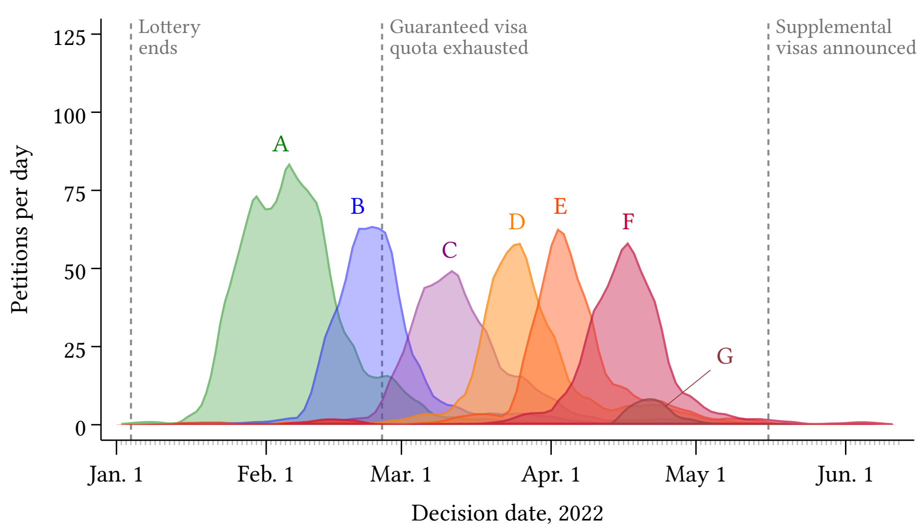
[15] *Federal Register*, March 4, 2019, 84 *FR* 7403.

Figure 2: H-2B 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.

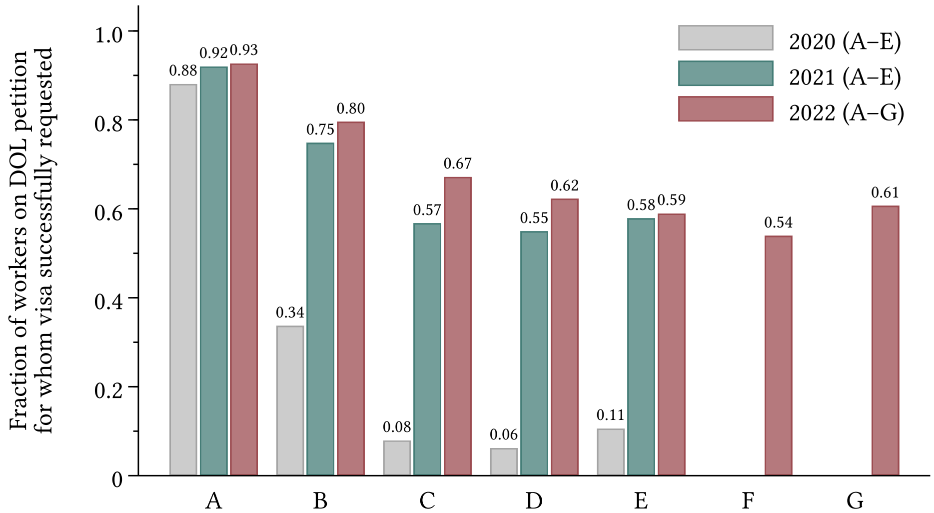
In this natural experiment, we consider 'treatment' as each U.S. firm's employment of low-skill immigrant workers on H-2B visas. Randomization into treatment at the firm level is continuous and fuzzy.

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.[16]

[16]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*,

Figure 3: 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 (USCIS 2023), in the average petition in each lottery-group and year (OFLC 2022), as a fraction of the number of workers for whom the each firm originally petitioned DOL (DOL 2022). 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 3). In the second half of fiscal year 2020, when no supplemental visas were approved by DHS, employers whose petition(s) received a 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

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).

‘never-takers’ (roughly 8%).

4 Pre-Analysis Plan, Firm Survey, and Instrument Specification

We conducted a novel survey to gather data on U.S. employers that entered the lotteries to employ low-skill foreign H-2B workers in mid-2021 and mid-2022, the second half of each fiscal year (April to September). Before data collection, we pre-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.[17]

4.1 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.[18] These associations claimed as members roughly 2,500 firms out of the 4,406 firms that entered the January 2021 lottery (57%). Respondents were asked roughly ten minutes of questions about firm performance and hiring in the second half (“summer season”) of each fiscal year. The pooled 2021 and 2022 core sample is 472 firms, that is, 251 from 2021 and 221 from 2022. Details of data collection and cleaning, and descriptive statistics, are in Appendix A3-A6.[19]

A first-order concern in a survey of this kind is bias from global nonresponse. The H-2B petitions reported by survey respondents constitute 6.3% of the universe of petitions in the lottery (834

[17]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.

[18]These associations are the National Association of Landscape Professionals, the Outdoor Amusement Business Association, the Seasonal Employment Alliance, and the American Seafood Jobs Alliance.

[19]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. 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. 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—Practical 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 September 2022.

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.

petitions out of 13,253, see Table 1). We test for nonresponse bias in three complementary ways.

First, the distribution of lottery results in the survey sample closely matches the distribution in the universe (Table 1). In 2021, the proportions of each lottery letter in the sample and universe are statistically indistinguishable, pairwise and collectively. In 2022, letter *C* petitions 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 more likely to respond (e.g. because they consider the program relevant) or less likely to respond (e.g. because lottery-losing firms use the survey to express frustration).[20]

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. First, in principle, firms with certain baseline traits (e.g. relatively large firms) could be differentially affected by the lottery result, which could make them more or less likely to respond to the survey. Second, and separately, 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 significant spurious correlation (Appendix Table A6).

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 common proxy test for nonresponse bias (e.g. Behaghel et al. 2015; Heffetz and Reeves 2019) rests on the assumption that some of the same latent firm traits that cause nonresponse are likely to cause delayed response. The results, in Appendix Figure A6, exhibit no significant heterogeneity by 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 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 broadly representative of the sampling universe, incompatible with severe heterogeneity in sampling by industry (Appendix Table A2).

[20] 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.

The survey has two 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—including any effects of firm survival (since only surviving firms are surveyed).[21]

4.2 Defining the instrumental variable

Natural randomization creates the opportunity to define an unusually valid instrumental variable for firms' employment of H-2B workers. We use two different pre-registered specifications.

Lottery win instrument: The first specification of the instrument is dichotomous and intuitive: Based on Figure 3, 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 4). 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

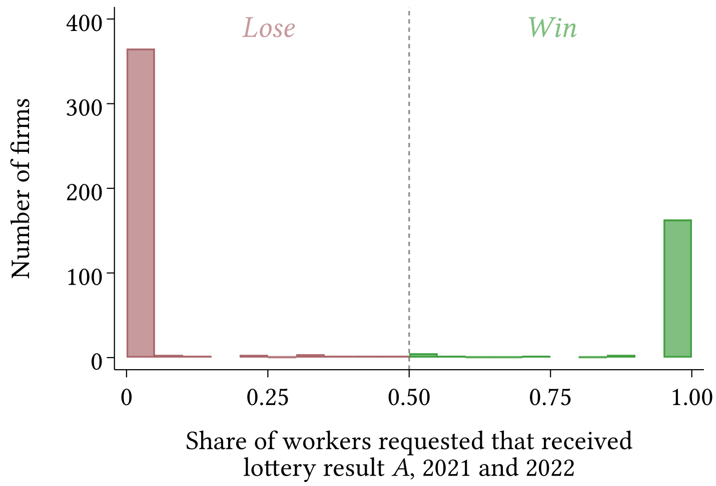
$$z_i \equiv \begin{cases} 1 & \text{if } s_i^A > 0.5 \\ 0 & \text{otherwise.} \end{cases} \tag{1}$$

This pre-specified specification has the advantage of simplicity and ease of communication: Firms either 'win' or 'lose', and all firms receive the same share of the workers they petitioned for, in expectation. It has two disadvantages. First, the pre-specified 0.5 threshold is arbitrary, and in principle the results could be sensitive to that choice. Second, in principle and for the very small number of firms in the middle of Figure 4, the firm-level probability of 'winning' is somewhat lower for firms that file multiple petitions.[22]

[21] 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.

[22] For example, if the petition-level win probability is $p = 0.35$, at the firm level Pr(win|1 petition) = 0.35 but Pr(win|3 petitions) = $\binom{3}{2}(0.35)^2(1-0.35) + (0.35)^3 = 0.28$. We thank Chris Walters for this clear illustration. In practice this concern would only affect the results for an extremely limited share of firms (Figure 4). The firm trait most clearly associated with filing multiple petitions is firm size, and all the results in the paper rest on regression specifications that control for predetermined firm size. Note also that firms would not be expected to strategically attempt to circumvent Dept. of Labor rules in order to split groups of workers into multiple petitions, because by the definition

Figure 4: 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.

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 3. For example, the rate of success for a worker on an A petition in the January 2021 lottery was $\rho^A = 0.92$, 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

$$z_i' \equiv \sum_{\ell \in \mathbb{L}} \rho^\ell s_i^\ell. \tag{2}$$

This 'expected share' instrument—also pre-specified—avoids the bias that in principle could arise from the 'lottery win' instrument for a small number of firms, as noted above. It has the disadvantage of less simplicity and less intuitive interpretation.

of 'winning' in equation (1) that would reduce the probability of winning.

5 Results

The above instruments allow us to estimate the reduced-form effect of the lottery result and the two-stage-least-squares effect of H-2B worker employment, for the 'complier' firms whose H-2B hiring was altered by the lottery outcome.[23] 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 restricting H-2B hires below the profit-maximizing level. In most of the analysis we use the simple linear regression specification of

$$y_{i,t} = \zeta + \theta I_{i,t} + \mathbf{X}'_{\mathbf{i,t-1}}\mathbf{\Phi} + \delta \mathbb{1}_t + \varepsilon_{i,t}, \tag{3}$$

where $y_{i,t}$ is the outcome for firm i in the current period t; $I_{i,t}$ is H-2B 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 $\mathbf{\Phi}$ 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 randomization-based $Z_{i,t}$, either the 'lottery win' or the 'expected share' instrument described above. We also consider the reduced-form specification

$$y_{i,t} = \zeta + \mu Z_{i,t} + \mathbf{X}'_{\mathbf{i,t-1}}\mathbf{\Phi} + \delta \mathbb{1}_t + \varepsilon_{i,t}. \tag{4}$$

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).[24] 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 6).

As is apparent from Figure 3, the lottery outcome strongly determines H-2B hiring. Winning the lottery causes firms to employ 86 percent more H-2B workers, corresponding to a first-stage semielasticity $\hat{\mu} = 0.618$ (first-stage regression in Appendix Table A4).

[23] Data and code at Clemens and Lewis (2025).

[24] 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).

5.1 Effect on primary outcomes: Revenue and U.S. hiring

Table 2 presents tests of the firm-level effects of the lottery result (Intent-to-Treat) and from H-2B employment (Treatment-on-Treated) in 2021 and 2022, with the regressions in equations (3) and (4). The outcomes are those pre-registered as "primary" outcomes: firm revenue and employment of low-skill U.S. workers. In this and other tables the firm sample is held constant across columns, to include only those firms that reported full baseline data.

The first column of Table 2 shows estimates from a simple OLS regression of revenue on H-2B workers. Revenue and H-2B employment are correlated across firms with elasticity 0.115, controlling for baseline traits. This simple correlation might understate the causal effect if, for example, firms with less revenue relative to baseline are more likely to employ H-2B workers.

Columns 2–5 consider the causal effect of H-2B worker employment on revenue. Column 2 of Table 2 presents a reduced-form regression of revenue on the 'lottery win' instrument (1). Winning the lottery causes firm revenue to grow by 14.4% (corresponding to a semielasticity of 0.135). Column 3 shows the second stage of a 2SLS regression of revenue on H-2B employment, with H-2B employment instrumented by the 'lottery win' instrument. The causal effect of H-2B employment on firm revenue is to raise it with elasticity 0.218. We report the p-value of the Anderson-Rubin χ^2 test, as recommended by Moreira (2009), which is robust to weak instrumentation (Andrews et al. 2019) that might invalidate traditional t-ratio inference (Lee et al. 2022).[25] This is reported in italics below each estimate of the coefficient of interest. The p-value of this test of a null effect of H-2B employment in the 2SLS 'lottery win' regression is 0.008. Columns 4 and 5 repeat the reduced-form and 2SLS analysis with the alternative 'expected share' instrument, with similar results: the causal elasticity of revenue to an increase in H-2B employment is 0.20 in column 5.

The remaining five columns of Table 2 change the dependent variable (outcome) to firms' employment of low-skill U.S. workers, but are otherwise identical to the first five columns. In column 6, U.S. temporary worker employment and H-2B temporary worker employment are correlated across firms with elasticity 0.225, conditional on baseline traits. The causal semielas-

[25] All 2SLS regressions we report are just-identified, reducing concerns about statistical inference distorted by weak instruments (Angrist and Kolesár 2024). We thank Baum et al. (2010) for community-contributed code.

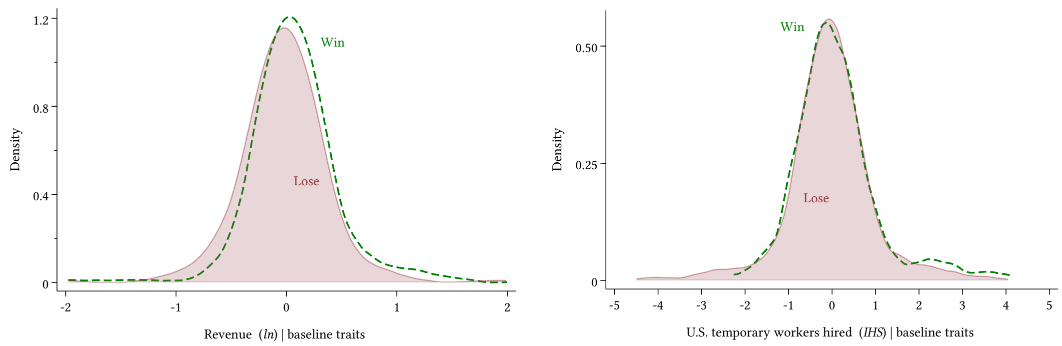
Table 2: Effect of H-2B workers on primary outcomes: Revenue and U.S. hiring

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. var:	*Revenue (ln)*					*U.S. temporary workers (IHS)*				
Estimator:	OLS		2SLS	OLS	2SLS	OLS		2SLS	OLS	2SLS
Instrument:			*Win*		*Share*			*Win*		*Share*
H-2B employed (IHS)	0.115		0.218		0.198	0.225		0.188		0.061
	(0.022)		(0.080)		(0.069)	(0.047)		(0.149)		(0.125)
Anderson-Rubin p-*val.*	—		*0.008*		*0.004*	—		*0.219*		*0.630*
Lottery win		0.135					0.116			
		(0.051)					(0.095)			
Expected share				0.443					0.136	
				(0.154)					(0.285)	
Outcome at baseline	0.817	0.856	0.780	0.858	0.787	0.810	0.804	0.809	0.803	0.805
	(0.057)	(0.056)	(0.065)	(0.056)	(0.062)	(0.028)	(0.029)	(0.028)	(0.029)	(0.029)
Full baseline controls	Yes	Yes	Yes	Yes	Yes	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 *Win* ('Lottery win') instrumental variable is an indicator variable for winning the lottery, that is, receiving 'A' on petitions totaling more than 50% of workers requested. The continuous *Share* ('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. 'Outcome at baseline' is predetermined value—in the year before the lottery—of the dependent variable (ln revenue in cols. 1–5, IHS of U.S. temporary workers hired in cols. 6–10). 'Full baseline controls' furthermore include the predetermined values of the prior year's number of U.S. year-round workers, number of U.S. temporary workers, and number of H-2B workers.

Figure 5: Reduced-form effects of the lottery, primary outcomes

(a) *Revenue* (b) *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 0.5 ln points (*a*) or 0.15 inverse hyperbolic sine (IHS) points (*b*). Residuals are estimated controlling for the full set of baseline traits, corresponding to columns 2 and 7 in Table 2, measured in the year prior to the lottery.

ticity of U.S. temporary employment to winning the lottery is 0.116 in column 7, an estimate that is not statistically significant at conventional levels (p = 0.22). In column 8, the causal elasticity of U.S. temporary employment to H-2B temporary employment is 0.188, though again with a p-value of 0.22, the test cannot reject the null hypothesis that this effect is zero. The last two columns yield point estimates that are positive but smaller using the 'expected share' instrument, also statistically indistinguishable from zero for the average firm.

Figure 5 illustrates the reduced-form effects of the 'lottery win' instrument on revenue and U.S. temporary employment, from Table 2 columns 2 and 7. The kernel density plots show the residual after regressing each outcome on the predetermined baseline traits, for lottery-winning versus lottery-losing firms. Figure 5a shows the reduced-form effect of winning the lottery across the full distribution of firm-level revenue conditional on baseline traits including baseline revenue. Figure 5b shows the reduced-form effect of the lottery result across the full distribution of U.S. temporary employment. The figures suggest that the broad conclusions from Table 2 are not driven by a small number of influential observations.

5.2 Effect on secondary outcomes: investment and profit

We now consider firm outcomes labeled as secondary in the pre-analysis plan. Table 3, columns 1–5, presents tests of the effect of low-skill foreign H-2B 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 ($)?*' Columns 6–10 present tests of the effect on the annual growth of firms' profits for the current year. But for these outcome variables, the regressions in Table 3 are identical to those in Table 2.[26]

H-2B employment causes firms to substantially expand investment. In column 2 of Table 3, winning the lottery causes investment to rise by a factor of 3.8 (semielasticity 1.325). In columns 3 and 5, H-2B employment causes greater investment with an elasticity of 2.07 or 1.47, depending on the instrument specification. This evidence is consistent with a large, positive, short-run effect of the ability to hire low-skill H-2B workers on firms' purchases of equipment, vehicles, structures, and land—implying substantial ripple effects outside the firms surveyed.

The rest of Table 3 addresses the effect on profits. Measuring effects on profits is complicated by the fact that firms in general are known to be reluctant to respond to direct questions about profits. Thus we seek 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

$$\%\Delta C \equiv \frac{R_1(1-\pi_1)}{R_0(1-\pi_0)} - 1. \tag{5}$$

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),

[26] Our survey did not collect information on the previous year's investment or profits, so the regressions in Table 3 do not control for baseline values of the *outcome*, as they did in Table 2. But the controls are identical to those in Table 2: the predetermined baseline values of: revenue, year-round U.S. workers, temporary U.S. workers, and H-2B workers.

[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]?*"

Table 3: Effect of H-2B workers on secondary outcomes: Investment and profit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. var:	*Investment (IHS)*					*Change in profit rate, year-on-year*				
Estimator:	OLS		2SLS	OLS	2SLS	OLS		2SLS	OLS	2SLS
Instrument:			*Win*		*Share*			*Win*		*Share*
H-2B employed (IHS)	0.403		2.072		1.466	0.090		0.152		0.151
	(0.197)		(0.721)		(0.610)	(0.021)		(0.075)		(0.067)
Anderson-Rubin p-*val.*	—		*0.001*		*0.012*	—		*0.047*		*0.024*
Lottery win		1.325					0.094			
		(0.415)					(0.048)			
Expected share				3.390					0.327	
				(1.366)					(0.146)	
Revenue, baseline (ln)	0.611	0.740	0.045	0.751	0.250	−0.195	−0.166	−0.215	−0.166	−0.214
	(0.266)	(0.258)	(0.367)	(0.261)	(0.338)	(0.061)	(0.058)	(0.066)	(0.059)	(0.064)
Full baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	456	456	456	456	456	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 (omitted year 2021). Robust standard errors in parentheses. The dichotomous *Win* ('Lottery win') instrumental variable is an indicator variable for winning the lottery, that is, receiving 'A' on petitions totaling more than 50% of workers requested. The continuous *Share* ('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' include the predetermined values of the prior year's number of U.S. year-round workers, number of U.S. temporary workers, and number of H-2B workers.

the year-on-year percentage change in profits can be estimated using only information reported on the survey (R_0, R_1, and $\%\Delta C$):

$$\ln\frac{\pi_1}{\pi_0} \approx \ln\frac{R_1}{R_0} - \ln\left(1 + \%\Delta C\right). \tag{6}$$

This year-on-year change in the *rate* of profit is the outcome variable in columns 6–10 of Table 3. H-2B employment causes faster growth in the profit rate, with an elasticity of 0.15 (cols. 8, 10), implying that a doubling of H-2B-worker employment causes dollar-value profits to rise by 40%.[28]

6 Robustness

We tested the robustness of the results to a series of changes, some prespecified and some not.

6.1 Prespecified robustness checks

Beyond the tests for global nonresponse described above, the preanalysis plan specified that we would test for heterogeneity of the core results by 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 2–3 are statistically indistinguishable in subsamples of firms that did or did not provide a postal code (Appendix Table A7).

The core results are furthermore robust to the prespecified test of adjusting statistical inference for multiple hypothesis testing by (asymptotically) controlling for the familywise error rate by the method of List et al. (2019, Thm. 3.1). This method is suitable for the dichotomous 'lottery win' treatment. Inference is not substantially affected.[29]

[28] The 15.2% increase in the rate of profit as a fraction of revenue (Table 3, col. 8) is augmented by the 21.8% increase in revenue (Table 2, col. 3). Dollar-value profits for lottery-losing firms are $R_\ell \cdot \pi_\ell$, which would rise by a factor of $\frac{1.218R_\ell \cdot 1.152\pi_\ell}{R_\ell \pi_\ell} = 1.403$.

[29] In columns 3 and 8 of Table 2 and column 3 of Table 3 the respective p-values exhibit a minor shift to 0.023, 0.230, and 0.002.

6.2 Alternative specifications that were not prespecified

The core results in Section 5 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 U.S. 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 4 reestimates the reduced-form regressions in columns 7 and 9 of Table 2 using the Poisson Pseudo-Maximum-Likelihood (PPML) estimator due to Silva and Tenreyro (2006), 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 U.S. workers. Using the 'Lottery win' instrument in column 1, the causal semielasticity of winning on U.S. 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 2, column 7. Using the 'Expected share' instrument in Table 4 column 2, the coefficient estimate is 1.157 with a p-value of 0.043, compared to the corresponding estimate of 0.136 with a p-value of 0.633 from Table 2, column 9. The two coefficient estimates in Table 4 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]

The results are furthermore robust to several non-pre-specified tests reported in Appendix Tables A13–A14. They are substantively invariant to using randomization inference, as recommended by Young (2018). The results are qualitatively similar using quantile (p50) regressions, both standard and IV, which are highly robust to influential 'outlier' observations. And the results do not arise from a single, dominant industry: the most common industry for H-2B workers is groundskeeping/landscaping, and the qualitative conclusions of the core analysis are robust to truncating firms of that industry (Figure A7).

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

Table 4: Effect of H-2B 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 U.S. 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 H-2B 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 more than 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.

7 A simple model to interpret the observed effects

We now impose a light theoretical structure on the firm-level effects observed above. This model allowed prespecification and interpretation of tests for heterogeneous effects according to firm traits, and will allow point estimates of the elasticity of substitution between H-2B foreign workers and U.S. workers.

The model predicts positive causal effects of low-skill immigrant employment on firm revenue. It predicts crowding out of low-skill U.S. workers by immigrants when the immigrant-U.S. elasticity of substitution in production is sufficiently high relative to the price elasticity of output demand—and crowding in of low-skill U.S. workers otherwise. It likewise predicts positive effects on investment and absolute profits, but not necessarily the rate of profit.[31]

[31]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.

Consider a firm in monopolistic competition that maximizes profits as it produces output by combining low-skill immigrant labor (I), low-skill U.S. 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. 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

$$Q(p) = Dp^{-\eta}, \tag{7}$$

where Q is output, p is price, $\eta > 1$ is the demand elasticity and D is a constant.[32]

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.

7.1 Effects on revenue

The first is that relaxing the hiring constraint on low-skill immigrants must increase the scale of the firm. Intuitively, relaxing a constraint must weakly increase the firm's dollar-value profits; Appendix A1 offers a proof. Output and revenue must also rise due to homogeneity.[33] That is,

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

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.

We can then derive expressions for the adjustments of other inputs that allow us to solve for the change in revenue $\left(\ln \frac{R_w}{R_\ell}\right)$ caused by the change in immigrant labor $\left(\ln \frac{I_w}{I_\ell}\right)$. We use a standard

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

[33] 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.

nested Constant Elasticity of Substitution (CES) form (e.g. Ottaviano and Peri 2012). Let

$$Q = zH^{\gamma}K^{\beta}\left(\alpha I^{\frac{\sigma-1}{\sigma}} + (1-\alpha)N^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}(1-\beta-\gamma)}, \tag{8}$$

where $\sigma \geq 0$ is the elasticity of substitution between immigrant and U.S. low-skill labor, H is high-skill U.S. labor, α, β, and γ are share parameters, and z a productivity shifter. H is treated as fixed within season, consistent with its empirical response in our study (Appendix Table A8).

7.2 Effects on investment

Under equation (8) capital's share of revenue is fixed at $\beta\frac{\eta-1}{\eta}$, so it responds (in proportional terms) exactly as revenues do to winning the lottery:

> **Lemma 1.** *Under equation (8), 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, as capital occupies a fixed share of revenue, which rises with immigrant hires under Proposition 1 (proved in Appendix A1). Capital's fixed share implies revenue and capital grow *pari passu*. Thus we can approximate[34]

$$\ln\frac{R_w}{R_\ell} \approx \frac{s_I}{1-s_K}\ln\frac{I_w}{I_\ell} + \frac{s_N}{1-s_K}\ln\frac{N_w}{N_\ell}, \tag{9}$$

where s_j is the revenue share of factor j. In the special case of little change in U.S. 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 (8) in which capital instead substitutes for low-skill labor. This 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). If this alternative specification applies here as well, capital stocks could instead *fall* in response to additional immigrant employment. The true effect is an empirical question.

[34] Start with the Euler equation $\ln\frac{R_w}{R_\ell} \approx s_I\ln\frac{I_w}{I_\ell} + s_N\ln\frac{N_w}{N_\ell} + s_K\ln\frac{K_w}{K_\ell}$. Substitute $\ln\frac{K_w}{K_\ell} = \ln\frac{R_w}{R_\ell}$ and rearrange.

7.3 Effects on U.S. worker employment

The response of U.S. worker employment is not *a priori* obvious either. A conventional story is that firms will prefer to hire "cheap" immigrant labor and displace U.S. workers. However, this story ignores the scale response in Proposition 1. Depending on how substitutable immigrants are for U.S. workers, relative to this scale response, restrictions on employing immigrants may either raise or lower U.S. employment (Friedberg and Hunt 1995).[35] This gives:

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

To see this, under (8) with fixed H, the U.S. employment response to greater immigrant employment is given by

$$\ln \frac{N_w}{N_\ell} \approx s_I \cdot \frac{(\eta - 1)(1 - \beta - \gamma\sigma) - (\sigma - 1)}{\Theta} \cdot \ln \frac{I_w}{I_\ell} \tag{10}$$

where $\Theta > 0$.[36] This implies a necessary and sufficient condition for immigrant employment to crowd in U.S. employment:

$$\frac{\ln(N_w/N_\ell)}{\ln(I_w/I_\ell)} > 0 \iff \frac{\eta - 1}{\sigma - 1} > \frac{1}{1 - \beta - \gamma\sigma}. \tag{11}$$

We then have:

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

We can now derive the revenue effect of immigrant employment, with capital adjustment. Substituting (10) into (9) gives

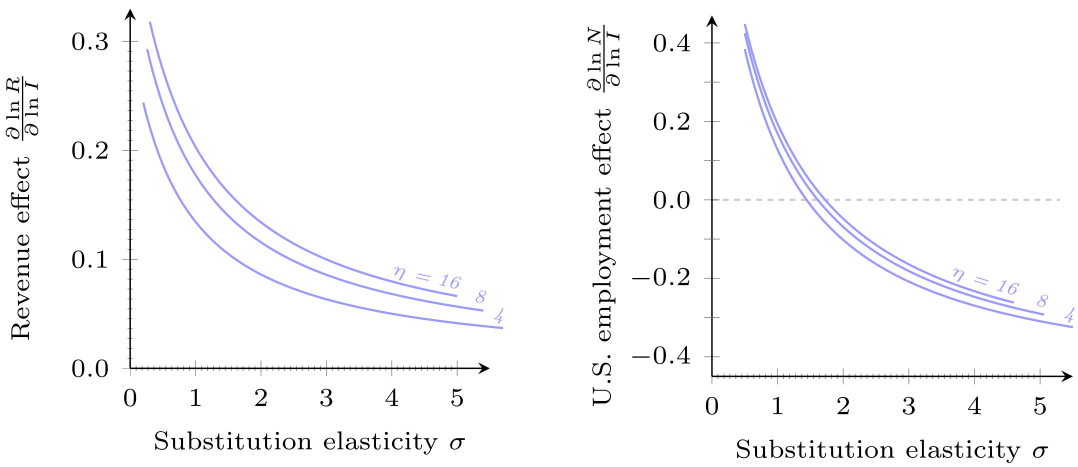
$$\ln \frac{R_w}{R_\ell} \approx \left[\frac{s_I}{1 - s_K} + \frac{s_N}{1 - s_K} s_I \left(\frac{(\eta - 1)(1 - \beta - \gamma\sigma) - (\sigma - 1)}{\Theta} \right) \right] \cdot \ln \frac{I_w}{I_\ell}. \tag{12}$$

[35] Immigrant wages are fixed by regulation in the empirical setting we study here: H-2B workers' wages are set by the federal government.

[36] That the ungainly quantity $\Theta \equiv \big((1 - \beta - \gamma)[(\eta - 1)(1 - s_N) + (\sigma - 1)s_N] + (\beta + \gamma)s_N\eta(\sigma - 1)\big) \times (1 - s_K) - (\eta - 1)(1 - \beta - \gamma)\sigma s_K s_N > 0$ is shown in Appendix Section A1.2.

Figure 6: 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 U.S. share of inner labor nest 0.668. Details in Appendix.

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 U.S. employment, positive or negative according to condition (11).

A graph of the revenue effect (12) is presented in Figure 6a, and the U.S. employment effect (10) in Figure 6b, using parameter values from the empirical analysis above. These confirm the key results above: The revenue effect of immigrant employment is nonnegative (Figure 6a). Both revenue and U.S. employment responses are falling in the substitution elasticity and rising in the output demand elasticity (Figures 6a and 6b). And there is a cutoff value of the substitution elasticity, relative to the output demand elasticity: Immigrants displace U.S. workers above that cutoff, and crowd in U.S. workers below it (equation (11), Figure 6b).

7.4 Heterogeneous effects under imperfect competition

We used this theoretical framework to motivate pre-specified tests for heterogeneous effects of H-2B workers according to firm traits. 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 above. It predicts that firms with less output market power—and thus a high price elasticity of output demand η—will exhibit larger effects of H-2B employment on revenue (Proposition 1), on U.S. employment (Lemma 2), and on investment (Lemma 1). Intuitively, monopolistically competitive firms employing added labor and facing relatively high output price elasticity will expand production relatively more, because by doing so they will drive down output prices relatively less. We expect to observe this in firms that are small relative to their market.

Imperfect competition in factor markets is not built into the simple model above, so we extend it here. The pre-analysis plan predicted a less negative or more positive effect of immigrant employment on U.S. employment in smaller labor markets, such as rural areas. To see why, suppose that U.S. 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 U.S. worker heterogeneity in their preferences over firms (Card et al. 2018; Manning 2021). U.S. workers' 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 U.S. employment in equation (10) becomes

$$\ln \frac{N_w}{N_\ell} \approx s_I \cdot \frac{\eta - \sigma}{(\eta - 1)(1 - s_N) + (\sigma - 1)s_N + \frac{\sigma}{e_N}(\eta - 1)} \cdot \ln \frac{I_w}{I_\ell}, \tag{13}$$

derived in Appendix A1.3. That is, the U.S. employment response to immigration is increasing in the U.S. labor supply elasticity, converging to equation (10) as $e_N \to \infty$.

The pre-analysis plan's prediction of larger treatment effects (13) 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 highlighted 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 U.S. 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 U.S. workers are paid closer to their marginal product, would have more difficulty recruiting U.S. workers away from their superior alternatives. 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. Appendix A2 presents a minimal formal spatial duopsony model of this intuitive distinction.

8 Heterogeneous effects and elasticity of substitution

We use the model above in two ways. The first is to conduct pre-specified tests of the heterogeneous effects predicted by the model. The second is to make point estimates of the elasticity of substitution between H-2B foreign workers and U.S. workers implied by our earlier estimates of the Policy-Relevant Treatment Effect (PRTE, Heckman and Vytlacil 2001).

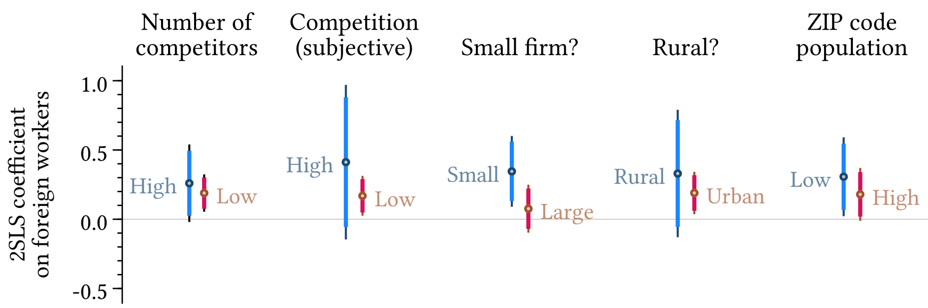
8.1 Heterogeneity in the treatment effect: Pre-specified tests

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, and more positive treatment effects on U.S. employment in rural areas than urban areas.

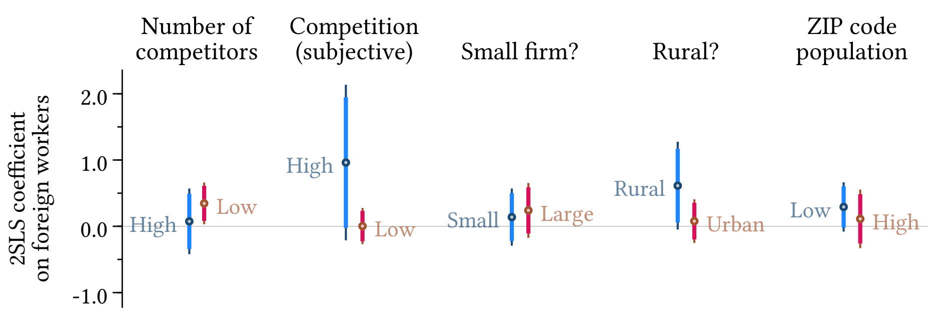
Figure 7 graphically presents tests of these predictions, in the first three columns of panels (a) and

Figure 7: Heterogeneous effects of H-2B workers employed, primary outcomes

(a) *Effect on revenue*



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



The unit of analysis is firms. Pooled 2021 and 2022 data. The vertical axis in each pane shows the 2SLS coefficient on H-2B workers employed (IHS) in a regression with full baseline controls, corresponding to the specification in columns 3 and 8 of Table 2. 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 U.S. Dept. of Agriculture as anything other than "Metropolitan Area, Core" (RUCA code 1; ERS 2020). "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 (NBER 2017). Full regression results in Appendix Tables A11–A12.

(b). The vertical axis in each panel shows the effect of H-2B employment on each outcome: the 2SLS regression coefficient on H-2B temporary employment using the 'lottery win' instrument, corresponding to columns 3 and 8 of 2. (Full regression results are in Appendix Tables A11–A12.) Though the differences are usually not statistically significant, they tend to be in the predicted direction.

First, firms that face greater objective or subjective competition, and firms that are smaller, exhibit larger effects of H-2B employment on revenue. This suggests that firms with greater output market power are less affected by exogenous changes in low-skill H-2B employment, as predicted (Figure 7a, cols. 1–3). In these tests, a firm is considered to face a 'high' *number of competitors* if it reports more than the median number, and 'low' otherwise. A firm is considered to face 'high' *subjective competition* if it reports that it would be 'very easy' (4 on a 4-point scale of ease) for competitors to steal its customers by underpricing. *Firm size* is considered 'small' if it had less than median revenue at baseline. In the tests for predicted heterogeneity of the effect on employment by competitive environment, the results are more mixed (Figure 7b, cols. 1–3).[37]

Second, the tests uniformly support the theoretical prediction of heterogeneous treatment effects on U.S. employment by rural/urban location, in the last two columns of Figure 7a and Figure 7b. 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 $p = 0.053$.[38]

These tests furthermore suggest high robustness of the core findings in Table 2. The sign of the effect measured in the core results, for example, does not significantly diverge from that of the core results in any of the 20 prespecified subsamples in Figure 7.

[37] 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.

[38] We consider *firm location* to be 'rural' if its postal code is classified by the U.S. Dept. of Agriculture 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.

8.2 Estimates of the elasticity of substitution

The regression results in Section 5 can, under the model's assumptions, yield estimates of the elasticity of substitution between low-skill foreign H-2B workers and U.S. workers. Specifically, we use the model to estimate—*within* firms—what Knoblach and Stöckl (2020) call the "effective" elasticity of substitution. That is, we include not only the purely technical substitution within a firm's current production technology, but also how it is shaped by imperfect input and output markets faced by the firm (as in e.g. Freeman and Medoff 1982; Amior and Manning 2020). Hicks himself recommended this, for estimating policy-relevant parameters: Our estimates include the influence of the institutional setting in which a marginal change in migration restrictions would occur, including market imperfections. "Concentration upon technical substitution alone would certainly be misleading," wrote Hicks (1936, 10), for the purpose of "interpreting facts."[39] That is, like many estimates in the literature, the estimates below do not decompose limits on substitution into those determined strictly by the production technology and those determined by limits on U.S. worker labor supply.

Estimating the elasticity of substitution requires two elements. First, we need the model to translate the regression results into an estimate of σ. We derive in Appendix A1.4 a more general form of equation (10) that allows for nonzero income shares of capital and year-round labor ($\beta, \gamma > 0$):

$$\frac{d\ln(N_w/N_\ell)}{d\ln(I_w/I_\ell)} \approx s_I \frac{e_N}{\sigma + e_N}\left(\frac{(\eta-1)\,(1-\beta-\gamma\sigma)-(\sigma-1)}{\Phi(\sigma, e_N)}\right), \tag{14}$$

where the denominator Φ is a complex expression.[40] In (14), as before, lower values of the separation elasticity e_N correspond to greater degrees of monopsony power by employers hiring U.S. workers for low-skill work. We then estimate the elasticity of substitution by interpreting the coefficient estimates in Table 2, column 10 as estimates of $\frac{d\ln(N_w/N_\ell)}{d\ln(I_w/I_\ell)}$, then solving for σ.[41]

Second, we need estimates of the other parameters in equation (14): the price elasticity of output

[39] Our estimate *omits* the influence of substitution processes that occur *between* firms—such as Rybczynski effects, known as the "community level" elasticity (Hicks 1936, 8) or "aggregate" elasticity (Knoblach and Stöckl 2020).

[40] $\Phi \equiv \big((\eta-1)(1-\beta-\gamma) - \frac{e_N}{\sigma+e_N} s_N\left[\sigma(\eta-1)(1-\beta-\gamma)-\eta(\sigma-1)\right]\big)(1-s_K) - \frac{e_N}{\sigma+e_N}(\eta-1)(1-\beta-\gamma)\sigma s_K s_N$. Note that as long as $\Phi > 0$, the condition for a positive response remains in the more restrictive equation (10), that is, $(\eta-1) > \frac{\sigma-1}{1-\beta-\gamma\sigma}$.

[41] To be conservative we choose the estimate arising from the 'expected share' instrument, which is less positive than the other.

Table 5: Estimates of the effective elasticity of substitution σ

(a) *No monopsony power* ($e_N \to \infty$)

	$\eta = 4$			$\eta = 8$			$\eta = 16$		
$\gamma =$	0.25	0.35	0.45	0.25	0.35	0.45	0.25	0.35	0.45
$\beta = 0.25$	1.43	1.22	1.07	1.75	1.40	1.16	1.99	1.51	1.22
	(0.50)	(0.43)	(0.37)	(0.61)	(0.49)	(0.40)	(0.69)	(0.53)	(0.42)
$\beta = 0.35$	1.30	1.11	0.97	1.56	1.24	1.03	1.74	1.33	1.07
	(0.45)	(0.39)	(0.34)	(0.54)	(0.43)	(0.36)	(0.61)	(0.46)	(0.37)
$\beta = 0.45$	1.17	1.00	0.87	1.36	1.08	0.90	1.50	1.14	0.92
	(0.41)	(0.35)	(0.30)	(0.47)	(0.38)	(0.31)	(0.52)	(0.40)	(0.32)

(b) *Substantial monopsony power* ($e_N \equiv 3$)

	$\eta = 4$			$\eta = 8$			$\eta = 16$		
$\gamma =$	0.25	0.35	0.45	0.25	0.35	0.45	0.25	0.35	0.45
$\beta = 0.25$	1.25	1.09	0.96	1.49	1.23	1.04	1.66	1.31	1.09
	(0.62)	(0.52)	(0.44)	(0.78)	(0.60)	(0.49)	(0.90)	(0.66)	(0.52)
$\beta = 0.35$	1.15	1.00	0.88	1.35	1.10	0.93	1.49	1.17	0.97
	(0.55)	(0.46)	(0.40)	(0.68)	(0.53)	(0.43)	(0.77)	(0.57)	(0.45)
$\beta = 0.45$	1.05	0.91	0.80	1.20	0.98	0.83	1.31	1.02	0.84
	(0.49)	(0.41)	(0.35)	(0.58)	(0.45)	(0.37)	(0.65)	(0.48)	(0.38)

Delta-method standard errors in parentheses. Derived from solving equation (14) for σ. Uses U.S. share of the inner labor nest 0.668, estimated from the U.S. employment regression in Table 2, col. 10, using the core firm sample $N = 472$. Details in Appendix A1.4.

demand η, the capital elasticity of output β, the high-skill labor elasticity of output γ, and the U.S. share of the low-skill labor nest $1 - \alpha$.

- *Output demand elasticity*: The literature suggests an output price elasticity around $\eta \approx 8$ 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.[42]

[42]Details in Appendix A8. 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 of 1.12 for "accommodation and food services", 1.12 for "wholesale trade", and 1.16 for "construction", imply demand elasticity $\eta = 7.3$–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. The exception among typical H-2B employers is Amusement

- *Capital elasticity of output*: We approximate $\beta \approx 0.35$. The capital share of revenue in the industries employing the large majority of H-2B workers is 0.292–0.310 (landscaping/groundskeeping and hospitality), corresponding to $\beta = 0.3 \cdot \frac{\eta}{\eta-1} \approx 0.35$ under $\eta = 8$.[43]

- *Labor shares*: Finally, we approximate $\gamma \approx 0.35$ and the U.S. share of low-skill labor $1-\alpha \approx 0.69$.[44]

The implied estimates of the elasticity of substitution σ are presented in Table 5. Table 5a presents estimates in the absence of employer monopsony power over U.S. workers in low-skill jobs, with the separation elasticity e_N tending to infinity. Our preferred estimate uses the parameter estimates in the middle of the plausible ranges above for the price elasticity of output demand ($\eta \approx 8$), the elasticity of output to capital ($\beta \approx 0.35$), and the income share of high-skill, year-round employees ($\gamma \approx 0.35$). These yield the estimate $\sigma = 1.24$, with a 95% confidence interval (0.40, 2.08), in the center of the table. The remainder of the table shows how this estimate changes under a wide range of different assumptions on the base parameters. Regardless of these varying assumptions on the other parameters of the model, the empirical estimates in Table 2, col. 10 imply values of σ that never fall outside the range 0.87–1.99.

Table 5b presents estimates assuming substantial monopsony power, with $e_N \equiv 3$. The quantitative conclusions of Table 5b change minimally, implying values of σ that never fall outside the range 0.80–1.66.

This suggests that low-skill H-2B 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

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).

[43] The other relevant industries' capital shares fall between the extremes 0.24–0.45, corresponding to $\beta \approx 0.27$–0.51. Details in Appendix A8. 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).

[44] The share of year-round U.S. employees in total employment in the core firm survey sample is 0.470 (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 U.S. workers in the inner (low-skill) labor nest in the survey sample is $1 - \alpha = 0.668$ (std. err. 0.012, $N = 470$).

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 elasticity of substitution. Our preferred estimate of $\sigma \approx 1.24$ 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. The evidence presented here is inconsistent with the firm-level elasticity for H-2B visa employers that we estimate taking any of these values, given that the highest upper bound on any 95% confidence interval implied by Table 5 is 3.42.[45] But the relatively minor difference between our estimates that exclude Rybczynski effects, and other estimates that include them, corroborates the limited importance of Rybczynski effects that has been found in the literature.

8.3 Aggregation of firm-level estimates

The firm-level analysis in Section 5 need not imply aggregate effects of equal magnitude. That said, this firm-level analysis contains some information about aggregate effects. Prima facie, we would expect adding more of a factor that did not exist before to raise GDP 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 U.S. employment in the aggregate if we observe no crowding out at the firm level.

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-

[45]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).

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.

We conduct three tests to address this question, none of which were pre-registered. First, we test for an effect of the lottery outcome on the competitive environment reported by firms. If 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 indistinguishable from zero (see Appendix Table A9).

Second, a different test arises from the partial replication in 2020 (Appendix Table A10). 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 3). In 2021, winning were relatively much more numerous, 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 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).

Amuedo-Dorantes et al. (2024) directly speak to business stealing. Their study also examines the H-2B program, but using a different approach (the arbitrary approval criteria used by the Department of Labor when it was unexpectedly overwhelmed with applications in 2018), and with data on the universe of firms. Despite the differences, the firm-level outcomes that their study has in common with our paper respond in a *quantitatively* similar manner, including a revenue elasticity of 0.14 (our paper, 0.20), and no evidence of "crowd out" of non-H-2B employment.[46] The paper shows, in addition, statistically precise zero impact on likely competitor firms' revenue and employment, that is, no evidence of business stealing.

Recent studies have found evidence of a more subtle form of market-level displacement: rather than pushing natives out of work, natives *avoid moving in* to labor markets impacted by immigration (Monras 2020; Dustmann et al. 2016b).[47] We cannot rule this out with our data. But the fact that we find strong evidence that U.S. workers' labor is a gross complement to H-2B workers suggests that there is less reason to think that would happen in this context.[48]

In fact, our 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.[49] 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).[50]

8.4 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 cannot hire. Theory and empirics nevertheless suggest limited potential for bias.

First, profit-maximizing employers willing and able to hire substitutes for lost H-2B workers on

[46] The fact that Amuedo-Dorantes et al.'s estimates are slightly smaller in magnitude may be due to the fact that the 2018 shock was unexpected. More on this in the conclusion. These authors also find evidence that firms denied H-2B visas were more likely to go out of business, but this develops outside the time frame of our study.

[47] U.S. workers also moved to urban areas when immigration was curtailed in 1920s (Abramitzky et al. 2023).

[48] Also, the set of U.S. workers who are willing to move long distances for a *temporary* low-skill job may be limited.

[49] Recall that we also impose that high-skill permanent employment H is unaffected by a shock to immigrant employment, which is realistic (and empirically confirmed) in the short-term setting we study. If, instead, there were an increase in the number of H-2B visas available (or the chance of being authorized to use them), permanent employment would be expected to respond. The first order effect of this would be larger revenue and employment responses than what we obtain (e.g., replace $1 - s_K$ with $1 - s_K - s_H$ in (12)).

[50] Also, Townsend and Allen (2024) show that visa-based restrictions on cross-firm mobility by migrants result in an inefficient allocation of workers across firms. At a market level, if more firms had access to H-2Bs, such inefficiencies could be reduced. (See also Mayda et al. 2023, who argue lotteries allocate visas inefficiently.)

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.[51] 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 U.S. workers in very basic low-skill jobs.

Next, the estimated treatment effects in Section 5 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—hypotheses that are rejected in Tables 2 and 3.

Moreover, the magnitude of those effects is informative about possible substitution into unobserved black-market employment. Consider the Euler equation (9), 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 the observed employment of authorized immigrants: $I_\ell^* \equiv (1+\phi)I_\ell$ such that $0 \leqslant \phi \leqslant 1$ 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

$$\phi \approx \underbrace{\ln \frac{I_w}{I_\ell}}_{\sim 0.618} + \underbrace{\frac{s_N}{s_I} \ln \frac{N_w}{N_\ell}}_{\sim 0.219} - \underbrace{\frac{1-s_K}{s_I} \ln \frac{R_w}{R_\ell}}_{\sim 0.828} = 0.009, \tag{15}$$

where the values on the right hand side are filled in from the empirics above.[52] 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 almost the entire

[51] 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).

[52] The estimate of $\ln \frac{I_w}{I_\ell} = 0.618$ is from the first-stage regressions in Appendix Table A4; the estimate of $\ln \frac{N_w}{N_\ell} = 0.116$ is from Table 2, col. 7; the estimate of $\ln \frac{R_w}{R_\ell} = 0.135$ is from Table 2 col. 2; $s_K = s_H = 0.35$; $s_I = (1-0.668) \times (1-s_K-s_H) = 0.106$ from Section 8.2; and $s_N \approx 0.2$.

resulting fall in *observed* immigrant and U.S. employment to explain it.

9 Conclusion

The U.S. has a long history of limiting contract foreign labor for low-skill work. In this tradition, H-2B visas are quota restricted, by law, to avoid "adversely affect[ing] the wages and working conditions of similarly-employed U.S. workers."[53] 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.

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 +0.20–0.22). 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 H-2B worker employment on U.S. employment (elasticity +0.61, Anderson-Rubin p=0.05).

These results are robust in several pre-registered subsamples. Scale effects are generally larger at both rural firms (consistent with U.S. labor supply being elastic in such markets) and at firms facing more competition (consistent with Burstein et al. [2020]'s finding 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 unusually transparent.

Why are the effects so uniformly positive despite widespread priors of a harm to U.S. workers?

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

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.[54]

What do these findings imply about the likely impact of increasing the H-2B visa quota? There is some potential for our estimates to 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 visa quota that our short-run estimates under a fixed quota do not fully capture.[55] 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.[56]

[54]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.

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

[56]Consistent with this, as noted earlier, Amuedo-Dorantes et al. (2024) found slightly smaller responses by firms to H-2B hires in 2018, when supply restrictions were more unexpected.

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Online Appendix

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

Michael A. Clemens and Ethan G. Lewis — June 2026

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 (8), let us begin with the general setup underlying proposition 1. Inverting the demand function (7) as $p = D^{\frac{1}{\eta}} Q^{-\frac{1}{\eta}}$, we have that revenues, $R = Q(p)p = D^{\frac{1}{\eta}} Q^{\frac{\eta-1}{\eta}}$. The firm's problem is to maximize profits

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

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

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

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

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

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

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

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

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

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

where the last step follows from homogeneity.[1] 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 particular, could choose $I_w \leq \bar{I}$, it must be that unconstrained firms have profits that are at least as large as constrained firms, and therefore $\frac{1}{\eta} D^{\frac{1}{\eta}} Q_w^{\frac{\eta-1}{\eta}} \geq \frac{1}{\eta} D^{\frac{1}{\eta}} Q_\ell^{\frac{\eta-1}{\eta}} + \lambda I_\ell$ (where recall that subscript w refers to unconstrained "winning" firms and ℓ refers to constrained "losing" firms). But this implies that

[1] This abstracts from permanent labor, which is described as fixed at share γ in the main text. Accounting for this changes the math slightly – instead, $C^* = (1-\gamma) \frac{\eta-1}{\eta} D^{\frac{1}{\eta}} Q^{\frac{\eta-1}{\eta}} + \mathcal{F} - \lambda I$, which slightly alters subsequent expressions – but it does not change any of the implications described here. We re-incorporate permanent labor below.

unconstrained revenues are weakly higher $D^{\frac{1}{\eta}} Q_w^{\frac{\eta-1}{\eta}} \geq D^{\frac{1}{\eta}} Q_\ell^{\frac{\eta-1}{\eta}}$, and therefore that unconstrained output is weakly higher $Q_w \geq Q_\ell$. Rearranging, the proportional revenue increase induced by a relaxation of the hiring constraint is larger the larger is the demand elasticity:

$$\ln(R_w/R_\ell) = \frac{\eta-1}{\eta} \ln(Q_w/Q_\ell).$$

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

$$\ln \frac{R_w}{R_\ell} \approx s_I \ln \frac{I_w}{I_\ell} + s_N \ln \frac{N_w}{N_\ell} + s_K \ln \frac{K_w}{K_\ell}, \tag{A.6}$$

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, U.S. 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.

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

$$\Pi_w/R_w - \Pi_\ell/R_\ell = \mathcal{F} D^{-\frac{1}{\eta}} \left(Q_\ell^{-\frac{\eta-1}{\eta}} - Q_w^{-\frac{\eta-1}{\eta}} \right) - \lambda I_\ell D^{-\frac{1}{\eta}} Q_\ell^{-\frac{\eta-1}{\eta}}.$$

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 (8) 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

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

In this case, the first order conditions become:

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

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

$$\beta \frac{\eta-1}{\eta} D^{\frac{1}{\eta}} z^{\frac{\eta-1}{\eta}} K^{\beta \frac{\eta-1}{\eta}-1} \left(\alpha I^{\frac{\sigma-1}{\sigma}} + (1-\alpha) N^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1} \frac{\eta-1}{\eta}(1-\beta)} = r \tag{A.10}$$

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:

$$\frac{rK_w}{R_w} = \frac{rK_\ell}{R_\ell} = \beta\frac{\eta - 1}{\eta} = s_K \tag{A.11}$$

which implies $\ln(K_w/K_\ell) = \ln(R_w/R_\ell)$. Recall that substituting this into (A.6) also delivers :

$$\ln(R_w/R_\ell) = \ln(K_w/K_\ell) \approx \frac{s_I}{1-s_K}\ln(I_w/I_\ell) + \frac{s_N}{1-s_K}\ln(N_w/N_\ell) \tag{A.12}$$

For U.S. employment, we can use the equality of (A.9) at different factor mixes:

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

to get that

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

where the ugly ratio of parameters that multiply the second term on right hand side are included in order to get it back into the form it was in the revenue function, (A.7). This allows us to construct the approximation:[2]

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

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

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

$$= \frac{\sigma s_K}{c_1}\ln(K_w/K_\ell) + \frac{s_I}{c_1}\left[\frac{\sigma(\eta-1)(1-\beta)-\eta(\sigma-1)}{(\eta-1)(1-\beta)}\right]\ln(I_w/I_\ell) \tag{A.16}$$

where

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

[2]This comes from applying (A.6) to (A.7), taking out the (ln separable) part assigned to capital.

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, we use (A.16) to show results for intuitive the two factor case (in which we also impose $\beta = 0$ so $s_K = 0$):

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

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

To include the adjustment of capital, we substitute the expression for capital, (A.12), into (A.16) :

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

Some algebra, plus the fact that $s_K \times \eta = \beta(\eta - 1)$ from (A.11), simplifies the numerator of this to:

$$\sigma s_K(\eta-1)(1-\beta) + (1-s_K)\left[\sigma(\eta-1)(1-\beta) - \eta(\sigma-1)\right] = (\eta-1)(1-\beta) - (\sigma-1) \tag{A.17}$$

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

$$\ln(N_w/N_\ell) = s_I\left[\frac{(\eta-1)(1-\beta) - (\sigma-1)}{c_2}\right]\ln(I_w/I_\ell), \tag{A.18}$$

where $c_2 \equiv (\eta - 1)(1 - \beta)[c_1(1 - s_K) - \sigma s_K s_N]$. The sign of c_2 is not obvious, but it is positive. 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

$$c_2 = c_1^{num}(1 - s_K) - \sigma s_N s_K(\eta - 1)(1 - \beta). \tag{A.19}$$

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 the production function in (8), that is,

$$Q = zH^\gamma K^\beta\left(\alpha I^{\frac{\sigma-1}{\sigma}} + (1-\alpha)N^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}(1-\beta-\gamma)}, \tag{A.20}$$

we have the revenue function:

$$R = D^{\frac{1}{\eta}} z^{\frac{\eta-1}{\eta}} H^{\gamma\frac{\eta-1}{\eta}} K^{\beta\frac{\eta-1}{\eta}}\left(\alpha I^{\frac{\sigma-1}{\sigma}} + (1-\alpha)N^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta-\gamma)}. \tag{A.21}$$

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 U.S. worker seasonal 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 (A.6) – but the factor shares need to be adjusted in some cases. (A.12), describing the responses of revenues and capital to N and I, holds as is. (A.16), describing N's response, requires adjustment to

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

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)

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

$$= s_I\left[\frac{\sigma s_K(\eta-1)(1-\beta-\gamma)+(1-s_K)\left[\sigma(\eta-1)(1-\beta-\gamma)-\eta(\sigma-1)\right]}{(\eta-1)(1-\beta-\gamma)[c_3(1-s_K)-\sigma s_K s_N]}\right]\ln(I_w/I_\ell) \tag{A.24}$$

$$= s_I\left[\frac{(\eta-1)(1-\beta-\gamma\sigma)-(\sigma-1)}{c_4}\right]\ln(I_w/I_\ell) \tag{A.25}$$

where $c_4 = c_3^{num}(1-s_K)-(\eta-1)(1-\beta-\gamma)\sigma s_K s_N$ and c_3^{num} is the numerator of c_3 (when written out in long form), shown above. Note that c_4 (which corresponds to Θ in the main text of the paper) (a) has a positive derivative with respect to σ (it can be written as $s_N\,(1-\beta-\gamma+(\gamma\eta+\beta)) > 0$) and (b) is greater than zero when $\sigma = 0$ (it can be written as $(1-s_K)\,[(1-\beta-\gamma)\;(\eta-1)-\eta s_N] > 0$ since $\eta\; s_N < (1-\beta-\gamma)\;(\eta-1)$ by definition of s_N), which jointly implies that $c_4 > 0$ for all $\sigma \geq 0$. The response of U.S. employment is thus positive if $(\eta-1) > \frac{(\sigma-1)}{1-\beta-\gamma\sigma}$, as was asserted in the text after Proposition 2.

For revenues, we go back to (A.12) to obtain

$$\ln(R_w/R_\ell) \approx \frac{s_I}{1-s_K}\ln(I_w/I_\ell) + \frac{s_N}{1-s_K}\ln(N_w/N_\ell) \tag{A.26}$$

$$= \frac{s_I}{1-s_K}\left(1+s_N\left[\frac{(\eta-1)(1-\beta-\gamma\sigma)-(\sigma-1)}{c_4}\right]\right)\ln(I_w/I_\ell). \tag{A.27}$$

A1.3 Incorporating Labor Supply – Simple Case

Now suppose that U.S. labor supply to the firm is upward sloping, due to "modern monopsony" labor market frictions (Manning 2021) or "classical monopsony" heterogeneity in U.S. workers' 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:

$$w_N = \left(1 + \frac{1}{e_N}\right)^{-1} R_N, \tag{A.28}$$

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.13) as follows:

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

Since $\left(\frac{1}{e_N} + \frac{1}{\sigma}\right)^{-1} = \frac{e_N}{\sigma+e_N}\sigma$, and $\ln\left(\frac{\alpha I_w^{\frac{\sigma-1}{\sigma}} + (1-\alpha) N_w^{\frac{\sigma-1}{\sigma}}}{\alpha I_\ell^{\frac{\sigma-1}{\sigma}} + (1-\alpha) N_\ell^{\frac{\sigma-1}{\sigma}}}\right)^{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta)} \approx s_I \ln(I_w/I_\ell) + s_N \ln(N_w/N_\ell)$, we now have that:

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

Furthermore, 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:

$$\ln(N_w/N_\ell) \approx \frac{s_I \frac{e_N}{\sigma+e_N}\sigma\left[\frac{\eta-\sigma}{\sigma(\eta-1)}\right]}{1 - s_N \frac{e_N}{\sigma+e_N}\sigma\left[\frac{\eta-\sigma}{\sigma(\eta-1)}\right]} \ln(I_w/I_\ell) \tag{A.31}$$

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

$$= s_I \frac{e_N(\eta - \sigma)}{e_N[(\eta - 1)(1 - s_N) + (\sigma - 1)s_N] + \sigma(\eta - 1)} \ln(I_w/I_\ell) \tag{A.33}$$

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

A1.4 Incorporating Labor Supply – General Case

If we include capital and permanent labor, along with the same (empirically supported) assumptions about adjustments as before (capital adjusts, but permanent labor does not), the proportional difference in equilibrium U.S. temporary labor is given by:

$$\left(\frac{1}{e_N}+\frac{1}{\sigma}\right)\ln(N_w/N_\ell)$$

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

This implies

$$\ln(N_w/N_\ell) \approx \frac{e_N}{\sigma+e_N}\frac{\sigma s_K}{c_3'}\ln(K_w/K_\ell) + \frac{e_N}{\sigma+e_N}\frac{\sigma s_I}{c_3'}\left[\frac{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta-\gamma)-1}{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta-\gamma)}\right]\ln(I_w/I_\ell) \tag{A.34}$$

where

$$c_3' = 1 - \frac{e_N}{\sigma+e_N}\sigma s_N\left[\frac{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta-\gamma)-1}{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta-\gamma)}\right]$$

Notice that this is already sufficient to see that the qualitative result is the same in this general case. As e_N goes to infinity—that is, the case where we ignored upward sloping U.S. labor supply—then $\frac{e_N}{\sigma+e_N}$ goes to one, c_3' goes to c_3 and A.34 goes to A.22. In contrast, as e_N goes to zero, there is no response of U.S. hires to immigrant hires, like in the last section.

We can also rearrange c_3' as

$$c_3' = \frac{(\eta-1)(1-\beta-\gamma) - \frac{e_N}{\sigma+e_N}s_N\left[\sigma(\eta-1)(1-\beta-\gamma)-\eta(\sigma-1)\right]}{(\eta-1)(1-\beta-\gamma)},$$

which allows us to define $c_3'^{num}$ as the numerator of this expression, which we will use below.

The last step is to solve for the expression for $\ln(N_w/N_\ell)$ in this general case. Here again we substitute in A.12 for $\ln(K_w/K_\ell)$ (into A.34), to obtain:

$$\begin{aligned}
\ln(N_w/N_\ell) \approx{}& \frac{e_N}{\sigma+e_N}\frac{\sigma s_K}{c_3'}\left[\frac{s_I}{1-s_K}\ln(I_w/I_\ell)+\frac{s_N}{1-s_K}\ln(N_w/N_\ell)\right] \\
&+ \frac{e_N}{\sigma+e_N}\frac{\sigma s_I}{c_3'}\left[\frac{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta-\gamma)-1}{\frac{\sigma}{\sigma-1}\frac{\eta-1}{\eta}(1-\beta-\gamma)}\right]\ln(I_w/I_\ell).
\end{aligned}$$

Finally, collecting terms and solving for $\ln(N_w/N_\ell)$:

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

The rightmost term can be rewritten as $\frac{\sigma s_K}{c_3'}\frac{s_I}{1-s_K}+\frac{s_I}{c_3'}\frac{1-s_K}{1-s_K}\frac{\sigma(\eta-1)(1-\beta-\gamma)-\eta(\sigma-1)}{(\eta-1)(1-\beta-\gamma)}$ and the middle term as $\frac{c_3'(1-s_K)}{c_3'(1-s_K)-\frac{e_N}{(\sigma+e_N)}\sigma s_K s_N}$. Substituting these in and collecting terms, we have that:

$$\ln(N_w/N_\ell) \approx s_I\frac{e_N}{\sigma+e_N}\left(\frac{\sigma s_K(\eta-1)(1-\beta-\gamma)+(1-s_K)\left[\sigma(\eta-1)(1-\beta-\gamma)-\eta(\sigma-1)\right]}{c_4'}\right)\ln(I_w/I_\ell) \tag{A.35}$$

$$= s_I\frac{e_N}{\sigma+e_N}\left(\frac{(\eta-1)(1-\beta-\gamma\sigma)-(\sigma-1)}{c_4'}\right)\ln(I_w/I_\ell) \tag{A.36}$$

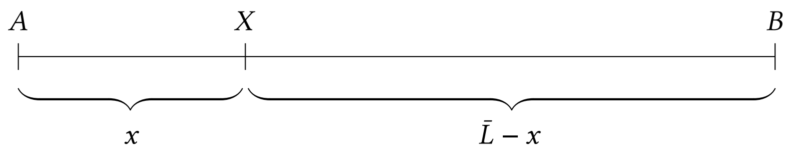
where $c_4'=(\eta-1)(1-\beta-\gamma)\left[c_3'(1-s_K)-\frac{e_N}{\sigma+e_N}\sigma s_K s_N\right]=c_3'^{num}(1-s_K)-\frac{e_N}{\sigma+e_N}(\eta-1)(1-\beta-\gamma)\sigma s_K s_N$. Notice that as long as $c_4'>0$, the condition for a positive response remains as before, that is, $(\eta-1)>\frac{(\sigma-1)}{1-\beta-\gamma\sigma}$. Also, A.36 goes to A.25 as e_N goes to infinity, and to zero as e_N goes to zero.

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 U.S. 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 Details on survey and data collection

The industry associations of H-2B employers sent the 2021 survey to their members seven weeks after the end of the second half of fiscal year 2021, on October 21, 2021, and followed up with email reminders to their members on November 1, 12, and 30. We received responses from October 21, 2021 through January

26, 2022. We closed the 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.

The title of the survey was "*Survey of U.S. 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.[3] Respondents were told that "U.S. worker" includes both citizens and lawful permanent residents. The survey respondents were well aware of their randomization outcome. One advantage of the purely online administration of the survey is that the enumeration experience is identical for all respondents, without regard to randomization status. There was no face-to-face contact that could in principle convey enumerator expectations of different responses by lottery winners versus lottery losers.

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

The survey measures profits indirectly, due to the well-known reluctance of firms to directly 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.[4] 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

[3]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.

[4]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.

Appendix Table A1: Summary statistics

	mean	*sd*	*min*	*max*	*count*
Revenue ($)_curr	8.7*e*+06	5.3*e*+07	5000.000	1.0*e*+09	472
H-2B temp. workers employed	22.570	44.817	0.000	412.000	472
U.S. temp. workers employed	31.867	130.207	0.000	1821.000	472
U.S. perm. workers employed	50.227	215.423	0.000	3600.000	471
Investment ($)	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 H-2B temp. workers employed	2.855	1.501	0.000	6.714	472
ihs H-2B 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
Low-population ZIP code	0.479	0.500	0.000	1.000	472
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.

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.

A4 Survey questionnaire

Figure A2 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.

A5 Summary statistics

Table A1 shows summary statistics across firms in the survey sample, pooled 2021 and 2022.

A6 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.

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	0.428
Golf courses/country clubs	46,536	0.098	478	0.035
Hospitality	43,349	0.091	894	0.065
Forestry	42,146	0.089	1,978	0.144
Seafood processing	42,100	0.089	1,292	0.094
Construction	38,928	0.082	382	0.028
Restaurants	11,856	0.025	66	0.005
Carnivals	10,534	0.022	637	0.046
Other	51,532	0.109	1,123	0.082

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.

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).

A7 First-stage regressions

Table A4 presents the first-stage regressions underlying the 2SLS estimates of Tables 2–3 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 $1 - e^{-0.618} = 46\%$.

Figure A3, corresponding to Figure 5 in the main text, shows the distribution of H-2B workers hired by lottery result, conditional on observed baseline traits.

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

Dep. var.:	H-2B 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)
H-2B 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 2–3. '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.

A8 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 H-2B workers in all temporary employment is 0.572, implying a U.S. 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 Landscaping Services industry has a low level of market share concentration. …The industry is characterized by a large number of small operators. According to the latest Economic Census, 94.0% of establishments employ fewer than 20 workers. Several companies have the resources to operate on a national scale and are typically integrated with landscape architecture departments, which enables them to bid for lucrative design-build-installation projects for commercial clients such as hotels and resorts. Nevertheless, the sheer volume of small-scale, low-value work conducted by nonemployers and small companies in the single-family housing market prevents these larger companies from capturing a substantial portion of revenue*" (Dmitry Diment,

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.

IBISWorld Industry Report 56173: Landscaping Services in the US, June 2022, p. 24).

A9 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.

A10 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 A7 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 H-2B 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.

A11 Robustness to influential observations

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

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, baseline (ln)	0.004	-0.002
	(0.021)	(0.007)
H-2B hired, baseline (IHS)	-0.002	0.001
	(0.013)	(0.004)
U.S. year-round hired, baseline (IHS)	-0.018	-0.002
	(0.018)	(0.006)
U.S. temporary hired, baseline (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. 'Baseline' for 2022 value is reported 2021 value; 'baseline' for 2021 value is reported 2020 value. All regressions include constant term and dummy variable for 2022.

A12 Robustness to randomization inference

Young (2018) notes that some data obtained from randomized controlled trials do not meet the conditions necessary to rely on the asymptotic properties of conventional standard errors. Table A14 shows the core results of the reduced-form regressions using the 'lottery win' instrument using Fisher's randomization inference as implemented by Heß (2017). The first column is an OLS regression of 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 col. 2 of Table 2, col. 6 of Table 2, and col. 2 of Table 3 respectively. The qualitative pattern of inference is identical to that in the core regressions of the main text using conventional standard errors.

A12.1 Components of the elasticity of substitution

As we derive estimates of the 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-U.S. worker elasticity of substitution comprises three independent effects.

First, the typically-estimated 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. 2016). 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."[5]

A13 Full regression results from tests for heterogeneous treatment effects

Table A11 and Table A12 report the full regression results underlying the coefficient plots in Figure 7.

A14 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 2–3 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.

[5]Relatively few empirical papers attempt to separate institutional determinants of the 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 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.

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
H-2B hired (IHS)	0.209	0.165	2.093
	(0.082)	(0.151)	(0.732)
H-2B 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 'H-2B hired' and its interaction with 'No ZIP'. The instruments are the 'lottery win' instrument and its interaction with 'No ZIP'.

A15 Effect on U.S. year-round employment

The preanalysis plan specified reporting tests of the effect of employing H-2B 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 H-2B workers exhibit higher employment of higher skill year-round U.S. workers, this relationship could arise from unobserved confounders. In the 2SLS specifications (cols. 4 and 8) any positive *effect* of H-2B 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 H-2B hiring occurred.

A16 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

Appendix Table A8: Effect of H-2B 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*	
H-2B temp. employed (IHS)			0.071	0.068			0.084	0.092
			(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)	0.938	0.876	0.919	0.876	0.938	0.876	0.915	0.876
	(0.025)	(0.042)	(0.028)	(0.041)	(0.025)	(0.042)	(0.028)	(0.041)
Full baseline controls	—	*Yes*	—	*Yes*	—	*Yes*	—	*Yes*
Number of firms	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 H-2B 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 H-2B temporary workers.

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.

A17 Partial replication in 2020

We partially replicate the 2021/2022 natural experiment in fiscal year 2020. This analysis was not prespecified because we did not anticipate that it would be possible. Although the Department of Labor conducted a very similar, independent lottery on January 1, 2020 for the second half of fiscal year 2020, our survey did not ask about firms' lottery-letter result from 2020. 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 3). 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.[6] For this reason the 2020 replication is partial rather than exact.

Figure A4 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.

Figure A5 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 Table 2 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. 2). 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 2021/2022 (Table 2, col. 4). 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 2021/2022 (Table 2, col. 7); and a coefficient on 'expected share' of 0.371 in 2020 (Table A10, col. 9) versus 0.136 in 2021/2022 (Table 2, col. 9).

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 H-2B 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. 5; 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 H-2B employment on U.S. worker employment (in 2020, Table A10, col. 10, coefficient 0.166 with p-val. 0.262; in 2021/2022, Table 2, col. 8, 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 U.S. 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.[7] The average national unemployment rate in the second half of fiscal 2020 was 10.9%; in 2021 it was 5.5%.[8] The similar magnitude of the point estimates in Tables 2 and A10 is inconsistent with any crucial dependency of the results on the tighter labor market conditions of 2021/2022.

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

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

[8]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		2SLS	OLS	2SLS	OLS		2SLS	OLS	2SLS
Instrument:			*Lottery win*		*Expected share*			*Lottery win*		*Expected share*
H-2B employed 2020 (IHS)	0.234		0.151		0.146	0.108		0.071		0.166
	(0.045)		(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.

A18 Treatment of zeros

In the 2021 survey, the question on investment was set up to be answered in the data type *currency*, which had the effect of defaulting to the value $0.00. In other words, item nonresponse cannot be distinguished from an affirmative answer of zero for investment in the 2021 survey. In 2022, the survey form was set up so that the response for investment was treated as freeform text, meaning that nonresponse (blank) can be distinguished from an affirmative zero. For this reason, in Table A15, we repeat the analysis of the investment outcome restricting the sample to the 2022 survey only. The results are not materially different compared to Table 3. In the restricted table, the reduced-form effect of *lottery win* on investment and the 2SLS effect instrumented by *lottery win* are larger in magnitude and remains highly statistically significant (cols. 2-3). Using the *expected share* instrument, both the reduced-form effect and the 2SLS effect increase in magnitude, though the standard errors become somewhat larger (the sample falls sharply, to 207 firms). The 2SLS effect with the *expected share* instrument is only statistically distinguishable from zero with a p-value of 0.159. There is no systematic decline in the coefficient estimates in the restricted regressions, and the increase in standard errors is what we would expect in the much smaller sample. We conclude that item nonresponse concealed as zeros in the 2021 data is not an important driver of these results.

Appendix Table A11: HETEROGENEOUS EFFECTS BY COMPETITION AND SIZE

	(1)	(2)	(3)	(4)	(5)	(6)
	Number of competitors		Competition (subjective)		Small firm	
	High	*Low*	*High*	*Low*	*Yes*	*No*
Dep. var.: Revenue (ln)						
H-2B employed (IHS)	0.260 (0.143)	0.189 (0.069)	0.412 (0.285)	0.169 (0.073)	0.345 (0.130)	0.076 (0.088)
And.-Rubin p-*val.*	0.074	0.006	0.117	0.024	0.006	0.408
N	272	200	169	303	226	246
Dep. var.: U.S. temporary workers employed (IHS)						
H-2B employed (IHS)	0.075 (0.253)	0.346 (0.161)	0.962 (0.599)	0.004 (0.139)	0.139 (0.220)	0.240 (0.211)
And.-Rubin p-*val.*	0.771	0.036	0.044	0.980	0.538	0.260
N	272	200	169	303	226	246
Dep. var.: Investment (IHS)						
H-2B employed (IHS)	0.986 (0.995)	3.161 (1.060)	2.922 (1.988)	1.748 (0.689)	3.837 (1.243)	0.493 (0.963)
And.-Rubin p-*val.*	0.309	0.000	0.108	0.005	0.000	0.609
N	262	194	160	296	217	239

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 'H-2B employed (IHS)' as the endogenous regressor, in a regression with full baseline controls, corresponding to the specification in col. 3 of Table 2, col. 8 of Table 2, and col. 3 of Table 3. 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).

Appendix Table A12: Heterogeneous effects by location

	(7)	(8)	(9)	(10)
	Rural		Low population	
	Yes	*No*	*Yes*	*No*
Dep. var.: Revenue (ln)				
H-2B employed (IHS)	0.330	0.189	0.307	0.179
	(0.235)	(0.078)	(0.145)	(0.097)
And.-Rubin p-*val.*	0.150	0.018	0.032	0.069
N	131	341	226	246
Dep. var.: U.S. temporary workers employed (IHS)				
H-2B employed (IHS)	0.613	0.078	0.292	0.112
	(0.338)	(0.168)	(0.190)	(0.226)
And.-Rubin p-*val.*	0.053	0.646	0.131	0.623
N	131	341	226	246
Dep. var.: Investment (IHS)				
H-2B employed (IHS)	4.418	1.339	3.820	0.695
	(2.253)	(0.756)	(1.278)	(0.913)
And.-Rubin p-*val.*	0.001	0.067	0.000	0.441
N	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 'H-2B employed (IHS)' as the endogenous regressor, in a regression with full baseline controls, corresponding to the specification in col. 3 of Table 2, col. 8 of Table 2, and col. 3 of Table 3. 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. "Rural" firms are those whose ZIP code is classified by the the U.S. Dept. of Agriculture as anything other than "Metropolitan Area, Core" (RUCA code 1; ERS 2020). "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 (NBER 2017).

Appendix Table A13: Robustness: Quantile regressions (p50)

Dep. var:	Revenue (ln)		U.S. temporary workers (IHS)	
Estimator:	Quantile	IV quantile	Quantile	IV quantile
H-2B employed (IHS)	0.078	0.112	0.047	0.006
	(0.008)	(0.054)	(0.010)	(0.055)
Number of firms	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, baseline controls, and a dummy for the year 2022. Quantile IV estimator due to Kaplan and Sun (2017). 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 A14: Robustness: Randomization inference

Dep. var:	H-2B 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

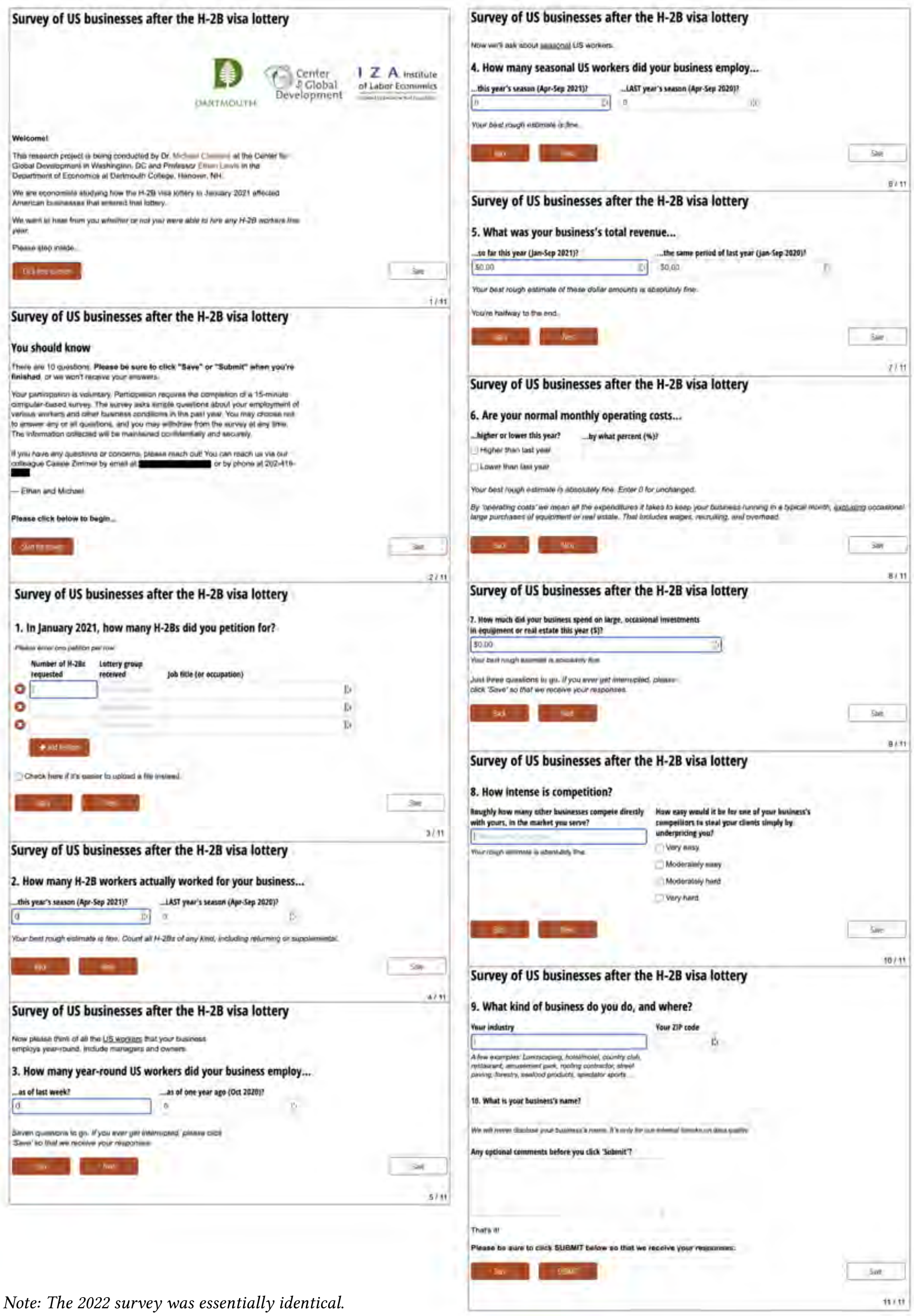
Robust standard errors in parentheses. Uses Fisher's randomization inference implemented by Heß (2017). Pooled data for the January 2021 and January 2022 lotteries. The unit of observation is firms. All regressions include constant term and a dummy for the year 2022.

Appendix Table A15: Effect of H-2B workers on Investment, 2022 only

	(1)	(2)	(3)	(4)	(5)
Dep. var:	*Investment (IHS), 2022 only*				
Estimator:	OLS		2SLS	OLS	2SLS
Instrument:			*Win*		*Share*
H-2B employed (IHS)	−0.092		4.709		3.188
	(0.310)		(2.870)		(2.759)
Anderson-Rubin p-*val.*	—		*0.017*		*0.159*
Lottery win		1.450			
		(0.615)			
Expected share				2.799	
				(2.018)	
Full baseline controls	Yes	Yes	Yes	Yes	Yes
Number of firms	207	207	207	207	207

Data for the January 2022 lottery only. The unit of observation is firms. All regressions include constant term. Robust standard errors in parentheses. The dichotomous *Win* ('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 *Share* ('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' include the predetermined values of the prior year's revenue, number of U.S. year-round workers, number of U.S. temporary workers, and number of H-2B workers.

Figure A2: The 2021 firm survey questionnaire as respondents saw it on 11 screens



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

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...

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...

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)

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

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

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

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

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

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

Your best rough estimate is fine.

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)?	...the same period of last year (Jan-Sep 2020)?
$0.00	$0.00

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

You're halfway to the end.

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? **...by what percent (%)?**

- Higher than last year
- Lower than last year

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.

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.

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?

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

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.

Any optional comments before you click 'Submit'?

That's it!

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

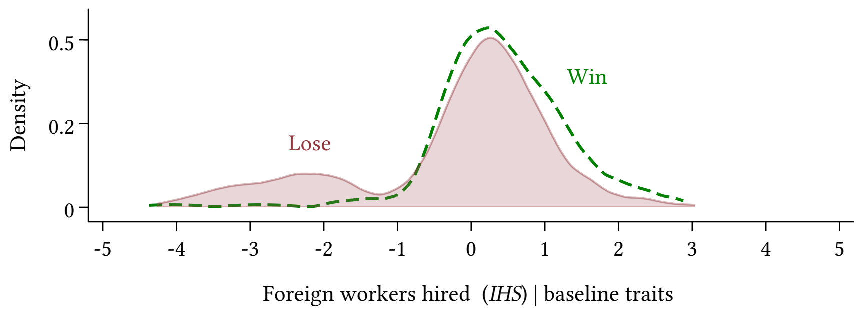
Save

11 / 11

Note: The 2022 survey was essentially identical.

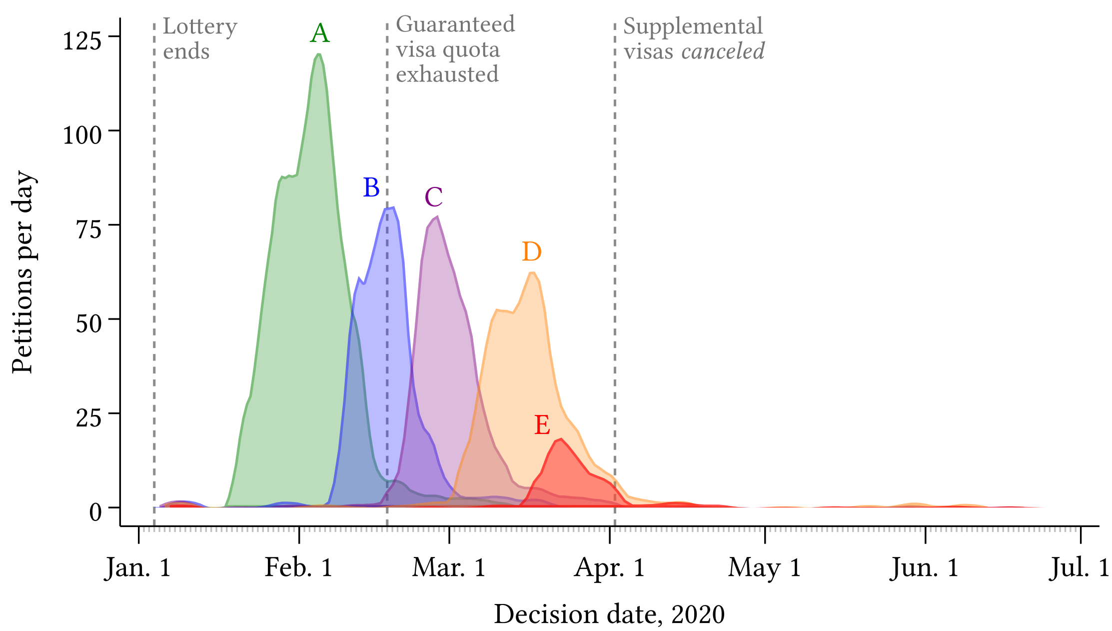
Figure A3: Reduced-form effect of lottery outcome on H-2B worker employment

(a) *H-2B workers employed*



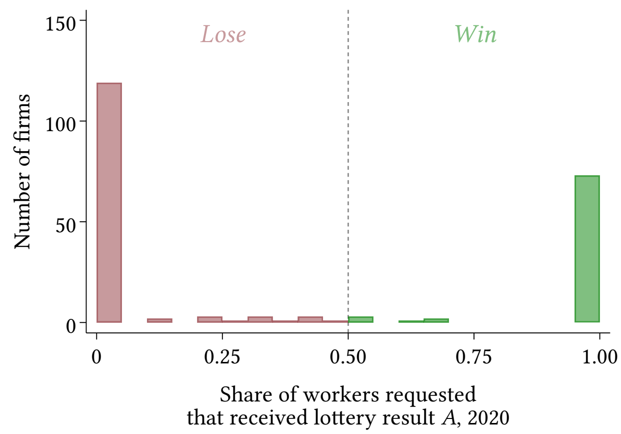
The unit of analysis is firms, pooled 2021 and 2022 samples. 'Win' is defined as a firm receiving randomized lottery letter '*A*' for petitions exceeding half of the total workers requested; all other results are defined as 'lose'. Graphs show Epanechnikov kernel density estimates with a bandwidth of 0.15 inverse hyperbolic sine (IHS) points. Residuals are estimated controlling for the full set of baseline traits, corresponding to column 4 in Table 2, measured in the year prior to the lottery.

Figure A4: H-2B worker petition decision dates by lottery result, universe of firms, second half of fiscal year 2020



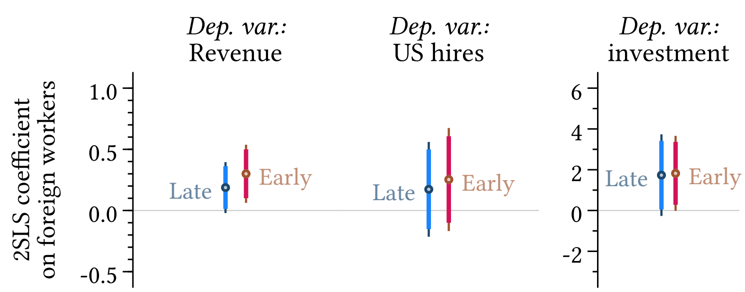
Shown is the universe of firms entering each lottery. Epanechnikov kernel densities, bandwidth 2 days. 'Decision date' is the date of the Department of Labor's decision on whether or not to certify each petition, a necessary condition of proceeding to petition USCIS for a visa. The 2020 lottery was conducted 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 A5: Defining a lottery 'win' at the firm level, 2020 lottery



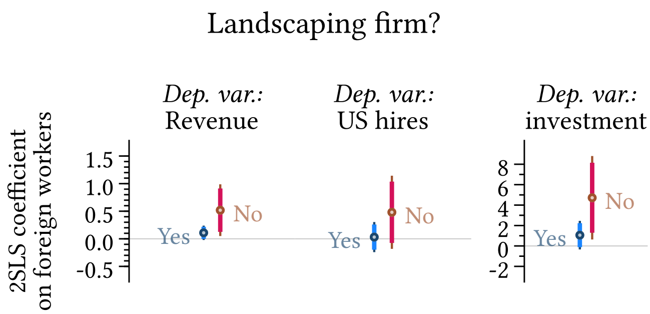
Only includes firms that voluntarily self-identified in the survey, allowing them to be matched to public records of their 2020 lottery result.

Figure A6: 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 H-2B workers employed (IHS) in a regression with full baseline controls, corresponding to col. 3 of Table 2, col. 8 of Table 2, and col. 3 of Table 3. The coefficients can be interpreted as elasticities. Thin vertical line shows 95% confidence interval, thick line shows 90% confidence interval.

Figure A7: 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 H-2B workers employed (IHS) in a regression with full baseline controls, corresponding to col. 3 of Table 2, col. 8 of Table 2, and col. 3 of Table 3. The coefficients can be interpreted as elasticities. Thin vertical line shows 95% confidence interval, thick line shows 90% confidence interval.

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