

Mahajan, Parag

**Working Paper**

## "Essential" migrants: Evidence from the 2020 H-2B visa lottery

Upjohn Institute Working Paper, No. 25-414

**Provided in Cooperation with:**

W. E. Upjohn Institute for Employment Research, Kalamazoo, Mich.

*Suggested Citation:* Mahajan, Parag (2025) : "Essential" migrants: Evidence from the 2020 H-2B visa lottery, Upjohn Institute Working Paper, No. 25-414, W.E. Upjohn Institute for Employment Research, Kalamazoo, MI,  
<https://doi.org/10.17848/wp25-414>

This Version is available at:

<https://hdl.handle.net/10419/315965>

**Standard-Nutzungsbedingungen:**

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

**Terms of use:**

*Documents in EconStor may be saved and copied for your personal and scholarly purposes.*

*You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.*

*If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.*

4-10-2025

## **“Essential” Migrants: Evidence from the 2020 H-2B Visa Lottery**

Parag Mahajan

*University of Delaware*, [paragma@udel.edu](mailto:paragma@udel.edu)

Upjohn Institute working paper ; 25-414

---

### **Citation**

Mahajan, Parag. 2025. "Essential' Migrants: Evidence from the 2020 H-2B Visa Lottery." Upjohn Institute Working Paper 25-414. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research. <https://doi.org/10.17848/wp25-414>

This title is brought to you by the Upjohn Institute. For more information, please contact [repository@upjohn.org](mailto:repository@upjohn.org).

## “Essential” Migrants: Evidence from the 2020 H-2B Visa Lottery

Parag Mahajan  
*University of Delaware*  
Email: [paragma@udel.edu](mailto:paragma@udel.edu)

Upjohn Institute Working Paper 25-414

April 2025

### ABSTRACT

I study how access to foreign-born workers impacts firms and local economics in times of acute crisis. The 2020 H-2B visa lottery randomly gave some U.S. firms the chance to hire low-wage, migrant workers during the height of the COVID-19 pandemic. Using administrative data across three government agencies, I find that access to H-2B workers led to decreased business closures, increased revenues, increased payroll, and increased employment in 2020. I also find suggestive evidence that these effects spilled over to non-participant firms within the same county.

**JEL Classification Codes:** J23, F22, J61

**Key Words:** migrant workers, H-2B visa, COVID-19 pandemic, firm dynamics

**Acknowledgments:** I am extremely grateful to Joe Ballegeer of the Philadelphia Federal Statistical Research Data Center for help in disclosing records. I also thank Rene Crespin, Olga Gorbachev, Susan Pozo, Bryan Stuart, Jeremy Tobacman, and Barton Willage, along with audiences at the APPAM Fall Research Conference, and SEA Annual Meetings for helpful comments. I gratefully acknowledge financial support from the W.E. Upjohn Institute for Employment Research Early Career Research Grant Number 22-58161-09. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at Federal Statistical Research Data Center under FSRDC Project Number 2105 (CBDRB-FY25-P2105-R11829).

# 1 Introduction

How important is the availability of foreign-born workers to firms and local economies during times of acute crisis? The COVID-19 pandemic brought this question into sharp focus amid widespread economic disruption, particularly on the supply side of the economy. The notion that some workers are “essential” because they support critical industries gained prominence during the pandemic, and there was renewed understanding that migrant workers were particularly overrepresented in these critical industries.<sup>1</sup>

Yet, beyond this simple recognition of their presence, policymakers still lack a firm understanding of the *degree* to which foreign-born workers contributed to firms and local economies during the pandemic. This uncertainty clouded policy decisions at the time: there was tension between closing off the border to prevent the spread of COVID-19 and allowing migrant workers into the country to perform “essential” work.<sup>2</sup>

In this paper, I provide the first well-identified evidence on this question. To do so, I combine detailed information on firm-level usage of the H-2B visa program from the Department of Labor (DoL) and the United States Citizenship and Immigration Services (USCIS) with administrative records on firm survival, payroll, employment, and revenues from the U.S. Census Bureau. I then study the 2020 H-2B visa lottery, which randomly assigned participating employers—over 95% of which were classified as “essential” by state and local orders<sup>3</sup>—the opportunity to hire foreign-born workers starting on April 1, 2020.

My most important result is that lottery-winning employers were more likely to stay in operation in 2020—the clearest indication that H-2B migrant workers were indeed essential to their employers during the peak of the COVID-19 crisis. Availability of H-2B workers also helped firms operate at scale, with lottery winners generating higher revenues and paying out higher wage bills. Average treatment effect on the treated estimates suggest economically meaningful magnitudes: employers that had the option to hire H-2B workers were two percentage points more likely to operate, generated 6 percent more in revenues, and disbursed 24 percent more in payroll relative to employers that did not. Effects did not last into 2021, when H-2B workers from the 2020 hiring season were no longer employed, indicating that these temporary workers provided temporary boosts to the employers that hire them, coinciding with the time period that they were employed.

Finally, I use the random assignment of H-2B workers at the firm level to construct a measure of random exposure to H-2B workers at the *county* level. I show that non-H-2B-

---

<sup>1</sup>See [this](#) report of the Joint Economic Committee of the U.S. Congress on the COVID-19 pandemic titled “Immigrants are Vital to the U.S. Economy.” See also this [congressional testimony](#) by the Center for American Progress, titled “Immigrants as Essential Workers During COVID-19.”

<sup>2</sup>The Center for Disease Control and Prevention (CDC) invoked Title 42 in March of 2020 to suspend border admissions and authorize immediate expulsion of unlawful entry to prevent the spread of COVID-19. Meanwhile, the State Department waived in-person interviews to ensure the arrival of workers on H-2A and H-2B continued, partly due to these workers’ importance to the U.S. food supply.

<sup>3</sup>Based on classifications developed in [Atalay et al. \(2020\)](#).

participating firms in counties that were more exposed to H-2B workers performed better across a range of outcomes in 2020 relative to firms in counties that were less exposed. These spillover effects reflect that the “essentiality” of H-2B workers was external to their employers.

This paper makes two key contributions to the literature. It is the first to provide credible evidence that foreign-born workers were essential to business operation, at scale, during the COVID-19 pandemic. Results indicate that the availability of H-2B workers decreased business closures at a time when more than \$500 billion in government spending was directed toward the same end, in the form of the Paycheck Protection Program (PPP). This study therefore underscores the potentially critical, cost-effective role of temporary migrant labor in maintaining business continuity during crises. Because it examines the causal effect of migrant availability during the COVID-19 pandemic, this work is related to but distinct from previous research on the effect of the COVID-19 pandemic on the availability of migrant workers (e.g., [Borjas and Cassidy, 2023](#); [Peri and Zaiour, 2023](#)).

Second, this paper is the first to combine H-2B lottery variation with full-sample, administrative firm records. It thus contributes to a growing literature that analyzes the impacts of immigration at the firm level. A limited set of previous studies have examined how hiring foreign-born workers on H-1B (“high-skill”) and H-2B (“low-skill”) visas impact firm performance and hiring behavior in the United States ([Kerr et al., 2015](#); [Dimmock et al., 2021](#); [Doran et al., 2022](#); [Amuedo-Dorantes et al., 2023](#); [Clemens and Lewis, 2024](#); [Glennon, 2024](#); [Mandelman et al., 2024](#); [Mahajan et al., 2024](#)). Results in this paper are similar in magnitude to previous H-2B studies at the firm level ([Clemens and Lewis, 2024](#); [Amuedo-Dorantes et al., 2023](#)); however, it also finds significant spillover effects on non-H-2B-participant firms that [Amuedo-Dorantes et al. \(2023\)](#) look for and do not detect in 2018. This is likely due to the singular nature of the pandemic time period being studied, relating these results to previous work on local resilience to the COVID-19 pandemic (e.g., [Levine et al., 2024](#); [Ayyagari et al., forth.](#)). In broader terms, this paper contributes to a burgeoning literature that centers the firm in the study of immigration.<sup>4</sup>

The remainder of this paper is organized as follows. Section 2 provides background on the H-2B visa lottery and its implementation during the COVID-19 pandemic. Section 3 describes the data sources and matching process used to link administrative records across agencies. Section 4 outlines the empirical strategy and presents the main results. Section 5 concludes.

## 2 The 2020 H-2B Visa Lottery

The H-2B visa program allows U.S. employers to hire foreign-born workers for temporary, non-agricultural positions. The program has an annual quota of 66,000 visas, divided equally

---

<sup>4</sup>See, e.g., [Dustmann and Glitz \(2015\)](#); [Mitaritonna et al. \(2017\)](#); [Gray et al. \(2020\)](#); [Beerli et al. \(2021\)](#); [Brinatti and Morales \(2021\)](#); [Egger et al. \(2022\)](#); [Marchal et al. \(2022\)](#); [Muñoz \(2023\)](#); [Amior and Stuhler \(2023\)](#); [Arellano-Bover and San \(2023\)](#); [Morales \(2023\)](#); [Silliman and Willén \(2024\)](#); [Orefice and Peri \(2024\)](#); [Signorelli \(2024\)](#); [Delgado-Prieto \(2025\)](#).

between the first and second halves of the fiscal year. Jobs offered through the program typically last up to nine months, provide at least 35 hours of work per week, and pay approximately \$15 per hour. Notably, H-2B employers tend to be positively selected on several firm characteristics. The median H-2B user ranks in the top quintile of their industry-age adjusted revenue per worker distribution (Mahajan, 2024) and has higher employment, payroll, and revenues than the average U.S. firm (Amuedo-Dorantes et al., 2023; Clemens and Lewis, 2024).

The process of hiring an H-2B worker involves multiple steps across three federal agencies. First, employers must obtain a Temporary Labor Certification (TLC) from the DoL. The TLC application requires firms to demonstrate both a lack of qualified U.S. workers and that H-2B hiring will not adversely affect local wages. In practice, TLC applications for the same worksite and occupation are consolidated onto one form. For the second half of fiscal year 2020 (2HFY), employers were given the window between January 1, 2020 and January 4, 2020 to submit their TLC applications.

Only upon receiving DoL certification of TLC applications can firms move to the next step of the H-2B procurement process, which is to file i129 petitions for visas from the USCIS. The USCIS processes applications sequentially until reaching the statutory quota of 33,000, at which point receipt is halted. Thus, firms whose DoL applications are processed sooner have a better chance of petitioning the USCIS for visas before the cap is reached.

Given this process, and due to overwhelming demand in years leading up to 2020, the DoL instituted a new lottery procedure for 2020. It randomly assigned each consolidated TLC form a letter from A through E. Consolidated forms were then processed based on alphabetical order of random group assignment. As seen in Figure 1, only applications assigned to groups A or B had any chance of being processed before USCIS reached the statutory cap, on February 18, 2020. The resulting “first stage” was not trivial: on average, firms that had all of their consolidated forms assigned to groups A or B secured approximately eight successful i129 petitions for H-2B workers in 2HFY 2020. In contrast, firms whose consolidated forms were entirely in groups C, D, or E had no possibility of hiring H-2B workers in 2HFY 2020. This lottery assignment is the basis of the identification strategy in this paper.

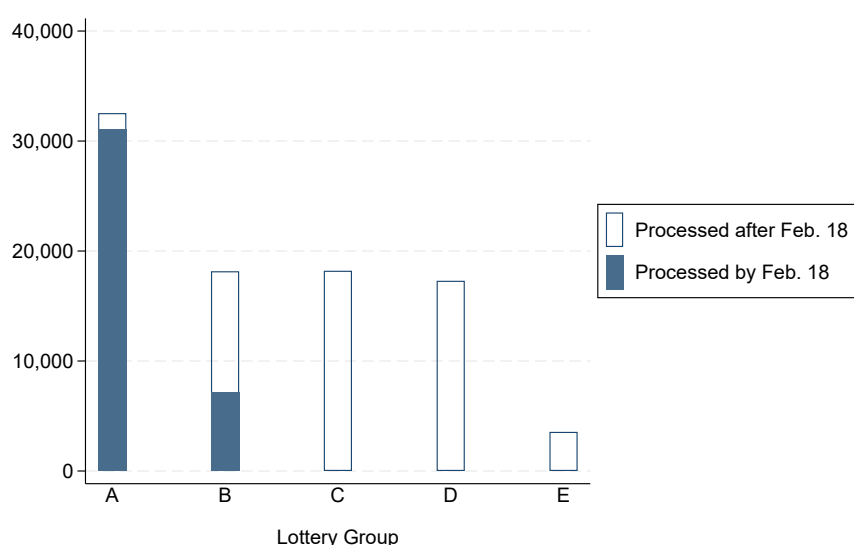
If firms’ i129 petitions are approved, potential H-2B workers are interviewed by the State Department before a final visa issuance is made. In 2020, a worker issued a visa in this third and final step could physically start working for their employer on April 1. Despite the halting of many immigration services at the time, the State Department gave consulates discretion to waive in-person interviews for H-2B workers specifically due to their importance to critical infrastructure during the pandemic.<sup>5</sup>

While the conversion from i129 petition to visa issuance and physical, on-site employment is typically high in other years, the timing of the 2HFY 2020 H-2B hiring cycle relative to the

---

<sup>5</sup>See the State Department’s [Important Announcement on H2 Visas](#).

**Figure 1: TLC Applications by Lottery Group**



**Source:** DoL Office of Foreign Labor Certification Performance Data.

**NOTE:** The USCIS stopped receipt of i129 petitions on February 18, 2020, so any TLC applications that were not processed by this date did not have a chance to convert to eventual H-2B visas.

COVID-19 pandemic created a unique situation.<sup>6</sup> Firms went through the first two steps of the H-2B procurement process at a time when COVID-19 was not yet widely recognized as a major threat to economic activity in the United States. However, firms' final decisions about whether to bring approved H-2B workers into the country were made after February 18 but before April 1, coinciding with the dramatic escalation of the pandemic across most U.S. localities. By late March 2020, most states had implemented stay-at-home orders, and business activity had contracted sharply. This created a situation wherein firms that won the lottery had secured the *option* to hire H-2B workers in a relatively normal environment but made their final hiring decisions during an unprecedented public health and economic crisis.

### 3 Data

To examine how access to H-2B workers impacted firms during the COVID-19 pandemic, I combine administrative data on H-2B visa applications from the Department of Labor and the USCIS with restricted-access longitudinal data on establishments and their parent firms from the U.S. Census Bureau.

The Department of Labor provides two key sources of data. First, information on firms' consolidated H-2B TLC forms comes from the DoL's Office of Foreign Labor Certification.<sup>7</sup> For each form, the data contains information on the employer name, address, state, ZIP code, the number of requested workers (applications), the number of certified requests, and the date

<sup>6</sup>In the years leading up to 2018, for example, the approved i129 to visa issuance conversion rate is usually around 80% (Amuedo-Dorantes et al., 2023).

<sup>7</sup>This data is publicly available from the [Department of Labor Performance Data](#) website.

the application was submitted. Critically, the Department of Labor also released the lottery letter assignments associated with each TLC form.<sup>8</sup>

Information on the next step of the H-2B procurement process comes directly from the USCIS.<sup>9</sup> This data contains information on the number of approved i129 petitions per fiscal year half for each employer, along with information on employer name, address, state, and ZIP code. While I therefore know the number of visa *approvals* each firm received toward the end of February 2020, I lack information on the number of of these approvals that actually converted into State Department issued visas and actual hires at the firm level. I return to the implications of not having data on this final step in Section 4.5.

Data on firm-level outcomes comes from the the 2021 version of the Longitudinal Business Database (LBD) (Chow et al., 2021). The LBD is an establishment-level panel data set constructed from administrative tax records for each U.S. non-farm, employee-hiring, private-sector establishment. Establishments are assigned unique, consistent identifiers that can be linked over time to create a true panel. The LBD contains establishment-level information on calendar Q1 employment and annual payroll for all employers. It also contains quarterly employment and payroll for single-establishment firms. Given that 2HFY H-2B visa workers are mostly employed in calendar Q2 and Q3, this level of granularity helps me specifically examine firm-level outcomes during the relevant H-2B hiring period. Quarterly employment is measured on March 12, June 12, September 12, and December 12. Employment counts include workers on H-2B visas.

Both the H-2B data and the Census firm data contain the employer name, state, city, and ZIP code. Thus, to link the H-2B data with the Census outcome data, I use a fuzzy matching procedure. I match 93.1% of lottery-subject, 2020 consolidated TLC forms, and 94.4% of lottery-subject, 2020 TLC worker applications to a source firm in the U.S. Census Bureau data, resulting in a sample of roughly 4,000 H-2B employer firms that were subject to the 2020 H-2B visa lottery.<sup>10</sup> Appendix A.1 provides a more detailed description of the matching process. To check that matching errors do not bias my results, Table A1 shows that an indicator for being assigned to lottery group A or B does not correlate with a binary indicator for being matched to the Census data, either at the consolidated form or application level.

For multi-unit firms, the unique firm identifiers (*firmid*) in the LBD allow me to link establishments to their parent firms. I then aggregate information across a firm's establishments to generate firm-level outcomes. A majority (around two-thirds) of firms can also be linked to annual revenue information. Additional details about aggregation to the firm level and ensuring results are not driven by spurious changes to *firmid* can be seen in Section A.2 of the Online Appendix.

---

<sup>8</sup>This data is publicly available from the [Department of Labor ETA Announcements](#).

<sup>9</sup>This data is publicly available from the [USCIS H-2B Employer Data Hub](#).

<sup>10</sup>All firm counts are rounded following Census Bureau requirements.



## 4 Methods and Results

### 4.1 Overview and Balance

The primary exposure variable of interest operationalizes Figure 1 by computing the proportion of a firm’s 2020 2HFY H-2B visa lottery applications that were assigned to lottery groups A or B: *Prop. A or B Apps<sub>j,2020</sub>*. In most cases, this is a binary variable, because firms typically file one consolidated form containing many applications, and lottery group assignment is made at the consolidated form level. However, because there are a small portion (less than 10 percent) of firms that file multiple consolidated forms, *Prop. A or B Apps<sub>j,2020</sub>* can be continuous.

Table 1 shows evidence that *Prop. A or B Apps<sub>j,2020</sub>* does not correlate with pre-treatment outcomes. I present both heteroskedasticity robust standard errors along with *p*-values based on randomization inference. Details on the randomization inference procedure can be found in Section B of the Appendix.

**Table 1: Balance Tests**

	Annual (2019)			2HFY (Q2–Q3, 2019)		
	(1) Active	(2) Log Payroll	(3) Log Revenues	(4) Active	(5) Log Payroll	(6) Log Employment
Prop. A or B Apps <sub>j,2020</sub>	-0.002 (0.004) [0.647]	0.024 (0.049) [0.610]	-0.017 (0.050) [0.729]	0.0001 (0.0062) [0.999]	0.006 (0.044) [0.913]	-0.014 (0.039) [0.735]
Observations (Firms)	4,000	4,000	2,700	3,700	3,600	3,400
Control Group Mean (levels)	0.971	\$2,907k	\$12,740k	0.938	\$524k	33.76

NOTE: Bivariate regressions with no controls. Heteroskedasticity robust standard errors in parentheses. Randomization inference *p* values in square brackets. Control group defined as firms with no A or B applications. 2HFY stands for “second half of the fiscal year.” 2HFY employment is defined as the average of calendar Q2 and Q3 employment. 2HFY outcomes are only available for single-unit firms. Firm counts rounded according to Census Bureau disclosure avoidance rules. The sample in Column (1) is the set of all firms with non-missing payroll information in 2019. The sample in Column (2) is the set of all firms with non-missing, positive payroll in 2019. The sample in Column (3) is the set of all firms with non-missing positive, revenues in 2019. The sample in Column (4) is the set of all single-unit firms with non-missing payroll in 2019. The sample in Column (5) is the set of all single-unit firms with non-missing, positive payroll in 2019. The sample in Column (6) is the set of all single-unit firms with non-missing, positive employment in 2019.

Control group means presented in the last row of Table 1 confirm that H-2B participating firms are positively selected on scale relative to the average U.S. firm. 2020 H-2B lottery participants grossed over 12.7 million USD in revenue and paid nearly 3 million USD to workers in 2019, on average. Thus, while few firms participate in the H-2B visa program, they are large and important enough to potentially generate spillover effects in their local economies, a notion that I probe further below.

## 4.2 Main Reduced Form Results

My main empirical specification estimates the reduced form effects of lottery luck on firm outcomes using Equation (1):

$$f(y_{jt}) = \alpha + \beta[\text{Prop. A or B Apps}_{j,2020}] + \gamma X_j + \varepsilon_j. \quad (1)$$

For continuous outcomes—payroll, revenues, and employment— $f(y_{jt})$  represents the Davis-Haltiwanger-Schuh (DHS) growth rate between 2019 and year  $t$ :  $f(y_{jt}) = \frac{(y_{jt} - y_{j,2019})}{(y_{jt} + y_{j,2019})/2}$ , and  $X_j = \log(y_{j,2019})$ . This ensures that  $\beta$  estimates the percentage change in  $y_{jt}$  as a result of the lottery while accommodating exit in the post-treatment period because the DHS growth rate is defined when  $y_{jt} = 0$  and  $y_{j,2019} \neq 0$ .<sup>11</sup> I also study the effects of lottery luck on whether or not a firm remains active, which I define using indicators for having positive payroll in a given time period. For these outcomes, simply,  $f(y_{jt}) = y_{jt}$  and  $X_j = y_{j,2019}$ .

For a given post-treatment year  $t \in \{2020, 2021\}$ , I consider two time windows: 1) the full year and 2) the second half of the fiscal year (2HFY), which spans the second and third calendar quarter of year  $t$ . The latter time window allows me to hone in on the period that most closely corresponds with the 2HFY H-2B worker hiring period. However, outcomes at this sub-annual frequency are only available for single-unit firms, as described in Section 3.

Table 2 presents the results from estimating Equation (1), and Panel A presents the headline results for 2020. Column (1) indicates that firms with higher rates of lottery luck were more likely to stay in operation. Columns (2) and (3) indicate that assignment of all lottery applications to groups A or B—which led to the opportunity to hire eight additional H-2B workers, on average, relative to firms that had all applications assigned to groups C, D, or E—increased annual payrolls by nearly 14 percent and annual revenues by more than 4 percent in 2020. Focusing specifically on the second half of the fiscal year, Columns (4) and (5) show that the results on active operation and payroll growth were larger during the H-2B hiring period among single-unit firms. Further, assignment of all lottery applications to groups A or B results in nearly 16 percent higher employment relative to firms that had all applications assigned to groups C, D, or E.

Panel B shows that the effects of being able to hire H-2B workers in 2020 do not extend to 2021. This is, in some ways, expected: the vast majority of H-2B workers are hired for nine months or less, so any lottery-induced hiring did not directly extend into 2021. Still, these results indicate that the relatively large effects of lottery luck on firm outcomes during the height of the COVID-19 pandemic were not long lasting. In particular, firms that did not

<sup>11</sup>Note that a standard model assessing the proportional changes associated with lottery luck would be:  $\log(y_{jt}) = \alpha + \beta[\text{Prop. A or B Apps}_{j,2020}] + \gamma \log(y_{j,2019}) + \varepsilon_j$ . This is equivalent to the following model:  $\log(y_{jt}) - \log(y_{j,2019}) = \alpha + \beta[\text{Prop. A or B Apps}_{j,2020}] + \gamma \log(y_{j,2019}) + \varepsilon_j$ . Equation (1) simply replaces  $\log(y_{jt}) - \log(y_{j,2019})$  with the DHS growth rate (which closely approximates log changes) in order to accommodate exit.

operate in 2020 as a result of lottery losses may have resumed activity by 2021, and that effects on firm activity in 2020 did not translate into effects on longer-run firm *survival*.<sup>12</sup>

**Table 2: Reduced Form Results**

	Annual			2HFY (Q2 & Q3)		
	(1) Active	(2) Payroll Growth	(3) Revenue Growth	(4) Active	(5) Payroll Growth	(6) Employment Growth
<b>Panel A: 2020 (During 2HFY 2020 H-2B Employment Window)</b>						
Prop. A or B Apps <sub><i>j</i>,2020</sub>	0.011** (0.005) [0.019]	0.136*** (0.017) [<0.001]	0.042** (0.017) [0.005]	0.018*** (0.007) [0.009]	0.177*** (0.020) [<0.001]	0.156*** (0.019) [<0.001]
<b>Panel B: 2021 (After 2HFY 2020 H-2B Employment Window)</b>						
Prop. A or B Apps <sub><i>j</i>,2020</sub>	-0.0002 (0.0064) [0.973]	0.008 (0.019) [0.701]	0.009 (0.018) [0.599]	0.001 (0.008) [0.927]	0.002 (0.021) [0.917]	0.012 (0.020) [0.558]
Observations (Firms)	4,000	4,000	2,700	3,700	3,600	3,400
Control Group Mean (2019 levels)	0.971	\$2,907k	\$12,740k	0.938	\$524k	33.76

NOTE: See Equation (1) for specification. Heteroskedasticity robust standard errors in parentheses. Randomization inference *p* values in square brackets. 2HFY stands for “second half of the fiscal year.” 2HFY employment is defined as the average of calendar Q2 and Q3 employment. 2HFY outcomes are only available for single-unit firms. Firm counts rounded according to Census Bureau disclosure avoidance rules. The samples used in each column are the same as in Table 1. In columns (2), (3), (5), and (6), growth rates refer specifically to Davis-Haltiwanger-Schuh (DHS) growth rates calculated relative to 2019, which accommodates zeroes in 2020 and 2021 values of the underlying variable due to exit (see Section 4.2).

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

### 4.3 Average Treatment Effect on the Treated Estimates

The structure of the lottery and the highly regulated nature of the H-2B visa system means that there is one-sided non-compliance among participant firms: firms whose lottery applications were assigned groups A or B had a chance to hire H-2B visa workers in 2HFY 2020, whereas firms whose lottery applications were all assigned to groups C, D, or E were both *de facto* and *de jure* excluded from hiring H-2B visa workers because they never had a chance to submit i129 petitions to the USCIS.<sup>13</sup> This institutional feature presents an opportunity to identify the average treatment effect on the treated (ATET) of having USCIS approval for H-2B hiring in the second half of fiscal year 2020.

Let  $D_i$  represent an indicator for whether a firm had any i129 approvals for new H-2B workers in 2020—that is, an indicator for having had the *option* to bring H-2B workers on

<sup>12</sup>This result differs from Amuedo-Dorantes et al. (2023), who find longer-run impacts on survival when firms were granted access to temporary, H-2B workers in 2018. There are two likely explanations for the difference: 1) the uniqueness of the economic environment during the COVID-19 pandemic, and 2) in 2018, rationing of access to H-2B workers was likely more unexpected than it was in 2020—in 2018, the rationing was announced ex-post, whereas in 2020, the lottery was announced in advance to participant firms.

<sup>13</sup>I have confirmed that there were no i129 petitions filed by firms fully assigned to lottery groups C, D, or E in the data.

board during the height of the pandemic.<sup>14</sup> Let  $F_j$  represent the number of lottery-subject consolidated forms submitted to the DoL by firm  $j$ .<sup>15</sup> Let  $Y_j(d), d \in \{0, 1\}$  represent potential outcomes for firm  $j$  in the state of the world that they are assigned  $D_i = d$ . Finally, let  $Z_j$  represent an indicator for whether or not *any* of a firm’s consolidated forms were assigned to lottery group A or B. Only *conditional on*  $F_j$  is  $Z_j$  randomly assigned, since firms that filed more consolidated forms mechanically have a higher chance of having one of them land in lottery groups A or B:  $Y_j(0), Y_j(1) \perp Z_j | F_j$ .<sup>16</sup>

Under this setup, I use the fact that

$$\underbrace{\mathbb{E}[Y_j(1) - Y_j(0) | D_j = 1]}_{\text{ATET}} = \sum_f \underbrace{\mathbb{E}[Y_j(1) - Y_j(0) | D_j = 1, F_j = f]}_{\text{ATET}(f)} \mathbb{P}[F_j = f | D_j = 1].$$

Due to one-sided non-compliance and the conditional randomness of  $Z_j$ , I can estimate  $\text{ATET}(f)$  using  $Z_j$  as an instrument for  $D_j$  at each level of  $f$ . I can then weight these estimates by  $\hat{\mathbb{P}}[F_j = f | D_j = 1]$  and sum to generate a consistent estimate of the overall ATET.

Table 3 presents the results from this exercise for 2020. They tell a similar story to Table 2, but one that is perhaps easier to interpret: firms that had the opportunity to hire H-2B workers in 2020 were two percentage points more likely to operate in 2020, had 24 percent higher payroll growth, and had 6 percent higher revenue growth relative to applicant firms that did not (Columns 1–3). Among single-unit firms in the second half of the fiscal year, firms that had the option to hire H-2B workers were 3.5 percentage points more likely to operate, had 32 percent higher payroll growth, and had 28 percent higher employment growth.

**Table 3: ATET Estimates**

	Annual (2020)			2HFY (Q2 & Q3, 2020)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Payroll Growth	Revenue Growth	Active	Payroll Growth	Employment Growth
$D_j$ : Option to Hire in 2020	0.020** (0.009)	0.238*** (0.031)	0.062** (0.027)	0.035*** (0.013)	0.317*** (0.035)	0.281*** (0.033)
Observations (Firms)	4,000	4,000	2,700	3,700	3,600	3,400
Control Group Mean (2019 levels)	0.971	\$2,907k	\$12,740k	0.938	\$524k	33.76

NOTE: Heteroskedasticity robust standard errors in parentheses. 2HFY stands for “second half of the fiscal year.” 2HFY employment is defined as the average of calendar Q2 and Q3 employment. 2HFY outcomes are only available for single-unit firms. Firm counts rounded according to Census Bureau disclosure avoidance rules. The samples used in each column are the same as in Table 1. In columns (2), (3), (5), and (6), growth rates refer specifically to Davis-Haltiwanger-Schuh (DHS) growth rates calculated relative to 2019, which accommodates zeroes in 2020 values of the underlying variable due to exit (see Section 4.2).

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

<sup>14</sup>Recall from Section 2 that USCIS approvals were made before full awareness of the pandemic, but final hiring decisions were likely made after.

<sup>15</sup>Recall that lottery group assignment was made at the consolidated form level.

<sup>16</sup>Appendix Section C.2 provides balance test results consistent with this assumption.

These results are particularly economically significant given that H-2B employers are already positively selected and given the policy context in 2020 more generally. The availability of H-2B workers increased business survival in 2020 by roughly the same amount or more than a PPP loan did, at a much smaller fraction of the cost, and increased employment at the firm by more than PPP loans (Hubbard and Strain, 2020; Autor et al., 2022; Gorbachev et al., 2025).<sup>17</sup>

#### 4.4 Spillovers Analysis

Given the magnitudes found above and the economic environment that accompanied the COVID-19 pandemic, the final set of empirical analyses aims to assess the effects on non-lottery-participant firms, which did not consider applying for H-2B workers in 2020. On one hand, H-2B workers and their employers may have been vital to sustaining supply chains and peer-to-peer business during the otherwise supply-constrained environment presented by the COVID-19 pandemic. On the other, consumer demand was also heavily rationed during this time period, and so the ability of a lottery-winning firm to stay open and sustain business may come at the direct cost of competitors.

To assess this empirically, I estimate the following model:

$$f(y_{j',2020}) = \alpha + \beta[\text{Prop. A or B Apps}_{\ell(j'),2020}] + \gamma X_{j'} + \varepsilon_{j'}, \quad (2)$$

where  $j'$  indexes a non-participating firm, and  $\text{Prop. A or B Apps}_{\ell(j'),2020}$  indexes the proportion of applications in  $j'$ 's county (location) that were assigned to lottery groups A or B.<sup>18</sup> Variation in  $\text{Prop. A or B Apps}_{\ell(j'),2020}$  arises because 1) the H-2B program is relatively small, and 2) lottery groups are assigned at the consolidated form rather than application level. Indeed, Appendix Section C.4 shows a large degree of variance in  $\text{Prop. A or B Apps}_{\ell(j'),2020}$  across the 900 U.S. counties that had a lottery participant.

Table 4 presents estimates from Equation (2), with several notable results. Across several measures, firms that did not participate in the 2020 H-2B lottery performed better when they were exposed to more H-2B workers in their county. As expected, magnitudes are not as large as for participants themselves but are nonetheless non-trivial. For example, firms that operated in counties where all H-2B lottery participants were assigned to lottery groups A or B featured 2 percent higher revenue growth in 2020 relative to firms in counties where all H-2B lottery participants were assigned to lottery groups C, D, or E (Column 3).

These results reflect that the local importance of the highly selected group of H-2B employers may extend beyond consumers to peer businesses as well. Given that a similar esti-

<sup>17</sup>Note that these benchmarks are meant to guide readers on magnitudes but do not represent direct, apples-to-apples comparisons. Gorbachev et al. (2025) measure survival using customer visits while this paper measures it through disbursed payroll. The employment effects here include the H-2B workers themselves, which might explain why results are larger than in Autor et al. (2022).

<sup>18</sup>Multi-unit firms are assigned counties based on their county of highest payroll disbursement in the LBD.

**Table 4: Spillovers onto Non-Participant Firms**

	Annual (2020)			2HFY (Q2–Q3, 2020)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Payroll Growth	Revenue Growth	Active	Payroll Growth	Employment Growth
Prop. A or B Apps $_{\ell(j'),2020}$	0.003 (0.002)	0.027** (0.013)	0.021** (0.009)	0.009* (0.005)	0.043* (0.023)	0.036* (0.020)
Observations (Firms)	5,210,000	4,780,000	3,034,000	5,114,000	4,286,000	4,017,000
Clusters (Counties)	900	900	900	900	900	900

NOTE: See Equation (2) for specification.  $\ell(j')$  refers to a non-participant firm  $j'$ 's county of primary operation. Standard errors, clustered at the county level, in parentheses. 2HFY stands for “second half of the fiscal year.” 2HFY employment is defined as the average of calendar Q2 and Q3 employment. 2HFY outcomes are only available for single-unit firms. Firm and county counts rounded according to Census Bureau disclosure avoidance rules. The sample in Column (1) is the set of all non-participant firms with non-missing payroll information in 2019. The sample in Column (2) is the set of all non-participant firms with non-missing, positive payroll in 2019. The sample in Column (3) is the set of all non-participant firms with non-missing positive, revenues in 2019. The sample in Column (4) is the set of all single-unit, non-participant firms with non-missing payroll in 2019. The sample in Column (5) is the set of all single-unit, non-participant firms with non-missing, positive payroll in 2019. The sample in Column (6) is the set of all single-unit, non-participant firms with non-missing, positive employment in 2019. In columns (2), (3), (5), and (6), growth rates refer specifically to Davis-Haltiwanger-Schuh (DHS) growth rates calculated relative to 2019, which accommodates zeroes in 2020 values of the underlying variable due to exit (see Section 4.2).

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

mation strategy in Amuedo-Dorantes et al. (2023) did *not* find evidence of spillover effects onto non-participants, the results in Table 4 also broach the notion that peer-to-peer business networks were especially vital in the supply-constrained environment of 2020.<sup>19</sup>

## 4.5 Additional Analyses

Several additional analyses and figures in the Online Appendix add texture to, but do not alter, the main thrust of the results presented above. Table C1 shows that adding control variables does not alter results, Table C2 shows that results on binary outcome variables are qualitatively unaltered when using logit estimation, and Table C3 shows that outcomes are balanced on the alternate exposure variable used for the ATET analysis, conditional on fixed effects for the number of consolidated forms filed. Figure C1 shows that there is a substantial degree of variation in the proportion of applications assigned to different lottery groups across counties, enabling the precision seen on estimates in Table 4 above. Table C5 shows that H-2B employers and workers are heavily concentrated in local services.

Finally, a natural question to ask is whether H-2B hiring crowded out opportunities for incumbent workers at this particularly vulnerable time in the labor market. Appendix Section C.3 attempts to address this question in multiple ways but is subject to caveats that preclude a definitive answer. Previous studies on U.S. employer-based visas have assessed this question by estimating the number of additional employees at the firm per i129 approval (e.g., Kerr et al., 2015; Doran et al., 2022; Amuedo-Dorantes et al., 2023; Mahajan et al., 2024). These

<sup>19</sup> An alternative, complementary explanation stems from consumer demand by H-2B workers themselves. However, the overall count of H-2B workers is relatively small, and these workers are paid relatively low wages. In contrast, the firms that they work for tend to be large and often produce intermediate goods. Thus, supply-side explanations are likely more operative here.

studies tend to use one employee per i129 approval as a benchmark because it indicates no crowd-in or crowd-out, assuming the i129 approval converts into an eventual, measured hire of the visa worker.<sup>20</sup> Appendix Section C.3.1 presents the results of such an analysis and shows that each i129 approval for an H-2B worker in 2HFY 2020 led to 0.36 additional employees (with standard error of 0.04).

However, the unique timing of the 2020 H-2B lottery, along with the lack of data availability on H-2B *hiring* (as opposed to i129 approval), generates a caveat to the interpretation of such an estimate that is particular to this paper. For 2HFY 2020, H-2B i129 petitions were approved before the pandemic, yet hiring decisions were made after. Thus, whereas in previous years, roughly 80 percent of i129 H-2B approvals converted into hires ([Amuedo-Dorantes et al., 2023](#)), this number was likely much lower in 2020. In other words, a standard benchmark of 1 (or even a more precise benchmark of 0.8) is not useful here. Instead, I can at most conclude that H-2B hiring did not generate one-for-one crowd-out of other workers, on net, because I can definitively rule out that there were 0 additional employees per i129 approval. Appendix Section C.3.2 includes a supplemental analysis based on jobs reported on PPP loan applications but is similarly inconclusive.

## 5 Conclusion

This study provides the first well-identified evidence that foreign-born workers were essential to business operations during the COVID-19 pandemic. Leveraging the randomized 2020 H-2B visa lottery, it finds that firms with access to foreign-born workers who could start on April 1, 2020, experienced decreased business closures, increased revenues, increased payroll, and increased employment during 2020. Furthermore, these effects extended beyond H-2B employers, with evidence of spillovers to non-participant firms in the same county. These findings underscore the critical role of migrant labor in maintaining business continuity during acute crises and suggest that flexible immigration policies may serve as cost-effective interventions when rapid responses are needed to safeguard public health and economic welfare.

---

<sup>20</sup>In the data here and in all of these previous works, the visa workers are included in firm employment counts. Thus an employee increase of one per visa hire reflects no net crowd-in or crowd-out.



## References

- Amior, Michael and Jan Stuhler**, “Immigration, Monopsony and the Distribution of Firm Pay,” IZA Discussion Papers 16692, Institute of Labor Economics (IZA) December 2023. (Cited on p. 3)
- Amuedo-Dorantes, Catalina, Esther Arenas-Arroyo, Parag Mahajan, and Bernhard Schmidpeter**, “Low-Wage Jobs, Foreign-Born Workers, and Firm Performance,” IZA Discussion Papers 16438, Institute of Labor Economics (IZA) September 2023. (Cited on p. 3, 4, 5, 9, 12, 13, 17, 23)
- Arellano-Bover, Jaime and Shmuel San**, “The Role of Firms and Job Mobility in the Assimilation of Immigrants: Former Soviet Union Jews in Israel 1990–2019,” IZA Discussion Papers 16389, Institute of Labor Economics (IZA) August 2023. (Cited on p. 3)
- Atalay, Enghin, Shigeru Fujita, Sreyas Mahadevan, Ryan Michaels, and Tal Roded**, “Reopening the Economy: What Are the Risks, and What Have States Done?,” *Research Brief*, July 2020. (Cited on p. 2)
- Autor, David, David Cho, Leland D. Crane, Mita Goldar, Byron Lutz, Joshua Montes, William B. Peterman, David Ratner, Daniel Villar, and Ahu Yildirmaz**, “An Evaluation of the Paycheck Protection Program Using Administrative Payroll Microdata,” *Journal of Public Economics*, 2022, 211, 104664. (Cited on p. 11)
- Ayyagari, Meghana, Yuxi Cheng, and Ariel Weinberger**, “Firm Resiliency: The Role of Spillovers,” *Journal of Financial and Quantitative Analysis*, forth. (Cited on p. 3)
- Beerli, Andreas, Jan Ruffner, Michael Siegenthaler, and Giovanni Peri**, “The Abolition of Immigration Restrictions and the Performance of Firms and Workers: Evidence from Switzerland,” *American Economic Review*, 2021. (Cited on p. 3)
- Belloni, Alexandre, Victor Chernozhukov, and Christian Hansen**, “Inference on Treatment Effects after Selection among High-Dimensional Controls,” *Review of Economic Studies*, 11 2013, 81 (2), 608–650. (Cited on p. 20)
- Borjas, George J. and Hugh Cassidy**, “The Fall and Rise of Immigrant Employment During the COVID-19 Pandemic,” in Solomon W. Polachek and Konstantinos Tatsiramos, eds., *50th Celebratory Volume*, Vol. 50 of *Research in Labor Economics*, Leeds: Emerald Publishing Limited, 2023, pp. 327–367. (Cited on p. 3)
- Brinatti, Agostina and Nicolas Morales**, “Firm Heterogeneity and the Impact of Immigration: Evidence from German Establishments,” 2021. Working Paper. (Cited on p. 3)
- Chow, Melissa C, Teresa C Fort, Christopher Goetz, Nathan Goldschlag, James Lawrence, Elisabeth Ruth Perlman, Martha Stinson, and T. Kirk White**, “Redesigning the Longitudinal Business Database,” Working Paper 28839, National Bureau of Economic Research May 2021. (Cited on p. 6)
- Clemens, Michael A. and Ethan G. Lewis**, “The Effect of Low-Skill Immigration Restrictions on US Firms and Workers: Evidence from a Randomized Lottery,” NBER Working Paper 30589, NBER 2024. (Cited on p. 3, 4)
- Delgado-Prieto, Lukas**, “Worker Responses to Immigration across Firms: Evidence from Colombia,” Technical Report 2025. (Cited on p. 3)
- Dimmock, Stephen G., Jiekun Huang, and Scott J. Weisbenner**, “Give Me Your Tired, Your Poor, Your High-Skilled Labor: H-1B Lottery Outcomes and Entrepreneurial Success,” *Management Science*, 2021. (Cited on p. 3)



- Doran, Kirk, Alexander Gelber, and Adam Isen**, “The Effects of High-Skilled Immigration Policy on Firms: Evidence from Visa Lotteries,” *Journal of Political Economy*, 2022, 130 (10), 2501–2533. (Cited on p. 3, 12, 23)
- Dustmann, Christian and Albrecht Glitz**, “How Do Industries and Firms Respond to Changes in Local Labor Supply?,” *Journal of Labor Economics*, 2015, 33 (3), 711–750. (Cited on p. 3)
- Egger, Dennis, Daniel Auer, and Johannes Kunz**, “Effects of Migrant Networks on Labor Market Integration, Local Firms and Employees,” *Working Paper*, 2022. (Cited on p. 3)
- Glennon, Britta**, “How Do Restrictions on High-Skilled Immigration Affect Offshoring? Evidence from the H-1B Program,” *Management Science*, 2024, 70 (2), 907–930. (Cited on p. 3)
- Gorbachev, Olga, Maria Luengo-Prado, and J. Christina Wang**, “Effects of the Paycheck Protection Program on Small Businesses’ Financial Health and Real Activity,” 2025. Working Paper. (Cited on p. 11)
- Gray, Rowena, Giulia Montresor, and Greg C. Wright**, “Processing immigration shocks: Firm responses on the innovation margin,” *Journal of International Economics*, 2020, 126, 103345. (Cited on p. 3)
- Hubbard, R. Glenn and Michael R Strain**, “Has the Paycheck Protection Program Succeeded?,” Working Paper 28032, National Bureau of Economic Research October 2020. (Cited on p. 11)
- Kerr, Sari Pekkala, William R. Kerr, and William F. Lincoln**, “Skilled Immigration and the Employment Structures of US Firms,” *Journal of Labor Economics*, 2015, 33 (S1), S147–S186. (Cited on p. 3, 12, 23)
- Levine, Ross, Chen Lin, and Wensi Xie**, “Local Financial Structure and the Pandemic’s Effect on the Distribution of Employment,” *Review of Corporate Finance Studies*, 05 2024, p. cfae008. (Cited on p. 3)
- Mahajan, Parag**, “Immigration and Business Dynamics: Evidence from U.S. Firms,” *Journal of the European Economic Association*, 2024. (Cited on p. 4)
- , **Nicolas Morales, Kevin Y. Shih, Mingyu Chen, and Agostina Brinatti**, “The Impact of Immigration on Firms and Workers: Insights from the H-1B Lottery,” IZA Discussion Papers 16917, Institute of Labor Economics (IZA) April 2024. (Cited on p. 3, 12, 23)
- Mandelman, Federico, Mishita Mehra, and Hewei Shen**, “Skilled Immigration Frictions as a Barrier for Young Firms,” *Working Paper*, 2024. (Cited on p. 3)
- Marchal, Léa, Guzmán Ourens, and Giulia Sabbadini**, “When Immigrants Meet Exporters: A Re-assessment of the Immigrant Wage Gap,” CESifo Working Paper Series 10092, CESifo 2022. (Cited on p. 3)
- Mitaritonna, Cristina, Gianluca Orefice, and Giovanni Peri**, “Immigrants and Firms’ Outcomes: Evidence from France,” *European Economic Review*, 2017, 96, 62 – 82. (Cited on p. 3)
- Morales, Nicolas**, “High-Skill Migration, Multinational Companies, and the Location of Economic Activity,” *Review of Economics and Statistics*, 2023. (Cited on p. 3)
- Muñoz, Mathilde**, “Trading Nontradables: The Implications of Europe’s Job-Posting Policy,” *Quarterly Journal of Economics*, 06 2023, 139 (1), 235–304. (Cited on p. 3)

- Orefice, Gianluca and Giovanni Peri**, “Immigration and Worker-Firm Matching,” *Review of Economics and Statistics*, 08 2024, pp. 1–45. (Cited on p. [3](#))
- Peri, Giovanni and Reem Zaiour**, “Changes in International Immigration and Internal Native Mobility after COVID-19 in the USA,” *Journal of Population Economics*, October 2023, 36 (4), 2389–2428. (Cited on p. [3](#))
- Signorelli, Sara**, “Do Skilled Migrants Compete with Native Workers?,” *Journal of Human Resources*, 2024. (Cited on p. [3](#))
- Silliman, Mikko and Alexander L.P. Willén**, “Worker Power, Immigrant Sorting, and Firm Dynamics,” CESifo Working Paper Series 11281, CESifo 2024. (Cited on p. [3](#))
- Wasi, N. and A. Flaaen**, “Record Linkage Using Stata: Preprocessing, Linking, and Reviewing Utilities,” *Stata Journal*, 2015, 15 (3), 672–697(26). (Cited on p. [17](#))

# Appendix

## A Data

### A.1 Linking Records across Government Agencies

Note that the match procedure and description below are the same as described in [Amuedo-Dorantes et al. \(2023\)](#).

The procedure is necessitated by a lack of common firm identifiers across the H-2B and Census data. The key variables contained in the H-2B data are employer name, employer state, and employer city, and employer ZIP code. I include i129 petitions and TLC applications as separate observations so that they can both be linked to a common firm identifier in the Census data. Using pre-processing commands described in [Wasi and Flaaen \(2015\)](#) along with some additional corrections of common mistakes, I clean the names of employers. I then collapse the dataset to the name-state-city-ZIP level.

On the Census side, I link the LBD to the CBPBR using unique, within-year establishment identifiers. The CBPBR also contains name, state, city, and ZIP information for employers. Notably, it includes two name fields and both mailing and physical address for the establishment. Because visa applications are filled out by employers, they may use either the physical or mailing address on their form. I therefore reshape the LBD-CBPBR dataset to have a unique observation for each employer's address. I perform the same pre-processing commands and collapse to the *lbdnum*-name-state-city-ZIP level. *lbdnum* is the longitudinal, unique, establishment-level identifier that enables all of the analyses in this paper.

The match proceeds in 6 steps, looping over states (implicitly requiring a match on state), using the `reclink2` command ([Wasi and Flaaen, 2015](#)):

1. Exact matching on all four variables.
2. Exact match on ZIP, fuzzy match on employer name1 and city, with more emphasis on name.
3. Exact match on ZIP, fuzzy match on employer name1 and city, with slightly less emphasis on the name and a higher match score requirement.
4. Fuzzy match on ZIP, employer name1, and city, with an even higher match score requirement.
5. Repeat steps 2–4 with employer name2.

Table A1 presents bivariate regressions of two binary variables, at the DoL H-2B TLC consolidated form level: an exposure variable that indicates whether a consolidated form was assigned to lottery group A or B, and an outcome variable that indicates whether a given consolidated form was matched to a Census employer. Column (1) is unweighted, while Column (2) is frequency weighted by the number of applications represented in a given consolidated form, replicating an application level regression.

**Table A1: Match Checks**

	$\mathbb{1}\{\text{Matched}\}$	
	(1)	(2)
$\mathbb{1}\{\text{A or B}\}$	0.006 (0.007)	0.002 (0.002)
Observation Level	Consolidated Form	Application
Observations	5,667	99,491
Dependent Variable Mean (Match Rate)	0.931	0.944

NOTE: Unit of observation is a consolidated TLC form when frequency weights are not used. Unit of observation is an individual TLC application with frequency weights are used (the frequency weights are the number of applications on a given consolidated form).

## A.2 Consistency of Firm Definition

Changes in the firm identifier, *firmed*, can occur in the LBD even without changes to underlying economic activity. For example, after a merger, establishments from an original company may continue operating under a new *firmed*. Thus, naively tracking outcomes at the *firmed* level could lead to false “deaths.” Given that firm survival and operation is a critical outcome in this paper, I took care to ensure a consistent definition of the firm over time.

To do this, I proceeded in two steps. First, the fuzzy H-2B to Census match generated a link between H-2B applications and specific LBD *establishments*, given that they were based on physical location (e.g., state, ZIP, address). I then assigned the linked establishment to a parent firm based on their *firmed* in 2019. In the few cases that an establishment did not have a valid *firmed* in the 2019 LBD, I assigned *firmed* from the LBD in other years, in the following order of priority: 2020, 2018, 2017, 2016, 2015, 2014, 2021. At this stage, each H-2B consolidated form maps to a specific *firmed* from a specific LBD year. I then define a firm as the set of establishments operating under that *firmed* from that specific LBD year. Then, that firm’s outcomes in other years are based on that same, consistent set of establishments. To be clear, in the vast majority of cases, this means that a firm is based on the set of establishments under a given *firmed* in operation as of 2019. Then, in 2020, a firm “shuts down operation” if all of those establishments cease activity. If those establishments continue operating under a different *firmed* in 2020, the firm is still considered “in operation.” Payroll and employment are simply measured as the sum of payroll and employment across this consistent set of establishments.

The cost of the above approach is that true acquisitions or losses of establishments by firms do not factor into results. Results estimated on *firmeds* directly are therefore also available upon request.

Three additional notes merit mention. In the spillovers analysis, non-H-2B participant firms are similarly tracked based on the set of establishments that worked under a given *firmed* in 2019 (since there is no preceding match step). Second and perhaps most importantly, this entire procedure ensures that single-unit firms are tracked based on their unique establishment identifier, which is of a true panel nature in the LBD, even if their *firmed* changes. Finally, this consistent *firmed* definition is not possible for the revenues outcome, which is measured at the *firmed* level each year (not at the establishment level).

## B Randomization Inference Procedure

I run  $S = 1,000$  simulations to construct randomization inference  $p$ -values. For each simulation  $s$ , I follow these steps:

1. Randomly re-assign lottery groups to consolidated TLC forms based on true proportions that were in groups  $\{A, B, C, D, E\}$
2. Collapse to firm level to generate placebo version of exposure: Prop. A or B Apps $_{j,2020}^s$
3. Re-estimate Equation (1), replacing Prop. A or B Apps $_{j,2020}$  with Prop. A or B Apps $_{j,2020}^s$ , then store resulting coefficient estimate  $\hat{\beta}^s$

The randomization inference  $p$ -value is then

$$\frac{\sum_s \mathbb{1}\{|\hat{\beta}^s| > |\hat{\beta}|\}}{S},$$

where the absolute values are in place to mimic two-sided testing.

## C Supplemental Figures and Tables

### C.1 Robustness

#### C.1.1 Post-Double Selection LASSO Controls

Here, I present the main reduced form results from Table 2 alongside the same results that include additional control variables, selected using a post-double selection LASSO procedure (Belloni et al., 2013). The candidate control variables are Census division fixed effects, NAICS sector fixed effects, a dummy variable indicator prior usage of the H-2B visa program, size quartile fixed effects based on the number of lottery applications filed, and size bin quartile fixed effects based on 2019 Q1 employment. As expected with a random exposure variable, results are often more precise, but the increase in precision is generally negligible qualitatively. Further, there are no meaningful changes in magnitudes.

**Table C1: 2020 Reduced Form Results With and Without Controls**

	Annual (2020)			2HFY (Q2–Q3, 2020)		
	(1) Active	(2) Payroll Growth	(3) Revenue Growth	(4) Active	(5) Payroll Growth	(6) Employment Growth
<b>Panel A: Without Controls</b>						
Prop. A or B Apps <sub><i>j</i>,2020</sub>	0.011** (0.005) [0.019]	0.136*** (0.017) [<0.001]	0.042** (0.017) [0.005]	0.018*** (0.007) [0.009]	0.177*** (0.020) [<0.001]	0.156*** (0.019) [<0.001]
<b>Panel B: With Controls</b>						
Prop. A or B Apps <sub><i>j</i>,2020</sub>	0.010** (0.005) [0.034]	0.133*** (0.016) [<0.001]	0.051*** (0.015) [<0.001]	0.017** (0.007) [0.010]	0.177*** (0.018) [<0.001]	0.157*** (0.017) [<0.001]
Observations (Firms)	4,000	4,000	2,700	3,700	3,600	3,400
Control Group Mean (levels)	0.971	\$2,907k	\$12,740k	0.938	\$524k	33.76

NOTE: See Equation (1) for specification. Heteroskedasticity robust standard errors in parentheses. Randomization inference  $p$  values in square brackets. Where applicable Davis-Haltiwanger-Schuh (DHS) growth rates are calculated relative to 2019. 2HFY stands for “second half of the fiscal year.” 2HFY employment is defined as the average of calendar Q2 and Q3 employment. 2HFY outcomes are only available for single-unit firms. Firm counts rounded according to Census Bureau disclosure avoidance rules.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

### C.1.2 Estimation with Logit for Binary Outcomes

Given the degree to which H-2B users are positively selected, the binary outcome of whether or not a firm remains active has a low rate of failure. This broaches the concern that the linearity assumption inherent in OLS estimation with these outcomes may be problematic. Table C2 re-estimates models for these outcomes using logit, presenting average partial effects side-by-side with the original linear probability model (LPM) results from Table 2. Results are highly robust to this alternate specification.

**Table C2: 2020 Reduced Form Extensive Margin Results**

Prop. A or B Apps <sub><i>j</i>,2020</sub>	Outcome: Active			
	0.0112** (0.0049) [0.019]	0.0111** (0.0049) [0.025]	0.0183*** (0.0070) [0.009]	0.0180*** (0.0069) [0.009]
Estimation	OLS (LPM)	Logit	OLS (LPM)	Logit
Observation Period	Annual	Annual	2HFY (Q2–Q3)	2HFY (Q2–Q3)
Observations (Firms)	4,000	4,000	3,700	3,700
Control Group Mean (Levels)	0.971	0.971	0.938	0.938

NOTE: See Equation (1) for specification. Heteroskedasticity robust standard errors in parentheses. Randomization inference  $p$  values in square brackets. LPM stands for “linear probability model.” When estimation is conducted using a Logit model, the results are reported as average partial effects. 2HFY stands for “second half of the fiscal year.” Firm counts rounded according to Census Bureau disclosure avoidance rules.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## C.2 Balance: Binary Treatment

The ATET calculations presented in Section 4.3 rely on the randomness of  $Z_j$ , an indicator for assignment of *any* consolidated forms to lottery groups A or B, conditional on the number of forms submitted. Table C3 presents the results of balance tests consistent with this notion, estimated using the following model:

$$f(y_{j,2019}) = \beta[Z_{j,2020}] + \alpha_{f(j)} + \varepsilon_j. \quad (3)$$

Here,  $f(j)$  is the number of consolidated forms filled out by firm  $j$  in the 2020 H-2B visa lottery, and  $\alpha_{f(j)}$  are fixed effects for each level of  $f(j)$  observed.

**Table C3: Balance Tests with Binary Treatment Assignment**

	Annual (2019)			2HFY (Q2–Q3, 2019)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Log Payroll	Log Revenues	Active	Log Payroll	Log Employment
$Z_j$ : Any A or B Apps $_{j,2020}$	-0.001 (0.005) [0.717]	0.041 (0.048) [0.378]	-0.005 (0.048) [0.923]	0.001 (0.006) [0.944]	0.022 (0.043) [0.583]	0.002 (0.039) [0.951]
Observations (Firms)	4,000	4,000	2,700	3,700	3,600	3,400
Control Group Mean (Levels)	0.971	\$2,907k	\$12,740k	0.938	\$524k	33.76

NOTE: See Equation (3) for specification. Heteroskedasticity robust standard errors in parentheses. Randomization inference  $p$  values in square brackets. Control group defined as firms with no A or B applications. 2HFY stands for “second half of the fiscal year.” 2HFY employment is defined as the average of calendar Q2 and Q3 employment. 2HFY outcomes are only available for single-unit firms. Firm counts rounded according to Census Bureau disclosure avoidance rules.



## C.3 Assessing Crowd-Out

### C.3.1 Employees per H-2B Approval

In prior studies of firm-level migrant hiring, a key question relates to incumbent (native) crowd-out (Kerr et al., 2015; Doran et al., 2022; Amuedo-Dorantes et al., 2023; Mahajan et al., 2024). Given that I lack data on employment *by nativity*, the standard way to approach this question here would be a regression of the following form:

$$\Delta 2\text{HFY Employment}_{j,2020} = \alpha + \beta^{IV}[\text{i129 Approvals}_{j,2020}] + \varepsilon_j, \quad (4)$$

where *Prop. A or B Apps*<sub>*j*,2020</sub> is used as an instrument for *i129 Approvals*<sub>*j*,2020</sub>.  $\beta^{IV}$  measures additional employees per i129 approval. Under the assumption that each i129 approval converts to an *actual hire*, deviation of  $\beta^{IV}$  from one indicates that employment of non-visa workers is being crowded out (since the Census employment counts include visa workers). This assumption is slightly problematic in normal times and highly problematic given the specific case of the 2020 H-2B visa lottery. DHS reports indicate that the i129 approval to visa issuance rate is closer to 80% (rather than 100%) in the years leading up to 2018.<sup>21</sup> More importantly, in 2020, H-2B i129 petitions were approved before the pandemic was truly impacting economic activity, yet visa issuance and hiring decisions were made after. Thus, it is unfortunately unclear which benchmark to use for  $\beta^{IV}$  to assess whether there is crowd out of non-H-2B workers.

I nonetheless estimate Equation (4), dividing both the outcome and exposure by 2019 employment in order to limit skewness in the outcome while retaining the standard interpretation of the IV coefficient. I find that each i129 approval led to 0.36 additional employees, with a standard error of 0.04 and a first-stage *F* statistic of 344. Due to the lack of a benchmark, the only real conclusion that can be drawn from this result is that a non-trivial portion of lottery winners did actually convert their i129s into hires.

### C.3.2 Alternate Analysis Using PPP Loan Application Data

A second potential approach to answering the crowd-out question comes from the relatively ubiquitous use of PPP loans during this time period. Employers that applied for PPP loans were required to list the number of jobs on their applications because loan amounts varied by the number of jobs. Critically, guidelines issued at the time suggested that foreign-born workers—whose primary residence was not in the U.S.—should be excluded from job counts.<sup>22</sup> Thus, H-2B employers' PPP loan applications should contain counts of non-H-2B jobs.

Data on PPP loan applications are available from the Small Business Administration, and I use a fuzzy matching procedure to match them directly to H-2B DoL 2020 lottery applications.<sup>23,24</sup> I then estimate the following specification:

$$\log(2\text{HFY PPP Jobs}_{j,2020}) = \alpha + \beta[\text{Prop. A or B Apps}_{j,2020}] + \varepsilon_j \quad (5)$$

on the sub-set of H-2B employers that I was able to match to PPP loan applications. *2HFY PPP Jobs*<sub>*j*,2020</sub> is the number of jobs reported on PPP loan applications that were sent in by a given firm between March

<sup>21</sup> See this DHS Report: [H-2B Usage and Recommendations](#).

<sup>22</sup> See, e.g., [here](#), for specific mention that H-2B workers should not be counted on applications.

<sup>23</sup> [SBA PPP loan data](#): <https://www.sba.gov/funding-programs/loans/covid-19-relief-options/paycheck-protection-program/ppp-data>.

<sup>24</sup> This process circumvents the need to use restricted-access data from the U.S. Census Bureau—all data is publicly available. However, because I do not have access to *firmid* in either the H-2B or PPP data, I define an employer based on name, address, location, and industry. Employers are hand-combined across locations when they are obviously part of the same firm (e.g., Brightview Landscaping).

and September of 2020.<sup>25</sup> I find that 62% of H-2B employers can be matched to a PPP loan application and 52% of H-2B employers filed a PPP loan application in 2HFY 2020.

**Table C4: Take-Up of PPP Loans and Job Counts Reported on PPP Loans**

	(1)	(2)
	$\mathbb{1}\{\text{Any PPP}\}$	Log Jobs Reported
Prop. A or B Apps <sub>j,2020</sub>	0.005 (0.015)	0.113** (0.047)
Observations (Firms)	4,633	2,430
Control Group Mean (Levels)	0.621	20.54

NOTE: Heteroskedasticity robust standard errors in parentheses. Control group defined as firms with no A or B applications.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table C4 shows in Column (1) that use of the PPP loan program is uncorrelated with the lottery (*Prop. A or B Apps<sub>j,2020</sub>*). Column (2), meanwhile, shows that PPP jobs *rose* among lottery winners. To the letter of the law, these results imply crowd-in of non-H-2B workers.

However, there are good reasons to interpret these results with caution. There was substantial confusion among firms around whether H-2B workers should be counted on PPP loan applications, particularly when the program first started.<sup>26</sup> Thus, applications in 2HFY 2020 may have included H-2B workers in job counts even if they were not supposed to. Finally, PPP loans were only available to firms with fewer than 500 employees, whereas no such restriction exists for the H-2B visa.

Thus, in conjunction with Section C.3.1, I believe the appropriately cautious conclusion to draw from these analyses is that H-2B workers did not *fully* crowd-out non-H-2B workers from employment during 2HFY 2020.

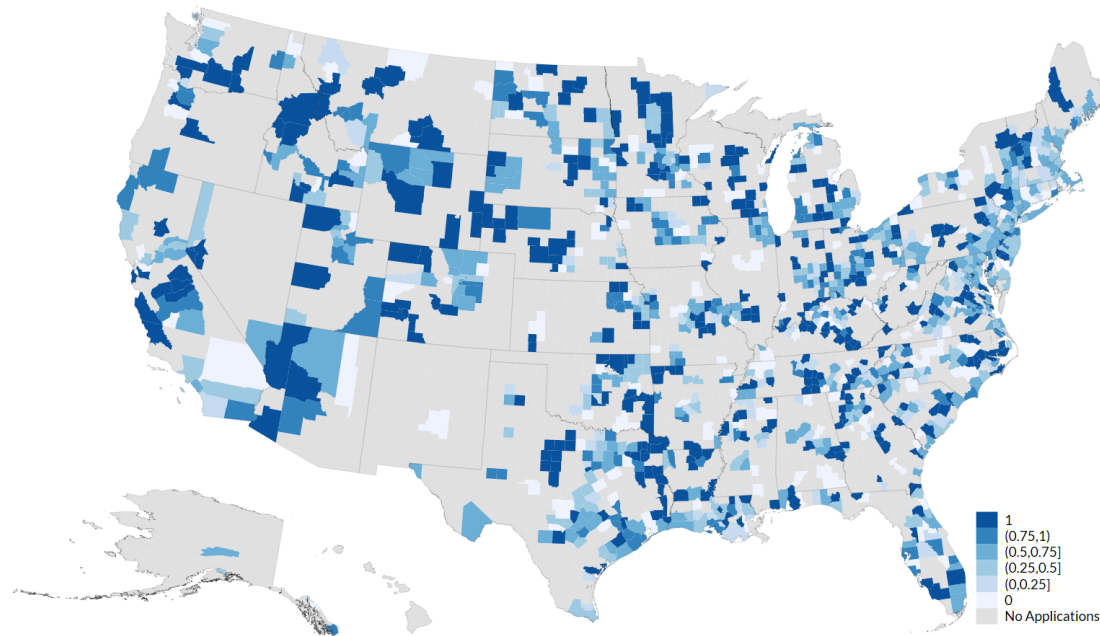
<sup>25</sup>In three cases, an employer filed more than one PPP loan application during 2HFY 2020. *2HFY PPP Jobs<sub>j,2020</sub>* is then the mean of the job count across these applications.

<sup>26</sup>This is evidenced by the fact that the SBA, USDA, and other organizations had to issue clarifications regarding whether H-2B workers should count toward job totals. Some firms may have considered their H-2B workers to have primary residences in the United States, even if they are temporary employees, and may have therefore nonetheless included them in job counts on their PPP applications.

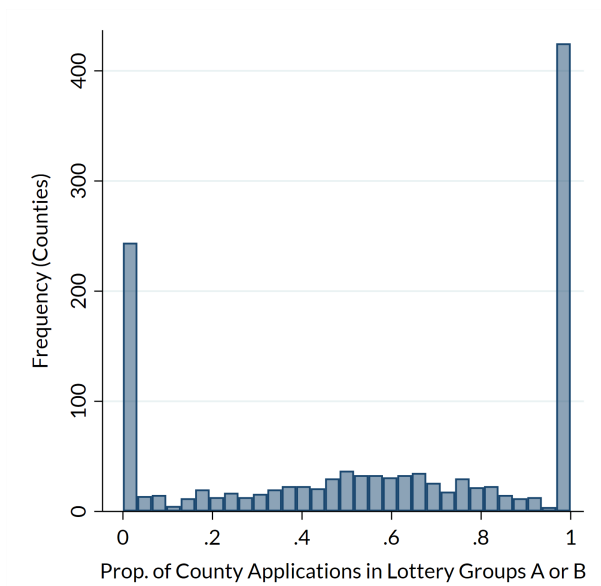
## C.4 Variation for Spillovers Analysis

**Figure C1:** Lottery Variation at Local Level

A: *Prop. A or B Apps<sub>ℓ,2020</sub>* by County



B: Histogram of *Prop. A or B Apps<sub>ℓ,2020</sub>*



**Source:** Department of Labor Office of Foreign Labor Certification Performance Data.

## C.5 Industries and Occupations of 2020 H-2B Lottery Workers

**Table C5:** Top Five Industries and Occupations on 2020 Lottery Applications

<b>A: Industry (NAICS)</b>		<b>Percent of Applications</b>
1.	Landscaping Services (56173)	52.5%
2.	Hotels & Motels (72111)	9.2%
3.	“Other” Amusement & Recreation (71399)	3.7%
4.	Janitorial Services (56172)	2.4%
5.	Support Activities for Forestry (11531)	2.2%
<b>B: Occupation (SOC)</b>		<b>Percent of Applications</b>
1.	Landscaping & Groundskeeping (37-3011)	54.5%
2.	Maids & Housekeeping (37-2012)	7.4%
3.	Amusement & Recreation Attendants (39-3091)	5.1%
4.	Construction Laborers (47-2061)	3.4%
5.	Forest & Conservation Workers (45-4011)	2.4%

**Source:** Department of Labor Office of Foreign Labor Certification Performance Data.