Migrant Workers and Economic Resilience: Evidence from the 2020 H-2B Visa Lottery*

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Abstract

I study how access to foreign-born workers impacts firms and local economies during periods of widespread disruption. 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. Drawing on several sources of administrative data, I find that access to H-2B workers led to decreased business closures, increased revenues, increased payroll, and increased employment in 2020, among lottery participants. A recentered IV approach further shows that gains extended to firms that did not hire H-2B workers, likely via downstream supply chain linkages.

JEL Codes: J23, F22, J61, L14

Key Words: migrant workers, H-2B, COVID-19, firm dynamics, supply chain resiliency

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1 Introduction

How "essential" are foreign-born workers to local economic resilience? The COVID-19 pandemic brought this question to the fore, yet policymakers entered 2020 with little quantitative evidence on the degree to which firms and local economies rely on low-wage, migrant workers. This uncertainty clouded policy decisions at the time, as reflected in the tension between closing off the border to prevent the spread of the virus and allowing migrant workers into the country to perform tasks deemed vital to the economy.[1] Understanding the degree to which migrant workers sustain economic activity is critical for designing labor market and immigration policies that can withstand future shocks.

In this paper, I provide 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) 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[2]—the opportunity to hire foreign-born workers starting on April 1, 2020. This unique combination of data and experimental variation allows me to quantify both the direct effects of H-2B access on lottery applicants and spillover effects on non-applicant firms along local supply chains. I find that temporary migrant workers can help stabilize outcomes of both their employers and those of downstream businesses that depend on them.

I conduct several analyses to draw this conclusion. First, I show 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.

I then show that these effects spilled over to firms that did not participate in the H-2B program, leveraging the fact that the H-2B visa lottery also creates random variation in exposure to H-2B workers at the local level. I first show that non-participating firms in counties with more H-2B workers experienced lower closure rates, higher revenues, higher payrolls,

[1] 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.

[2] Based on classifications developed in Atalay et al. (2020).

and higher employment than peers in less-exposed counties. These effects were concentrated among firms that were not themselves operating in the same industry as H-2B employers. Then, I combine information from the Bureau of Economic Analysis (BEA) input–output matrix with lottery variation to measure upstream and downstream exposure to H-2B workers at the county-industry level. Applying the Borusyak and Hull (2023) recentered IV approach, I find that the external benefits of H-2B workers largely traveled downstream along the supply chain, to firms that likely sourced their inputs from H-2B employers.

This paper makes three key contributions to the literature. It is the first to provide credible evidence that foreign-born workers were essential to U.S. 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).

This paper's second and most novel contribution is to offer evidence that guest workers have external effects in local production networks, beyond just the firms that directly employ them. The most closely related work in this vein is Akgündüz et al. (2024), who study how Syrian refugee inflows affected Turkish manufacturing through cross-regional input trade. Here, as in Galaasen et al. (2025), I instead focus on cross-industry effects of immigrants *within* area. However, while Galaasen et al. (2025) highlight the role of immigrants as consumers, I find that the nature of the H-2B visa program fosters a more important role for H-2B workers on the production side of the local economy. This nascent approach to understanding the economic impacts of migrant workers connects the immigration literature to the broader study of how shocks propagate in supply chains (e.g., Barrot and Sauvagnat, 2016; Bernard et al., 2019; Boehm et al., 2019; Carvalho et al., 2021; Khanna et al., 2022).[3] In focusing on the pandemic period, this paper offers causal evidence that access to guest workers may help curb the supply-chain cascades typically triggered by firm exits during economic downturns (Taschereau-Dumouchel, 2025) and relates to previous work on local resilience during the COVID-19 pandemic (e.g., Levine et al., 2024; Ayyagari et al., forth.).

Third, 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

[3]This approach is also linked to the the broader literature on the dissipation of migration shocks, which has tended to focus on internal migratory responses across areas (e.g., Card and DiNardo, 2000; Card, 2001; Borjas, 2006; Monras, 2020; Amior, 2021).

immigration at the firm level[4], and is particularly related to recent works that focus on the reallocation of economic activity across firms (Mitaritonna et al., 2017; Orefice and Peri, 2024). A limited set of previous studies have specifically 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 goes beyond these works in examining whether and how these effects propagate up and down the supply chain.[5] In broader terms, this paper contributes to a burgeoning literature that centers the firm in the study of immigration. It also contributes to an older literature documents muted or positive impacts of inflows of low-wage workers on incumbents.[6]

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 presents the results on lottery participants. Section 5 presents the spillovers analysis. Section 6 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 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).

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

[4]See, in addition, Dustmann and Glitz (2015); 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 Willèn (2024); Signorelli (2024); Delgado-Prieto (2025).

[5]Clemens and Lewis (2024) conduct a "partial rather than exact" analysis of the 2020 lottery, using the subset of their 2021/2022 survey respondents who voluntarily self-identified by name and can be matched to public records ($N \approx 200$). Because this analysis conditions on later survey participation and self-identification, it may be subject to selection concerns; nonetheless their 2020 results are qualitatively similar to those found in Section 4 here.

[6]See, e.g., Card (1990); Kugler and Yuksel (2008); Allen et al. (2018); Peri et al. (2022).

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 I-129 petitions for visas from the United States Citizenship and Immigration Services (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 result was not trivial: on average, firms that had all of their consolidated forms assigned to groups A or B secured approximately eight successful I-129 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 strategies used in this paper.

If firms' I-129 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.[7]

While the conversion from I-129 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 COVID-19 pandemic created a unique situation.[8] 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.

[7] See the State Department's Important Announcement on H2 Visas.

[8] In the years leading up to 2018, for example, the approved I-129 to visa issuance conversion rate is usually around 80% (Amuedo-Dorantes et al., 2023).

3 Data

To examine how access to H-2B workers impacted firms during the COVID-19 pandemic, I merge 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.[9] 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 the application was submitted. Second and critically, the Department of Labor also released the lottery letter assignments associated with each TLC form.[10]

Information on the next step of the H-2B procurement process comes directly from the USCIS.[11] This data contains information on the number of approved I-129 visa 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, visa approvals by the USCIS do not always convert into visa issuances by the State Department or ultimate *hires* by the firm, and this was particularly true in 2020. I lack reliable information on the approval-to-hire conversion rate in 2020, and I return to the implications of not having data on these final steps in Sections 4.3 and C.6.

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 at the employer level. I match 93.1% of lottery-subject, 2020 consolidated TLC forms, and 94.4% of lottery-subject, 2020 TLC worker applications (weighting each consolidated form by the number of applications it contains) to a source firm in the U.S. Census Bureau

[9] This data is publicly available from the Department of Labor Performance Data website.
[10] This data is publicly available from the Department of Labor ETA Announcements.
[11] This data is publicly available from the USCIS H-2B Employer Data Hub.

data, resulting in a sample of roughly 4,000 H-2B employer firms that were subject to the 2020 H-2B visa lottery.[12] 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.

4 The Effect of H-2B Workers on Lottery Participants

4.1 Empirical Strategy

I start by studying the effect of the lottery on participants. The primary exposure variable of interest for these analyses 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*$_{j,2020}$. 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*$_{j,2020}$ can be continuous.

Appendix Table C1 presents balance tests and control group means. As expected, I do not find any evidence that *Prop. A or B Apps*$_{j,2020}$ correlates with pre-treatment outcomes. Control group means presented in the last row of Table C1 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 in Section 5.

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

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

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 =

[12] All firm counts are rounded following Census Bureau requirements.

$\log(y_{j,2019})$. This ensures that β 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 $y_{j,2019} \neq 0$.[13] 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.

4.2 Main Results

Figure 2 presents the results from estimating Equation (1). The headline results, for 2020, are presented in blue sqiares. The bottom panel indicates that firms with higher rates of lottery luck were more likely to stay in operation, and that this effect was larger for the set of single-unit firms that are observed specifically during the H-2B hiring period. The top panel indicates that lottery winners enjoyed substantial increases in payroll, revenues, and employment relative to losers. Specifically, assignment of all lottery applications to groups A or B—which led to the opportunity to hire eight additional H-2B workers, on average—increased annual payrolls by nearly 14 percent and annual revenues by more than 4 percent in 2020. Results on payroll growth were once again larger during the H-2B hiring period, and assignment of all lottery applications to groups A or B resulted in nearly 16 percent higher 2HFY employment relative to firms that had all applications assigned to groups C, D, or E.

The maroon circles indicate 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 2020 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 operate in 2020 as a result of lottery losses may have resumed activity by 2021, and effects on firm activity in 2020 therefore did not necessarily translate into effects on longer-run firm *survival*.[14]

[13] 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: $\log(y_{jt}) - \log(y_{j,2019}) = \alpha + \beta[\text{Prop. A or B Apps}_{j,2020}] + (\gamma - 1)\log(y_{j,2019}) + \varepsilon_j$. 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.

[14] 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.

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 I-129 petitions to the USCIS. 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.

Appendix Section C.4 provides further methodological details, and Appendix Table C7 presents ATET estimates for 2020. They tell a similar story to Figure 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.

Both the reduced form and ATET results are particularly economically significant given the policy context in 2020. 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).[15]

4.3 Additional Results

Several additional analyses in the Online Appendix add texture to, but do not alter, the main thrust of the results presented above. Table C4 shows that adding control variables does not alter results, Table C5 shows that results on binary outcome variables are qualitatively unaltered when using logit estimation, and Table C8 provides suggestive evidence that the firm-level impacts of H-2B hiring were larger in counties harder-hit by the pandemic.

Finally, a natural question to ask is whether H-2B hiring crowded out opportunities for incumbent workers at lottery participating firms, at this particularly vulnerable time in the labor market. Appendix Section C.6 attempts to address this question using data from the USCIS and PPP loan applications, but these analyses are subject to caveats that preclude a definitive answer. Instead, I can at most conclude that H-2B hiring did not generate one-for-one crowd-out of other workers, on net.

[15] 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).

While the within-firm crowd out question is important and contentious in prior literature, it elides the question of how *non-participating* firms reacted to random increases or decreases in availability of H-2B workers in the markets that they operated in. Firm-level crowd-in or crowd-out estimates among participants are only one piece of the broader, policy-relevant impact of H-2B workers on employment if non-participating firms are affected. Given the magnitudes found above and the economic environment that accompanied the COVID-19 pandemic, I therefore turn to studying effects on non-participant firms, which did not apply for H-2B workers in 2020.

5 Spillover Effects of H-2B Workers

5.1 Spillovers Within and Across Industry

To start, I split the set of non-participating firms into two groups:

1. "Cross-industry non-participants" (indexed by j'): firms operating in a county with at least one 2020 H-2B lottery participant but that operated in a different industry than all such participants.

2. "Within-industry non-participants" (indexed by j''): firms operating in the same county *and* industry as at least one H-2B participant.

In each group, the impact of increased exposure to H-2B workers at geographically close lottery participating firms is a priori ambiguous. Among within-industry non-participants, two opposing forces operate: on one hand, increased ability of competitors to access H-2B workers may have resulted in business stealing from competing non-participants; on the other, it may have alleviated constraints on labor availability during a time of acute labor scarcity.

For the group of cross-industry non-participants, 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. However, 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 cost of competitors, insofar as consumers are willing to substitute consumption across industries. Finally, though the program is small in size, H-2B workers may have nonetheless themselves provided an injection of consumer demand into local economies. Indeed, an overarching question when analyzing cross-industry non-participants is whether the H-2B program is large enough in size to have any impact outside of the firms that use it and their close competitors.

To assess the quantitative importance of these explanations, I start by estimating the following models:

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

$$f(y_{j'',2020}) = \alpha + \beta[\text{Prop. A or B Apps}_{\ell(j''),k(j''),2020}] + \gamma X_{j''} + \varepsilon_{j''}, \tag{3}$$

recalling that j' indexes a cross-industry non-participating firm and j'' indexes a within-industry non-participating firm. *Prop. A or B Apps*$_{\ell k,2020}$ is the proportion of county ℓ and BEA industry k's H-2B applications that were in lottery groups A or B.[16] *Prop. A or B Apps*$_{\ell,2020}$ is the proportion of applications in j''s county that were assigned to lottery groups A or B. Variation in the exposure variables 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.7 shows a large degree of variance in *Prop. A or B Apps*$_{\ell,2020}$ across the 900 U.S. counties that had a lottery participant.[17]

Table 1 presents estimates from Equations (2) and (3). 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, but there is much stronger evidence that H-2B workers benefited local firms in industries *other* than those in which they were hired. That is, Panel A shows that—despite the small program size—H-2B workers appear to have important effects external to the firms they work for, across industries. While positive estimates from Equation (3) in Panel B indicate some scope for alleviation of labor scarcity, standard errors preclude definitive conclusions.[18] Additional results in Appendix Section C.5 show that these cross-industry spillovers from Panel A were magnified in larger, urban counties with high population density and worse initial exposure to the pandemic. Appendix Section C.7 shows that these spillovers show up in county-level aggregates.

5.2 The Role of Consumer Demand

What, then, drives these cross-industry spillovers? I first assess the hypothesis that it is consumer demand of H-2B workers themselves. To to so, I use the 2017 BEA's detailed national input-output (I-O) matrices to define Prop Final$_{k(j')}$ for each non-participant firm: the proportion of the firm's BEA industry k's output that is sold as final goods by consumers.[19] I then estimate the following specification on the set of cross-industry non-participants:

$$f(y_{j',2020}) = \alpha + \beta[\text{Prop. A or B Apps}_{\ell(j'),2020}] + \theta[\text{Prop. Final}_{k(j')}] + \delta[\text{Prop. A or B Apps}_{\ell(j'),2020} \times \text{Prop. Final}_{k(j')}] + \gamma X_{j'} + \varepsilon_{j'}. \quad (4)$$

[16] Multi-unit firms are assigned counties and industries based on their county and industry of highest payroll disbursement in the LBD. I use the NAICS-commodity crosswalk provided by the BEA to match firm industries provided in the LBD to BEA industry-commodities.

[17] 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 1. Table C15 shows that H-2B employers and workers are heavily concentrated in local services.

[18] The results from Equation (3) are also the most comparable to spillover results presented in Amuedo-Dorantes et al. (2023). In that paper and here, the notion that there are negative spillover effects on competitor firms does not have a lot of support in the data.

[19] Specifically, I use the 2017 Detailed Supply Use Table (detailed tables are not available for 2018 or 2019).

Since Prop Final$_{k(j')} \in [0, 1]$, β in Equation (4) estimates the spillover effect for a firm that operates in an industry that is fully intermediate goods selling, whereas δ tells us the differential effect for firms that operate in industries that are fully final goods selling. In short, if spillover effects were operating entirely through the consumer demand channel, we would roughly expect $\beta \approx 0$ and $\delta \gg 0$. θ is a non-causal parameter that indicates how final goods-selling firms did relative to intermediate-selling firms during the pandemic.

Table 2 presents estimates from Equation (4). I find that that cross-industry non-participant firms in fully intermediate sectors experience non-trivial spillovers from exposure to H-2B workers in their counties ($\hat{\beta} > 0$) across all outcomes, indicating that consumer demand cannot be the sole reason for the spillover effects found in Panel A of Table 1. Effects are larger at final-goods-selling firms in terms of sales, but this is not matched by other measures of performance and labor utilization ($\hat{\delta} \approx 0$ for outcomes other than revenues).

It is important to note that these results are not entirely dispositive of the notion that H-2B employee consumer demand impacted local economies: presumably, an increase in consumer demand would filter down the supply chain to intermediate goods producing firms. Nonetheless, the small size of the H-2B workforce, the program's temporary nature[20], and the small estimated $\hat{\delta}$ found in Table 2 all suggest that the consumer demand effects generated by permanent immigration in other settings (i.e., Galaasen et al., 2025) may not operate here. That 66% of H-2B employer production is in intermediate goods further suggest that spillovers may be more likely on the production side of the local economy.[21]

5.3 Spillovers Upstream and Downstream in the Supply Chain

Thus, I finally focus on the notion that cross-industry spillover effects are occurring on the supply side of the local economy and assess whether they are driven by firms upstream or downstream in the supply chain.[22] To do this, I once again utilize the 2017 BEA detailed input-output matrices to construct measures of exposure to H-2B workers in local input and output markets. Then, motivated by a similar reweighting procedure used in Acemoglu et al. (2016), I define:

$$\text{H-2B Input Shock}_{\ell k,2020} \equiv \sum_{k' \neq k} s^{I}_{k'k} \times \text{Prop A or B Apps}_{\ell k'}$$

$$\text{H-2B Output Shock}_{\ell k,2020} \equiv \sum_{k' \neq k} s^{O}_{kk'} \times \text{Prop A or B Apps}_{\ell k'}$$

[20] Temporary, guest workers are more likely to save and remit earnings relative to permanent immigrants (Dustmann and Mestres, 2010; Amuedo-Dorantes et al., 2005).

[21] This figure is calculated as follows: $\sum_f \omega_f (1 - \text{Prop. Final}_{k(f)})$, where f indexes a consolidated form entered into the lottery, $k(f)$ is the BEA industry corresponding to the NAICS code indicated on a given form f, and ω_f is the proportion of lottery applications accounted for by form f.

[22] To use a concrete example, see this report from the Maryland Department of Agriculture indicating that the loss of H-2B crab shuckers would result in job losses of both fishers who catch the crab (upstream) and restaurants who ultimately sell the crab to final consumers (downstream).

where $s^{I}_{k'k}$ is the share of industry k inputs that come from industry k', and $s^{O}_{kk'}$ is the share of industry k outputs that are sold to industry k'. I then estimate the following model, among across-industry non-participants:

$$f(y_{j',2020}) = \alpha + \beta_I[\text{H-2B Input Shock}_{\ell(j'),k(j'),2020}] + \beta_O[\text{H-2B Output Shock}_{\ell(j'),k(j'),2020}] + \gamma X_{j'} + \varepsilon_{j'}. \quad (5)$$

While *Prop A or B Apps*$_{\ell k'}$ for any given k' is exogenous by virtue of the lottery, *H-2B Input Shock*$_{\ell k,2020}$ and *H-2B Output Shock*$_{\ell k,2020}$ weight these exogenous shocks with potentially endogenous exposures from the BEA I-O matrix. I thus utilize the recentered IV approach introduced by Borusyak and Hull (2023, 2020) to isolate identifying variation that stems from the exogenous shocks. In short, I run reduced form regressions that control for non-random exposure in the vector X'_j. I describe these controls in further detail in Appendix Section B.2.

Figure 3 displays resulting estimates from Equation (5), and presents a relatively clear picture: H-2B workers primarily generated external effects downstream in the supply chain. The green squares show that a one standard deviation increase in the H-2B input shock leads to increases in payroll disbursement and revenue generation of 1.2 percentage points and 1.3 percentage points in 2020, respectively, indicating that firms whose suppliers are able to hire H-2B visa workers also benefit from their own indirect exposure to these migrants. These downstream effects were further pronounced for single-unit firms during the H-2B hiring period of Q2-Q3 2020. There are two plausible, related explanations for these results, based on the results from Section 4: 1) firms faced lower prices from suppliers who were able to operate at scale thanks to H-2B workers; 2) firms faced fewer supply chain disruptions due to outright exit in their relevant input markets. Meanwhile, focusing on the purple circles, evidence that H-2B workers generated spillovers from upstream is less conclusive.

6 Conclusion

This study provides the first well-identified evidence that foreign-born workers were essential to U.S. 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 local spillovers to non-participant firms that likely arise due to supply chain linkages. These findings underscore the critical role of migrant labor in maintaining business continuity during economic disruptions and suggest that flexible immigration policies may serve as cost-effective interventions when rapid responses are needed to safeguard public health and economic welfare. They also demonstrate that the impacts of migrant workers on production extend beyond the firms that hire them.

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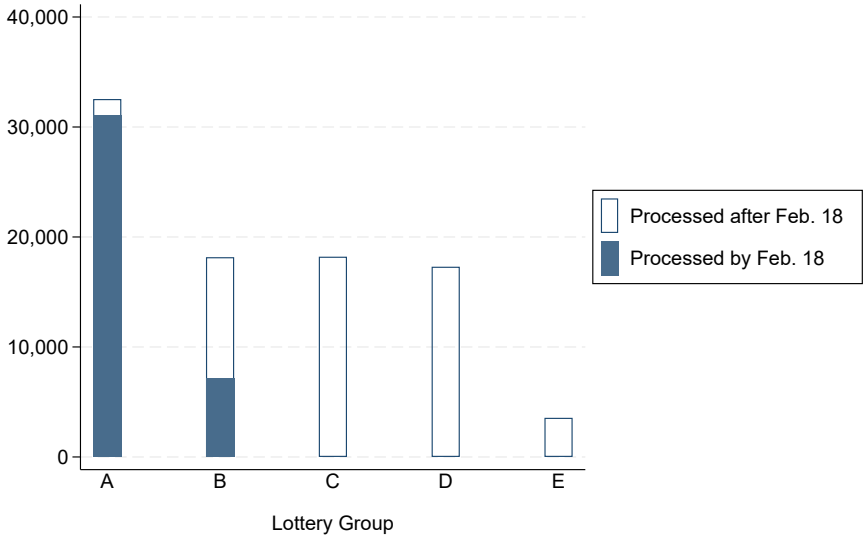
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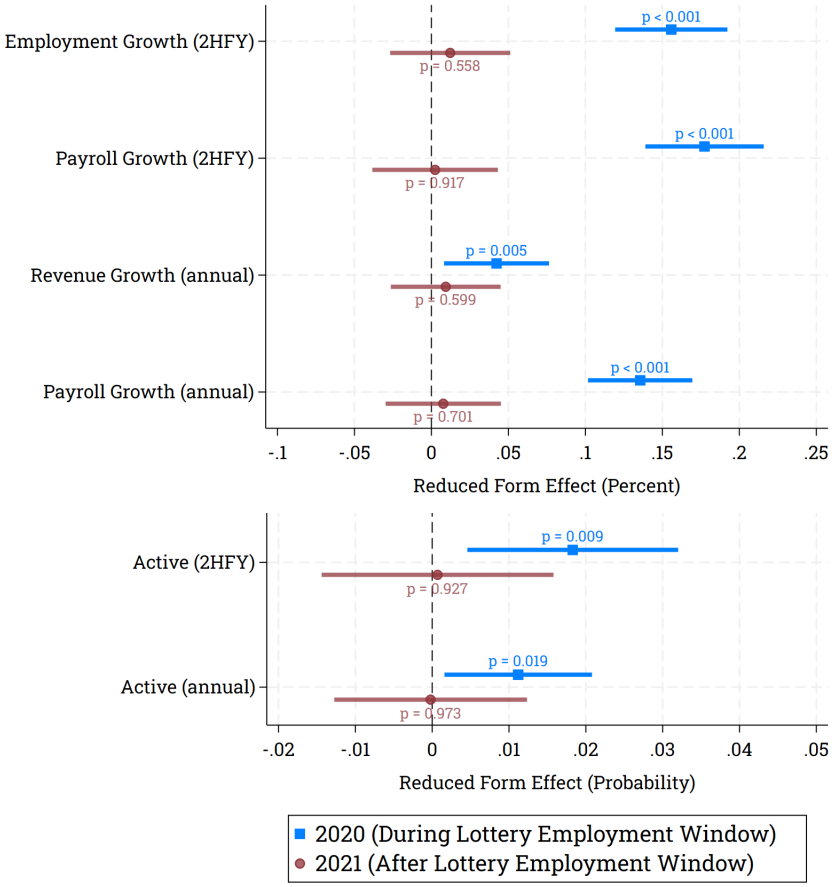
Figures and Tables

Figure 1: TLC Applications by Lottery Group



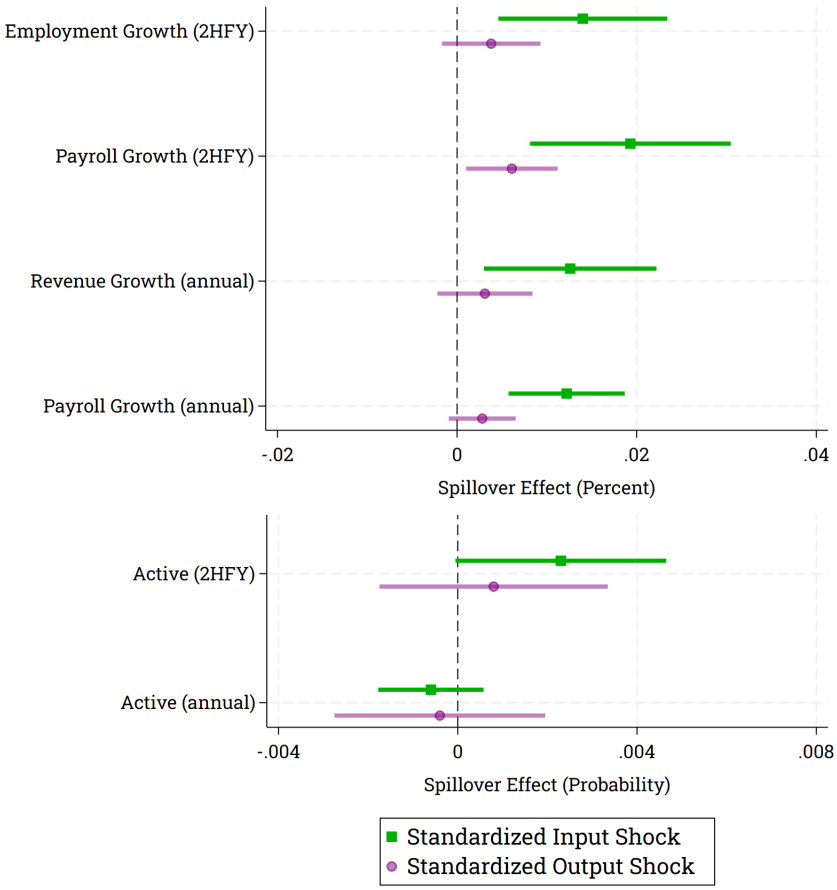
SOURCE: DoL Office of Foreign Labor Certification Performance Data.
NOTE: The USCIS stopped receipt of I-129 petitions on February 18, 2020, so any TLC applications that were not processed by this date did not have a chance to convert to eventual H-2B visas.

Figure 2: The Effect of H-2B Access on Lottery-Participating Firms



NOTE: See Equation (1) for specification. Thick bands represent 95% confidence intervals based on conventional, heteroskedasticity robust standard errors. Reported *p*-values are based on alternate randomization inference (see Appendix Section B.1). 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. The samples used for each outcome are the same as in Table C1. In the top panel, 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). Exact point estimates and control group means can be seen in Appendix Table C2.

Figure 3: Spillovers on Non-Participants Upstream and Downstream of H-2B Workers



NOTE: See Equation (5) for specification, with exposure varables standardized. Thick bands represent 95% confidence intervals based on conventional standard errors, clustered at the county level. 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. The samples used for each outcome are the same as in Table 1, Panel A. In the top panel, 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). Exact point estimates and standard errors can be seen in Table C3.

Table 1: Spillovers Within and Across Industry

	Annual (2020)			2HFY (Q2–Q3, 2020)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Payroll Growth	Revenue Growth	Active	Payroll Growth	Employment Growth
Panel A: Cross-Industry Non-Participants						
Prop. A or B Apps$_{\ell(j'),2020}$	0.002	0.039***	0.030***	0.012**	0.062***	0.048**
	(0.002)	(0.014)	(0.010)	(0.006)	(0.023)	(0.021)
Observations (Firms)	4,253,000	3,905,000	2,452,000	4,131,000	3,483,000	3,483,000
Clusters (Counties)	900	900	900	900	900	900
Panel B: Within-Industry Non-Participants						
Prop. A or B Apps$_{\ell(j''),k(j''),2020}$	0.003	0.025	0.034*	0.008	0.042	0.037
	(0.003)	(0.017)	(0.020)	(0.007)	(0.027)	(0.024)
Observations (Firms)	532,000	488,000	319,000	526,000	449,000	449,000
Clusters (Counties)	900	900	900	900	900	900

NOTE: See Equation (2) for specification in Panel A See Equation (3) for specification in Panel B. $\ell(j')$ refers to a cross-industry non-participant firm j''s county of primary operation, $\ell(j'')$ refers to a within-industry non-participant firm j'''s county of primary operation, and $k(j'')$ refers to a within-industry non-participant firm j'''s BEA industry 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. 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).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Spillover Heterogeneity by Final Goods Use

	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$_{\ell(j'),2020}$	0.005*	0.033***	0.018**	0.014**	0.049**	0.040**
	(0.003)	(0.012)	(0.009)	(0.006)	(0.021)	(0.019)
Prop. A or B Apps$_{\ell(j'),2020}$	-0.004*	0.011	0.019**	-0.003	0.021	0.014
$\times$ Prop. Final$_{k(j')}$	(0.002)	(0.009)	(0.009)	(0.003)	(0.013)	(0.010)
Prop. Final$_{k(j')}$	0.007***	-0.014**	-0.061***	0.007***	-0.068***	-0.060***
	(0.001)	(0.006)	(0.006)	(0.002)	(0.009)	(0.007)
Observations (Firms)	4,253,000	3,905,000	2,452,000	4,131,000	3,483,000	3,483,000
Clusters (Counties)	900	900	900	900	900	900

NOTE: See Equation (4) for specification. $\ell(j')$ refers to a non-participant firm j''s county of primary operation and $k(j')$ refers to a non-participant firm j''s BEA commodity. 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. 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).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix for Online Publication

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 I-129 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-address type. 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 bivarate 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	0.002
	(0.007)	(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, *firmid*, 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 *firmid*. Thus, naively tracking outcomes at the *firmid* 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). Second, I then assigned the linked establishment to a parent firm based on their *firmid* in 2019. In the few cases that an establishment did not have a valid *firmid* in the 2019 LBD, I assigned *firmid* 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 *firmid* from a specific LBD year.

I then define a firm as the set of establishments operating under that *firmid* 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 *firmid* 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 *firmid* 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 *firmid*s 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 *firmid* 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 *firmid* changes. Finally, this consistent *firmid* definition is not possible for the revenues outcome. Revenues—unlike payroll and employment—are reported as an annual total for an entire *firmid* (not at the establishment level), and this measure may therefore incorporate acquisitions or divestitures after 2019.

B Methods

B.1 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*$^s_{j,2020}$
3. Re-estimate Equation (1), replacing *Prop. A or B Apps*$_{j,2020}$ with *Prop. A or B Apps*$^s_{j,2020}$, 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.

B.2 Recentered IV Details

In order to utilize the Borusyak and Hull (2023) approach, I use the same $S = 1,000$ simulation draws as described in Appendix B.1 to reconstruct 1,000 placebo versions of each shock. Specifically, I follow these steps within each simulation:

1. Randomly re-assign lottery groups to consolidated TLC forms based on true proportions that were in groups $\{A, B, C, D, E\}$ (same as Step 1. in B.1)
2. Collapse to firm level to generate placebo version of exposure: *Prop. A or B Apps*$^s_{j,2020}$ (same as Step 2. in B.1)
3. Further collapse to county-commodity level to generate placebo exposure: *Prop. A or B Apps*$^s_{\ell k}$
4. For each cross-industry non-participate, construct

$$\text{H-2B Input Shock}^s_{\ell k,2020} \equiv \sum_{k' \neq k} s^I_{k'k} \times \text{Prop A or B Apps}^s_{\ell k'}$$
$$\text{H-2B Output Shock}^s_{\ell k,2020} \equiv \sum_{k' \neq k} s^O_{kk'} \times \text{Prop A or B Apps}^s_{\ell k'}$$

Then, I generate controls

$$\mu^I_{\ell k} \equiv \frac{1}{S} \sum_s \text{H-2B Input Shock}^s_{\ell k,2020}$$
$$\mu^O_{\ell k} \equiv \frac{1}{S} \sum_s \text{H-2B Output Shock}^s_{\ell k,2020}.$$

Finally, $\mu^I_{\ell(j'),k(j')}$ and $\mu^O_{\ell(j'),k(j')}$ are included as controls in $X_{j'}$ of Equation (5). Note that the working paper version of Borusyak and Hull (2023) includes further details about why this approach is still valid with multiple exposure variables of interest (Borusyak and Hull, 2020). Randomization inference based p-values on $\hat{\beta}_I$ and $\hat{\beta}_O$ from Equation (5) are naturally generated from this process and are available upon request.

C Supplemental Figures and Tables

C.1 Balance

Table C1: Balance Tests

	Annual (2019)			2HFY (Q2–Q3, 2019)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Log Payroll	Log Revenues	Active	Log Payroll	Log Employment
Prop. A or B Apps$_{j,2020}$	-0.002	0.024	-0.017	0.0001	0.006	-0.014
	(0.004)	(0.049)	(0.050)	(0.0062)	(0.044)	(0.039)
	[0.647]	[0.610]	[0.729]	[0.999]	[0.913]	[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.

C.2 Point Estimates Corresponding to Figures

Table C2: Reduced Form Results (Figure 2)

	Annual			2HFY (Q2 & Q3)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Payroll Growth	Revenue Growth	Active	Payroll Growth	Employment Growth
Panel A: 2020 (During 2HFY 2020 H-2B Employment Window)						
Prop. A or B Apps$_{j,2020}$	0.011**	0.136***	0.042**	0.018***	0.177***	0.156***
	(0.005)	(0.017)	(0.017)	(0.007)	(0.020)	(0.019)
	[0.019]	[<0.001]	[0.005]	[0.009]	[<0.001]	[<0.001]
Panel B: 2021 (After 2HFY 2020 H-2B Employment Window)						
Prop. A or B Apps$_{j,2020}$	-0.0002	0.008	0.009	0.001	0.002	0.012
	(0.0064)	(0.019)	(0.018)	(0.008)	(0.021)	(0.020)
	[0.973]	[0.701]	[0.599]	[0.927]	[0.917]	[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 C1. 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).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C3: Spillovers Upstream and Downstream of H-2B Workers (Figure 3)

	Annual (2020)			2HFY (Q2–Q3, 2020)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Payroll Growth	Revenue Growth	Active	Payroll Growth	Employment Growth
Std. H-2B Input Shock$_{\ell(j'),k(j'),2020}$	-0.0006	0.0122***	0.0126**	0.0023*	0.0193***	0.0140***
	(0.0006)	(0.0033)	(0.0049)	(0.0012)	(0.0057)	(0.0048)
Std. H-2B Output Shock$_{\ell(j'),k(j'),2020}$	-0.0004	0.0028	0.0031	0.0008	0.0061**	0.0038
	(0.0012)	(0.0019)	(0.0027)	(0.0013)	(0.0026)	(0.0028)
Observations (Firms)	4,253,000	3,905,000	2,452,000	4,131,000	3,483,000	3,483,000
Clusters (Counties)	900	900	900	900	900	900

NOTE: See Equation (5) for specification, with exposure variables standardized. $\ell(j')$ refers to a non-participant firm j''s county of primary operation and $k(j')$ refers to a non-participant firm j''s BEA-defined commodity. 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. 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).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.3 Robustness

C.3.1 Post-Double Selection LASSO Controls

Here, I present the main reduced form results from Table C2 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 C4: 2020 Reduced Form Results With and Without Controls

	Annual (2020)			2HFY (Q2–Q3, 2020)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Payroll Growth	Revenue Growth	Active	Payroll Growth	Employment Growth
Panel A: Without Controls						
Prop. A or B Apps$_{j,2020}$	0.011**	0.136***	0.042**	0.018***	0.177***	0.156***
	(0.005)	(0.017)	(0.017)	(0.007)	(0.020)	(0.019)
	[0.019]	[<0.001]	[0.005]	[0.009]	[<0.001]	[<0.001]
Panel B: With Controls						
Prop. A or B Apps$_{j,2020}$	0.010**	0.133***	0.051***	0.017**	0.177***	0.157***
	(0.005)	(0.016)	(0.015)	(0.007)	(0.018)	(0.017)
	[0.034]	[<0.001]	[<0.001]	[0.010]	[<0.001]	[<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.3.2 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 C5 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 C2. Results are highly robust to this alternate specification.

Table C5: 2020 Reduced Form Extensive Margin Results

	Outcome: Active			
Prop. A or B Apps$_{j,2020}$	0.0112**	0.0111**	0.0183***	0.0180***
	(0.0049)	(0.0049)	(0.0070)	(0.0069)
	[0.019]	[0.025]	[0.009]	[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.4 Average Treatment Effect on the Treated Estimates

C.4.1 Methods

Let D_j represent an indicator for whether a firm had any I-129 approvals for new H-2B workers in 2020—that is, an indicator for having had the *option* to bring H-2B workers on board during the height of the pandemic.[23] Let F_j represent the number of lottery-subject consolidated forms submitted to the DoL by firm j.[24] 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: $Y_j(0), Y_j(1) \perp Z_j | F_j$.[25]

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[26] and the conditional randomness of Z_j, I can estimate ATET(f) using Z_j as an instrument for D_j at each level of f.[27] 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.

C.4.2 Balance: Binary Treatment

Table C6: 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	0.041	-0.005	0.001	0.022	0.002
	(0.005)	(0.048)	(0.048)	(0.006)	(0.043)	(0.039)
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 (6) for specification. Fixed effects for the number of consolidated forms filed by firm j included in all specifications. Heteroskedasticity robust standard errors in parentheses. 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 ATET calculations presented in Section C.4 rely on the randomness of Z_j, an indicator for assignment of *any* consolidated forms to lottery groups A or B, condiditional on the number of forms submitted. Table C6 presents the results of balance tests consistent with this notion, estimated using the following model:

$$f(y_{j,2019}) = \beta[Z_{j,2020}] + \alpha_{f(j)} + \varepsilon_j. \tag{6}$$

[23] Recall from Section 2 that USCIS approvals were made before full awareness of the pandemic, but final hiring decisions were likely made after.

[24] Recall that lottery group assignment was made at the consolidated form level.

[25] Appendix Section C.4.2 provides balance test results consistent with this assumption.

[26] I have confirmed that there were no I-129 petitions filed by firms fully assigned to lottery groups C, D, or E in the data.

[27] In practice, in one fully saturated regression for ease of post-estimation weighted aggregation.

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.

C.4.3 Results

Table C7: 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**	0.238***	0.062**	0.035***	0.317***	0.281***
	(0.009)	(0.031)	(0.027)	(0.013)	(0.035)	(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 C1. 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).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.5 Heterogeneity

C.5.1 COVID-19 Severity

To assess whether participating firms in counties that were more exposed to the pandemic were more reliant on their H-2B workers, I interact *Prop. A or B Apps*$_{j,2020}$ with a (standardized) measure of county-level excess deaths in March of 2020, obtained from Paglino et al. (2023). Results are presented in Table C8. They suggest that firms facing higher initial exposure to the pandemic relied on their H-2B workers more to remain in operation and operate at scale.

Then, to assess whether non-participants in counties that were more exposed to the pandemic were more reliant on supply chain reslience generated by H-2B workers, I interact *Prop. A or B Apps*$_{\ell(j'),2020}$ with the same standardized measure of county-level excess deaths in March of 2020, in Equation (2). Results are presented in Table C9. While not conclusive, they suggest that spillover effects were larger in counties that were initially harder hit by the pandemic, particularly in Q2 and Q3 of 2020. Non-causal results on the standardized excess death rates indicate the havoc wreaked by the COVID-19 pandemic on economic activity.

Table C8: Heterogeneity by County COVID-19 Severity—Participants

	Annual			2HFY (Q2 & Q3)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Payroll Growth	Revenue Growth	Active	Payroll Growth	Employment Growth
Prop. A or B Apps$_{j,2020}$	0.009**	0.126***	0.041**	0.015**	0.169***	0.150***
	(0.005)	(0.018)	(0.019)	(0.007)	(0.021)	(0.020)
	[0.058]	[<0.001]	0.007	0.031	[<0.001]	[<0.001]
Prop. A or B Apps$_{j,2020}$	0.004*	0.021**	0.003	0.007**	0.017*	0.014
×Std. Excess Deaths$_{\ell(j),\text{March 2020}}$	(0.003)	(0.010)	(0.007)	(0.004)	(0.010)	(0.010)
	[0.056]	[0.058]	[0.741]	[0.041]	[0.161]	[0.253]
Std. Excess Deaths$_{\ell(j),\text{March 2020}}$	-0.002	-0.016**	-0.001	-0.004	-0.013*	-0.012*
	(0.002)	(0.007)	(0.007)	(0.003)	(0.008)	(0.006)
Observations (Firms)	4,000	4,000	2,700	3,700	3,600	3,400

NOTE: Results are from a modified version of Equation (1), adding the interaction term and the main effect for excess deaths. Heteroskedasticity robust standard errors in parentheses. Randomization inference p values in square brackets, on causal terms. Where applicable Davis-Haltiwanger-Schuh (DHS) growth rates are calculated relative to 2019 (see Section 4). 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.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C9: Spillover Heterogeneity by County COVID-19 Severity

	Annual			2HFY (Q2 & Q3)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Payroll Growth	Revenue Growth	Active	Payroll Growth	Employment Growth
Prop. A or B Apps$_{\ell(j'),2020}$	0.0011	0.0281***	0.0206**	0.0081***	0.0449***	0.0337***
	(0.0016)	(0.0089)	(0.0089)	(0.0028)	(0.0136)	(0.0105)
Prop. A or B Apps$_{\ell(j'),2020}$	0.0017	0.0074	0.0024	0.0060*	0.0187*	0.0197**
× Std. Excess Deaths Mar. '20$_{\ell(j'),2020}$	(0.0016)	(0.0069)	(0.0083)	(0.0035)	(0.0103)	(0.0097)
Std. Excess Deaths Mar. '20$_{\ell(j'),2020}$	-0.0037***	-0.0325***	-0.0228***	-0.0128***	-0.0524***	-0.0468***
	(0.0012)	(0.0051)	(0.0050)	(0.0025)	(0.0076)	(0.0070)
Observations (Firms)	4,253,000	3,905,000	2,452,000	4,131,000	3,483,000	3,483,000

NOTE: Results are from a modified version of Equation (2), adding the interaction term and the main effect for excess deaths. Standard errors clustered at county level. All results are for 2020. 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 C1. 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).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.5.2 Population (Density)

To assess whether cross-industry spillover effects were generally stronger in areas with thicker markets, I conduct three heterogeneity analyses: 1) by whether or not a given county is located in a metropolitan area, as defined by the Office of Management and Budget in 2013; 2) by (standardized) county log population counts; 3) by (standardized) log population density. Results all indicate that spillover effects were larger in denser markets, where supply chains are more likely to operate on a local level.

Table C10: Spillover Heterogeneity by County Metropolitan Classification

	Annual			2HFY (Q2 & Q3)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Payroll Growth	Revenue Growth	Active	Payroll Growth	Employment Growth
Prop. A or B Apps$_{\ell(j'),2020}$	-0.0005	-0.0039	-0.0063	-0.0029	-0.0075	-0.0049
	(0.0022)	(0.0082)	(0.0096)	(0.0027)	(0.0107)	(0.0082)
Prop. A or B Apps$_{\ell(j'),2020}$	0.0028	0.0467***	0.0384***	0.0164**	0.0759***	0.0584**
$\times \mathbb{1}\{\text{Metro}_{\ell(j'),2020}\}$	(0.0034)	(0.0169)	(0.0146)	(0.0069)	(0.0267)	(0.0237)
$\mathbb{1}\{\text{Metro}_{\ell(j'),2020}\}$	-0.0102***	-0.0802***	-0.0786***	-0.0236***	-0.1143***	-0.0823***
	(0.0023)	(0.0126)	(0.0109)	(0.0050)	(0.0194)	(0.0169)
Observations (Firms)	4,253,000	3,905,000	2,452,000	4,131,000	3,483,000	3,483,000

NOTE: Results are from a modified version of Equation (2), adding the interaction term and the main effect indicating whether a firm's county is metropolitan. Standard errors clustered at county level. All results are for 2020. 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 C1. 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).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C11: Spillover Heterogeneity by County Population

	Annual			2HFY (Q2 & Q3)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Payroll Growth	Revenue Growth	Active	Payroll Growth	Employment Growth
Prop. A or B Apps$_{\ell(j'),2020}$	0.0029	0.0522***	0.0377***	0.0175**	0.0860***	0.0695***
	(0.0028)	(0.0156)	(0.0117)	(0.0070)	(0.0256)	(0.0244)
Prop. A or B Apps$_{\ell(j'),2020}$	0.0032	0.0375***	0.0281***	0.0149**	0.0644***	0.0547***
$\times$ Std. Log Pop$_{\ell(j'),2019}$	(0.0024)	(0.0124)	(0.0091)	(0.0060)	(0.0204)	(0.0201)
Std. Log Pop$_{\ell(j'),2019}$	-0.0061***	-0.0480***	-0.0404***	-0.0173***	-0.0760***	-0.0614***
	(0.0016)	(0.0077)	(0.0054)	(0.0038)	(0.0128)	(0.0126)
Observations (Firms)	4,253,000	3,905,000	2,452,000	4,131,000	3,483,000	3,483,000

NOTE: Results are from a modified version of Equation (2), adding the interaction term and the main effect for standardized log population. All results are for 2020. 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 C1. 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).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C12: Spillover Heterogeneity by County Population Density

	Annual			2HFY (Q2 & Q3)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Payroll Growth	Revenue Growth	Active	Payroll Growth	Employment Growth
Prop. A or B Apps$_{\ell(j'),2020}$	0.0017	0.0327***	0.0173*	0.0123***	0.0573***	0.0472***
	(0.0019)	(0.0095)	(0.0102)	(0.0038)	(0.0150)	(0.0127)
Prop. A or B Apps$_{\ell(j'),2020}$	0.0033**	0.0254***	0.0099	0.0131***	0.0484***	0.0447***
× Std. Log Pop. Dens.$_{\ell(j'),2020}$	(0.0016)	(0.0074)	(0.0098)	(0.0039)	(0.0121)	(0.0116)
Std. Log Pop. Dens.$_{\ell(j'),2019}$	-0.0059***	-0.0532***	-0.0427***	-0.0180***	-0.0819***	-0.0678***
	(0.0011)	(0.0060)	(0.0076)	(0.0026)	(0.0083)	(0.0076)
Observations (Firms)	4,253,000	3,905,000	2,452,000	4,131,000	3,483,000	3,483,000

NOTE: Results are from a modified version of Equation (2), adding the interaction term and the main effect for standardized log population density. All results are for 2020. 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 C1. 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).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.6 Assessing Firm-Level Crowd-Out

C.6.1 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:

$$\Delta\text{2HFY Employment}_{j,2020} = \alpha + \beta^{IV}[\text{I-129 Approvals}_{j,2020}] + \varepsilon_j, \tag{7}$$

where Δ2HFY Employment$_{j,2020}$ represents the 2019-2020 change in 2HFY employment at the firm and *Prop. A or B Apps*$_{j,2020}$ is used as an instrument for *I-129 Approvals*$_{j,2020}$. β^{IV} measures additional employees per I-129 approval (obtained from USCIS data). Under the assumption that each I-129 approval converts to an *actual hire*, $\beta^{IV} < 1$ indicates that employment of non-visa workers is being crowded out (since the Census employment counts include visa workers).

However, this assumption is slightly problematic in normal times and highly problematic given the specific case of the 2020 H-2B visa lottery. DHS resports indicate that the I-129 approval to visa issuance rate is closer to 80% (rather than 100%) in the years leading up to 2018.[28] More importantly, in 2020, H-2B I-129 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 β^{IV} to assess whether there is crowd out of non-H-2B workers.

I nonetheless estimate Equation (7), 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 I-129 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 I-129s into hires.

C.6.2 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.[29] Thus, H-2B employers' PPP loan applications should be limited to 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.[30,31] I then estimate the following specification:

$$\log(\text{2HFY PPP Jobs}_{j,2020}) = \alpha + \beta[\text{Prop. A or B Apps}_{j,2020}] + \varepsilon_j \tag{8}$$

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

[28] See this DHS Report: H-2B Usage and Recommendations.

[29] See, e.g., here, for specific mention that H-2B workers should not be counted on applications.

[30] SBA PPP loan data: https://www.sba.gov/funding-programs/loans/covid-19-relief-options/paycheck-protection-program/ppp-data.

[31] 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.[32] 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 C13: Take-Up of PPP Loans and Job Counts Reported on PPP Loans

	(1) $\mathbb{1}\{\text{Any PPP}\}$	(2) Log Jobs Reported
Prop. A or B Apps$_{j,2020}$	0.005	0.113**
	(0.015)	(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. Column (2) is estimated conditional on $\mathbb{1}\{\text{Any PPP}_j\} = 1$.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C13 shows in Column (1) that use of the PPP loan program is uncorrelated with the lottery (*Prop. A or B Apps*$_{j,2020}$). 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.[33] 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.6.1, 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. It is also worth reiterating here that a key point of this paper is that increased employment at non-participant firms represents a pure crowd-in effect not measured by the estimates generated by a firm-level analysis of participants—increased employment at non-participants was of non-H-2B workers by definition.

[32] In three cases, an employer filed more than one PPP loan application during 2HFY 2020. *2HFY PPP Jobs*$_{j,2020}$ is then the mean of the job count across these applications.

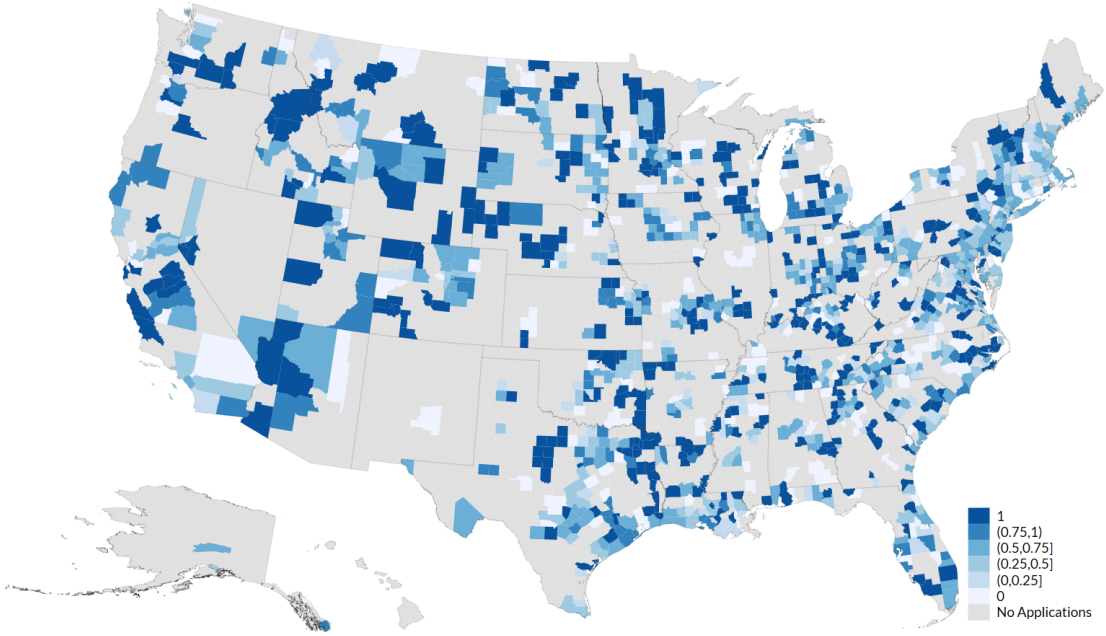
[33] This is evidenced by the fact that the SBA, USDA, and other organizations had to issue clarifications regarding whether H-2B workers should count toward job totals. Some firms may have considered their H-2B workers to have primary residences in the United States, even if they are temporary employees,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

C.7 Vetting and Testing County-Level Random Variation in H-2B Worker Exposure

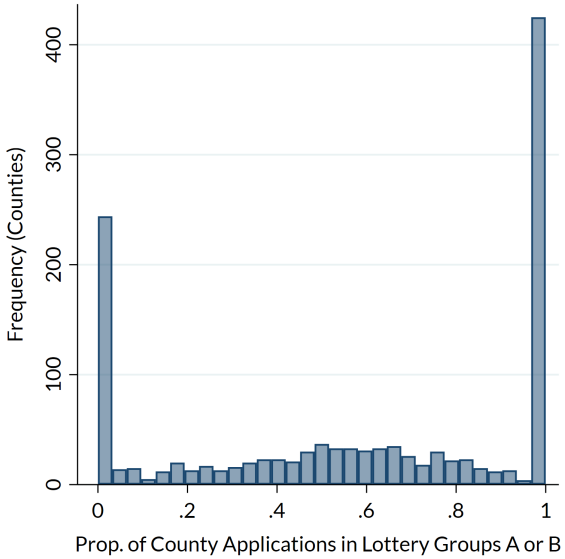
C.7.1 Plotting the Degree of Variation

Figure C1: Lottery Variation at Local Level

A: *Prop. A or B Apps*$_{\ell,2020}$ by County



B: Histogram of *Prop. A or B Apps*$_{\ell,2020}$



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

C.7.2 Assessing County-Level Crowd-Out

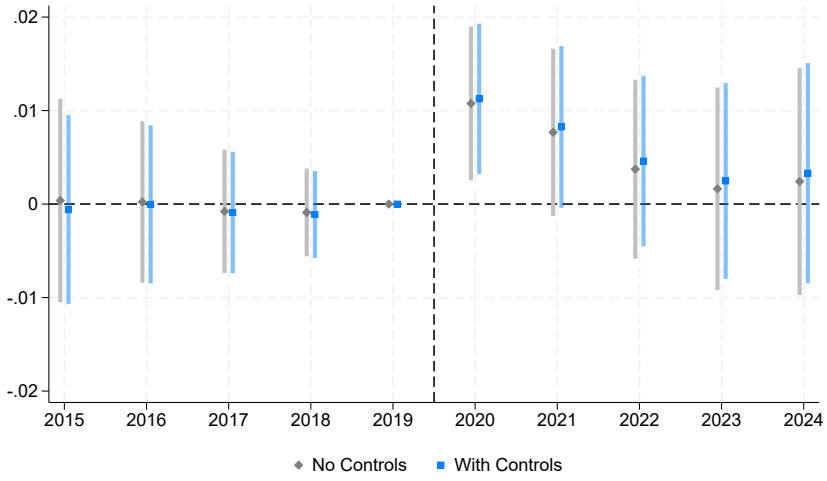
The spillover effects found in Section 5 imply that the H-2B visa program may have a larger impact than its raw size implies, well beyond H-2B employers themselves. Here, I examine market-level effects: whether *counties* more exposed to H-2B visa workers experienced larger employment growth in the second half of fiscal year 2020. To do so, I match H-2B applications (which contain employer ZIP code) to counties using a crosswalk provided by the U.S. Department of Housing and Urban Development.

On one hand, the results from Sections 5 and C.6 imply that there is likely a positive employment effect of H-2B visa worker presence at this level of aggregation, given that the size of the H-2B visa program is only 33,000 visas nationally. On the other hand, it is unclear if this effect is big enough to show up in data aggregated at the county level. To test this empirically, I start by running the following event study model:

$$\log(\text{2HFY Emp}_{\ell t}) = \sum_{\tau \neq 2019} \beta_\tau [\text{Prop. A or B Apps}_{\ell,2020} \times \mathbb{1}\{t = \tau\}] + \Gamma X_{\ell t} + \delta_t + \alpha_\ell + \varepsilon_{\ell t}. \tag{9}$$

Equation (9) serves several functions: for $\tau < 2020$ it provides balance tests for employment growth in county ℓ for the county-level exposure *Prop.* AB_ℓ, which is defined as the proportion of all H-2B applications filed by firms operating in county ℓ that were assigned to lottery groups A or B. For $\tau = 2020$, it is a county-level treatment effect of exposure to H-2B visa workers on total employment. I use quarterly county-level employment counts from the QCEW and average across calendar quarters two and three of a given year to get *2HFY Emp*$_{\ell t}$. When included, $X_{\ell t}$ is a vector of controls that includes a Bartik control for labor demand interacted with year fixed effects and H-2B intensity (2020 applications divided by total county 2019 2HFY employment) interacted with year fixed effects.[34]

Figure C2: The Reduced Form Impact of Local Lottery Luck on (Log) 2HFY Employment



Sources: Department of Labor Office of Foreign Labor Certification Performance Data, QCEW, CBP (for Bartik control).

Figure C2 presents results. As expected—given that firm-level random assignment implies county-level randomness in exposure to H-2B visa workers—I fail to detect evidence of pre-trends in county-level employment prior to 2020. In 2020, I find a detectable effect on county-level employment from H-2B visa lottery luck. Amid the exceptional economic environment presented by 2020, a county whose

[34] The Bartik control variable is constructed as: $\sum_k \frac{E_{\ell k,2019}}{E_{\ell,2019}} \times (\log(E_{k,2020}) - \log(E_{k,2019}))$, where E represents employment, k represents a 4-digit NAICS code, and variables are measured from the County Business Patterns (CBP).

employers were fully assigned to lottery groups A or B enjoyed 1% higher employment relative to counties whose employers were all assigned to groups C, D, or E.

How do these county level effects compare to the firm-level crowd-out estimates presented in Section C.6? To assess this, I estimate the following, analogous, county-level model:

$$\Delta\text{2HFY Employment}_{\ell,2020} = \alpha + \beta^{IV}_{county}[\text{I-129 Approvals}_{\ell,2020}] + \Gamma X_\ell + \varepsilon_\ell, \tag{10}$$

using *Prop. A or B Apps*$_{\ell,2020}$ as an instrument for total I-129 approvals at the county level. Table C14 presents results. As before, both the left and right hand side are scaled (divided by 2019 county-level 2HFY employment).

I find surprisingly high estimates, converging around 5–7 additional employees at the county level per H-2B I-129 approval. While the confidence intervals are relatively large, all of these estimates reject the null hypothesis that the effect is equal to the firm level effect at the 10% level, reinforcing the notion that spillover effects of H-2B workers were present in local economies during 2HFY 2020.

Table C14: Employment per I-129 Approval, County Level

	Outcome: Δ2HFY Employment$_{\ell,2020}$			
	(1)	(2)	(3)	(4)
I-129 Approvals$_{\ell,2020}$	7.88*	7.56**	6.53**	5.93**
	(4.37)	(3.37)	(2.97)	(2.65)
Observations (Counties)	1,136	1,136	1,136	1,136
First Stage F Stat	6.8	17.5	14.2	17.5
p-value: $H_0 : \beta^{IV}_{county} = \hat{\beta}^{IV}$	0.09	0.03	0.04	0.03
Controls		✓	✓	✓
State FE			✓	✓
Outcome Winsorized at 1-99% Level				✓

NOTE: Heteroskedasticity robust standard errors in parentheses. Control variables are a Bartik control for labor demand and a measure of H-2B intensity at the county level—2020 H-2B visa applications divided by 2019 employment.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.8 Industries and Occupations of 2020 H-2B Lottery Workers

Table C15: Top Five Industries and Occupations on 2020 Lottery Applications

A: Industry (NAICS)		Percent of Applications
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: Occupation (SOC)		Percent of Applications
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.