EXHIBIT 13

The Impact of Immigration on Firms and Workers: Insights from the H-1B Lottery[*]

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Abstract

We study how changes in the availability of highly educated, foreign-born workers impacts firm performance and recruitment behavior. We combine two rich data sources: 1) administrative employer-employee matched data from the US Census Bureau; and 2) firm-level information on the first large-scale H-1B visa lottery in 2007. Using an event-study approach, we find that lottery wins lead to increased firm hiring of college-educated, immigrant labor along with increases in scale and survival. These effects are stronger for small, skill-intensive, and high-productivity firms that participate in the lottery. We do not find evidence for displacement of native-born, college-educated workers at the firm-level, on net. However, this result masks dynamics among more specific subgroups of incumbents that we further elucidate.

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1   Introduction

The impact of immigration on receiving economies depends in large part on individual firms' recruitment decisions and subsequent performance. Yet, two major barriers complicate the study of how firms respond to changes in access to immigrant labor. First, immigrants tend to cluster in labor markets that feature high-productivity firms, which confounds the relationship between immigration and firm-level outcomes. Second, with some notable exceptions, firm-level data on foreign-born employment is scarce. Thus, most of the literature measures a firm's exposure to immigration at the labor market level rather than at the firm level, not distinguishing between the direct effect of immigrant availability to firms and the broader market conditions that affect firms regardless of whether they hire immigrants.

In this paper, we overcome both of these barriers in a particularly high-stakes setting: the hiring of foreign-born college graduates through the H-1B visa program. The H-1B program was designed to help U.S. employers recruit highly skilled foreign workers in specialized occupations for which they report difficulty finding domestic workers. Each applicant is required to have a qualifying job offer from a sponsoring firm. In this context, we study the impact of a firm's ability to hire the immigrants they intended to sponsor, using plausibly exogenous variation generated by the first large-scale H-1B visa lottery in 2007. We do so by merging employer-employee linked data from the US Census Bureau to a measure that closely approximates a firm's lottery success. Using this rich dataset, we find that lottery wins help firms increase their employment, revenues, and survival probabilities. While there are limited effects on college-educated natives, we find heterogeneous responses across firms and specific subgroups of incumbent workers.

We begin by constructing a proxy of a firm's success in the 2007 H-1B lottery. In 2007, for the first time, *all* applications for H-1B workers went through a lottery to determine which visa applicants were allowed to work in the United States. Through a Freedom of Information Act (FOIA) request from the United States Citizenship and Immigration Services (USCIS), we obtained a dataset on approved H-1B applications—including lottery winners—along with the Employer Identification Numbers (EINs) of associated employers. We combine this dataset with information on all intended applications for H-1B visas submitted by firms to the Department of Labor—Labor Conditions Applications (LCAs). We take several steps to ensure that our measure of intended applications closely tracks true lottery applications. Then, using these two sources, we construct our firm-level, lottery-induced shock to immigrant availability: the number of visas the firm won in the lottery divided by the number of visas the firm intended to apply for—the lottery "win rate."

Next, we merge our lottery measure to restricted access US Census Bureau economic data from the Longitudinal Employer-Household Dynamics (LEHD) program (Kerr et al., 2015). Our LEHD dataset contains the universe of employer-employee matched data from twenty-five

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US states from 2002 to 2011 and includes wages, employment histories, and workers' country of birth, among other variables. We augment the LEHD data with information on firm-level revenue, employment, and payroll from the Longitudinal Business Database (LBD), a comprehensive panel of non-farm, private sector US firms. The LEHD and LBD provide a unique opportunity to study how workers and firms in the United States respond to immigrant hiring restrictions. Our main sample consists of firms that participated in the H-1B lottery in 2007 and were successfully matched to our Census Bureau economic data. These 20,000 firms are not necessarily representative of all U.S. employers: they are disproportionately in the tech sector, have explicitly revealed a desire to hire foreign-born workers, and, by program design, have attested that they were unable to find qualified U.S. workers. Nonetheless, they are an economically significant group, collectively employing 16 million workers in 2006—roughly 13% of total nonfarm, private U.S. employment.

We proceed by conducting an event study analysis to understand the impact of the lottery win rate on firm performance and hiring. This empirical approach allows us to concisely evaluate whether our measure of lottery success is likely to be conditionally exogenous and examine its impact on firm-level outcomes. Importantly, we show that firms with more 2007 lottery success were not trending differently than firms with less 2007 lottery success prior to 2007 across several outcomes, including usage of the H-1B program itself. Quarterly data from the LEHD further allow us to show that even though the lottery took place in April of 2007, responses to lottery success largely started in the fourth quarter of 2007, precisely when the law allows workers tied to lottery-winning applications to begin working.

Turning to our main firm-level results, we first assess how total firm employment increases when a firm has success in the 2007 H-1B lottery. According to our estimates, one additional lottery win increases firm employment by 0.83 workers relative to a firm that loses the lottery, and this effect is persistent for at least five years after the lottery occurs. The employment effects are stronger for lottery-winning firms with less than ten employees, who see a full one-for-one increase in their employment relative to lottery-losing firms. Larger firms see much smaller effects on overall employment, suggesting that small firms might be more constrained in their ability to find alternate workers in the event of losing the lottery.

Next, we examine how employment composition at the firm changes after winning the lottery. We document a first-stage relationship: lottery winners increase their employment of foreign-born workers with college degrees—after 2007. However, this increase is not one-for-one: we estimate that each lottery win results in roughly 0.3 additional college-educated immigrant workers employed at the firm. This is a key contribution of our work, as previous studies of H-1B lotteries lack demographic information on workers, and assume that each win translates into one additional H-1B immigrant worker.[1] This incomplete adjustment may reflect several factors,

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[1] We use the term "immigrant" to describe any worker who is foreign-born, regardless of citizenship or residency status.

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including that lottery-losing firms were still able to hire immigrant workers through other channels, crowd-out of incumbent immigrant workers, or that sponsored workers ultimately accept jobs in other countries.

We find limited evidence that lottery wins impact a firm's employment of native workers. We estimate that each lottery win increases employment of native college workers by 0.1 employees, on net (with a 95% CI lower bound of -0.09). Across firm size classes, we similarly rule out crowd out effects on native college workers of more than 0.1 per lottery win with a high degree of confidence. While the utilization of native college graduates does not seem to respond significantly to additional H-1B workers, we do find a slight crowd-out of natives who are more similar to the H-1B immigrants such as young, college-educated, low tenure natives. We also find that hiring of non-college workers (of all nativities) tends to rise in response to lottery wins, consistent with increased demand for complementary workers. In fact, a majority of the 0.83 workers associated with each lottery win represents crowd-in of non-college workers.

We next evaluate how other firm performance indicators respond to winning the lottery. First, we find that the lottery has important implications for firm survival: firms that win all of their lottery applications are 2.5 percentage points more likely to remain active than firms that lose all of their applications. We also find expansions in firm revenues and payroll that coincide with the increase in scale found in our employment results. Finally, we find suggestive but statistically imprecise evidence that revenues per worker and average wages paid to the employees of winning firms increase in response to lottery wins.

In order to probe both the internal and external validity of our results, we obtained access to a different Census project with approval to use LEHD and LBD firm outcomes to analyze the short-run impacts of the more recent 2020 H-1B visa lotteries. While the availability of data in years after 2020 is more limited, a key advantage is that we can directly measure lottery wins and applications, without relying on the LCAs to construct the win rate. Results from the 2020 lotteries corroborate our 2007 findings: firms with greater lottery success exhibit increased utilization of college-educated immigrant workers, no significant crowd-in or crowd-out of native workers, and significant increases in firm scale and survival through 2022. These results suggest that potential measurement issues related to the LCAs are unlikely to drive our findings.

All told, our main firm-level results indicate that lottery wins enable firms to scale up without generating large amounts of substitution away from native workers. Plugging our estimates into a theoretical model with firms employing heterogeneous labor inputs yields an elasticity of substitution of 4.3 between college-educated immigrant and native workers.

While our main firm-level results provide new insights on how firms adjust scale and employment composition in response to the availability of immigrants, they may mask important heterogeneous effects across firms and incumbent workers. To dig deeper into these dynamics, we present two sets of additional analyses. First, we explore which firms respond the most

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to lottery luck. Firms that are skill-intensive, immigrant-intensive, pay high wages, and have higher labor productivity are more likely to expand in terms of employment, and even crowd-in native workers, after winning the lottery. We interpret these results as a sign that high-wage and high-productivity firms may be more dependent on workers with specialized skills who may only be available from abroad.

The employer-employee matched data in the LEHD offers a unique opportunity to differentiate between how the lottery impacts firm-level aggregates and how the lottery impacts *individuals*. We tackle this latter question by estimating individual-level difference-in-differences models that compare the career trajectories of incumbent workers at lottery-winning firms to those of similar workers at lottery-losing firms. We find distinct effects across groups of workers. On one hand, young, college-educated workers with low tenure at the firm experience a 4-5% wage increase when working at a lottery-winning firm, regardless of nativity. Similarly, non-college workers at winning firms experience wage gains of 3%. On the other hand, high-tenure, young, college-educated natives at lottery-winning firms experience a 5% wage reduction and a 3.6% higher likelihood of leaving the firm. Our results therefore indicate that it is this higher tenure, young, native college group that may be most substitutable with immigrants among incumbent workers. However, since this group only represents a small fraction of incumbent workers—a clear majority of incumbents seem to benefit from exposure to new H-1B coworkers.

Our work makes four main contributions. First, we are among the first papers to use the rich, employer-employee panel data from the LEHD to study the impact of immigrant hiring on firm-level decision-making. Second, we bring credible identification to this question by exploiting plausibly exogenous variation induced by the H-1B lottery. Third, we provide new evidence on the degree of substitutability between skilled immigrants and natives within the firm, a necessary parameter for understanding the welfare impacts of immigration. Fourth, we provide new evidence on how high-skilled immigration restrictions impact firm performance and incumbent worker career trajectories.

A nascent, growing literature studies the impact of firm-level immigrant hiring shocks on firm performance and labor-labor substitution (see, e.g., Amuedo-Dorantes et al., 2023a; Clemens and Lewis, 2022; Kerr et al., 2015), including a limited number of papers that have specifically used H-1B lotteries as a source of variation (Clemens, 2013; Dimmock et al., 2021; Doran et al., 2022; Glennon, 2023; Mandelman et al., 2024). Of particular note is Doran, Gelber and Isen (2022), who use the H-1B lotteries in 2005 and 2006 to evaluate the impact of immigrants on firm employment and patenting. These lotteries were held among the subset of applications that were received on the day that the H-1B cap was met in each year, ultimately covering 2,750 firms that applied for an H-1B visa. In contrast, we focus on the 2007 lottery, where H-1B visa applications immediately exceeded the cap and were therefore *all* placed into a random lottery. Our analysis covers 20,000 LBD-enumerated firms and 13,500 LEHD-enumerated firms applying for cap-subject H-1B visas in 2007. As such, we complement Doran, Gelber and Isen

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(2022) by analyzing a potentially more representative sample of H-1B employers.[2]

A key additional contribution of our work is that our data contains significant detail on the composition of employment within firms that has not been studied in prior H-1B literature. We are therefore able to directly examine how hiring an H-1B immigrant worker impacts the employment of other worker groups within the firm, including similarly skilled native workers. To our knowledge, the only other paper using the LEHD to study the impact of skilled immigration on firms is Kerr et al. (2015), who study how changes to the H-1B cap affected employment composition within a sample of 319 large firms. We instead exploit lottery variation and use the near-universe of firms that intended to hire H-1B workers in 2007 and 2020.

Other papers have focused on the impact of the H-1B program on alternative outcomes such as the selection of skilled immigrants (Kato and Sparber, 2013; Mayda et al., 2018; Shih, 2016), multinational activity (Glennon, 2023; Morales, 2023), entrepreneurship (Dimmock et al., 2021; Mandelman et al., 2024), occupation choice (Bound et al., 2018; Khanna and Morales, 2025), innovation (Choudhury and Kim, 2019; Hunt and Gauthier-Loiselle, 2010; Kerr and Lincoln, 2010; Khanna and Lee, 2019), local employment opportunities in H-1B-related occupations (Peri et al., 2015a), productivity (Peri et al., 2015b), and cross-border effects on trade and firm performance in Canada (Brinatti and Guo, 2023).[3] We contribute to this literature by looking at the hiring behavior and composition of employment of individual US firms, as well as the adjustment of individual worker career paths in response to H-1B worker inflows.

Finally, other papers have also combined rich microdata and causal identification to study the impact of market-level immigration shocks on firms and workers in other contexts (see, e.g., Arellano-Bover and San, 2023; Beerli et al., 2021; Brinatti and Morales, 2023; Brinatti and Guo, 2023; Clemens et al., 2018; Dustmann and Glitz, 2015; Dustmann et al., 2016; Egger et al., 2022; Gray et al., 2020; Mahajan, 2024; Mayda et al., 2023; Mitaritonna et al., 2017; Muñoz, 2023; Orefice and Peri, 2020; Signorelli, 2024; Silliman and Willén, 2024). Results from these studies answer a somewhat different question than ours: while we study the effect of a firm-level shock to immigrant availability for firms demanding immigrant workers, these studies examine unexpected immigration into an area. Nonetheless, most of the results from these studies broadly align with ours. Firms that do hire immigrants expand more in terms of scale and with either negligible or small positive impacts on native employment, while results for individual incumbent workers are mixed. Our setup focuses on the hiring of high-skill immigrants and has the distinction of combining three main ingredients: 1) having a large set of treated firms (the universe of H-1B applicants), 2) using rich employer-employee matched data from the United

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[2]Doran, Gelber and Isen (2022) also analyze the effect of 2007 lottery wins on employment and patents as a robustness check on their 2005-06, using a methodology that follows the broad contours of ours but with some important differences. We discuss how we compare results in Section F.

[3]The H-1B program also has the potential to impact higher education (Kato and Sparber, 2013). Literature finds international students, many of whom eventually apply for an H-1B, cross-subsidize domestic students at US universities and increase college selectivity (Bound et al., 2021; Chen, 2021; Chen et al., 2020; Shih, 2017).

States, and 3) exploiting a firm-level source of exogenous variation, the H-1B lottery.

While our analysis yields new insights into how a set of high-productivity firms respond to immigrant hiring constraints, it is important to acknowledge certain limitations. Most notably, we cannot speak to general equilibrium effects or the overall impact of immigration policy on the US. Our research design identifies within-firm relative responses and does not capture adjustments across firms or labor markets, which might include spillovers to firms and sectors that do not use immigrant labor or individual workers that are not incumbent to H-1B hiring firms (e.g., Dustmann et al., 2016).

As a final point, we clarify the interpretation of our identified effects. While we describe our empirical variation as capturing the effects of gaining access to H-1B workers, it can equivalently be viewed as measuring the consequences of losing access for firms that would otherwise have hired them. As such, our findings may also be viewed as quantifying the costs to firms of being denied specific high-skill foreign workers, rather than capturing the full labor market impact of the H-1B program or high-skill immigration more generally.

2 Overview of the H-1B Lotteries

The H-1B visa was created in 1990 to provide authorization for college-educated foreign nationals to work in specialty occupations in the United States. Since fiscal year (FY) 2004, H-1B annual quotas for new employment at for-profit employers have remained fixed at 65,000 visas under the Regular Cap and 20,000 visas under the Advanced Degree Exemption (i.e., the ADE Cap, for applicants with a master's degree or higher from a US educational institution). These quotas have been binding in each year since FY 2004.[4]

H-1B visas are allocated on a first-come, first-served basis. In the mid-2000s demand for H-1B visas started growing. Figure 1 displays the number of days to reach the H-1B cap from the start of the filing period for each application season, denoted by calendar years 2001-2019. From 2003 application period onward, the cap was reached in successively fewer days from the start of filing, consistent with expansionary economic conditions prior to the Great Recession.

On the first day of the filing period in 2007 (i.e., April 2nd, 2007), USCIS received an unprecedented number of applications that already exceeded the Regular Cap. USCIS held a lottery to distribute all 65,000 visas among the 123,480 applications received between April 2-3, 2007. A lottery was not held to distribute the ADE Cap of 20,000 visas, as the volume of ADE applications remained below the cap until the end of April.[5] This marked the first time that the

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[4]Universities and other nonprofit/government entities are exempt from these H-1B quotas

[5]In 2005, procedures were updated to allow for randomized selection of petitions under two scenarios. First, petitions received on the "final receipt date"—defined as the date in which the number of petitions exceeds the cap—would be subject to a randomized lottery. Second, if the final receipt date is the first day of the application period, the entire cap would be randomly allocated across applications received on the first two days of the filing period. See *70 FR 23775, May 2005*: https://www.govinfo.gov/content/pkg/FR-2005-05-05/pdf/

entire Regular Cap was distributed by random lottery. The USCIS held small pilot lotteries in 2005 and 2006 to assess feasibility on a sample of just under 3,000 firms, as studied in Doran, Gelber and Isen (2022).[6]

Figure 1: Days-in-Filing for the H-1B Visa Cap, 2001-2019
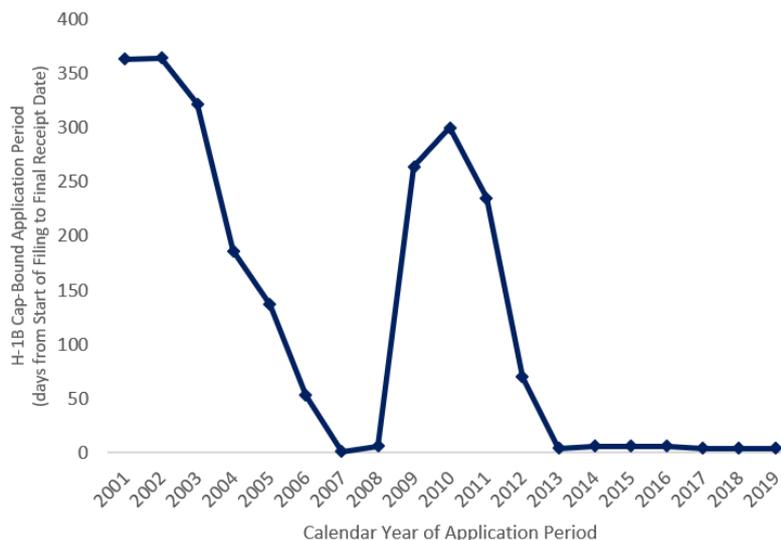
Note. Figure illustrates the number of days that it took for applications for new, cap-subject H-1B visa workers to meet the cap. Final receipt dates are provided by the USCIS, and we calculate days in filing by counting the number of days from the start of the filing period (generally April 1 or April 2) to the final receipt date.

Hence, the 2007 lottery created an unexpected shock to all firms that applied for H-1B workers. Compared to later lotteries in 2008 and in each year since 2013, the 2007 lottery was both much less anticipated and also less complicated. In later years, lotteries were separately held for the Regular Cap and the ADE Cap, and losing applicants in the ADE Cap lottery participated in the Regular Cap lottery.

Finally, it is important to note that other visa classes may accommodate skilled workers, but these are much smaller in size than the H-1B program (Shih, 2024).[7] Interviews of 34 firms revealed that employers facing lower H-1B quotas secure skilled workers through other, "sometimes more costly" means – a pattern which has been corroborated in empirical studies (Kato and Sparber, 2013; Shih, 2016).[8] This has two implications: 1) whether lottery wins lead to greater skilled immigrant hiring is ultimately an empirical question—we are the first to estimate this relationship in Section 5.2; and 2) even in cases where the lottery does not affect *who* is hired, the additional costs required to overcome lottery losses may still impact measures of firm performance like survival and revenue generation, which we study in Section 5.3.

FR-2005-05-05.pdf.

[6]The final receipt dates for the FY2006 Regular visa cap, the FY 2006 ADE visa cap, the FY 2007 Regular visa cap, and the FY 2007 ADE visa cap, were August 10, 2005, January 17, 2006, May 26, 2006, and July 26, 2006, respectively.

[7]These include, for example, the L-1, TN, E-3, and H-1B1 visas.

[8]See report GAO-11-26 here: https://www.gao.gov/products/gao-11-26.

2.1 The H-1B Application Process

To understand our empirical measurement of firm success in the lottery, we clarify the timeline of the H-1B application process. Employers submit H-1B visa applications on behalf of workers they wish to hire. First, firms must file a Labor Conditions Application (LCA) with the Department of Labor, in which they attest that H-1B workers will not harm incumbent workers and also provide information about the job, the work start and end dates, the work location, and the associated salary.

After LCA approval, employers may submit an H-1B visa application. The primary document of this application is the I-129 form that provides information about the employer, demographic and other personal information about the worker, the occupation, the work start and end dates, and the wage/salary. The I-129 form is tied to a specific worker, and firms must pay filing fees that range from $2,000 to $10,000, not including attorney fees, which can be substantial.[9]

Figure 2 visually depicts the H-1B application timeline for 2007. LCAs were filed in Q1 2007, which we explain in greater detail in Section 4. USCIS began accepting H-1B applications (i.e., I-129 petitions and associated documents) on April 2, 2007. The lottery was then held for those applications received between April 2 and 3.

An important feature crucial to our analysis is that only data on lottery winners were retained. USCIS returned the applications of lottery losers without further processing, and hence there is no data available on the exact number of applications (i.e., I-129 petitions) submitted by each firm. We describe how we overcome this using LCA data to approximate applications in Section 4. We also validate our main results from the 2007 lottery using the 2020 lottery, where we do have data on applications that lost the lottery.

Finally, for lottery winners, October 1, 2007 marked the earliest date the H-1B worker could begin working. Hence, Q4 2007 (which is equivalently Q1 of FY 2008) is the earliest that workers selected in the lottery could actually start working for the firm.

3 Data Description

3.1 Administrative Employer-Employee Matched Data

We obtain access to a rich collection of employer-employee level data from the US Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) for the period 2002-2011. The LEHD contains the universe of individual worker histories and their associated firms, for which we were granted access to records for twenty-five US states.[10] These states accounted for

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[9]In addition, a complete H-1B application must also include a copy of the approved LCA, the formal job offer letter, and other supporting documents (e.g., educational transcripts, etc.).

[10]The selection of states is determined between Census and individual states, and is not at the discretion of the researchers. We requested access to records from all states, and were approved to access LEHD data from the

Figure 2: H-1B I-129 Application Timeline
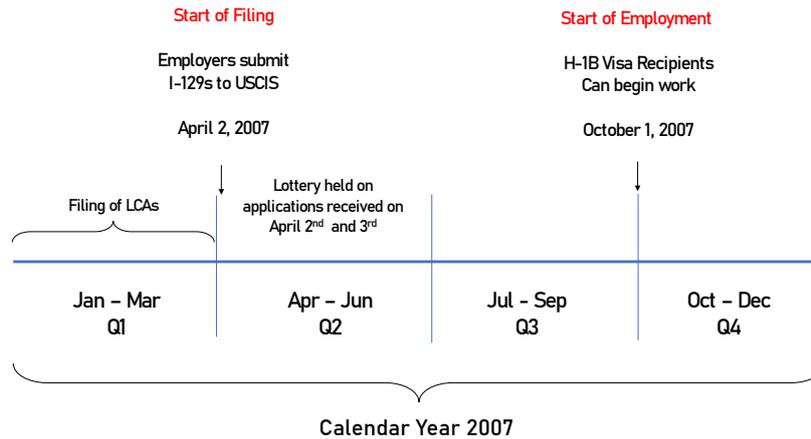
Note. Figure illustrates the application timeline for the 2007 application season. Quarters (Q1-Q4) correspond to the 2007 calendar year.

53% of the H-1B I-129 petitions for new employment, 48% of total college-educated workers, and 52% of total college-educated immigrants in the United States during our sample period (2002-2011).[11]

The LEHD data contains information on quarterly earnings for each individual-employer pair, taken from employer-reported information to state unemployment agencies. The data also includes individual characteristics such as date of birth, gender, race, education level, and place of birth. Many of these, such as place of birth, are taken from administrative sources like the Social Security Administration. In some cases—particularly for education—the data is imputed by the Census Bureau using observed education for individuals who participate in the American Community Survey. The data do not contain information on an individual's occupation. More details on the LEHD can be found in Vilhuber (2018).

We complement LEHD data by linking it to the Longitudinal Business Database (LBD) at the firm-level. The LBD consists of the universe of private sector establishments in the United States, with information on employment, payroll, revenues, firm age, industry, and exact location. Furthermore, a majority of LBD firms can be matched to firm-level revenues annually. The LBD includes ownership linkages between establishments, which allow us to aggregate employment and payroll to the firm-level. While much previous work on the H-1B program has been limited to the sample of publicly held firms using databases like Compustat (e.g., Mayda et al., 2023), the LBD allows us to look at the near-universe of firms that participate in the program, and the LEHD allows us to examine the near-universe of individual workers in the

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following 25 states: Arizona, Arkansas, California, Colorado, District of Columbia, Delaware, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Missouri, Montana, Nevada, New Mexico, North Dakota, Oklahoma, Oregon, Pennsylvania, Tennessee, Texas, Washington, and Wyoming.

[11]Calculations done with I-129 records for FY 2003-2012 and Public Use American Community Survey for 2002-2011.

twenty-five states for where we obtained access.

3.2 H-1B Data

Our H-1B visa information comes from two different sources. First, we obtained individual-level H-1B visa application records from the I-129 form through a Freedom of Information Act request from the US Citizenship and Immigration Services (USCIS) for the period 2001-2011. These data include detailed demographic information about the worker, firm-level information, and information about the job position. Worker-level information includes the country of origin, age, degree type, and current visa. Firm-level information include firm name, address, and federal tax identification numbers (EIN). Information about the job position include the occupation, salary, and work start and end dates. As noted before, USCIS only retained information of those applications that won the lottery. Hence, the I-129 records are useful in measuring H-1B lottery winners, as explained in Section 4.

A second source for H-1B visa applications are records of Labor Condition Applications (LCA) from the Department of Labor's Office of Foreign Labor Certification. These records provide information from each application, detailing the date of filing, application status, employer name and address, job position, job code, number of positions requested, work start and end dates, work locations, wage/salary associated with the position, and the prevailing wage associated with the area-occupation. We use the LCAs to construct a proxy for lottery-subject visa applications, as described in Section 4.

We merge the LEHD data with the I-129 and LCA records in two stages. In the first stage, I-129 records are matched to LCA records by first searching for exact matches on firm name and address. All LCA records are retained to keep those with zero lottery wins. Remaining unmatched I-129 records are then linked to LCAs using a fuzzy matching approach developed by Flaeen and Wasi (2015) as follows: fuzzy matches initially are searched over name–street–city–state–zip, then name–city–state–zip, then name–city–state, then name–state, and finally name only. This procedure results in 98% of I-129 records being matched to a corresponding LCA record, and 81% of the linked I-129-LCA records have EINs (to be used in the second stage of matching).

In the second stage, we linked I-129-LCA data to US Census data via the federal tax identification number (EIN) that is common in both datasets. Records remaining unmatched—-including records with zero lottery wins—are then linked via fuzzy matching approach based on the same successively less stringent manner as above. Once completed, we manually inspected the list of companies and removed poor matches to ensure that the quality of the match is high.

4   H-1B Lottery Success: Measurement & Identification

The 2007 H-1B Regular Cap lottery (henceforth, lottery) generated random, exogenous variation in the ability of firms to hire skilled immigrants. Our measure of firm-level lottery success divides the number of successful lottery applications of a given firm by the total number of likely-lottery applications, as defined in Equation (1):

$$\text{Win Rate}_j = \frac{\text{Lottery Wins}_j}{\text{Likely-Lottery Applications}_j}. \tag{1}$$

Calculating the number of lottery wins for each firm can be achieved with high precision using granular applicant information contained in our USCIS I-129 records. We classify winning lottery petitions as those received between April 2-3, 2007, from private sector firms (excluding universities and other non-profit institutions). We exclude ADE applications by removing individuals on an F-1 visa who possessed a master's degree or higher at the time of application. Finally, we retain only those applications that requested an employment start date on or after October 1, 2007 —the first day that winning H-1B workers could legally commence employment. These restrictions result in 64,723 winning I-129 petitions, remarkably close to the actual cap of 65,000.

However, while the USCIS kept these detailed records on successful lottery applications, it did not keep records on unsuccessful lottery applications: losing applications were returned to sender without any record. Hence, measuring each firm's exact number of lottery applications is not possible as USCIS I-129 administrative records only contain data on lottery winners. Thus, in Section 4.1, we turn to several steps to construct our denominator for the number of likely-lottery applications at the firm-level.

4.1   Proxying Applications

One potential proxy for the number of lottery applications—with precedent in the H-1B literature—is the number of approved LCAs filed by a firm prior to April 1, 2007. However, using raw counts of LCAs as a proxy for total lottery applications will induce measurement error in win rates. LCAs are filed prior to the I-129 petition, carry no explicit filing fee, and do not require firms to eventually submit an I-129. Hence, the actual number of I-129 applications—our ideal denominator for the win rate—is less than or equal to the number of LCAs. In other words, win rates calculated using LCAs are weakly smaller than the ideal win rate using actual I-129 applications.

Such measurement error might induce an endogenous correlation between win rates and firm outcomes. First, this mismeasurement could reflect a fixed firm-trait. For example, if large firms with in-house legal teams always file many excess LCAs, win rates constructed using LCAs as

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a proxy for applications would be correlated with firm size. Second, such error may arise from a pre-trend or pre-lottery shock to firm outcomes. For example, after having submitted their LCAs, a firm might experience a negative shock that precludes submission of the I-129 application. Under this scenario, win rates using LCAs would be endogenously related to pre-lottery changes in firm outcomes.

We develop several refinements and an empirical strategy to more accurately proxy applications and reduce the scope for endogeneity, thereby improving upon prior research (Dimmock et al., 2021; Glennon, 2023; Peri et al., 2015a) which has only relied on simple counts of LCAs. We then demonstrate through balance tests, evaluation of pre-trends, and other checks, that our corrected measure ($Win\ Rate_j$) captures plausibly exogenous variation in win rates within the confines of our difference-in-differences estimation strategy.

First, we consider lottery-bound applications to be among the set of LCAs filed between March 1 and April 3, 2007. This is due to a feature known as "pre-dating" of LCA applications (Peri et al., 2015a), where employers seeking to hire H-1B workers submitted LCAs for approval in advance of the H-1B application submission date (April 2, 2007).[12] Our analysis of the pattern and timing of LCA filings reveals an unusually large volume of LCA filings beginning on March 1 that is consistent with firms preparing for the start of the cap-subject H-1B filing season in April.[13]

Second, we require that LCAs in this date range were filed six months before the stated employment start date on the LCA (i.e., employment start dates between August 1 and October 3). This is because rules limit filing of LCAs to at most 6 months before the proposed work start date. Employers filing predated LCAs in March, for example, would use the full 6 month duration and list work start dates in September. Because H-1B winners could not legally begin working in October, this meant that firms filing in March were willing to forgo up to 1 month of work duration (out of the 3-year total) for their H-1B workers. LCAs filed less than 6 months from start date are usually employers exempt from the quota (e.g. universities may file LCAs in March with start dates in July for newly hired academic employees).

Third, we remove LCAs filed during this period that were for non-lottery H-1B applications by taking advantage of the fact that other non-lottery H-1B applications also must be accompanied by an approved LCA. Hence, we calculate the total *non-lottery* I-129 filings of each firm (e.g., renewals, or changes of status, such as upgrading occupations within the firm, etc.) between March 1 and April 3 and subtract this number from the count of LCAs between the same period.[14]

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[12]We detail these issues related to LCA "predating" in Appendix Section B.

[13]Results from using alternative windows (e.g. Jan 1-April 3 or Feb 1-April 3) do not substantively change our findings and are available upon request.

[14]Rules stipulate that each I-129 application requires a previously successful, corresponding LCA application. This applies to renewals, change of status, and other types of applications to amend H-1B petitions. We remove any observations for whom Likely-Lottery Applications$_j \leq 0$ after this step.

Finally, we address remnant mismeasurement by identifying and removing observations whose proxied applications (i.e., the count of LCAs after completing the three refinements above) remain improbably small or large given the number of lottery wins that we observe. This "filtering" procedure exploits information on the true win probability for any application, which USCIS publicly reported to be $\frac{65,000}{123,480} = 0.5264$. The number of winning applications for a given firm follows a binomial distribution with success probability 0.5264 and number of trials given by the number of applications.

Given this, we remove observations for which the probability of observing the pair of our proxied application count and number of lottery wins is less than 1%.[15] To clarify by way of example, the probability of observing a firm with 0 lottery wins and a proxied number of applications equal to 7 (or more) would be less than 0.01 when the binomial success probability is 0.5264. Our filtering procedure would therefore remove any observation with 0 observed wins and 7 or more proxied likely-lottery bound LCAs.

4.2   Win Rate Summary Statistics

Table 1 summarizes our measurement of *Win Rate$_j$*. Columns (1) and (2) of Table 1 are constructed at the Employer Identification Number (EIN) level using only the linked I-129-LCA data.[16] Column (3) contains disclosed statistics from US Census data, reported at the "firmid" level —the Census Bureau's internal firm identifier. "Firmid" is more aggregated than EIN, representing the highest possible organizing unit for business entities.[17]

Several key points emerge from Column (1), which is tabulated from our *unfiltered* sample of linked I-129-LCA data. Despite our initial restrictions – only counting LCAs from March 1-April 3 and removing non-lottery I-129 filings from LCA counts – our proxy for applications *Likely-Lottery Applications$_j$* still overestimates the true number of lottery applications reported by USCIS (182,951 vs 123,480), resulting in an estimated overall fraction of winning applications of 30% (i.e., 55,565/182,951) instead of 53%. The average win rate calculated across EINs (46%) is much larger than this overall fraction of winners (30%). These differences suggest that without further refinement, measurement error in win rates may be sizable.

The utility of our filtering method can be seen by comparing Column (2) to Column (1). Removing probabilistic outliers greatly reduces the average application count (e.g., from 9 in Column (1) to 4 in Column (2)), while only slightly reducing average wins. Thus, filtering brings the average win rate across firms (47%) into parity with the overall fraction of winners

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[15]Further details of our filtering strategy and its utility can be found in Appendix Section C.

[16]In order to avoid excessive disclosure avoidance review, we report these exclusively from I-129/LCA data at the EIN level.

[17]The variable "firmid" is the Census Bureau's internal identifier for "firms," which represents the highest possible organizing unit for a business entity. Firms may comprise of one or multiple different establishments. The EIN, in many cases, does not represent the firm, as firms may often be associated with various EINs, each representing subdivisions of the business.

Table 1: Estimated FY 2008 Lottery Characteristics

	Sample		
	(1)	(2)	(3)
	EIN sample	EIN sample (filtered)	LBD firmid sample (filtered)
Panel A: *Lottery Wins$_j$*			
Mean	2.77	1.89	1.62
Std. Dev.	(32.15)	(11.68)	(5.52)
Total	55,565	35,903	32,170
Panel B: *Likely-Lottery Applications$_j$*			
Mean	9.11	4.11	3.68
Std. Dev.	(93.21)	(21.89)	(10.81)
Median	1	1	–
Total	182,951	78,080	73,180
Panel C: *Win Rate$_j$*			
Mean	0.46	0.47	0.41
Std. Dev.	(0.42)	(0.42)	(0.42)
Panel D: Other Lottery Characteristics			
EIN Count	20,072	18,963	–
Prop. EINs with *Likely-Lottery Applications$_j$* = 1	0.52	0.55	–
Prop. EINs with *Wins$_j$* = 0	0.35	0.36	–
Prop. EINs with *Lottery Wins$_j$* = 1	0.44	0.46	–
Prop. EINs with *Lottery Wins$_j$* > 1	0.21	0.18	–

Note. Column (1) presents summary statistics for the full sample of likely-lottery participants measured with our I-129 data. Column (2) presents the results for the sample of firms with valid win rates after the filtering process explained in Section C. Columns (1) and (2) consider a "firm" as a unique EIN, which stands for Employer Identification Number, and are calculated using only our I-129 data. Column (3) calculates sample statistics for LBD firms, which is the firm identifier at the Census. EINs match with but do not correspond exactly to the LBD firm IDs used in our analysis. There are often multiple EINs per LBD firm ID. With the exception of this table, an LBD firm ID is generally what we mean when we reference firm j. The EIN count in Column (2) is lower than our firm count from the LBD (20,000) because we find more matches between firms that had *Likely-Lottery Applications$_j$* > 0 but *Lottery Wins$_j$* = 0 in our internal matching procedure between the DoL data and the Census data.

within this sample (35,903/78,080=46%)—a key indicator that our win rates from the filtered sample are more likely to capture lottery variation. Column (3) similarly displays disclosed statistics from our primary analysis sample after filtering and also after aggregating EINs to the "firmid" level (hereafter, firms). Compared to Column (1), average applications are also smaller, and the average win rate in this filtered sample (41%) is once again nearly identical to the overall share of wins (32,170/73,180≈40%).[18]

In Panel D, we observe skewness in both wins and applications, which helps contextualize the results in Section 5. While the average number of wins is almost two and the average number of applications is four, standard deviations are large, indicating the presence of a small number of firms that file many applications. Panel D shows that roughly 50% of EINs only file one application, and hence either win zero or exactly one H-1B petition. The share winning more than one is less than 20%. Given that our regressions are conducted at the firm-year level and do not weight by firm size, much of our identifying variation comes from firms that apply for

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[18]We are unable to report EIN level statistics in Panel D for Column (3) since the unit of analysis is at the firmid level.

only one H-1B worker.

4.3   Difference-in-Differences Event Study Approach

We first detail how we operationalize our empirical approach and then discuss identification before we turn to results. We estimate continuous difference-in-differences event study models specified by Equation (2):

$$y_{jt} = \sum_{\tau \neq b} \beta_\tau \left[\text{Win Rate}_j \times \mathbb{1}(\tau = t) \right] + \Gamma X_{jt} + \alpha_j + \alpha_{kt} + \varepsilon_{jt}, \tag{2}$$

where j denotes the firm. For LEHD outcomes available at a quarterly frequency, t indexes a calendar quarter, in which case the omitted period is $b = 2007Q1$ (the quarter before the lottery took place). For outcomes available at an annual frequency, t indexes a calendar or fiscal year, in which case $b = 2006$. Outcomes y_{jt} include firm-level variables such as revenues, total employment, and employment of specific subgroups. In most cases, we apply the inverse hyperbolic sine transformation to outcomes to approximate log changes while retaining zeros.[19]

Firm fixed-effects (α_j) account for time-invariant differences across firms that might correlate with win rates and outcomes, including a firm's tendency to apply for excess LCAs. Four-digit industry-by-time dummies (α_{kt}) help absorb aggregate shocks (e.g., the Great Recession) that affect all firms similarly. Finally, X_{jt} controls for pre-lottery firm employment interacted with time fixed effects to restrict comparisons to firms of similar sizes.[20] These control variables play two roles: 1) absorbing the potentially large amount of residual variation specific to industry and firm size that was unrelated to the lottery but nonetheless created by the Great Recession; 2) removing the influence of systematic, time-varying differences in the propensity to apply for excess LCAs that are either correlated with initial size or learned within industry.

Equation (2) directly helps us assess concerns regarding the win rate proxy *Win Rate$_j$*. Generally, we can use estimated $\hat{\beta}_\tau$ for pre-lottery τ to evaluate whether firms with different win rates were following similar trends before the lottery. When estimating Equation (2) at a quarterly frequency, we can be even more precise: $\hat{\beta}_\tau, \tau \leq 2006Q4$, represents pure balance tests, while $\hat{\beta}_\tau, \tau \in \{2007Q2, 2007Q3\}$, allows us to examine whether firms began reacting to the results of the lottery in the two quarters between the lottery itself and the H-1B worker start date of

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[19]In Section D.1, we show that our results are robust to alternate ways of transforming our key outcome variables following the suggestions of Roth and Chen (2022). We prefer a log-like transformation in our outcomes because it allows for common scaling across LBD outcomes (measured across all establishments in a firm) and LEHD outcomes (measured across establishments in a firm that are located in one of our 25 approved states) and because it reduces noise in y_{jt}.

[20]We interact the inverse hyperbolic sine of a firm's employment in March 2007 (pre-lottery) interacted with time-period indicators.

October 1, 2007 (the first day of 2007Q4). While we expect immediate post-lottery effects[21], the direct impact of hiring an immigrant worker through the H-1B program should be most reflected in $\hat{\beta}_\tau, \tau \geq 2007Q4$. We also note that by measuring the difference in outcomes between 2006Q4 and 2007Q1, $-\hat{\beta}_{2006Q4}$ presents our most direct test of whether firm shocks that occurred between LCA filing and I-129 application submission—which would have had to occur in 2007Q1—may be driving our results.

The other utility of Equation (2) is that it allows us to examine post-lottery dynamics in detail. We are particularly interested in whether the lottery generates a permanent advantage for winners or whether such an advantage dissipates. When estimates from Equation (2) are roughly in line with a one-time, permanent jump in a secondary outcome, we sometimes present estimates from a standard continuous difference-in-differences specification:

$$y_{jt} = \beta \left[\text{Win Rate}_j \times \text{Post}_t \right] + \Gamma X_{jt} + \alpha_j + \alpha_{kt} + \varepsilon_{jt}, \tag{3}$$

where the coefficient of interest, β, measures the impact of the win rate on firm-level outcomes for the post-lottery period relative to the pre-lottery period. Here, LEHD outcomes are measured in calendar Q4 of a given year, and t always indexes a year. In most specifications, $Post_t$ is therefore an indicator for $t \geq 2007$.[22] Estimates from Equation (3) help us paint a more complete picture of the lottery's effects in a concise way while avoiding excessive disclosure review burden on the US Census Bureau.

We limit the sample to firms that apply for at least one lottery-subject visa application in 2007 according to our proxy and that survive the outlier filtering strategy laid out in Section 4.1. In the end, we can analyze roughly 13,500 firms using the LEHD and roughly 20,000 firms using the LBD in fully balanced panels from 2002-2011. The former sample is restricted to lottery applicants and their establishments operating in at least one of the twenty-five states in our LEHD data listed above but allows us to analyze employment of specific subgroups. The latter sample contains our full sample of lottery applicants in 2007 but only allows us to analyze total firm employment, total firm payroll, total revenues, and other outcomes that can be generated from these three basic measures. These panels are fully balanced, because we retain inactive firms by changing missing entries to zero values (e.g., if a firm exits, we give it a value of zero employment for the remaining years in the sample and keep it in the panel).

4.4   Identifying Assumptions

Causal identification in our setting requires that our win rate measure, conditional on covariates specified above, is unrelated to other unobserved determinants of firm employment and

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[21]Lottery results were known by May for most firms. Winning firms may have immediately undertaken other investments.

[22]The lone exception is LBD-measured employment, which is measured in March of a given year. For this outcome, $Post_t$ is therefore an indicator for $t \geq 2008$.

performance. Crucial to this is whether our corrections to the win rate sufficiently mitigate potentially endogenous measurement error.

As stated earlier, excess LCA filing that is reflective of a fixed-firm trait is accounted for by firm fixed effects. The difference-in-differences design effectively eliminates any confounding influence from fixed factors, like firm size. Additionally, we find no evidence of pre-lottery shocks or pre-trends that cause firms to submit fewer H-1B applications than their LCAs filed. Throughout the presentation of results in Section 5, our event study analysis finds no significant pre-trends across the full range of our outcome variables in the years or quarters leading up to the 2007 lottery. Key outcomes do not respond until Q4 2007, when winning H-1B workers are legally allowed to begin working at firms.

We also perform more targeted balance checks to assess the scope for pre-lottery shocks by unconditionally regressing pre-lottery changes in firm outcomes on their win rate. We measure changes in outcomes in the years prior to the lottery, but also in the first and second quarters of 2007, between when LCAs are filed (i.e., January-March 2007) and when H-1B applications must be submitted (April 2007). The results of these balance checks are presented below, in Table 2.

Table 2: Pre-lottery Balance Test for Key Outcomes

	Δ Employment LBD March 06 - March 07	Δ Pay LBD March 06 - March 07	Δ Average Wage LBD March 06 - March 07	Δ Employment LEHD Q1 06 - Q1 07	Δ Employment LEHD Q1 07 - Q2 07
Win Rate$_j$	0.001	-0.006	-0.059	0.023	-0.004
	(0.032)	(0.149)	(0.106)	(0.086)	(0.012)
N Obs	20,000	20,000	20,000	13,500	13,500

Note. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. These are cross-sectional regressions of our measured win rate on growth rates of key outcomes, with no other controls included. Number of firms is rounded as per the US Census Bureau's disclosure rules. Columns 1-3 present the results for the growth rate in employment, payroll, and wages between March 2006 and March 2007, as measured by the LBD. Column 4 regresses the win rate on the employment growth rate between Q1 of 2006 and Q1 of 2007 for the LEHD sample of firms. Finally, Column 5 regresses the win rate on the growth rate in LEHD employment between Q1 2007 and Q2 2007.

Columns (1)-(3) examine the growth rate in employment, payroll, and average wages (payroll per worker) from March 2006 to March 2007 (just prior to when the lottery occurred). Column (4) examines the change in employment from Q1 2006 to Q1 2007 from the LEHD sample. Finally, Column (5) uses an outcome uniquely available to us through the LEHD: employment changes between Q1 2007, when LCAs are filed, and Q2 2007, when H-1B I-129 applications are filed.

Finally Appendix C shows that our filtering correction reduces the negative correlation between win rates and applications that arises because applications are on average too large, and reduces left-skewness apparent in the unfiltered distribution of win rates due to extra mass in small win rates (i.e., since applications again are too large, win rates are too small, on average).

Taken together, these checks support our identification strategy, and our corrections for measurement error help reduce the scope for endogeneity bias. We now turn to our results on H-1B hiring and assess whether greater success in the lottery translates into increased H-1B hiring.

5   Firm-Level Results

5.1   H-1B Hiring

We begin the analysis by testing whether our measure of lottery success is consistent with the expected firm-level responses in I-129 approvals for new employment and employment renewal. A firm that is successful in the lottery should have a pronounced, mechanical increase observed I-129 approvals for new employment during 2007. Further, if successful firms exercise their option to renew H-1B workers, we expect to see higher I-129 records for renewal three years later in 2010. Perhaps most importantly, if we have isolated lottery variation, we should not expect to see significant estimates of $\hat{\beta}_\tau$ for new employment or renewals prior to 2007.

Figure 3: Effect of Lottery Win Rate on Granted H-1B Petitions

(a) New Employment   (b) Renewals
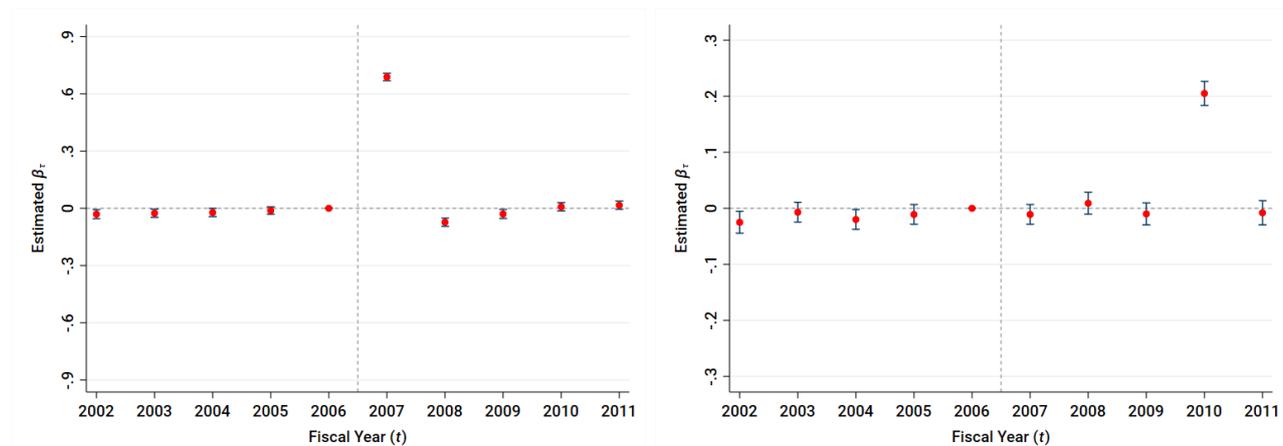
Note. See Equation (2) for specification, where t indexes a fiscal year and $b = 2006$. We plot the estimated coefficients $\hat{\beta}_\tau$ and their 95% confidence intervals. Vertical dashed line separates pre-lottery years (balance tests) from post-lottery years (treatment effect estimates). Omitted period is 2006. Number of firms is roughly 20,000 (rounded as per the US Census Bureau's disclosure rules). Standard errors clustered at the firm-level. The dependent variables are the inverse hyperbolic sine of the count of I-129 petitions for new employment (Panel a) or renewals (Panel b) filed in year t for workers starting in fiscal year $t + 1$. These data are from the USCIS. Regressions include firm fixed effects, industry-time fixed effects, and the inverse hyperbolic sine of employment in March 2007 interacted with time fixed effects.

Figure 3 presents estimates from Equation (2) for I-129 approvals and provides several pieces of validation for our approach. First, it shows negligible divergence in pre-trends between winning and losing firms in I-129 approvals for new H-1B workers. Interestingly, the expected, mechanical spike in 2007 is followed by small drops in I-129 approvals for new H-1B workers

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in 2008 and 2009, possibly indicating that 2007 lottery losers were more motivated to apply for H-1B workers in subsequent years. Meanwhile, Figure 3b shows that winning firms are substantially more likely to have I-129 records for H-1B employment renewals in 2010. We do not find an effect on renewals in any other year. Given that renewals are not subject to the lottery, we view these precise zeros as further corroboration of our identifying assumptions. Our results indicate that a firm that wins all of their lottery-subject H-1B applications, see 70% more new approved H-1B visa applications in 2007 and submits 20% more H-1B visa renewals in 2010 when compared to a firm that loses all of their lottery-subject applications.

5.2 Workforce Composition

We next move to our main results by asking how hiring H-1B workers alters the worker composition of a firm, utilizing the unique data contained in the LEHD. We focus on two critical questions: 1) Does the provision of H-1B visas found in Figure 3 translate into the utilization of workers with characteristics of H-1B visa holders in the LEHD data? and, 2) the canonical question of whether immigrant workers crowd out native counterparts with similar characteristics, studied here in the context of the H-1B program.

Before answering these questions, we show that lottery wins increased total firm employment. Figure 4 plots estimated coefficients from Equation (2) where the outcome is the inverse hyperbolic sine of total firm employment. Echoing results from Section 5.1, lottery winners and losers were not trending differently in terms of total employment prior to the realized outcomes of the lottery. We then see a discrete jump after the start of the H-1B hiring period (2007Q4) that lasts throughout our study period—a permanent increase in employment that lasts beyond the initial H-1B employment spell of three years. As we discuss further in Section 5.2.2, our results imply that each lottery win leads to 0.83 additional LEHD workers in total at the firm starting in 2007Q4, a figure that includes the potential lottery-induced H-1B hire.

As H-1B visa status is not identifiable in the LEHD, we define H-1B-like immigrants as those workers who were born outside of the United States and Puerto Rico, who have a college degree, who are between the ages of 25 and 40, and who have less than three years of tenure at the firm. This definition is based on the characteristics of most H-1B workers but neither captures all H-1B workers nor excludes all non-H-1B workers.[23] We also test the effect of lottery wins on a firm's total immigrant college workforce—the number of foreign-born workers classified as having a college degree in the LEHD—and the effect of lottery wins on the total immigrant workforce, regardless of educational attainment. These measures are less likely to miss H-1B

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[23]Some reasons why this measure is likely broader than the true H-1B workers at the firm is that it may capture those working on an L-1 status, OPT, or a green card. Even under an H-1B status, they might have been working at other US firms the years before and thus would not be subject to the 2007 lottery. However, this measure may also miss some H-1B workers, such as those who are under 25, above 40, or who are incorrectly coded as not having a college degree in the LEHD. We also note that, given the high rate of renewal, an alternative measure of H-1B workers could include those with less than six instead of three years of tenure.

Figure 4: Effect of Lottery Win Rate on Total Firm Employment

(a) LEHD   (b) LBD (March of year t)
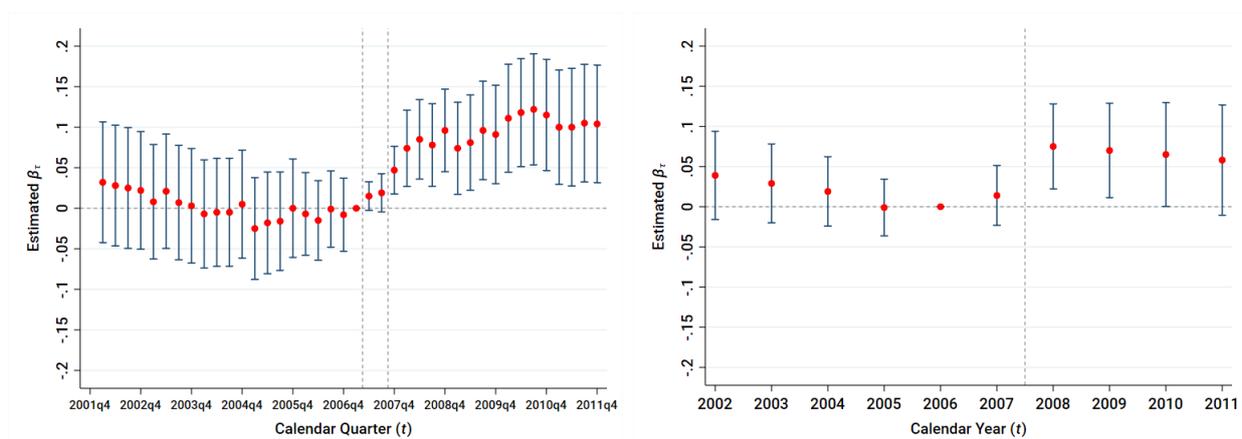
Note. See Equation (2) for specification. In Subfigure 4a, t indexes a calendar quarter, with omitted period $b = 2007Q1$. In Subfigure 4b, t indexes a calendar year, with omitted period $b = 2006$. Each subfigure plots the estimated coefficients $\hat{\beta}_\tau$ and their 95% confidence intervals. In Subfigure 4a, first vertical dashed line separates pre-lottery quarters (placebo tests) from quarters in which lotteries may have affected behavior but before 2007 lottery H-1B workers could be hired (anticipation tests), whereas second vertical dashed line separates pre-lottery-hire quarters from post-lottery quarters (treatment effect estimates). In Subfigure 4b, the outcome is measured in the LEHD and the number of firms is roughly 13,500 (rounded as per the US Census Bureau's disclosure rules). In Subfigure 4b, vertical dashed line separates pre-lottery years (placebo tests) from post-lottery years (treatment effects). In Subfigure 4b, the outcome is measured in the LBD and number of firms is roughly 20,000 (rounded as per the US Census Bureau's disclosure rules). Standard errors clustered at the firm-level in both figures. The dependent variable in each figure is the inverse hyperbolic sine of the given labeled outcome. Regressions across each subfigure include firm fixed effects, industry-time fixed effects, and the inverse hyperbolic sine of employment in March 2007 interacted with time fixed effects.

immigrant workers but are also likely to contain more non-H-1B immigrant workers.

Figures 5a, 5c, and 5e show that lottery-winning firms increase their use of workers in each category. First, Figure 5a shows a clear increase in winners' employment of H-1B-like immigrant workers immediately after the 2007 lottery. Given that this group has less than three years of tenure at the firm, the hump-shaped response displayed in Figure 5a is specifically consistent with increased hiring of H-1B workers in 2007, as seen in Figure 3a. Figures 5c and 5e further show a permanent increase in the employment of foreign-born workers with college degrees and foreign-born workers more generally during our study period, potentially due to the response in renewals seen in Figure 3b.

Our estimates imply that firms increase their hiring of (college-educated) immigrant workers by around 10% when they win all of their lottery applications. As discussed further in Section 5.2.2, this translates into each H-1B lottery win leading to 0.3 additional immigrant workers with a college degree (95% CI: [0.13,0.44]). Thus, while there is a permanent increase in immigrant hiring among H-1B lottery winners, this relationship is less than 1-to-1—individual applicants that ended up winning the H-1B lottery may have ultimately taken jobs in other nations, incumbent skilled immigrants may have been crowded out, or lottery losers may have found

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Figure 5: Effect of Lottery Win Rate on Employment of Selected Subgroups
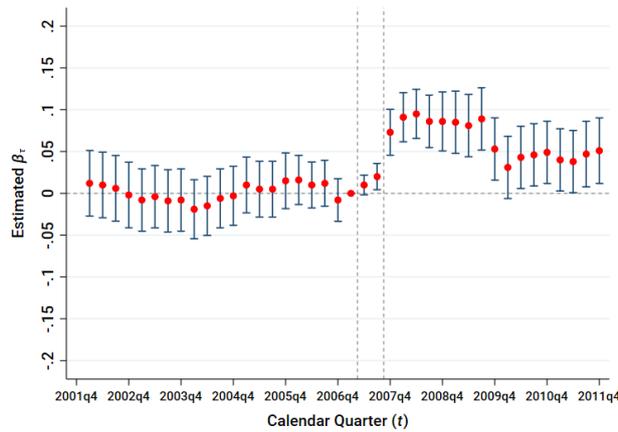
(a) H-1B-Like Immigrants

(b) H-1B-Like Natives
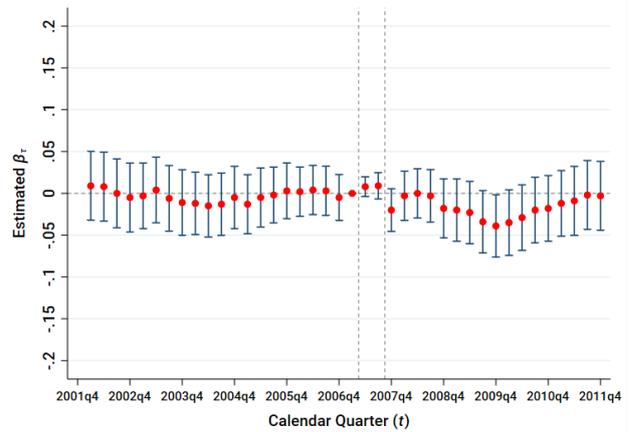
(c) College Immigrants
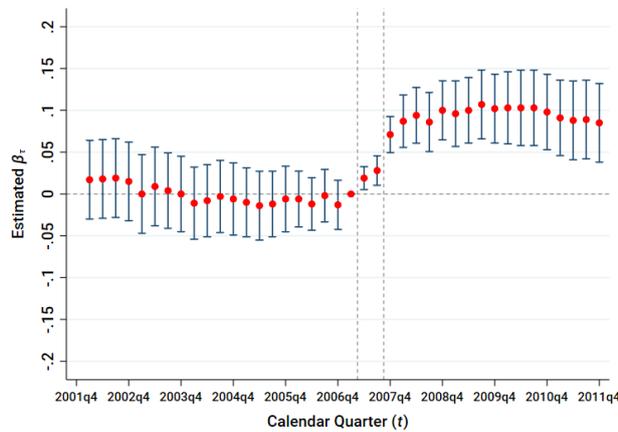
(d) College Natives
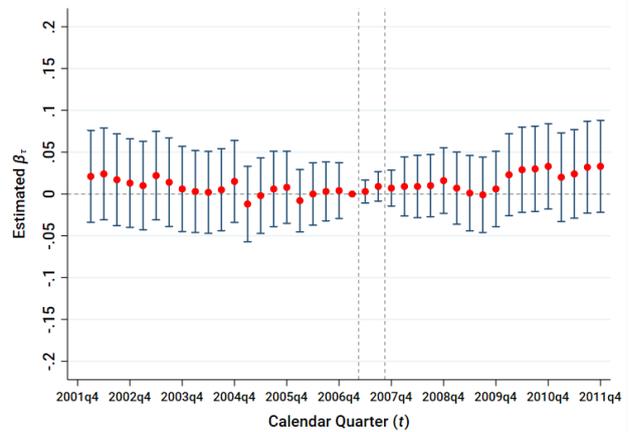
(e) All Immigrants
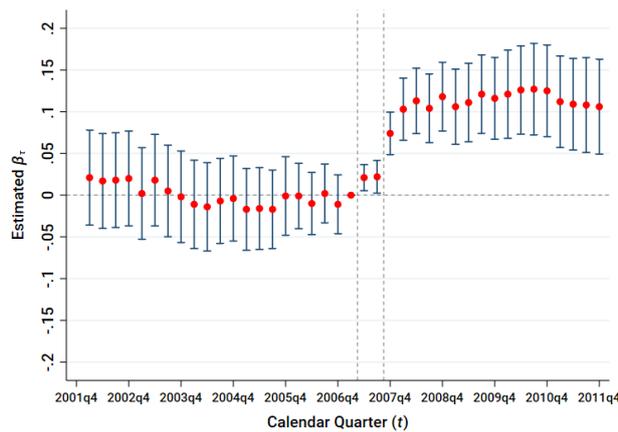
(f) All Natives
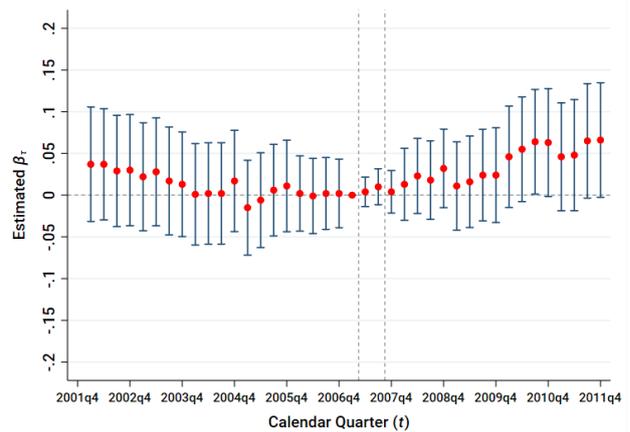
Note. See Equation (2) for specification, where t indexes a calendar quarter and omitted period $b = 2007Q1$. We plot the estimated coefficients $\hat{\beta}_\tau$ and their 95% confidence intervals. First vertical dashed line separates pre-lottery quarters (placebo tests) from quarters in which lotteries may have affected behavior but before 2007 lottery H-1B workers could be hired (anticipation tests). Second vertical dashed line separates pre-lottery-hire quarters from post-lottery quarters (treatment effect estimates). Number of firms is roughly 13,500 (rounded as per the US Census Bureau's disclosure rules). Standard errors clustered at the firm-level. Dependent variables in each subfigure are the inverse hyperbolic sine of the worker count from the labeled group. These outcomes are measured using the LEHD. Regressions include firm fixed effects, industry-time fixed effects, and the inverse hyperbolic sine of employment in March 2007 interacted with time fixed effects.

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other ways to hire immigrant workers.[24] Still, in total, the left panels of Figure 5 demonstrate that the H-1B lottery wins permanently increase firm-level immigrant intensity.

To further explore whether we are capturing H-1B hiring in the LEHD, we split H-1B-like immigrants by nationality. Results in Table A3 of Appendix Section D.2 show that lottery wins lead to large increases in employment of Indian and other non-North-American H-1B-like immigrants but declines in employment of Mexican and Canadian H-1B-like immigrants, who can also be hired on TN visas. Thus, we find specific evidence for one alternate channel through which lottery losing firms may fill their demand for highly educated immigrant workers.

We next discuss our key estimates on immigrant-native substitutability in the right panels of Figure 5, which can be directly compared to their corresponding left panels. Figure 5b presents evidence of limited substitution away from native workers who we consider most substitutable with H-1B workers—those native workers who are 25-40 years old, with less than three years of tenure at the firm, and with a college degree.[25] Meanwhile, Figure 5d does not show any evidence of a decline in the native college workforce more broadly, and Figure 5f similarly does not show a decline in the overall native workforce at a lottery-winning firm. As described in Section 5.2.2 below, our results imply that each H-1B lottery win increases employment of native college workers by 0.1 (95% CI: [-0.09,0.29]). We further explore the possibility of complementarities between H-1B and non-H-1B immigrant workers, along with effects on incumbent native workers in Section 7. The limited labor market effects on natives could also be specific to the nature of H-1B rules, which require firms to provide evidence that they will pay H-1B workers competitive wages and attest that they were unable to find suitable natives to fill H-1B positions.

We note that across all panels in Figure 5, we find at most limited evidence of anticipation effects that occur after the lottery has been resolved but before H-1B workers are hired ($\hat{\beta}_{2007Q2}$ and $\hat{\beta}_{2007Q3}$). The lack of anticipation effects on native employment, in particular, militates against concerns that our win rate is correlated with latent, secular labor demand increases at the firm level. There is some evidence of small, positive anticipatory effects on the hiring (or retention) of immigrant workers with college degrees. Part of this effect might be due to H-1B workers being at the firm before the lottery takes place under an alternative, more temporary immigration status (e.g. OPT or L-1 visa). Losing firms would then lose such workers when

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[24]Calculations from the FY 2021 lottery shows that among all the applications selected in the lottery, only about 75% went on to receive H-1B visa approval, indicating some winners may have taken jobs elsewhere. Alternatively, losing firms may have found other immigrant workers as evidenced by qualitative findings in a Government Accountability Office report to Congress regarding the H-1B program and suggestive evidence provided in the appendix of Doran, Gelber and Isen (2022). Finally, I-129 approvals do not necessarily convert into issued visas, as the prospective H-1B employee must pass a State Department interview and background check. Government reports from the H-2B visa program suggest that the I-129 to visa conversion rate is roughly 80% (Amuedo-Dorantes et al., 2023a).

[25]This exactly mimics our definition of "H-1B-like immigrant" workers except for nativity.

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unable to convert them to an H-1B visa.[26,27]

5.2.1 Differences by Size

As discussed in Section 4, much of our identifying variation comes from firms that apply for exactly one H-1B worker. For these, likely smaller firms, the USCIS essentially conducts a coin flip to determine whether the firm will receive an H-1B worker. For very large employers that tend to apply for many H-1B workers, the lottery introduces substantially less randomness in workforce composition because the lottery is held at the application level. To confirm this intuition and provide further context around our primary results on employer composition, we present heterogeneity by initial firm size in Figure 6.

Figure 6: Heterogeneity by Initial Employer Size
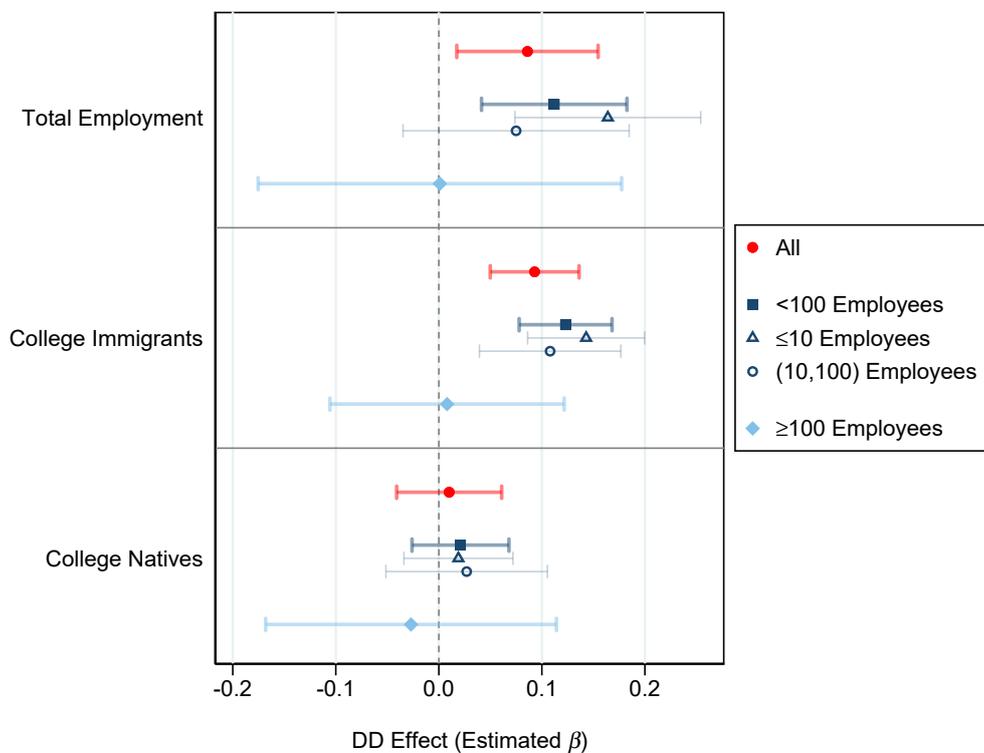
Note. Each plotted coefficient comes from a separate regression estimated using Equation (3), with t indexing a calendar year and omitted period $b = 2006$. Outcomes (labeled on the left) are transformed using the inverse hyperbolic sine. Each regression is estimated using a subset of firms based on their 2007Q1 employment, as labeled in the legend. Employment counts that generate sample splits are measured using the LBD.

We plot estimated coefficients from Equation (3), where the outcome is the inverse hyperbolic

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[26]12% of 2007 lottery applications came from individuals already in the U.S. on an F-1 visa, indicating they could have been on OPT. More generally, according to the Department of Homeland Security, roughly 48% of initial approvals for H-1B visas for FY2008 were for individuals already in the US.

[27]Also note that the results for college-educated immigrants and natives in Figures 5c and 5d are qualitatively very similar to those that focus on all immigrants and natives in Figures 5e and 5f. Therefore, we don't believe the imputation of education in the LEHD is biasing our estimates.

sine of total employment, employment of college immigrants, or employment of college natives. We run these difference-in-differences regressions, splitting employers based on whether they were at least 100 employees in 2007Q1 (light blue diamonds) or less than 100 employees in 2007Q1 (navy blue squares). We then further split the less than 100 employee results into regressions on firms with between 10 and 100 employees in 2007Q1 (hollow navy blue circles) and those with less than 10 employees in 2007Q1 (hollow navy blue triangles). For comparison, we also plot overall effects, across all size classes, in red circles.

Two key features emerge from Figure 6. First, identifying variation stems primarily from smaller firms. Intuitively, firms with large numbers of applications have low variation in win rates due to the law of large numbers. This ultimately manifests in the tighter standard errors for small firms in Figure 6 and the fact that the overall results ("All") closely match the results for firms under 100 employees. Second, even if we ignore standard errors and take point estimates at face value, the lottery appears to have no impact on larger firms—either in terms of total employment counts *nor* native crowd out. Thus, point estimates indicate that larger firms are able to fill their demand for college immigrant workers through non-lottery channels even if they are left unexpectedly short by the lottery. Indeed, we find evidence consistent with the notion that firms of all sizes find other ways to hire college immigrants if they lose the lottery in Section 5.2.2, and this is especially true for larger firms. We therefore conclude that our results are most relevant for the roughly 75% of our lottery participants who originally had less than 100 employees.

5.2.2   Magnitudes

Our event study coefficients can be approximately interpreted as the percent change in a given outcome for a firm had it won all of its lottery applications relative to if it had lost all of its lottery applications. To obtain an alternate interpretation that more directly speaks to magnitudes in our employment effects, we present an instrumental variable strategy that allows us to quantify how much one additional H-1B win increases the employment of different types of workers. The second stage regression is a modified version of our difference-in-differences specification from Equation (3):

$$\text{Scaled Employment}_{jt}^{g} = \beta_m[\text{Scaled Lottery Wins}_{j,2007} \times \text{Post}_t] + \Gamma X_{jt} + \alpha_j + \alpha_{kt} + \varepsilon_{jt}. \quad (4)$$

In Equation (4), *Scaled Employment*$_{jt}^{g}$ is the employment of a given group of workers g at firm j in time t divided by total firm employment in 2006Q4, as reported in the LEHD. *Scaled Lottery Wins*$_{j,2007}$ is similarly the number of 2007 lottery wins divided by total firm employment in 2006Q4 (the same denominator). Because the outcome and independent variable are scaled in the same way, the interpretation of β_m is the number of additional employees in group g per lottery win in the post-treatment period. Control variables remain the same as in Equation (3). Estimating Equation (4) use OLS would yield biased estimates of β_1, as the

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raw number of lottery wins (not divided by applications) reflects several endogenous firm-level factors, including firm demand for H-1B workers. Thus, we use $Win\ Rate_{j,2007} \times Post_t$ as an instrument for $Scaled\ Lottery\ Wins_{j,2007} \times Post_t$ and estimate Equation (4) using two-stage least squares (2SLS).[28]

Table 3 presents IV results divided in three panels, corresponding to three samples: the full sample, small firms (less or equal than 10 employees), and medium size firms (between 10 and 100 employees). The columns show the effects on total employment, employment of college-educated immigrants, employment of college-educated natives and employment of non-college workers, respectively. The panel including the full sample of firms shows that one additional H-1B lottery win increases total firm employment by 0.83 workers. Of those 0.83 workers, 0.29 are college-educated immigrants, 0.1 college-educated natives, and 0.44 non-college-educated workers.

These numbers highlight two main results. First, total employment at the firm-level increases by less than 1, implying some crowd out. However, this is driven by the fact that college-educated immigrants increase by substantially less than 1. While data limitations preclude further examination, a plausible explanation is that lottery-losing firms were able to hire immigrants through other channels despite losing the lottery. Additionally, however, it could be the case that individuals ultimately took jobs in other nations despite winning the H-1B lottery, or that incumbent skilled immigrant workers experience crowd-out. Second, H-1B hires seem to crowd-in other workers, such as non-college workers of all nativities, as lottery-winning firms *expand* their usage of workers outside of the immigrant college group. These results are consistent with, e.g., non-college workers being complements to highly educated immigrants. We incorporate such complementarities into our theoretical framework in Section 5.5.

Given that our lottery variation is driven by smaller firms, we proceed by looking into how these magnitudes change for different employment size groups. For firms with at most ten employees as of 2007, one additional lottery win increases total employment by one, and half of that is accounted for by college-educated immigrants. Interestingly, for small firms, there is a crowd-in of 0.18 college educated-natives, which indicates that the lottery eases a constraint that allows them to expand in all types of workers relative to losing firms. For the group of firms with employment between ten and one hundred, the effects are more muted. Total employment for winning firms increases by 0.2 employees for each additional lottery win, and it is almost fully accounted for by an increase of college-educated immigrants. The smaller magnitudes for college-educated immigrants highlight that bigger firms might find it easier to hire immigrant

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[28]Results from Equation (4) estimated using 2SLS with $Win\ Rate_{j,2007} \times Post_t$ as the instrumental variable are lower bounds for the overall magnitudes due to multi-unit firms that may have establishments in states that aren't in our LEHD sample. This because the numerator of $Scaled\ Lottery\ Wins_{j,2007}$ covers all lottery wins for the firm, whereas the numerator of $Scaled\ Employment_{jt}^g$ only includes employment within our LEHD states. We do not think this is a major factor in biasing results, however, because most of our identification comes from smaller firms that are unlikely to be multi-unit. Results in which we only study firms that are fully contained within our LEHD states are available upon request.

workers in the event of losing the lottery.

Our employment per approval magnitudes differ from those found in Doran, Gelber and Isen (2022), who study the 2005 and 2006 last-day-of-filing H-1B lotteries. In Appendix F, we discuss differences across these two studies—and why we do not believe they are contradictory—in more detail. In particular, Doran, Gelber and Isen (2022) focus on a set of firms that, on average, had already secured the right to hire some H-1B workers and faced random variation in their ability to hire *additional* H-1B workers. We focus on a set of firms that faced random variation in their ability to hire *any* H-1B workers. Both papers contain evidence that the burden of any extant crowd-out falls on other immigrant workers rather than natives.

Table 3: Changes in Employment per Lottery Win

| **Full Sample** | Outcome: Scaled Employment of Group | | | |
	Total Employment	College Immigrants	College Natives	Non-College
Scaled Lottery Wins$_{j,2007}$ × Post$_t$	0.827*	0.287***	0.099	0.442
	(0.445)	(0.079)	(0.098)	(0.325)
Number of Firms	13,500	13,500	13,500	13,500
Number of Observations	137,000	137,000	137,000	137000
1st Stage F-Stat	318.5	318.5	318.5	318.5

| **≤ 10 Employees** | Outcome: Scaled Employment of Group | | | |
	Total Employment	College Immigrants	College Natives	Non-College
Scaled Lottery Wins$_{j,2007}$ × Post$_t$	1.006***	0.459***	0.176*	0.370**
	(0.333)	(0.125)	(0.090)	(0.150)
Number of Firms	5000	5000	5000	5000
Number of Observations	48000	48000	48000	48000
1st Stage F-Stat	249.7	249.7	249.7	249.7

| **(10,100) Employees** | Outcome: Scaled Employment of Group | | | |
	Total Employment	College Immigrants	College Natives	Non-College
Scaled Lottery Wins$_{j,2007}$ × Post$_t$	0.196	0.167***	0.040	-0.012
	(0.291)	(0.053)	(0.092)	(0.180)
Number of Firms	5000	5000	5000	5000
Number of Observations	52000	52000	52000	52000
1st Stage F-Stat	61.2	61.2	61.2	61.2

Note.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ This table shows the 2SLS estimates corresponding to equation 4, where the instrument is given by *Win Rate*$_{j,2007}$ × *Post*$_t$ as the instrumental variable. Firm and observation counts rounded as per the US Census Bureau's disclosure rules. Standard errors clustered at the firm-level. The top panel presents the results for the full sample, the middle panel for firms that have less or equal than 10 employees in 2007, and the bottom panel for firms that have between 10 and 100 employees win 2007. "Scaled" refers to dividing by a given variable by 2006Q4 LEHD employment at the firm. Both employment counts (outcomes) and lottery wins (endogenous, independent variable) are scaled this way. All regressions include industry-time fixed effects, firm fixed effects and the IHS of employment in 2007 interacted with year dummies.

5.2.3   Robustness Checks

In Appendix Section D.1, we probe the robustness of our key results on total employment, employment of immigrant college graduates, and employment of native college graduates on several fronts. First, results are not sensitive to alternate control sets—including dropping the employment control—or alternate ways of approximating percent changes other than the inverse hyperbolic sine, as advocated in Roth and Chen (2022). Second, our results do not depend on the additional assumptions required when using a continuous exposure variable in a difference-in-differences analysis (Callaway et al., 2024; de Chaisemartin et al., 2024) by presenting results using a binary version of our lottery success measure. Finally, we present the difference-in-differences results for all of our outcome variables (see Table A4).

5.3   Firm Performance

Having shown that lottery wins increase firm employment without generating net employment losses for substitutable natives, we next turn to the effect of lottery wins on other key indicators of firm performance. These analyses use outcomes measured in the LBD and are available for the universe of H-1B lottery applicants, subject to our sampling restrictions.

Table 4 presents our main firm performance results, returning to our preferred reduced form difference-in-differences specification, Equation (3). First, Column (1) indicates that there is a large extensive margin response to lottery wins. Firms that win all of their lottery applications are 2.5 percentage points more likely to have positive payroll—our proxy for actively operating—in a given post-lottery year than firms that lose all of their lottery applications. A nontrivial set of lottery participants appear to be reliant on H-1B workers for survival.

This extensive margin response also help explain the large scale effect suggested by Columns (2) and (3). Specifically, we find that revenues and payroll both rise by more than 20% at firms that win all of their lottery applications relative to firms that lose all of their lottery applications. These estimates are consistent with those of Clemens and Lewis (2022) and Amuedo-Dorantes et al. (2023a) for the H-2B program. The large magnitudes of these effects coincide with the notion that our identifying variation is driven by smaller firms that may be especially reliant on procuring an H-1B worker for survival and for generating revenue streams.

Finally, in Columns (4) and (5), Table 4 provides suggestive but imprecise evidence that labor productivity increases among continuing firms after lottery wins. Both revenues per workers and average annual wages (payroll per worker) see marginal increases in the post-lottery period among winners who continue to operate relative to losers who continue to operate. The positive result for wages might be an implication of the new H-1B immigrants having higher wages than the average worker at the firm. This is consistent with the fact that the wage increases are indeed driven by college-educated immigrants (see Appendix Table A4). However, in Section 7, we also show that winning the H-1B lottery has both positive and negative wage impacts for

Table 4: Effect of Lottery Win Rate on Firm Performance Measures

| | (1) | (2) | (3) | Conditional on Active | |
| | | | | (4) | (5) |
	$\mathbb{1}$[Active]	Revenues	Payroll	Revenues per Worker	Average Wage
Win Rate$_j$ × Post$_t$	0.025***	0.266***	0.215***	0.019	0.012*
	(0.006)	(0.068)	(0.058)	(0.013)	(0.007)
Firms	20,000	20,000	20,000	20,000	20,000
Observations	199,000	199,000	199,000	199,000	199,000

Note. See Equation (3) for specification. Standard errors clustered at the firm-level. Each outcome are measured using the LBD. Outcomes in Columns (2) through (5) are transformed using the inverse hyperbolic sine. Regressions include firm fixed effects, industry-time fixed effects, and the inverse hyperbolic sine of employment in March 2007 interacted with time fixed effects. Firm and observation counts rounded as per the US Census Bureau's disclosure rules. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

certain incumbent workers.

In total, the results in Table 4 run counter to the notion that H-1B workers hired through the 2007 lottery simply provided "cheap labor" for winning firms.[29] Increases in survival and scale coupled with *increases* in average wages suggest that firms relied on H-1B workers to increase productive capacity. We provide several additional estimates from Equation (3) that align with these conclusions in Appendix Section D.3, Table A4.

5.4   Validation: The 2020 H-1B Lotteries

As a final validation, we provide supplementary firm-level results from the 2020 H-1B visa lotteries. The results from the 2020 lotteries address internal validity because the available data accurately measure lottery wins *and* applications. After years of following the procedure described in Section 4, the USCIS implemented a new electronic registration system for lottery-subject workers with approved LCAs who intended to apply for cap-subject I-129 petitions in 2020. The 2020 H-1B lotteries were conducted among these registrations, and a recent FOIA request made information on these registrations—including lottery results—available to the public.[30]

We therefore turn to an analogous event study specification to Equation (2) but using the win rate for the 2020 lottery, Win Rate$_j^{2020}$, constructed as the total number of wins relative to the total number of *true* applications.[31]

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[29]Additionally, claims that H-1B winning firms simply out-contract their workers to H-1B losing firms would likely imply weaker effects on revenue, as losing firms would still be able to utilize skilled labor from winning firms. Such out-contracting would likely occur from the largest H-1B employers. Results without large employers can be seen in our heterogeneity analysis, in Section 5.2.1.

[30]See here: https://github.com/BloombergGraphics/2024-h1b-immigration-data.

[31]Our win and application counts include both Regular and ADE applications, which is why we still prefer a difference-in-differences approach. See Appendix Section E for more details.

Relative to Win Rate$_j$ (from 2007), the key advantage of Win Rate$_j^{2020}$ is that the denominator represents the exact total number of H-1B applications each firm submitted. In 2007, only LCA counts can be used to proxy for applications. This raised the possibility that win rates could be correlated with firm-level shocks that led firms to not submit an H-1B application despite submitting an LCA. Win Rate$_j^{2020}$ is not subject to these concerns, as the denominator represents the true number of H-1B applications. We relegate all details regarding the implementation of the 2020 lottery to Appendix E.

For the purpose of this analysis we obtained access to a new Census project where the LBD is available through 2022 and the LEHD is available through 2021. However, a key difference in the 2020 lottery analysis—and the reason we view it as validation—is that we have at most five quarters of follow-up (as opposed to 17) in our LEHD data: the start of the 2020 H-1B hiring period is 2020Q4, and the time period for which we observe all 25 of our LEHD states ends in 2021Q4.

Figure 7 shows the impact of 2020 lottery luck on total firm employment (top row) and the employment of immigrant and native college workers (bottom row). As in 2007, lottery success in 2020 is consistent with increases in firm employment of approximately similar magnitude (in percent terms) in our LEHD and LBD measures. In the case of the 2020 lottery, the LBD results are more precise, but results from both data sources indicate that one-for-one crowd-out is unlikely. Further, we observe an increase in the number of college-educated immigrants at winning firms and no significant change in utilization of college-educated native workers, mirroring our 2007 lottery results. Appendix Figure A5 expands this analysis to other subgroups, and Appendix Table A5 further shows that winning firms in 2020 see higher revenues, payroll, and survival. We take these results as an important check that our 2007 results are robust and unlikely to be driven by measurement issues relating to our LCA proxy for applications.

5.5   Implied Elasticity of Substitution

What do our reduced form estimates imply about the elasticity of substitution between skilled H-1B immigrants and natives? We adopt a simple framework in which firms produce a particular variety of good under monopolistic competition using a nested-CES aggregate of different labor inputs to illustrate how lottery-induced variation in skilled immigrant employment can generate scale effects that mitigate substitution between immigrants and natives.[32] We assume workers consume a composite good Y, composed of different varieties indexed by z as in Equation (5):

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[32]Similar frameworks have been applied to study firm-level immigration. For example, see Waugh (2017), Clemens and Lewis (2022), Mahajan (2024), and Brinatti and Morales (2023).

Figure 7: Effect of 2020 Lottery Win Rate on Firm Employment

(a) LEHD Total Employment

(b) LBD Employment (March of year t)

(c) College Immigrants

(d) College Natives
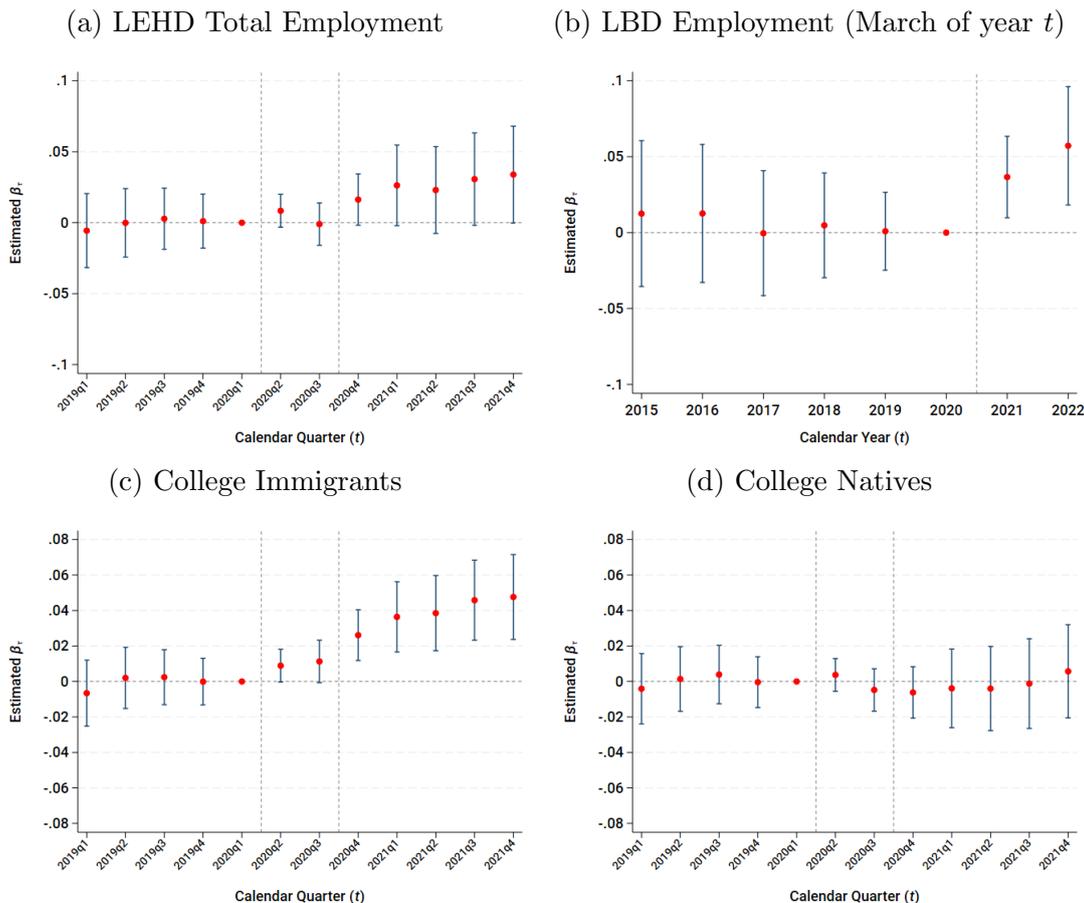
Note. See Equation (E2) for specification. In Subfigures 7a, 7c, and 7d t indexes a calendar quarter, with omitted period $b = 2020Q1$. In Subfigure 7b, t indexes a calendar year, with omitted period $b = 2020$. Each subfigure plots the estimated coefficients $\hat{\beta}_\tau$ and their 95% confidence intervals. In Subfigures 7a, 7c, and 7d, the first vertical dashed line separates pre-lottery quarters (placebo tests) from quarters in which lotteries may have affected behavior but before 2007 lottery H-1B workers could be hired (anticipation tests), whereas second vertical dashed line separates pre-lottery-hire quarters from post-lottery quarters (treatment effect estimates). In Subfigures 7a, 7c, and 7d, the outcomes are measured in the LEHD and the number of firms is roughly 25,000 (rounded as per the US Census Bureau's disclosure rules). In Subfigure 7b, vertical dashed line separates pre-lottery years (placebo tests) from post-lottery years (treatment effects). In Subfigure 7b, the outcome is measured in the LBD and number of firms is roughly 33,000 (rounded as per the US Census Bureau's disclosure rules). Standard errors clustered at the firm-level in both figures. The dependent variable in each figure is the inverse hyperbolic sine of the given labeled outcome. Regressions across each subfigure include firm fixed effects, industry-time fixed effects, and the inverse hyperbolic sine of employment in March 2020 interacted with time fixed effects. See Appendix Section E for more.

$$Y = \left(\int_{J_z} (y(z))^{\frac{\varepsilon-1}{\varepsilon}} dz \right)^{\frac{\varepsilon}{\varepsilon-1}} \tag{5}$$

where J_z represents the set of varieties available in the country, and $\varepsilon > 1$ is the elasticity of demand. Higher values of ε imply that consumers find it easier to switch consumption across varieties when prices change. Each variety is produced by a firm indexed by j that has a production function as in Equation (6):

$$y_j = \psi_j L_j^\delta H_j^{1-\delta} \longrightarrow H_j = \left(\alpha I_j^{\frac{\sigma-1}{\sigma}} + (1-\alpha)N_j^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}} \tag{6}$$

where ψ_j is the firm's specific productivity. Firms use only labor inputs of non-college educated workers L_j and college-educated workers H_j which are aggregated in a Cobb Douglas manner with δ representing the share of non-college labor in the total wage bill. We assume college workers are a CES composite of skilled immigrants and natives. The elasticity of substitution $\sigma > 1$ captures how substitutable college-educated immigrants and natives are within the firm, where higher values of σ indicate greater substitutability between immigrants and natives.

We analytically compute the change in demand for native college-educated labor following an exogenous change in the number of college-educated immigrants in the firm. In our setting, this change arises via the H-1B visa lottery, which generated an exogenous increase in the number of immigrant college workers hired at winning firms relative to losing firms. We obtain equation (7) by totally log-differentiating the first order condition of the firm optimization problem for native college labor, assuming competitive native labor markets[33] (we relegate intermediate steps to Appendix E.4):

$$\frac{d\log N_j}{d\log I_j} = \frac{s_I\left[(\varepsilon-\sigma)-\delta(\varepsilon-1)\right]}{(\varepsilon-1)(1-s_L)(1-\delta)-\left[(\varepsilon-\sigma)-\delta(\varepsilon-1)\right]s_N}, \tag{7}$$

where s_I, s_N, s_L are the wage bill of college-immigrants, college-natives, and non-college labor as a share of total revenues, respectively. Equation (7) shows that the elasticity of native labor with respect to immigrants is a function of these shares, the non-college share parameter δ, the demand elasticity ε and the elasticity of substitution between immigrants and natives σ.

To gain intuition on the role of the elasticities (ε and σ), we can consider a version of the model in which there are no non-college workers, such that $\delta = s_L = 0$. The relative log change of natives to immigrants reduces to:

$$\left.\frac{d\log N_j}{d\log I_j}\right|_{\delta=s_L=0} = \frac{s_I(\varepsilon-\sigma)}{(1-s_N)\varepsilon + s_N\sigma - 1}. \tag{8}$$

Equation (8) implies that whether there is crowd-in or crowd-out of natives depends on the sign of $\varepsilon - \sigma$, as the denominator is positive. If $\varepsilon > \sigma$, the scale effect dominates: when immigrants arrive at the firm, the firm can expand output without having to reduce the price as much. Such expansion prompts the firm to hire *more* natives instead of substituting them with immigrants. On the contrary, if $\sigma > \varepsilon$, the substitution effect dominates, and the influx of immigrants does

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[33]Recent literature has shown that immigration in monopsonistic labor markets can lead to sizable negative effects on native employment (Amior and Manning, 2020; Amior and Stuhler, 2023). This, however, is inconsistent with our finding of minimal substitution. Additionally our modeling choice is consistent with our finding of no firm-level wage impacts.

not generate a big enough scale effect to prevent firms from reducing native employment. A similar intuition holds in the expanded model when $\delta > 0$ and $s_L > 0$.

Equation (7) allows us to use our reduced form results to back out the implied substitution between immigrants and natives σ. First, we set ε to 2.92, which is the median value estimated by Broda and Weinstein (2006) for differentiated products. Second, we compute directly from the data the values for $s_L = 0.24$, $s_I = 0.17$, $s_N = 0.25$ and $\delta = 0.36$. Note that our sample of firms of H-1B applicants is quite immigrant-intensive and college-intensive.

Third, we use our estimates from Table A4 to estimate the value of the right-hand side of Equation (7). We focus on the relationship between H-1B like natives and H-1B like immigrants, as that is the immigrant group that is shocked by the lottery. The estimate for $\frac{d \log N}{d \log I}$ can be approximated by the relative difference-in-differences coefficients of H-1B like natives and H-1B like immigrants. Given our values for the other parameters, the estimated value of σ is 4.3. This estimate is close to the value estimated by other papers with similar frameworks (Brinatti and Morales, 2023; Burstein et al., 2020; Cortes, 2008).[34]

6   Heterogeneity Analysis

As a next step, we investigate whether certain firm characteristics drive our results. We focus our analysis on three main outcomes: total firm employment, native college-educated employment, and immigrant college-educated employment. To estimate differential response to winning the H-1B lottery across firms, we set up a triple difference approach:

$$
\begin{aligned}
y_{jt} = \beta \left[\text{Win Rate}_j \times \text{Post}_t\right] + \gamma \left[\text{Post}_t \times Z_j\right] + \\
\delta \left[\text{Win Rate}_j \times \text{Post}_t \times Z_j\right] + \Gamma X_{jt} + \alpha_j + \alpha_t + \varepsilon_{jt} \, ,
\end{aligned} \tag{9}
$$

where Z_j stands for the standardized version of a continuous firm characteristic. The coefficient β will capture the response to winning the lottery for firms with average value of characteristic Z_j. Coefficient γ captures the differential time-trend for firms with one standard deviation above average of characteristic Z_j relative to firms with average characteristic Z_j regardless of their lottery outcome. Finally, the key coefficient of interest is δ, which captures the differential impact of winning the lottery for firms with one standard deviation above the mean in characteristic Z_j relative to firms with average Z_j. By comparing coefficient β with coefficient $\beta + \delta$, we can quantify the differential impact of winning the lottery across firms with different values of Z_j.

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[34]The H-1B like natives is the closest we can get in our data to the group that is the most substitutable with the H-1B immigrants. If we had occupation data and could focus on computer scientists, we would likely find higher degrees of substitutability for that specific group of natives.

We explore four main characteristics in our analysis which we measure in 2006, before the lottery took place. First, we compute the number of college graduates as a share of firm employment to proxy for the skill intensity of the firm. Second, we compute the number of immigrant college graduates as a share of total college graduates, as a measure of immigrant intensity for college labor. Third, we compute average wage as a measure of overall worker quality and skill. Fourth, we compute the labor productivity at the firm by calculating the ratio of revenues to total employment.

As shown in Table 5, our four characteristics broadly present a similar picture. In terms of total employment, firms with a higher immigrant share, higher wages, or higher productivity expand by almost double when winning the lottery relative to firms with average value of these characteristics. To understand the interpretation, we can focus on the result for wages (Column 3). A firm that pays an average level of wages to their employees and wins all of their lottery applications increases their total employment by 6.4% relative to a firm that loses all of their applications. However, a firm that pays wages that are one standard deviation above the mean expands employment by an additional 10.8% when winning all of their lottery applications, relative to a firm that loses all of their lottery applications. Similar results hold for firms with higher labor productivity (Column 4) and firms more reliant on immigrant college workers (Column 2). These results highlight the idea that high-wage, and high-productivity firms, along with firms that have already revealed that immigrant workers are important to their production function, benefit more from hiring H-1B immigrants and were likely more constrained in hiring high-skill workers.[35]

When looking at native employment, in the middle panel of Table 5, we see that firms with high immigrant shares, high wages, and high productivity expand their native-college employment when winning the lottery relative to firms with average values of these characteristics. This reinforces the idea that immigration eases a production constraint for high-wage, high-productivity firms that allows them to also hire more of other labor inputs. Finally, as shown in the bottom panel of Table 5, firms that have high values of the characteristics expand their immigrant-college employment by more than those with average values of the characteristic. This is suggestive that firms with higher productivity might recruit immigrants that are harder to find substitutes for in the domestic labor market.

7   Individual-Level Results

As a final step, we explore how the H-1B lottery affects the career trajectories of incumbent workers at lottery-participating firms. Since we are interested in understanding how incumbent workers respond when firms get a positive immigration shock, we focus on firms that partici-

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[35]For instance, if high-wage/high-productivity firms require skills that are harder to find the the labor market, access to immigrants can relax that constraint.

Table 5: Triple-difference Estimates by Firm-level Characteristics

	(1)	(2)	(3)	(4)
Outcome: Q4 Employment				
Characteristic (Z_j):	College Share	Immigrant Share	Average Wage	Revenues per Worker
Win Rate$_j$ × Post$_t$	0.082**	0.078**	0.064**	0.054
	(0.036)	(0.037)	(0.036)	(0.036)
Z_j × Post$_t$	0.012	-0.001	0.053**	0.041**
	(0.021)	(0.023)	(0.024)	(0.020)
Win Rate$_j$ × Z_j × Post$_t$	0.056	0.076**	0.108**	0.069**
	(0.034)	(0.034)	(0.042)	(0.031)
Outcome: Native College Employment				
Characteristic (Z_j):	College Share	Immigrant Share	Average Wage	Revenues per Worker
Win Rate$_j$ × Post$_t$	0.003	0.007	-0.008	-0.012
	(0.026)	(0.026)	(0.027)	(0.026)
Z_j × Post$_t$	-0.006	0.021	0.037**	0.010
	(0.014)	(0.016)	(0.017)	(0.013)
Win Rate$_j$ × Z_j × Post$_t$	0.026	0.041*	0.061**	0.051**
	(0.023)	(0.023)	(0.031)	(0.022)
Outcome: Immigrant College Employment				
Characteristic (Z_j):	College Share	Immigrant Share	Average Wage	Revenues per Worker
Win Rate$_j$ × Post$_t$	0.094***	0.095***	0.082***	0.075***
	(0.024)	(0.025)	(0.023)	(0.024)
Z_j × Post$_t$	-0.008	-0.021	0.035**	0.032**
	(0.014)	(0.015)	(0.015)	(0.013)
Win Rate$_j$ × Z_j × Post$_t$	0.048**	0.047**	0.057*	0.036*
	(0.023)	(0.023)	(0.030)	(0.021)
Firms	13,500	13,500	13,500	13,500
Observations	137,000	137,000	137,000	137,000

Note.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ Firm and observation counts rounded as per the US Census Bureau's disclosure rules. Standard errors clustered at the firm-level. The top panel presents the estimates for the outcome of the IHS of total employment. The outcome of the middle panel is the IHS of total employment of college-educated natives. The outcome of the bottom panel is the IHS of total employment of college-educated immigrants. All outcome variables are computed using the LEHD. The characteristics Z_j are standardized and computed in 2006.

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pated in the lottery and have less than 100 employees.[36] We then extract the full job histories between 2001 and 2011 for all individuals who worked at those firms in 2006, on the eve of the H-1B lottery. Using this sample, we run difference-in-differences regressions at the worker-year level as shown in Equation (10):

$$y_{it}^g = \beta^g[\text{Win Rate}_{j_0(i)}^g \times \text{Post}_t^g] + \Gamma X_{it}^g + \delta_i^g + \delta_t^g + \varepsilon_{it}^g,$$

where the superscript g indicates that we run this regression separately for different groups of workers. The key coefficient of interest, β^g, captures the interaction between the lottery outcome at the individual's employing firm in 2006 ($Win\ Rate_{j_0(i)}$) and an indicator for the post-period (2007 or later). The subscript $j_0(i)$ stands for the lottery-participating firm that individual i was working for in time $t_0 = 2006$. We include the same controls as in Equation (3) based on firm $j_0(i)$ (regardless of where individual i works at time t). We also include individual fixed effects (δ_i) and gender-wage-bin-year fixed effects in X_{it}.[37] Effectively, we compare how the career paths of individuals who worked at firms that were successful at the lottery change relative to the career path of similar individuals working at firms that were unsuccessful at the lottery.

We focus on two main outcomes: the evolution of log individual wages and the probability of the worker being at their 2006 firm at time t. As we expect heterogeneous effects of the lottery across incumbent workers, we separate individuals into six groups g based on age, tenure, nativity, and education. H-1B immigrants tend to be below the age of 40, are college educated, and by definition have low tenure at the firm. Therefore, specifically, we separate workers by whether: 1) they were above or below 40 years old in 2006, 2) were above or below three years of tenure at the lottery firm in 2006, 3) have a college degree, and 4) are immigrants or natives. We combine all college workers above 40 years old into a single group, as well as pooling all non-college workers together.

Table 6 presents the results for these groups of workers. Interestingly, young college-educated workers with low tenure who worked at a firm that won the lottery see a wage increase between 4-5% relative to workers of that same group who worked at firms that lost the lottery. One interpretation of these results is that incumbent workers who are young and low tenure might be those who work most closely with and thereby learn the most from the incoming H-1B worker. Jarosch et al. (2021) measure learning from coworkers through the increase in wages of incumbents after being exposed to a productive new coworker. Our results would also be consistent with a richer model of the firm, relative to that presented in Section 5.5, in which workers perform specific tasks/occupations and the arrival of new H-1B workers allows existing

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[36]As shown in Section 5, firms with more than 100 employees who win the lottery do not hire a statistically different number of immigrants than firms that lose the lottery following the results in Section 5.2.1, so we exclude them from this analysis.

[37]To determine the wage bins, we classify individuals into 5 wage groups based on their income in 2006.

low-tenure, college, young workers to move up the occupation ladder. Non-college workers at lottery-winning firms also seem to benefit from the H-1B immigrant coworkers, as they increase their wages by 3.1% after the lottery relative to those in lottery-losing firms.

Table 6: Individual-Level Analysis

	Outcome		
Group	Log Wage	$\mathbb{1}$(At Lottery Firm)	Number of observations
Immigrants, college, low tenure, age ≤ 40	0.050** (0.025)	0.020 (0.016)	175,000
Natives, college, low tenure, age ≤ 40	0.041* (0.024)	-0.004 (0.016)	122,000
Immigrants, college, high-tenure, age ≤ 40	-0.015 (0.023)	-0.003 (0.016)	79,000
Natives, college, high-tenure, age ≤ 40	-0.054** (0.024)	-0.036** (0.016)	103,000
College workers, age > 40	0.013 (0.012)	-0.011 (0.015)	583,000
Non-college workers	0.031** (0.013)	-0.001 (0.011)	1,374,000

Note.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ Year-person observation counts rounded as per the US Census Bureau's disclosure rules. Standard errors clustered at the firm-level. The first column presents the results for the outcome of $\log(\text{wage}_{i,t})$ at the individual level. The outcome of the second column is a binary variable that takes the value of 1 if the individual still works at their 2006 firm and 0 otherwise. The coefficients presented are the interaction between $\mathbb{1}$(Individual belongs to group g) $\times$ Win Rate$_{j_0(i)}$ $\times$ $\mathbb{1}$(year ≥ 2007). Groups are defined based on an individual's age and tenure in 2006. The Win Rate$_{j_0(i)}$ is the win rate of the firm where the employee worked in 2006. We limit the sample to individuals working in 2006 at lottery-participating firms that had less than or equal to 100 employees.

However, not all worker groups benefit from the exposure to an H-1B coworker. Young, college-educated natives who have high tenure experience 5.4% wage reductions when tied to lottery-winning firms relative to those who were tied to lottery-losing firms at the time of the lottery. Some of this wage reduction may be due to job separation: these workers are 3.6% less likely to remain at the lottery firm after 2007. In conjunction, these results are consistent with the notion that firms demand higher tenure, young, native college workers in response to increased availability of young, immigrant college workers. In effect, we allow the data to tell us that this group of incumbent workers is most substitutable to the new H-1B immigrants. We also note that the increased separation probability higher turnover of high tenure natives might also explain the positive effects on low tenure workers, who may be promoted once more senior workers move to other jobs.

All in all, these results reveal the importance of separately analyzing the effect of exposure to immigrants on incumbent firms and incumbent individuals. In Section 5, we found that individual firms tend to expand and survive in response to H-1B lottery wins. Here, we find that young, low tenure, college workers and non-college workers of all nativities are most likely to benefit from these firm-level impacts. Meanwhile, higher tenure, young, native college gradates appear to pay a cost. These are important, individual-level heterogeneities in the response to lottery wins that a firm-level-only analysis would miss. In this sense, our individual-level analysis mirrors that in Dustmann et al. (2016), particularly in its ability to elucidate important heterogeneities in response to immigrant exposure that are not detectable without administrative panel data on workers. It also echoes a broader point in the immigration literature: just as the local economic effects of immigration may not correspond to the effects of immigration on local incumbents due to internal migration, the firm-level effects of immigrant hiring may not correspond to the effects of exposure to immigrant co-workers on incumbent workers due to firm turnover (Amior, 2021; Borjas, 1999, 2006; Monras, 2020, 2021).

Nonetheless, to understand these results in the aggregate, it is important to note that the group of native workers that are displaced by the H-1B immigrants and see lower earnings only represent 4.2% of the total population of incumbents, while non-college workers alone represent 56%. This suggests that while there are distributional consequences to immigration, on average, the majority of incumbent workers benefit from the new H-1B workers.

8   Conclusion

All told, we provide several new, well-identified estimates to the empirical literature on the impact of high-skill immigration on US firms and workers. A key finding is that one additional H-1B lottery win increases total firm employment by 0.83 additional employees, 0.3 of which is accounted by a newly hired college-educated immigrant. As such, we do not find not sizable crowd-out of native workers. For firms with less than ten employees, the effect is even larger, as winning an additional I-129 application increases firm size by one employee. While lottery-losing firms are likely able to hire some immigrants regardless, lottery-winning firms expand in other types of complementary workers such as non-college graduates. Lottery-winning firms also enjoy increases in revenue generation and survival. High-wage, high-productivity firms expand more and even exhibit crowd-in of natives with college degrees when winning the lottery. Finally, the impact of winning the lottery on incumbent workers is small on average, but lottery wins generate positive wage spillovers for young, low tenure college graduates and non-college workers but negative wage spillovers for young, high tenure, native college graduates.

Our analysis makes two contributions that are relevant for understanding the broader impacts of high-skill immigration. First, we directly estimate how firms are affected when they are unable to hire the foreign-born workers they sought through the H-1B program—these effects

are economically meaningful in their own right, given the scale and salience of H-1B use among high-productivity firms. As selective employer-sponsored visa programs become more popular in high-income countries (for instance Canada's Global Talent Stream or the UK's Skilled Worker Visa program among others), our results provide insights on the effect of such programs on domestic firms and workers. Second, we provide a credibly identified estimate of the elasticity of substitution between immigrant and native workers within firms—a key input to any welfare analysis of immigration policy (e.g., Ottaviano and Peri, 2008). However, both contributions come with important scope conditions. The elasticity we estimate is necessary for assessing welfare implications, but not sufficient: doing so also requires understanding labor market reallocation across firms and sectors (Dustmann et al., 2016; Mitaritonna et al., 2017). Similarly, understanding the full impact of H-1B policy requires complementing our firm-level results with evidence on how non-H-1B firms and non-incumbent workers are affected. We view addressing these broader general equilibrium questions as a valuable direction for future research.

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A   Additional Details on the H-1B Program

The H-1B visa was created in 1990 to provide authorization for college-educated foreign nationals to work in specialty occupations in the United States. The program has undergone a variety of reforms primarily related to its quota, which was initially set at 65,000 per year. In 1998, Congress increased the quota to 115,000 for FYs 1999 and 2000, and to 195,000 for FYs 2001-2003. For FY 2004, the quota returned to its initial level of 65,000, but Congress authorized an additional quota of 20,000 visas for those with a master's degree or higher from a US educational institution (referred to as the "Advanced Degree Exemption" or ADE Cap). In 2000, US universities and other nonprofit entities were exempt from H-1B quotas, and in 2004, governmental entities also became exempt. Since FY 2004, the number of visas issued has exceeded the cap in each year mainly due to these exemptions.

The entire Regular Cap of 65,000 was distributed by random lottery for the first time in 2007, our year of study. A similar procedure was used to distribute visas under the FY 2009 cap, with 163,000 applications received for 85,000 cap visas between April 1-8, 2008, implying a similar win rate. However, ADE visa applications were first included in the lottery for the Regular visa cap (of 65,000). Nonselected ADE applications were then pooled into a second lottery for the ADE Cap (20,000). This substantially complicates analysis of the 2008 lottery.

As mentioned in Section 2, the H-1B program is the primary pathway to hire skilled workers from abroad. The alternative programs to hire high skill workers are significantly smaller. The L-1 and TN programs are less than 15% of the size of the H-1B (Morales, 2023), while the H-1B1 and E-3 visa usage is even smaller. Graduates from a US institution are allowed to work in the United States for 1-2 years after graduation through the OPT. Over 40% of new H-1B visa winners in 2014 were F-1 visa holders, and at least 30% of students on OPT transition to an H-1B status (Chen, 2019). In 2008, the OPT program extended the period of work authorization from twelve months to twenty-nine months for individuals graduating in selected STEM fields (Amuedo-Dorantes et al., 2023b).

B   LCA Predating and Anticipation

LCA predating, first documented in Peri et al. (2015a)[38], refers to the practice whereby firms seeking to apply for cap-subject H-1B workers file LCAs in advance of the start of the H-1B filing period (which begins on the first business day in April of each year). Filing rules require that LCAs must be submitted to the Department of Labor no earlier than 6 months from the employment start date listed on the LCA. Because the H-1B program stipulates that new visa recipients may only begin working in October–the start of the following fiscal year–LCAs should conceivably be filed just prior to when the H-1B application (which consists of the approved LCA) should be submitted.

In an ideal scenario, a company seeking to hire an H-1B worker to start on October 1st could submit their LCA on April 1st, receive quick approval, and submit the H-1B application on the same day. However, delays in LCA processing, possibly due to errors or requests for further information, would delay the ultimate submission of the H-1B application. As such, firms began to predate the start dates on the LCA application so that they could file and receive LCA approval before the start of the H-1B filing. Predating helps gaurantee that firms would be ready to submit complete H-1B applications on the first business day of April.

Firms began predating applications following increasing demand for H-1B visas. Figure A1 shows the number of days the H-1B cap took to distribute for each fiscal year.[39] The vertical axis shows days-in-filing, calculated

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[38]Credit is due to Chad Sparber who first noticed predating in LCA data.

[39]This figure is similar to Figure 1 in the main text. For clarity, we use Fiscal Years in the horizontal axis to

as the number of days between the start of the application filing period (usually April 1 or 2), and the final receipt date (i.e., the day that USCIS received enough petitions to hit the cap).

Figure A1: Days Needed to Reach H-1B Cap by Fiscal Year
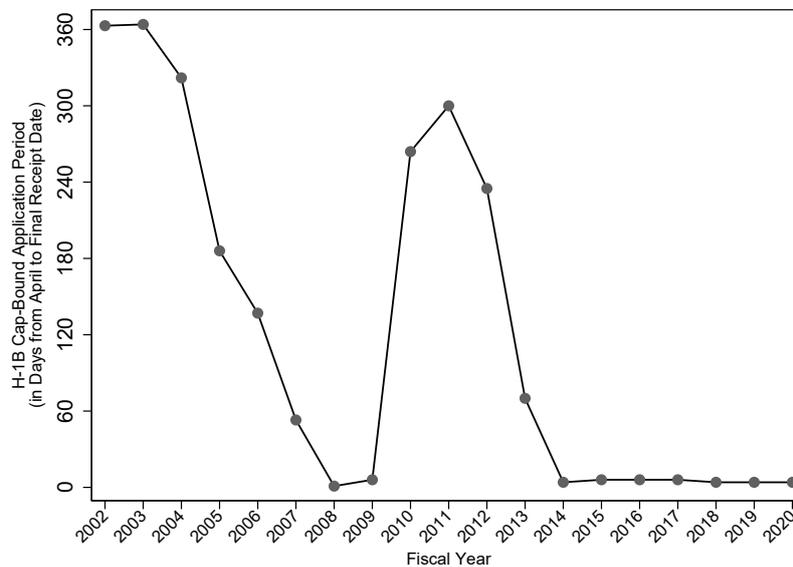
In earlier application seasons, such as those distributing visas for FYs 2002, 2003, and 2004, the cap was either not exhausted or took nearly an entire year to distribute. For FY 2005, in which USCIS began accepting applications in April 2004, demand noticeably increases for H-1B visas as the number of days-in-filing falls – the H-1B quota takes increasingly fewer days to be reached. Compared with FY 2004, H-1B distribution for FYs 2005, 2006, and 2007 took only six months, four and a half months, and two months, respectively. For FYs 2008 and 2009, (where filing opened in April 2007 and 2008), USCIS received an overwhelming number of applications on the first day of the filing period and held lotteries. The effects of the Great Recession are seen in FYs 2010-2012 as demand slows down, and the days-in-filing increase once again to almost a full year. As the economy recovers from the Great Recession, the days-in-filing once again falls quickly. Each year since FY 2014, cap-subject H-1B visas have been distributed by lottery.

We then track the pattern of LCA filing for applications corresponding to each fiscal year and see how it changes with demand and the days-in-filing. Figure A2 shows the total number of approved LCA applications by week, for each H-1B filing season from FY 2003-2017. Note the weeks correspond to the calendar year, so for example, week 1 for the FY 2003 figure refers to the week of January 1, 2002 and week 40 refers to the week of October 1, 2002 – the start of FY 2003. A vertical line is displayed for the week in which the H-1B application season begins (i.e., the week of April 1, which usually corresponds to week 13 or 14 of the calendar year).

In times of low demand, when the H-1B cap takes a long time to distribute, we see roughly uniform LCA filing throughout the calendar year. As days-in-filing starts to decrease, we start to see a mass of LCA applications grow starting around April 1. FY 2008 and 2009 are clear that a huge mass of LCA applications are filed and approved not only in the first week of April, but also in the weeks prior. For the FY 2008 lottery, which is the focus of this paper, LCA applications begin spiking around weeks 8 and 9, which correspond to the beginning of March. Hence, we set March 1st as the date in which we start counting LCAs, as the spike most likely includes many lottery-subject applications.

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align with analysis of LCA analysis below.

As the Great Recession occurs, this LCA predating slows down. As the recovery happens and demand once again surges, predating behavior increases as the number of LCA applications filed and approved grows around April 1. Since FY 2014, it is clear that predating is now the norm, and most LCA applications appear to be filed before April 1.

Such predating behavior presents at least two issues. First, this implies particular selection of firms into the lottery. In its simplest form, this means firms that could anticipate high demand and a lottery, ended up predating LCA applications to ensure they could submit a complete H-1B application by April 1. Those firms that either were unaware of overall demand, were unaware of the lottery protocol, or for some other reason could not match with a worker and get a completed application ready in time, did not partake. Ultimately, the firms that select into the lottery might be different from the set of firms that apply in non-lottery years. This needs to be considered when thinking about the external validity of these findings.

To get a sense of how characteristics of H-1B applicants may differ, we plot the share of applications going to computer-related occupations and to the top two H-1B receiving countries, India and China, for each fiscal year in Figure A3. Periods that had high demand, a very small number of days-in-filing, and large predating behavior, are shaded in blue. A clear pattern emerges that the share going to computer scientists and to India/China tends to rise during these periods. Hence, it is possible that firms with strong networks to computer scientists in India were those that either were able to anticipate the lotteries and/or able to quickly match with workers so that they could apply by April 1.

A second issue pertains to the internal validity of this natural experiment. Firms anticipating the lottery may be able to partially mitigate its impact. For example, a firm that anticipates a lottery and believes the odds to be equivalent to a coin flip can mitigate the lottery rationing by applying for double the amount of workers it actually needs. If the overall win-rate of the lottery is 50% and a firm needs to hire 1,000 workers, then it can get close to this target in expectation by filing 2,000 applications. However, it should be noted that this is not a trivial task. Companies need to file for LCA approval, and actually match with real workers in order to file the H-1B application. They also need to pay filing fees associated with all 2,000 applications and attorney fees where applicable. In practice, it is more likely that this mitigation strategy could reasonably be pursued by very large firms looking to hire a large number of workers, rather than smaller startup companies. In later checks, we remove the largest applicants from the analysis as a robustness check on this.

While LCAs exhibit predating, with many employment start dates listed earlier than October 1, 2007, rules stipulate that employment start dates listed on I-129 applications should not be earlier than October 1. As a result, "predating" manifests in I-129 forms through an earlier than usual employment end date. Instead of a normal three-year duration (e.g., October 1, 2007-October 1, 2010), many applications list end dates that are one or two months early (e.g. October 1, 2007-September 1, 2010). Thus, by predating, firms are forgoing one or two months of potential employment of their H-1B worker to make sure they are able to file I-129 applications on time.

C   Filtering Probabilistic Outliers

We identify probabilistic outliers based on the likelihood of observing the stated number of applications given the number of wins we observe and the overall likelihood of success for any given application. To do this, we use the negative binomial distribution, which describes the number of failures (applications - wins) expected given a number of trials (applications), successes (wins), and true win probability. From publicly available numbers referred to above, we infer that the true win probability for any application should be $\frac{65,000}{123,480} = 0.5264$. We then filter out any observations whose application-win combinations are low likelihood given this probability of

Figure A2: LCA Application Filing by Week, FY 2003-2017
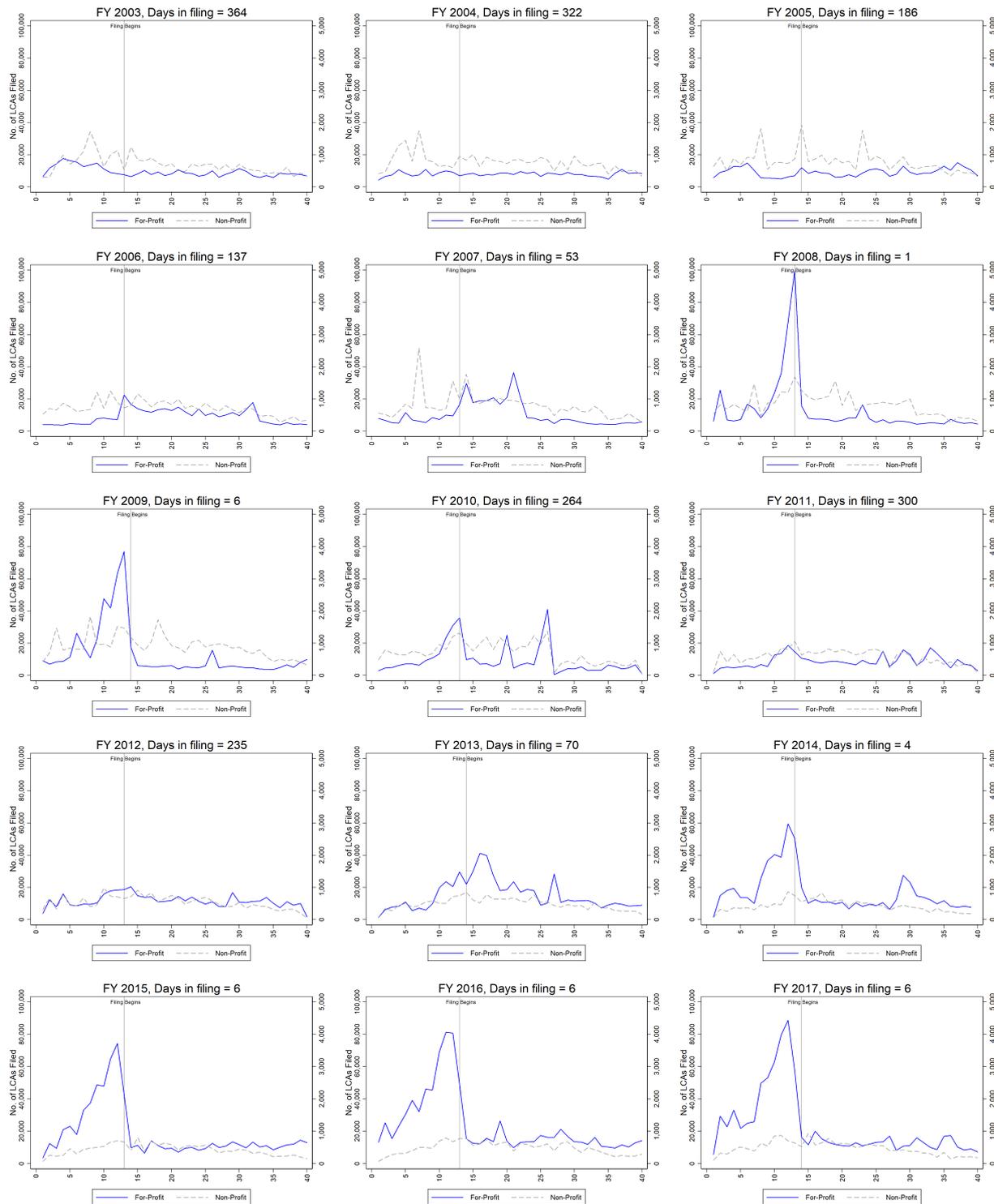
Note. Figure shows LCA filing by week for each calendar year. Note that 1 refers to week 1, hence the week of January 1. Week 13/14 is marked by the vertical black line and refers to the week of April 1, the start of the H-1B filing period. Week 40 refers to the week of October 1, or the start of the subsequent fiscal year for which firms are applying for H-1B visas. We separate applications into for-profit (cap-subject) and nonprofit (cap-exempt).

success—those that occur with probability less than 0.01.[40] We refer to samples that drop such outliers from

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[40]For example, a firm that is observed to have 1 win in I-129 data with a 52.64% chance of winning any given application has a 0.005 probability of having submitted 7 or more lottery applications and a 0.011 probability

Figure A3: Compositional Changes in Periods with High Predating
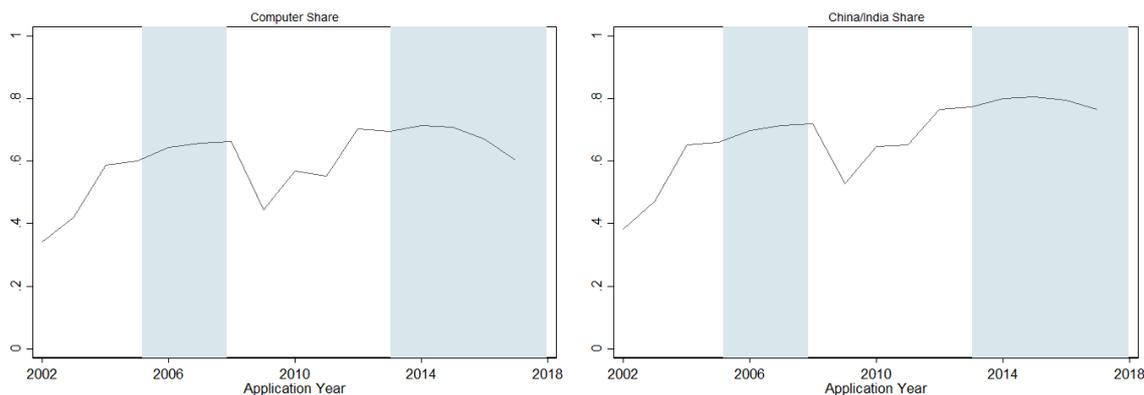
Note. Figure shows the share of approved I-129s awarded to computer-related occupations, and also to individuals from India and China for each fiscal year between 2002-2018. Regions shaded in blue show periods with a very small number of days-in-filing and also an associated high amount of LCA predating activity.

the dataset as "filtered." This helps ensure that the remaining set of firms have more or less "reasonable" application counts.

Figure A4 illustrates the utility of filtering out probabilistic outliers visually. Figures A4a and A4b compare the distributions of the raw win rate (without dropping probabilistic outliers) and the filtered win rate (that drops outliers), to a truly random win rate where we randomly draw 52.64% of observed applications in our data. Due to LCAs likely being an overcount of the true number of applications, the raw win rate has greater mass in the left tail of the distribution toward smaller win rates, relative to the truly random distribution. The filtered win rate distribution still has some extra mass in lower win rates but is closer to what would be expected in truly random data.

Figure A4c illustrates a local polynomial smoothed plot of win rates against applications. Ideally, in truly random data, there should be no correlation between win rates and applications. The raw win rates show a strong negative correlation, where average win rates decline as application size grows—a result of our denominator especially overstating the true number of applications for large applicants. After removing probabilistic outliers, the relationship is much closer to what one expects from truly random data: there is a much less pronounced correlation between Win Rate$_j$ and Likely-Lottery Application$_j$. Furthermore, as expected under the law of large numbers under a true lottery, Win Rate$_j$ converges to 0.5264 among EINs with more applications.

The visual evidence in Figure A4c is confirmed by regression analysis presented below, in Table A1, which shows a drastic decline in the correlation between Win Rate$_j$ and Likely-Lottery Application$_j$. We address the remnant correlation with our firm fixed effects, and our firm size control, as described in Sections 4.4 and 4.3.

In total, our filtering method has the benefit of retaining only firms for whom we believe Win Rate$_j \approx$ Ideal Win Rate$_j$ but at the cost of only analyzing 73,180 out of 123,480 total lottery applications.

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of having submitted 6 or more lottery applications. We thus drop any firms with 1 estimated win and 7 or more estimated lottery applications. Similarly, the probability that a firm with 20 observed wins submitted 27 or fewer applications is 0.019, and the probability that a firm with 20 observed wins submitted 26 of fewer applications is 0.0098. So, we filter out any firms with 20 estimated wins and 26 or fewer applications. Note that this procedure embeds our confidence in observing I-129 lottery wins relative to I-129 lottery applications.

Table A1: Bivariate Regressions of Win Rate$_j$ on Likely-Lottery Applications$_j$

Sample:	Unfiltered (Raw)	Filtered
Likely-Lottery Applications$_j$	-7.946***	-0.286***
	(1.310)	(0.080)
EINs (Observations)	20,072	18,963

Note. Robust standard errors in parentheses. Both "Filtered" and "Raw" refer to our measure Win Rate$_j$, but "Filtered" refers to the measure after outliers have been eliminated by the procedure described above, in Section C. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure A4: Filtered and Raw Win Rates

(a) Raw vs. Random Win Rate   (b) Filtered vs. Random Win Rate
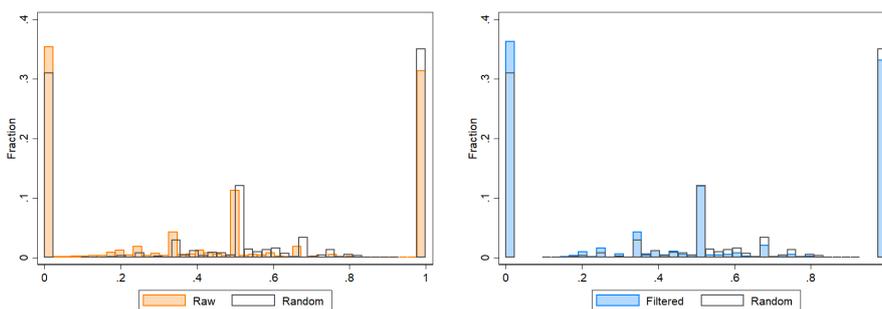
(c) Local Polynomial Smooth Plot of Win Rates vs. Applications
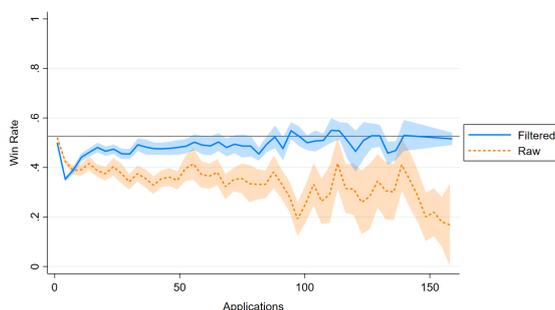
Note. All figures are constructed using I-129 data, at the EIN level. Both "Filtered" and "Raw" refer to our measure Win Rate$_j$, but "Filtered" refers to the measure after outliers have been eliminated by the procedure described above, in Section C. There are 20,072 EINs represented in "Raw" win rates, and 18,963 EINs represented in the "Filtered" win rates.

D   Robustness and Supplemental Results

D.1   Robustness

Here, we probe the robustness of our key firm-level scale and composition results along several dimensions. Our outcomes of interest are (percent) changes to total employment (Panel A of Table A2, the employment of college immigrants (Panel B of Table A2, and the employment of college natives at the firm-level (Panel C of Table A2, and our estimating equation is Equation (3). In each panel, Column (3) is our preferred specification. Column (1) omits control vector X_{jt}. Column (2) includes the initial employment control from X_{jt} but omits industry-by-year fixed effects. Column (4) includes an additional control for the inverse hyperbolic sine of Likely-Lottery LCAs$_j$ interacted with year fixed effects. Column (5) uses a binary independent variable based on whether the win rate is more than 50%. Finally, Column (6) replaces the inverse hyperbolic sine transformation with one of the recommended replacements in Roth and Chen (2022). Specifically, we define $\text{extlog}(y_{jt}) \equiv \mathbb{1}\{y_{jt} = 0\}(-2) + \mathbb{1}\{y_{jt} > 0\}\log(y_{jt})$. That is, our outcome is now the log of our employment count, but we replace the undefined $\log(0)$ with a value of -2. This choice implies that we consider going from 0 to 1, a growth rate of 200%. This is in line with the Davis et al. (1996) growth rates that are common in the firm dynamics literature.[41]

We also note that a previous draft of this paper—available upon request—used the extlog transformation throughout. Across outcomes that were used in both papers, results are unchanged qualitatively.

D.2   Likely H-1B Hiring by Origin

To further explore whether our lottery measure is capturing likely H-1B hiring, we split H-1B-like immigrants by nationality. As shown in Table A3, we separately estimate Equation (3) for Indian H-1B-like workers, Canadian/Mexican H-1B-like workers, and H-1B-like workers from all other foreign nationalities. Indians account for 48.3% of all I-129 records for new employment, while Canadians account for only 3.85%, and Mexicans for 1.28%. Besides the H-1B program, college-educated Mexicans and Canadians can work in the United States under a TN employment status, which is a specific visa category established as part of the North American Free Trade Agreement (NAFTA). Therefore, we should expect the hiring of Indians and other non-North-Americans to exhibit larger responses to the H-1B lottery relative to the hiring of Canadians and Mexicans. Table A3 illustrates that this is the case and also implies some potential for substitution toward Canadian and Mexican workers for lottery losers since those workers can be pursued through alternative channels.

D.3   Additional Difference-in-Differences Results

Table A4 presents additional results from estimating Equation (3) not provided in the main text. Results support our broad conclusions that H-1B lottery wins enable firms to expand production without generating large substitution effects on native workers.

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[41]The Davis-Haltiwanger-Schuh growth rate in x between $t = 0$ and $t = 1$ is $\frac{2(x_1 - x_0)}{x_1 + x_0}$.

Table A2: Robustness

	Panel A: y = Total Employment					
	(1) $\mathrm{asinh}(y)$	(2) $\mathrm{asinh}(y)$	(3) $\mathrm{asinh}(y)$	(4) $\mathrm{asinh}(y)$	(5) $\mathrm{asinh}(y)$	(6) $\mathrm{extlog}(y)$
Win Rate$_j$ × Post$_t$	0.076**	0.085**	0.086**	0.096***		0.126***
	(0.035)	(0.035)	(0.035)	(0.035)		(0.045)
$\mathbb{1}\{$Win Rate$_j \geq 0.5\}$ × Post$_t$					0.077**	
					(0.031)	
Employment Control		✓	✓	✓	✓	✓
Industry-Year FE			✓	✓	✓	✓
LCA control				✓		

	Panel B: y = College Immigrants					
	(1) $\mathrm{asinh}(y)$	(2) $\mathrm{asinh}(y)$	(3) $\mathrm{asinh}(y)$	(4) $\mathrm{asinh}(y)$	(5) $\mathrm{asinh}(y)$	(6) $\mathrm{extlog}(y)$
Win Rate$_j$ × Post$_t$	0.099***	0.100***	0.093***	0.104***		0.159***
	(0.022)	(0.022)	(0.022)	(0.022)		(0.032)
$\mathbb{1}\{$Win Rate$_j \geq 0.5\}$ × Post$_t$					0.080***	
					(0.021)	
Employment Control		✓	✓	✓	✓	✓
Industry-Year FE			✓	✓	✓	✓
LCA control				✓		

	Panel C: y = College Natives					
	(1) $\mathrm{asinh}(y)$	(2) $\mathrm{asinh}(y)$	(3) $\mathrm{asinh}(y)$	(4) $\mathrm{asinh}(y)$	(5) $\mathrm{asinh}(y)$	(6) $\mathrm{extlog}(y)$
Win Rate$_j$ × Post$_t$	0.012	0.008	0.010	0.011		0.024
	(0.026)	(0.026)	(0.026)	(0.026)		(0.035)
$\mathbb{1}\{$Win Rate$_j \geq 0.5\}$ × Post$_t$					0.013	
					(0.022)	
Employment Control		✓	✓	✓	✓	✓
Industry-Year FE			✓	✓	✓	✓
LCA control				✓		

Note. See Equation (3) for specification, with t indexing a calendar year and omitted period $b = 2006$. Standard errors clustered at the firm-level. Dependent variables are all measured using the LEHD in calendary Q4. "Employment control" corresponds to the inverse hyperbolic sine of employment in March 2007 interacted with time fixed effects. "Industry-Year FE" corresponds to 4-digit industry by year fixed effects. "LCA control" corresponds to the inverse hyperbolic sine of Likely-Lottery LCAs$_j$ interacted with year fixed effects. asinh() refers to the inverse hyperbolic sine transformation. $\mathrm{extlog}(y) \equiv \mathbb{1}\{y = 0\}(-2) + \mathbb{1}\{y > 0\}\log(y)$. Firm and observation counts rounded as per the US Census Bureau's disclosure rules. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A3: Effect of Lottery Win Rate on Employment of H-1B-Like Workers, by Origin

	Outcome: IHS H-1B-like immigrant workers			
Origin Region:	All	India	Mexico/Canada	Other
Win Rate$_j \times \mathbb{1}$(Year $\geq$ 2007)	0.064***	0.047***	-0.011**	0.030**
	(0.015)	(0.009)	(0.005)	(0.013)
Firms	13,500	13,500	13,500	13,500
Observations	137,000	137,000	137,000	137,000

Note. See Equation (3) for specification, where t indexes a calendar year and $b = 2006$. Standard errors clustered at the firm-level. The dependent variable is the inverse hyperbolic sine of the count of H-1B-like immigrant workers from a given origin at firm j in the fourth quarter of year t. These outcomes are measured using the LEHD. Regressions include firm fixed effects, industry-time fixed effects, and the inverse hyperbolic sine of employment in March 2007 interacted with time fixed effects. Firm and observation counts rounded as per the US Census Bureau's disclosure rules. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A4: Difference-in-Differences Estimates

Panel A: Firm Scale

	LBD Employment	LEHD Employment	Revenues	Noncollege Workers
Win Rate$_j$ × Post$_t$	0.050*	0.086**	0.266***	0.053*
	(0.028)	(0.035)	(0.068)	(0.031)
Firms	20,000	13,500	20,000	13,500
Observations	199,000	137,000	199,000	137,000

Panel B: Immigrant-Native Substitutability

	Immigrant College Employment	Immigrant H-1B-Like Employment	Native College Employment	Native H-1B-Like Employment
Win Rate$_j$ × Post$_t$	0.093***	0.064***	0.010	-0.015
	(0.022)	(0.015)	(0.026)	(0.017)
Firms	13,500	13,500	13,500	13,500
Observations	137,000	137,000	137,000	137,000

Panel C: Payroll and Mean Wages

	Payroll	Overall Mean Wage	Imm. College Mean Wage	Nat. College Mean Wage
Win Rate$_j$ × Post$_t$	0.215***	0.001	0.020*	0.004
	(0.058)	(0.009)	(0.012)	(0.012)
Firms	20,000	13,500	13,500	13,500
Observations	199,000	137,000	137,000	137,000

Note. See Equation (3) for specification. Standard errors clustered at the firm-level. Dependent variables are all transformed using the inverse hyperbolic sine. All variables are all measured using the LEHD, except for LBD Employment, Payroll, and Revenues. Regressions include firm fixed effects, industry-time fixed effects, and the inverse hyperbolic sine of employment in March 2007 interacted with time fixed effects. Firm and observation counts rounded as per the US Census Bureau's disclosure rules. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

E   Validation: Implementation of the 2020 Lotteries

E.1   Background and Measurement

In this section, we provide details on the implementation of our validation check using the 2020 lotteries. In addition to addressing the internal validity of our estimates, these results also address external validity given the inversion of economic conditions across 2007 and 2020. In the 2007 lottery, firms applied for H-1B workers prior to the Great Recession but hired them at the height of the subsequent economic downturn. In the 2020 lottery, firms applied for workers at the height of the COVID-19 pandemic, but hired these workers for a period that largely coincide with the historically tight labor markets that occurred in the pandemic recovery period.

We use information on the lottery-subject I-129 applications for 2020. The data is very similar to the FOIA data we describe in Section 3.2, with the important distinction that it also includes records of I-129 applications that were unsuccessful in the lottery. The H-1B registrations for 2020 were selected randomly in a two-step process: 1) an initial lottery among all 275,000 registrants for 65,000 regular cap slots; 2) a second lottery among losers from 1) who were filing under the ADE, for 20,000 additional slots. Only lottery winners were invited to submit finalized I-129 petitions to the USCIS.

Given the special treatment of ADE registrations in the two-step lottery, our ideal win rate for the 2020 lottery is application-type-specific. However, the FOIA data on the 2020 lottery lacks information on whether registrations were filed under the ADE. Thus, our measured win rate combines both types of applications in the numerator and denominator:

$$\text{Win Rate}_j^{2020} \equiv \frac{\text{Lottery Wins}_{j,2020}^{ADE} + \text{Lottery Wins}_{j,2020}^{REG}}{\text{Lottery Registrations}_{j,2020}^{ADE} + \text{Lottery Registrations}_{j,2020}^{REG}}, \tag{E1}$$

where REG refers to non-ADE (regular) registrations and ADE refers to ADE registrations. Since registration types are not observed, our measured Win Rate$_j^{2020}$ combines the outcomes of both lotteries. For that reason, we still need to include firm and industry-time fixed effects to control for the possibility of firms with higher proportions of ADE registrations being unobservably different than firms with lower ADE registrations. Nonetheless, pooling ADE and non-ADE applications to construct the 2020 lottery is unlikely to drive our results. First, in an earlier version of our paper, we pooled 2007 and 2008 lotteries including those in the 2008 ADE lottery, obtaining very similar results to our baseline estimates for the non-ADE lottery of 2007.[42] Second, Doran, Gelber and Isen (2022) also pool ADE and non-ADE wins to "improve statistical power" and argue that this does not drive their results for the 2005 and 2006 lotteries.

E.2   Data and Research Design

We therefore turn to an analogous event study specification to Equation (2), relaxing the assumption that Win Rate$_j^{2020}$ is exogenous and instead assuming that firms with higher levels of Win Rate$_j^{2020}$ are on parallel trends to firms with lower levels of Win Rate$_j^{2020}$, conditional on our time-varying size control:

$$y_{jt} = \sum_{\tau \neq b} \beta_\tau \left[\text{Win Rate}_j^{2020} \times \mathbb{1}(\tau = t) \right] + \Gamma X_{jt}^{2020} + \alpha_j + \alpha_{kt} + \varepsilon_{jt}, \tag{E2}$$

$$y_{jt} = \beta \left[\text{Win Rate}_j^{2020} \times \text{Post}_t^{2020} \right] + \Gamma X_{jt}^{2020} + \alpha_j + \alpha_{kt} + \varepsilon_{jt}. \tag{E3}$$

Equation (E2) is the analogous event study to Equation (2), with $b = 2020$ for LBD outcomes (when t represents a year) and $b = 2020Q1$ for LEHD outcomes (when t represents a quarter). Equation (E3) is the analogous

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[42]Results for this exercise are available upon request.

difference-in-differences specification. X_{jt}^{2020} is the inverse hyperbolic sine of 2020-Q1 LBD employment interacted with period fixed effects. The key assumption is that firms with different win rates in 2020 were not trending differently, conditional on size and the other fixed effects.

The primary Census project was approved access to LEHD records through 2012; we therefore use a different Census project with access to the LEHD through 2021. This switch, however, results in analyses of the 2020 lottery being performed on a slightly different set of 25 states that the new Census project was approved to access.[43] This fact provides a second source of external validity on our 2007 results. Ideally, we would also compare how the 2020 lottery differs when using the real applications vs the LCAs. However, in 2020, LCAs were not required before submitting an application, so the LCA records for lottery losers are incomplete.

E.3   Additional Results

Figure A5 then delves into these results by breaking Figure 7a into impacts on selected sub-groups of workers, as in Figure 5, and finds similar results. We do not find evidence of non-parallel trends in any outcome prior to the lottery, but we do find highly significant effects on immigrant utilization when the H-1B hiring period starts in 2020-Q4. The magnitudes of this increased utilization is smaller, perhaps indicating an increased ability of lottery losers to hire immigrant workers through other channels in 2020.[44] Meanwhile, we do not find any evidence of substitution away from or crowding in of native workers.

Next, Table A5 presents results from Equation (E3) for key indicators of firm scale and firm performance from the LBD. As in the 2007 lottery, 2020 lottery wins lead to statistically significant increases in firm scale and survival. Magnitudes are similar to 2007 in terms of scale and survival, but there is no detectable increase or decrease in measures of labor productivity from 2020 lottery wins. In sum, our 2020 results continue to support our findings in 2007, that scale effects are large enough to combat the need to substitute away from native workers.

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[43]The states are Alabama, Arizona*, Arkansas*, California*, Colorado*, District of Columbia*, Delaware*, Idaho*, Illinois*, Indiana*, Iowa*, Kansas*, Maryland*, Michigan, Montana*, Nebraska, Nevada*, New Mexico*, North Dakota*, Ohio, Oklahoma*, Oregon*, Pennsylvania*, Tennessee*, and Utah. A * indicates a state that is also included in our 2007 lottery results from the LEHD.
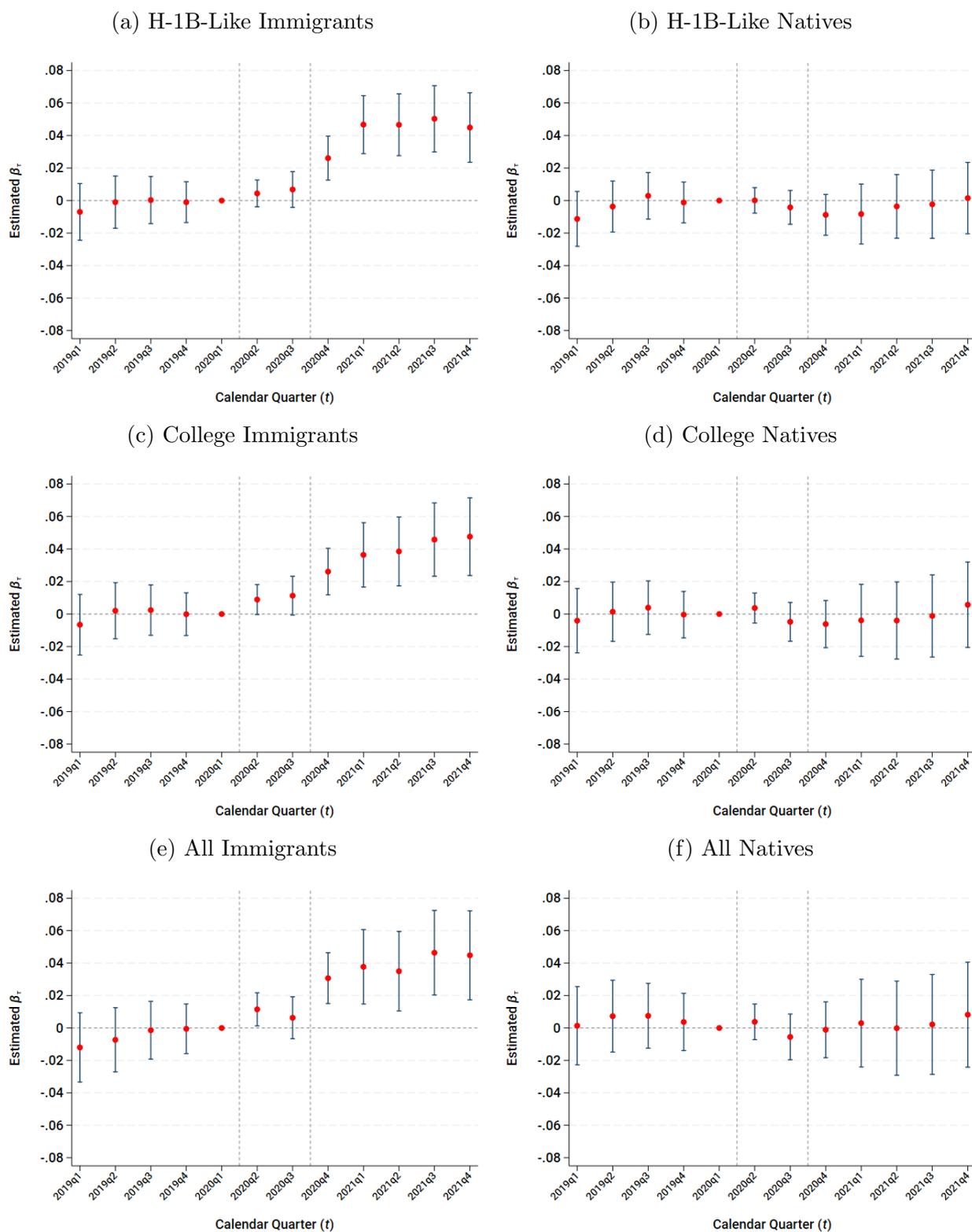
[44]Unlike in 2007, the H-1B lottery was anticipated and in place for several years by 2020, allowing for greater possibility for planning of contingencies for losing firms.

Table A5: Effect of 2020 Lottery Win Rate on Firm Performance Measures

| | (1) | (2) | (3) | Conditional on Active | |
| | | | | (4) | (5) |
	$\mathbb{1}$[Active]	Revenues	Payroll	Revenues per Worker	Average Wage
Win Rate$_j$ × Post$_t$	0.016***	0.190**	0.213***	-0.018	0.005
	(0.005)	(0.093)	(0.077)	(0.013)	(0.007)
Firms	33,000	33,000	33,000	33,000	33,000
Observations	262,000	262,000	262,000	262,000	262,000

Note. See Equation (E3) for specification. Standard errors clustered at the firm-level. Each outcome are measured using the LBD. Outcomes in Columns (2) through (5) are transformed using the inverse hyperbolic sine. Regressions include firm fixed effects, industry-time fixed effects, and the inverse hyperbolic sine of employment in March 2020 interacted with time fixed effects. Firm and observation counts rounded as per the US Census Bureau's disclosure rules. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure A5: Effect of 2020 Lottery Win Rate on Employment of Selected Subgroups

(a) H-1B-Like Immigrants

(b) H-1B-Like Natives

(c) College Immigrants

(d) College Natives

(e) All Immigrants

(f) All Natives

Note. See Equation (E2) for specification, where t indexes a calendar quarter and omitted period $b = 2020Q1$. We plot the estimated coefficients $\hat{\beta}_\tau$ and their 95% confidence intervals. First vertical dashed line separates pre-lottery quarters (placebo tests) from quarters in which lotteries may have affected behavior but before 2020 lottery H-1B workers could be hired (anticipation tests). Second vertical dashed line separates pre-lottery-hire quarters from post-lottery quarters (treatment effect estimates). Number of firms is roughly 25,000 (rounded as per the US Census Bureau's disclosure rules). Standard errors clustered at the firm-level. Dependent variables in each subfigure are the inverse hyperbolic sine of the worker count from the labeled group. These outcomes are measured using the LEHD. Regressions include firm fixed effects, industry-time fixed effects, and the inverse hyperbolic sine of employment in March 2020 interacted with time fixed effects.

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E.4 Theory Appendix

In this section, we provide additional details on the derivations of the theoretical framework in Section 5.5.

The firm's objective is to maximize variable profits:

$$\pi_j = PY^{\frac{1}{\varepsilon}}\psi_j^{\frac{\varepsilon-1}{\varepsilon}}L_j^{\delta\frac{\varepsilon-1}{\varepsilon}}\left(\alpha I_j^{\frac{\sigma-1}{\sigma}}+(1-\alpha)N_j^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}(1-\delta)\frac{\varepsilon-1}{\varepsilon}} - w_L L_j - w_I I_j - w_N N_j,$$

where P is the "CES ideal price index" for the final good and ψ_j is firm-level TFP.

First-order conditions are:

$$\left(\frac{\varepsilon-1}{\varepsilon}\right)\delta PY^{\frac{1}{\varepsilon}}\psi_j^{\frac{\varepsilon-1}{\varepsilon}}L_j^{\delta\frac{\varepsilon-1}{\varepsilon}-1}H_j^{(1-\delta)\frac{\varepsilon-1}{\varepsilon}} = w_L \tag{E4}$$

$$\left(\frac{\varepsilon-1}{\varepsilon}\right)(1-\delta)PY^{\frac{1}{\varepsilon}}\psi_j^{\frac{\varepsilon-1}{\varepsilon}}L_j^{\delta\frac{\varepsilon-1}{\varepsilon}}H_j^{(1-\delta)\frac{\varepsilon-1}{\varepsilon}-1}(1-\alpha)N_j^{-\frac{1}{\sigma}} = w_N \tag{E5}$$

$$\left(\frac{\varepsilon-1}{\varepsilon}\right)(1-\delta)PY^{\frac{1}{\varepsilon}}\psi_j^{\frac{\varepsilon-1}{\varepsilon}}L_j^{\delta\frac{\varepsilon-1}{\varepsilon}}H_j^{(1-\delta)\frac{\varepsilon-1}{\varepsilon}-1}\alpha I_j^{-\frac{1}{\sigma}} = w_I. \tag{E6}$$

Totally differentiating the first-order condition for skilled natives, and setting $d\log w_N = 0$ yields:

$$\frac{d\log(N_j)}{d\log(I_j)} = \frac{\{(\varepsilon-\sigma)-\delta(\varepsilon-1)\}s_I}{(\varepsilon-1)(1-s_L)(1-\delta)-\{(\varepsilon-\sigma)-\delta(\varepsilon-1)\}s_N},$$

where s_x represents the share of factor x in revenue. $d\log w_N = 0$ comes from the fact that firm j does not have influence over the market wage for native college workers, w_N.

While less straightforward than the simple two-factor case, the impact on native employment still depends on the relationship between the output demand elasticity and the elasticity of substitution. The numerator is less than zero when σ is sufficiently larger than ε: $\frac{\frac{\varepsilon-1}{\varepsilon}}{\frac{\sigma-1}{\sigma}} < \frac{1-s_L}{1-\delta}$. The denominator is difficult to sign, but it is positive for admissible ranges of parameter values.

F Comparison with Doran, Gelber and Isen (2022)

Our results provide new evidence on the impact of immigration on firm-level outcomes. To put our findings in context, we next compare them with the findings of Doran, Gelber and Isen (2022), henceforth DGI, who also use H-1B lottery variation and study the impact of winning the lottery on total firm employment.

Using IRS data on total employment and profits, DGI focus predominantly on the H-1B lotteries that occurred in 2005 and 2006, where only the subset of firms that applied for H-1B employment on the day that the cap filled were included. For these smaller lotteries, they have the ideal data since they observe both actual applications and lottery winners. They find that one additional H-1B win reduces the total employment at the firm by 0.5, while in our results, we find that one additional H-1B win increases total firm employment by 0.83 workers.

Here, we highlight one reason that our results likely diverge from DGI, owing to a difference in the set of firms being studied across the two papers and the environments they faced. Specifically, firms that applied for visas on the last day of filing for 2005 and 2006 (used by DGI) likely differed in their need for H-1B workers than the full sample of H-1B applicants that applied in 2007 (as in our paper). We arrive at this conclusion by using our FOIA USCIS data to closely match the DGI 2005 and 2006 sample, as seen in Table A6. We then use this data to highlight some key differences across the main sample used in DGI and the main sample used here.

A key difference between the 2005 and 2006 lotteries (DGI) and the 2007 lottery (here) is that firms in 2005

Table A6: Doran, Gelber and Isen (2022) Sample

	As reported in Doran, Gelber and Isen (2022)	Author's Calculations
Total CY 2005-2006 (FY2006-2007) Lottery EINs	2,750	2,840
EINs participating in lottery in both years	300	311
2005 Regular Lottery Win Rate	0.04	0.06
2005 ADE Lottery Win Rate	0.17	0.16
2006 Regular Lottery Win Rate	0.98	0.94
2006 ADE Lottery Win Rate	0.55	0.58

Note. Left column: taken from text of Doran, Gelber and Isen (2022). Right column: based on authors' calculations of USCIS FOIA data.

and 2006 had several days to file applications for H-1B workers that were not subject to a lottery. In Table A7, we show that the majority of firms in the 2005 and 2006 lotteries had done so, with success. For example, in the 2005 regular lottery, firms that were subject to the last-filing-date lottery had already procured a mean of 18.8 H-1B workers and a median of 2 H-1B workers between April 1, 2005, and August 9, 2005 (the last day of filing was August 10, 2005). In the 2005 regular lottery, 63% of firms participating had already succeeded in getting a USCIS approval for and H-1B worker before the last day of filing. Numbers are similar for the 2006 regular and 2006 ADE lottery and more modest for the small 2005 ADE lottery. In this paper, these numbers are zero in the 2007 regular lottery, as all applications were subject to the lottery.

We therefore believe it is fair to posit that the effects we find in this paper combine the intensive and extensive margin—the ability for a firm to get *any* H-1B workers with the ability for a firm to meet its exact demand for H-1B workers—whereas DGI primarily identify intensive margin effects—the effect of getting the marginal H-1B worker into a firm. In our view, *both* of these effects are of substantial policy interest. Policies that allow intensive users of the H-1B program to further increase usage may be more likely to feature crowd-out, as found in DGI, whereas policies that expand the availability of H-1B workers to a wider swath of firms may be more likely to feature scale expansion and limited crowd-out, as found here.

Beyond Table A7, we also note that the fact that demand for H-1B workers was high enough that I-129 applications exceeded the cap right away in 2007 but not high enough for that to be the case in 2005 or 2006 indicates that applicant firms may have valued H-1B workers more in 2007, potentially due to shortages in the labor market as a whole. In this case, it is natural to think that outcomes for firms in the 2007 were more reliant on getting H-1B workers.

Finally, while DGI cannot directly observe whether the displaced workers are natives or immigrants; in their Appendix Table 13, they use imputation methods to predict workers' nativity. In this analysis, they do not find evidence of native crowd-out due to immigration, which is consistent with our results. The key difference in results comes from immigrants, as they find that the displacement effects seem to be driven by near one-for-one reduction in employment of other immigrants at the firm. This echoes our results in some ways: in Table 3, we find that the only group potentially experiencing statistically significant crowd out is immigrant college workers (with 95% CI upper bounds all below 1). That is, despite the fact that each I-129 approval represents the right to hire one immigrant college worker, we find that each I-129 approval only leads to 0.287 additional immigrant college workers in Table 3 Panel A. Hence, incumbent, previously-hired H-1B workers may have been crowded-out. When put in these terms, the key difference between our results and DGI, then, is that we do not find *full* crowd-out of other immigrant college workers.

Moving beyond comparisons *across* lotteries, DGI also present an estimate of crowd-out from the 2007 lottery as a robustness check. They also proxy the number of applications with the number of LCAs and find that

Table A7: Cap-Subject H-1B Approvals Prior to Lottery

		Mean	Median	Prop. ≥ 1	Mean Filled Demand	Number of EINs
Doran, Gelber and Isen (2022)	2005 Regular Lottery	18.8	2	0.63	0.37	1,148
	2005 ADE Lottery	2.31	0	0.39	0.20	244
	2006 Regular Lottery	15.7	1	0.59	0.32	1,612
	2006 ADE Lottery	31.3	2	0.64	0.39	232
This paper	2007 Regular Lottery	0	0	0	0	$\approx 20,000$

Note. Calculations based on our identification of the Doran, Gelber and Isen (2022) sample in our USCIS FOIA data. See Table A6. "Filled demand" is defined as the number of cap-subject H-1B approvals prior to the lottery divded by the sum of the number of cap-subject H-1B applications prior to the lottery and the number of cap-subject H-1B applications in the lottery.

additional H-1B win increases firm employment by just 0.36 employees (crowd-out of 0.64).[45] While this estimate is lower than our estimate of 0.87 additional employees per H-1B hire, we note that our estimates are contained within their confidence intervals.

We also want to emphasize two key improvements to the DGI approach *within* the study of the 2007 lottery. First, as discussed in Section 4, we improve the measurement of the lottery by removing the applications that were part of the ADE Cap, applications that were likely submitted for purposes other than new employment, and firms that featured extreme outlier application counts. Second, as discussed in Section 4.4, when using LCAs to proxy for applications, we believe it is prudent to go beyond cross-sectional regressions and implement a difference-in-differences approach that controls for firm, industry-time, and size-time fixed effects in order to isolate the random component of the lottery.

In sum, we believe this paper provides an important complement to the well-identified estimates contained in DGI regarding the last-day-of-filing lotteries conducted in 2005 and 2006. Our lottery affected a larger number of firms, and losses in our lottery froze more firms out of the H-1B procurement process altogether. In our view, it is therefore natural that the two papers would find different crowd-out estimates. The "right" estimate of crowd-out owing to H-1B hiring will therefore depend on the context and policy in question.

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[45]Using the sample of all firms, they estimate that one H-1B win generates a decrease in employment of *other* workers by 0.64 employees, after subtracting one to total firm employment. This is consistent with an increase in total firm employment of 1-0.64=0.36 workers.