Visa Access and the Geography of Skilled Work: Evidence from the H-1B Lottery

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Abstract

We compare H-1B lottery winners and losers, matching administrative lottery records to LinkedIn career histories and US patent applications. We use random lottery selection as an instrument for receiving a visa. Whether losing the initial lottery has lasting effects depends on workers' ability to reach US employment through other routes and on whether worker-firm matches can be preserved abroad. Over three to six years, receiving a visa raises the probability of starting a US job by 2.4 percentage points, yet 65.7% of registrants who lose their initial H-1B lottery eventually start a US job. Failing to receive a visa also raises the probability of starting a foreign position at a US-headquartered company by 1.7 percentage points, a 44% increase relative to the 3.8% mean, including positions within the sponsor's corporate family. Receiving a visa also affects early careers, raising annual salary by 6.7% and the likelihood of filing a patent application by 38%. We interpret these estimates as evidence on where the frictions associated with the H-1B lottery bind. H-1B access changes where some worker-firm matches are realized and affects early-career outcomes, while adjustment through other routes allows most initial lottery losers eventually to start a US job.

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1. Introduction

What happens to a sponsor-worker relationship when the worker's H-1B registration is not selected? The annual cap is 65,000 new H-1B workers, plus up to 20,000 additional places for workers with advanced degrees from US institutions. Demand exceeds these quotas, so the sponsor may need another way to employ the worker. It may replace the worker in the United States or preserve the relationship abroad. The worker may also report a US job later through another lottery or immigration pathway.

Replacement reallocates employment across workers, while a relationship abroad reallocates activity across countries. If workers who lose their initial H-1B lottery take US jobs later, only the route and timing of US work changes. Visa counts do not reveal these responses. Sponsor-level employment can combine the registrant's response with other hiring inside the firm. We therefore ask where registrants work after the lottery and whether the initial worker-firm match is preserved and, if so, where it is realized. We also ask how initial access affects workers' early-career outcomes.

We study these questions by tracking worker career outcomes following the lottery. We begin with administrative records on 1,804,286 eligible H-1B registrations for FY2021 through FY2024. We match 61,839 registrations (covering 47,380 workers and 24,127 sponsoring employers) to LinkedIn career histories from Revelio Labs. These histories follow reported job starts across countries and employers for three to six years and record education and salary. Because workers report their own histories, the data follow them out of the United States. We also link registrants to USPTO patent applications.

We use the H-1B lottery as a source of exogenous variation in initial H-1B access. Because registrations typically exceed quotas, USCIS randomly selects a subset of registrations. The agency first selects against the 65,000 worker cap unconditionally among all registrants, then again among advanced-degree registrations not selected in the first draw for an additional 20,000 places. In our matched sample, 37.0% of registrations are selected. Selection permits the sponsoring employer to file an I-129 petition, but it does not guarantee approval. Among selected registrations, 79.8% of petitions are approved.

We use an approved petition as our measure of visa receipt and instrument it with lottery selection. The resulting estimates describe the effect of receiving a visa for registrants whose petition approval changes when their registration is selected. The exclusion restriction requires that lottery selection affect subsequent outcomes only through petition approval. This assumption is plausible because selection affects H-1B access only by determining eligibility to file a petition. We later show that selected and non-selected registrations are balanced on pre-lottery characteristics, consistent with the random assignment underlying our research design.

We match registrations to LinkedIn profiles in stages. We first match the sponsoring

employer named in the registration data to a Revelio company using employer identification numbers and company names. Within that matched employer, we then search for profiles reporting a US position that overlaps the lottery draw year. Among these candidates, we require gender and country of birth to match the registration and the birth year inferred from the profile to fall within two years of the reported birth year. We retain a registration only when its demographic combination is unique within the sponsor and cohort and exactly one profile satisfies all of these criteria. This procedure yields 61,839 matched registrations representing 47,380 workers and 24,127 sponsoring employers.

We construct the match using only fields observed for all registrations. Selected registrations that proceed to an I-129 petition contain additional information, but using these fields would make match quality depend mechanically on lottery selection. We instead hold them out and use them to assess match quality ex post. Among 17,712 matched workers with approved petitions, 96.0% satisfy at least one withheld correspondence criterion. The profile reports the petition degree or a higher degree for 89.0% of workers with usable degree information, the petition worksite state appears in at least one of five profile-side sources for 84.8%, and the petition job title matches for 65.3% of workers with raw title data. These comparisons provide a validation check on the matching procedure.

The matched sample is not a random cross-section of the registration data. To enter the sample, a profile must report a US position at the sponsor during the draw calendar year. This window extends beyond the March lottery, so some information used for matching is observed after selection. The selected share in the matched sample is 2.4 percentage points higher than the 34.6% share in the matchable sample. Match rates are lower at larger sponsors because more registrants share the same observed characteristics (Appendix Tables A1 and A2). The matched sample nevertheless closely resembles the larger matchable pool in gender, birth year, and representation from India and China, the two largest countries of birth in our data. We therefore view the estimates as most informative about registrants with detailed public career profiles, especially those sponsored by smaller employers.

Within the matched sample, winning and losing registrations remain similar on most characteristics measured before the lottery. Nearly every standardized difference is below 0.05. The main exceptions are that selected registrants are about nine months younger and 1.4 percentage points less likely to be men. These differences, however, also appear in the universe of H-1B registrations and are arguably not a product of our matching procedure. The regressions control for age, gender, and other measured characteristics from before the lottery.

With this design in place, we turn to the central question: does initial H-1B access affect whether workers subsequently start a US job? We find that receiving a visa raises

the probability of reporting a new US position during the three-to-six-year follow-up. A visa recipient is 2.4 percentage points more likely than a comparable registrant who does not receive one to report such a start, a 3.5% increase relative to the 67.8% mean. Yet 65.7% of workers who lose their initial H-1B lottery also report a new US position. This combination is consistent with workers using other routes to reach a later US position.

We next ask whether limited H-1B access shifts worker-firm matches abroad. A registrant who does not receive a visa is 1.7 percentage points more likely to start a foreign position at a US-headquartered company, a 44.2% increase relative to the 3.8% mean. Foreign positions at US-headquartered companies account for 38% of the mean rate of foreign job starts but 44% of the estimated effect of not receiving a visa. Within the initial sponsor's corporate family, the difference is 1.1 percentage points, or 62.2% of the 1.7% mean. Because matching begins with a US position at the sponsor, this outcome links the same worker's initial sponsor match to a later foreign position within the same corporate family. The estimate is consistent with some worker-firm matches being preserved abroad.

One might ask whether these foreign positions reflect workers' own opportunities rather than a response by the firm. The sponsor-family result follows the initial relationship more closely than the broader US-headquartered-company outcome, but it does not identify who chooses the foreign location. We find no statistically significant change in the geographic response as home-country income rises, which weakens a simple explanation in which richer home countries alone drive the response. Worker opportunities may nevertheless shape where registrants go. Taken together, these patterns are consistent with firms facilitating the continuation of some worker-firm matches abroad.

We next turn from where worker-firm matches are realized to how initial H-1B access affects workers' early-career outcomes. We begin with salary. Within 24 months, a visa recipient earns 6.7% more annually than a comparable registrant who does not receive one. Applied to the $115,315 mean, this difference equals approximately $7,731 for each year it persists. Receiving a visa does not change whether salary data are available, reducing concern that differential coverage explains the result. Because the estimate conditions on observing a newly reported position with salary data, it describes salary among those positions rather than unconditional earnings.

Beyond salary, initial H-1B access also affects education, entrepreneurship, and innovation. Receiving a visa lowers the probability of starting a new educational program by 2.6 percentage points, a 45.3% decline relative to the 5.7% mean, with the reduction concentrated at US institutions. It also lowers the probability of entering a founder-classified role within 24 months by 0.33 percentage points, or 27.9% of the mean. At the same time, receiving a visa raises the probability of filing a patent application within 18 months by 0.26 percentage points, a 38.3% increase relative to the

mean.

We also ask whether workers who lose their initial H-1B lottery and later start another US job are pushed into positions for which they are overqualified. We find no evidence of this. Their subsequent jobs do not entail significant declines in salary rank or salary relative to measured qualifications. Because this comparison conditions on workers who later report another US position, we interpret it as descriptive rather than causal.

We find that initial H-1B access is one step in a longer adjustment process. Many workers who lose their initial lottery later start US jobs through other routes, while some worker-firm matches are preserved abroad. Initial access also affects early careers: receiving a visa raises salary and patenting, while reducing additional education and entrepreneurship. Among lottery losers who later start another US job, we find no evidence that they move into positions below their qualifications. Initial H-1B access therefore changes where workers and firms adjust and the path workers take early in their careers, even when many ultimately reach US employment.

Our primary contribution is to measure these adjustments at the worker level using causal variation in initial H-1B access. The estimates reveal both the friction created by the lottery and the margins through which workers and firms respond to it. Following the same registrant across employers and countries allows us to distinguish delayed US employment, the preservation of worker-firm matches abroad, and changes in early-career outcomes that sponsor-level data cannot separate. More broadly, the results show that the economic consequences of restricting initial H-1B access depend not only on whether workers ultimately reach US employment, but also on the adjustment costs they incur and where worker-firm matches are realized.

1.1 Related literature

The closest studies use H-1B lotteries to estimate effects at the sponsoring-firm level. Doran, Gelber, and Isen (2022) find that each additional H-1B worker is offset by fewer other workers at the sponsor, changing workforce composition while leaving firm patenting roughly unchanged. Mahajan et al. (2026) find that lottery wins increase the hiring of college-educated immigrants, firm scale, and survival, without a net decline in college-educated native employment at those firms. In samples of venture-backed startups, Chen, Hshieh, and Zhang (2021) and Dimmock, Huang, and Weisbenner (2022) find that greater lottery success raises financing and patenting and increases the probability of an IPO or acquisition. Sharma and Sparber (2024) model how random allocation changes firms' search for foreign workers and dissipates quota rents. Together, these studies show that sponsor responses can combine growth, substitution, and changes in recruiting behavior.

Firm-level estimates combine the sponsored worker's path with hiring and substitution elsewhere in the firm. Broader evidence likewise shows that skilled immigration can change firms' employment structures (Kerr, Kerr, and Lincoln (2015)). These estimates also stop

at the US border. Tax and payroll records end when US employment ends, so they cannot distinguish a registrant who moves abroad from one who leaves the workforce. A move that preserves the sponsor relationship from another country therefore looks the same as an exit from the labor force. We complement this work by following the registrant named in each lottery entry across employers and countries. We measure the location and timing of later jobs, including foreign positions within the sponsor's corporate family, and the registrant's own observed patent applications. This change in the unit of analysis reveals worker-level adjustments that the sponsor's net outcome aggregates. It also separates those adjustments from place-level estimates, which can combine several worker responses (Dustmann et al. (2025)).

Another literature connects skilled immigration to innovation and entrepreneurship. Kerr and Lincoln (2010) link H-1B reforms to changes in ethnic invention, Hunt and Gauthier-Loiselle (2010) estimate immigration's contribution to patenting, and Peri, Shih, and Sparber (2015) study foreign STEM workers and productivity in US cities. Bell et al. (2019) show that exposure to innovation, not ability alone, shapes who becomes an inventor. Our lottery design varies initial work authorization as one such environmental margin. Research also documents immigrants' disproportionate representation among US entrepreneurs (Azoulay et al. (2022); Kerr and Kerr (2020)), their higher patent output, citations, and patent value relative to native-born inventors (Bernstein et al. (2025)), and differences in innovative and entrepreneurial outcomes across entry visas (Hunt (2011)). These studies motivate our worker patenting and founder-classified outcomes. Our patent measure follows the named worker after random variation in initial work authorization. The founder-classified outcome is narrower: it records a profile title and does not by itself establish that the worker created a firm.

Migration policy can also shape education and career investment. Shih (2016) finds that the 2003 reduction in the H-1B cap reduced international student enrollment, while Amuedo-Dorantes, Shih, and Xu (2023) find that extensions of Optional Practical Training increased international matriculation and the measured academic quality of entering students. These aggregate enrollment responses do not show that an individual registrant returns to school when a registration is not selected. More broadly, Adda, Dustmann, and Görlach (2022) show that expected migration duration changes human-capital accumulation and earnings. Research on immigrant labor-market adjustment shows that education and initial wage position can diverge Dustmann, Frattini, and Preston (2013), that selective employment and out-migration affect measured earnings profiles Lubotsky (2007); Rho and Sanders (2021), and that formal recognition of foreign qualifications raises employment and earnings Brücker et al. (2021). Our lottery design estimates how initial H-1B access changes subsequent education and salary. The qualification analysis is descriptive among unselected registrants later observed in another US job and does not establish occupational

downgrading.

Finally, several studies research how immigration policy and immigrant networks allocate activity across locations (Glennon (2024b)). Burstein et al. (2020) show that tradability shifts adjustment to immigration shocks toward changes in output and away from displacement of local workers. Grogger and Hanson (2015) show how source- and destination-country conditions shape foreign-born US PhDs' intended locations. Foley and Kerr (2013) link ethnic inventor networks within US multinationals to activity in countries tied to those networks. At the firm level, Glennon (2024a) finds that H-1B restrictions increase employment at foreign affiliates of US multinationals, while Nayak, Moreira, and Mudambi (2025) links the 2004 cap reduction to more geographically dispersed multinational inventor networks and lower patent output. We identify a worker-level margin within this response by tracing registrants across countries and employers after random lottery selection. The sponsor-family result shows that some foreign-position responses occur within the sponsor's corporate family.

The remainder of the paper is organized as follows. Section 2 describes the H-1B registration and petition process. Section 3 explains the data and match construction. Section 4 presents the empirical design. Section 5 reports summary statistics and covariate balance. Section 6 presents the main results. Section 7 concludes.

2. Institutional background

The H-1B program allows US employers to sponsor foreign workers for temporary employment in specialty occupations. These positions generally require at least a bachelor's degree or its equivalent. H-1B work authorization is tied to the employer and job described in the employer's petition. Our setting concerns new H-1B employment subject to the annual cap. It does not include extensions for existing H-1B workers or employment at universities and research organizations that are exempt from the cap.[1]

2.1 The annual cap and lottery

Congress generally limits new H-1B workers subject to the cap to 65,000 each fiscal year. Up to 20,000 additional places are available to workers with a master's degree or higher from a US institution. Demand exceeded the available places in every year of our sample, so USCIS used a lottery to determine which employers could file petitions.

Our sample covers the first four years of the electronic registration system, FY2021 through FY2024. During March, an employer submitted a short electronic registration for each worker it intended to sponsor. The registration identified the employer and worker but did not include the full petition. A given employer could submit only one registration

[1] See USCIS, H-1B Specialty Occupations.

for a worker, but different employers could each register the same worker during our sample period.[2]

USCIS conducted the initial lottery after the March registration period closed. It first selected registrations toward the regular cap. Registrations for workers with an eligible US advanced degree that were not selected received another chance through the advanced-degree exemption. Selection was random within each draw. Selection rates differed across lottery years because the number of registrations changed.

USCIS sometimes conducted additional selections later in the year. These rounds occurred when the petitions filed after the initial draw were not expected to fill the cap. A registration that remained unselected could therefore receive a later selection notice, but it could not proceed to the petition stage unless USCIS selected it.

2.2 From selection to H-1B employment

Figure 1 summarizes the calendar for the initial selection round. The lottery occurs in March of the calendar year before the fiscal year named in the registration. For example, USCIS conducted the initial FY2024 lottery in March 2023.

Selection allows the employer to file a full Form I-129 petition. It does not provide work authorization. The selection notice gives the employer a filing window of at least 90 days. For the initial FY2024 selection, this window ran from April 1 through June 30, 2023.[3]

A full petition contains information that USCIS does not review at registration. The employer must include a Labor Condition Application certified by the Department of Labor. This document identifies the intended worksite and contains the employer's wage commitments.[4] The petition must also establish that the position qualifies as a specialty occupation and that the worker has the required qualifications.

USCIS may approve the petition, deny it, or request more evidence. The employer may also decide not to file after receiving a selection notice. Throughout the paper, we use an approved I-129 petition as our measure of receiving a visa. This distinction explains why lottery selection does not guarantee visa receipt.

The earliest start date for employment under an initial H-1B petition subject to the cap is generally October 1. The FY2024 registration and lottery occurred in March 2023, but an approved worker generally could not begin H-1B employment until October 1, 2023. We date outcomes from March of the draw year because workers and employers can begin responding when the lottery result becomes known.

[2] USCIS introduced electronic registration for the FY2021 lottery. See USCIS, Characteristics of H-1B Specialty Occupation Workers, Fiscal Year 2020.

[3] See USCIS, Fiscal Year 2024: H-1B Petitions.

[4] See Department of Labor, H-1B Labor Condition Application.

2.3 Worker choices after the lottery

The effect of the lottery depends partly on the worker's status at the time of the draw. Many prospective H-1B workers are recent US graduates working through Optional Practical Training (OPT). OPT generally provides 12 months of work authorization. Graduates in eligible STEM fields may receive a further 24-month extension.[5]

A selected worker may continue working under OPT while the employer files the H-1B petition. For eligible F-1 students, a timely filed petition can also extend status and work authorization until the October 1 H-1B start date. An unselected worker can continue working only while some other authorization remains valid. The worker's remaining time under OPT therefore affects how quickly the lottery result changes employment.

An approved petition provides a longer path to US employment. Initial H-1B status generally lasts up to three years and may be extended. Once a worker begins H-1B employment, changing employers does not require entering the lottery again. The new employer files Form I-129, and an eligible worker can generally begin the new job as soon as the petition is properly filed with USCIS.[6]

An unselected registration closes the usual H-1B route for that fiscal year unless USCIS conducts another selection. The employer may register the worker again the following year. The worker may also pursue employment that is exempt from the cap or seek another type of work authorization.

Education provides another possible route to remaining in the United States. A worker can begin another degree and return to student status. A new degree may also create another period of eligibility for practical training.

Workers without a US option must decide where to work. A multinational sponsor can retain the worker at one of its foreign worksites. The worker may instead return home or move to another country, such as Canada. A later selected registration and approved petition can allow the employer to bring the worker back to the United States.

The outcomes in the paper follow these choices. Employment and geographic outcomes measure where workers go after the draw. Education outcomes measure whether they begin another degree. Employer attachment measures whether the relationship with the sponsor survives. Salary and job qualifications capture changes in the worker's US career. Patent filings measure whether the disruption also changes innovative activity.

The next section describes how we link H-1B registrations to career histories and observe these responses after the lottery.

[5] See USCIS Policy Manual, Practical Training.

[6] This process is commonly called an H-1B transfer, although the new employer files a new petition. See USCIS, How Do I Hire a Foreign National for Short-Term Employment in the United States?

3. Data

3.1 H-1B registration and Revelio data

We obtain H-1B registration records for fiscal years 2021 through 2024 through a Freedom of Information Act request to USCIS. The data contain 1,804,286 eligible registrations, exactly matching USCIS's published eligible-registration totals for these four cohorts. USCIS selected 572,191 registrations, or 31.7%. Appendix A documents these counts and the fields in the registration data.

The registration records include country of birth, country of nationality, birth year, gender, sponsoring employer, and lottery outcome. For selected registrations, the data also report information from the employer's I-129 petition and the final petition decision. We do not use these petition fields to construct the match because they are unavailable for unselected registrations.

We combine the registration records with Revelio data drawn from LinkedIn profiles. These data provide employment and education histories, job locations, and modeled salaries. The histories allow us to observe workers before and after the lottery draw.

3.2 Matching registrations to profiles

Figure 2 summarizes the matching process. We first match sponsoring employers to Revelio companies using employer identification numbers and company names. We match 65,024 of 98,456 unique sponsoring employer identification numbers. These employers account for 1,330,049 registrations, or 73.7% of the registration data. Employer identification numbers or exact company-name matches produce more than 92% of the employer matches. For fuzzy matches with state information in both sources, we discard candidates whose states disagree. Appendix A.2 provides the full employer-matching procedure.

We next search for a worker profile within each sponsoring employer and draw year. A candidate must report a US position at the sponsoring employer during the draw year. The profile-side country proxy and gender must agree with the registration. Finally, the birth year inferred from the profile must be within two years of the registration birth year.

We construct the profile-side country proxy from a foreign bachelor's degree for 69% of the matches. Another foreign degree identifies 12%, and sustained foreign work experience identifies the remaining 19%. We infer birth year from education dates and classify gender from first names. Appendix A.3 describes these measures and their timing.

We retain a registration only when exactly one profile satisfies these candidate criteria. This rule leaves 61,839 matched registrations. They represent 47,380 workers sponsored by 24,127 employers. Appendix A.4 reports the registrations removed at each step of the worker match.

The strict matching rules reduce the matched share. Figure 2 maps out the composition of 439,317 registrations with the information needed for a unique match. We match 14.1% of this set to the H-1B registration data. This percentage is the match rate under the current rules, not an estimate of Revelio coverage.

The matched and matchable samples are similar on the demographics reported in Appendix Table A1. The male share differs by 0.9 percentage points, the Indian share by 1.2 percentage points, and the Chinese share by 0.1 percentage points. The average birth year differs by approximately one year. Match rates are lower at larger sponsors because more registrants share the same observable characteristics within those employers, as Appendix Table A2 shows.

The matched sample contains a modestly larger share of selected registrations than the matchable sample: 37.0% compared with 34.6%. The difference ranges from 2.5 to 4 percentage points within individual draw years. Appendix Table A1 reports the overall difference, and Appendix A.4 reports the within-year range. We therefore interpret the estimates as effects for registrants who can be matched to professional profiles. Section 5.2 examines whether selected and unselected registrations remain comparable within this sample.

3.3 Held-out profile–petition correspondence

We compare the matched profiles with I-129 petition fields that played no role in constructing the match. The comparison covers 17,712 unique workers with approved petitions. These are descriptive correspondence checks, not independent validation of every registration–profile link. Appendix A.5 defines each measure and its denominator.

The profile reports the petition degree or a higher degree for 89.0% of workers with usable degree information. The petition worksite state appears in at least one of five profile-side location sources for 84.8%. The job-title criterion is met for 65.3% of workers with raw title data, and the field-of-study criterion is met for 44.7%. Overall, 96.0% meet at least one criterion.

Profile–petition correspondence is also stronger when the two birth-year measures are closer. The job-title rate is 75.2% for workers with identical birth years, 66.1% with a one-year difference, and 50.7% with a two-year difference. The other three measures follow the same pattern. Because education dates help construct the inferred birth year, this gradient is not an independent validation test. Main results are also stable under alternative birth-year match specifications, though results are strongest when the birth-year match is tighter. Appendix Table A6 separates exact birth-year matches from matches with one- and two-year differences.

3.4 Outcome variables

We construct outcomes from the employment and education histories beginning in March of the draw year. Employment location comes from the country reported for the

position. For the telemigration outcomes and event studies, we do not count a foreign listing as located abroad when a concurrent US position covers at least 80% of its recorded year span. Appendix A.6 reports this adjustment.

The employment outcomes record whether a worker starts any new job and where that job is located. We distinguish positions in the United States, the worker's country of birth, Canada, another foreign country, and a foreign worksite of the sponsoring employer.

The education outcomes record whether a worker starts a new degree. We separately identify enrollment in the United States, the worker's country of birth, Canada, and another foreign country. The geographic outcomes combine employment and education records to describe where the worker moves after the lottery. The remaining career outcomes include an indicator for whether the worker founds a company and worker salary information from Revelio.

All indicator outcomes are multiplied by 100, so the regression coefficients are in percentage points. Appendix B provides the complete variable definitions.

3.5 Auxiliary data

Two auxiliary data sets support later tests. We construct comparison workers who did not register for the lottery but held similar roles at the same companies as matched registrants. Subsection 6.4 describes this sample.

We also measure innovation by linking registrant-company pairs to inventors and assignees in USPTO patent data. Subsection 6.5 describes the patent match and its validation.

4. Empirical design

We use the H-1B lottery to estimate the causal effect of receiving a visa on post-lottery outcomes. USCIS selects registrations at random. It conducts separate draws for the regular cap and the advanced-degree cap, but selection within each pool remains random. Selection allows the registrant's employer to file an I-129 petition. It does not guarantee that USCIS will approve the petition. In our matched sample, 79.8% of selected registrations result in an approved petition.

Approval at the I-129 stage is not random. USCIS assesses whether the position qualifies as a specialty occupation and whether the registrant meets the relevant requirements. A direct comparison of registrants with approved and unapproved petitions would therefore combine the effect of receiving a visa with differences in their positions and qualifications. We address this problem using two-stage least squares (2SLS):

$$Y_i = \alpha + \beta Granted\ Visa_i + \Gamma X_i + \theta_{y(i)} + \eta_{c(i)} + \epsilon_i, \tag{1}$$

where i indexes registrations and Y_i is a post-lottery outcome. $Granted\ Visa_i$ indicates an approved I-129 petition, which we use as our measure of receiving a visa. We instrument $Granted\ Visa_i$ with $Lottery\ Selected_i$. The coefficient β is the average effect of receiving a visa for registrants whose petition approval changes when their registration is selected (Angrist, Imbens, and Rubin (1996), Imbens and Angrist (1994)).

The control vector X_i contains characteristics measured before the lottery. It includes average Revelio-modeled, pre-lottery salary and age at the draw. We control for the natural log of registrant salary, gender, an indicator for an advanced (master's or doctoral) degree, as well as a separate indicator for whether the degree is in STEM. Lottery-year fixed effects absorb differences in selection rates and economic conditions across lottery cohorts. We cluster standard errors at the sponsoring-employer level. We include country-of-birth fixed effects to absorb any bias introduced from using this field in the registration matching process by comparing selected and unselected registrants born in the same country.

Standard 2SLS assumptions require that lottery selection affects post-lottery outcomes only through receiving a visa. Selection alone does not provide work authorization, which supports this assumption. Still, selection could change the plans of a registrant whose petition is later denied. Though we cannot measure this response separately, it would affect estimates only through the 20.2% of selected registrants in our sample who were not granted a visa. A false positive would require massive employment and career trends for this group, well above sample means. We view a direct effect of this magnitude as unlikely.

An alternative is to estimate the effect of lottery selection itself, regardless of whether USCIS approves the petition. Doran, Gelber, and Isen (2022) make this intent-to-treat effect their main specification to investigate what happens to firm outcomes when it receives another H-1B lottery win. In contrast, our question is how receiving a visa changes a worker's outcomes. Appendix Table A4 reports the corresponding intent-to-treat estimates. These estimates are about 20% smaller than the 2SLS estimates and yield the same qualitative conclusions.

Each table reports the Kleibergen-Paap first-stage F statistic and the sample mean of the outcome. The baseline first-stage F statistic is 57,686.8. The appendix reports estimates that use lottery selection directly, impose stricter birth-year matching rules, and add sponsoring-employer fixed effects.

5. Descriptive statistics and covariate balance

The summary statistics provide two benchmarks for the results. Registrant characteristics show which workers our estimates describe. Outcome means show whether the estimated effects are large or small relative to how frequently each outcome

occurs. We then examine whether selected and unselected registrations remain comparable after our matching process.

5.1 Summary statistics

Table 1 describes the matched analysis sample. The sample contains 61,839 registrations corresponding to 47,380 workers. The unit of observation is a registration, so the same worker can appear in more than one lottery cohort.

The average registrant is 31 years old, and 59.5% are men. Most have an advanced degree. Master's degrees account for 58.2% of the sample, and 8.2% have a doctorate. About 62% studied a STEM field.

Registrants born in India account for 48.4% of the sample. Registrants born in China account for another 12.5% (Appendix Table A1). This concentration is one reason that the main regressions compare selected and unselected registrants born in the same country.

The mean salary before the lottery is $60,879, and the median is $56,457. The sample is therefore highly educated and concentrated in STEM fields. Some of these workers can continue working through Optional Practical Training, enter another H-1B lottery, or enroll in another degree program after an unsuccessful draw. These alternatives make it less obvious that receiving a visa from the initial registration will affect outcomes measured several years later.

Lottery selection does not always result in a visa. USCIS selected 37.0% of the registrations in the sample, while 29.5% resulted in an approved petition. Thus, 79.8% of selected registrations resulted in a visa. This difference motivates the 2SLS estimator described in Section 4.

New job starts are common during the post-lottery observation period. About 74.5% of registrants start a new job somewhere, and 67.8% start a US job. About 10.2% start a job in their home country. Another 6.0% start a job in a different foreign country, including 1.3% who start one in Canada.

These location outcomes accumulate over the three-to-six-year observation period. A registrant can therefore enter several categories by starting jobs in different countries at different times. The indicators do not imply that the jobs are held concurrently.

The high rate of new job starts is important for interpreting the results. Our employment estimates measure whether receiving a visa changes where registrants eventually start jobs. They do not measure whether the registrant is employed in a particular country immediately after the lottery.

The remaining outcomes are less common. New educational enrollment occurs for 5.9% of the sample. About 1.5% found a company, and 1.0% have a post-lottery patent application.

Salary data in Revelio are available for 48,178 registrations. Among these registrations, the mean salary after the lottery is $116,423 and the median is $109,433.

Because these outcome frequencies differ substantially, the same percentage-point effect can have very different economic importance across outcomes. The results section therefore reports each estimate in percentage points and compares it with the corresponding sample mean.

5.2 Covariate balance

USCIS selects registrations before we match them to LinkedIn profiles. Our matching process could nevertheless change the composition of the selected and unselected groups. Figure 3 tests whether the two groups remain similar on characteristics measured before the lottery.

Selection rates differ across lottery years and between the regular and advanced-degree draws. We account for these differences in two steps. First, we estimate each registration's probability of selection using its lottery year, country of birth, and degree level. We then give more weight to selected registrations from groups with lower selection rates and to unselected registrations from groups with higher selection rates. This weighting makes the selected and unselected groups reflect a more similar mix of lottery years, countries, and degree levels.

We then compare the groups on each pre-lottery characteristic. The comparisons continue to account for lottery year, country of birth, and advanced-degree status. We standardize each characteristic so that the estimated differences are measured in standard deviations. Figure 3 reports these differences and their 95% confidence intervals.

Selected and unselected registrants are similar on most observed characteristics. Nearly every estimated difference is smaller than 0.05 standard deviations. The main exception is age. Selected registrants are about 0.12 standard deviations, or nine months, younger on average. Selected registrants are also about 1.4 percentage points less likely to be men based on the standardized difference and standard deviation reported in Table 1. These differences are similar in magnitude to the pre-match universe of H-1B registrants.

Differences in salary before the lottery and in several job-title categories are smaller than 0.05 standard deviations. STEM education, most fields of study, and patenting before the lottery show little difference.

The main regressions control for age, gender, salary before the lottery, degree level, and STEM education. They also include country-of-birth and lottery-year fixed effects. Appendix Table A5 shows how the estimates change when we compare selected and unselected registrants sponsored by the same employer. Appendix Table A6 reports the estimates separately for exact, one-year, and two-year birth-year matches.

In addition to comparing selected and unselected registrants, we examine whether the matched sample resembles the larger set of registrations that were eligible for matching. This comparison helps us assess how broadly the estimates that follow may apply.

The observable composition changes little after matching. The male share is 59.5% in the matched sample and 60.4% in the matchable sample. The corresponding shares for registrants born in India are 48.4% and 49.6%. The shares born in China are 12.5% and 12.6%, and the average birth year differs by approximately one year (Appendix Table A1).

The matched sample is somewhat more likely to contain selected and approved registrations. Its selected share is 2.4 percentage points higher than in the matchable sample. Conditional on selection, its approval rate is 2.8 percentage points higher.

The sample covers 24,127 sponsoring employers and includes employers of every size. Matching rates are higher at smaller employers because fewer candidate profiles and registrations share the same matching characteristics. Appendix Table A2 documents this pattern and shows that many large employers also contribute observations.

Registrants from countries with fewer H-1B entries and those at smaller employers are more likely to enter our sample. Their matching characteristics are less likely to identify several possible profiles. The estimates may therefore describe these registrants more closely than registrants from the largest source countries and sponsors. Even so, the matched and matchable samples are similar on observable demographics, and the matched sample includes registrants from all major source countries and employers of every size (Appendix Tables A1 and A2).

6. Results

Receiving a visa can shape both a registrant's subsequent career and the sponsoring employer's response to the lottery. We first examine where registrants work, whether they remain with the sponsor, whether they return to school, and how their earnings change. We then study employers' responses and the quality of the jobs that registrants ultimately obtain.

6.1 Main results

6.1.1. Job starts and their location

Table 2 examines whether a registrant ever starts a new job after the lottery. We observe position starts from March of the draw year through February 2026. The observation period therefore ranges from almost three years for the FY2024 cohort to almost six years for the FY2021 cohort.

This long period makes the effect on US job starts uncertain. Registrants who do not receive a visa from their initial registration have time to continue working under Optional Practical Training, enter a later H-1B lottery, or pursue another route to US employment. Section 2 describes these alternatives. Any US job obtained through one of these routes counts toward our outcome.

Column (1) provides no evidence that receiving a visa changes whether a registrant ever starts a new job somewhere. The estimated effect is -0.295 percentage points, relative

to a sample mean of 74.6%, and is not statistically distinguishable from zero. In the baseline specification, receiving a visa changes the location of subsequent job starts rather than whether a registrant starts another job.

Receiving a visa nevertheless raises the probability of ever starting a US job. The estimate in column (2) is 2.397 percentage points, or 3.5% of the 67.8% sample mean. This effect is not mechanical because jobs obtained through later immigration pathways also count toward the outcome. The positive estimate indicates that registrants who do not receive a visa initially do not fully close the gap, even after several years. In the raw data, 65.7% of unselected registrants start a US job during our three-to-six-year observation period. Receiving a visa raises this probability by only 2.397 percentage points.

The corresponding estimates for jobs outside the United States are negative. Receiving a visa reduces the probability of starting a job in the registrant's home country by 2.704 percentage points, or 26.8% of the sample mean. It also reduces the probability of starting a job in another foreign country by 1.603 percentage points, or 26.7% of the sample mean.

Column (5) reports Canada separately. Receiving a visa reduces the probability of starting a Canadian job by 0.677 percentage points, or 51.3% of the 1.3% sample mean. Canada is included in the broader foreign-country outcome in column (4), so these estimates should not be added together.

Receiving a visa also reduces the probability of starting a job at the sponsoring employer's foreign worksite. The estimate in column (6) is -1.021 percentage points, or 53.4% of the sample mean. This result is consistent with some sponsoring employers retaining initially unsuccessful registrants outside the United States while pursuing another route to bring them to the United States. The estimate does not, by itself, establish why the employer places the worker abroad or whether the same position moved overseas.

The location outcomes are cumulative and are not mutually exclusive. A registrant can start jobs in more than one country during the observation period. We therefore interpret each coefficient as a separate change in the probability of starting a job in that location, rather than as an accounting decomposition of where workers go.

The intent-to-treat estimates have the same signs and are about 20% smaller than the 2SLS estimates (Appendix Table A4). The US and home-country estimates also retain their signs across the alternative matching rules and the specification with sponsoring-employer fixed effects (Appendix Tables A5 and A6). The US estimate slightly loses precision only under the two-year birth-year match tolerance.

The other conclusions are less stable. The estimate for starting any new job becomes positive and significant with sponsoring-employer fixed effects, and some of the foreign-location estimates attenuate under the alternative specifications. We therefore place the most weight on the US and home-country results.

These results show that receiving a visa from the initial registration affects *where*

registrants start jobs even after those who do not receive one have had several years to adjust. It does not clearly change whether they start a new job unconditionally. We next examine whether receiving a visa also changes how long registrants remain with the sponsoring employer.

6.1.2. Employer attachment

The job-location results suggest that an unsuccessful lottery outcome can change where a registrant works without necessarily ending the relationship with the sponsoring employer. We examine that relationship directly by asking whether selected and unselected registrants remain with the sponsor in the years after the draw.

The expected effect is not obvious. An unselected registrant may stay with the sponsor to enter a later lottery or work at one of its foreign locations. A visa recipient also has a reason to stay because H-1B status is initially tied to the sponsoring employer. They may transfer H-1B status to a new employer later on, but must first be hired and sponsored there. Thus, both lottery outcomes create reasons to remain with the initial sponsor.

We count a registrant as attached in a calendar year if a Revelio position at the sponsoring employer, or at its ultimate corporate parent, overlaps that year. Our matching process requires a position at the sponsor in the draw year. Attachment in that year is therefore part of sample construction rather than an outcome. We begin interpreting the estimates in the first calendar year after the draw.

Figure 4 reports the intent-to-treat effect of lottery selection. We use this specification because the registrant and sponsor can respond when selection is announced, before USCIS decides whether to approve the petition. Selection raises the probability of remaining attached by 3.9 percentage points in the first calendar year after the draw. This is a 4.9% increase relative to the 79.4% attachment rate among unselected registrants.

The difference narrows to 2.3 percentage points in the second year and 2.2 percentage points in the third year. It is 1.2 percentage points in the fourth year ($p = 0.058$). The later estimates use fewer lottery cohorts because those cohorts must have enough post-lottery years in the data. The corresponding 2SLS estimates of the effect of receiving a visa range from 4.9 percentage points in the first year to 1.5 percentage points in the fourth year.

The attachment results show that an unsuccessful lottery outcome does not usually end the sponsor-worker relationship immediately. We next examine another possible adjustment: returning to school.

6.1.3. Education

Table 3 examines whether receiving a visa changes the probability that a registrant starts another educational program during the same three-to-six-year period used for the employment outcomes. Column (1) considers any educational program. Columns (2) and

(3) examine programs in the United States and the registrant's home country. Columns (4) and (5) examine programs in other foreign countries and Canada.

The estimates use lottery selection to isolate the effect of receiving a visa. The regressions compare registrants from the same country of birth and lottery year. They also account for age and gender, along with education and salary before the lottery.

The main result is that visa recipients are less likely to start another educational program, primarily because they are less likely to study at a US institution. In column (1), a visa recipient is 2.605 percentage points less likely than a comparable registrant who does not receive one to start any educational program. This is a 45.3% reduction relative to the 5.7% sample mean. Column (2) shows that a visa recipient is 2.154 percentage points less likely to start a program at a US institution. This is a 49.3% reduction relative to the 4.4% sample mean.

The US result is consistent with some registrants using education as another way to remain in the United States after an unsuccessful lottery outcome. Section 2 explains how another degree may allow a worker to return to student status and later become eligible for another period of practical training. Our data do not report a registrant's immigration status or reason for enrolling. Because the outcome covers as many as six years, some programs may also reflect later investments in skills rather than an immediate response to the lottery.

Column (3) shows no corresponding change in education in the registrant's home country. The estimated difference is -0.060 percentage points and is not statistically distinguishable from zero. The additional education among registrants who do not receive a visa therefore does not appear to come from returning home to study.

Columns (4) and (5) show smaller differences at institutions in other foreign countries. A visa recipient is 0.342 percentage points less likely to start a program outside both the United States and the registrant's home country, a 31.1% decrease relative to a mean of 1.1%. The estimated difference for Canada is 0.134 percentage points, larger in magnitude than the mean registrant enrollment rate of 0.1% in Canadian institutions itself. Canada is included in the broader outcome in column (4), so the two estimates should not be viewed as independent.

The any-program and US-program estimates remain negative and statistically significant in robustness checks. The corresponding intent-to-treat estimates are -2.066 and -1.708 percentage points (Appendix Table A4). The US and any-education estimates decline slightly but remain significant when we include sponsoring-employer fixed effects (Appendix Table A5). Point estimates also remain negative in each birth-year match category (Appendix Table A6). The estimates for programs in other foreign countries are less stable, becoming no longer statistically distinguishable from zero with sponsoring-employer fixed effects or among matches with a two-year birth-year difference.

Taken together, the estimates suggest that receiving a visa reduces the probability of starting another educational program, primarily at a US institution.

6.1.4. Salary and founder roles

Visa receipt may shape subsequent careers through the salary associated with new positions and the decision to work as a founder. Table 4 examines both margins. Columns (1) and (2) report whether salary data are available. Columns (3) and (4) report modeled salary conditional on having such data. Columns (5) and (6) report whether the registrant starts a founder role.

The common 24-month window holds the length of follow-up fixed across cohorts. Odd-numbered columns use this window, while even-numbered columns use all available years through February 2026. For example, the all-years window follows the FY2021 cohort for 66 months after its final selection round but follows the FY2024 cohort for only 31 months. Earlier cohorts therefore have more than twice as much time to report a new position or founder role. The common window gives every cohort exactly 24 months and ends at least seven months before the March 2026 data cutoff.

Receiving a visa does not appear to change whether salary data are available. The estimates in columns (1) and (2) are 0.722 and 0.616 percentage points. These equal 1.2% and 0.9% of the corresponding sample means of 58.4% and 68.7%. Neither estimate is statistically distinguishable from zero. The similar estimates reduce concern that differential coverage alone accounts for the salary results. The salary regressions nevertheless remain conditional on having salary data for a newly reported position.

Within that conditional sample, the estimates suggest that visa recipients have higher modeled salaries in both windows. In column (3), the coefficient of 0.0649 implies that a visa recipient's modeled salary is 6.7% higher than that of a comparable registrant who does not receive one ($100 \times (e^{0.0649} - 1) = 6.7\%$). Applied to the dollar-level mean modeled annual salary found in column (3) of \$115,315, this corresponds to approximately \$7,731 per year. Column (4) reports a similar difference of 5.7% over all available years. If the annual difference of \$7,731 persisted for ten years, its present value at a 5% discount rate would be approximately \$59,700.

Two features limit the interpretation of this result. The estimate conditions on observing a newly reported position with salary data, so it describes pay in those positions rather than unconditional earnings. Revelio's salary model also uses job location, so part of the difference may reflect the geographic shift documented above rather than higher pay within a location. We therefore read the salary result as consistent with the geographic findings rather than as an independent estimate of the return to a visa.

The founder estimates suggest that visa recipients are less likely to start working as founders in both windows. The estimated difference is 0.329 percentage points in column

(5) and 0.423 percentage points in column (6). These estimates equal 27.9% and 20.2% of the corresponding sample means of 1.2% and 2.1%. We construct the outcome from raw position titles containing "founder" or "co-founder." Advisory roles and member roles do not qualify. We also exclude founder's-office and generic chief-officer titles.

The appendix checks broadly reinforce the baseline salary and founder findings. These checks use the common 24-month window. In the intent-to-treat specification, lottery selection raises conditional log modeled salary by 0.052 and reduces the probability of starting a founder role by 0.261 percentage points. Both estimates are statistically significant at the 1% level (Appendix Table A4). With sponsoring-employer fixed effects, the conditional salary difference remains positive at 2.9% and statistically distinguishable from zero. The founder estimate also remains negative at −0.201 percentage points, although it is estimated less precisely (Appendix Table A5).

The signs remain unchanged across the three birth-year match categories: every conditional salary estimate is positive, and every founder estimate is negative. The salary estimate is statistically significant for exact and one-year matches, while the founder estimate is statistically significant for exact and two-year matches (Appendix Table A6). Salary-data availability remains statistically unchanged in the intent-to-treat specification and every birth-year category, although it rises by 1.499 percentage points with sponsoring-employer fixed effects. These checks as a whole are consistent with higher conditional modeled salary and fewer founder roles following visa receipt across a range of specifications, even though not every individual estimate is precise.

6.1.5. Post-lottery geography

The employment and education results leave an important ambiguity. A registrant who does not receive a visa is more likely to report a job abroad, yet is also more likely to start a program at a US institution. Does the education response offset the employment response, or does a geographic difference remain once both are counted?

Appendix Table A3 combines reported position and education starts observed from March of the draw year through February 2026. Columns (1)–(4) examine the United States, the registrant's country of birth, another non-US country, and Canada. The outcomes are cumulative and can overlap. Canada enters the country-of-birth outcome for registrants born there and the other-non-US outcome for other registrants, so column (4) should not be added to either column.

Under the current construction, column (1) also sets the US indicator to one when the profile contains no new post-lottery work or education start and the most recent pre-lottery position is not coded as ending in or after the draw year.

The geographic difference remains after work and education are combined. A visa recipient is 2.864 percentage points more likely than a comparable registrant who does not receive one to meet the US outcome definition. This is a 3.3% increase relative to

the 87.1% sample mean. A visa recipient is 2.716 percentage points less likely to report a new job or degree in the country of birth, 1.834 percentage points less likely to report one in another non-US country, and 0.696 percentage points less likely to report one in Canada. These reductions equal 26.5%, 27.1%, and 51.0% of their respective sample means. Among the foreign outcomes, the country of birth has the largest difference in percentage points. Canada's larger proportional difference reflects its small 1.4% mean.

Together with the near-zero estimate for starting any new job, these results point to a change in where post-lottery activity is reported rather than whether a new job is reported. The geographic evidence nevertheless leaves the paper's central question unresolved: what becomes of the sponsored job in particular? The next subsection turns from where registrants report activity to whether the firm-worker match survives.

6.2 The firm-worker match survives abroad

A new position abroad does not by itself reveal what happens to the sponsored match. The registrant may have found a different employer. Table 5 separates these possibilities using positions that begin from March of the draw year through February 2026. Column (1) reports any new foreign position at a US-headquartered company, a margin we call telemigration. Column (2) reports a foreign position at the matched sponsor or one of its subsidiaries. Column (3) reports positions at other US-headquartered employers. Columns (4) and (5) ask whether a foreign position at a US-headquartered company is in the registrant's country of birth or in Canada. Column (6) reports foreign positions at any employer. These registration-level outcomes can overlap and should not be added together with the intent to construct the effect of visa receipt on joint outcomes. As above, we instrument visa receipt with lottery selection.

Compared with a visa recipient, a comparable registrant who does not receive one is 1.670 percentage points more likely to start a foreign position at a US-headquartered company. This is a 44.2% increase relative to the 3.8% sample mean.

Column (2) moves closer to the sponsored match. A comparable registrant who does not receive a visa is 1.070 percentage points more likely to report a later foreign position at the matched sponsor or one of its subsidiaries. This is a 62.2% increase relative to the 1.7% mean. By definition, each registrant is indexed by a sponsoring firm-draw year pair. The dependent variable in this column follows the same worker from that relationship to a later position abroad within the sponsor's corporate family. Column (3) shows a separate 1.029-percentage-point increase at other US-headquartered employers, or 36.7% of its 2.8% mean.

Among foreign positions at US-headquartered companies, the registrant's country of birth is the largest named destination. A comparable registrant who does not receive a visa is 0.908 percentage points more likely to start such a position there, a 35.4% increase relative to the mean. The corresponding estimate for Canada is 0.375 percentage points,

or 87.2% of its smaller 0.4% mean.

Column (6) provides a broader benchmark. A comparable registrant who does not receive a visa is 3.759 percentage points more likely to start a foreign position at any employer, relative to a 9.9% mean. Comparing the sample means of telemigration (3.78%) to any foreign employer (9.95%), we note that positions at US-headquartered companies represent roughly 38% of the sample prevalence of foreign employment. Yet, the corresponding point estimates for the effect of receiving an H-1B visa on foreign employment (3.759 and 1.670, respectively) would indicate that 44% of the effect is concentrated among US firms. The foreign hiring response of unselected lottery registrants is therefore disproportionately concentrated within the US corporate sector.

Table 5 accumulates new position starts over the available follow-up. Figure 5 instead traces the annual probability of holding a foreign position at a US-headquartered company around each worker's first observed draw. We construct a panel dataset at the registrant-year level. The dependent variable is the first differenced telemigration indicator, which absorbs the registrant fixed effect. We regress this dependent variable on the interaction between indicators for event time (relative to the registrant's draw-year) with an indicator for going unselected in the lottery, and plot these coefficients (along with 95% confidence intervals). We include lottery cohort–year fixed effects. The two pre-draw event–year estimates are individually indistinguishable from zero. After the draw, the unselected-minus-selected difference widens from 0.12 percentage points in the draw year to 0.42 after one year, 0.84 after two years, and 1.08 after three years. The estimates become statistically distinguishable from zero in years two and three.

Figure 6 examines two margins behind this growing foreign-employment difference. Panel A traces foreign positions within the sponsor's corporate family. The unselected-minus-selected difference reaches 0.57 percentage points by year three. Panel B traces positions in Canada at any employer. This difference emerges later and reaches 0.60 percentage points by year three. These event studies do not count a foreign spell when a concurrent US position covers at least 80% of its recorded year span, reducing the risk that an overlapping affiliation is mistaken for employment abroad (Appendix A.6).

The direct intent-to-treat estimates have the same signs as their two-stage least squares counterparts across all six outcomes. Taken together, the cumulative and annual results show that failure to receive a visa need not dissolve the sponsor-worker relationship. The same worker becomes more likely to report a position abroad within the sponsor's corporate family, while the broader foreign-employment response is disproportionately concentrated at US-headquartered companies. This worker-level pattern complements evidence that multinationals respond to H-1B restrictions by shifting skilled employment to foreign affiliates (Glennon (2024a)). It is consistent with a firm response, although workers' foreign opportunities may also shape where they go. The next subsection tests that possibility.

6.3 Does the geographic response vary with home-country income?

The sponsor-worker results show that some matches survive abroad. They do not tell us whether workers with stronger opportunities at home are driving that response. Registrants from richer countries may be more willing to leave the US when they do not receive a visa because they have more attractive opportunities at home. If that account is important, the geographic response should be larger among registrants from higher-income countries.

We test this prediction using the natural log of 2019 GDP per capita in the registrant's country of birth, from the World Bank. We use 2019 to avoid pandemic-era changes and standardize the measure to have mean zero and standard deviation one. We instrument $Granted\ Visa$ with lottery selection and $Granted\ Visa \times GDP$ with lottery selection interacted with GDP. The regressions include country-of-birth and lottery-year fixed effects. They also control for log modeled pre-lottery salary, age, gender, an advanced degree, and a STEM major. Country-of-birth fixed effects absorb the direct relationship between GDP and the outcomes.

Table 6 reports the results for the same four geographic outcomes used in Appendix Table A3. The US outcome equals one if the profile reports a post-lottery US job or degree or, when no new job or degree is reported, shows no recorded departure from the US. The other outcomes capture reported post-lottery jobs or degrees in the country of birth, another non-US country, and Canada. These outcomes can overlap.

At the sample mean of log GDP per capita, the coefficient on $Granted\ Visa$ implies that a visa recipient is 2.844 percentage points more likely than a comparable registrant who does not receive one to satisfy the US measure. A visa recipient is 2.717 percentage points less likely to report a post-lottery job or degree in the country of birth, 1.922 percentage points less likely to report one in another non-US country, and 0.719 percentage points less likely to report one in Canada.

The interaction estimates are small. A one-standard-deviation increase in log GDP per capita changes the estimated effect of $Granted\ Visa$ by 0.187 percentage points for the US outcome, 0.118 for the country of birth, -0.190 for another non-US country, and 0.047 for Canada. None is statistically distinguishable from zero. Every 95% confidence interval lies between -0.74 and 0.89 percentage points. The estimates therefore rule out large changes in the geographic response as home-country income rises.

The lack of a statistically significant interaction with home-country income weakens a simple explanation in which the response is driven mainly by workers from richer countries choosing attractive (plausibly US-headquartered) opportunities abroad. Together with the direct evidence that some matches remain within the sponsor's corporate family, this result supports the view that firms play an important role in moving these jobs across borders.

This adjustment margin matters for current policy. A Department of Homeland

Security rule effective for the FY2027 cap season gives registrations at higher occupational wage levels more chances of selection, and the Department of Labor has proposed raising the prevailing-wage levels used in the H-1B program.[7] These changes postdate our sample, so we do not estimate their effects. Our findings point to an additional outcome that evaluations should measure. When a match does not receive a visa, the sponsor may keep the worker abroad, so fewer H-1B entries need not translate one-for-one into more US hiring.

The geographic evidence establishes both where registrants go and that some sponsor-worker matches survive abroad. We next turn to registrants who remain in the US and ask whether the jobs they take use their qualifications.

6.4 Do unselected registrants take US jobs below their qualifications?

We begin with salary, the most direct measure of whether the next job rewards a worker's qualifications. Table 7 follows each unselected registrant from the job named in the H-1B registration to her first later US job at a different employer. The later job must begin after both the lottery draw and the sponsored job. At each job, the comparison group consists of workers (in the Revelio data, but not in the H-1B sample) who began the same standardized role at the same employer during the preceding 36 months. We retain the earliest eligible unselected draw for each worker and require at least five peers at both jobs.

We restrict the later job to one beginning before the second anniversary of the draw in Panel A. This fixed window gives every lottery cohort the same two years of follow-up. We then repeat the analysis, allowing the first eligible later job to occur at any point through February 2026 in Panel B. An FY2021 registrant can therefore contribute a move almost six years after the draw, whereas an FY2024 registrant can contribute one for almost three years. The fixed-window and unrestricted estimates describe different samples, not successive points on the same workers' paths. In both parts, columns (1) and (2) report means at the sponsored and later jobs, column (3) reports the within-worker change, and column (4) reports the row-specific sample size.

Within 24 months, real modeled annual salary rises from \$129,121 to \$136,877. The \$7,756 increase equals 6.0% of the sponsored-job mean and is statistically significant at the 1% level. The worker's mean salary percentile among peers rises by 1.12 points, and log salary relative to qualifications rises by 0.007. Neither peer-relative change is statistically distinguishable from zero. Log salary relative to qualifications subtracts the log salary predicted from six observed qualifications among peer workers in the same jobs. The fixed-window comparison in Panel A therefore indicates higher salary and no

[7] The DHS final rule was published on December 29, 2025, and took effect on February 27, 2026. The Department of Labor published its proposed rule on March 27, 2026. See DHS final rule and Department of Labor proposed rule.

detectable decline in pay relative to qualifications.

The unrestricted comparison results presented in Panel B necessitate a more mixed interpretation. Real modeled salary rises by $2,494, or 1.8% of the sponsored-job mean, but the increase is not statistically distinguishable from zero. The mean salary percentile falls by 2.23 points, while log salary relative to qualifications falls by 0.041, or about 4%. This measure declines from 0.045 at the sponsored job to 0.004 at the later job. The salary premium present at the sponsored job therefore disappears, but salary at the later job remains almost exactly in line with the worker's measured qualifications.

Salary alone cannot reveal whether a worker moves among less-qualified coworkers. Figure 7 addresses that question in the unrestricted sample. For each of six qualifications, the gap equals the registrant's value minus the peer mean, measured in peer-worker standard deviations. A more positive gap means that the registrant stands farther above, or less far below, her peers on that measure.

All six gaps become more positive at the later job. The experience gap moves from −0.152 to 0.049 standard deviations, an increase of 0.201. The gap in average real salary across prior jobs moves from −0.321 to −0.078, an increase of 0.243. The increases range from 0.083 to 0.138 standard deviations for prior seniority, number of prior jobs, schooling, and an advanced degree. Each increase is statistically significant at the 1% level (Appendix Table A8).

A wider gap could arise because registrants gain qualifications or because they move to jobs with less-qualified peers. Appendix Table A8 separates these two components. Registrants gain 0.342 standard deviations of experience between jobs, while peers at the later job average 0.141 standard deviations more experience than peers at the sponsored job. The stronger later peers offset part of the registrants' gain, leaving the 0.201 increase in the gap. The same pattern holds for prior salary: the worker component rises by 0.309 standard deviations and the peer mean by 0.066, leaving a 0.243 increase. Later-job peers also have greater prior seniority and more prior jobs. The work-history gaps widen because registrants add more experience and prior positions, and show a larger rise in prior-job salary, than the corresponding peer means. The gaps do not widen because later-job peers have weaker work histories. The same pattern appears within the common 24-month window.

The positive changes in experience, prior salary, and advanced degrees remain when we vary the required number of peers, exclude registrants selected in a later draw, allow the later job to be at any US employer, retain repeat registrations, or include other matched registrants among the peers (Appendix Table A9). When we instead hold workers' qualifications at their sponsored-job values, the experience and prior-salary gaps fall. This final comparison again shows that later-job peers have stronger, not weaker, work histories.

The fixed-window results show continued career progress. Expressed in natural units,

the worker-side changes that Table A8 reports in peer-standard-deviation terms imply that by the time unselected registrants start another US job within two years of the draw, they have accumulated an average of 1.7 years of experience and added 1.3 positions to their recorded work histories. Their average real salary across prior jobs rises by $12,561, and the share holding an advanced degree rises by 4.7 percentage points. Their later peers also have stronger work histories, so the wider worker-peer gap comes from workers arriving at new employers with stronger resumes, not from weaker peer groups. Their later jobs pay $7,756 more than the sponsored jobs, a 6% increase, with no decline in pay relative to peers or measured qualifications. These workers reach new US employers with stronger resumes and higher salaries.

This conclusion applies only to workers whom we observe in another US job. It does not describe registrants who leave or never report another US position. Nor do stronger resumes and higher salaries show that the transition leaves innovative work unchanged. We next ask whether receiving a visa changes worker patenting.

6.5 Does receiving a visa increase worker patenting?

Table 8 estimates how receiving an H-1B visa changes the probability that a registrant appears as an inventor on at least one USPTO patent application. We link registrants to applications through Revelio's employee-patent crosswalk and date each application by its filing date. The unit of observation is a registration. Each column uses our baseline 2SLS specification and compares a visa recipient with a comparable registrant who does not receive one.

The filing period requires care because an application enters the crosswalk only after it is published or granted. The crosswalk is a December 2024 snapshot, and its latest observed filing date is in September 2024. We use March 1 of the draw year as the cutoff for every cohort.

Columns (1) and (2) count applications filed during the first 18 months beginning on March 1. This gives every cohort the same filing period, but not the same time for an application to enter the snapshot. An FY2021 window ends in August 2021, leaving more than three years for an application to appear. An FY2024 window ends in August 2024, leaving only a few months. Column (1) includes all four cohorts. Column (2) includes FY2021 and FY2022, for which historical publication lags imply that roughly 98% or more of filings during this period are observable.

Column (3) counts every observed application filed from March 1 onward. It therefore gives earlier cohorts more potential follow-up than later cohorts. Column (4) uses the same filing period and indicates whether an application lists the registrant at a US inventor address. Column (5) uses the column (3) outcome among registrants who appear on an application filed before March 1 of the draw year.

Within the common 18-month period, a visa recipient is 0.260 percentage points more

likely to file an observed application than a comparable registrant who does not receive one. This is a 38.3% increase relative to the sample mean of 0.679 percentage points (column (1)). Among FY2021 and FY2022 registrants, the estimate is 0.363 percentage points on a mean of 1.008 percentage points, or 36.0% (column (2)). The early-cohort estimate is equally large in proportional terms even though nearly all filings during its outcome period are observable. Using all applications observed from March 1 onward, the estimate is 0.207 percentage points, a 21.1% increase relative to the 0.981 percentage point mean (column (3)).

The location result connects worker patenting to the geographic response above. Receiving a visa raises the probability of an application listing the registrant at a US inventor address by 0.230 percentage points. This is a 26% increase relative to the 0.871 percentage point mean (column (4)). The US-address estimate is, if anything, slightly larger than the same-period estimate for any application (column (3)). This indicates that essentially all of the increase in patent filing attributable to receiving a visa is recorded at US inventor addresses. Because some registrants file applications with addresses in both the United States and another country, we cannot divide the overall estimate cleanly between US and foreign filings.

The response is particularly large among prior inventors. In this group, receiving a visa raises the probability of another observed application by 9.1 percentage points. This is a 32.6% increase relative to the 27.9 percentage point mean (column (5)).

Observed patenting before March 1 is similar among selected and unselected registrants (Figure 3). The table nevertheless measures only whether a registrant appears on at least one observed application. It does not measure the number or value of applications. The crosswalk may also miss applications that it cannot link to a profile, and publication delays hide recent filings. We therefore interpret the estimates as changes in observed application filing, not as complete measures of invention or evidence about a particular mechanism.

These worker-level estimates can coexist with firm-level evidence. Doran, Gelber, and Isen (2022) find that additional H-1B workers leave firm patenting roughly unchanged, while Chen, Hshieh, and Zhang (2021) and Dimmock, Huang, and Weisbenner (2022) find positive effects for start-ups. When a candidate does not receive a visa, a firm can substitute another inventor. Its patent count can therefore remain stable even if the affected worker becomes less likely to appear on an application (though this process may be more difficult for start-ups with hard-to-substitute skilled labor). Our design isolates this worker margin: receiving a visa raises the worker's probability of filing an observed patent application, including one that lists a US inventor address.

7. Conclusion

The principal effect of initial H-1B access is on where subsequent work occurs. Across three to six years of follow-up, a visa recipient is 2.4 percentage points more likely than a comparable registrant who does not receive one to start a US job and 2.7 points less likely to start one in the country of birth. A comparable registrant who does not receive a visa is 1.7 points more likely to report a position abroad at a US-headquartered company, a 44% increase relative to the 3.8% mean. For positions within the sponsor's corporate family, the difference is 1.1 percentage points. Some sponsor-worker matches therefore survive abroad, and the foreign-employment response is concentrated within the US corporate sector.

Workers also adjust to the initial constraint along other margins. Registrants who do not receive a visa are more likely to start an educational program or a position with a founder or co-founder title. A descriptive comparison among those later observed at another US employer shows continued career progress: within 24 months, they reach the later job with stronger work histories and a real modeled salary that is 6.0% higher than at the sponsored job. Within the same period, visa recipients have 6.7% higher modeled salary than comparable registrants who do not receive one, conditional on salary data for a reported position. Visa recipients are also 38% more likely to file an observed patent application within 18 months, and most of the increase comes from registrants who file an application with a US inventor address. We conclude that initial H-1B access determines where careers develop and where inventive activity is recorded, while workers continue to build careers through other routes.

If an H-1B visa from the initial registration were the only route to US work, the effect on US job starts would be far larger than 2.4 percentage points. In fact, 65.7% of unselected registrants start a US job during the available follow-up. For many workers, not being selected initially changes the route and timing of US employment rather than eventual access. Policymakers can change the number of available visas or the rule used to allocate them, with wage weighting as one possible allocation rule. The relevant welfare margins are how quickly workers reach US jobs and where firm-worker matches operate while workers adjust. Estimating how alternative cap and allocation rules change the timing and location of these matches is a promising area for future research.

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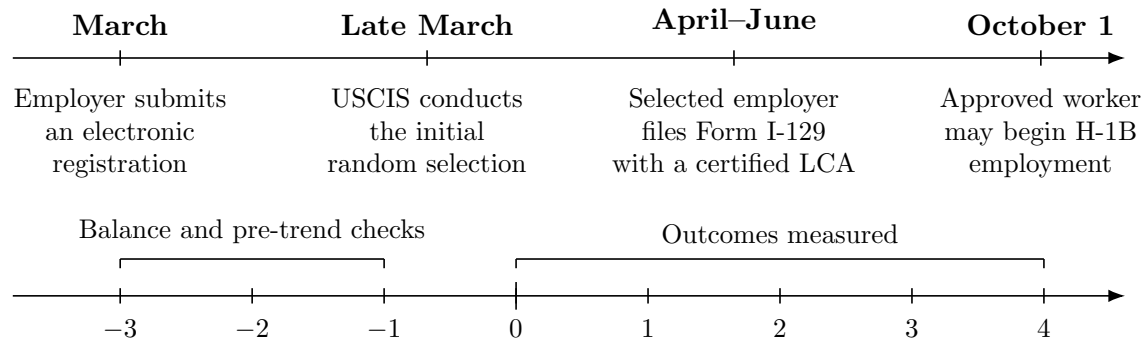
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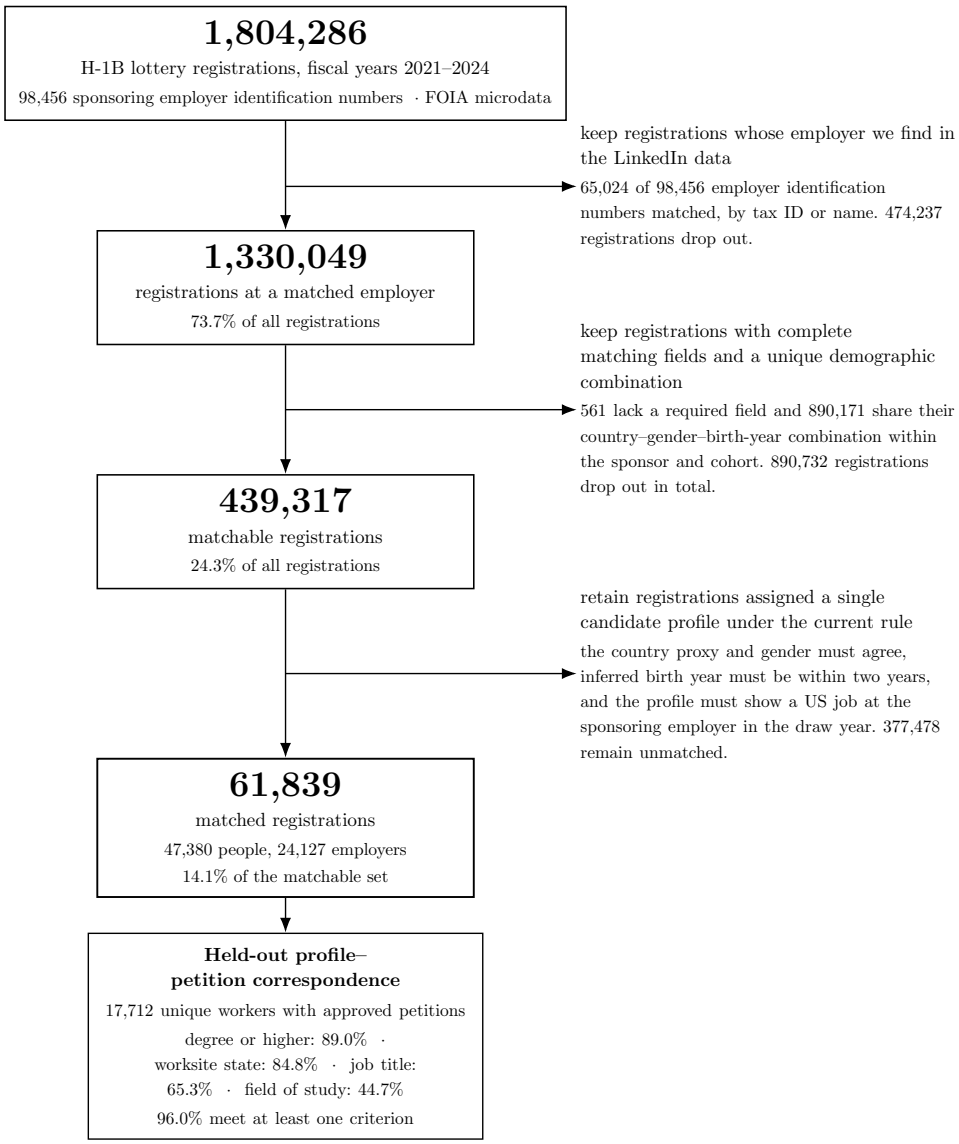
8. Figures

Figure 1: **H-1B registration, petition, and employment timeline**



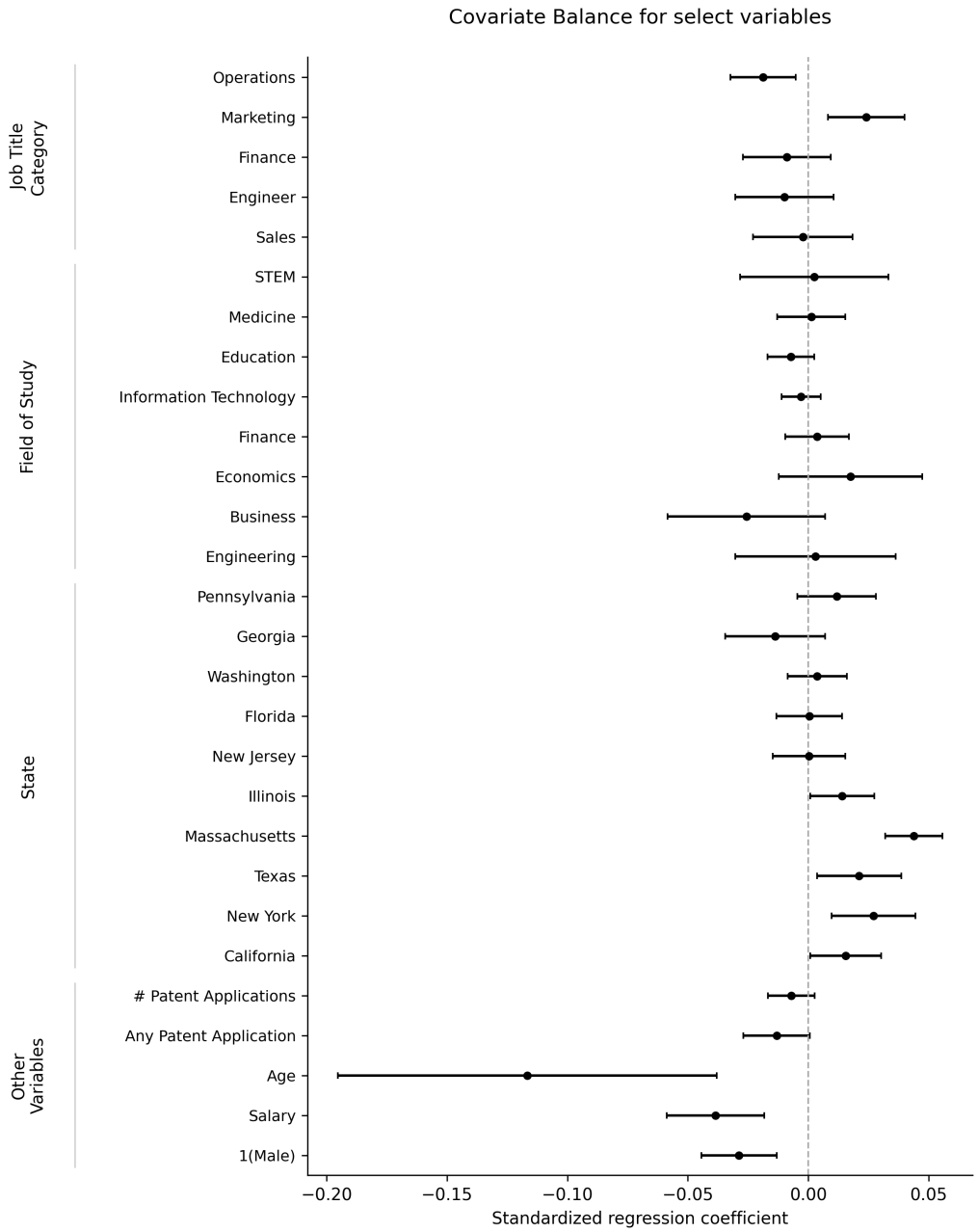
This figure shows the initial selection round for the FY2021–FY2024 lotteries. The registration and lottery occur in March of the calendar year before the fiscal year named in the registration. Selection permits the employer to file Form I-129; it does not provide work authorization. The lower axis shows event time in years relative to the draw: outcomes are measured from March of the draw year (year 0) onward, and the pre-draw years support covariate-balance and pre-trend checks. Career histories are observed through February 2026, and year-level outcomes use complete calendar years through 2025, so the FY2021 cohort (drawn in March 2020) is observed for all of years 0–4, while the FY2024 cohort (drawn in March 2023) is observed only through year 2.

Figure 2: **From H-1B lottery registrations to the matched LinkedIn sample**



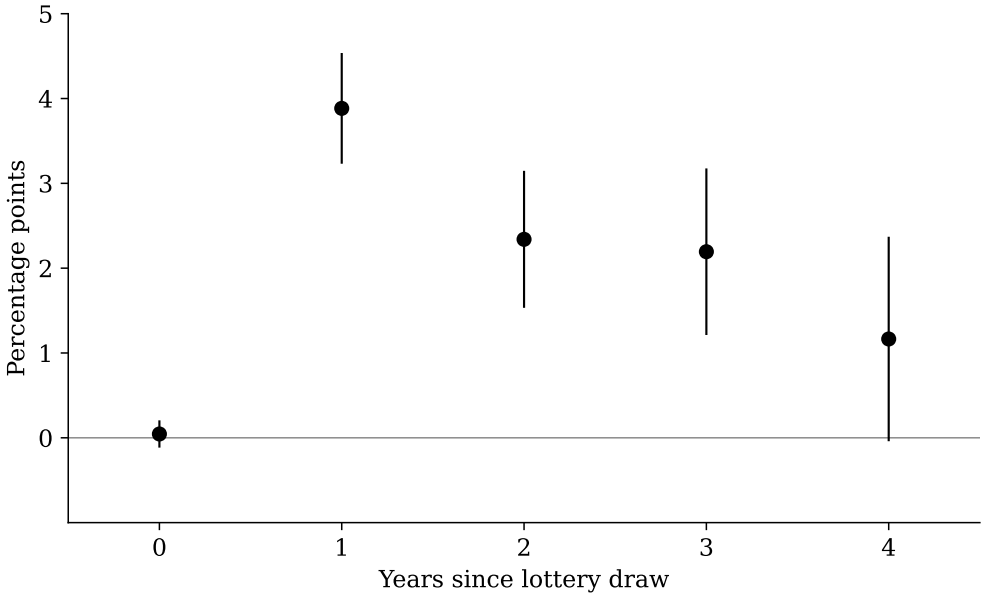
This figure reports registration counts at each stage of sample construction. The same worker may appear through registrations in more than one cohort; the 61,839 matched registrations represent 47,380 workers and 24,127 sponsors. Registration counts come from USCIS microdata obtained through a FOIA request, and profile data come from Revelio. Appendix A describes the employer and profile matches. The bottom box reports descriptive correspondence between held-out I-129 petition fields and the matched profiles of 17,712 unique workers with approved petitions. The denominators differ across the four checks.

Figure 3: **Balance in observables across selected and unselected sample registrants**



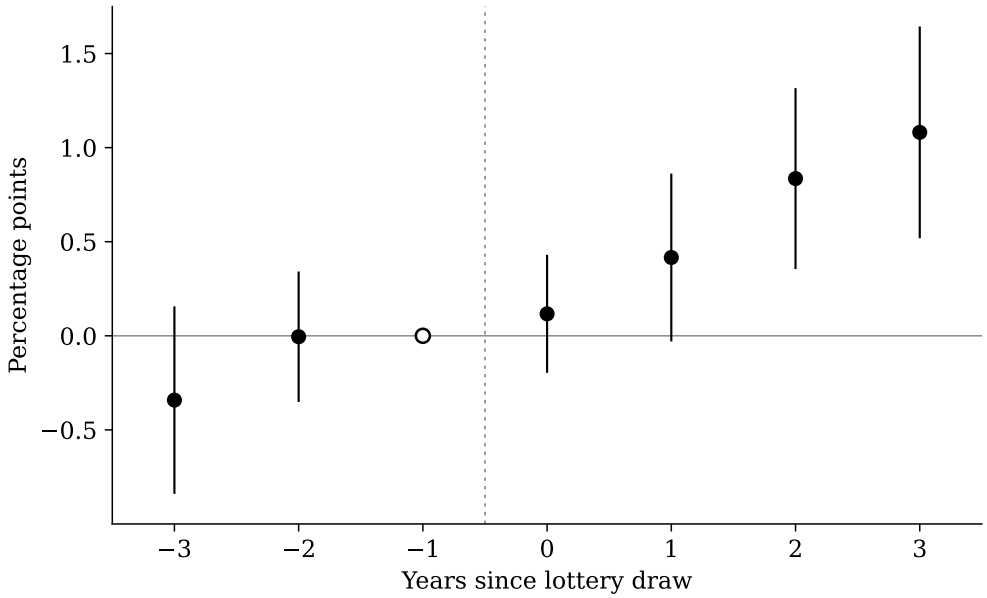
This figure compares selected and unselected registrants on characteristics measured before the lottery. Each point is the coefficient on lottery selection from a weighted least squares regression in which the dependent variable has been standardized. The regressions include country-of-birth, lottery-year, and advanced-degree fixed effects. Inverse-probability weights come from a logit model of lottery selection using the same characteristics. Bars report 95% confidence intervals based on standard errors clustered by country of birth. Appendix B defines the variables.

Figure 4: **Employer attachment after the lottery draw**



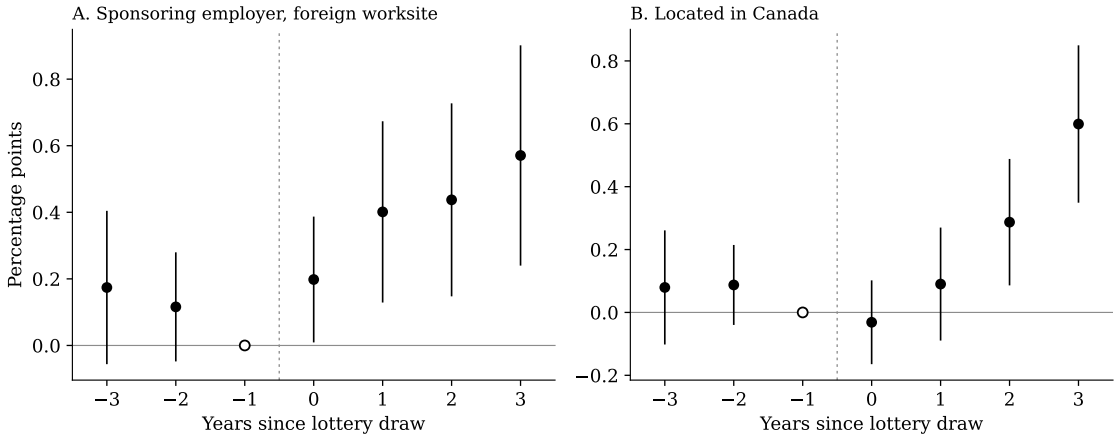
This figure plots intent-to-treat estimates of the effect of lottery selection on remaining employed at the sponsoring employer in each year after the lottery draw. The unit of observation is a registration. A registrant counts as attached in year τ if an active position at the sponsoring employer, or at its ultimate corporate parent, overlaps the τ-th calendar year after the draw. The sample includes the matched registrations from the 2021–2024 lotteries with the relevant calendar year observed by 2025, so years 3 and 4 exclude the most recent cohorts. Each point is the coefficient on *Lottery Selected* from a separate regression with lottery-year and country-of-birth fixed effects and controls for the natural log of modeled pre-lottery average salary, age at lottery, and indicators for male, advanced degree, and STEM degree. Coefficients are in percentage points. Bars represent 95% confidence intervals based on standard errors clustered by sponsoring employer.

Figure 5: **Employment abroad at US-headquartered firms around the lottery draw**



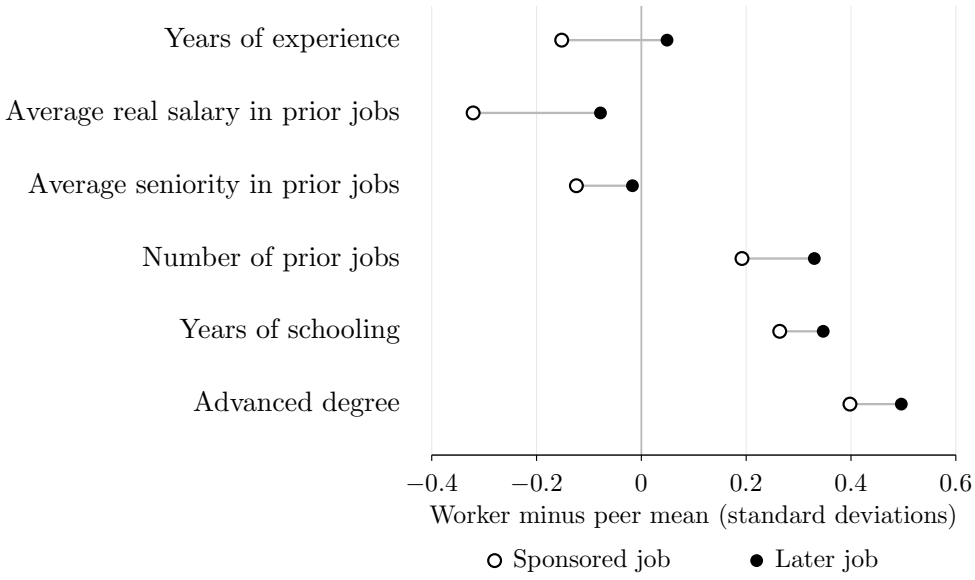
This figure plots event-study estimates of the effect of going unselected in the H-1B lottery on holding a position located outside the United States at a US-headquartered company. The unit of observation is a registrant-year. The sample includes the 47,380 matched registrants from the 2021–2024 lotteries, observed from three years before to three years after the calendar year of the lottery draw (March of year 0, marked by the dotted line). Estimates are first differences relative to year −1 within registrant, so registrant fixed effects difference out, with lottery-cohort × event-time effects; the hollow marker denotes the omitted year. A foreign position fully covered by a concurrent US position is not counted as located abroad. Coefficients are in percentage points. Bars represent 95% confidence intervals based on standard errors clustered by sponsoring employer. Variable definitions may be found in Appendix B.

Figure 6: **Foreign worksites of the sponsoring employer and relocation to Canada**



This figure plots event-study estimates of the effect of going unselected in the H-1B lottery on holding a position at the sponsoring employer located outside the United States (Panel A) and on holding a position located in Canada (Panel B). The unit of observation is a registrant-year. The sample, specification, and construction follow Figure 5: first differences relative to year -1 within registrant, lottery-cohort $\times$ event-time effects, and the concurrent-position rule for foreign spells. Coefficients are in percentage points. Bars represent 95% confidence intervals based on standard errors clustered by sponsoring employer.

Figure 7: **Qualifications at sponsored and later US jobs**



This figure follows unselected registrants from the job named in the H-1B registration to their first later job at a different US employer. The later job must begin after both the lottery draw and the sponsored job. We retain the earliest eligible unselected draw for each worker and use all eligible later jobs observed through February 2026. A peer began the same standardized role at the same employer during the previous 36 months, and the sample requires at least five peers at both jobs. Each point is the worker's qualification minus the peer mean at that job, in units of the peer-worker standard deviation. Qualifications use only information dated before the relevant job begins. The sample contains 1,682 workers, or 1,658 for the prior-salary and prior-seniority measures. No controls or fixed effects are included. Specifics on variable definitions may be found in Appendix B.

9. Tables

Table 1: **Summary statistics**

This table reports summary statistics for the matched sample. The sample consists of 61,839 H-1B lottery registrations matched to LinkedIn profiles. The table is restricted to the variables used in the analysis: the lottery and visa indicators, the regression controls, and the outcome variables whose sample means benchmark the estimates in the tables that follow. For each variable, we report the count of non-missing observations, means, standard deviations, minima, 25th percentiles, medians, 75th percentiles, and maxima in columns (1) through (8), respectively. Panel A reports registrant characteristics and lottery outcomes. Panel B reports variables measured before the H-1B lottery. Panel C reports outcomes measured after the H-1B lottery. Specifics on variable definitions may be found in Appendix B.

Variable	N	Mean	SD	Min	25%	Median	75%	Max
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Registrant characteristics and lottery outcomes								
Age at Lottery	61,839	31	6	19	27	30	34	68
Male	61,839	0.595	0.491	0.000	0.000	1.000	1.000	1.000
Lottery Selected	61,839	0.370	0.483	0.000	0.000	0.000	1.000	1.000
Granted Visa	61,839	0.295	0.456	0.000	0.000	0.000	1.000	1.000
High GDP Country	61,839	0.372	0.483	0.000	0.000	0.000	1.000	1.000
Patent Application Dummy	61,839	0.010	0.098	0.000	0.000	0.000	0.000	1.000
# Patent Applications	61,839	0.025	0.421	0.000	0.000	0.000	0.000	38.000
Panel B: Pre-lottery variables								
Highest Degree: Bachelor	61,839	0.316	0.465	0.000	0.000	0.000	1.000	1.000
Highest Degree: Master	61,839	0.582	0.493	0.000	0.000	1.000	1.000	1.000
Highest Degree: Doctor	61,839	0.082	0.274	0.000	0.000	0.000	0.000	1.000
Major: STEM (broad)	61,578	0.622	0.485	0.000	0.000	1.000	1.000	1.000
Modeled Pre-Lottery Salary	61,357	60,879	29,166	1,579	41,517	56,457	75,358	583,182
Panel C: Post-lottery outcomes								
Any Post-Lottery Work	61,839	0.745	0.436	0.000	0.000	1.000	1.000	1.000
Foreign work at H-1B Sponsor	61,839	0.019	0.137	0.000	0.000	0.000	0.000	1.000
Work: United States	61,839	0.678	0.467	0.000	0.000	1.000	1.000	1.000
Work: Home Country	61,839	0.102	0.302	0.000	0.000	0.000	0.000	1.000
Work: Other Non-US	61,839	0.060	0.237	0.000	0.000	0.000	0.000	1.000
Work: Canada	61,839	0.013	0.114	0.000	0.000	0.000	0.000	1.000
Any Post-Lottery Education	61,839	0.059	0.235	0.000	0.000	0.000	0.000	1.000
Education: United States	61,839	0.044	0.206	0.000	0.000	0.000	0.000	1.000
Education: Home Country	61,839	0.004	0.067	0.000	0.000	0.000	0.000	1.000
Education: Other Non-US	61,839	0.011	0.105	0.000	0.000	0.000	0.000	1.000
Education: Canada	61,839	0.001	0.037	0.000	0.000	0.000	0.000	1.000
Founder	61,839	0.015	0.122	0.000	0.000	0.000	0.000	1.000
Modeled Post-Lottery Salary	48,178	116,423	58,932	2,084	78,957	109,433	143,964	990,972

Table 2: **Effect of H-1B visa receipt on employment outcomes**

This table reports the effect of visa receipt on various post-lottery employment outcomes. The sample consists of H-1B lottery applicants matched to LinkedIn profiles. The dependent variables in this table are indicators for any new work experience in column (1), any new US work experience in column (2), any new work experience in the registrant's home country in column (3), any new work experience at any other non-US country in column (4), any new work experience based in Canada in column (5), and a post-lottery job at the same sponsoring employer in a foreign country in column (6). We determine work experience location using the country of the position in Revelio, and all new experiences are measured post-H-1B lottery. Indicator dependent variables are multiplied by 100 for readability. We use two-stage least squares. The primary independent variable is *Granted Visa*, which represents final approval of the visa petition, instrumented by selection in the H-1B lottery. We control for log modeled pre-lottery salary, age, gender, holding an advanced degree pre-lottery, and majoring in a STEM field pre-lottery. All regressions include country-of-birth and lottery-year fixed effects. We report coefficients on the independent variables, the Kleibergen-Paap first-stage F statistic, as well as the unconditional sample mean for the column's dependent variable, times 100. Standard errors (in parentheses) are clustered at the employer level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively. Specifics on variable definitions may be found in Appendix B.

	(1) Any New Work	(2) US Work	(3) Home Country Work	(4) Other Non-US Work	(5) Canada Work	(6) Foreign Sponsor
Granted Visa	-0.295	2.397***	-2.704***	-1.603***	-0.677***	-1.021***
	(0.443)	(0.484)	(0.317)	(0.258)	(0.130)	(0.151)
First-Stage KP F	57686.76	57686.76	57686.76	57686.76	57686.76	57686.76
Dep. Var. Mean	74.633	67.812	10.098	6.008	1.320	1.912
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Lottery Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	61,072	61,072	61,072	61,072	61,072	61,072
R^2	0.026	0.015	0.040	0.003	0.002	0.002

Table 3: **Effect of H-1B visa receipt on education outcomes**

This table reports the effect of visa receipt on various post-lottery education outcomes. The sample consists of H-1B lottery applicants matched to LinkedIn profiles. The dependent variables in this table are indicators for any new degree in column (1), any new degree at a US institution in column (2), any new degree at an institution from the registrant's home country in column (3), any new degree from any other non-US country in column (4), and any new degree at a Canadian institution in column (5). We determine an educational institution's location using Revelio school country, and all new degree start dates are measured post-H-1B lottery. Indicator dependent variables are multiplied by 100 for readability. We use two-stage least squares. The primary independent variable is *Granted Visa*, which represents final approval of the visa petition, instrumented by selection in the H-1B lottery. We control for log modeled pre-lottery salary, age, gender, holding an advanced degree pre-lottery, and majoring in a STEM field pre-lottery. All regressions include country-of-birth and lottery-year fixed effects. We report coefficients on the independent variables, the Kleibergen-Paap first-stage F statistic, as well as the unconditional sample mean for the column's dependent variable, times 100. Standard errors (in parentheses) are clustered at the employer level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively. Specifics on variable definitions may be found in Appendix B.

	(1) Any Education	(2) US Education	(3) Home Country Education	(4) Other Non-US Education	(5) Canada Education
Granted Visa	-2.605***	-2.154***	-0.060	-0.342***	-0.134***
	(0.250)	(0.222)	(0.069)	(0.113)	(0.043)
First-Stage KP F	57686.76	57686.76	57686.76	57686.76	57686.76
Dep. Var. Mean	5.749	4.369	0.422	1.100	0.133
Controls	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Lottery Year FE	Yes	Yes	Yes	Yes	Yes
N	61,072	61,072	61,072	61,072	61,072
R^2	0.013	0.010	0.003	0.003	0.000

Table 4: **Effect of H-1B visa receipt on modeled salary and founder roles**

This table reports the effect of visa receipt on post-lottery modeled salary and founder roles for H-1B lottery registrations matched to LinkedIn profiles. Columns (1) and (2) indicate whether the registrant has salary data in Revelio for at least one position that starts during the relevant window. Columns (3) and (4) report the natural log of one plus mean Revelio-modeled salary across those positions and are conditional on having salary data. Columns (5) and (6) indicate whether the registrant starts a founder role. Odd-numbered columns use a common 24-month window that begins after each cohort's final reported selection round. Even-numbered columns use all available years from the same date through February 2026. Indicator outcomes are multiplied by 100. We estimate the specifications by two-stage least squares, instrumenting *Granted Visa* with lottery selection. We control for age and log modeled pre-lottery salary. We also include a gender indicator and separate indicators for holding an advanced degree and majoring in a STEM field before the lottery. All regressions include country-of-birth and lottery-year fixed effects. Standard errors (in parentheses) are clustered at the sponsoring-employer level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively. Appendix B defines the variables.

	(1)	(2)	(3)	(4)	(5)	(6)
	Salary Data 24 Months	Salary Data All Years	Log Mean Modeled Salary 24 Months	Log Mean Modeled Salary All Years	Founder Role 24 Months	Founder Role All Years
Granted Visa	0.722	0.616	0.065***	0.056***	-0.329***	-0.423***
	(0.519)	(0.478)	(0.009)	(0.008)	(0.115)	(0.156)
First-Stage KP F	57686.76	57686.76	40392.34	49588.90	57686.76	57686.76
Dep. Var. Mean	58.411	68.722	11.484	11.507	1.179	2.098
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Lottery Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	61,072	61,072	35,670	41,968	61,072	61,072

Table 5: **Post-lottery employment abroad at US-headquartered firms**

This table examines whether the position tied to an H-1B registration relocates outside the US when the registrant does not obtain a visa. The unit of observation is a registrant. The sample consists of 61,839 matched H-1B registrations (47,380 individuals) from the 2021–2024 lotteries. Dropping registrations with missing controls leaves 61,310. The dependent variables are indicators for any new post-lottery position located outside the US at a US-headquartered company in column (1), at the sponsoring employer's foreign worksite in column (2), at a US-headquartered employer other than the sponsor in column (3), located in the registrant's country of birth in column (4), located in Canada in column (5), and located outside the US at any employer in column (6). Outcomes are dated from March of the draw year. Indicator dependent variables are multiplied by 100 for readability. We use two-stage least squares. The primary independent variable is *Granted Visa*, an indicator for final approval of the visa petition, instrumented by selection in the H-1B lottery. We control for log modeled pre-lottery salary, age, gender, holding an advanced degree pre-lottery, and majoring in a STEM field pre-lottery. All regressions include country-of-birth and lottery-year fixed effects. We report the coefficient on the independent variable, the Kleibergen-Paap first-stage F statistic, and the unconditional sample mean of the column's dependent variable, times 100. A foreign position fully covered by a concurrent US position is not counted as located abroad (Appendix A.6). Standard errors (in parentheses) are clustered at the employer level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively. Specifics on variable definitions may be found in Appendix B.

	(1) Telemigration	(2) Sponsor Foreign Worksite	(3) Other US-HQ Employer	(4) Home Country	(5) Canada	(6) Abroad Any Employer
Granted Visa	-1.670***	-1.070***	-1.029***	-0.908***	-0.375***	-3.759***
	(0.212)	(0.144)	(0.182)	(0.175)	(0.079)	(0.318)
First-Stage KP F	59328.54	59328.54	59328.54	59328.54	59328.54	59328.54
Dep. Var. Mean	3.780	1.720	2.800	2.560	0.430	9.950
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Lottery Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	61,310	61,310	61,310	61,310	61,310	61,310

Table 6: **Effect of H-1B visa receipt on registrant geography by home-country GDP**

This table reports the sensitivity of the effect of an H-1B visa to the wealth of the registrant's home country. The sample consists of H-1B lottery applicants matched to LinkedIn profiles. Registrants from countries without World Bank GDP data are excluded. The dependent variables in columns (1) through (4) are indicators for at least one post-lottery education or job at an institution/company based in various geographic locales. Locales are the United States in column (1), the registrant's country of birth in column (2), any other non-US country in column (3) and Canada in column (4). We also assume (in column (1)) that a registrant who did not report an end date to their sponsoring employer job, and did not make any new employment or education entries remained in the US. All dependent variables were measured post-H-1B lottery. We use two-stage least squares. The primary independent variable is the interaction between *Granted Visa*, an indicator for final approval of the visa petition, and the natural log of the home country's 2019 GDP per capita from the World Bank, standardized to mean zero and standard deviation one. We instrument *Granted Visa* and its interaction with lottery selection and its interaction with the same GDP measure. We explicitly model the level effect of *Granted Visa*. The level effect of GDP is absorbed by the country fixed effects. We also control for log modeled pre-lottery salary, age, gender, holding an advanced degree pre-lottery, and majoring in a STEM field pre-lottery. All regressions include country-of-birth and lottery-year fixed effects. We report coefficients on the independent variables, the Kleibergen-Paap first-stage F statistic, and the unconditional sample mean of the column's dependent variable, times 100. Appendix Table A7 repeats the test with an indicator for above-median GDP. Standard errors (in parentheses) are clustered at the employer level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively. Specifics on variable definitions may be found in Appendix B.

	(1) US	(2) Home Country	(3) Other non-US	(4) Canada
Granted Visa × Log GDP per Capita	0.187	0.118	-0.190	0.047
	(0.356)	(0.342)	(0.279)	(0.147)
Granted Visa	2.844***	-2.717***	-1.922***	-0.719***
	(0.361)	(0.321)	(0.274)	(0.133)
First-Stage KP F	16089.45	16089.45	16089.45	16089.45
Dep. Var. Mean	87.076	10.240	6.784	1.380
Controls	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Lottery Year FE	Yes	Yes	Yes	Yes
N	60,198	60,198	60,198	60,198

Table 7: **Salary at sponsored and later US jobs**

This table follows unselected registrants from the job named in the H-1B registration to their first later job at a different US employer. The later job must begin after both the lottery draw and the sponsored job. We retain the earliest eligible unselected draw for each worker. Columns (1) and (2) report means at the two jobs, and column (3) reports the within-worker change. Panel A requires the later job to begin before the second anniversary of the draw. Panel B uses all eligible later jobs observed through February 2026. A peer began the same standardized role at the same employer during the previous 36 months, and the sample requires at least five peers at both jobs. Modeled salaries come from Revelio and are converted to 2023 dollars using monthly CPI-U. Log salary relative to qualifications is the natural log of one plus real salary, minus the value predicted from a pooled regression among peer workers. The regression includes a fixed effect for each job and controls for years of experience, average real salary and seniority in prior jobs, number of prior jobs, years of schooling, and an advanced degree. No other controls or fixed effects are included. Parentheses report standard errors for column (3), two-way clustered by the sponsored and later employers. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively. Specifics on variable definitions may be found in Appendix B.

	(1) Sponsored job	(2) Later job	(3) Change	(4) N
Panel A: Later job begins before the second anniversary of the draw				
Real modeled salary (2023 $)	129,121	136,877	7,756***	1,086
			(2,418)	
Percentile of real salary among peers	48.77	49.90	1.12	1,086
			(1.07)	
Log salary relative to qualifications	0.016	0.023	0.007	1,068
			(0.014)	
Panel B: All eligible later jobs observed through February 2026				
Real modeled salary (2023 $)	136,172	138,666	2,494	1,669
			(2,326)	
Percentile of real salary among peers	50.95	48.72	-2.23**	1,669
			(0.89)	
Log salary relative to qualifications	0.045	0.004	-0.041***	1,645
			(0.011)	

Table 8: **Effect of H-1B visa receipt on patent-application filing**

This table reports estimates of the effect of H-1B visa receipt on patent-application filing. The unit of observation is a registration. All outcomes are indicators multiplied by 100 for readability, and applications are dated by filing date. We use two-stage least squares. Columns (1) and (2) indicate at least one application filed during the first 18 months beginning on March 1 of the draw year. Column (1) includes all four cohorts, while column (2) includes FY2021–22. Column (3) indicates at least one observed application filed on or after March 1 of the draw year. Column (4) indicates at least one such application that lists the registrant at a US inventor address. Column (5) uses the column (3) outcome among registrants with an application filed before March 1 of the draw year. *Granted Visa* indicates an approved I-129 petition and is instrumented with lottery selection. Controls are log modeled pre-lottery salary, age at the lottery, a male indicator, an advanced-degree indicator, and a STEM-field indicator. All regressions include country-of-birth and lottery-year fixed effects. Standard errors are clustered by sponsoring employer and reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively. Specifics on variable definitions may be found in Appendix B.

	(1) Any Application Within 18 Mo.	(2) Any Application Within 18 Mo.	(3) Any Application From March Onward	(4) Any Application With US Inventor Address	(5) Any Application From March Onward
Granted Visa	0.260***	0.363**	0.207*	0.230**	9.083**
	(0.091)	(0.158)	(0.114)	(0.105)	(3.709)
First-Stage KP F	57632.97	40082.35	57632.97	57632.97	2081.94
Dep. Var. Mean	0.679	1.008	0.981	0.871	27.883
Controls	Yes	Yes	Yes	Yes	Yes
Lottery Year FE	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Filing Window	18 mo.	18 mo.	Any	Any	Any
Sample	Full	FY21–22	Full	Full	Prior Inventors
N	61,079	25,386	61,079	61,079	1,058
R-squared	0.006	0.008	0.007	0.006	0.018

H-1B Visa Lottery and Real Outcomes

Appendix

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Appendix A. Sample construction

We begin with anonymous H-1B registration data for fiscal years 2021 through 2024 obtained through a Freedom of Information Act request to USCIS. The analysis file contains 1,804,286 eligible registrations. The cohort totals exactly match the eligible-registration counts published by USCIS.[8]

The registration data include country of birth, country of nationality, birth year, gender, sponsoring-employer identification number, employer name and location, registration year, and lottery status. These fields are available for more than 99% of registrations. USCIS selected 572,191 registrations in the four cohorts.

Selection allows the sponsoring employer to file Form I-129. The petition reports the offered position, worksite, pay, and the worker's degree and field of study. USCIS then approves or denies the petition. These fields are available only for selected registrations and therefore do not enter the matching procedure.

Appendix A.1 Characterizing the matchable set of H-1B registrations

We construct the main sample by matching H-1B registrations to LinkedIn profiles in Revelio. The match uses only fields available for selected and unselected registrations: employer, gender, birth year, and country of birth. We reserve the I-129 degree, field of study, job title, and worksite for the held-out correspondence checks in Appendix A.5.

Figure 2 visually maps out the three sequential restrictions that define the matchable set. First, the employer crosswalk excludes 474,237 registrations, leaving 1,330,049 registrations at sponsors identified in Revelio. Second, 561 registrations lack gender, country, or birth-year information, leaving 1,329,488. Third, we require each registration to have a unique country–gender–birth-year combination within its sponsor and lottery cohort to reduce the risk of ambiguous profile matches. This restriction removes an additional 890,171 registrations. The resulting matchable set contains 439,317 registrations, or 24.3% of the registration data.

Appendix A.2 Company matching

We first create a crosswalk between the employer identification numbers and names in the H-1B and Revelio data. The H-1B data contain 98,456 unique sponsoring employer identification numbers, and Revelio contains 7,399,289 US companies. We allow a Revelio company or its ultimate parent to identify the sponsor. We first match employer identification numbers and then use exact and approximate company-name matches for the remaining sponsors.

[8] https://www.uscis.gov/working-in-the-united-states/temporary-workers/h-1b-specialty-occupations/h-1b-electronic-registration-process

Employer-identifier matches link 5,628 sponsoring employer identification numbers to Revelio companies. We standardize the names of the remaining sponsors by removing special characters and legal suffixes and converting the text to uppercase. Exact standardized-name matches add 54,496 sponsors. Approximate name matching produces 15,938 additional candidates. For fuzzy matches with state information in both sources, we discard candidates whose states disagree; 4,900 fuzzy matches remain. State agreement is therefore an additional screen only for fuzzy matches when both states are observed.

Together, the identifier, exact-name, and fuzzy-name procedures match 65,024 of the 98,456 sponsoring employer identification numbers.

Appendix A.3 Revelio matchable set

The employer crosswalk supplies the company and parent-company identifiers used to collect candidate profiles. We construct a profile-side proxy for country of birth from education and employment histories. We first use the country of a foreign bachelor's degree. When that field is unavailable, we use another foreign degree and then the modal foreign country across at least 365 days of foreign work. These sources provide the country proxy for 69%, 12%, and 19% of the final matches, respectively. A country of education or work need not be a person's country of birth, so we treat this measure as a proxy to compare with the registration country of birth.

The current implementation constructs the country and birth-year proxies once using records through the latest lottery cohort, rather than reconstructing them from information observed before each worker's draw.

We infer birth year from education dates and the typical completion age for each degree type. When a completion date is unavailable, we use the degree's start date and typical duration. Because these calculations are approximate, the final match allows the inferred birth year to differ from the registration birth year by as much as two years.

We retain candidate profiles with an inferred foreign country and a US position at a matched employer. This filter leaves 12,464,880 worker-employer records covering 5,776,217 workers. The mean birth year is 1988, and the median is 1990.

Appendix A.4 H-1B-Revelio match

Within each sponsor and lottery cohort, a candidate profile must satisfy four conditions. The candidate must hold a US position at the sponsor that was active during the draw year, the calendar year in which the lottery was held. The profile-side country proxy must match the registration country of birth, and gender must also agree. Finally, the inferred birth year must be within two calendar years of the registration birth year.

We do not retain a match when the candidate set is empty or contains more than one profile. In the matching run, 852,905 registrations were skipped as duplicate combinations

within their employer and lottery year.[9] We discarded a further 377,478 registrations that either did not match any candidate profile on country, gender, and draw year, or else matched to more than one such profile. The final sample contains 61,839 of the 439,317 matchable registrations, a match rate of 14.1%. This percentage is the match rate under the current rules, not an estimate of Revelio coverage: a registration can remain unmatched because no profile exists, required profile fields are incomplete, or several profiles satisfy the matching criteria.

A registration can fail to match for several reasons. The profile may not report the foreign education or work history needed to infer country of birth. Education dates may also imply a birth year outside the two-year window. Other registrants may have no LinkedIn profile, an incomplete profile, or several profiles with the same matching characteristics. We treat each of these cases as unmatched rather than relax the rule and increase the risk of false matches.

We merge the matched profiles with Revelio education, experience, and modeled-salary records. We also merge the I-129 information available for selected registrations. Of the 61,839 registrations, 22,878 were selected in the lottery, or 37.0%. Within each draw year, the matched sample is 2.5 to 4 percentage points more likely to be selected than the registration population. The estimates therefore describe registrants who can be matched to professional profiles. Among the selected registrations, 18,246 have an approved petition, 448 have a denied petition, and 4,184 have an unknown petition status.

Table A1 compares the matched and matchable groups. Their gender composition, India and China shares, and birth years are similar. The matched group has a 2.4 percentage point larger selected share. Conditional on selection, it also has a 2.8 percentage point larger approval share.

Panel A of Table A2 shows that the match rate declines with sponsor size, from 20.8% among sponsors with one to five registrations to 4.7% among sponsors with more than 5,000. Larger sponsors contain more registrants who share the matching characteristics. Panel B shows that the final sample nevertheless includes many large sponsors; Meta, Microsoft, Goldman Sachs, Oracle, and Apple contribute the largest numbers of matched registrations.

Appendix A.5 Held-out profile–petition correspondence

The I-129 petition fields do not enter the profile match. We use them instead for descriptive correspondence checks among workers with approved petitions. The 18,246 approved registrations correspond to 17,712 unique matched workers.

[9] The run checks duplicates only at employers with at least one candidate profile. Counted over all crosswalked employers, the uniqueness restriction removes the 890,171 registrations reported in Appendix A.1.

The profile reports the petition degree or a higher degree for 89.0% of the 14,175 workers with usable degree information. The petition worksite state appears in at least one of five profile-side sources for 84.8% of 17,702 workers: a position at the sponsor, a position at any employer, the profile location, the sponsor's headquarters, or raw position-location text. At least half of the retained petition job-title words appear in a raw profile title for 65.3% of 9,592 workers. Raw title strings are available for 81.7% of approved workers with a petition title. At least half of the retained petition field-of-study words appear in a Revelio major for 44.7% of 17,420 workers.

Overall, 16,996 of the 17,712 workers, or 96.0%, meet at least one of these four criteria. The denominators differ because the source fields have different coverage. The checks also search information throughout the profile, the degree criterion permits a higher degree, and the state criterion accepts any of five sources. They therefore measure profile–petition correspondence; they do not independently establish that every registration–profile link is correct.

Correspondence declines as the absolute birth-year difference increases. For example, the job-title rate is 75.2% when the registration and inferred birth years agree, 66.1% when they differ by one year, and 50.7% when they differ by two years. The other three rates follow the same monotonic pattern. This pattern is consistent with stronger correspondence when the birth-year proxy is closer.

Appendix A.6 Position records used for telemigration outcomes

The 47,380 matched workers are associated with 363,658 unique position records in the Revelio data. The telemigration build removes 766 records with missing start dates, leaving 362,892. Location-specific measures require a reported position country, and measures of foreign positions at US-headquartered firms also require the employer's headquarters country. Records missing the required geography cannot set the corresponding outcome indicator.

Some profiles retain a foreign affiliation while reporting a concurrent US position. For the telemigration outcomes and event studies, we do not treat a foreign position as located abroad when a US position for the same worker covers at least 80% of its recorded year span. Of 121,269 position records with a reported non-US country, 15,915 meet this rule, or 13.1%; 105,354 remain classified as foreign positions.

Appendix B. Variable Definitions

Variable Definitions

Variable	Definition
Panel A: Treatment Variables	
Lottery Selected	Indicator taking on the value of 1 if the registration was selected in the H-1B lottery, and 0 otherwise.
Granted Visa	Indicator taking on the value of 1 if the registrant's I-129 visa petition received final approval, and 0 otherwise. In the two-stage least squares specifications, this indicator is instrumented by selection in the H-1B lottery.
Panel B: Dependent Variables from Tables 2-6, or their appendix analogs	
Any New Work	Indicator taking on the value of 1 if the registrant started any new job following the H-1B lottery, and 0 otherwise.
US Work	Indicator taking on the value of 1 if the registrant started a new job at a position located in the US following the H-1B lottery, and 0 otherwise.
Foreign Sponsor	Indicator taking on the value of 1 if the registrant started a new, non-US, post-lottery position at the employer that sponsored their H-1B lottery registration, and 0 otherwise.
Home Country Work	Indicator taking on the value of 1 if the registrant started a new job at a position located in their country of birth following the H-1B lottery, and 0 otherwise.
Other Non-US Work	Indicator taking on the value of 1 if the registrant started a new job at a position located in any country other than the US or their country of birth following the H-1B lottery, and 0 otherwise.
Canada Work	Indicator taking on the value of 1 if the registrant started a new job at a position located in Canada following the H-1B lottery, and 0 otherwise.
Any Education	Indicator taking on the value of 1 if the registrant started any new college degree following the H-1B lottery, and 0 otherwise.
US Education	Indicator taking on the value of 1 if the registrant started a new college degree at an institution in the US following the H-1B lottery, and 0 otherwise.
Home Country Education	Indicator taking on the value of 1 if the registrant started a new college degree at an institution in their country of birth following the H-1B lottery, and 0 otherwise.
Other Non-US Education	Indicator taking on the value of 1 if the registrant started a new college degree at an institution in any country other than the US or their country of birth following the H-1B lottery, and 0 otherwise.
Canada Education	Indicator taking on the value of 1 if the registrant started a new college degree at an institution in Canada following the H-1B lottery, and 0 otherwise.
Founder Role	Indicator taking on the value of 1 if the registrant starts a position during the relevant window whose raw LinkedIn title contains "founder" or "co-founder" as a standalone word, and 0 otherwise. Titles that reference founders without conveying a founding role, such as "founding member," "founding advisor," or positions in a founder's office, and generic chief-officer titles, do not qualify.

Definitions continue on the next page.

Variable Definitions (continued)

Variable	Definition
Panel B: Dependent Variables from Tables 2-6, or their appendix analogs (continued)	
Salary Data	Indicator taking on the value of 1 if the registrant has a Revelio-modeled salary for at least one position that starts during the relevant window, and 0 otherwise.
Log Mean Modeled Salary	The natural log of one plus the mean Revelio-modeled salary across the registrant's positions that start during the relevant window. Defined only for registrants with Salary Data equal to 1.
US	Indicator taking on the value of 1 if either the registrant started a new job at a firm headquartered in the US post-lottery, started a new degree at a US institution post-lottery, or logged no end date or new work/education entries post lottery.
Home Country	Indicator taking on the value of 1 if either the registrant started a new job at a firm headquartered in their country of birth post-lottery or started a new degree at an institution based in their country of birth post-lottery.
Other non-US	Indicator taking on the value of 1 if either the registrant started a new job at a firm headquartered in any other country than the US or their country of birth post-lottery or started a new degree at an institution based in any other country than the US or their country of birth post-lottery.
Canada	Indicator taking on the value of 1 if either the registrant started a new job at a firm headquartered in Canada post-lottery or started a new degree at a Canadian institution post-lottery.
Log GDP Per Capita	Natural log of the 2019 per capita GDP of the registrant's country of birth, as reported by the World Bank website.
High GDP	Indicator taking on the value of 1 if the registrant's country of birth was above median in 2019 per capita GDP, as reported by the World Bank website, and 0 otherwise.

Definitions continue on the next page.

Variable Definitions (continued)

Variable	Definition
Panel C: Employment Abroad at US-Headquartered Firms (Table 5)	
Telemigration	Indicator taking on the value of 1 if the registrant started any new post-lottery position located outside the US at a US-headquartered company, and 0 otherwise. A foreign position covered at 80% or more of its duration by a concurrent US position of the same registrant is not counted as located abroad.
Sponsor Foreign Worksite	Indicator taking on the value of 1 if the registrant started a new post-lottery position at a foreign worksite of the employer that sponsored their H-1B registration, and 0 otherwise.
Other US-HQ Employer	Indicator taking on the value of 1 if the registrant started a new post-lottery position located outside the US at a US-headquartered employer other than the sponsor, and 0 otherwise.
Home Country	In these tables only, an indicator taking on the value of 1 if the registrant started a new post-lottery position at a US-headquartered company with the position located in their country of birth, and 0 otherwise.
Canada	In these tables only, an indicator taking on the value of 1 if the registrant started a new post-lottery position at a US-headquartered company with the position located in Canada, and 0 otherwise.
Abroad Any Employer	Indicator taking on the value of 1 if the registrant started any new post-lottery position located outside the US at any employer, and 0 otherwise.
Panel D: Location Indicators	
State Indicators	Indicator variables for the US headquarters state of the sponsoring employer.
Panel E: Field of Study Indicators	
Field of Study	Indicator variables for the registrant's field of study on their most recent degree.
STEM	Indicator equal to 1 if the Revelio field of study classification for the registrant's most recent degree is "Engineering," "Information Technology," "Chemistry," "Mathematics," "Physics," "Biology," or "Statistics."
Panel F: Job Title Categories	
Sales	Indicator equal to 1 if the Revelio job title classification for the registrant at the sponsoring employer is "Sales."
Engineer	Indicator equal to 1 if the Revelio job title classification for the registrant at the sponsoring employer is "Engineer."
Finance	Indicator equal to 1 if the Revelio job title classification for the registrant at the sponsoring employer is "Finance."
Marketing	Indicator equal to 1 if the Revelio job title classification for the registrant at the sponsoring employer is "Marketing."
Operations	Indicator equal to 1 if the Revelio job title classification for the registrant at the sponsoring employer is "Operations."

Definitions continue on the next page.

Variable Definitions (continued)

Variable	Definition
Panel G: Other Control Variables	
1(Male)	Indicator variable equal to 1 if the registrant is male.
Salary	Revelio-modeled salary for the registrant's most recent position beginning before the lottery draw.
Age	Age of the registrant at the time of H-1B lottery registration.
Advanced Degree	Indicator equal to 1 if the registrant holds an advanced degree (Master's or above).
Panel H: Job Qualification Variables	
Years of Experience	Total time worked before the relevant job began, counting months in overlapping positions only once.
Average Real Salary in Prior Jobs	Mean Revelio-modeled annual salary across the worker's recorded positions beginning before the relevant job, converted to 2023 dollars using CPI-U for each position's start month.
Average Seniority in Prior Jobs	Mean Revelio seniority score across the worker's recorded positions beginning before the relevant job. Revelio codes seniority from 1 for entry-level positions to 7 for senior executives.
Number of Prior Jobs	Number of recorded positions beginning before the relevant job.
Years of Schooling	Education measure assigned from the highest degree completed before the relevant job: 11 for a doctorate, 6 for a master's degree, and 4 for any other observed degree.
Advanced Degree	Indicator equal to 1 if the worker's highest degree completed before the relevant job was a master's degree or doctorate, and 0 otherwise.

Definitions continue on the next page.

Variable Definitions (continued)

Variable	Definition
Panel I: Within-Worker Job Comparisons (Table 7, Figure 7, and Tables A8–A9)	
Peer Worker	A worker who began the same standardized role at the same employer during the 36 months before the registrant began the job.
Worker-Peer Qualification Gap	The registrant's qualification minus the mean among peer workers, in units of the standard deviation in the peer-worker reference sample. Qualifications use only information dated before the job began.
Real Modeled Salary	Revelio's modeled annual salary for the position, converted to 2023 dollars using CPI-U for the month in which the position began.
Salary Percentile Among Peers	The percentile rank of the registrant's real modeled salary among peer workers at the same job.
Log Salary Relative to Qualifications	The natural log of one plus real modeled salary, minus the value predicted by a pooled regression among peer workers. The regression includes a fixed effect for each job and years of experience, average real salary and seniority in prior jobs, number of prior jobs, years of schooling, and an advanced degree.
Panel J: Patent Variables (Table 8)	
Any Application From March Onward	Indicator taking on the value of 1 if the registrant is listed as an inventor on at least one USPTO patent application filed on or after March 1 of the draw year, and 0 otherwise. Applications are dated by filing date. Equal to zero for registrants with no linked application.
Any Application Within 18 Mo.	Indicator taking on the value of 1 if the registrant is listed as an inventor on at least one USPTO patent application filed during the first 18 months beginning on March 1 of the draw year, and 0 otherwise.
Any Application With US Inventor Address	Indicator taking on the value of 1 if the registrant is listed as an inventor on at least one USPTO patent application filed on or after March 1 of the draw year that lists the registrant at a US inventor address, and 0 otherwise.
FY21-22	Indicator taking on the value of 1 if the registrant is a member of the FY2021-22 H-1B lottery cohort.
Prior Inventor	Indicator taking on the value of 1 if the registrant is listed as an inventor on at least one USPTO patent application filed before March 1 of the draw year, and 0 otherwise. Defines the subsample in column (5) of Table 8.

Appendix C. Supplementary Tables

Table A1: **Comparison of matched and matchable samples**

This table compares basic observable characteristics among matched and matchable registrations. The unit of observation in both columns is a registration. Differences are calculated as Matched minus Matchable.

	Matched N = 61,839	Matchable N = 439,317	Difference
Demographics			
% Male	59.5%	60.4%	-0.9%
% India	48.4%	49.6%	-1.2%
% China	12.5%	12.6%	-0.1%
Average birth year	1990.5	1989.5	0.9
Median birth year	1992	1991	1
Lottery outcomes			
Selected in lottery	37.0%	34.6%	2.4%
Approved (given selected)	79.8%	76.9%	2.8%

Table A2: **Employer match rates by size and top employers**

This table reports employer representation in the matchable and matched samples. Employer size is the number of registrations sponsored in the raw H-1B data. Panel A groups employers by this measure. The employer count includes Revelio-matched sponsors with at least one matchable registration; the remaining columns report matchable registrations, matched registrations, and their ratio. Panel B lists the 20 sponsors with the most matched registrations and reports their raw, matchable, and matched registration counts and match rates.

Panel A: Match rates by employer size

Employer Size Bin	# Employers	# Matchable	# Matched	Match Rate
1–5	49,516	87,987	18,310	20.8%
6–20	8,228	74,625	12,866	17.2%
21–100	4,594	122,940	15,348	12.5%
101–500	2,073	103,433	11,365	11.0%
501–5000	271	36,235	3,289	9.1%
5000+	16	14,097	661	4.7%

Panel B: Top 20 employers by matched records

Employer Name	# Raw Records	# Matchable	# Matched	Match Rate
Meta Platforms, Inc.	11,449	1,762	125	7.1%
Microsoft Corporation	13,158	1,785	113	6.3%
Goldman Sachs & Co LLC	3,159	1,090	111	10.2%
Oracle America, Inc.	4,076	689	92	13.4%
Apple Inc.	8,951	2,075	89	4.3%
Bloomberg LP	2,143	590	86	14.6%
The Boston Consulting Group, Inc.	1,848	885	82	9.3%
NVIDIA Corporation	2,463	649	82	12.6%
Google LLC	14,436	2,167	80	3.7%
Micron Technology, Inc.	1,301	404	76	18.8%
Cummins Inc.	1,705	636	76	11.9%
Deloitte & Touche LLP	3,061	994	75	7.5%
Bain & Company, Inc.	794	439	73	16.6%
Qualcomm Technologies, Inc.	3,699	627	73	11.6%
JPMorgan Chase & Co.	3,168	919	73	7.9%
EPAM Systems, Inc.	1,497	525	69	13.1%
Intuit Inc.	1,211	408	64	15.7%
Cisco Systems, Inc.	2,841	599	61	10.2%
Amazon.com Services LLC	36,206	2,302	57	2.5%
Uber Technologies, Inc.	1,879	645	56	8.7%

Table A3: **Effect of H-1B visa receipt on registrant geography**

This table reports the effect of visa receipt on whether the registrant moved to a new country, and which countries they moved to. It pools the employment margins of Table 2 and the education margins of Table 3 into a single location call per registrant. The sample consists of H-1B lottery applicants matched to LinkedIn profiles. The dependent variables in columns (1) through (4) are indicators for at least one post-lottery education or job at an institution/company based in various geographic locales. Locales are the United States in column (1), the registrant's country of birth in column (2), any other non-US country in column (3) and Canada in column (4). We also assume (in column (1)) that a registrant who did not report an end date to their sponsoring employer job, and did not make any new employment or education entries remained in the US. All dependent variables were measured post-H-1B lottery. We use two-stage least squares. The primary independent variable is *Granted Visa*, which represents final approval of the visa petition, instrumented by selection in the H-1B lottery. We control for log modeled pre-lottery salary, age, gender, holding an advanced degree pre-lottery, and majoring in a STEM field pre-lottery. All regressions include country-of-birth and lottery-year fixed effects. We report coefficients on the independent variables, the Kleibergen-Paap first-stage F statistic, as well as the unconditional sample mean for the column's dependent variable, times 100. Standard errors (in parentheses) are clustered at the employer level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively. Specifics on variable definitions may be found in Appendix B.

	(1) US	(2) Home Country	(3) Other non-US	(4) Canada
Granted Visa	2.864***	-2.716***	-1.834***	-0.696***
	(0.356)	(0.319)	(0.271)	(0.132)
First-Stage KP F	57686.76	57686.76	57686.76	57686.76
Dep. Var. Mean	87.064	10.265	6.767	1.366
Controls	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Lottery Year FE	Yes	Yes	Yes	Yes
N	61,072	61,072	61,072	61,072
R^2	0.004	0.040	0.004	0.002

Table A4: **Intent-to-treat estimates for the main outcomes**

This table collects the intent-to-treat estimates for the main outcomes of the paper. Each row is one outcome, and each cell shows the coefficient above its standard error in parentheses. Column (1) repeats the two-stage least squares baselines from Tables 2, 3, and 4, where winning the lottery instruments for *Granted Visa*. Column (2) replaces *Granted Visa* with *Lottery Selected*, so each outcome is regressed on lottery selection directly. We control for age and log modeled pre-lottery salary. We also include a gender indicator and separate indicators for holding an advanced degree and majoring in a STEM field before the lottery. All regressions include country-of-birth and lottery-year fixed effects. In Panel C, Salary Data, Log Mean Modeled Salary, and Founder Role are measured over the common 24-month window defined in Table 4. The number of observations for Salary Data and Founder Role equals the N reported in Panels A and B, while Log Mean Modeled Salary is estimated on the smaller sample with salary data: 35,670 observations in column (1) and 35,669 in column (2). Standard errors (in parentheses) are clustered at the employer level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1) Baseline (IV)	(2) Intent-to-treat
Panel A: Employment		
Any New Work	-0.295	-0.235
	(0.443)	(0.352)
US Work	2.397***	1.902***
	(0.484)	(0.385)
Home Country Work	-2.704***	-2.146***
	(0.317)	(0.253)
Other Non-US Work	-1.603***	-1.272***
	(0.258)	(0.205)
Canada Work	-0.677***	-0.538***
	(0.130)	(0.103)
Foreign Sponsor	-1.021***	-0.810***
	(0.151)	(0.120)
N	61,072	61,072
Panel B: Education		
Any Education	-2.605***	-2.066***
	(0.250)	(0.199)
US Education	-2.154***	-1.708***
	(0.222)	(0.176)
Home Country Education	-0.060	-0.048
	(0.069)	(0.055)
Other Non-US Education	-0.342***	-0.272***
	(0.113)	(0.090)
Canada Education	-0.134***	-0.106***
	(0.043)	(0.034)
N	61,072	61,072
Panel C: Other career outcomes		
Salary Data	0.722	0.572
	(0.519)	(0.412)
Log Mean Modeled Salary	0.065***	0.052***
	(0.009)	(0.007)
Founder Role	-0.329***	-0.261***
	(0.115)	(0.091)

Table A5: **Coefficient stability across match construction and employer fixed effects**

This table reports the coefficient on *Granted Visa* from the main two-stage least squares specifications when fixed effects for the sponsoring employer are added. Column (1) repeats the baseline estimates from Tables 2, 3, and 4. Column (2) adds the employer fixed effects to the baseline specification. Each cell reports the coefficient with its standard error in parentheses. We control for age and log modeled pre-lottery salary. We also include a gender indicator and separate indicators for holding an advanced degree and majoring in a STEM field before the lottery. All regressions include country-of-birth and lottery-year fixed effects. In Panel C, Salary Data, Log Mean Modeled Salary, and Founder Role are measured over the common 24-month window defined in Table 4. The number of observations for Salary Data and Founder Role equals the N reported in Panels A and B, while Log Mean Modeled Salary is estimated on the smaller sample with salary data: 35,670 in columns (1) and (2). Standard errors (in parentheses) are clustered at the employer level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1) Baseline	(2) Employer FE
Panel A: Employment outcomes		
Any New Work	-0.295	-0.021
	(0.443)	(0.533)
US Work	2.397***	0.968*
	(0.484)	(0.575)
Home Country Work	-2.704***	-1.130***
	(0.317)	(0.376)
Other Non-US Work	-1.603***	-0.282
	(0.258)	(0.295)
Canada Work	-0.677***	-0.217*
	(0.130)	(0.130)
Foreign Sponsor	-1.021***	-0.398**
	(0.151)	(0.186)
N	61,072	61,072
Panel B: Education outcomes		
Any Education	-2.605***	-1.431***
	(0.250)	(0.283)
US Education	-2.154***	-1.185***
	(0.222)	(0.253)
Home Country Education	-0.060	-0.031
	(0.069)	(0.078)
Other Non-US Education	-0.342***	-0.112
	(0.113)	(0.127)
Canada Education	-0.134***	-0.040
	(0.043)	(0.040)
N	61,072	61,072
Panel C: Other career outcomes		
Salary Data	0.722	1.499**
	(0.519)	(0.636)
Log Mean Modeled Salary	0.065***	0.029***
	(0.009)	(0.011)
Founder Role	-0.329***	-0.201
	(0.115)	(0.130)

Table A6: **Coefficient stability across birth-year match tolerance**

This table reports the coefficient on *Granted Visa* from the main two-stage least squares specifications, re-estimated on subsamples of the matched sample defined by the gap between the birth year on the H-1B registration and the birth year implied by the LinkedIn profile. Column (1) repeats the baseline full-sample estimates from Tables 2, 3, and 4. Column (2) restricts the sample to matches where the two birth years agree exactly. Column (3) restricts the sample to matches where the gap is exactly one year, and column (4) to matches where the gap is exactly two years. Each cell reports the coefficient with its standard error in parentheses. We control for age and log modeled pre-lottery salary. We also include a gender indicator and separate indicators for holding an advanced degree and majoring in a STEM field before the lottery. All regressions include country-of-birth and lottery-year fixed effects. In Panel C, Salary Data, Log Mean Modeled Salary, and Founder Role are measured over the common 24-month window defined in Table 4. The number of observations for Salary Data and Founder Role equals the N reported in Panels A and B, while Log Mean Modeled Salary is estimated on the smaller sample with salary data: 35,670 in column (1), 10,343 in column (2), 14,869 in column (3), and 10,423 in column (4). Standard errors (in parentheses) are clustered at the employer level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1) Baseline	(2) Exact match	(3) Gap of 1 year	(4) Gap of 2 years
Panel A: Employment outcomes				
Any New Work	-0.295	-0.561	-0.375	0.185
	(0.443)	(0.779)	(0.679)	(0.863)
US Work	2.397***	3.047***	2.346***	1.739*
	(0.484)	(0.863)	(0.745)	(0.940)
Home Country Work	-2.704***	-3.882***	-2.275***	-1.969***
	(0.317)	(0.571)	(0.477)	(0.640)
Other Non-US Work	-1.603***	-2.344***	-1.625***	-0.640
	(0.258)	(0.476)	(0.390)	(0.494)
Canada Work	-0.677***	-1.395***	-0.611***	-0.012
	(0.130)	(0.266)	(0.191)	(0.228)
Foreign Sponsor	-1.021***	-1.503***	-1.083***	-0.490*
	(0.151)	(0.281)	(0.231)	(0.280)
N	61,072	17,494	25,412	18,141
Panel B: Education outcomes				
Any Education	-2.605***	-3.218***	-3.261***	-1.023**
	(0.250)	(0.457)	(0.384)	(0.478)
US Education	-2.154***	-2.788***	-2.642***	-0.806**
	(0.222)	(0.414)	(0.345)	(0.411)
Home Country Education	-0.060	-0.000	-0.076	-0.123
	(0.069)	(0.116)	(0.098)	(0.150)
Other Non-US Education	-0.342***	-0.336*	-0.586***	0.062
	(0.113)	(0.191)	(0.174)	(0.231)
Canada Education	-0.134***	-0.152*	-0.141**	-0.105
	(0.043)	(0.079)	(0.066)	(0.075)
N	61,072	17,494	25,412	18,141
Panel C: Other career outcomes				
Salary Data	0.722	0.004	0.758	1.382
	(0.519)	(0.931)	(0.810)	(1.012)
Log Mean Modeled Salary	0.065***	0.094***	0.075***	0.018
	(0.009)	(0.015)	(0.013)	(0.017)
Founder Role	-0.329***	-0.340*	-0.096	-0.701***
	(0.115)	(0.205)	(0.180)	(0.219)

Table A7: **Effect of H-1B visa receipt on registrant geography: above-median GDP split**

This table repeats Table 6 with a binary measure of home-country wealth in place of the continuous one. The sample consists of H-1B lottery applicants matched to LinkedIn profiles. The dependent variables in columns (1) through (4) are indicators for at least one post-lottery education or job at an institution/company based in various geographic locales. Locales are the United States in column (1), the registrant's country of birth in column (2), any other non-US country in column (3) and Canada in column (4). We use two-stage least squares. The primary independent variable is the interaction between *Granted Visa*, an indicator for final approval of the visa petition, and *High GDP*, an indicator for the per capita GDP of the registrant's country of birth being above median in 2019. We instrument *Granted Visa* and its interaction with lottery selection and its interaction with *High GDP*. We explicitly model the level effect of *Granted Visa*. The level effect of *High GDP* is absorbed by the country fixed effects. We also control for log modeled pre-lottery salary, age, gender, holding an advanced degree pre-lottery, and majoring in a STEM field pre-lottery. All regressions include country-of-birth and lottery-year fixed effects. We report coefficients on the independent variables, the Kleibergen-Paap first-stage F statistic, and the unconditional sample mean of the column's dependent variable, times 100. Standard errors (in parentheses) are clustered at the employer level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively. Specifics on variable definitions may be found in Appendix B.

	(1) US	(2) Home Country	(3) Other non-US	(4) Canada
Granted Visa × High GDP	-0.373	0.674	0.027	0.075
	(0.701)	(0.635)	(0.545)	(0.254)
Granted Visa	3.016***	-2.991***	-1.845***	-0.727***
	(0.461)	(0.406)	(0.346)	(0.171)
First-Stage KP F	15424.40	15424.40	15424.40	15424.40
Dep. Var. Mean	87.064	10.265	6.767	1.366
Controls	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Lottery Year FE	Yes	Yes	Yes	Yes
N	61,072	61,072	61,072	61,072

Table A8: **Decomposing changes in qualifications relative to peers**

This table decomposes the within-worker qualification changes summarized in Figure 7. The unit of observation is an unselected registrant, with the earliest eligible unselected draw retained for each worker. The later job is the first eligible job at a different US employer. A peer begins the same standardized role at the same employer during the previous 36 months, and each sample requires at least five usable peers at both jobs. Entries in columns (1) through (5) are in units of the peer-worker standard deviation. Panel A requires the later job to begin before the second anniversary of the draw. Panel B uses all eligible later jobs observed through February 2026. Columns (1) and (2) report the worker's qualification minus the peer mean at each job. Column (3) reports the change in this gap. Columns (4) and (5) report the changes in the worker's qualification and the peer mean, so column (3) equals column (4) minus column (5). Qualifications use only information dated before the relevant job begins. No controls or fixed effects are included. Parentheses report standard errors for columns (3) through (5), two-way clustered by sponsored and later employer. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively. Specifics on variable definitions may be found in Appendix B.

	(1) Gap at sponsored job	(2) Gap at later job	(3) Change (2) − (1)	(4) Change in worker	(5) Change in peer mean	(6) N
Panel A: Later job begins before the second anniversary of the draw						
Years of experience	-0.148	-0.011	0.137***	0.251***	0.114***	1,086
			(0.018)	(0.007)	(0.018)	
Avg. real salary in prior jobs	-0.316	-0.112	0.204***	0.263***	0.059***	1,068
			(0.022)	(0.009)	(0.020)	
Avg. seniority in prior jobs	-0.118	-0.035	0.082***	0.131***	0.049***	1,068
			(0.019)	(0.008)	(0.017)	
Number of prior jobs	0.230	0.397	0.168***	0.391***	0.224***	1,086
			(0.015)	(0.006)	(0.016)	
Years of schooling	0.250	0.346	0.096***	0.093***	-0.003	1,086
			(0.024)	(0.013)	(0.020)	
Advanced degree	0.391	0.477	0.087***	0.097***	0.011	1,086
			(0.023)	(0.013)	(0.020)	
Panel B: All eligible later jobs observed through February 2026						
Years of experience	-0.152	0.049	0.201***	0.342***	0.141***	1,682
			(0.015)	(0.007)	(0.015)	
Avg. real salary in prior jobs	-0.321	-0.078	0.243***	0.309***	0.066***	1,658
			(0.018)	(0.009)	(0.017)	
Avg. seniority in prior jobs	-0.124	-0.017	0.106***	0.166***	0.059***	1,658
			(0.016)	(0.007)	(0.015)	
Number of prior jobs	0.192	0.330	0.138***	0.429***	0.291***	1,682
			(0.012)	(0.006)	(0.013)	
Years of schooling	0.264	0.347	0.083***	0.094***	0.012	1,682
			(0.020)	(0.010)	(0.016)	
Advanced degree	0.398	0.496	0.098***	0.106***	0.008	1,682
			(0.019)	(0.011)	(0.016)	

Table A9: **Qualification changes under alternative samples and peer thresholds**

This table reports the change in the worker's qualification relative to the peer mean under alternative samples and peer-support thresholds. Columns (1) through (3) report changes in units of the peer-worker standard deviation. Columns (4) and (5) report the number of registrations for the prior-salary measure and the other two measures. The baseline uses all eligible later jobs through February 2026, one registration per worker, a different-employer later job, and at least five usable peers at both jobs. Each remaining row changes the feature named in its label. Selection in a later draw is excluded only when it occurs before the later job begins. The all-registrations row retains repeat registrations and clusters standard errors by worker. The final row holds the worker's qualification at its sponsored-job value, so the change reflects only the difference between the two peer groups. No controls or fixed effects are included. Parentheses report standard errors for columns (1) through (3). Except in the all-registrations row, they are two-way clustered by sponsored and later employer; in that row, they are clustered by worker. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively. Specifics on variable definitions may be found in Appendix B.

	(1)	(2)	(3)	(4)	(5)
	Change in worker-peer gap			N	
	Years of experience	Avg. real salary in prior jobs	Advanced degree	Prior salary	Other measures
Baseline: support ≥ 5	0.201***	0.243***	0.098***	1,658	1,682
	(0.015)	(0.018)	(0.019)		
Support ≥ 1	0.173***	0.215***	0.112***	4,112	4,253
	(0.014)	(0.015)	(0.017)		
Support ≥ 10	0.228***	0.247***	0.075***	1,045	1,059
	(0.018)	(0.023)	(0.022)		
Support ≥ 16	0.210***	0.242***	0.088***	738	750
	(0.020)	(0.025)	(0.024)		
Later job before second anniversary	0.137***	0.204***	0.087***	1,068	1,086
	(0.018)	(0.022)	(0.023)		
Excluding selection in a later draw	0.195***	0.233***	0.093***	1,475	1,499
	(0.016)	(0.019)	(0.020)		
Later job at any US employer	0.182***	0.205***	0.064***	2,995	3,030
	(0.009)	(0.011)	(0.012)		
All registrations	0.215***	0.248***	0.096***	1,989	2,016
	(0.015)	(0.016)	(0.018)		
Including other matched registrants as peers	0.202***	0.242***	0.101***	1,670	1,694
	(0.015)	(0.018)	(0.020)		
Worker held fixed at sponsored-job value	-0.141***	-0.066***	-0.008	1,658	1,682
	(0.015)	(0.017)	(0.016)		