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STEM OPT and the Path to Work: H-1B Access and Early Career Outcomes

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STEM OPT and the Path to Work: H-1B Access and Early Career Outcomes*

Abstract

In the United States, international graduates must convert Optional Practical Training (OPT) into an H-1B visa before their work authorization expires. A 2016 reform gave STEM graduates seven more months, without changing which fields qualified, just as H-1B adjudication tightened. Linking employer sponsorship filings to H-1B petitions, we follow graduates from sponsorship through adjudication to early employment. The share of STEM applications reaching the petition stage rose 11 percentage points. Approval fell for Master's applicants and processing slowed for Master's and PhD cases. Employment continuity still rose for Master's graduates; private-sector employment and earnings rose for PhD recipients.

JEL classification

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OPT, H-1B visa, work authorization, high-skilled immigration, immigration policy

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I. Introduction

The United States hosts more international students than any other country. In 2024/25, 1.18 million international students were enrolled in U.S. institutions, and a record 294,253 were on Optional Practical Training (OPT), 21 percent more than a year earlier (Institute of International Education, 2025). OPT is now the largest temporary work program for high-skilled workers in the United States (Ruiz and Budiman, 2018). Whether these graduates stay in the United States depends largely on their transition from OPT, a temporary work authorization tied to their degree, to employer-sponsored H-1B status. Demand for cap-subject H-1B visas has exceeded the annual limit of 85,000 in every recent year, and visas are allocated by lottery (U.S. Citizenship and Immigration Services, 2025a). Graduates who do not complete this step before OPT expires must leave the country or change status.

This transition matters for three reasons. First, it determines whether the United States retains the skills it helps create. Many international graduates hold STEM degrees, and high-skilled immigrants raise innovation and productivity (Hunt and Gauthier-Loiselle, 2010; Peri et al., 2015). A graduate who leaves after OPT takes that training elsewhere. Second, it shapes early careers. The time a graduate can remain employed while seeking sponsorship affects whether the worker–employer match survives, which sector the graduate enters, and what the job pays. Employers also lose when a match they invested in ends because of a lost lottery or a delayed petition. Third, current U.S. policy has been focused on this step. A 2025 proclamation imposed a $100,000 payment on new H-1B petitions (Executive Office of the President, 2025), and a 2026 proclamation further extended it through September 2027 (Executive Office of the President, 2026). USCIS guidance exempts petitions for workers who change status from inside the United States (U.S. Citizenship and Immigration Services, 2025b). OPT workers are already in the country, so the OPT-to-H-1B pathway avoids the payment and is the least costly route into longer-term U.S. employment. A 2025 rule also replaced the equal-odds lottery with a wage-weighted one, first used for the fiscal year 2027 cap (U.S. Department of Homeland Security, 2025). Each registration now enters the draw

one to four times, according to which of the four Department of Labor prevailing-wage levels the offered wage reaches. New graduates are usually hired at the lowest levels, so their odds fall. Both changes raise the value of remaining in authorized employment while seeking sponsorship. How much that time helps is the question we address.

We study the 2016 STEM OPT extension. OPT allows graduates on F-1 visas to work in their field of study (U.S. Citizenship and Immigration Services, 2025c). A 2008 reform allowed STEM graduates to extend OPT from 12 to 29 months (Congressional Research Service, 2024), and reforms in 2011 and 2012 broadened the list of STEM-eligible majors (Israel and Batalova, 2021). The 2016 reform extended authorized work for already eligible STEM graduates from 29 to 36 months (U.S. Department of Homeland Security, 2016). No new fields gained STEM designation between 2014 and 2020. For some graduates, the extra seven months added an H-1B filing cycle. As a result, the reform offers a direct test of how additional time in authorized employment affects the transition to H-1B status.

Prior research shows that OPT reforms affect STEM major choice (Amuedo-Dorantes et al., 2019), post-graduation retention (Demirci, 2019), and the size and composition of the international student population (Amuedo-Dorantes et al., 2023). Related work examines how international graduates enter the U.S. labor market through OPT (Beine et al., 2023). However, much less is known about what happens thereafter, once international graduates on OPT seek H-1B sponsorship. This transition may also differ by degree level. Master's graduates account for most OPT participants (Neufeld, 2019) and often move directly into private-sector jobs that are subject to the H-1B cap. In contrast, PhD recipients are more likely to start in academic or research positions exempt from the H-1B cap (Amuedo-Dorantes and Furtado, 2019).

We examine how the 2016 extension affected international graduates working under OPT as they sought H-1B employment. We combine two administrative sources: Labor Condition Applications (LCAs) filed with the Department of Labor, which record employer attempts to sponsor foreign workers, and Form I-129 petitions from the Department of Homeland Secu-

rity, which record H-1B adjudication and the terms of subsequent employment. Linked, the two sources follow graduates through each stage the reform targets: sponsorship attempts, the lottery, adjudication, and early-career employment.

No dataset follows prospective international students from the decision to study in the United States through enrollment, field choice, OPT, and H-1B sponsorship, so every study of OPT examines part of the sequence. We focus on the stage the reform targets most directly, graduates working under OPT who seek H-1B sponsorship, and our estimates capture the reform's effect at that stage, given earlier choices about whether to study in the United States, choose STEM, or stay after graduation.

A simple worker–employer match framework organizes the analysis. Longer authorization raises the chance of lottery selection and the value of continuing the match; adjudication frictions lower both. We then estimate a difference-in-differences design that compares STEM and non-STEM graduates, who were differentially exposed to the extension. At the occupation-year level, we combine LCA and I-129 records to study application attempts and the share of LCAs that reach the petition stage. At the individual level, we use I-129 petitions to study petition approval, processing times, private-sector employment, employment continuity, and earnings by degree level.

We find that the 2016 OPT extension expanded access to the H-1B pipeline. The share of STEM LCAs reaching the petition stage in a given cycle, known in the literature as the lottery winning rate, rose by 11.1 percentage points in our preferred specification, or 31 percent relative to the mean. Even our most conservative estimate, without industry controls, is positive and significant at the 10 percent level. STEM LCA filings also rose by about 30 percent relative to non-STEM filings, consistent with graduates remaining in the pipeline longer and filing in an additional cycle. However, greater access did not bring uniformly smoother transitions. Processing delays grew from 2017, and Requests for Evidence (RFEs) rose sharply in 2018. In this environment, approval rates fell by 1.5 percentage points for STEM Master's applicants, and processing times rose by 8 to 9 percent for Master's

and by about 25 percent for PhD applicants. Graduates still gained in the labor market. Employment continuity rose by about 8 percentage points among Master's graduates, and private-sector employment and reported wages rose by 3.4 percentage points and 5 to 6 percent among PhD recipients.

Three threats could bias these estimates, and we address each in turn. The first is selection into STEM. The extension may have led some students to choose or remain in STEM fields. Pre-existing differences could also produce diverging trends for STEM and non-STEM graduates. To address this concern, we restrict the analysis to years in which the list of STEM fields did not change. We also test for differential changes in applicant composition, estimate event studies, and reweight on pre-reform characteristics. Finally, we drop non-STEM graduates and compare STEM F-1 graduates with STEM J-1 and TN/E-3 visa holders, whom the extension did not cover. Selection into these visa types differs, but the main patterns broadly persist.

The second threat is contemporaneous shocks that differed by STEM status: faster growth in labor demand for STEM workers and the tightening of H-1B adjudication after 2017. We address labor demand with occupation-specific unemployment trends, industry and state fixed effects, and a Bartik-type labor-demand control. None of these controls overturns the lottery, adjudication, or continuity results. We address adjudication tightening by restricting the sample to the years before RFE intensity peaked, and the adjudication patterns already appear in that window.

The third threat is measurement. In the aggregate analysis, the lottery winning rate depends on how LCAs are matched to petitions and on how employers file LCAs. Inference also relies on a limited number of occupation clusters. The lottery estimate holds when unmatched LCAs are treated as non-selections rather than as missing. The ratio of petitions to LCA-covered workers shows no differential shift by STEM status. The estimate also holds under wild cluster bootstrap inference and when computer and mathematical occupations are excluded. In the individual-level analysis, STEM status depends on the list of STEM

fields and on how mixed-field degrees are classified. The results are broadly similar under the 2016 DHS list of STEM majors and when mixed-field degrees are assigned to the non-STEM group.

The results separate access to the H-1B pipeline from progression through it. More time helped STEM graduates reach the petition stage and helped Master's graduates stay with their employers. The adjudication environment shaped how far they progressed from there. Descriptive evidence on RFE intensity, on cap-subject and cap-exempt petitions, and on time elapsed between LCA filing and adjudication is consistent with this reading.

The analysis makes three contributions to research on high-skilled migration and post-study employment. First, it studies a stage of the pathway that prior work has not reached. Research on OPT has examined enrollment, major choice, OPT participation, employment under OPT, and initial retention (Amuedo-Dorantes et al., 2019; Demirci, 2019; Amuedo-Dorantes et al., 2023; Beine et al., 2023). International students respond to visa rules when deciding whether and where to study (Shih, 2017; Chen, 2021; Chen et al., 2023), and visa access shapes employer sponsorship and hiring (Shih, 2016; Bound et al., 2018; Mayda et al., 2018). We study the step in between: how the length of temporary work authorization shapes the move from OPT to employer-sponsored status.

Second, the 2016 reform isolates the value of additional authorized time. Earlier OPT reforms lengthened work authorization and expanded the list of eligible fields at the same time. Their effects mix longer work rights with new entrants into STEM. The 2016 reform only extended work authorization, from 29 to 36 months, for fields that already qualified. No fields gained eligibility between 2014 and 2020. This allows us to estimate the effect of more time on graduates already in the pipeline, holding eligibility fixed.

Third, we show that the gains from more time depend on the stage of the transition and on the adjudication environment. We link employer LCAs from the Department of Labor to individual Form I-129 petitions from DHS. Studies of the H-1B lottery typically build winning rates from LCAs at the firm or city level (Peri et al., 2015; Dimmock et al.,

2022; Glennon, 2024; Mahajan et al., 2024). The linked records let us trace the same reform through sponsorship attempts, lottery access, adjudication, employment continuity, sector, and earnings, separately by degree level. Longer work authorization widened access to the H-1B pipeline, but gains at later stages depended on how petitions were adjudicated, and they differed by degree.

The findings also inform current policy. Temporary work rights and the rules for employer-sponsored status operate jointly. Longer work authorization preserves worker–employer matches and adds chances to seek sponsorship. How much of that access becomes retention depends on how the next stage is administered. Our estimates show that time in authorized employment helps graduates reach the petition stage. The 2025 H-1B changes make that time more valuable. Wage-weighted selection lowers per-cycle odds for entry-level graduates. The new $100,000 payment does not apply to changes of status within the United States, a route that OPT workers already use. Other countries face the same questions as they compete to retain internationally trained graduates. Policies to retain foreign graduates should be judged both by how long they extend work authorization and by how well they support the move to longer-term status.

The rest of the paper proceeds as follows. Section II. describes the institutional setting and the 2016 extension. Section III. sets out the framework and its predictions. Section IV. describes the data, and Section V. the empirical design and threats to identification. Section VI. presents the main findings, Section VII. the robustness checks, and Section VIII. the mechanisms. Section IX. concludes.

II. Institutional Setting

Optional Practical Training (OPT) allows international students on an F-1 visa to gain work experience in occupations related to their field of study (U.S. Citizenship and Immigration Services, 2025c). Originally designed as a temporary training program, OPT permitted

students to work for up to 12 months before either leaving the country or transitioning to a different legal status. Work experience may be gained before degree completion (*i.e.*, pre-completion), in which case it counts toward the total OPT duration, or after degree completion (*i.e.*, post-completion). The latter is far more common. Students may apply up to 90 days before completing their academic program and no later than 60 days afterward. During post-completion OPT, workers may accumulate up to 90 days of unemployment.

Until 2008, the OPT period was limited to 12 months for all majors (Amuedo-Dorantes et al., 2023). In 2008, the program was expanded to allow an additional 17 months for students in science, technology, engineering, and mathematics (STEM) fields, extending the total period to 29 months (Congressional Research Service, 2024). Subsequent reforms in 2011 and 2012 broadened the list of STEM-eligible majors. Beginning in 2012, students in STEM majors with six-digit CIP codes ending in "99" (formerly excluded from the 2008 reform) became eligible for the extension, along with several fields with substantial STEM content, such as certain environmental studies programs and quantitative economics majors. These changes granted newly eligible students access to the 17-month extension, raising their total OPT duration from 12 to 29 months.

These earlier reforms were motivated by concerns about the loss of U.S.-trained STEM talent due to difficulties securing H-1B visas. Amuedo-Dorantes et al. (2019) show that the 2008 reform encouraged already-enrolled students to shift toward STEM majors, while Demirci (2019) document increases in post-graduation stay rates. Amuedo-Dorantes et al. (2023) evaluate the combined effects of the 2008, 2011, and 2012 reforms and find that international enrollments rose by 18 percent in bachelor's programs and 30 percent in master's programs, accompanied by improvements in student quality.

In 2016, OPT underwent another major reform: the 17-month STEM extension was replaced with a 24-month extension, allowing eligible STEM graduates to remain in the United States for up to 36 months in total (U.S. Department of Homeland Security, 2016). Unlike the earlier reforms, the 2016 change extended the duration of authorized employment

for fields that were already STEM-eligible. It also increased the additional unemployment allowance during the STEM extension from 30 to 60 days, raising the maximum across the full OPT period from 120 to 150 days. The reform provided both additional authorized employment time and greater flexibility to search for employment while maintaining legal status.

The 2016 rule also changed employer obligations (U.S. Department of Homeland Security, 2016). Employers of STEM OPT workers had to adopt a formal training plan (Form I-983), attest that duties, hours, and compensation matched those of similarly situated U.S. workers, and participate in E-Verify, which the 2008 rule already required. These provisions applied only to the STEM extension and are part of the reform we evaluate. By raising the cost of employing STEM OPT workers, they would, if anything, dampen the increases in application attempts and employment we document. The rule also allowed graduates to base the extension on an earlier U.S. STEM degree. We assign STEM status using the degree reported on the I-129 petition. The petition records the highest degree, so graduates whose highest degree is non-STEM but who hold an earlier STEM degree remain in our comparison group. Their added eligibility would bias our estimates toward zero. Cap-gap relief, which extends OPT for students with pending H-1B petitions until October 1, was unchanged and applied to all F-1 students.

No additional fields gained STEM designation during our 2014–2020 estimation window; the next expansion of the STEM list came in January 2022. The policy variation we use is an increase in the duration of work authorization for already eligible fields, not a change in which fields qualified.

Figure 1 summarizes the institutional pathway studied in the paper. The extension first lengthened the period during which STEM graduates can remain in authorized employment under OPT. This additional time may have increased H-1B application attempts through Labor Condition Applications (LCAs), allowed graduates to remain eligible for subsequent lottery cycles, and increased progression to Form I-129 adjudication. Conditional on reaching

this stage, the reform may also have affected early-career labor market outcomes, including employment continuity, private-sector employment, and earnings. The figure also previews the two levels of analysis used below. At the occupation-year level, we combine LCA and I-129 data to study application attempts and the share of LCAs reaching the petition stage. At the individual level, we use Form I-129 petitions to examine adjudication outcomes and subsequent employment trajectories.

III. Conceptual Framework

Two forces shaped how the 2016 STEM OPT extension affected the transition to H-1B employment: the extra time in authorized employment that the reform created, and the adjudication environment in which H-1B petitions were processed. A simple worker–employer match framework helps separate the two and organizes the empirical analysis around three predictions.

Consider an international graduate working under post-completion OPT and an employer deciding whether to keep the worker on and pursue H-1B sponsorship. Let T denote the time and flexibility afforded by OPT authorization, including the duration of authorized employment and the permitted unemployment period, and let A denote the degree of adjudication friction, including Requests for Evidence (RFEs), processing delays, and uncertainty over approval. The expected net value of pursuing sponsorship is given by

$$V(T, A) = P_S(T)\, P_A(A)\, R(T, A) - C(A),$$

where $P_S(T)$ is the probability that the graduate is selected in the H-1B lottery in at least one of the filing cycles available during OPT, $P_A(A)$ is the probability that the petition is approved once selected, $R(T, A)$ is the expected value of continuing the employment match, and $C(A)$ is the cost of sponsorship and of the uncertainty surrounding adjudication. Normalizing the value of not pursuing sponsorship to zero, the employer sponsors the worker

when $V(T, A) \geq 0$.

For cap-subject applicants, OPT duration matters because it determines how many lotteries they can enter. Let q be the probability of selection in a single lottery and $n(T)$ the number of filing cycles available while the graduate remains authorized to work, with $n(T)$ non-decreasing in T. The probability of being selected at least once is given by

$$P_S(T) = 1 - (1 - q)^{n(T)}.$$

Selection within any given lottery is random, but every additional cycle raises the chance of eventually being selected:

$$\frac{\partial P_S}{\partial n} = -(1 - q)^{n(T)} \ln(1 - q) > 0.$$

Hence $P_S(T)$ is weakly increasing in OPT duration and rises strictly whenever the extension opens up an additional filing cycle. The magnitudes are not small. With a single-cycle selection probability of 46 percent, roughly the average cap-subject rate in the early years of our sample, P_S goes from 46 percent with one cycle to about 71 percent with two. By lengthening OPT from 29 to 36 months, the reform may have given some STEM graduates exactly that additional cycle. Cap-exempt applicants face no lottery, so for them $P_S(T) = 1$.

The first prediction concerns access to the petition stage. Longer OPT duration raises V by giving employers more time to file and, for cap-subject applicants, by raising P_S. More matches should proceed to sponsorship, and LCA filings should rise. We cannot observe $P_S(T)$ itself because we do not follow individuals across filing cycles. What we observe is the per-cycle lottery winning rate, the ratio of cap-subject I-129 petitions to LCA filings within each occupation and year. That ratio is the product of two probabilities: that an LCA goes on to a filed petition, and that a filed petition is selected. The second is random, so additional cycles by themselves do not move the ratio. A graduate who files again adds one LCA to that year's cell and, with probability q, one selected petition. The reform can

move the first probability, however. With more authorized time left, fewer sponsored workers separate from the employer, change status, or lose eligibility before the petition is filed. We expect the extension to raise both LCA filings and the per-cycle winning rate in STEM occupations.

The second prediction concerns the value of the match. Additional authorized time lets workers accumulate U.S. experience, gives employers longer to learn about their productivity, and allows the employment relationship to continue while sponsorship is pursued. Under this employer-learning and match-attachment mechanism,

$$\frac{\partial R(T, A)}{\partial T} > 0.$$

Longer authorization should strengthen employment continuity and improve access to private-sector and better-paid positions. How this plays out depends on the beneficiary's degree. Bachelor's and Master's graduates are concentrated in cap-subject jobs, so for them more time raises both P_S and R. PhD recipients more often start at universities and nonprofit research organizations that are exempt from the cap. For them $P_S = 1$, and additional time matters only through R.

The third and final prediction concerns adjudication. Greater scrutiny lowers the probability of approval, erodes the value of the match by delaying the transition and adding uncertainty, and raises costs:

$$\frac{\partial P_A(A)}{\partial A} < 0, \qquad \frac{\partial R(T, A)}{\partial A} < 0, \qquad \frac{\partial C(A)}{\partial A} > 0,$$

so that $\partial V(T, A)/\partial A < 0$ for a given OPT duration. Tighter adjudication should lower approval rates and lengthen processing times, and it should do so most for cap-subject petitions. H-1B adjudication did tighten from 2017 onward. That tightening was not part of the OPT reform, but it coincided with the reform's implementation.

The aggregate analysis tests the first prediction using LCA filings and the lottery win-

ning rate. The individual-level analysis takes up the second and third, examining petition approval, processing time, private-sector employment, employment continuity, and reported wages. Here the model does not deliver a signed prediction. The two forces work against each other: outcomes improve when the value of additional authorized time outweighs the costs of delay and uncertainty, and they deteriorate when adjudication frictions dominate. The reduced-form estimates capture their combined effect. Evidence on RFEs, cap-subject and cap-exempt petitions, and elapsed time in the OPT-to-H-1B transition helps distinguish the role of additional authorized time from that of adjudication frictions.

IV. Data

Our analysis draws on administrative data from two complementary sources that allow us to examine both aggregate H-1B dynamics and individual-level transitions from OPT to H-1B. The first dataset consists of Labor Condition Applications (LCAs) filed with the U.S. Department of Labor (DOL), which capture employer sponsorship activity for H-1B workers.[1] The second dataset contains individual Form I-129 petitions obtained from the Department of Homeland Security (DHS). These records provide detailed information on H-1B adjudication, including approval decisions, processing times, employer identifiers, job descriptions, and degree information. The two sources capture key stages of the OPT-to-H-1B pathway: employer sponsorship activity measured through LCAs; lottery selection among cap-subject applicants, reflected in their appearance in the I-129 adjudication records; petition outcomes and processing times; and subsequent employment-related outcomes. When the number of

[1] A certified Labor Condition Application (LCA) is a prerequisite to filing Form I-129 and is intended to ensure that hiring a foreign worker does not adversely affect U.S. workers' wages and working conditions. During most of our sample period, employers seeking cap-subject H-1B visas submitted complete I-129 petitions, including a certified LCA, before USCIS conducted the lottery. Only petitions selected in the lottery entered the adjudication process and appear in the I-129 records used in our analysis; unselected petitions were returned and are not observed. USCIS introduced electronic pre-registration for the FY2021 cap season, after which only employers with selected registrations submitted Form I-129. Cap-exempt employers, such as universities and nonprofit research organizations, are not subject to the lottery and may file Form I-129 directly after obtaining a certified LCA.

timely filed, eligible cap-subject petitions exceeded the annual limit, USCIS selected petitions randomly. Access to the lottery was not allocated on a first-come, first-served basis. The timing of when the petition was submitted to USCIS within the annual filing cycle depended largely on employers and their legal representatives rather than mechanically on a worker's OPT start date.

LCAs are required for all H-1B petitions and include information on the date the case was submitted, case status,[2] proposed employment start date, visa class, employer name, job title, and the Standard Occupational Classification (SOC) code associated with the requested job, among other details. Because the LCA data do not report field of study, STEM status in the aggregate analysis is assigned at the occupation level. We classify occupations using the Census Bureau's SOC-based STEM taxonomy. To maintain consistency across years, we restrict attention to LCAs associated with new employment, excluding amendments, concurrent employment, and filings by cap-exempt institutions such as universities and nonprofit research organizations. Throughout the aggregate lottery analysis, we focus on cap-subject filings because cap-exempt employers do not participate in the annual H-1B lottery. These restrictions align the aggregate sample with the population exposed to the cap-subject selection process.

Form I-129 petitions provide the individual-level information needed to analyze adjudication and early-career labor market outcomes. These records contain the beneficiary's degree field, degree level, country of birth, job description, reported prevailing-wage information, employer name and location,[3] adjudication outcomes, and the timing of each adjudicative

[2]The case statuses associated with the last significant event or decision include *certified* (approved), *certified-withdrawn* (approved but later voluntarily withdrawn by the employer), *denied*, and *withdrawn* (pulled before adjudication). *Certified-withdrawn* cases may reflect changes in hiring plans, applicant decisions, or outcomes of the H-1B lottery process.

[3]Although the I-129 records also contain worksite-location information, we do not use these fields to study geographic or worksite mobility. Worksite-location reporting is highly incomplete before 2017 and appears to have changed around our reform window, likely in connection with contemporaneous USCIS filing-location and service-center assignment practices. Because this reporting change coincides with the policy period, mobility outcomes constructed from these fields are not reliable for our difference-in-differences analysis.

action.[4] STEM status in the individual-level analysis is assigned using CIP codes reported on the petition, not the SOC-based measure used in the aggregate analysis. The use of CIP codes allows us to classify STEM status based directly on academic training rather than occupation.

A. Aggregate-Level Analysis

For the aggregate analysis, we combine LCA and I-129 data to construct a cap-subject lottery winning rate at the occupation-by-year level.[5] We exclude amendments, extensions, and other continuations of employment to focus on new cap-subject petitions. Petitions from cap-exempt employers are removed because these do not enter the lottery. We retain petitions whose employment start dates align with the October 1 cap cycle so that the analysis reflects the annual timing of cap-subject filings.[6] We further restrict the sample to beneficiaries whose first H-1B petition follows directly after an F-1 OPT period, improving comparability between STEM and non-STEM graduates in terms of visa status at filing. Finally, nationality-based exemptions, such as those available to citizens of Chile and Singapore under the H-1B1 program, are excluded because these applicants do not enter the general lottery.

One measurement challenge is that some certified LCAs cannot be matched to I-129 petitions at the occupation-year level. In our baseline specification, we do not treat all such non-matches as lottery losses. Non-merged LCAs may reflect true non-selection in the H-1B lottery. They may also capture cases that drop out earlier in the pipeline, for

[4]Salary information in the I-129 records reflects the prevailing wage filed by the employer as part of the LCA, which represents a statutory floor rather than actual compensation. For high-skilled workers in private-sector STEM positions, actual earnings frequently exceed prevailing wage filings, which may attenuate observed earnings responses in our analysis.

[5]We collapse and merge the two datasets at the occupation-by-year level and calculate the lottery winning rate for each occupation-by-year cell as: $\frac{\text{I-129 petitions}}{\text{LCA filings}}$.

[6]Under USCIS rules, H-1B cap-subject petitions may not be filed more than six months in advance of the intended start date, and the earliest allowable filing date is April 1 for jobs starting on or after October 1 of the same year. See U.S. Citizenship and Immigration Services (2025a).

example because the employer does not proceed with petition filing, the worker separates from the employer, the worker changes legal status, or the case is withdrawn before an I-129 petition is submitted. Coding all non-merged LCAs as zero outcomes would conflate non-participation after LCA filing with lottery non-selection and understate the winning rate. Our baseline measure treats non-merged LCAs as missing outcomes. Section C. instead counts every unmatched LCA as a lottery loss. The estimates are close to the baseline and remain statistically significant.

A related issue is that employers may submit a single LCA covering multiple workers, or multiple LCAs for the same worker, so that LCA filings do not map one-to-one onto H-1B petitions. This practice, sometimes referred to as LCA bundling, would matter for our design only if the relationship between LCAs and petitions changed differentially for STEM and non-STEM occupations around the 2016 reform. We test for such a change in Section C. and find no evidence of one.

Figure 2 illustrates the constructed lottery winning rates for STEM and non-STEM occupations. Because there were far fewer LCA records in 2020, likely due to the pandemic,[7] we focus on the trend from 2014 to 2019. Over this period, the average constructed lottery winning rate was 26 percent for non-STEM occupations and 48 percent for STEM occupations. The magnitudes are broadly consistent with published cap-subject selection rates from the Department of Homeland Security (DHS) (U.S. Department of Homeland Security, 2018).[8] They are also consistent with the findings of Mahajan et al. (2024), who argue that H-1B lottery winning rates calculated using LCAs are slightly lower than those based on I-129 application data, which is not publicly available from USCIS.[9]

[7]The number of LCAs in 2020 was less than one-third of the annual average recorded between 2014 and 2019, so we exclude 2020 from the aggregate lottery analysis. The individual-level analysis retains 2020 because I-129 petition records do not show a comparable disruption; adjudication of petitions filed in prior years continued through 2020. Results from the individual-level analysis are qualitatively similar when 2020 is excluded.

[8]DHS reported an overall selection rate of 46 percent for H-1B cap-subject petitions from fiscal years 2014 to 2017.

[9]Because I-129 application data are not publicly available from USCIS, LCA data are conventionally used

B. Individual-Level Analysis

For the individual-level analysis, we use I-129 records to examine petition processing and labor market outcomes among workers who reach the H-1B petition stage. Unlike the aggregate lottery analysis, this sample includes both cap-subject and cap-exempt petitions, allowing us to study adjudication and employment outcomes across the broader OPT-to-H-1B pathway.

We restrict the individual-level analysis to fiscal years 2014–2020. Earlier years are excluded because successive updates to the list of STEM-eligible CIP codes during the early 2010s may have affected major choice and STEM classification in ways unrelated to the 2016 reform. Focusing on 2014–2020 separates the 2016 expansion in OPT duration from earlier changes in STEM eligibility. During this window, STEM eligibility was stable: no additional fields gained STEM designation until the next expansion of the STEM list in January 2022.

Appendix Table A1 reports summary statistics for the merged occupation-year LCA and I-129 data used in the aggregate analysis. Appendix Table A2 details the I-129 petition records used to construct the individual-level estimation samples.

Table 1 reports differences in petition adjudication and labor market outcomes between STEM and non-STEM applicants. On average, STEM cases take longer to process. STEM applicants are more likely to be employed in the private sector and to remain with the same employer, and they earn higher salaries. These unconditional differences do not account for individual characteristics and should not be interpreted as effects of the reform. The next section presents the empirical framework, which controls for these characteristics and estimates differential post-reform changes between STEM F-1 applicants and comparison-group applicants.

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to construct H-1B lottery winning rates. Several studies compile such rates at the firm and city level, including Peri et al. (2015), Dimmock et al. (2022), Glennon (2024), and Mahajan et al. (2024). See Appendix Figure A1 for lottery winning rates by 2-digit SOC occupation categories.

V. Methodology

A. OPT Extension Effects on Lottery Winning Rates

We begin by examining aggregate changes in H-1B application attempts and progression through the annual cap-subject lottery. This analysis tests the first prediction of Section III. —namely, that the extension raised LCA filings and the share of LCAs reaching the petition stage, and places the individual-level results in context.

Using the occupation-year data described above, we define the lottery winning rate as the number of I-129 petitions associated with cap-subject filings divided by the number of LCA filings. This constructed measure captures progression from LCA filing to the I-129 petition stage and is distinct from the individual petition approval rate examined below, which measures the probability that an I-129 petition is approved conditional on reaching the adjudication stage. Following the literature, we refer to this ratio as the lottery winning rate.[10] We estimate the following difference-in-differences (DiD) specification:

$$Y_{jt} = \alpha + \beta \text{treat}_{jt} + \gamma_1 U_{jt} + X_{jt}\gamma_2 + \delta_j + \delta_t + \varepsilon_{jt} \tag{1}$$

where Y_{jt} denotes the lottery winning rate for occupation j in fiscal year t. The treatment indicator equals 1 for STEM occupations in the post-policy period, fiscal years 2017–2019, and 0 otherwise.[11] The model includes occupation fixed effects (δ_j), year fixed effects (δ_t), occupation-specific unemployment rates (U_{jt}), and a vector of occupation-year covariates (X_{jt}) capturing education, nationality composition, industry distribution, and firm size. The coefficient β captures the differential post-reform change in the lottery winning rate for STEM relative to non-STEM occupations. Standard errors are clustered by SOC occupation to allow for correlation across fiscal years within occupations.

[10] See Section III. for how this per-cycle measure relates to cumulative lottery access.

[11] STEM eligibility for this aggregate analysis relies on the 2010 Census STEM and STEM-related Occupation Code List, consistent with the information available in LCA records.

Because selection within a given cap-subject lottery is random, the reform cannot change the probability that a filed petition is selected in that lottery. It can change the ratio by raising the share of LCAs that proceed to a filed petition within a given cycle, since more authorized time leaves fewer cases to drop out before filing. The coefficient β captures this margin. The increase in cumulative access across cycles is reflected more directly in the number of LCA filings, which we examine as a separate outcome using the same specification.

To assess pre-policy comparability and examine dynamic effects, we estimate the following event-study specification:

$$Y_{jt} = \alpha + \sum_{\substack{k=2014 \\ k\neq 2016}}^{2019} \beta_k(\text{STEM}_j \times 1(Year = k)) + \gamma_1 U_{jt} + X_{jt}\gamma_2 + \delta_j + \delta_t + \varepsilon_{jt} \tag{2}$$

where the β_k coefficients trace the evolution of relative lottery winning rates for STEM versus non-STEM occupations before and after the reform, with 2016 omitted as the reference year.

B. OPT Extension Effects on Adjudication and Employment

Having established aggregate changes in lottery winning rates, we turn to the individual-level analysis using I-129 administrative records. This analysis examines how the OPT extension affected petition adjudication and employment outcomes among individuals observed in the H-1B application pipeline. It speaks to the second and third predictions of Section III.: longer authorized employment should strengthen the worker–employer match, while adjudication frictions work against it.

Selection into the lottery is only the first stage of the H-1B process. Conditional on selection, workers face varying approval probabilities and processing times, both of which may influence job transitions and early-career outcomes. Because adjudication occurred during a period of heightened scrutiny, including increased use of Requests for Evidence (RFEs), approval and processing time may reflect both the OPT reform and changes in the adjudication environment. For this reason, we do not control for RFEs in the main

specification and examine their role separately in Section VIII.. To estimate differential post-reform changes in individual-level outcomes, we use the following DiD specification:

$$Y_{imt} = \alpha + \beta \text{treat}_{imt} + X_i\gamma + \delta_m + \delta_t + \varepsilon_{imt} \tag{3}$$

where Y_{imt} represents one of several outcomes for applicant i, in major m, in fiscal year t: petition approval, processing time, private-sector employment, employment continuity with the current employer, and earnings. Processing time is measured in days as the difference between the case receipt date and the adjudication action date recorded in the I-129 data. Although the treatment indicator is defined at the fiscal-year level, reflecting the reform's operation through annual academic and H-1B filing cycles rather than as an immediate within-month shock, the processing time measure itself captures fine-grained variation in adjudication timing.

The treatment indicator equals 1 for STEM majors in the post-policy period, defined as fiscal years 2017–2020. We define fiscal year 2017 as the first post-reform period because the 2016 rule was finalized in May 2016, after the April 2016 H-1B filing window had already closed. STEM graduates already working under the old 29-month OPT authorization could not have adjusted their behavior, by remaining in the pipeline for additional lottery cycles, until the April 2017 filing window at the earliest. Administrative processing lags and employer compliance timelines reinforce this choice: sponsors filing LCAs and I-129 petitions on behalf of OPT workers would not have fully incorporated the new 36-month horizon into hiring and sponsorship decisions until the 2016–17 academic and filing cycle. The event-study estimates provide further support, showing no differential movement between STEM and non-STEM applicants in 2016 relative to the pre-reform period.[12]

The vector X_i includes demographic characteristics such as gender, degree level, and country of birth. The model includes major fixed effects (δ_m) and year fixed effects (δ_t). The

[12]Specifications that define the post-period as beginning in 2016 yield attenuated estimates for petition approval, as expected given that 2016 was only a partial treatment year.

coefficient β captures the differential post-reform change in outcomes for STEM relative to non-STEM applicants observed at the I-129 stage. Because the two forces in Section III. push in opposite directions, β captures their combined effect, and its sign for the employment outcomes is an empirical question.

To evaluate pre-policy comparability for each outcome, we estimate event-study versions of the model:

$$Y_{imt} = \alpha + \sum_{\substack{k=2014 \\ k\neq 2016}}^{2020} \beta_k(\text{STEM}_m \times 1(Year = k)) + X_i\gamma + \delta_m + \delta_t + \varepsilon_{imt} \tag{4}$$

These coefficients trace the evolution of differences between STEM and non-STEM applicants before and after the policy change for each outcome.

Finally, we assess whether the patterns in the data are consistent with the two forces described in Section III.: the additional time under lawful employment created by the longer OPT period, and the administrative frictions associated with adjudication. Section VIII. describes this analysis.

C. Threats to the Estimates

Our estimates face three threats: selection into STEM, contemporaneous shocks that differed by STEM status, and measurement. The first two bear on the identifying assumption that, absent the extension, outcomes for STEM and non-STEM graduates would have evolved similarly. That assumption does not require the two groups to be alike in levels. The third concerns how we measure outcomes and STEM status. We address each threat in Section VII..

Selection can arise in two ways. Earlier OPT reforms affected field-of-study choices, so pre-existing selection could generate different trends for STEM and non-STEM graduates. The 2016 extension itself may have encouraged some students to choose or remain in STEM fields, changing the composition of STEM applicants after 2016. Such sorting is part of

the policy's total effect, but it complicates a comparison that relies only on STEM and non-STEM F-1 graduates. We address selection with a stable STEM list, composition tests, event studies, reweighting on pre-reform characteristics, and within-STEM comparisons with J-1 and TN/E-3 visa holders.

The second threat is contemporaneous shocks. Labor demand for STEM workers may have shifted after 2016 for reasons unrelated to OPT, and H-1B adjudication tightened over the same period. We address labor demand with occupation-specific unemployment trends, industry and state fixed effects, and a Bartik-type control. We address adjudication tightening by restricting the sample to the years before RFE intensity peaked.

The third threat is measurement, which arises in both analyses. The lottery winning rate depends on how LCAs are matched to petitions and on employers' LCA filing practices, and inference for it relies on a limited number of occupation clusters. STEM status in the individual analysis depends on the STEM list used and on how mixed-field degrees are classified. We recode unmatched LCAs as lottery losses, test for differential changes in the ratio of petitions to LCA-covered workers, and use wild cluster bootstrap inference. We also re-estimate the individual-level results under the 2016 DHS STEM list and with mixed-field degrees reassigned to the comparison group.

VI. Main Findings

The results follow graduates through the stages the reform targets, starting with access to the petition stage, moving to adjudication, and ending with employment and earnings.

A. Access to the Petition Stage

The first question is whether more time helped STEM graduates reach the petition stage, the first prediction of Section III.. Table 2 reports estimates from Equation 1, which compares the change in the lottery winning rate for STEM and non-STEM applicants. The estimate

is positive and statistically significant in every specification, and its size turns on a single control. Without covariates, the rate rises by 5.7 percentage points, significant at the 10 percent level (column 1). Adding education, nationality, region, firm size, or the occupation-specific unemployment trend one at a time leaves it between 5.6 and 7.1 points, significant at the 10 percent level in each case except the specification with regional controls, which reaches the 5 percent level (columns 2–5 and 7). Industry shares are different: they raise the estimate to 11.5 points on their own (column 6) and to 11.1 points in the full specification (column 8), which is our preferred one. That estimate implies a 31 percent increase relative to the sample mean of 35 percent and is significant at the 1 percent level.

Industry matters because conversion from LCA filing to petition differs across industries. Firms in some of them, IT staffing for example, file many LCAs per petition, so a shift in the industry mix of an occupation's filings can move the ratio even if nothing else changes. The larger estimate in column 6 is consistent with STEM filings moving after 2016 toward industries with lower conversion rates. The complication is that industry shares are measured from I-129 records and could themselves respond to the reform. Fixing them at their 2014–2016 occupation averages does not resolve this, because occupation fixed effects absorb them.[13] We treat the estimate without industry controls as the lower end of the range. If the post-2016 shift toward low-conversion industries is unrelated to the reform, that estimate understates the effect; even so, it is positive and statistically significant.

The timing of the increase fits the mechanics of the reform. Figure 3 reports event-study estimates from Equation 2. The pre-reform coefficients show no differential trends, and after 2016 the winning rate for STEM applicants rises, with the gap widening over the post-reform period. A gradual phase-in would produce exactly this pattern: in the first post-reform cycle, only graduates near the end of their original authorization could use the extra time for another filing, whereas later cycles included more cohorts whose lottery access

[13] With industry shares fixed at their 2014–2016 occupation averages, the specification in column 6 of Table 2 reproduces the estimate without controls in column 1, and the full specification reduces to one without industry controls, yielding 7.7 percentage points (standard error 3.2), significant at the 5 percent level.

reflected the longer authorization from the start.

More time should also raise the number of attempts, not only the share that reach the petition stage. Appendix Table A3 shows that STEM LCA filings rose by 0.26 log points relative to non-STEM filings, or about 30 percent, as graduates who remain in the pipeline longer file in an additional cycle. The reform expanded both application attempts and the share of attempts reaching the petition stage, as the first prediction implies. Section VII. examines whether selection, contemporaneous shocks, or measurement could account for these results.

B. Petition Approval Rates and Processing Times

Reaching the petition stage is only part of the transition. A selected petition must still be approved, and the reform coincided with tighter adjudication. The third prediction of Section III. is that such frictions lower approval and lengthen processing, most for cap-subject petitions. Table 3 reports approval outcomes for the individual-level sample. The pooled estimate is negative in every specification but loses significance once field-of-study and year fixed effects are included, and the decline that survives the full set of controls is concentrated among Master's graduates, whose approval rate falls by 1.5 percentage points in the most complete specification, or 2.1 percent relative to a mean of 69 percent. The event study in Figure 4 places this decline in the years of strictest adjudication. There are no differential pre-reform trends; for Master's graduates the gap opens in 2017 and 2018, at roughly 3 to 5 percentage points, then narrows in 2019 and disappears by 2020 as approval rates recovered. STEM Master's applicants gained access to the lottery but faced lower relative approval while the adjudication environment was most restrictive.

Adjudication also slowed. Table 4 reports the number of days between petition receipt and adjudication among approved cases. For the full sample, processing times rose by 7.3 to 9.1 days, or 8 to 10 percent of the 95.5-day mean,[14] and the delays fell on advanced-

[14]Appendix Table A4 confirms that processing times also rose for denied and revoked cases, though smaller

degree applicants. For Master's graduates, processing times rose by 7.7 to 9.3 days, or 8 to 9 percent of a 101.9-day mean; for PhD applicants, by 12.6 to 13.4 days, or 24 to 26 percent of a 51.8-day mean. The event-study evidence is cleaner for PhD graduates. In Figure 5, their processing times show no differential pre-reform trend, rise sharply after 2016, and remain elevated through the end of the sample period, matching the DiD estimates. Master's processing times also rise after 2016, but the joint test of the pre-reform coefficients rejects parallel trends ($p = 0.0414$; Appendix Table A5), so that estimate should be read with more caution.

In sum, greater access to the lottery did not make the rest of the transition easier. Approval fell for STEM Master's applicants and processing times rose, most clearly for PhD applicants, consistent with the third prediction. Section VIII. relates these patterns to the cap-subject route and to PhD moves out of cap-exempt positions.

C. Employment, Continuity, and Earnings

The extra time also changed where graduates worked and whether they stayed with their employers. The second prediction of Section III. is that longer authorization raises the value of the match, with the channel depending on where each degree group starts, and the responses differ by degree in just that way. Table 5 shows a modest relative increase in private-sector employment among STEM applicants, 0.65 percentage points from a base of 95 percent in the preferred specification. The increase is concentrated among PhD recipients, whose private-sector employment rises by 3.41 percentage points, or 4.9 percent relative to a mean of 70 percent. Estimates for Bachelor's and Master's graduates are much smaller because most of them already worked in the private sector before the reform, whereas PhD recipients more often started in universities and nonprofit research organizations, and the additional OPT time made it more feasible for them to wait for another lottery cycle and move into cap-subject private-sector employment. Figure 6 shows no differential pre-reform

sample sizes required pooling across degree levels.

trend and a clear post-2016 increase for PhD recipients. The response peaks in 2018 and then attenuates, consistent with a front-loaded move out of academic and other cap-exempt positions that had largely run its course by the end of the period, with the COVID-19 disruption to hiring adding to the decline in 2020.

Master's graduates responded differently: they stayed with their employers. We measure continuity with petitions that signal ongoing or concurrent employment.[15] Table 6 reports the estimates.[16] Continuity rises by 6.01 percentage points in the full sample and by 8.18 points among Master's graduates, a 10.2 percent increase relative to a mean of 80 percent. This is the group that relies most on cap-subject employment and the annual lottery, so extra authorized time is especially valuable to them for keeping a worker–employer match intact between filing cycles. Figure 7 shows the increase emerging after 2016 and persisting through the post-reform period.

Earnings follow the same degree pattern as private-sector employment. Table 7 shows no statistically significant change in annual salaries for the full sample or for Bachelor's and Master's graduates, but for PhD recipients salaries rise by 5.4 to 6.1 log points, about 5.5 to 6.3 percent. Figure 8 shows a gradual and persistent rise after the reform, and the joint test does not reject parallel pre-reform trends for this group (Appendix Table A5). The gains occurred in both cap-exempt and non-exempt sectors (Appendix Table A6), so they are not only a by-product of the move into the private sector. One caveat applies to all the salary estimates: salaries in the I-129 records are the prevailing wages reported with the LCA, not actual pay, so the estimates measure changes in reported prevailing wages. The distinction

[15]The continuity measure includes (i) *amended petition*, which indicates that the requested worker(s) will be continuing employment with the same employer with a material change to job duties; (ii) *change previous employment*, which indicates continuation with the same employer without a material change to job duties; (iii) *continued employment*, which indicates ongoing employment with the same employer; and (iv) *new concurrent employment*, which indicates that the requested worker(s) will begin employment with an additional employer. Because concurrent employment involves an additional employer, we interpret this outcome as petition-level employment attachment rather than literal same-firm retention in every case.

[16]This sample is considerably smaller than the full estimation sample, 4,747 observations overall, reflecting the relatively rare occurrence of amended, continued, or concurrent employment petitions in the I-129 records (Appendix Table A2). Results for Bachelor's and PhD graduates should be interpreted cautiously given limited statistical power.

matters most in private-sector STEM jobs, where pay can exceed the prevailing wage.

The reform's effects differ along the pathway, as the framework anticipates. More time widened access: STEM graduates filed more LCAs, and a larger share reached the petition stage. What happened next depended on adjudication and on degree. Master's graduates faced lower approval rates during the strictest years but remained attached to their employers more often, while PhD recipients faced longer processing times but moved into private-sector jobs and reported higher wages.

VII. Robustness Checks

We now return to the three threats raised in Section C.. For each, we discuss the aggregate evidence first and the individual-level evidence second, where both apply.

A. Selection into STEM

The first concern is that STEM and non-STEM graduates differ in ways that affect outcomes. They may have been on different paths before the reform, or the reform itself may have changed who applies after 2016. We take up both possibilities in turn.

We start with the definition of the treated group. As described in Section B., no fields gained STEM designation between 2014 and 2020. The reform changed how long STEM graduates could work, not which fields qualified, so the treated group is made up of the same fields before and after 2016.

The same fields can still attract different applicants, so we next ask whether the STEM applicant pool changed after the reform. Appendix Figure A2 and Appendix Table A7 track degree and nationality composition by year. The share of applicants from India or China does not change differentially ($p = 0.5018$). The share with a bachelor's degree does ($p = 0.0043$), but it moves back and forth, from negative in 2018 to positive in 2020, and this shift does not drive our results: controlling for degree composition leaves the aggregate estimate unchanged

(Table 2, column 2), and we estimate the individual-level models separately by degree. Nor can growth in the overall number of international students explain the estimates, since year fixed effects absorb changes common to STEM and non-STEM applicants.

Composition tests cover only what we observe. To look for unobserved differences, we check whether outcomes were already diverging before the reform. For the lottery winning rate, approval, private-sector employment, and employment continuity, the event studies show no such divergence (Figures 3, 4, 6, and 7). Joint tests of the pre-reform coefficients reject in three of twenty outcome-by-degree cells (Appendix Table A5). Two involve salaries, in the pooled and Master's samples, where we find no significant earnings effect to begin with. The third is Master's processing time, which we treat with caution in Section VI..[17]

We also reweight the sample so that STEM and non-STEM applicants look alike before the reform. Appendix Table A8 uses inverse probability weights based on country of origin, degree level, and 2-digit industry. Once weighted, the two groups are balanced on these characteristics (Appendix Table A9), and the estimates remain broadly consistent with the main findings.

Each of these checks still compares STEM and non-STEM F-1 graduates. Our last check drops non-STEM graduates entirely. Appendix Tables A10 and A11 keep only STEM petitions and compare F-1 applicants with J-1 and TN/E-3 visa holders, none of whom were eligible for the OPT extension. Because everyone in these samples is in STEM, selection into STEM cannot drive the comparison. These comparison groups are selected in their own ways, but for reasons unrelated to F-1 students' choice of field: J-1 holders come through exchange, research, and sponsoring-institution arrangements, while TN and E-3 eligibility depends on nationality and is tied to a specific job.

Both comparisons yield results close to the baseline. Relative to J-1 holders, F-1 applicants face lower approval rates and substantially longer processing times after the reform,

[17] With only two pre-reform years, a conventional placebo reform test has limited power. We rely on the event-study specification to assess pre-reform parallel trends.

and the delays extend to Master's graduates. The PhD results also hold up: processing times rise, and so do private-sector employment, continuity, and earnings. The comparison with TN/E-3 holders shows the same adjudication pattern, with processing delays now visible among Bachelor's and Master's graduates as well, and PhD recipients again moving into private-sector jobs. Some estimates are significant in one comparison but not the other, which likely reflects differences in the size and composition of the J-1 and TN/E-3 samples. Still, the baseline and the two alternative comparisons rely on different groups, each selected in its own way, and all three yield similar patterns. This makes it unlikely that selection into STEM drives our main findings.

B. Contemporaneous Shocks

Even if selection is not a concern, something else may have changed after 2016 that affected STEM graduates differently. The two obvious candidates are labor demand and the tightening of H-1B adjudication.

Demand for STEM workers may have grown faster after 2016 for reasons that had nothing to do with OPT. In the aggregate analysis, the occupation-specific unemployment trend picks up part of this variation. Adding it alone gives an estimate of 6.3 percentage points, close to the 5.7 without controls (Table 2, column 7).

In the individual-level analysis, we add industry and state fixed effects (Appendix Table A12).[18] The adjudication and continuity estimates barely move. The private-sector employment and salary estimates shrink and lose significance, which is what we would expect: part of what longer work authorization does is let graduates change sectors, especially PhD recipients leaving universities and nonprofit research organizations for private-sector

[18] Employer fixed effects would absorb time-invariant firm-level heterogeneity, but they are not feasible in our data. The sample includes 108,250 distinct employers, many of which appear only once. Singleton firms are dropped when employer fixed effects are included, which substantially reduces the estimation sample and statistical power. Even among repeat employers, within-firm variation in STEM hiring over the short panel is limited. The 2-digit NAICS industry and state fixed effects address broad industry-composition concerns while preserving sample size.

jobs, and controlling for the employer's industry after the reform absorbs part of that move, not just preexisting industry differences.

Fixed effects capture broad differences across industries and states, but demand could also have shifted within them. To get at this, we build a Bartik-style measure that predicts labor demand for each major from pre-reform industry employment shares and national industry employment growth, and we let its relationship with the outcomes differ by STEM status and after the reform (Appendix Table A13).[19] If faster demand growth were behind our results, STEM applicants facing stronger predicted demand should show larger changes after the reform. That is not what we find: the triple interaction is statistically insignificant for every outcome, in the pooled sample and in each degree group.

We also ask whether predicted demand is related to STEM outcomes at any point, before or after the reform, using joint tests of the two Bartik interactions (Appendix Table A14). For the employment and earnings outcomes, we find no relationship in any degree group. The tests do reject for processing time among Master's and PhD applicants ($p = 0.0011$ and $p = 0.0008$), and marginally for Master's approval ($p = 0.0914$). For processing time, however, the two coefficients are individually insignificant and offset each other (Appendix Table A13): predicted demand is positively associated with STEM processing times before the reform, and the association disappears afterward. If anything, stronger predicted demand goes with smaller post-reform delays. Labor demand does not explain the rise in processing times, nor the employment and earnings results.

The second change is the tightening of H-1B adjudication. Processing times began to lengthen in 2017, and RFE intensity rose sharply in 2018. If our adjudication results simply

[19] The Bartik measure is constructed using ACS data as $\sum_j s_{mj}^{2014-2016} g_{jt}$, where $s_{mj}^{2014-2016}$ is the share of workers in major m employed in industry j during the base period 2014–2016, and g_{jt} is the national employment growth rate in industry j at time t. The specification includes the full set of interactions. Because the measure varies only by major and year, its level and its interaction with the post-reform indicator are absorbed by the CIP and fiscal-year fixed effects. The estimated terms include STEM × Bartik, STEM × Post, and STEM × Bartik × Post. Because the Bartik measure is not centered, the STEM × Post coefficient is evaluated at a Bartik value of zero, near the lower bound of the support (mean 22.3, standard deviation 29.6, range −2.0 to 85.1). It is not directly comparable to the baseline difference-in-differences estimate.

reflected the RFE surge, they should not appear before it. Appendix Table A15 restricts the sample to 2015–2017.[20] Even in this window, approval falls for Master's applicants and processing times rise for Master's and PhD petitions. The adjudication patterns were already present before the surge. Section VIII. looks at the adjudication environment in more detail.

C. Measurement and Inference

Finally, we examine whether the results depend on how we measure outcomes and treatment. We run three checks on the aggregate analysis and two on the individual-level analysis.

We first vary how we treat unmatched LCAs. The lottery winning rate depends on linking LCAs to petitions, and not every LCA can be matched. Our baseline treats unmatched cells as missing; Appendix Table A16 instead counts every unmatched LCA as a lottery loss. The estimates are 7.3 percentage points without controls and 5.8 with the occupation-specific unemployment trend, close to the baseline estimates of 5.7 and 6.3 and statistically significant. The remaining covariates come from I-129 records, which do not exist for unmatched cells, so this check covers only the specifications without I-129 controls.

Next, we examine employers' filing practices. The winning rate could move if employers changed how many workers they list on each LCA. We track the ratio of I-129 petitions to LCA-covered workers by STEM status and test for a differential shift after the reform. The ratio evolves similarly for STEM and non-STEM occupations (Appendix Figure A4), and the difference-in-differences estimate is small and statistically indistinguishable from zero (Appendix Table A17).[21] Changes in how employers use LCAs do not explain the rise in the lottery winning rate.

Finally, we turn to inference. The aggregate analysis relies on a limited number of occupation clusters. Wild cluster bootstrap p-values are below 0.10 in every specification and

[20] Information on RFEs published by USCIS is available from 2015 to 2020, as documented in Appendix Figure A3. The sample is restricted to 2015–2017, when RFE rates were available and remained stable across the three years (0.22, 0.21, and 0.21, respectively).

[21] The test relies on a small number of STEM-by-year and non-STEM-by-year observations.

equal 0.002 in the preferred one (Appendix Table A18). Nor do computer and mathematical occupations drive the result. Without them, the preferred estimate is 10.4 percentage points and significant at the 1 percent level, while the estimates without industry controls keep their size (5.1 to 6.5 points) but lose precision, and only the specification with regional controls stays significant.

In the individual-level analysis, both checks focus on how we assign STEM status. First, Appendix Table A19 re-estimates the main specifications with the DHS list of STEM majors as updated in 2016, the list in force throughout the post-reform period.[22] The results look much like the baseline, with point estimates close to the original ones; only the decline in Master's approval and the small Master's private-sector gain lose significance.

Second, we reclassify fields of study that combine STEM and non-STEM components. In the main analysis, we count an individual as STEM-eligible if the field description includes a STEM field, so a student listed as majoring in "Engineering and Law" is classified as STEM. Appendix Table A20 moves these mixed-field cases to the non-STEM comparison group instead, and the findings are largely unchanged.

Altogether, these checks leave the main results in place. Neither selection into STEM nor the labor-demand and adjudication changes of the period account for them, with one qualification: once we control for employer industry, part of the PhD gains in private-sector employment and earnings is absorbed, because moving across industries is part of how those gains arise. The way we build the lottery winning rate, conduct inference, or define STEM status does not change the picture either. The evidence is strongest for wider access to the H-1B pipeline and for employment continuity among Master's graduates, the two results

[22] The DHS STEM Designated Degree Program List identifies the fields of study that the U.S. Department of Homeland Security designates as science, technology, engineering, and mathematics (STEM). The list was updated in 2011, 2012, and 2016. The main analysis uses the 2011 list. Fields added in 2012 account for less than 2 percent of the sample. The 2016 list classifies them as STEM. Because the 2016 DHS STEM list does not always provide sufficient detail to classify fields at the 6-digit CIP level, with some fields listed only at the 2-digit level (e.g., 14 Engineering, 26 Biological and Biomedical Sciences, 27 Mathematics and Statistics, and 40 Physical Sciences), we refer back to the 2011 list for 6-digit CIP codes that cannot be separately identified from the 2016 list.

that hold in every specification.

VIII. Mechanisms

The main results raise a question. If the extension widened access to the H-1B pipeline, why did approval fall for STEM Master's applicants and processing slow, most clearly for PhD cases? And why did the gains take different forms by degree, with continuity for Master's graduates and private-sector moves and higher pay for PhDs? We look at two features of the period that help answer these questions: the adjudication environment the reform ran into, and the extra time the reform created between temporary work authorization and H-1B adjudication. The evidence in this section is descriptive rather than causal. It asks whether the patterns in the data are consistent with the framework in Section III..

A. Administrative Frictions and Adjudication Dynamics

The first feature is the adjudication environment the reform ran into. The extension took effect just as H-1B scrutiny was rising. Appendix Figure A3 shows that RFE issuance rates were roughly stable through 2017, rose sharply in 2018, and peaked in 2019.[23] Approval probabilities fell over the same years, reaching their lowest point in 2018.

The tightening did not fall evenly. It hit cap-subject petitions, the very pathway the extension was meant to help, since extra OPT time matters mainly by allowing graduates to enter more H-1B lottery cycles. Appendix Table A21 compares cap-subject and cap-exempt petitions. Cap-subject petitions had lower approval rates and longer processing times in every year before 2020, and both gaps widened sharply after 2016. In 2017, cap-subject cases took 137 days on average, against 24 days for cap-exempt cases, and the approval gap rose from 8 percentage points in 2016 to 20 in 2017, staying above 10 through 2019 before

[23]The figure is generated using official RFE data published by USCIS for the period from 2015 to 2020. See *USCIS I-129 Petition for a Nonimmigrant Worker, Specialty Occupations (H-1B), by Fiscal Year, Month, and Case Status: October 1, 2014–September 30, 2020* (U.S. Citizenship and Immigration Services, 2020).

closing in 2020.

Appendix Table A22 links annual RFE intensity to these outcomes. Official RFE data cover 2015–2020 and vary by year, so the estimates show how outcomes moved with the adjudication environment over that period. Higher RFE intensity goes with substantially longer processing times for cap-subject petitions. For STEM cap-subject petitions, it also goes with lower approval rates, although this differential is significant only at the 10 percent level, and the corresponding differential for processing time is imprecise. The extension, in short, took effect as the cap-subject route became more demanding.

The same environment shows up in labor-market outcomes. Appendix Table A23 relates national RFE and approved-RFE volumes to private-sector employment, employment continuity, and salary. Because these volumes vary only by fiscal year, they replace the fiscal-year fixed effects and may also pick up other national changes in the same year. Years with more RFEs are years with lower employment continuity, while years with more approved RFEs show higher continuity, particularly among Master's graduates. Appendix Figure A5 adds that post-2017 earnings gains are more visible among approved petitions in several STEM-intensive industries. Labor-market outcomes moved with the adjudication environment and with how petitions fared in it.

B. Extended Work Authorization and the H-1B Transition

The second feature is the extra time the reform created between temporary work authorization and H-1B adjudication. Extending STEM OPT from 29 to 36 months gave graduates more room to stay employed while pursuing H-1B sponsorship and, for some, another lottery cycle. To capture time spent in the transition, we link LCA and I-129 records and count the days between the LCA received date and the I-129 action date. Because LCA received dates are published only from 2017, we observe this interval in the post-reform years alone.[24]

[24]The proxy is constructed by linking LCA and I-129 records through the LCA case number and measuring the number of days between the LCA received date and the I-129 action date. The LCA received date need not coincide with the start of OPT employment, and the I-129 action date need not coincide with

STEM graduates spend longer in this transition than non-STEM graduates (Appendix Table A24). The gap is largest for PhD recipients, about 53 days in 2018 and 55 days in 2019, and is also significant for Master's graduates from 2017 to 2019 and for Bachelor's graduates in 2017 and 2018. By 2020, the gap narrows for Bachelor's and Master's graduates but remains significant for PhD recipients. This is what we would expect if STEM graduates used the extra authorization to stay employed longer while moving toward H-1B status.

Longer transitions also go with better early-career outcomes (Appendix Table A25). A longer interval is associated with higher private-sector employment in the full sample and in every degree group, and with higher employment continuity among Master's graduates. For PhD recipients, a 10 percent longer interval is associated with roughly 0.8 percentage points higher private-sector employment and 0.5 percent higher reported wages; for Master's graduates, with roughly 0.2 percentage points higher continuity. The earnings associations differ by degree, positive for Bachelor's and PhD recipients and negative for Master's graduates.

Together, the two sets of evidence fit the framework in Section III.. The extension gave STEM graduates more time to stay employed and pursue sponsorship. STEM graduates did spend longer in the transition, and those longer transitions went with stronger employment outcomes. At the same time, the cap-subject route the extension relied on became harder to navigate, with more RFEs, lower approval rates, and longer waits. PhD recipients, who used the extra time to move from cap-exempt into cap-subject employment, met those frictions in the process, which fits their larger processing delays. That combination helps explain how wider access to the H-1B pipeline could coexist with greater adjudication frictions and with gains that differed by degree.

OPT expiration. The interval measures time within the transition rather than OPT use.

IX. Summary and Conclusions

We study the 2016 STEM OPT extension, which lengthened authorized employment for international STEM graduates from 29 to 36 months. The question is what a longer post-study work program delivers when the next stage, employer-sponsored status, is administered separately. Previous work on OPT examines enrollment and employment outcomes. We follow graduates through the H-1B pipeline itself. Linking Department of Labor LCAs to DHS I-129 petitions, we observe each stage of the move from OPT to H-1B status: sponsorship attempts, lottery access, adjudication, and early-career employment. Our difference-in-differences design compares STEM and non-STEM graduates before and after the reform.

More time opened access to the H-1B pipeline. The share of STEM LCAs reaching the petition stage, known in the literature as the lottery winning rate, rose by 11.1 percentage points in our preferred specification, or about 31 percent relative to the mean. Even our most conservative estimate, 5.7 percentage points without industry controls, is positive and significant at the 10 percent level. STEM LCA filings also rose by about 30 percent relative to non-STEM filings. The extra seven months let graduates stay in the pipeline longer and file in an additional cycle. At the selection rates of the period, one more cycle raises the chance of being selected at least once from under one half to about seven in ten.

Moving through the pipeline became harder. The reform coincided with tighter H-1B adjudication. Processing times began to lengthen in 2017, and RFEs rose sharply in 2018. After the reform, processing times rose by 8 to 9 percent for STEM Master's applicants and by about 25 percent for PhD applicants, and approval rates for STEM Master's applicants fell by 1.5 percentage points. The tightening was concentrated among cap-subject petitions, the route the extension was meant to help.

Graduates still gained in the labor market, and the gains followed where each group started. Master's graduates, who rely most on the annual lottery, were more likely to stay attached to their employers, with the continuity measure rising by about 8 percentage points. PhD recipients, who more often begin in cap-exempt academic positions, moved into private-

sector jobs and saw reported wages rise by 5 to 6 percent, a pattern consistent with a move out of cap-exempt positions into the cap-subject route.

These results hold up to a wide range of checks. Pre-reform trends are flat for the lottery winning rate, approval, private-sector employment, and employment continuity. The estimates survive reweighting on pre-reform characteristics, controls for labor demand, and alternative definitions of STEM status and of the lottery winning rate. The adjudication patterns appear before the RFE surge. When we drop non-STEM graduates and compare STEM F-1 graduates with STEM J-1 and TN/E-3 visa holders, who were not covered by the extension, the main patterns persist. The evidence is strongest for wider access to the H-1B pipeline and for employment continuity among Master's graduates, the two results that hold in every specification. Two limitations remain. Salaries in the I-129 records are prevailing wages rather than actual pay, and the pre-reform window is short. Our data also cover graduates who entered the H-1B pipeline, not those who left the United States or stayed on OPT without seeking sponsorship, so the estimates speak to outcomes within the pipeline rather than to retention overall.

Two forces explain these results. More time kept graduates employed, preserved worker–employer matches, and added chances to enter the lottery. Tighter adjudication added delay and uncertainty to the final step and absorbed part of what the extra time created. Descriptive evidence on RFE intensity, on cap-subject and cap-exempt petitions, and on time elapsed between LCA filing and adjudication is consistent with both. STEM graduates spent longer in the transition after the reform, and longer transitions went with better employment outcomes, while the cap-subject route they relied on became slower and less certain. Temporary work rights and the administration of longer-term status operate jointly. Judging the extension by its length alone misses this.

The same interaction is present in current policy. The 2025 H-1B changes make time in authorized employment more valuable, since wage-weighted selection lowers per-cycle odds for entry-level graduates and changes of status within the United States are exempt from

the new payment requirement. Our estimates suggest that the value of that time depends on what happens at adjudication: an extension that widens access can still deliver less if the next stage slows. Other countries face a similar situation as they lengthen post-study work rights while leaving the path to permanent status to a separate process. Post-study work programs should be judged both by how long they extend work rights and by how well the next stage functions.

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Table 1. Descriptive Statistics for I-129 Petitions, 2014–2020

	All		STEM		Non-STEM		Difference
	Mean	S.D.	Mean	S.D.	Mean	S.D.	(3) – (5)
Likelihood of I-129 Petition Approval	0.70	0.46	0.70	0.46	0.71	0.45	-0.012***
Processing Days for Approved Cases	95	93	96	93	94	91	2.188***
Employment in the Private Sector	0.95	0.21	0.96	0.19	0.92	0.27	0.043***
Employment Continuity	0.81	0.39	0.81	0.39	0.79	0.41	0.029**
Annual Salary (Deflated 2010 Dollar)	71,958	28,119	74,950	26,210	61,869	31,753	13,081***
Observations	389,195		299,536		89,659		

Notes: Authors' calculations are based on individual administrative records from U.S. Citizenship and Immigration Services Form I-129, obtained from the Department of Homeland Security through a Freedom of Information Act request. The data span 2014 to 2020. Salary is reported in constant 2010 dollars. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 2. Difference-in-Differences Results for Aggregated Occupation-Year Level Lottery Winning Rate

	H-1B Lottery Winning Rate							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treat	0.0574*	0.0578*	0.0625*	0.0708**	0.0562*	0.1147***	0.0627*	0.1110***
	(0.0316)	(0.0316)	(0.0315)	(0.0280)	(0.0317)	(0.0280)	(0.0333)	(0.0318)
Control for the share of group characteristics on the occupation by year level								
Education		✓						✓
Country (China, India, Other)			✓					✓
Region				✓				✓
Log firm size					✓			✓
Industry						✓		✓
Occupation-Specific Unemployment Trend							✓	✓
FY Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
SOC Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
N	145	145	145	145	145	145	145	145
Y Mean	0.35	0.35	0.35	0.35	0.35	0.35	0.35	0.35

Notes: The table presents estimates from the difference-in-differences model as specified in Equation 1. The sample period is from 2014 to 2019, with the post-policy period covering 2017 to 2019. STEM eligibility is specified according to the 2010 Census STEM and STEM-related Occupation Code List. The dependent variable is the H-1B lottery winning rate, calculated as $\frac{\text{I-129 petitions}}{\text{LCA filings}}$, using LCA and I-129 data that are collapsed and merged at the occupation-year level. Standard errors are clustered by SOC occupation (25 clusters). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 3. Difference-in-Differences Results for Approval Likelihood

	Likelihood of I-129 Petition Approval: All Cases		
	(1)	(2)	(3)
Panel A: All Degrees			
treat	-0.0186**	-0.0151	-0.0095
	(0.0082)	(0.0097)	(0.0080)
N	389,195	389,195	389,195
Y Mean	0.70	0.70	0.70
Panel B: Bachelor's Degree			
treat	0.0177	0.0166	0.0127
	(0.0196)	(0.0176)	(0.0182)
N	43,757	43,757	43,757
Y Mean	0.71	0.71	0.71
Panel C: Master's Degree			
treat	-0.0222**	-0.0186*	-0.0148*
	(0.0086)	(0.0096)	(0.0082)
N	300,602	300,602	300,602
Y Mean	0.69	0.69	0.69
Panel D: Doctorate Degree			
treat	0.0113	0.0093	0.0089
	(0.0101)	(0.0101)	(0.0099)
N	44,836	44,836	44,836
Y Mean	0.80	0.80	0.80
Field of Study FE		✓	✓
Fiscal Year FE		✓	✓
Individual Characteristics			✓

Notes: The table presents estimates from the difference-in-differences model as specified in Equation 3. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. The dependent variable is a binary variable, with 1 indicating approved status and 0 for other statuses (including denied, revoked, pending, and administrative closed cases). STEM eligibility is specified according to the Department of Homeland Security's 2011 STEM Designated Degree Program List. Specification (1) includes the full interaction between a STEM eligibility indicator and a post-policy period indicator. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 4. Difference-in-Differences for Processing Time

	Difference in Days between Action and Received Date: Approved Cases		
	(1)	(2)	(3)
Panel A: All Degrees			
treat	8.6245***	9.1258**	7.3426**
	(2.8636)	(3.8124)	(3.4022)
N	273,462	273,462	273,462
Y Mean	95.48	95.48	95.48
Panel B: Bachelor's Degree			
treat	-2.2831	1.2367	0.8052
	(6.1837)	(4.9470)	(4.7012)
N	31,064	31,064	31,064
Y Mean	103.26	103.26	103.26
Panel C: Master's Degree			
treat	7.7311***	9.2655***	8.6973***
	(2.1184)	(2.9752)	(2.5977)
N	206,584	206,584	206,584
Y Mean	101.88	101.88	101.88
Panel D: Doctorate Degree			
treat	12.5627***	13.4410***	13.1516***
	(3.1750)	(3.2636)	(3.2922)
N	35,814	35,814	35,814
Y Mean	51.81	51.81	51.81
Field of Study FE		✓	✓
Fiscal Year FE		✓	✓
Individual Characteristics			✓

Notes: The table presents estimates from the difference-in-differences model as specified in Equation 3. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. The dependent variable is a continuous variable, indicating difference in days between the act date (the date of the last adjudicative action indicating the current status) and the received date (the date on which USCIS received the petition). STEM eligibility is specified according to the Department of Homeland Security's 2011 STEM Designated Degree Program List. Specification (1) includes the full interaction between a STEM eligibility indicator and a post-policy period indicator. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 5. Difference-in-Differences for Private Sector Employment

	Employment in the Private Sector		
	(1)	(2)	(3)
Panel A: All Degrees			
treat	0.0203**	0.0130***	0.0065**
	(0.0080)	(0.0035)	(0.0031)
N	389,195	389,195	389,195
Y Mean	0.95	0.95	0.95
Panel B: Bachelor's Degree			
treat	0.0017	0.0006	0.0011
	(0.0034)	(0.0032)	(0.0029)
N	43,757	43,757	43,757
Y Mean	0.98	0.98	0.98
Panel C: Master's Degree			
treat	0.0012	0.0019	0.0029*
	(0.0028)	(0.0020)	(0.0017)
N	300,602	300,602	300,602
Y Mean	0.98	0.98	0.98
Panel D: Doctorate Degree			
treat	0.0311**	0.0347**	0.0341**
	(0.0152)	(0.0140)	(0.0145)
N	44,836	44,836	44,836
Y Mean	0.70	0.70	0.70
Field of Study FE		✓	✓
Fiscal Year FE		✓	✓
Individual Characteristics			✓

Notes: The table presents estimates from the difference-in-differences model as specified in Equation 3. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. The dependent variable is a binary variable, equal to 1 if the individual is employed in the private sector and 0 if employed in the exempt sector, such as a university or college. STEM eligibility is specified according to the Department of Homeland Security's 2011 STEM Designated Degree Program List. Specification (1) includes the full interaction between a STEM eligibility indicator and a post-policy period indicator. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 6. Difference-in-Differences for Employment Continuity

	Employment Continuity		
	(1)	(2)	(3)
Panel A: All Degrees			
treat	0.0333	0.0569**	0.0601**
	(0.0290)	(0.0246)	(0.0235)
N	4,747	4,747	4,747
Y Mean	0.81	0.81	0.81
Panel B: Bachelor's Degree			
treat	-0.0595	-0.0688	-0.1091
	(0.0572)	(0.0739)	(0.0698)
N	525	525	525
Y Mean	0.84	0.84	0.84
Panel C: Master's Degree			
treat	0.0476	0.0810***	0.0818***
	(0.0383)	(0.0272)	(0.0266)
N	3,764	3,764	3,764
Y Mean	0.80	0.80	0.80
Panel D: Doctorate Degree			
treat	0.0521	0.1053	0.0955
	(0.1166)	(0.1363)	(0.1423)
N	458	458	458
Y Mean	0.83	0.83	0.83
Field of Study FE		✓	✓
Fiscal Year FE		✓	✓
Individual Characteristics			✓

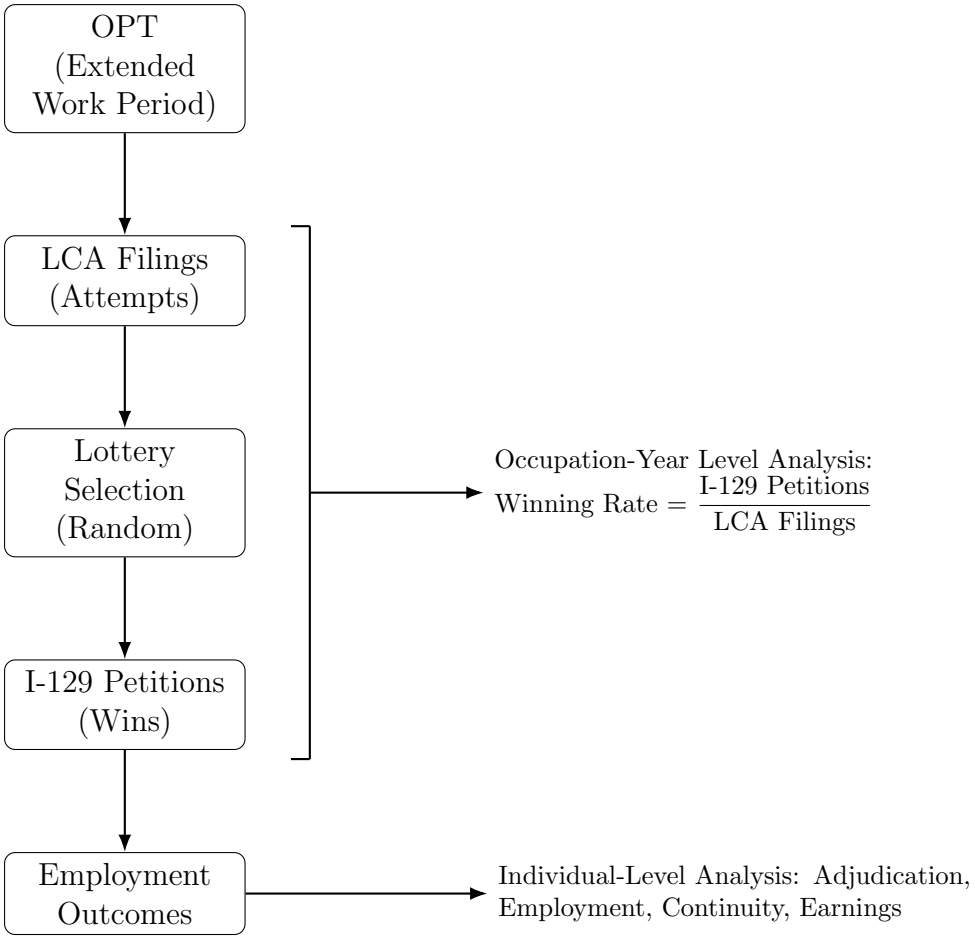
Notes: The table presents estimates from the difference-in-differences model as specified in Equation 3. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. The dependent variable is a petition-based employment-continuity indicator, equal to 1 for petitions classified as continued or concurrent employment and 0 for petitions classified as switching employers. The measure should not be read as a direct longitudinal worker-level retention measure. The sample includes individuals who are already employed. STEM eligibility is specified according to the Department of Homeland Security's 2011 STEM Designated Degree Program List. Specification (1) includes the full interaction between a STEM eligibility indicator and a post-policy period indicator. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at the 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 7. Difference-in-Differences for Annual Salary

	Log Annual Salary		
	(1)	(2)	(3)
Panel A: All Degrees			
treat	-0.0069	-0.0134	-0.0109
	(0.0255)	(0.0264)	(0.0265)
N	351,355	351,355	351,355
Y Mean	11.06	11.06	11.06
Panel B: Bachelor's Degree			
treat	0.0230	0.0090	0.0078
	(0.0411)	(0.0361)	(0.0355)
N	38,135	38,135	38,135
Y Mean	10.95	10.95	10.95
Panel C: Master's Degree			
treat	-0.0163	-0.0168	-0.0226
	(0.0282)	(0.0305)	(0.0311)
N	271,358	271,358	271,358
Y Mean	11.06	11.06	11.06
Panel D: Doctorate Degree			
treat	0.0610***	0.0563***	0.0536***
	(0.0229)	(0.0204)	(0.0202)
N	41,862	41,862	41,862
Y Mean	11.15	11.15	11.15
Field of Study FE		✓	✓
Fiscal Year FE		✓	✓
Individual Characteristics			✓

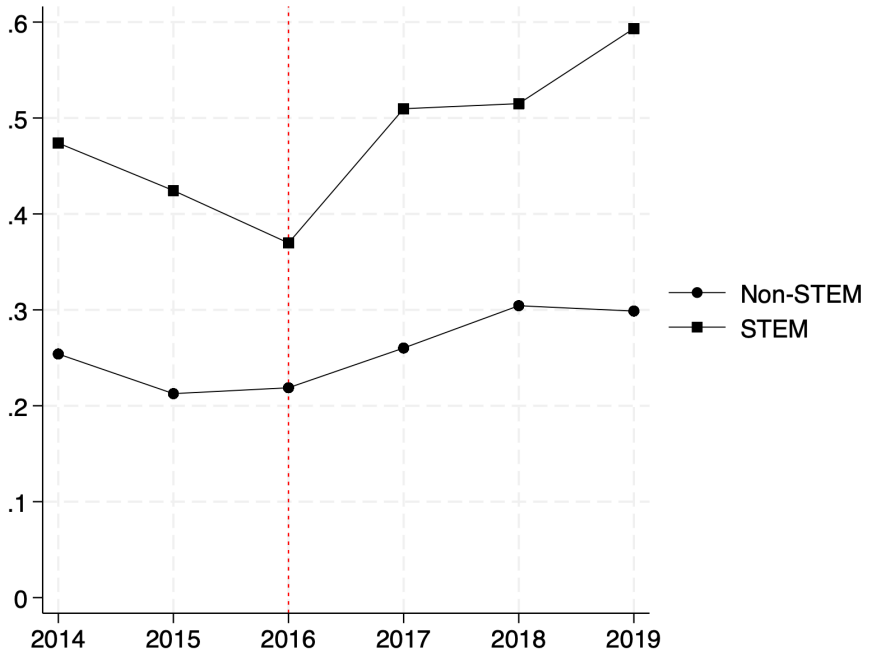
Notes: The table presents estimates from the difference-in-differences model as specified in Equation 3. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. The dependent variable is the log of annual salary. All salaries are adjusted to constant 2010 dollars and capped between 0 and $1,000,000$. STEM eligibility is specified according to the Department of Homeland Security's 2011 STEM Designated Degree Program List. Specification (1) includes the full interaction between a STEM eligibility indicator and a post-policy period indicator. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Figure 1. OPT-to-H-1B Pipeline



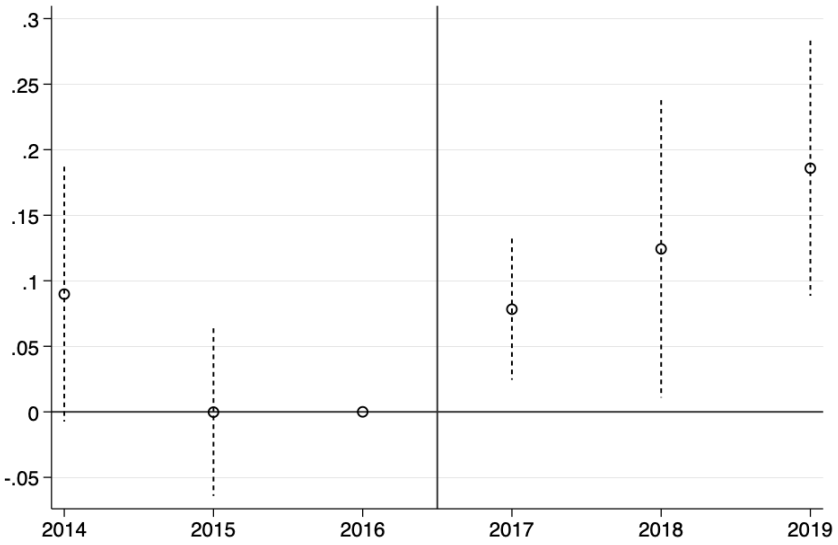
Notes: The figure summarizes the pathway through which the 2016 STEM OPT extension may affect observed outcomes in our data. By lengthening the period of authorized employment under OPT, the reform may increase the number of H-1B application attempts through Labor Condition Applications (LCAs), the likelihood of remaining eligible for additional lottery cycles, and progression to Form I-129 adjudication. Conditional on movement through this pipeline, the reform may also affect early-career labor market outcomes, including employment continuity, private-sector employment, and earnings.

Figure 2. Aggregated Occupation-Year Level Trend in H-1B Lottery Winning Rate



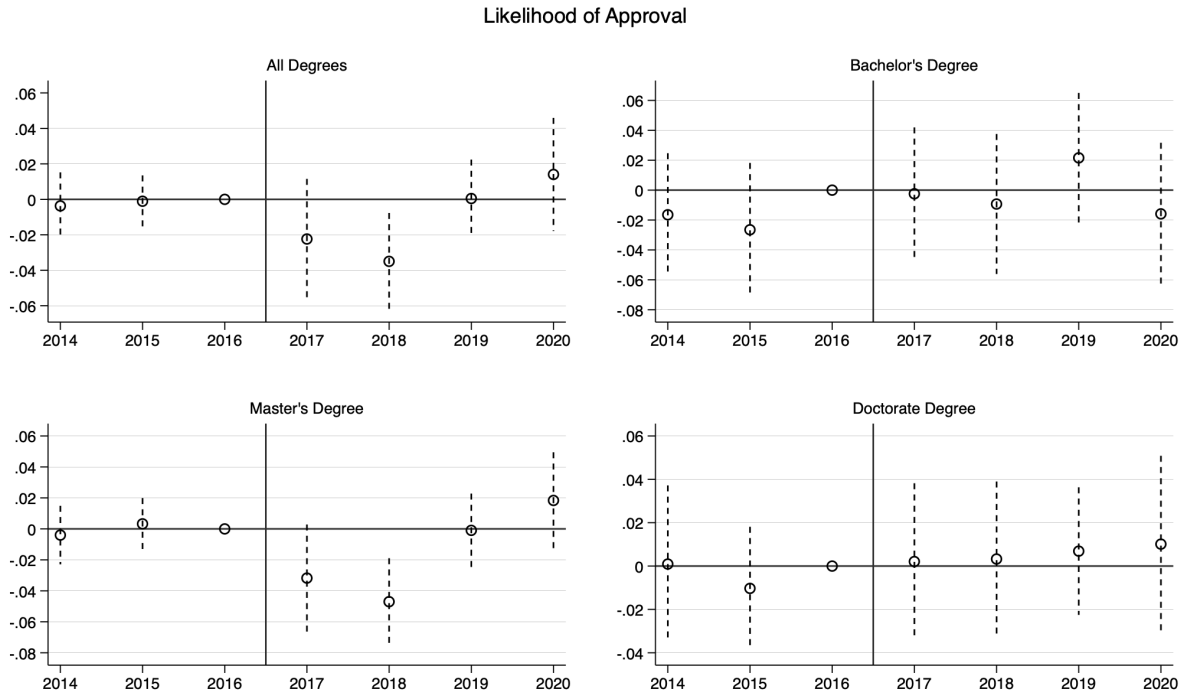
Notes: The figure plots the yearly trend in the H-1B lottery winning rate at the aggregated occupation-year level for STEM and non-STEM occupations. STEM eligibility is specified according to the 2010 Census STEM and STEM-related Occupation Code List. The H-1B lottery winning rate is calculated as $\frac{\text{I-129 petitions}}{\text{LCA filings}}$, using LCA and I-129 data that are collapsed and merged at the occupation-year level.

Figure 3. Aggregated Occupation-Year Level Event Study on H-1B Lottery Winning Rate



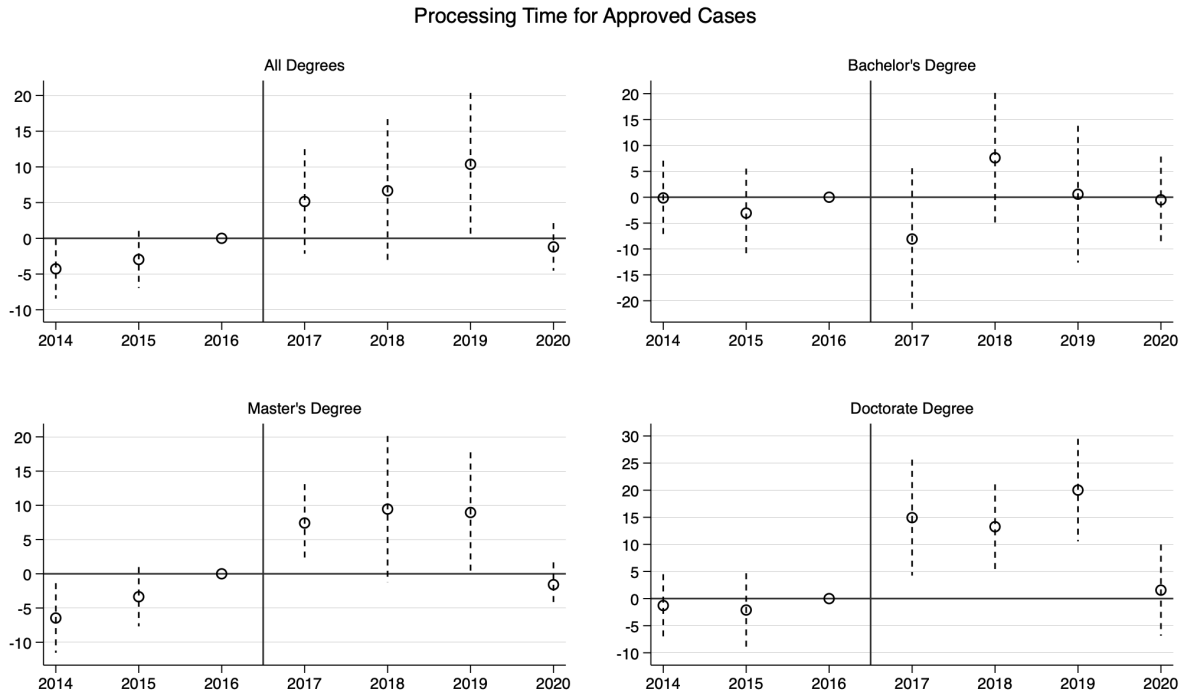
Notes: The figure presents the event study estimates and 95% confidence intervals for the aggregated occupation-year level H-1B lottery winning rate, as specified in Equation 2. STEM eligibility is specified according to the 2010 Census STEM and STEM-related Occupation Code List. The H-1B lottery winning rate is calculated as $\frac{\text{I-129 petitions}}{\text{LCA filings}}$, using LCA and I-129 data that are collapsed and merged at the occupation-year level.

Figure 4. Event Study Coefficients: I-129 Petition Approval Rate



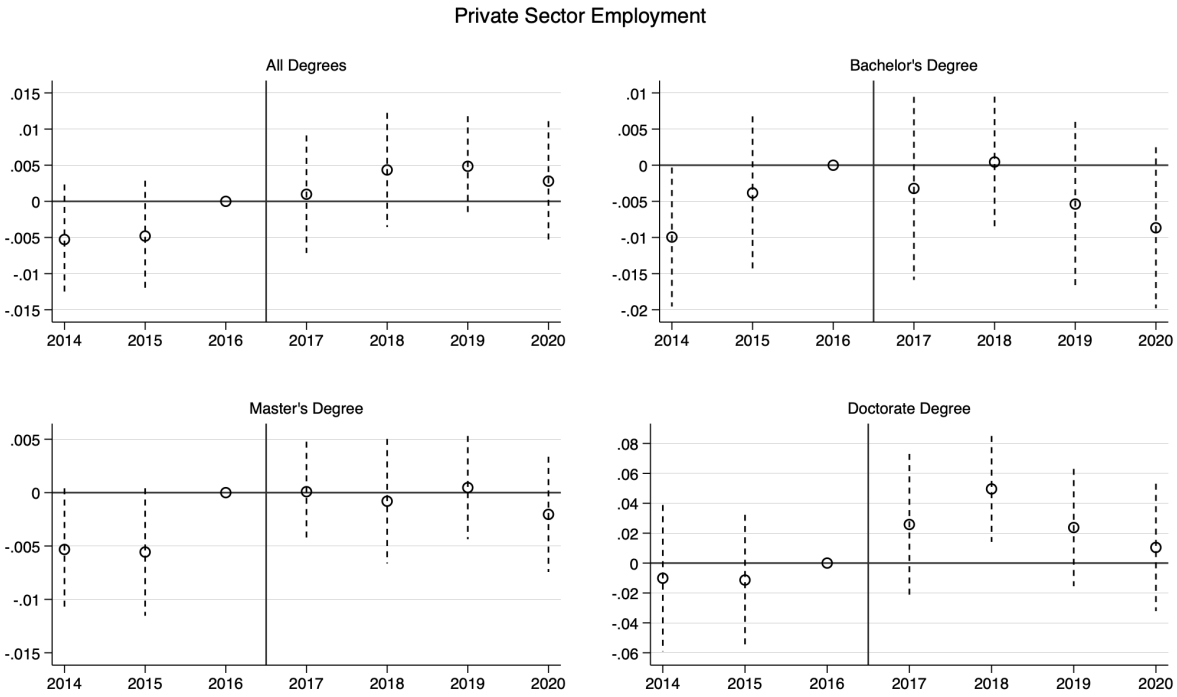
Notes: The figures display the event-study estimates and 95% confidence intervals for the individual-level outcomes, as specified in Equation 4. The sample period covers 2014 to 2020. The reference period is 2016. All estimates include field-of-study and fiscal-year fixed effects, and control for individual characteristics including gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level.

Figure 5. Event Study Coefficients: Timing of I-129 Processing



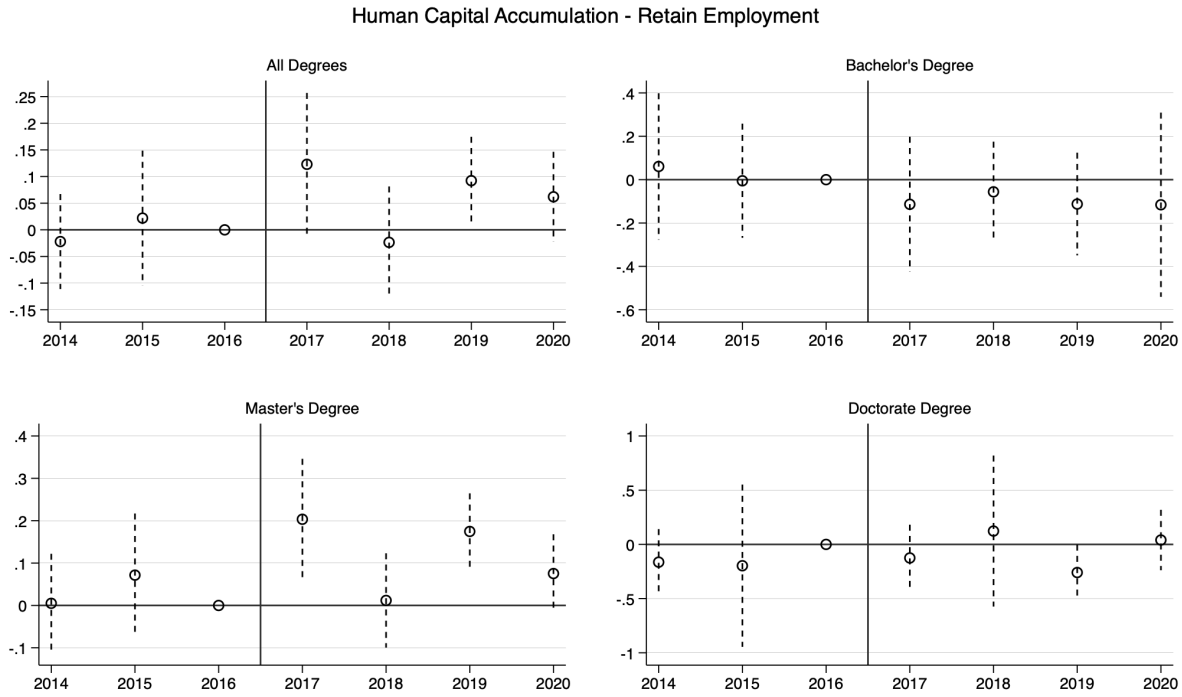
Notes: The figures display the event-study estimates and 95% confidence intervals for the individual-level outcomes, as specified in Equation 4. The sample period covers 2014 to 2020. The reference period is 2016. All estimates include field-of-study and fiscal-year fixed effects, and control for individual characteristics including gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level.

Figure 6. Event Study Coefficients: Private Sector Employment



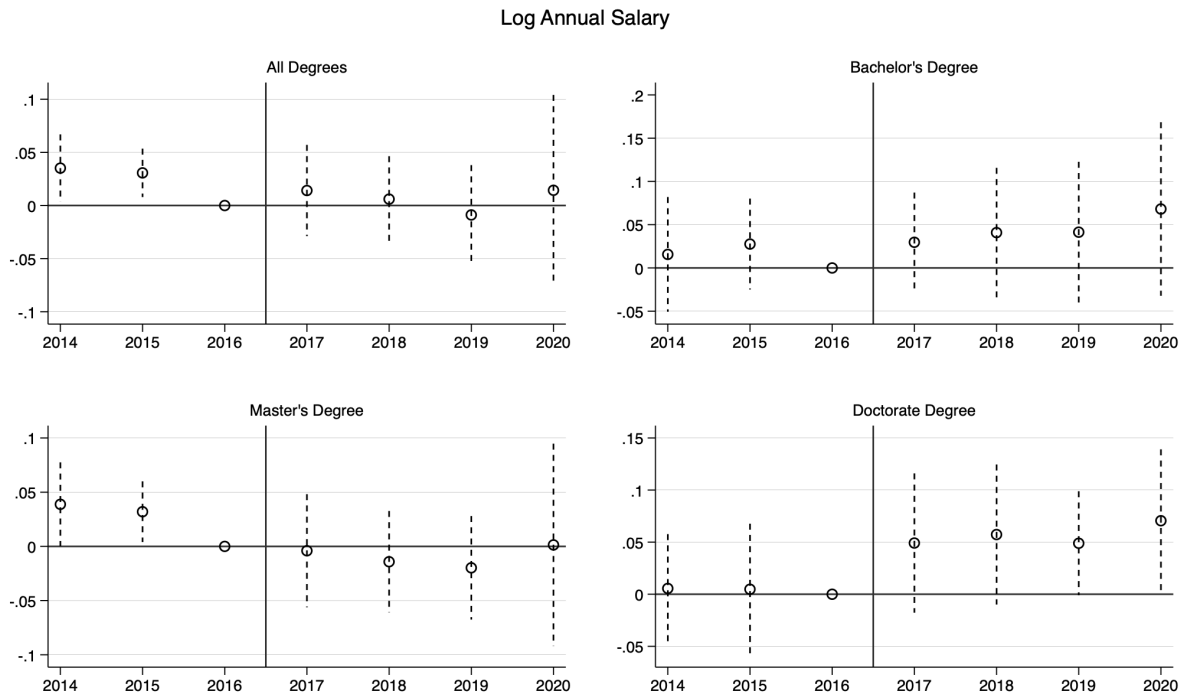
Notes: The figures display the event-study estimates and 95% confidence intervals for the individual-level outcomes, as specified in Equation 4. The sample period covers 2014 to 2020. The reference period is 2016. All estimates include field-of-study and fiscal-year fixed effects and control for individual characteristics including gender, degree level, and country of birth. Standard errors are clustered by major at the 6-digit CIP code level.

Figure 7. Event Study Coefficients: Employment Continuity



Notes: The figures display the event-study estimates and 95% confidence intervals for the individual-level outcomes, as specified in Equation 4. The sample period covers 2014 to 2020. The reference period is 2016. All estimates include field-of-study and fiscal-year fixed effects, and control for individual characteristics including gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level.

Figure 8. Event Study Coefficients: Annual Salary



Notes: The figures display the event-study estimates and 95% confidence intervals for the individual-level outcomes, as specified in Equation 4. The sample period covers 2014 to 2020. The reference period is 2016. All estimates include field-of-study and fiscal-year fixed effects, and control for individual characteristics including gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level.

Appendix: Additional Tables and Figures

Table A1. Summary Statistics for Merged LCA and I-129 Data, 2014–2019

	STEM		Non-STEM		
	Mean	S.D.	Mean	S.D.	Difference
	(1)	(2)	(3)	(4)	(1) – (3)
Education					
Bachelor's Degree	0.063	0.051	0.153	0.209	-0.090**
Professional Degree	0.060	0.124	0.036	0.112	0.024
Master's Degree	0.417	0.071	0.462	0.066	-0.045***
Doctorate Degree	0.105	0.090	0.040	0.069	0.065***
Region					
Midwest	0.188	0.116	0.123	0.077	0.065***
South	0.239	0.078	0.243	0.127	-0.004
Northeast	0.322	0.141	0.370	0.139	-0.048*
West	0.252	0.115	0.264	0.122	-0.012
Country of Origin					
India	0.511	0.162	0.246	0.186	0.265***
China	0.269	0.168	0.366	0.166	-0.097***
Other	0.220	0.092	0.389	0.181	-0.169***
2-Digit NAICS Industry					
Agriculture, Forestry, Fishing and Hunting	0.001	0.003	0.001	0.002	0.000
Mining, Quarrying, and Oil and Gas Extraction	0.005	0.010	0.005	0.015	0.000
Utilities	0.002	0.003	0.003	0.007	-0.001
Construction	0.028	0.058	0.034	0.119	-0.006
Manufacturing (Food, textile, apparel, etc.)	0.011	0.023	0.013	0.021	-0.002
Manufacturing (Wood, paper, printing, etc.)	0.075	0.108	0.027	0.056	0.048***
Manufacturing (Metal, machinery, electronics, etc.)	0.131	0.164	0.072	0.103	0.059**
Wholesale Trade	0.015	0.020	0.026	0.037	-0.011
Retail Trade (Motor vehicles, furniture, electronics, etc.)	0.014	0.033	0.014	0.025	0.000
Retail Trade (Clothing, general merchandise, nonstore retail, etc.)	0.003	0.006	0.007	0.013	-0.004
Transportation and Warehousing (Air, truck, and pipeline transportation, etc.)	0.003	0.006	0.005	0.008	-0.002
Transportation and Warehousing (Postal services, couriers, warehousing, etc.)	0.001	0.002	0.001	0.003	0.000
Information	0.020	0.032	0.083	0.144	-0.063**
Finance and Insurance	0.036	0.083	0.061	0.092	-0.025
Real Estate and Rental and Leasing	0.005	0.018	0.014	0.023	-0.009*
Professional, Scientific, and Technical Services	0.340	0.204	0.326	0.231	0.014
Management of Companies and Enterprises	0.006	0.011	0.005	0.010	0.001
Administrative and Support and Waste Management Services	0.020	0.034	0.018	0.031	0.002
Educational Services	0.005	0.016	0.026	0.068	-0.021*
Health Care and Social Assistance	0.200	0.316	0.092	0.216	0.108**
Arts, Entertainment, and Recreation	0.001	0.005	0.013	0.039	-0.012
Accommodation and Food Services	0.001	0.002	0.006	0.009	-0.005**
Other Services (except Public Administration)	0.002	0.004	0.056	0.177	-0.054*
Public Administration	0.002	0.002	0.006	0.015	-0.004*
Firm Size					
Log Firm Size	5.958	1.162	6.234	2.407	-0.276
Occupation-Year Cell	31		114		

Notes: Authors' calculations are based on LCA and I-129 data that are collapsed and merged at the occupation-year level. The data span from 2014 to 2019. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

Table A2. Composition of I-129 Petition Records, 2014–2020

	All Degrees	Bachelor's	Master's	Doctorate
Panel A				
I-129 Petitions in F-1 Status	444,564	46,231	320,267	47,352
I-129 Petitions in F-1 Status and Applying for an H-1B Visa	420,232	46,021	320,152	47,338
H-1B Applicants with Identifiable STEM/Non-STEM Major	389,195	43,757	300,602	44,836
Panel B				
Case Status				
Approved Cases	273,462	31,064	206,584	35,814
Other Cases	115,733	12,693	94,018	9,022
Employment				
Private Sector	370,300	42,999	296,006	31,295
Exempt Sector	18,895	758	4,596	13,541
Employment Continuity				
New Employment	384,448	43,232	296,838	44,378
Change of Employment	908	85	747	76
Retain Employment	3,839	440	3,017	382
Total	389,195	43,757	300,602	44,836

Notes: Table presents the sample composition of individual administrative records from U.S. Citizenship and Immigration Services Form I-129, obtained from the Department of Homeland Security through a Freedom of Information Act request. The data span the years 2014 to 2020. Case statuses include *approved, administratively closed, Department of State revoked, revoked (was initially approved), denied,* and *pending*. Exempt-sector employment refers to H-1B cap-exempt sectors such as universities and colleges. Retain employment includes (i) *amended petition*, which indicates that the requested worker(s) will be continuing employment with the same employer with a material change to job duties; (ii) *change previous employment*, which indicates continuation with the same employer without a material change to job duties; (iii) *continued employment*, which indicates ongoing employment with same employer; and (iv) *new concurrent employment*, which indicates that the requested worker(s) will begin employment with an additional employer.

Table A3. Difference-in-Differences for H-1B Application Attempts

Outcome	Log LCA Filings
Sample Period	2014–2019
treat	0.2630**
	(0.1013)
N	314
Y Mean	4.95
Fiscal Year FE	Yes
SOC Fixed Effects	Yes

Notes: The table presents estimates from a difference-in-differences model of the log number of Labor Condition Application (LCA) filings on the interaction between a STEM eligibility indicator and a post-policy period indicator. LCAs are interpreted as H-1B application attempts within the OPT-to-H-1B pipeline. STEM eligibility is defined using the 2010 Census STEM and STEM-related Occupation Code List. The unit of observation is the occupation-year cell. Specifications include occupation and fiscal year fixed effects. The sample period covers 2014 to 2019, with the post-policy period beginning in 2017. Standard errors are clustered by SOC occupation. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A4. Difference-in-Differences for Processing Time by Case Status

	Processing Time: Denied Cases			Processing Time: Revoked Cases			Processing Time: Pending or Closed Cases		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample: All Degrees									
treat	5.6034	10.3640***	9.0897**	23.1676**	24.3179*	24.0973*	57.9319	47.5168	-22.0437
	(3.7601)	(3.8802)	(3.9172)	(10.8597)	(12.7827)	(13.0292)	(104.9122)	(125.8308)	(137.1679)
N	47,929	47,929	47,929	67,199	67,199	67,199	603	603	603
Y Mean	235.68	235.68	235.68	685.35	685.35	685.35	314.07	314.07	314.07
Field of Study FE		✓	✓		✓	✓		✓	✓
Fiscal Year FE		✓	✓		✓	✓		✓	✓
Individual Characteristics			✓			✓			✓

Notes: The table presents estimates from the difference-in-differences model as specified in Equation 3. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. The dependent variable is a continuous variable, indicating difference in days between the act date (the date of the last adjudicative action indicating the current status) and the received date (the date on which USCIS received the petition). STEM eligibility is specified according to the Department of Homeland Security's 2011 STEM Designated Degree Program List. Specifications (1), (4), and (7) include the full interaction between a STEM eligibility indicator and a post-policy period indicator. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A5. Joint Test: Pre-Reform Event-Study (P-Values)

	Likelihood of I-129 Petition Approval	Timing of Processing if Approved	Employment in the Private Sector	Employment Continuity	Log Annual Salary
All Degrees	0.9025	0.1218	0.3266	0.7841	0.0228
Bachelor's	0.4985	0.5936	0.1162	0.9367	0.5814
Master's	0.6598	0.0414	0.1032	0.6095	0.0481
Doctorate	0.6870	0.8294	0.8642	0.4273	0.9775

Notes: The table presents p-values from F-tests of the null hypothesis that the pre-reform event-study coefficients are jointly equal to zero. The test uses the 2014 and 2015 coefficients, with 2016 as the reference year.

Table A6. Difference-in-Differences for Annual Salary by Exempt Sector

	Log Annual Salary: Exempt Sector			Log Annual Salary: Private Sector		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Degrees						
treat	-0.0053	-0.0064	-0.0001	-0.0141	-0.0155	-0.0138
	(0.0189)	(0.0208)	(0.0171)	(0.0264)	(0.0282)	(0.0287)
N	18,759	18,759	18,759	332,596	332,596	332,596
Y Mean	10.88	10.88	10.88	11.07	11.07	11.07
Panel B: Bachelor's Degree						
treat	-0.0761	-0.0489	-0.1310	0.0233	0.0102	0.0092
	(0.1451)	(0.1951)	(0.2162)	(0.0410)	(0.0358)	(0.0352)
N	741	741	741	37,394	37,394	37,394
Y Mean	10.50	10.50	10.50	10.96	10.96	10.96
Panel C: Master's Degree						
treat	-0.0310	-0.0431	-0.0475	-0.0175	-0.0179	-0.0237
	(0.0405)	(0.0377)	(0.0356)	(0.0284)	(0.0307)	(0.0314)
N	4,562	4,562	4,562	266,796	266,796	266,796
Y Mean	10.75	10.75	10.75	11.06	11.06	11.06
Panel D: Doctorate Degree						
treat	0.0206	0.0289*	0.0248*	0.0748**	0.0619*	0.0603*
	(0.0215)	(0.0153)	(0.0147)	(0.0357)	(0.0351)	(0.0348)
N	13,456	13,456	13,456	28,406	28,406	28,406
Y Mean	10.94	10.94	10.94	11.24	11.24	11.24
Field of Study FE		✓	✓		✓	✓
Fiscal Year FE		✓	✓		✓	✓
Individual Characteristics			✓			✓

Notes: The table presents estimates from the difference-in-differences model as specified in Equation 3. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. The dependent variable is the log of annual salary. All salaries are adjusted to constant 2010 dollars and capped between 0 and $1,000,000$. STEM eligibility is specified according to the Department of Homeland Security's 2011 STEM Designated Degree Program List. Specifications (1) and (4) include the full interaction between a STEM eligibility indicator and a post-policy period indicator. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A7. Test of the Composition of STEM Applicants

	(1) Graduates with Bachelor's Degree	(2) Graduates from India or China
STEM × 2014	-0.0100	0.0121
	(0.0076)	(0.0084)
STEM × 2015	0.0109	-0.0084
	(0.0069)	(0.0092)
STEM × 2017	-0.0064	-0.0031
	(0.0054)	(0.0064)
STEM × 2018	-0.0135*	-0.0125
	(0.0079)	(0.0102)
STEM × 2019	0.0137	-0.0205
	(0.0086)	(0.0142)
STEM × 2020	0.0306*	-0.0291*
	(0.0168)	(0.0173)
N	389,195	389,195
Y Mean	0.11	0.81
Field of Study FE	✓	✓
Fiscal Year FE	✓	✓

Notes: The table reports coefficient estimates from regressions of educational attainment and country of origin on interactions between STEM status and fiscal-year indicators. The dependent variable in Column (1) is an indicator for applicants with a bachelor's degree. The dependent variable in Column (2) is an indicator for applicants from India or China. The sample period is 2014–2020, with 2016 as the reference year. Joint tests of the null that the STEM interactions for 2017–2020 are equal to zero yield $p = 0.0043$ for educational attainment and $p = 0.5018$ for country of origin. All specifications include field-of-study and fiscal-year fixed effects. Standard errors are clustered by major at the 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A8. Robustness Check: Inverse Probability Weighting

Dependent Variables:	Likelihood of I-129 Petition Approval	Timing of Processing if Approved	Employment in the Private Sector	Employment Continuity	Log Annual Salary
	(1)	(2)	(3)	(4)	(5)
Panel A: All Degrees					
treat	-0.0100	5.9212*	0.0085***	0.0536**	-0.0211
	(0.0071)	(3.1197)	(0.0032)	(0.0228)	(0.0259)
N	387,096	271,836	387,096	4,721	349,353
Y Mean	0.71	94.32	0.94	0.80	11.03
Panel B: Bachelor's Degree					
treat	0.0081	-2.3499	0.0084**	-0.0969	-0.0086
	(0.0161)	(4.4080)	(0.0035)	(0.0733)	(0.0372)
N	43,533	30,890	43,533	524	37,917
Y Mean	0.71	103.12	0.98	0.85	10.94
Panel C: Master's Degree					
treat	-0.0147*	5.2966**	0.0060***	0.0699**	-0.0340
	(0.0075)	(2.1559)	(0.0017)	(0.0276)	(0.0303)
N	299,424	205,688	299,424	3,744	270,186
Y Mean	0.69	100.87	0.98	0.79	11.04
Panel D: Doctorate Degree					
treat	0.0105	11.1059***	0.0318**	0.0352	0.0594***
	(0.0090)	(3.4023)	(0.0160)	(0.1338)	(0.0161)
N	44,139	35,258	44,139	453	41,250
Y Mean	0.80	47.34	0.62	0.83	11.12
Field of Study FE	✓	✓	✓	✓	✓
Fiscal Year FE	✓	✓	✓	✓	✓
Individual Characteristics	✓	✓	✓	✓	✓

Notes: The table presents robustness checks for the main outcomes reported in Tables 3 to 7. The difference-in-differences specification is estimated using inverse probability weighting (IPW) based on propensity score matching with pre-reform country, education, and two-digit industry. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A9. IPW Covariate Balance by STEM Status

Covariate	P-Value
Country	0.515
Education	0.611
Industry (2-digit)	0.240

Notes: The table presents p-values from tests of whether the weighted mean differs by treatment status. Robust standard errors are used. All p-values exceed 0.10, indicating no significant differences between STEM and non-STEM petitions on observable characteristics in the pre-reform period.

Table A10. Robustness Check: F-1 vs. J-1, STEM Sample

Dependent Variables:	Likelihood of I-129 Petition Approval	Timing of Processing if Approved	Employment in the Private Sector	Employment Continuity	Log Annual Salary
	(1)	(2)	(3)	(4)	(5)
Panel A: All Degrees					
F-1 x Post	-0.0331***	45.7977***	0.0352***	0.1059	0.0269**
	(0.0109)	(2.5760)	(0.0067)	(0.0746)	(0.0127)
N	313,908	220,633	313,908	3,909	284,574
Y Mean	0.70	92.88	0.94	0.82	11.11
Panel B: Bachelor's Degree					
F-1 x Post	-0.0003	5.2360	0.0074	-0.0841	0.1300**
	(0.0225)	(9.5745)	(0.0147)	(0.2159)	(0.0509)
N	28,109	20,517	28,109	381	24,261
Y Mean	0.73	100.08	0.98	0.83	11.10
Panel C: Master's Degree					
F-1 x Post	-0.0077	28.8802***	0.0332	0.1211	0.0148
	(0.0408)	(10.3336)	(0.0230)	(0.1014)	(0.0291)
N	237,506	162,289	237,506	3,063	214,952
Y Mean	0.68	102.58	0.99	0.81	11.12
Panel D: Doctorate Degree					
F-1 x Post	-0.0033	23.4725***	0.0614***	0.1694**	0.0597***
	(0.0085)	(1.8087)	(0.0139)	(0.0794)	(0.0130)
N	48,293	37,827	48,293	465	45,361
Y Mean	0.78	47.35	0.68	0.85	11.08
Field of Study FE	✓	✓	✓	✓	✓
Fiscal Year FE	✓	✓	✓	✓	✓
Individual Characteristics	✓	✓	✓	✓	✓

Notes: The table presents robustness checks for the main outcomes reported in Tables 3 to 7. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. The sample is restricted to STEM petitions only. The comparison group is J-1 visa holders. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A11. Robustness Check: F-1 vs. TN/E-3, STEM Sample

Dependent Variables:	Likelihood of I-129 Petition Approval	Timing of Processing if Approved	Employment in the Private Sector	Employment Continuity	Log Annual Salary
	(1)	(2)	(3)	(4)	(5)
Panel A: All Degrees					
F-1 x Post	-0.0213*	11.9839***	0.0171***	0.1460***	-0.0170
	(0.0116)	(2.6801)	(0.0060)	(0.0400)	(0.0266)
N	305,448	214,424	305,448	3,987	275,758
Y Mean	0.70	96.27	0.96	0.82	11.13
Panel B: Bachelor's Degree					
F-1 x Post	-0.0087	8.6304***	0.0043***	0.1229	-0.0033
	(0.0149)	(2.4116)	(0.0016)	(0.0860)	(0.0368)
N	31,017	22,910	31,017	486	26,780
Y Mean	0.74	102.23	0.99	0.85	11.15
Panel C: Master's Degree					
F-1 x Post	-0.0367*	20.6776***	0.0023	0.1657	-0.0308
	(0.0202)	(3.6352)	(0.0087)	(0.1407)	(0.0377)
N	237,963	162,664	237,963	3,084	215,285
Y Mean	0.68	102.74	0.99	0.81	11.12
Panel D: Doctorate Degree					
F-1 x Post	0.0170	1.2819	0.0795***	-0.1404	-0.0236
	(0.0315)	(6.4061)	(0.0249)	(0.3133)	(0.0611)
N	36,468	28,850	36,468	417	33,693
Y Mean	0.79	55.02	0.75	0.84	11.17
Field of Study FE	✓	✓	✓	✓	✓
Fiscal Year FE	✓	✓	✓	✓	✓
Individual Characteristics	✓	✓	✓	✓	✓

Notes: The table presents robustness checks for the main outcomes reported in Tables 3 to 7. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. The sample is restricted to STEM petitions only. The comparison group is TN and E-3 visa holders. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A12. Robustness Checks: Adding Industry and State Fixed Effects

Dependent Variables:	Likelihood of I-129 Petition Approval	Timing of Processing if Approved	Employment in the Private Sector	Employment Continuity	Log Annual Salary
	(1)	(2)	(3)	(4)	(5)
Panel A: All Degrees					
treat	-0.0080	6.7332*	0.0012	0.0567**	-0.0074
	(0.0077)	(3.5845)	(0.0024)	(0.0220)	(0.0236)
N	389,195	273,462	389,195	4,747	351,355
Y Mean	0.70	95.48	0.95	0.81	11.06
Panel B: Bachelor's Degree					
treat	0.0077	0.9367	-0.0018	-0.1018	0.0034
	(0.0183)	(4.4326)	(0.0026)	(0.1184)	(0.0327)
N	43,757	31,064	43,757	525	38,135
Y Mean	0.71	103.26	0.98	0.84	10.95
Panel C: Master's Degree					
treat	-0.0116*	8.2978***	0.0016	0.0690***	-0.0150
	(0.0068)	(2.4973)	(0.0019)	(0.0244)	(0.0271)
N	300,602	206,584	300,602	3,764	271,358
Y Mean	0.69	101.88	0.98	0.80	11.06
Panel D: Doctorate Degree					
treat	0.0060	11.7417***	0.0034	0.0735	0.0278
	(0.0100)	(3.3283)	(0.0094)	(0.1282)	(0.0190)
N	44,836	35,814	44,836	458	41,862
Y Mean	0.80	51.81	0.70	0.83	11.15
Field of Study FE	✓	✓	✓	✓	✓
Fiscal Year FE	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓
Individual Characteristics	✓	✓	✓	✓	✓

Notes: The table presents robustness checks for the main outcomes reported in Tables 3 to 7. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A13. Robustness Checks: Controlling for Bartik Labor Demand

Dependent Variables:	Likelihood of I-129 Petition Approval	Timing of Processing if Approved	Employment in the Private Sector	Employment Continuity	Log Annual Salary
	(1)	(2)	(3)	(4)	(5)
Panel A: All Degrees					
STEM × Bartik × Post	0.0005	-0.5622	-0.0008	0.0073	0.0033
	(0.0016)	(0.4991)	(0.0009)	(0.0159)	(0.0027)
STEM × Bartik	0.0000	0.4720	0.0008	-0.0072	-0.0033
	(0.0014)	(0.4695)	(0.0009)	(0.0162)	(0.0027)
STEM × Post	-0.0227	13.7473*	0.0127	0.0025	-0.0360
	(0.0209)	(7.3986)	(0.0079)	(0.1178)	(0.0298)
N	388,766	273,148	388,766	4,743	350,954
Y Mean	0.70	95.50	0.95	0.81	11.06
Panel B: Bachelor's Degree					
STEM × Bartik × Post	-0.0058	-0.8028	-0.0003	-0.0074	0.0056
	(0.0048)	(0.9311)	(0.0013)	(0.0367)	(0.0069)
STEM × Bartik	0.0057	0.8102	0.0002	0.0070	-0.0052
	(0.0047)	(0.9506)	(0.0012)	(0.0368)	(0.0070)
STEM × Post	0.0590	6.8159	0.0040	-0.0495	-0.0402
	(0.0413)	(8.6692)	(0.0105)	(0.2870)	(0.0582)
N	43,697	31,020	43,697	525	38,083
Y Mean	0.71	103.25	0.98	0.84	10.95
Panel C: Master's Degree					
STEM × Bartik × Post	0.0019	-0.5327	-0.0010	0.0175	0.0031
	(0.0020)	(0.5464)	(0.0007)	(0.0178)	(0.0037)
STEM × Bartik	-0.0012	0.4101	0.0010	-0.0181	-0.0031
	(0.0019)	(0.5238)	(0.0007)	(0.0182)	(0.0037)
STEM × Post	-0.0429**	15.8000**	0.0115*	-0.0334	-0.0482
	(0.0215)	(6.8021)	(0.0060)	(0.1342)	(0.0394)
N	300,305	206,373	300,305	3,761	271,080
Y Mean	0.69	101.90	0.98	0.80	11.06
Panel D: Doctorate Degree					
STEM × Bartik × Post	-0.0028	-0.6627	-0.0024	-0.0542	0.0012
	(0.0032)	(0.7721)	(0.0052)	(0.0855)	(0.0075)
STEM × Bartik	0.0029	0.5006	0.0021	0.0566	-0.0011
	(0.0033)	(0.7829)	(0.0052)	(0.0859)	(0.0076)
STEM × Post	0.0290	22.3453***	0.0605	0.4503	0.0431
	(0.0273)	(6.1665)	(0.0431)	(0.6622)	(0.0617)
N	44,764	35,755	44,764	457	41,791
Y Mean	0.80	51.81	0.70	0.83	11.15
Field of Study FE	✓	✓	✓	✓	✓
Fiscal Year FE	✓	✓	✓	✓	✓
Individual Characteristics	✓	✓	✓	✓	✓

Notes: The table presents robustness checks for the main outcomes reported in Tables 3 to 7. The reported coefficients correspond to the full interaction between the Bartik labor-demand measure, STEM status, and the post-reform indicator. The Bartik measure is constructed using ACS data as $\sum_j s_{mj}^{2014-2016} g_{jt}$, where $s_{mj}^{2014-2016}$ is the share of workers in major m employed in industry j during the base period 2014–2016, and g_{jt} is the national employment growth rate in industry j at time t. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at the 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A14. Joint Tests: STEM × Bartik and STEM × Post × Bartik (P-Values)

	Likelihood of I-129 Petition Approval	Timing of Processing if Approved	Employment in the Private Sector	Employment Continuity	Log Annual Salary
All Degrees	0.3077	0.1209	0.6541	0.8160	0.1136
Bachelor's	0.4810	0.6728	0.3520	0.9716	0.4039
Master's	0.0914	0.0011	0.1685	0.5771	0.2394
Doctorate	0.6660	0.0008	0.3619	0.2860	0.5967

Notes: The table presents p-values from F-tests of the null that STEM × Bartik and STEM × Post × Bartik are jointly equal to zero, for each outcome and degree group. The corresponding coefficient estimates are reported in Appendix Table A13.

Table A15. Adjudication Outcomes Before the RFE Surge, 2015–2017

Dependent Variables:	Likelihood of I-129 Petition Approval	Timing of Processing if Approved
	(1)	(2)
Panel A: All Degrees		
STEM × Post	-0.0198	4.7326
	(0.0174)	(3.7185)
N	103,941	69,410
Outcome Mean	0.67	103.99
Panel B: Bachelor's Degree		
STEM × Post	-0.0026	-8.0792
	(0.0234)	(7.0493)
N	13,099	9,074
Outcome Mean	0.69	109.06
Panel C: Master's Degree		
STEM × Post	-0.0303*	7.1643**
	(0.0178)	(2.9566)
N	79,096	50,976
Outcome Mean	0.64	111.99
Panel D: Doctorate Degree		
STEM × Post	0.0033	13.9592**
	(0.0182)	(5.7365)
N	11,746	9,360
Outcome Mean	0.80	55.56
Field of Study FE	✓	✓
Fiscal Year FE	✓	✓
Individual Characteristics	✓	✓

Notes: The table presents robustness checks for the adjudication outcomes reported in Tables 3 and 4 using a restricted 2015–2017 sample, during which information on requests for evidence was available as published by USCIS. The post-policy period is limited to 2017. This window precedes the sharp rise in RFE intensity beginning in 2018, documented in Appendix Figure A3. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at the 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A16. Robustness Checks: Treating Unmatched LCAs as Non-Selections

	H-1B Lottery Winning Rate			
	(1)	(2)	(3)	(4)
Unmatched LCAs treated as	Missing	Non-selected	Missing	Non-selected
treat	0.0574*	0.0734**	0.0627*	0.0581*
	(0.0316)	(0.0282)	(0.0333)	(0.0330)
Occupation-Specific Unemployment Trend			✓	✓
FY Fixed Effects	✓	✓	✓	✓
SOC Fixed Effects	✓	✓	✓	✓
N	145	265	145	170
Y Mean	0.35	0.19	0.35	0.30

Notes: Columns (1) and (3) reproduce columns (1) and (7) of Table 2, where occupation-year cells with unmatched LCAs are treated as missing. Columns (2) and (4) re-estimate the same specifications treating unmatched LCAs as non-selections. The other specifications in Table 2 cannot be estimated on this sample because their covariates are constructed from I-129 records, which are unavailable for unmatched cells. The sample period is 2014–2019, with the post-policy period covering 2017–2019. Standard errors are clustered by SOC occupation. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A17. Test for Changes in the Petition-to-LCA Ratio

	Petition-to-LCA-Covered-Worker Ratio
STEM × Post	0.0768
	(0.1205)
STEM	0.0199
	(0.0949)
Post	0.0986
	(0.0768)
Year by STEM Status Group	22
Y Mean	0.28

Notes: The table reports difference-in-differences estimates of the petition-to-LCA ratio. The regression corresponds to Figure A4. The sample consists of annual observations from 2010 to 2020 for STEM and non-STEM occupations (22 observations). The dependent variable is the ratio of H-1B petitions filed to LCA-covered workers. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A18. Robustness Checks for Aggregate Lottery Winning Rates

	H-1B Lottery Winning Rate							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Use Wild Cluster Bootstrap P-values								
treat	0.0574*	0.0578*	0.0625*	0.0708**	0.0562*	0.1147***	0.0627*	0.1110***
	(0.0316)	(0.0316)	(0.0315)	(0.0280)	(0.0317)	(0.0280)	(0.0333)	(0.0318)
Bootstrap p-value								
	[0.054]	[0.060]	[0.044]	[0.005]	[0.061]	[0.001]	[0.056]	[0.002]
N	145	145	145	145	145	145	145	145
Y Mean	0.35	0.35	0.35	0.35	0.35	0.35	0.35	0.35
Panel B: Drop Computer and Mathematical Occupations								
treat	0.0511	0.0513	0.0559	0.0648**	0.0505	0.1073***	0.0567	0.1042***
	(0.0332)	(0.0331)	(0.0329)	(0.0294)	(0.0336)	(0.0297)	(0.0346)	(0.0338)
N	139	139	139	139	139	139	139	139
Y Mean	0.35	0.35	0.35	0.35	0.35	0.35	0.35	0.35
Control for the share of group characteristics on the occupation by year level								
Education		✓						✓
Country (China, India, Other)			✓					✓
Region				✓				✓
Log firm size					✓			✓
Industry						✓		✓
Occupation-Specific Unemployment Trend							✓	✓
FY Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
SOC Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table reports robustness checks for the aggregate occupation-year lottery-winning-rate results in Table 2. Panel A reports wild cluster bootstrap p-values for the baseline specifications, clustering by SOC occupation. Panel B re-estimates the same specifications after excluding computer and mathematical occupations, with standard errors clustered by SOC occupation. The sample period is 2014–2019, with the post-policy period covering 2017–2019. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A19. Robustness Checks: Using Alternative STEM Eligibility List

Dependent Variables:	Likelihood of I-129 Petition Approval	Timing of Processing if Approved	Employment in the Private Sector	Employment Continuity	Log Annual Salary
	(1)	(2)	(3)	(4)	(5)
Panel A: All Degrees					
treat	-0.0075	5.8821*	0.0068**	0.0505**	-0.0138
	(0.0082)	(3.3607)	(0.0031)	(0.0249)	(0.0277)
N	388,859	273,222	388,859	4,744	351,035
Y Mean	0.70	95.51	0.95	0.81	11.06
Panel B: Bachelor's Degree					
treat	0.0125	-1.6632	0.0017	-0.1232*	-0.0099
	(0.0190)	(4.5912)	(0.0030)	(0.0704)	(0.0355)
N	43,652	31,005	43,652	523	38,037
Y Mean	0.71	103.17	0.98	0.84	10.95
Panel C: Master's Degree					
treat	-0.0125	8.2063***	0.0021	0.0807***	-0.0218
	(0.0083)	(2.6074)	(0.0016)	(0.0271)	(0.0326)
N	300,547	206,548	300,547	3,764	271,309
Y Mean	0.69	101.89	0.98	0.80	11.06
Panel D: Doctorate Degree					
treat	0.0082	14.5345***	0.0377**	-0.0710	0.0606***
	(0.0107)	(3.5141)	(0.0151)	(0.1129)	(0.0223)
N	44,660	35,669	44,660	457	41,689
Y Mean	0.80	51.92	0.70	0.83	11.15
Field of Study FE	✓	✓	✓	✓	✓
Fiscal Year FE	✓	✓	✓	✓	✓
Individual Characteristics	✓	✓	✓	✓	✓

Notes: The table presents robustness checks for the main outcomes reported in Tables 3 to 7. STEM eligibility is specified according to the DHS 2016 STEM Designated Degree Program List. All specifications include field-of-study and fiscal-year fixed effects and individual characteristics, as in column (3) of the main tables. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A20. Robustness Checks: Using Alternative Control Group Classification

Dependent Variables:	Likelihood of I-129 Petition Approval	Timing of Processing if Approved	Employment in the Private Sector	Employment Continuity	Log Annual Salary
	(1)	(2)	(3)	(4)	(5)
Panel A: All Degrees					
treat	-0.0124	5.1287	0.0052	0.0668***	-0.0185
	(0.0081)	(3.2677)	(0.0032)	(0.0217)	(0.0233)
N	389,195	273,462	389,195	4,747	351,355
Y Mean	0.70	95.48	0.95	0.81	11.06
Panel B: Bachelor's Degree					
treat	0.0142	-3.3995	0.0003	-0.0956	-0.0134
	(0.0176)	(4.3522)	(0.0027)	(0.0658)	(0.0358)
N	43,757	31,064	43,757	525	38,135
Y Mean	0.71	103.26	0.98	0.84	10.95
Panel C: Master's Degree					
treat	-0.0201**	7.2181***	0.0022	0.0833***	-0.0287
	(0.0080)	(2.5542)	(0.0016)	(0.0260)	(0.0264)
N	300,602	206,584	300,602	3,764	271,358
Y Mean	0.69	101.88	0.98	0.80	11.06
Panel D: Doctorate Degree					
treat	0.0101	13.2291***	0.0339**	0.1128	0.0524**
	(0.0095)	(3.1492)	(0.0144)	(0.1453)	(0.0202)
N	44,836	35,814	44,836	458	41,862
Y Mean	0.80	51.81	0.70	0.83	11.15
Field of Study FE	✓	✓	✓	✓	✓
Fiscal Year FE	✓	✓	✓	✓	✓
Individual Characteristics	✓	✓	✓	✓	✓

Notes: The table presents robustness checks for the main outcomes reported in Tables 3 to 7. The sample period is from 2014 to 2020, with the post-policy period covering 2017 to 2020. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A21. Mechanism: I-129 Adjudication by Cap Status, 2014–2020

	Approval Rate			Processing Days		
Fiscal Year	Cap-Exempt Mean (1)	Cap-Subject Mean (2)	Difference (2) – (1)	Cap-Exempt Mean (3)	Cap-Subject Mean (4)	Difference (4) – (3)
2014	0.76	0.71	-0.049***	28.42	56.47	28.05***
2015	0.77	0.71	-0.065***	39.75	70.25	30.50***
2016	0.79	0.71	-0.078***	39.72	77.65	37.93***
2017	0.82	0.62	-0.201***	23.66	137.04	113.38***
2018	0.78	0.61	-0.168***	42.44	136.37	93.93***
2019	0.79	0.68	-0.106***	24.02	121.74	97.72***
2020	0.86	0.85	-0.008	26.77	74.89	48.12***

Notes: Authors' calculations are based on individual administrative records from U.S. Citizenship and Immigration Services Form I-129, obtained from the Department of Homeland Security through a Freedom of Information Act request. The data spans from 2014 to 2020. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

Table A22. Mechanism: RFE Intensity, Approval, and Processing Time, 2015–2020

	Average Approval Rate			Average Processing Days		
	All Petitions	Cap-Exempt Petitions	Cap-Subject Petitions	All Petitions	Cap-Exempt Petitions	Cap-Subject Petitions
	(1)	(2)	(3)	(4)	(5)	(6)
RFE Rate	0.0078	-0.1729	0.1325	72.1655***	11.7933	95.0067***
	(0.1017)	(0.1595)	(0.1293)	(25.9672)	(22.8181)	(33.2275)
STEM	0.0860*	0.0081	0.1312**	-5.8949	10.0217	-17.1446
	(0.0482)	(0.0824)	(0.0562)	(12.2554)	(11.6071)	(14.8676)
STEM × RFE Rate	-0.1640	0.0946	-0.3223*	15.9690	-27.2871	46.1581
	(0.1455)	(0.2400)	(0.1690)	(38.7024)	(37.1027)	(44.4446)
Degree-Major Cell	1,267	531	736	1,222	511	711
Y Mean	0.78	0.82	0.74	67.92	33.93	92.35

Notes: The table relates RFE intensity to adjudication outcomes at the degree-major cell level. The dependent variables are the average approval rate in Columns (1)–(3) and average processing days in Columns (4)–(6). Columns (1) and (4) use all petitions, Columns (2) and (5) restrict to cap-exempt petitions, and Columns (3) and (6) to cap-subject petitions. The RFE rate is the annual share of petitions receiving a Request for Evidence. Given the short 2015–2020 window and the aggregate nature of the RFE measure, the estimates should be interpreted as descriptive associations rather than causal effects. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A23. Mechanism: RFEs and Labor Market Outcomes, 2015–2020

	Private Sector Employment	Retain Employment	Annual Salary
	(1)	(2)	(3)
Panel A: All Degrees			
treat	0.0077***	0.0900***	0.0010
	(0.0024)	(0.0272)	(0.0093)
RFEs	0.0011	-0.3274***	0.0604
	(0.0076)	(0.0783)	(0.0555)
Approved RFEs	-0.0102	0.3537***	0.0746
	(0.0090)	(0.1234)	(0.0697)
N	349,952	4,250	316,163
Y Mean	0.95	0.82	11.07
Panel B: Bachelor's Degree			
treat	0.0011	-0.0668	-0.0128
	(0.0023)	(0.0603)	(0.0239)
RFEs	0.0277*	-0.0029	0.3143***
	(0.0141)	(0.4856)	(0.1093)
Approved RFEs	-0.0375**	0.2606	-0.1419
	(0.0160)	(0.6292)	(0.1360)
N	37,841	452	32,982
Y Mean	0.98	0.85	10.96
Panel C: Master's Degree			
treat	0.0032***	0.1171***	-0.0073
	(0.0009)	(0.0313)	(0.0106)
RFEs	0.0013	-0.3723***	0.0484
	(0.0042)	(0.0861)	(0.0560)
Approved RFEs	-0.0083	0.3854***	0.0873
	(0.0053)	(0.1377)	(0.0712)
N	273,142	3,399	246,769
Y Mean	0.99	0.81	11.07
Panel D: Doctorate Degree			
treat	0.0368***	0.0850	0.0724***
	(0.0116)	(0.0794)	(0.0141)
RFEs	0.0066	-0.7103	-0.0795
	(0.0515)	(0.4644)	(0.0571)
Approved RFEs	-0.0308	0.8233	0.1391*
	(0.0638)	(0.7674)	(0.0739)
N	38,969	399	36,412
Y Mean	0.70	0.85	11.16
Field of Study FE	✓	✓	✓
Individual Characteristics	✓	✓	✓

Notes: The table presents regressions using individual I-129 data for 2015–2020. RFEs and approved RFEs are the logarithms of annual national volumes reported by USCIS. Because these measures vary only by fiscal year, the specifications include field-of-study fixed effects and individual characteristics but no fiscal-year fixed effects. The RFE coefficients may also capture other national changes occurring in the same year. Individual characteristics include gender, degree level, and country of birth. Standard errors are clustered by major at the 6-digit CIP code level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A24. Mechanism: OPT-to-H-1B Transition Time, 2017–2020

	Days from LCA Receipt to I-129 Action		
	STEM Mean (1)	Non-STEM Mean (2)	Difference (1) – (2)
Bachelor's Degree			
2017	397.2	352.8	44.4***
2018	350.6	319.6	31.0***
2019	279.2	268.7	10.5
2020	178.2	175.8	2.4
Master's Degree			
2017	385.9	361.5	24.3***
2018	313.7	285.2	28.5***
2019	287.0	267.9	19.2***
2020	178.6	175.8	2.8
Doctorate Degree			
2017	353.8	339.3	14.5
2018	259.5	207.0	52.5***
2019	237.5	182.3	55.2***
2020	146.7	115.7	31.0***

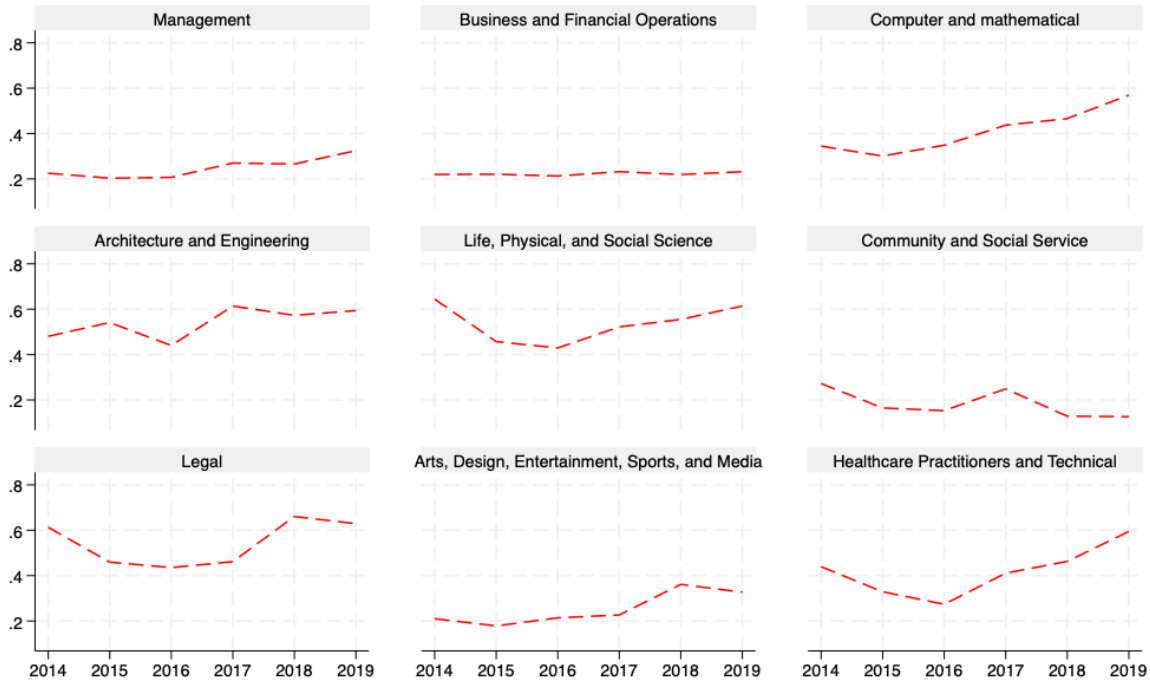
Notes: The table reports mean OPT-to-H-1B transition time by degree level and fiscal year, separately for STEM and non-STEM petitions, together with the STEM minus non-STEM difference. Transition time is measured as the number of days between the LCA received date and the I-129 action date for records linked through the LCA case number. Because these dates do not coincide with the beginning and end of actual OPT employment, the measure captures elapsed time in the observable OPT-to-H-1B transition rather than exact OPT duration. The sample period covers 2017 to 2020, the years for which LCA received dates are available in the records published by the Department of Labor. Stars denote the significance of a two-sided test of the difference in means. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A25. Mechanism: Transition Time and Labor Market Outcomes, 2017–2020

	Private Sector Employment	Retain Employment	Annual Salary
	(1)	(2)	(3)
Panel A: All Degrees			
Log Days to I-129 Action	0.0289***	0.0192	0.0035
	(0.0037)	(0.0134)	(0.0052)
N	205,707	2,321	186,142
Y Mean	0.96	0.86	11.09
Panel B: Bachelor's Degree			
Log Days to I-129 Action	0.0199***	-0.0326	0.0209**
	(0.0031)	(0.0301)	(0.0084)
N	19,322	220	16,917
Y Mean	0.99	0.88	10.98
Panel C: Master's Degree			
Log Days to I-129 Action	0.0157***	0.0239*	-0.0085**
	(0.0024)	(0.0143)	(0.0039)
N	166,392	1,898	150,687
Y Mean	0.99	0.85	11.09
Panel D: Doctorate Degree			
Log Days to I-129 Action	0.0762***	0.0152	0.0489***
	(0.0057)	(0.0344)	(0.0056)
N	19,993	203	18,538
Y Mean	0.72	0.91	11.20
Field of Study FE	✓	✓	✓
Fiscal Year FE	✓	✓	✓
Individual Characteristics	✓	✓	✓

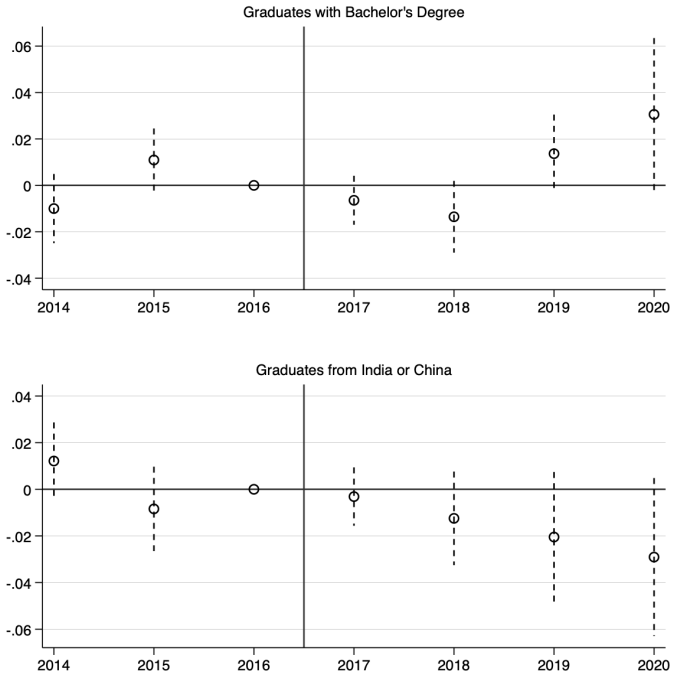
Notes: The table presents estimates relating OPT-to-H-1B transition time to labor market outcomes using merged I-129 and LCA data. Transition time is measured as the log number of days between the LCA received date and the I-129 action date for records linked through the LCA case number. Because these dates do not coincide with the beginning and end of actual OPT employment, the measure captures elapsed time in the observable OPT-to-H-1B transition rather than exact OPT duration. The sample period covers 2017 to 2020, during which LCA received dates are available in the records published by the Department of Labor. All specifications include field-of-study and fiscal-year fixed effects and control for gender, degree level, and country of birth. Standard errors are clustered by major at the 6-digit CIP code level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Figure A1. Lottery Winning Rate by 2-Digit SOC Occupation Categories



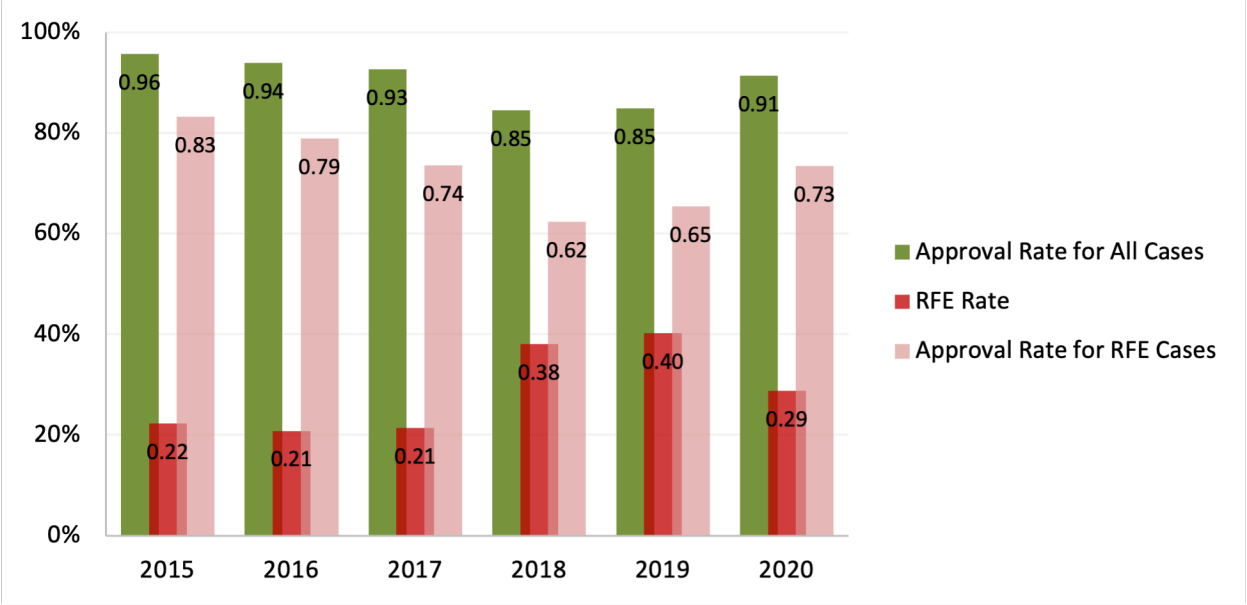
Notes: The figure plots the yearly trend in the H-1B lottery winning rate at the aggregated occupation-year level by 2-digit SOC occupation categories. STEM eligibility is specified according to the 2010 Census STEM and STEM-related Occupation Code List. The H-1B lottery winning rate is calculated as $\frac{\text{I-129 petitions}}{\text{LCA filings}}$, using LCA and I-129 data that are collapsed and merged at the occupation-year level.

Figure A2. Composition of STEM Applicants



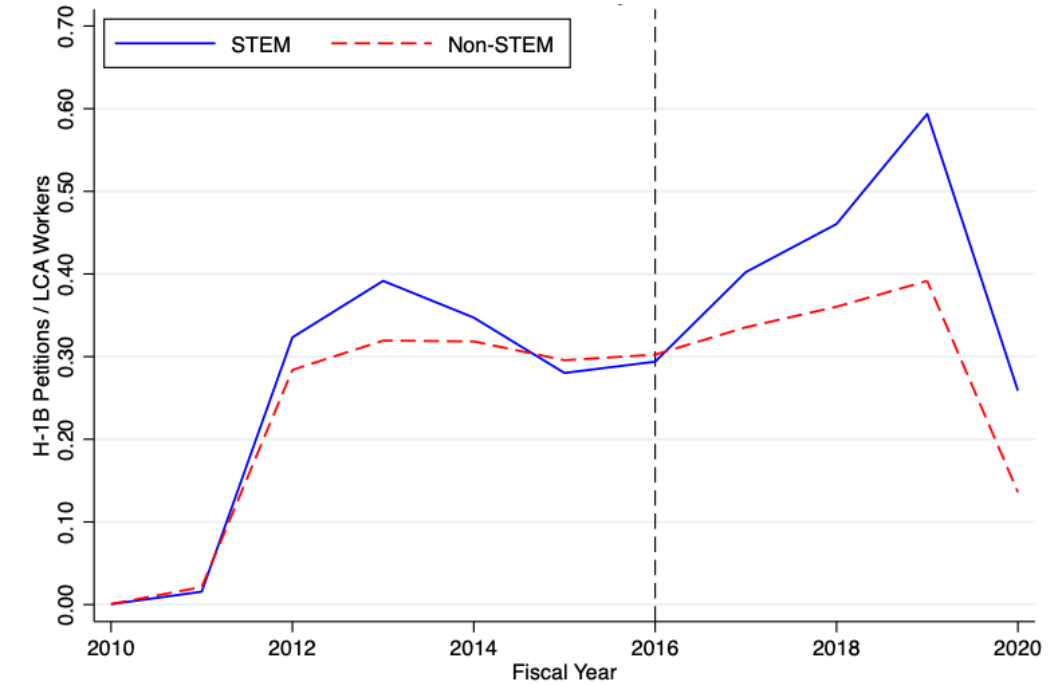
Notes: The figure plots coefficient estimates and 95% confidence intervals from regressions of educational attainment and country of origin on interactions between STEM status and fiscal-year indicators. The sample period is 2014–2020, with 2016 as the reference year. All specifications include field-of-study and fiscal-year fixed effects. Standard errors are clustered by major at the 6-digit CIP code level. Joint tests of the 2017–2020 coefficients are reported in Appendix Table A7.

Figure A3. Mechanism: The Role of Requests for Evidence (RFE)



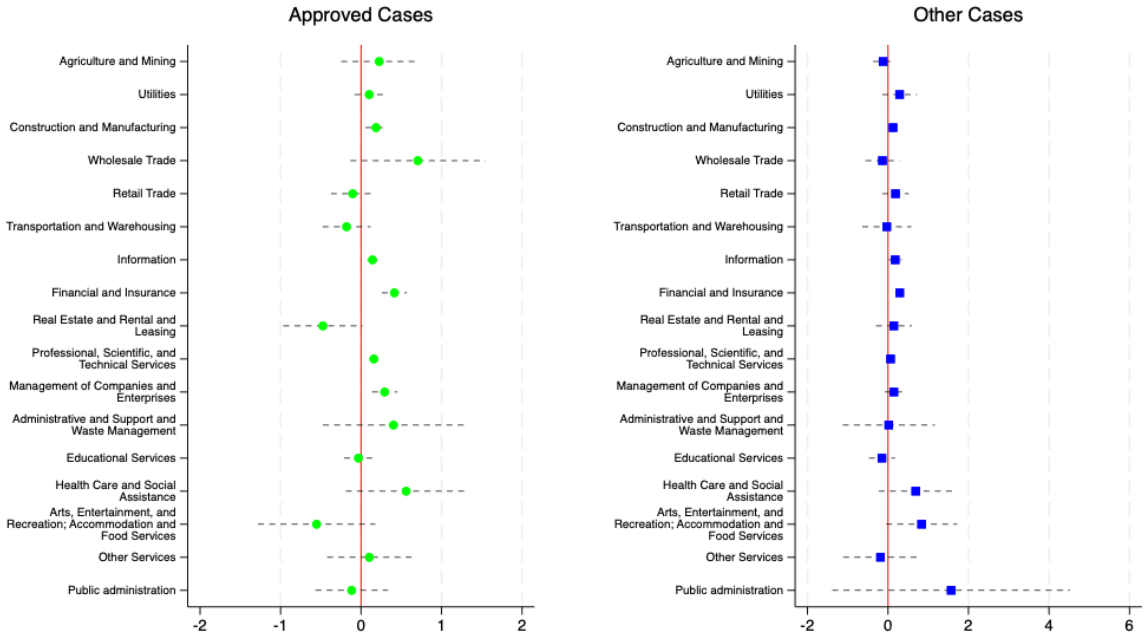
Notes: Authors' calculations based on USCIS I-129 - Petition for a Nonimmigrant Worker Specialty Occupations (H-1B) by Fiscal Year, Month, and Case Status: October 1, 2014 - September 30, 2020, https://www.uscis.gov/sites/default/files/document/reports/I129_Quarterly_Request_for_Evidence_FY2015_FY2020_Q4.pdf The approval rate for all cases indicates the percentage of petitions approved out of total completions which include initially approved and initially denied cases. The RFE rate indicates the percentage of completions with an RFE out of total completions. The approval rate for RFE cases indicates the percentage of petitions with an RFE that were approved out of all completions with an RFE. The figure covers the sample period from 2015 to 2020, during which information on requests for evidence was available as published by USCIS.

Figure A4. Petition-to-LCA Ratio by STEM Status



Notes: This figure displays the yearly petition-to-LCA ratio by STEM status from 2010 to 2020. The vertical dashed line indicates the 2016 policy reform.

Figure A5. Mechanism: Post-2017 Salary Effects by Industry



Notes: The figures display the regression coefficients and corresponding 95% confidence intervals for the post-2017 dummy variable. The sample period covers 2014 to 2020 and includes cap-subject workers. The dependent variable is the log of annual salary. All salaries are adjusted to constant 2010 dollars and capped between 0 and 1,000,000. All regressions include field-of-study, fiscal-year, and state fixed effects. Estimates also control for individual characteristics including gender, degree level, and country of birth and apply robust standard errors.