International Students and the U.S. Skilled Workforce: Evidence from Optional Practical Training*

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Abstract

U.S. higher education has become an increasingly important point of entry into the skilled labor market for foreign workers, while employer-sponsored immigration has remained constrained. I study whether Optional Practical Training (OPT), which allows international students to work temporarily after graduation, expands the U.S. skilled workforce or simply shifts employment across immigration routes and workers. To answer this question, I construct new linked data that follow international graduates' U.S. employment across immigration routes and beyond OPT. Using changes in university program classifications that make graduates eligible for up to two additional years of OPT, I find that eligibility adds 5.3 months of U.S. employment per international graduate over the first three years after graduation. Roughly one-quarter of the additional OPT work replaces work that would have otherwise been done through other immigration routes. In a complementary firm design, I find no evidence that OPT workers crowd out other workers at the firms where they are employed. Applied to a proposed OPT fee, these estimates imply that work through other immigration routes or by other workers would only partly offset substantial losses of skilled employment.

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1 Introduction

Over the past twenty years, the college-educated foreign-born workforce in the U.S. has more than doubled, and today immigrants account for roughly two-thirds of Silicon Valley's technical workers (U.S. Bureau of Labor Statistics, 2006, 2025; Joint Venture Silicon Valley, 2026). Throughout this rapid growth, however, employer-sponsored immigration has remained constrained by statutory quotas that have stayed fixed since 2005 (Donovan, Kandel and Wilson, 2024). Through which route, then, have firms expanded their employment of skilled foreign workers?

I study one increasingly important point of entry into the U.S. labor market: U.S. higher education. In 2023, nearly half of all college-educated noncitizen workers had earned their highest degree in the U.S., about a 29% increase relative to 2010.[1] For international students, the transition from education to employment operates through Optional Practical Training (OPT), which permits up to three years of work after graduation. Propelled by a rapidly growing international student population and policy changes extending its duration, OPT has grown substantially over the past two decades. By 2024, new OPT work authorizations, including extensions, were nearly twice the number of H-1B approvals for initial employment (Figure 1).

In recent years, this pathway has attracted substantial political scrutiny. Proponents argue that OPT fills skilled jobs in key industries, while critics contend that employers hire international graduates at the expense of native-born workers. The U.S. Department of Homeland Security's October 2026 proposal to charge universities $100,000 for a student to begin OPT and then extend it has given this debate new urgency.[2]

In this paper, I ask whether and how OPT expands the U.S. skilled workforce. Additional authorization need not translate one-for-one into additional U.S. employment: some graduates who work on OPT might otherwise obtain permission to do the same work through another immigration route. Similarly, firms that hire more OPT workers might otherwise fill the same jobs with other workers. Understanding OPT's impact on the U.S. skilled labor market therefore requires following graduates' employment across immigration routes and firms' hiring and employment of other workers.

I use individual-level administrative records from the Student and Exchange Visitor Information System (SEVIS), covering the universe of international students on F-1 visas

[1] Author's survey-weighted calculations from the 2010 and 2023 National Survey of College Graduates. The population comprises college-educated noncitizens residing in the U.S. and working for a U.S. employer, including permanent and temporary residents.

[2] The proposed rule would charge a university $70,000 to recommend a student for OPT and $30,000 for each later recommendation (U.S. Department of Homeland Security, 2026).

from 2006–2022.[3] These records describe students' degree programs and work authorizations, including their use of OPT. A central obstacle is that they do not follow graduates' employment after their period as an F-1 student or OPT participant ends. The released files also omit students' names, making them difficult to link to external data on subsequent labor market outcomes. I overcome these hurdles by linking the student records to likely matches among individual LinkedIn profiles supplied by Revelio Labs. Although student names are withheld, the administrative records retain OPT employer names alongside students' schools, fields of study, program dates and nationalities. I use education histories to identify plausible profiles, then compare employment histories, fields of study and inferred country of origin to distinguish among potential matches. The resulting links allow me to follow employment beyond OPT and distinguish work during further study from work through other routes.[4]

To study employers' responses, I combine the administrative records to follow each employer's OPT hiring over time and link employers by name and location to hiring and employment records from LinkedIn. I also link these employers to administrative records on their demand for H-1B workers and approved petitions. This separate employer linkage lets me examine firms' hiring and employment alongside their demand for skilled foreign workers through multiple immigration routes.

I begin by studying the direct effects of OPT authorization on graduates' own U.S. employment, using variation in OPT eligibility. While OPT originally granted one year of post-graduate work authorization to international graduates, an extension was introduced in 2008 that grants graduates of designated science, technology, engineering, and mathematics (STEM) programs two additional years of authorization.[5] The added authorization gives universities an incentive to seek STEM eligibility for their degree programs. Eligibility depends on the reporting code a university assigns to a program, and related codes can describe similar curricula. An economics program, for example, can move from General Economics to the STEM-eligible code Econometrics and Quantitative Economics without changing its curriculum or advertised degree name. I refer to these changes in reporting codes as "relabeling." Programs relabeled at different times, changing eligibility across successive graduating cohorts. Using a difference-in-differences design, I compare changes in graduates' U.S. employment at relabeled programs with changes at programs in the same fields that have not yet relabeled or never relabel. I reweight these comparison programs to make their

[3] I use 'international student' throughout to refer to students on F-1 visas, the primary U.S. student visa.

[4] I give more weight to linked graduates whose recorded characteristics make them less likely to be matched. The main employment results also hold after adjusting these weights so that OPT participation among linked graduates matches participation in the full student records; Sections 3 and 4.7 describe the linkage and its checks.

[5] The extension initially added 17 months when it was first introduced and was lengthened to 24 months in 2016.

characteristics before relabeling similar to those of the programs that relabel.

The employment effect of longer OPT depends on whether graduates use the additional authorization and what they would have done without it. I compare the increase in work on OPT with the increase in total U.S. work to separate additional employment from work that shifts across immigration routes, then ask whether the gains persist after OPT ends. Relabeling substantially increases international students' use of OPT. Before relabeling, about half of international graduates in programs that later relabeled used OPT, but virtually none used the extension. After relabeling, graduates become 10.2 percentage points more likely to use OPT at all [SE 1.5] and 23.6 points more likely to use the extension [SE 1.7], relative to graduates in comparison programs.

Over the first three years after graduation, relabeling increases recorded U.S. employment by 5.3 months [SE 1.4] per graduate, while work on OPT increases by 7.2 months [SE 0.9]. The estimated ratio is 0.73 [SE 0.15], implying that, on net, roughly seven in ten additional months worked on OPT add to U.S. employment, while the remainder replaces work graduates would otherwise have done through another route.[6] The employment gains are concentrated in the first three years after graduation. In the separate sample followed for five years, years four and five add an estimated 1.0 month of work [SE 0.8], consistent with continued gains beyond OPT. The main three-year results are robust to alternative estimators, linkage weights and choices of comparison programs. Domestic graduates of the same programs have small, imprecise employment responses, providing a comparison for changes that could affect both groups.

I next turn to the employers that hire these graduates, asking whether their recruitment through OPT comes at the expense of other workers. To study this question, I use broader growth in programs eligible for the STEM extension and firms' earlier hiring ties to the schools offering them. When a school graduates more students from these programs, firms that have previously recruited heavily from it gain access to more potential workers. I compare firms with large and smaller increases in this hiring pool, accounting for differences in their earlier hiring and employment. These programs train domestic as well as international students, so the comparison follows how firms respond to an expanding supply of U.S.-educated graduates.

The main results show no evidence of crowdout of other workers at these firms. Recruitment through OPT rises by 29.1% [SE 4.0], while average monthly employment of other college-educated workers changes by 0.14 workers per firm [SE 0.16], relative to a mean of

[6] One alternative is further study, which can provide work authorization during and after another degree: work through later student programs falls with extension eligibility, and the administrative records independently show less time in further study. Longer OPT can also give graduates more time to pursue employer sponsorship; recorded requests to change to H-1B status rise by 3.6 percentage points [SE 0.9], from 13.7%, although these requests are undated and do not establish approval.

25.3 without the larger expansion.[7] Firms hire more native recent graduates and student interns, with no detectable decline in employment of native college-educated workers or in hiring of other native workers and foreign workers educated abroad. The increase in H-1B requests is consistent with firms seeking to retain some of their OPT recruits through employer sponsorship.[8]

Finally, I use these estimates to assess a hypothetical fee of $70,000 for initial OPT and $100,000 for initial OPT followed by the extension. The employment a fee removes depends on how many graduates forgo OPT, how much additional U.S. work it would have allowed them to do, and how other workers respond. Even though each graduate's additional employment is concentrated in a few years, successive cohorts sustain a larger workforce. Combining the participation scenarios in Clemens (2026) with the estimated five-year employment response gives an annual loss of about 76,000 worker-equivalents [SE 26,000] when students bear the fee and 170,000 [SE 58,000] when universities do, by the fifth policy year. These projections assume the same employment response for all deterred participants and no net change in other workers' employment. A richer model allows initial OPT participation and extension use to respond separately and permits work through other routes to adjust; its corresponding fifth-year losses are about 62,000 and 139,000 worker-equivalents.

Related Literature. This paper contributes to two main strands of literature. First, a growing body of literature studies international students in the U.S. Much of this work focuses on international students as inputs into the higher education system, studying how international enrollment impacts university revenue and domestic student outcomes (Shih, 2017; Bound et al., 2020; Chen, 2021; Anelli, Shih and Williams, 2023; Avdeev, Forthcoming).[9] Within this literature, studies of STEM OPT reforms find increases in international enrollment, particularly at selective schools and in STEM majors, as well as increases in domestic STEM degree completions (Amuedo-Dorantes, Furtado and Xu, 2019; Amuedo-Dorantes, Shih and Xu, 2023; Khoo, 2025; Amuedo-Dorantes and Shih, 2026). Most closely related to my first empirical design, other papers study the phenomenon of program 'relabeling' in Economics or

[7]The employment measure excludes workers who first joined as foreign recent U.S. graduates or foreign student interns, and the estimate permits a decline of about 0.18 workers per firm. A separate analysis of existing employees is sensitive to adjustment for earlier retention histories, with larger declines under additional adjustments (Appendix D.3).

[8]The employer records do not trace individual transitions from OPT to H-1B. The comparison concerns established firms with prior recruiting ties; complementary evidence from Kwon, Lee and Monras (2026) finds increases in firm counts and native high-skilled employment in exposed local labor markets following the 2016 extension expansion.

[9]Relatedly, several papers study the upstream determinants of international enrollment (Shih, 2016; Chen, Howell and Smith, 2023; Khanna et al., 2025; Jia et al., 2025). See Bound et al. (2021) for a full review of work on international students in the context of the U.S. higher education system.

Business in response to STEM OPT eligibility.[10] I build on this literature by systematically documenting these relabeling events across fields and showing what they do to graduates' authorization, employment and immigration status.

A related literature studies the transition from U.S. higher education into the workforce. Bound et al. (2015*b*) document the role of U.S. higher education in supplying foreign IT workers and the constraints imposed by work visas. Beine, Peri and Raux (2023) use variation in tuition to estimate how increases in international graduates translate into first-time OPT employment.[11]

At the individual level, several papers examine how the STEM OPT extension affected labor market outcomes by comparing graduates of different majors, or workers in different occupations, before and after national policy changes (Demirci, 2019, 2020; Guo, Gong and Pang, 2024).[12] Demirci (2019) uses administrative data to show that the 2008 STEM OPT extension induced a small increase in eligible graduates' likelihood of using OPT at all and a large increase in their likelihood of using the extension, consistent with my results on OPT usage. Using LinkedIn career histories, Chen (2026) studies how the same reform affected graduates' first U.S. jobs, subsequent retention and the match between their education and employment.

Other papers study how OPT and changes in the international student population affect local labor markets (Peri and Basso, 2016; Kwon, Lee and Monras, 2026). Kwon, Lee and Monras (2026) study the 2016 expansion across local labor markets (commuting zones) with different pre-reform concentrations of OPT-eligible international students. They find increases in high-skilled immigrant labor supply, the number of firms, and the employment and wages of native high-skilled workers. Evidence on native workers differs across settings: Demirci (2020) finds reduced employment among recent native master's graduates but higher earnings among experienced native master's graduates in the same fields. After Canada relaxed post-graduation work restrictions, Qiu (2026) finds lower employment in the year of graduation and lower earnings over the first three years for native graduates in regions more exposed to international graduates. I complement this work by examining recruitment, sponsorship and

[10] Kim (2022) finds an increase in the number of STEM extension-eligible 'Management Science' programs following the major's addition to the DHS list in 2012. Hu and Winters (2025) find that the introduction of STEM-designated economics programs increases economics degrees awarded to international students. Underwood, Marshall and Bilen (2026) find increases in both the number and share of economics degrees awarded to international students and women. Mahon and Asarta (2024) and Kim (2025) find that schools with higher baseline shares of foreign students and large, selective, research-intensive schools relabel Economics programs more often.

[11] In the U.K., Ma and Wang (2026) combine evidence on enrollment with a model of education and work choices to study how post-study work restrictions affect where international students study and work.

[12] Relatedly, Beine, Peri and Raux (2024) use changes in tuition to study how a larger international student share affects a cohort's entrepreneurship, focusing on master's programs.

employment of existing workers at established firms, while the commuting-zone estimates capture broader local changes in firm counts and native labor-market outcomes.

This work motivates questions that require following graduates beyond just their usage of OPT: does OPT affect U.S. employment, or simply replace other immigration channels? By linking student records to authorization and career histories, I estimate how much of the additional work on OPT represents additional U.S. employment, accounting for work through other routes. The separate employer analysis examines recruitment, sponsorship and employment at established firms as their graduate pools expand.

Second, this paper contributes to a large literature on the effects of high-skilled immigration on domestic labor markets. Much of this research points to skilled immigrants having a positive effect on firm productivity and innovation, with limited employment crowd-out of native workers. For instance, skilled immigration has been linked to increased patenting, knowledge spillovers, and firm expansion and entry across a variety of settings (Hunt and Gauthier-Loiselle, 2010; Moser, Voena and Waldinger, 2014; Beerli et al., 2021).[13] Other work, however, finds that productivity effects differ across industries (see, e.g., Paserman, 2013), or that skilled immigration may adversely affect some groups of existing workers (see, e.g., Borjas and Doran, 2012).

In particular, much of the evidence on the effects of contemporary skilled immigration on the U.S. labor market comes from the H-1B program. At an aggregate level, this literature documents gains in innovation, productivity, and native welfare (Kerr and Lincoln, 2010; Peri, Shih and Sparber, 2015; Bound, Khanna and Morales, 2018; Abramitzky et al., 2026). Evidence on individual firms is more mixed. Studies using the H-1B lottery find that access to these workers supports growth, survival and start-up performance in some settings (Chen, Hshieh and Zhang, 2021; Dimmock, Huang and Weisbenner, 2022; Mahajan et al., 2024), while Doran, Gelber and Isen (2022) find displacement of other workers and limited innovation gains. Using firm-level data, Kerr, Kerr and Lincoln (2015) find that hiring young skilled immigrants is associated with firm expansion and a shift toward younger rather than older skilled native workers. A calibrated model in Bound et al. (2015*a*) also finds that access to foreign IT workers can reduce employment of domestic IT workers. Additional work studies how firms adjust when H-1B hiring is restrained, finding that firms respond by retaining incumbent H-1B workers, moving work abroad through offshoring, and acquiring other firms to obtain skilled employees (Mayda et al., 2023; Glennon, 2024; Chen, Hshieh and Zhang, 2024).

[13] Related work documents immigrants' disproportionate contributions to innovation and entrepreneurship (Azoulay et al., 2022; Bernstein et al., Forthcoming).

These papers largely study changes to a single immigration program, but the resulting employment effects also depend on the other routes available to workers and firms. I contribute to this literature by examining how substitution between immigration routes and transitions into employer sponsorship shape the employment effects of OPT. Accounting for these responses is particularly important when using historical estimates to project the effects of new immigration restrictions. In an application to a hypothetical OPT fee, my simulations illustrate how work through alternative routes can partly offset the loss of OPT employment, while fewer transitions into H-1B employment can extend that loss beyond the OPT period.

The remainder of the paper proceeds as follows. Sections 2 and 3 describe the institutional background and data. Section 4 estimates the effect of eligibility for an extension to OPT on graduates' U.S. employment, while Section 5 examines firms' hiring and employment responses as eligible programs at their recruiting schools expand. Section 6 considers the implications for the U.S. workforce and a hypothetical OPT fee. Section 7 concludes.

2 International Students and Optional Practical Training

The U.S. places no annual numerical cap on F-1 visas, the principal visa category for academic study and the focus of this paper.[14] Students' opportunities to work, however, are governed by restrictions on the timing, duration, and type of employment. In this section I begin by describing the growth and composition of the international student population. I then explain the work authorization available through OPT and its STEM extension, before examining the other routes graduates can use to remain and work in the U.S. and their use after graduation.

2.1 The Growth of International Students in the U.S.

The international student population has grown rapidly over the past twenty years (Appendix Figure A.1). Between 2004 and 2025, international students and OPT participants roughly doubled both in number (from 573,000 to 1.18 million) and as a share of total higher-education enrollment (from 3.4 to 6.1%). India and China account for much of this growth, together representing more than half of the international total in 2024–2025 (Institute of International Education, n.d., 2025).

[14] International student visas are divided into three categories: J visas, for exchange students or temporary researchers, M visas, for vocational training or study at nonacademic institutions, and F visas, for full-time students enrolled in degree-granting educational programs or language training at authorized academic institutions.

Recent growth has also been unevenly distributed across different types of postsecondary programs. Bachelor's and master's programs make up the bulk of international graduates, and recent growth has been particularly concentrated in master's programs (Appendix Figure A.1, panel b). Although the absolute number of international students in research-doctorate programs is smaller, international students receive more than a quarter of all U.S. research doctorates. International students make up a small share of those earning professional doctorates and associate's degrees, so I exclude both from my main analysis.[15] Finally, growth has also been concentrated in a relatively small set of majors. The ten fields with the largest increases account for 60% of the overall rise in international bachelor's, master's, and research-doctorate completions between 2005 and 2024.[16]

2.2 Optional Practical Training

Even prior to the creation of the F-1 visa, foreign students were permitted to remain in the U.S. for a limited time following graduation for "employment for practical training". This was formalized in 1992 through the Optional Practical Training (OPT) program, which allows students on F-1 visas to work in the U.S. for up to 12 months to complement their education (U.S. Immigration and Naturalization Service, 1947, 1992). Unlike most forms of temporary work authorization in the U.S., such as the H-1B visa, OPT does not require an employer to apply on the student's behalf. Instead, a school official recommends the student for OPT, and the student applies to the federal government for work authorization. Once approved, the student can move between qualifying employers and may also be self-employed, as long as the work remains related to the degree. Because standard OPT is not tied to a specific employer, there is also limited wage oversight: unpaid positions are permitted and there is no salary threshold or requirement for comparable pay. [17]

STEM OPT Extension. In 2008, the Department of Homeland Security (DHS) introduced an optional 17-month extension for students graduating with qualifying STEM degrees; in 2016, this extension was expanded to 24 months, allowing eligible graduates to work for up to three years in total. Under the 2016 rules, the STEM extension places more requirements on

[15] International students pursuing associate's degrees may also enter under the M-1 rather than the F-1 visa, complicating analysis based on F-1 records.

[16] Computer science, related information fields, and quantitative business programs account for the largest shares, followed by programs in data analytics, economics, applied mathematics, and engineering.

[17] OPT workers and their employers may also be exempt from Social Security and Medicare taxes while the worker remains a nonresident for tax purposes, generally during a worker's first five years of residency in the U.S. (Internal Revenue Service, n.d.). Students must report changes in employment and may spend no more than 90 days unemployed during the initial OPT period after graduation (U.S. Immigration and Customs Enforcement, 2010).

employers than the initial OPT period. Each qualifying employer must employ the student for at least 20 hours per week in a genuine employer–employee relationship, complete a formal training plan with the student for submission to the school, and enroll in E-Verify, an electronic system that checks permission to work against DHS and Social Security Administration records.[18]

2.3 Other Pathways to U.S. Work After Graduation

What are the other channels international students could use to work in the U.S. after graduation, either in place of OPT or after OPT expires? Figure 2 maps the routes open to an F-1 graduate, and Figure 3 shows which of them linked graduates hold in each of the five years after graduation. Below, I describe the available routes and their relevance to international students.

Additional Education. International students can enroll in a new qualifying program to restore their F-1 status and a new degree may make them eligible for another period of OPT.[19] Students can also work *while enrolled* as an F-1 student. They may work on campus for up to 20 hours a week during the academic term, and off campus through OPT or, more commonly, a separate program called Curricular Practical Training (CPT).[20] Unlike OPT, work on CPT must be an "integral part of an established curriculum" (U.S. Department of Homeland Security, 2023).[21] Graduate programs that require immediate practical training, however, may authorize CPT from the first semester, and some schools have built master's programs around this provision (so-called 'day-one CPT' programs).[22]

H-1B. The H-1B visa is an employer-sponsored temporary work visa for skilled foreign workers. Unlike OPT, an employer applies on the worker's behalf, and authorization is tied

[18]The student's duties, hours and pay must also be comparable to those of U.S. workers in similar positions. Unlike H-1B sponsorship, however, the extension does not require a separate application to the Department of Labor (U.S. Department of Homeland Security, 2008, 2016).

[19]To be eligible for another period of OPT, the new program must be at a higher degree level. Under the 2016 rules, the STEM extension may only be used twice over a lifetime.

[20]Among linked 2010–2017 graduates, 97% of recorded U.S. work months during enrollment with either CPT or pre-graduation OPT authorization have CPT authorization. OPT used before graduation reduces the authorization available after graduation.

[21]Examples of work that might be considered "integral" to a degree program include nursing clinical placements and teaching practicums. In practice, many schools offer courses that allow students to receive credit for any summer internship, making them eligible for CPT (Boston College, Office of International Students and Scholars, n.d.; DePaul University, International Student and Scholar Services, 2023). Twelve months or more of full-time CPT makes a student ineligible for OPT at that degree level.

[22]Among graduates in the programs and years used in my employment analysis, 0.7% have CPT authorization beginning within 30 days of enrollment.

to employment with that sponsor. The H-1B also allows 'dual intent': workers may pursue permanent residence while holding temporary work status, whereas F-1 applicants must intend to leave the U.S. after their studies and authorized practical training. Employers face both limits on visas and costs of sponsorship. For most private employers, new H-1B approvals are limited to 85,000 per year (consisting of 65,000 places plus 20,000 reserved for holders of qualifying U.S. master's or higher degrees); when demand exceeds this cap, employers enter a lottery held each spring for work that generally begins in October.[23] Employers must first file a Labor Condition Application with the Department of Labor, attesting that they will pay at least the higher of the prevailing wage or the wage paid to comparable employees, and that hiring the worker will not harm other employees' working conditions. They must also pay petition fees and any legal or administrative costs of sponsorship.[24]

OPT provides a bridge from graduation to H-1B employment. Former F-1 students accounted for 30% of initial-employment H-1B approvals in 2008 and nearly 60% in 2018 (Appendix Figure A.1). OPT lets a graduate work while an employer enters the lottery, and the STEM extension adds up to two further attempts before authorization runs out.[25] Graduates of qualifying U.S. master's and doctoral programs also have better chances of selection because they qualify for the additional places reserved for them.

Other statuses in the U.S. Graduates can also obtain work authorization through an employer or through family ties. Employer-based routes include permanent residence and several temporary visas that are not subject to the H-1B cap. The L-1 allows multinational employers to transfer workers from a related company abroad, providing a route back to the U.S. for graduates who leave to work overseas.[26] The O-1 is available to individuals of extraordinary ability, including qualifying startup founders.[27] Employer-sponsored permanent residence usually requires the employer to obtain labor certification from the Department of Labor. Family ties offer another route to permanent residence, including through marriage, or to status as another visa holder's dependent. Some dependent statuses allow work, while

[23] Universities and certain affiliated nonprofit and research employers are exempt from the cap (Donovan, Kandel and Wilson, 2024).

[24] In September 2025, after the period studied here, a presidential proclamation introduced an additional $100,000 payment requirement for certain H-1B petitions for workers outside the U.S. (Executive Office of the President, 2025).

[25] For the graduates studied here, 'cap-gap' provisions could extend a qualifying student's F-1 status and existing work authorization through September 30 while awaiting an October 1 H-1B start date.

[26] The L-1 generally requires one continuous year of qualifying employment abroad within the preceding three years. A graduate could therefore join a multinational's foreign office after leaving the U.S. and later return through an intracompany transfer.

[27] For example, John Collison, cofounder of the payments company Stripe, obtained an O-1 visa in 2010 after leaving Harvard to work on the company (McBride, 2012).

others permit residence but not employment.[28]

Which routes graduates take. Figure 3 follows bachelor's, master's and doctoral graduates from 2010–2017 whose student-visa records I link to professional profiles. I combine information on their visas and U.S. jobs to describe their main route in each year after graduation.[29] OPT is the most common route immediately after graduation, covering 54% of graduates in the first year, but graduates follow several paths as it expires. By the fifth year, 23% are inferred to hold H-1B status, while about a third hold F-1 status through a subsequent program or work in the U.S. through other routes. These paths show why OPT's effect on U.S. employment depends not just on access to H-1B visas, but also on graduates' ability to study and work through other routes.

3 Data and Linkage

To follow employment across immigration routes, I construct two datasets that link administrative F-1 records to worker-level work and education histories. In this section I give an overview of the data sources and linkage methods that I use to construct these datasets. Appendix B provides further details.

3.1 Data Sources

F-1 Administrative Data (SEVIS). The backbone of both datasets is an individual-level administrative database on F-1 visa holders: the Student and Exchange Visitor Information System (SEVIS). This is a centralized administrative database used by schools to report details about F-1 students' programs, funding, and authorization for off-campus employment (including OPT) to Immigration and Customs Enforcement (ICE). I use a snapshot of this database from 2023, obtained by the Institute for Progress from ICE via a Freedom of Information Act (FOIA) request (U.S. Immigration and Customs Enforcement, 2024; Institute for Progress, n.d.). The data cover the universe of international students in the U.S. on F-1 visas from 2006–2022, spanning 68 million rows and an estimated 4.6 million individuals. The records report citizenship, schools and locations, fields and degree levels, program dates, tuition, fees, and funding sources. They also report work-authorization types and dates and

[28] Certain H-4 spouses of H-1B workers may apply for work authorization. F-2 dependents of students cannot work in F-2 status.

[29] The figure uses the route accounting for the largest part of each year, which need not exceed half the year. H-1B periods are inferred from requests to change status; the records do not contain approvals. Other U.S. work is identified from jobs reported outside OPT, H-1B, and a subsequent student program and its associated OPT. Graduates without a recorded route have not necessarily left the U.S.

employer names and locations, although student names are withheld. Requests to change immigration status provide information on subsequent routes, including H-1B, but do not establish that a request was approved.[30]

School-Program Administrative Data (IPEDS). For analysis at the school-program level, I use data from the Integrated Postsecondary Education Data System (IPEDS) (National Center for Education Statistics, 2026*a*,*b*). Throughout the paper, I define 'schools' using IPEDS institutions, combining branch locations with their main campus.[31] For my analysis, I use IPEDS completions data, which reports the number of degrees awarded in each academic year by school, program (I limit my analysis to bachelor's, master's, and research doctorate programs), and field of study (as defined by six-digit codes in the Classification of Instructional Programs, or CIP). The data also separately report completions by domestic students and international students.[32] IPEDS tuition and fee data supplement the costs reported in SEVIS.

Individual- and Employer-Level Outcomes Data (Revelio). My primary source of outcome data is Revelio Labs' individual profile dataset (Revelio Labs, 2026*b*,*a*). For individual graduates, these data list detailed work and education histories ("profiles") from LinkedIn, observed as of September 2026. Education histories report institutions, fields, degree levels, and dates, and work histories report employers, positions, locations, and employment dates. The histories can reveal work after a graduate's time in F-1 status ends, including work outside the U.S. For the employer analysis, I group profiles by company, combining duplicate Revelio company entries that describe the same operating employer.

LinkedIn coverage is not universal, but it is comparatively high among college-educated workers and in the sectors that employ many international graduates (Gottfried, 2024; Amanzadeh, Kermani and McQuade, 2024; Zhu, Fritzler and Orlowski, 2018). In a comparison with IPEDS, profile education counts with matched schools and fields amount to about 55% of bachelor's, master's and doctoral degree awards over 2008–2024. The distribution across degree levels and broad fields, measured by the first two digits of the CIP code, also resembles the national distribution, especially in the categories containing the study programs (Appendix B.1.2). I assess selection into the linked F-1 sample below and examine how

[30] Requests are undated and do not capture all later statuses: graduates who obtain a visa through consular processing or after their student record closes may have no corresponding request. Appendix C describes how I use this information to classify work by immigration route.

[31] IPEDS covers institutions participating in Title IV federal student aid programs. Some schools eligible to enroll F-1 students, such as standalone language schools, are outside this universe.

[32] IPEDS defines international students by nonresident status, while SEVIS covers F-1 students. Degree awards and administrative program records therefore need not coincide; Appendix B.1.1 compares the two sources.

completely its profiles record work in Appendix B.2.2.

Additional Administrative Immigration Records. I supplement these sources with administrative data on employers' use of employer-sponsored immigration. To measure demand for H-1B visas, I use the number of positions requested on Labor Condition Applications (LCAs), a prerequisite for H-1B visas and a commonly used proxy for demand (see e.g. Kerr and Lincoln, 2010). For the lotteries held in 2020–2023, I also observe employers' H-1B registrations in USCIS records obtained and released by Bloomberg News under FOIA (U.S. Citizenship and Immigration Services, 2024*b*).[33] To measure approved H-1B petitions, I use data obtained from USCIS via FOIA request and generously provided by Neil Ruiz. Approved petitions can include continuing employment and do not count distinct new immigrants. I also use applications for permanent labor certification, commonly called PERM applications, to measure employer sponsorship for permanent residence (U.S. Department of Labor, n.d.; U.S. Department of Labor, Office of Foreign Labor Certification, n.d.).

3.2 Individual Linkage

The SEVIS records contain no student names, but their education and employment histories provide information that can identify corresponding profiles in the Revelio data. I first match schools in both data sources to IPEDS using names, locations, and relationships between campuses, and classify reported majors using CIP codes.[34] For a given F-1 record, I construct a shortlist of profiles with a plausible education history at the same school, comparing degree level, graduation timing, and field. OPT employers, other schools in a student's history, and evidence on country of origin help distinguish the remaining candidates; a shared uncommon employer is particularly informative.[35] I combine this information to select a profile when the evidence is sufficiently strong, resolving competing claims on the same profile in favor of the stronger link. Appendix B.2.1 describes this selection rule and how I avoid counting a profile twice within a program and graduating cohort.

In the programs and graduating cohorts used for the main three-year employment analysis, linked profiles provide employment outcomes for 52,890 of 409,727 degree records, or 12.9%.[36] Graduates who use OPT are more likely to be linked because their employers

[33]These records cover fiscal years 2021–2024. A worker could have registrations submitted by several employers during this period, so registrations do not count distinct workers.

[34]School matches account for different names and branch campuses; reported majors are mapped to a common field classification. Appendix B.2.1 describes these steps.

[35]Country evidence in profiles comes from education, work, locations and available names.

[36]These counts include treated and comparison programs and count each degree record once across the three annual employment estimates. They represent 396,090 people, of whom 49,482 contribute linked histories; a person can appear with more than one degree.

provide additional evidence for matching. If these graduates also work more in the U.S., an unweighted average of their outcomes need not represent all graduates. For programs used in the individual analysis that gained eligibility for the OPT extension, I first compare linked graduates with the full administrative records in the same graduating classes (Appendix B.2.2). Their degree and field shares are similar, but linked graduates are more likely to have recorded OPT authorization: within classes with a link, this share is 20.1 percentage points higher before weighting.

I estimate each graduate's probability of linkage using citizenship, degree level, graduation cohort, field, the composition of the graduating class, and administrative education and employment histories. I then apply inverse probability weights, weighting each linked graduate by the inverse of this probability when averaging outcomes within programs and graduating cohorts.[37] For the weighted average to represent the full cohort, linked and unlinked graduates with the same observed characteristics must have the same average outcomes.

The weights reduce the excess share with recorded OPT authorization from 20.1 to 13.2 percentage points and narrow the linked sample's larger response in extension use. The National Survey of College Graduates (NSCG) provides an independent benchmark for subsequent U.S. presence in a broader population; weighting brings bachelor's and master's recorded work or study closer to that benchmark. The main employment results are robust to further adjusting these weights to match OPT participation in the full administrative sample.

3.3 Employer Linkage

For the employer analysis, I link SEVIS employers and the H-1B, LCA, and PERM records to companies in the Revelio data using names, other names under which the same business operates, locations, and other identifying information. I standardize the names and use the other identifiers to distinguish similarly named employers, reviewing links that are not straightforward. These direct employer links do not require successful links between individual students and profiles. A firm that I can identify but that has no corresponding administrative record is recorded as zero when the source covers the activity; a firm that I cannot identify is left missing.[38]

[37] Class composition includes the share of other graduates in the same program and class who have the same citizenship. This captures differences in how distinctive a student's country information is among otherwise similar students. Appendix B.2.3 describes the predictors and weighting procedure.

[38] Appendix B.3 describes the links and coverage rules.

4 Direct Effects: OPT and Graduates' U.S. Employment

How does access to OPT affect international graduates' U.S. employment? The answer depends on three components. First, the effect depends on take-up: while policy changes affect the relative cost or benefit of working in the U.S. after graduation, students may still prefer to return home. Second, even among students who use OPT, the effect on their U.S. employment depends on *what they would have done without access to OPT*: if graduates would have worked in the U.S. through a different route, additional work on OPT simply replaces that work. Finally, although OPT provides temporary work authorization, it may help graduates obtain longer-term authorization, such as an H-1B visa, extending its effect beyond the time graduates spend on OPT. In this section, I disentangle these components of the aggregate effect of OPT on U.S. employment using variation in eligibility for a two-year extension to OPT.

4.1 Program Relabeling

Eligibility for the STEM OPT extension depends on a program's six-digit CIP code. These codes are maintained by the National Center for Education Statistics for reporting purposes; NCES supplies a title, a definition and illustrative examples for each code, but schools choose how to classify their programs (National Center for Education Statistics, 2022), and related codes can describe similar curricula. An economics program, for example, can be reported as General Economics, which does not confer STEM OPT eligibility, or Econometrics and Quantitative Economics, which does. This flexibility has allowed universities to 'relabel' programs in order to qualify their international graduates for the extension. The relabelings I study span economics, architecture, business, and design, to name just a few fields (Appendix Table A.1).

To use these changes to study OPT, I need to distinguish a change in classification from a change in the program itself. Schools themselves describe relabelings as changes in classification rather than curriculum. The University of Southern California explained that its undergraduate business programs' move to Management Science "was made because it better reflects the content of the curriculum, not as a result of a curricular change," and Northwestern's economics department wrote that "the curriculum of our degree and the name of our degree are not changing" after their relabeling to Econometrics (USC Marshall School of Business, 2023; Northwestern University Department of Economics, 2024, n.d.). These accounts help establish what the code changes represent. Interpreting the resulting

employment changes as effects of eligibility also requires that relabeling not independently change graduates' prospects through reputation or recruitment, which I assess below.

Why do otherwise similar programs relabel in different years? Relabelings within a field come in waves, both because a program can only be relabeled once a suitable code is on the STEM list and because schools follow their peers. Within a wave, the year a given program is relabeled also depends on its university's internal approval process. A change of code may need approval only from the department, or may also require approval from a college-wide faculty body, the provost's office and, at some public institutions, a state higher education board (National Center for Education Statistics, 2022; Marshall and Underwood, 2022).

Identifying Relabelings. A relabeling moves completions from an existing non-STEM code to a STEM-eligible code, rather than creating or closing a degree program. I identify these changes in IPEDS by looking for a large, persistent fall in degrees reported under the old code alongside a corresponding rise under a previously unused eligible code, supplementing these patterns with program-specific web evidence. Figure 4 shows this shift and how the classifications recorded in IPEDS correspond to eligibility in SEVIS. The administrative analysis includes 335 treated programs at 169 schools first reported under their new codes in 2014–2019.[39]

The ideal comparison programs would offer similar degrees and face similar employment prospects, while their graduates remain ineligible for the extension. I draw them from the same original non-STEM codes, including programs that never relabel and programs that relabel sufficiently later. These programs provide a comparison within the same fields; reweighting on their pre-treatment characteristics addresses observable differences in who they educate, as described below.

Eligibility can change a year before a relabeling appears in IPEDS: degree conferral and SEVIS program completion can fall in different calendar years, and some schools allow recent graduates still on initial OPT to update their code. I therefore allow the cohort immediately before the first relabeled IPEDS year to be partly eligible and use the preceding cohort as the reference.

[39] I require complete IPEDS coverage in the four pre-treatment years, at least five completions across those years, positive F-1 completions in the reference cohort, and an observed Carnegie classification. I exclude unresolved code changes from both treatment and comparison groups. Appendix D.1 describes the identification and review of relabelings.

4.2 Empirical Strategy

I use a difference-in-differences design, comparing cohorts graduating before and after relabeling with the corresponding cohorts in comparison programs. Because relabelings are staggered, I estimate effects separately for programs first relabeled in each year and then combine them. This avoids using already-treated programs as comparisons when effects can differ across programs or over time (Goodman-Bacon, 2021; Sun and Abraham, 2021).

Comparison programs and weights. For each relabeling year g, I form a separate sample, or stack, containing the programs first relabeled in g and the comparison programs that remain untreated throughout the period used to measure the outcome and for one additional year, which allows for eligibility to change before the new code appears in IPEDS. Treated programs are more international than the typical comparison program and their graduates use OPT more often, and employment outcomes plausibly evolved differently for heavily international programs over this period. I therefore reweight comparison programs by inverse probability weighting: I estimate each program's propensity score from pre-treatment program size, student composition, OPT use, enrollment trends, tuition and institutional characteristics, and assign comparison programs their estimated odds of treatment, $\widehat{p}/(1-\widehat{p})$ (Abadie, 2005); Appendix D.2 lists the predictors. Table 1 shows how this changes the comparison: the average international share rises from 19% to 32%, closer to the treated programs' 39%, while OPT use rises from 37% to 53%, compared with 52% in treated programs. Tuition and degree composition also move closer to those of treated programs. Panel B reports additional origin and profile-linkage characteristics that do not enter the comparison weights.

Estimation and aggregation. For program p in stack g and graduation year t, I estimate

$$y_{pgt} = \sum_{e \neq -1} \theta_{g,e} D_{pg} \mathbf{1}\{t - (g-1) = e\} + \alpha_{pg} + \delta_{g,k(p),t} + \varepsilon_{pgt}, \tag{1}$$

where D_{pg} identifies treated programs and α_{pg} are program-by-stack fixed effects. The group $k(p)$ combines degree level with one of four fields: business and finance, social sciences, design and arts, and other fields. The graduation-year fixed effects $\delta_{g,k(p),t}$ are specific to each stack and field–degree group, so treated programs are compared with programs at the same degree level in the same field group, allowing their common changes to differ across relabeling years.

For student outcomes, I also weight programs by their number of F-1 graduates in the reference cohort, so that the estimates represent the average international graduate of a treated program; outcomes about programs themselves, such as completions or their

international share, instead weight programs equally. I combine the estimates in two steps. First, I average across relabeling years for each cohort relative to the change, giving more weight to years whose treated programs have larger F-1 populations in their reference cohorts. I then average equally over the first four graduating cohorts reported under the new code, $e = 1, \ldots, 4$:[40]

$$\widehat{\theta}_e = \sum_g \omega_{g,e} \widehat{\theta}_{g,e}, \qquad \widehat{\theta} = \frac{1}{4} \sum_{e=1}^{4} \widehat{\theta}_e. \tag{2}$$

Here $\omega_{g,e}$ is relabeling year g's share of the reference-cohort F-1 population in the treated programs contributing to the estimate. For program outcomes I use program counts in place of graduate counts.[41]

For the main employment results, I conduct inference with 499 bootstrap draws at the school level, since programs at the same school share approval processes, international student offices and local labor markets. Each draw refits the treatment model and the linkage model described below, so the standard errors reflect uncertainty in both the comparison-program weights and the inverse-linkage weights. Within the linked sample, I use the same draws for employment, authorization and status-accounting outcomes, so that standard errors for sums and ratios account for their covariance.[42]

Samples and outcomes. For F-1 graduates of the 335 treated programs and their comparison programs, I use SEVIS records to measure whether they use OPT or its extension, use CPT to work before graduation, return to F-1 study, or have a recorded request to change immigration status.[43] To measure employment, I use graduates in treated and comparison programs linked to Revelio profiles, weighting each by the inverse of their estimated linkage probability before computing average employment for each program and graduating class (Appendix B.2.3). I use the job dates in these profiles to determine whether a graduate has a U.S. job on the fifteenth of each month, counting overlapping jobs once and measuring time since graduation from the program-end date in SEVIS. Averaging these monthly observations within each year and multiplying by twelve gives months of work, which I sum across years

[40] The preceding cohort, $e = 0$, may already be partly eligible for the extension. It remains in the event study but is excluded from this average.

[41] The programs and treatment cohorts contributing to an estimate can differ across outcomes and event times, particularly for outcomes measured several years after graduation, which require comparison programs to remain untreated for longer. The employment increase persists when the same comparison programs are used for each year after graduation (Section 4.7); Section 4.6 reports estimates for earlier and later relabelings.

[42] The bootstrap does not incorporate uncertainty in profile assignment or missing job histories; Appendix D.2 gives further details.

[43] Curricular Practical Training (CPT) authorizes work that is part of the student's curriculum; I measure recorded authorization. These indicators use the full administrative history.

to measure cumulative employment. Because profiles do not report hours, work during F-1 study counts as half-time unless full-time authorization is recorded (Appendix C).

The main employment sample follows graduates over the first three years after graduation, requiring the full three years to fall within SEVIS coverage through October 2023, and includes 248 of the 335 relabeled programs.[44] Over these three years, I combine the jobs reported in profiles with SEVIS histories to distinguish work on OPT after the original degree, work during inferred H-1B periods, work associated with a later F-1 program and its authorizations, and other work.[45] For longer-run effects, I follow each graduate in a second sample for five years using profiles through September 2026. This later end date allows me to include more recent graduating classes and observe whether graduates keep working in the U.S. after SEVIS records end, although I can no longer distinguish the immigration routes associated with that work.

Identification. The design rests on a conditional parallel-trends assumption: absent relabeling, outcomes would have changed similarly in treated and reweighted comparison programs within field group, degree level and year. It also requires that the reference cohort be unaffected and that treated programs have untreated counterparts with similar size, student composition and the other characteristics used to estimate the propensity score. The pre-treatment event studies provide a check on parallel trends, although the employment differences before relabeling are imprecisely estimated and so leave some uncertainty about differential trends. In Section 4.7, I show that the employment increase persists with longer comparison buffers, the same comparison programs throughout, and a doubly robust estimator.

The employment outcomes add a second requirement. Linked graduates must represent their full cohort once I adjust for observed characteristics, and neither linkage nor job reporting can change differentially after relabeling. The estimated change in the share linked is small and imprecise, but the inverse-linkage weights reduce the linked sample's larger response in extension use without fully removing it. I therefore also adjust the weights to reproduce administrative OPT participation in treated and in comparison programs, and Section 4.7 shows that the employment increase survives this adjustment.

Finally, interpreting the change as an effect of OPT eligibility requires that relabeling not improve employment prospects on its own, through curriculum, reputation or recruitment. Domestic graduates of the same programs, who cannot use OPT, provide a check on changes

[44] For each estimate, I require linked profiles for the class graduating two years before the new code appears in IPEDS and for the class being compared, along with eligible comparison programs.

[45] The last category includes work during education reported only in profiles; all four categories sum to total employment. H-1B periods are inferred from undated requests rather than observed approvals, which affects the classification of work but not whether a U.S. job is recorded.

that would affect all graduates: their estimated employment responses are small and imprecise, giving no clear evidence of a common improvement. Relabeling could also change who enrolls. Total completions change little, but the international share rises, so the estimates include changes in the mix of graduates.[46] Employment nevertheless increases substantially in the first relabeled graduating cohort, whose students had less opportunity to adjust enrollment in response to the change, which makes gradual changes in whom programs attract a less likely explanation for the main results (Section 4.7).

4.3 OPT Participation

The prospect of a longer period of work authorization can draw graduates into initial OPT as well as keep existing users on it after the first year. I first check that relabeling changes the codes recorded in SEVIS, which determine graduates' eligibility, and then examine initial OPT use and extension use. Before relabeling, only 2.3% of F-1 graduates of treated programs had a STEM-eligible code recorded in SEVIS; after relabeling, this share rises by 78.2 percentage points [SE 1.4] (Appendix Figure A.2). Use of the extension rises by 23.6 percentage points [SE 1.7], from 1.2%, and use of OPT at all rises by 10.2 percentage points [SE 1.5], from 51.6% (Figure 5).

Graduates also work more in the U.S. before completing their degrees: the share of the final program year spent in recorded U.S. work rises by 3.7 percentage points [SE 1.6], counting work during F-1 study at one-half unless full-time work is authorized (Appendix Table A.6). The share authorized for CPT during the degree rises by 5.3 percentage points [SE 1.4] from 18.7% (Appendix Table A.2).[47]

4.4 U.S. Employment in the First Three Years

More work on OPT need not translate one-for-one into additional U.S. employment, because graduates may otherwise have done the same work through another route. I therefore compare the effect on total U.S. work with the effect on work on OPT over the first three years after completion. If work through other routes is unchanged, the two effects are equal; a smaller total effect means that some of the additional OPT work is offset by less work through other routes.

[46] Comparison programs at treated schools remain in the sample. If relabeling attracts students away from those programs, it could also change the composition and employment outcomes of the comparison group.

[47] The final program year is the twelve months before the SEVIS program-end month, when students may already know about the program's new eligibility. This estimate uses the broader sample observed over that window and is excluded from the post-completion totals. Appendix Table A.6 separates work by recorded authorization; most of the increase occurs in months without a recorded off-campus authorization, so the employment measure is broader than internships.

Relabeling increases recorded U.S. work by 5.3 months [SE 1.4] per graduate over these three years (Figure 6). The increase is already present in the first relabeled graduating cohort, at 4.9 months [SE 1.4], and persists across the following cohorts. For the same graduates over the same three years, work on OPT and the extension rises by 7.2 months [SE 0.9] per graduate (Figure 8). The estimated ratio of additional U.S. work to additional OPT work is 0.73 [SE 0.15]: on net, roughly seven in ten months of additional OPT work add to U.S. employment, while the remaining months replace work graduates would otherwise have done through other routes.

The net change in work through other routes can conceal different responses: OPT may replace work through some routes while helping graduates reach others. I use the status histories to distinguish these responses (Figure 8). The changes in these components, together with the increase in OPT work, add up to the total employment effect.

Further study. Without the extension, some graduates would have remained in the U.S. after initial OPT by starting another F-1 program, with opportunities to work during the program and through its subsequent OPT authorization. Work through a later F-1 program falls by 1.2 months per graduate [SE 0.3] over the first three years after graduation. More than half of this reduction, 0.65 months [SE 0.22], is work with full-time Curricular Practical Training (CPT) authorization in the new program (Appendix Table A.6). The administrative records for all F-1 graduates in the analysis, including those without a linked profile, show the same pattern: time spent in a later F-1 program or its associated authorization falls by 1.0 month [SE 0.2], from 1.8 months (Appendix Table A.6). Return to F-1 study within two years falls by 4.3 percentage points [SE 0.8], from 7.8% (Appendix Table A.2).

The H-1B visa. Although H-1B provides an alternative to the extension, the extension can also give graduates more time to pursue sponsorship before their OPT runs out. In the administrative sample, the share of graduates with a recorded request to change to H-1B status rises by 3.6 percentage points [SE 0.9], from 13.7% (Appendix Table A.2).[48] Graduates may also use the extra OPT time before moving to H-1B, so more requests need not imply more H-1B work within the first three years. Over that period, the estimated increase in work during an inferred H-1B period is 0.4 months [SE 0.5]. The employer-level lottery comparison in Appendix Figure A.3 is consistent with graduates using the extension as a fallback when their employers receive fewer H-1B selections.

[48]The requests are undated and observed through October 2023, so earlier graduating cohorts have longer follow-up.

Other routes. The remaining category covers work under another recorded status or with no recorded status, including work during education reported only in profiles. The estimated reduction through these other routes is 1.2 months per graduate [SE 0.8].

4.5 Longer-Term Employment

Employment gains may persist beyond the usual OPT period if the additional time helps graduates move to another route to U.S. work. To examine this, Figure 7 follows the same graduates for five years using profile histories available through September 2026. This sample includes later graduates than the main three-year sample used to classify work by immigration status because profiles extend beyond SEVIS. Within this sample, the first three years contribute 4.4 months [SE 1.3] of additional U.S. work, compared with 5.3 months in the main sample. The fourth and fifth years together add a further 1.0 month [SE 0.8], bringing the five-year total to 5.5 months [SE 1.9] per graduate (Figure 7). The point estimates place most of the gain in the first three years; the estimate for years four and five is positive but imprecise.[49]

4.6 Differences Across Cohorts and Graduates

The five-year averages combine relabelings at different points in time. For programs relabeled in 2014–2016, the first three years contribute 7.2 months [SE 1.2] of additional U.S. work, and years four and five add 4.9 months [SE 1.5]. For the 2017–2019 relabelings, the corresponding estimates are 3.9 months [SE 1.4] and 0.4 months [SE 0.9] (Appendix Table A.4). The gains for the earlier cohorts show that longer OPT can support U.S. employment beyond the authorization period, making graduates' subsequent routes to work an important part of its lasting workforce contribution.[50]

Both bachelor's and master's graduates work more in the U.S. after relabeling. Over the first three years, the estimated gains are 4.5 months [SE 1.7] for bachelor's graduates and 6.0 months [SE 1.8] for master's graduates. The ratios of additional total work to additional OPT work are 0.82 [SE 0.27] and 0.66 [SE 0.17], respectively (Appendix Figure A.5). Both point estimates imply that most of the extra work on OPT adds to total U.S. employment, suggesting that limiting longer authorization to graduates with advanced degrees would forgo meaningful workforce gains among bachelor's graduates.[51]

[49] Appendix Table A.4 reports the five-year estimates for earlier and later relabeling cohorts and under a common set of comparison programs.

[50] These comparisons hold graduates fixed within each five-year sample, but also compare different programs and calendar periods; they do not by themselves identify why persistence differs.

[51] The difference between the two ratios is imprecisely estimated, at 0.16 [SE 0.30], using the same bootstrap

Responses in extension use also differ by citizenship. Extension use rises by 44.4 percentage points [SE 3.6] among graduates from India and by 18.0 percentage points [SE 2.3] among graduates from China (Appendix Table A.7).[52] This pattern may reflect differences in demand for U.S. work visas and opportunities to work elsewhere. These differences suggest that restrictions on post-graduation work could change the composition as well as the size of the international graduate workforce, with graduates from India potentially responding more strongly than those from China.

4.7 Robustness

The employment response remains substantial when I change the estimator or the comparison programs (Appendix Table A.4). Over five years, the doubly robust estimator, which combines reweighting with a model of comparison programs' outcome changes, gives a gain of 6.3 months [SE 2.6]; using the same comparison programs throughout gives 4.2 months [SE 1.8], compared with 5.5 months [SE 1.9] in the main specification.[53] I also require comparison programs to remain untreated longer after the outcome window. Over three years, extending this buffer from one year to two or three years gives employment gains of 4.8 months [SE 1.3] and 4.4 months [SE 1.3], respectively, compared with 5.3 months [SE 1.4] in the main specification.

I next assess two possible sources of differential changes: changes to the programs themselves and changes in which graduates are observed. A related concern is that treated programs' employment prospects may already have been improving relative to comparison programs. The two earlier cohorts in Figure 6 work about one month less than the reference cohort, although their confidence intervals include zero. These estimates cannot exclude a differential trend that would account for part of the post-relabeling increase; changing the estimator or comparison window does not by itself address that possibility.

Domestic graduates of the same programs help assess changes to the programs themselves: they cannot use OPT, but changes in curriculum, reputation or recruitment could affect them too. Their estimated employment responses are small and imprecise, providing no clear evidence of a common improvement in employment prospects (Appendix Table A.3).[54]

draws. The bachelor's estimates use relabelings in 2015–2019 and the master's estimates use 2014–2019; not every outcome is observed for every program. Appendix Table A.7 reports extension use by degree and citizenship.

[52] I do not report employment responses by citizenship: fewer treated and comparison programs have linked outcomes within each country, limiting statistical power. The participation estimates use the full administrative sample.

[53] The doubly robust estimates use relabelings in 2015–2019, whereas the main and fixed-comparison specifications use 2014–2019.

[54] The estimated changes in domestic graduates' U.S. employment are 1.2 [SE 0.8], 0.3 [SE 0.9] and 0.5 [SE

International students may nevertheless respond differently. The event studies are informative here: both extension use and employment rise in the first relabeled graduating cohort, whose students had less opportunity to adjust enrollment in response to the change. This timing makes gradual changes in whom programs attract a less likely explanation for the main results.[55]

For the linked outcomes, the relevant question is whether the observed graduates capture the employment response of the full cohort. The estimated change in the share linked after relabeling is 0.8 percentage points [SE 1.0] (Appendix Figure A.4). Administrative comparisons show that the weights reduce, but do not eliminate, the linked sample's larger response in extension use (Appendix B.2.2). I therefore also examine weights further adjusted to match administrative OPT participation separately in treated and comparison programs. This specification covers the 2016–2019 treatment cohorts, so both the weights and the sample differ from the main specification. The employment effect is 4.3 months [SE 1.5], and the ratio of the effects on total work and work on OPT is 0.71 [SE 0.20], close to the main estimate.[56]

Even when a graduate's work is additional, an employer might otherwise have filled the same job with another worker. The next section asks a separate question: when employers gain access to a larger pool of graduates from eligible programs, do they hire more workers and expand employment, or replace workers they would otherwise have hired or retained?

5 OPT and Other Workers

Section 4 shows that longer OPT authorization raises international graduates' U.S. employment, while replacing some work they would have done through other routes. I now turn to the employers that hire these graduates, since OPT's contribution to the U.S. skilled workforce also depends on whether the jobs they fill would otherwise have gone to other skilled workers. To study this question, I use broader changes in the number of graduates from programs eligible for the STEM extension, including growth in existing programs, the creation of new programs, and changes in eligibility. I compare firms with larger and smaller increases in access to these graduates, using their historical recruiting ties to the schools where the expansions occur. I first describe the program expansions and firm comparison, then

1.0] percentage points in the first three calendar years. Domestic and foreign origin are inferred from profiles rather than observed citizenship. This comparison does not require SEVIS links and measures calendar years rather than years since program completion.

[55]The international share of completions rises by 4.4 percentage points [SE 0.8], while the estimate for total completions is −0.6 graduates [SE 1.7] (Appendix Table A.3). The design allows the mix of graduates to change and retains comparison programs at treated schools, where enrollment may also shift between programs.

[56]Appendix B.2.2 describes the review of candidate links and examines how profiles record authorized work.

examine recruitment through OPT, employment and hiring of other workers, and employers' use of H-1B sponsorship.

5.1 Program Expansions and Firm Exposure

In Section 2, I describe how the growth in international graduates has been concentrated in a relatively small set of majors. I use changes in the size of programs eligible for longer OPT to study how employers respond to an expanding supply of graduates. The same program expansion can matter more for a firm that regularly recruits from the school than for a firm with few recruiting ties to it. My measure combines these school changes with firms' earlier recruiting patterns.

School program expansions. I start with the bachelor's and master's degrees awarded at each school in IPEDS, classifying programs by whether their graduates qualify for the STEM extension in that year. The share of degrees awarded in eligible programs can rise because those programs grow or because other programs shrink. To isolate the first source of change, I measure the contribution of growth in eligible programs to each annual change in the share, then add up the positive contributions after 2008. I denote the resulting expansion index by H_{kt} for school k in reporting year t; Appendix D.3 gives the decomposition and the sample of schools. The measure captures additional graduates in eligible fields, programs reclassified into eligible fields, and fields newly added to the eligibility list. These programs train domestic as well as international students, so the firm comparison concerns growth in the broader graduate pool that employers can access.

Connecting schools to firms. I link school-level changes to firms using Revelio education and employment histories, identifying the U.S. postsecondary education preceding each worker's first job at a firm in 2008–2013. When graduation dates are missing, I estimate which education record preceded the hire; when the school link is uncertain, I divide its contribution across candidate schools. Let N_{fk} be the number of earlier hires attributed to school k at firm f, counting uncertain school links fractionally. A school's expansion receives more weight when it supplied a larger share of these earlier hires:

$$w_{fk} = \frac{N_{fk}}{\sum_j N_{fj}}, \qquad E_{ft} = \sum_k w_{fk} H_{kt}. \tag{3}$$

For example, an expansion-index increase of five percentage points at a school adds three points to a firm's exposure if 60% of its attributed hires came from that school, but only one

point if the share was 20%. The recruiting shares are fixed in the 2008–2013 window, so a firm's later hiring cannot raise its own exposure.

Treated and comparison firms. I identify large increases in firms' access to eligible graduates during 2015–2019 and compare them with smaller increases over the same years. A treated episode begins when the annual exposure increase reaches 2.21 percentage points, the 75th percentile of firms' largest annual increases, after a year below that threshold; a firm can contribute more than one episode. Comparison firms' increases remain below 1.72 points, the median of the same distribution, in the treatment cohort's year and the following three years. Both groups must have observed exposure and outcomes over the comparison window and positive employment in the four years ending two years before the measured expansion, so the design follows established firms (Appendix D.3). The main sample contains 40,203 treated episodes at 36,210 existing firms. Appendix Table A.8 reports sample sizes, and Figure A.6 shows the exposure paths.

5.2 Empirical Strategy

I use a difference-in-differences design, comparing changes in outcomes at firms with large increases in access to eligible graduates with changes at firms experiencing smaller increases. As in Section 4, I estimate effects separately for each treatment cohort, the firms experiencing a large increase in the same year, and then combine them.

Comparison firms and weights. Treated firms are much smaller than the typical comparison firm before the expansion, averaging 51 U.S. workers compared with 291, and hire fewer workers (Table 2). Within each treatment cohort, I therefore reweight the eligible comparison firms to resemble treated firms in their earlier outcomes, size and growth, foreign-worker composition, OPT hiring, H-1B demand, exposure and recruiting histories, industry, and geography. I estimate each firm's propensity score using inverse probability tilting, which chooses the weights so that the means of these characteristics balance between treated and comparison firms, and assign comparison firms their estimated odds of treatment, $\widehat{p}/(1-\widehat{p})$, normalized to sum to one within the cohort. For monthly and year-end employment counts, the weights use each measure's own earlier mean and trend, so different outcomes can receive different comparison weights (Appendix D.3). Reweighting brings comparison firms close to treated firms in prior employment and hiring; Panel B of the table reports additional hiring measures that do not enter the weights for those rows.

Estimation and aggregation. Employers may respond before the measured expansion: IPEDS dates completions by academic year, while the main OPT hiring measure uses the calendar year of the graduate's program end, and firms can recruit students before the larger cohorts graduate. I therefore treat the preceding year as partly exposed. Let g denote the calendar year defining the treatment cohort and event time be $e = t-(g-1)$, so the reference year is $e = -1$, the partly exposed year is $e = 0$, and the measured expansion occurs at $e = 1$.

For annual hiring, sponsorship and employment counts, I compare changes from the reference year at treated firms with weighted changes at comparison firms in treatment cohort g:

$$\widehat{\tau}_{g,e} = \frac{1}{N_{1g}} \sum_{f:D_{fg}=1} \Delta Y_{fge} - \sum_{f:D_{fg}=0} \widehat{\omega}_{fg} \Delta Y_{fge}, \qquad \Delta Y_{fge} = Y_{f,g+e-1} - Y_{f,g-2}. \tag{4}$$

Here D_{fg} identifies treated episodes, N_{1g} is their number, and $\widehat{\omega}_{fg}$ are the comparison-firm weights described above. I aggregate treatment cohorts by their shares of treated episodes (Callaway and Sant'Anna, 2021) and average the year of the measured expansion and the following two years:

$$\widehat{\tau}^{\text{post}} = \sum_{g} \frac{N_{1g}}{\sum_j N_{1j}} \left(\frac{1}{3} \sum_{e=1}^{3} \widehat{\tau}_{g,e} \right). \tag{5}$$

The estimated effect gives equal weight to treated firm-episodes, so it describes the average treated firm rather than the average worker. I report the average effect in levels and, for hiring, also as a percentage of the counterfactual mean, the estimated mean at treated firms without the larger exposure increase. I conduct inference with bootstrap draws clustered by corporate group, to account for subsidiaries and franchises of the same company. Each draw re-estimates the comparison weights, treatment-cohort shares and outcome contrasts, so the intervals reflect uncertainty in both the effect and its counterfactual mean.[57]

Samples and outcomes. The main hiring, H-1B sponsorship and employment estimates use the 40,203 treated episodes in 2015–2019 described above, together with their comparison firms. I use SEVIS records to measure recruitment through OPT and Revelio profiles to examine whether it is accompanied by changes in firms' employment and hiring of other workers. In SEVIS, I count each graduate–employer relationship once within the graduate's program-end cohort, regardless of when the job begins or ends; in the profiles, hiring counts a worker's first job at the firm.[58]

To distinguish recruitment through U.S. education from other hiring, I separate recent

[57] Hiring and sponsorship use 499 draws; monthly and year-end employment counts and the existing-worker estimates use 99, so their uncertainty estimates are preliminary. Appendix D.3 describes the bootstrap.

[58] The two sources cover overlapping sets of workers, so their recruitment estimates are alternative measures of the same response.

graduates and student interns from other workers. Recent graduates completed U.S. postsecondary education within a year before joining the firm, while student interns have a completed first job lasting less than six months that overlaps U.S. postsecondary enrollment. I also distinguish native and foreign workers using the origin inferred from their profiles. The remaining hiring groups show whether firms recruit fewer workers through other channels.[59]

Hiring more workers need not raise employment if the new hires replace workers who leave, so I also measure employment directly, counting workers with recorded U.S. jobs at each month-end and averaging across the twelve months. The main outcome counts workers with a recorded bachelor's or higher degree, excluding those who first joined the firm as foreign recent U.S. graduates or foreign student interns; it covers native degree holders, including recent graduates, and other foreign degree holders. I keep each worker's entry classification fixed through later jobs and returns to the firm, and also examine total employment with and without the degree restriction.[60]

To examine whether firms retain fewer existing employees, I follow a fixed group of non-intern workers employed at the firm at the start of the partly exposed year, $g-1$. At the ends of g, $g+1$ and $g+2$, I measure the share still employed at the original firm and the share whose original continuous period of employment there has ended, which I call a first departure. Because the share of any group still employed at a firm declines as the group is followed over time, I compare these outcomes with those of workers employed at the same firm four years earlier, after the same length of follow-up, and estimate the change relative to comparison firms (Appendix D.3).

Finally, I measure employers' demand for H-1B workers using positions requested on Labor Condition Applications and, from 2020, lottery registrations. I examine approved petitions separately, including new and continuing petitions and dating them by the fiscal year of decision. These outcomes distinguish firms' attempts to sponsor workers from the approvals they receive.[61]

[59]The origin classification uses education, employment, location and name evidence, with a probability of foreign origin of at least 0.5 defining foreign workers. It does not observe citizenship or birthplace, and the foreign recent-graduate and intern group does not directly identify OPT status. Within other foreign hiring, I also examine workers educated abroad without recorded U.S. education; they may already have worked in the U.S.

[60]Among workers who did not enter as foreign recent graduates or foreign student interns, a completed bachelor's or higher degree is recorded for 49.1% of employment months before exposure and 52.2% afterward, pooling employment across treated firm-episodes; workers with no recorded education may still hold a degree. Appendix D.3 gives the construction details.

[61]Requested positions can count the same worker more than once. I also examine applications to certify jobs for employer-sponsored permanent residence, which do not count green cards granted, and cap sponsorship counts at the annual 99th percentile of positive values. The OPT-hiring split in Appendix Figure A.11 uses graduates' recorded H-1B requests, which are undated and not attributed to the employer being studied.

Identification. The design requires that, without the larger increase in access to graduates, outcomes at treated firms would have changed like those at the reweighted comparison firms. Predetermined recruiting shares prevent firms' current hiring choices from mechanically determining their exposure, while reweighting accounts for differences in earlier hiring, employment, growth, and recruiting patterns. A remaining concern is a change in demand shared by firms and their recruiting schools: an expanding industry could both hire more workers and induce schools to expand the programs from which it recruits. In Section 5.5, I examine whether the recruitment response is specific to eligible programs and how the results change with alternative graduation dates and employment measures.[62]

The estimates describe employers' responses to larger pools of graduates from eligible programs, including domestic graduates. This comparison allows me to ask whether the accompanying increase in OPT hiring comes with a decline in other hiring or employment at the same firms; it does not isolate the effect of an additional OPT worker or the net national employment response.

5.3 Recruitment Through OPT

Firms substantially increase recruitment through OPT when their access to graduates from eligible programs expands (Figure 9). They add 0.156 OPT employer relationships [SE 0.021] per firm and annual program-end cohort, a 29.1% increase [SE 4.0] relative to the counterfactual mean of 0.536. Professional histories show a similar response: hiring of foreign recent graduates and student interns rises by 0.111 workers [SE 0.019] per firm and year, or 26.5% [SE 3.7]. The response also extends to students who have not yet completed their degrees. Foreign student internships rise by 31.5% [SE 4.7], with an increase already visible in the partly exposed year (Appendix Figure A.11).

5.4 Employment and Hiring of Other Workers

The increase in OPT recruitment is not accompanied by a detectable decline in employment of other college-educated workers. The estimated change in average monthly employment outside the foreign recent-graduate and student-intern group is 0.142 workers per firm [SE 0.162], relative to a counterfactual mean of 25.3, and the 95% confidence interval excludes declines larger than about 0.18 workers, or 0.7% of that mean (Figure 10). The point estimate for native degree-holder employment is close to zero (−0.003 workers [SE 0.131]), while

[62]The comparison also requires sufficiently similar treated and comparison firms, an unaffected reference year, and that larger expansions do not themselves change comparison firms' outcomes in ways that confound the contrast. Earlier outcomes enter the weights, so their balance is not an independent test of parallel trends.

other foreign degree-holder employment rises by 0.242 [SE 0.073]. Looking across the entire workforce, including workers without a recorded degree, also gives a positive point estimate: total employment is 0.302 workers higher [SE 0.285], relative to a counterfactual mean of 49.1 (Figure 10, panel C).[63]

The hiring results provide a closer view of whether firms recruit fewer workers through other channels (Appendix Figure A.7). The estimated changes are 1.8% [SE 1.3] for other native hiring, −0.1% [SE 2.0] for other foreign hiring, and −1.2% [SE 2.8] for hiring of foreign-educated workers without recorded U.S. education, with no clear offsetting decline in any group; the confidence intervals exclude declines larger than about 4% in other foreign hiring and 7% among foreign-educated workers. Native recent-graduate and student-intern hiring instead rises by 0.100 workers [SE 0.050] per firm and year, or 3.4% [SE 1.6], consistent with expanding programs supplying both native and foreign graduates.[64]

Greater recruitment could affect not only new hiring but also workers already at the firm. Under the main specification, employment at the original firm falls by 0.27 percentage points [SE 0.18], relative to a counterfactual employment rate of 62.8%, and first departures rise by 0.25 percentage points [SE 0.17] (Appendix D.3). These estimates provide no clear evidence of an increase in departures among existing workers, and the confidence interval excludes a decline in employment at the original firm larger than about 0.62 percentage points. Appendix D.3 examines how the results change when I additionally balance comparison firms on earlier workers' employment and departure rates.[65]

H-1B sponsorship also rises as firms recruit more through OPT. Requested positions increase by 0.179 [SE 0.055] per firm and year (Figure 11), and approved petitions rise by 0.089 [SE 0.028] (Appendix Figure A.12).[66] The increase in requests indicates that OPT recruitment does not simply replace firms' demand for H-1B workers. It likely reflects firms sponsoring their OPT hires to continue working on H-1B, so the two routes can support successive stages of the same employment relationship.[67]

[63]Outcomes use their own earlier employment histories to estimate comparison weights, so components need not add to the combined estimate. Including foreign recent graduates and interns gives a change in total recorded degree-holder employment of 0.206 workers [SE 0.170] (Appendix Figure A.8). Appendix Figure A.10 reports unrestricted monthly and December 31 employment.

[64]Because the expansions also increase access to domestic graduates, the native-graduate response does not identify the effect of an additional OPT hire on demand for native workers.

[65]Excluding workers who first joined as foreign recent graduates or student interns gives effects of −0.25 percentage points [SE 0.18] on employment and 0.24 [SE 0.17] on first departures. The whole-firm flows in Appendix Figure A.9 include workers hired after expansion and therefore differ from this fixed group of existing employees.

[66]The estimate for applications to certify jobs for employer-sponsored permanent residence is −0.011 per firm and year [SE 0.012].

[67]The employer records do not identify individual transitions from OPT to H-1B.

5.5 Robustness

A concern with the employer comparison is that firms and their recruiting schools could respond to the same increase in demand. I examine whether the OPT response reflects a general increase in recruiting from these schools by separating hires from programs eligible for the STEM extension from hires from other programs. For this comparison, I assign OPT hires to the July–June academic year used by IPEDS to count degrees. The increase is concentrated in the programs whose graduates are eligible for longer authorization: 0.175 OPT employer relationships [SE 0.021] from eligible programs and −0.001 [SE 0.005] from other programs (Appendix Table A.9), consistent with employers drawing on the expanding eligible pool. Aligning graduation dates also leaves the overall recruitment result similar and substantially reduces the earlier negative contrast.[68]

For employment, I examine whether the results depend on observing workers at the end of the year or throughout the year. For the broader workforce without a degree restriction, neither the average monthly count nor the December 31 count shows a clear decline in native or other foreign employment (Appendix Figure A.10). The monthly average captures employment throughout the year and also has smaller earlier differences in native employment.[69]

Finally, firms recruiting from the same school may experience common changes even when they belong to different corporate groups. Clustering by the school from which each firm historically recruited the most workers leaves the uncertainty around the recruitment estimates essentially unchanged: the standard error on OPT hiring is 0.022, compared with 0.021 under corporate-group clustering, and the standard error on foreign graduate and intern hiring remains 0.019 (Appendix Table A.9).

6 Implications for the U.S. Skilled Workforce

The preceding sections measure two margins of OPT's contribution to the U.S. workforce. Section 4 shows that access to the extension raises graduates' U.S. employment while replacing some work through other routes, and Section 5 shows that employers recruit more through OPT as their graduate pools expand. I now apply this evidence to a particular policy question: how would a hypothetical fee on OPT affect U.S. skilled employment? The graduate estimates

[68]The average OPT hiring effect is 0.175 [SE 0.022] under academic-year dating, compared with 0.156 [SE 0.021] under calendar-year dating. In the year before the reference year, the contrast changes from −0.030 [SE 0.012] to −0.004 [SE 0.011]. I retain calendar-year dating in the main analysis to align administrative hiring with profiles, which often record graduation only by year.

[69]The estimates for native employment are 0.141 workers [SE 0.240] in the monthly average and 0.430 [SE 0.250] on December 31. Some earlier differences remain in the degree-restricted series: the other-foreign contrast in the year before the reference is −0.016 workers [SE 0.008].

supply the employment response in this calculation; moving from graduates' work to national employment also requires an assumption about other workers' responses.

6.1 From Participation to Employment

Additional employment concentrated in the first years after graduation can still sustain a larger workforce, because each year brings a new graduating cohort. A fee reduces that employment as successive cohorts forgo OPT. The employment loss depends on how many graduates forgo it each year, how much additional U.S. work OPT would have allowed them to do, and how other workers' employment responds. Let q denote the annual reduction in participants, e_h the additional U.S. work-years per participant in postgraduation year h, and m the multiplier for the net employment response, including other workers. A multiplier of one counts the graduates' own employment loss; a value below one allows other workers to replace some of that work, while a value above one adds employment losses elsewhere.

I measure the loss in worker-equivalents: twelve months of recorded U.S. work count as one worker-equivalent, using the study-period weighting described in Section 4.2. With the same participation decline in each cohort, the employment loss in policy year t is

$$L_t = mq \sum_{h=1}^{t} e_h, \qquad t = 1, \ldots, 10. \tag{6}$$

In the first year, the loss consists of the first affected cohort's first-year response. In the second year, that cohort's second-year response is joined by the next cohort's first-year response, and so on. Thus L_t measures the annual average employment gap in year t, rather than work-years lost cumulatively since the fee began.

I begin with a simple calculation that assigns the same estimated employment response to every graduate deterred from OPT. I then allow initial OPT participation and extension use to respond separately, and calculate how the lost OPT work changes work through H-1B visas, further study, and other routes. This second calculation uses observed work histories to distinguish when graduates work on initial OPT and the extension, and the historical estimates to describe how work through other routes responds. Both calculations add the employment changes of successive cohorts and make the same assumption about other workers' employment.

6.2 Inputs and Assumptions

Participation under the fee (q). The October 2026 proposal would charge universities for recommending OPT (U.S. Department of Homeland Security, 2026). I use the fee scenarios

and participation projections in Clemens (2026): $70,000 for initial OPT and $100,000 for initial OPT followed by the STEM extension. When students pay, Clemens uses the response of international enrollment to the cost of a degree; when universities pay, he compares the charge with the margin a university earns on a degree. At the lower and higher fee levels, respectively, he projects participation declines of 28.8% and 35.0% when students bear the fee, and 63.5% and 79.3% when universities do.

For the simple calculation, I apply the lower-fee response to graduates who use OPT without the extension and the higher-fee response to those who use both. Combining these rates with annual OPT flows gives approximately 62,000 fewer participants when students bear the fee and 139,000 when universities do.[70]

Graduates' additional employment (e_h). For the simple calculation, I use the annual employment effects of extension eligibility in Section 4, divided by the increase in extension use in the same five-year sample. This gives 1.23 additional work-years [SE 0.42] per additional extension user over five years. The employment measure includes all observed routes, so it already nets out the work graduates would otherwise have done.[71]

The simple calculation treats every deterred graduate alike. The richer calculation instead starts with the five-year work histories of graduates who used OPT after completing a degree in 2017–2018. Using these histories, I reduce work on initial OPT by the lower-fee participation response and work on the extension by the higher-fee response, while allowing work through H-1B visas, further study, and other routes to change. For H-1B work, I combine historical lottery selection rates with a fitted likelihood of applying and starting work if selected, allowing this likelihood to differ between graduates working on OPT and those who are not. I choose these likelihoods and the response of initial OPT work to reproduce the historical effects of extension eligibility and the baseline amount of work during inferred H-1B periods. Historical changes in later study and other work determine how much lost OPT work these alternatives replace. The net employment loss is the reduction in OPT and H-1B work minus the additional work through later study and other routes. I multiply this average loss per baseline participant by the annual flow of 194,554; the participation decline is already included in the reduction in OPT work.[72]

[70] I start with 194,554 initial OPT authorizations and 95,384 extension authorizations with employment start dates in 2024 (U.S. Immigration and Customs Enforcement, n.d.*b*), using their difference, 99,170, to approximate the annual number using OPT without the extension. For example, the student-paid calculation is $99{,}170 \times 0.288 + 95{,}384 \times 0.350$. The fee scenarios and participation projections use 2024 dollars.

[71] The numerator is 0.45 additional work-years per linked graduate over five years, and the denominator is a 37.0 percentage-point increase in extension authorization use in that same sample. I form the ratio in each of 499 school-level bootstrap samples. Dividing by additional extension use expresses the eligibility effect per additional participant; it does not separately identify the effect of taking up OPT.

[72] The baseline contains 161,326 participants with complete five-year follow-up, including 32,775 profile

These inputs support the projections under four assumptions.

1. Participation responses apply to the specified groups. In the simple calculation, the two annual authorization flows approximate separate groups using OPT without and with the extension. In the richer calculation, the two fee responses instead apply to work on initial OPT and on the extension, with extension work falling by the higher-fee rate alone. Both calculations hold enrollment fixed, assume no switching from OPT with the extension to initial OPT alone, and use the source's combined bachelor's/master's participation rates for all degrees, an assumption that Appendix E.5 relaxes. The projections concern graduates who still complete a U.S. degree; a fee that also changed enrollment would add a further response.

2. Historical employment responses apply to graduates deterred by the fee. The simple calculation assigns the same estimated employment response to every lost participant, including those deterred from initial OPT. The richer calculation instead applies the historical responses through H-1B work, further study, and other work to the loss of OPT work. It assumes that 95% of H-1B work continues from one year to the next and carries the third-year later-study responses into subsequent years. Comparing the two calculations changes both these assumptions and the sample used to describe graduates' work.

3. Other workers' employment is unchanged ($m = 1$). Section 5 finds that firms recruit more through OPT without a detectable decline in other measured hiring. Complementary evidence comes from other OPT reforms: Kwon, Lee and Monras (2026) find increases in firm creation and native high-skilled employment in exposed local labor markets, while Guo, Gong and Pang (2024) find higher domestic employment in STEM occupations, particularly information technology. I use $m = 1$ as the reference assumption, so the projected national loss equals graduates' own employment loss. These findings support a reference case without displacement, while the positive employment spillovers in these studies suggest that broader losses could be larger.[73] Appendix E varies m. A multiplier that is constant over time changes the size of the projected loss without changing its timing.

links. I adjust linked histories to administrative characteristics, extension use, and authorization duration. The model matches the first-year share of the historical OPT response, the cumulative H-1B/OPT response ratio, and cumulative inferred H-1B work. Appendix E gives the monthly calculations, source lottery rates, and sensitivity exercises.

[73] These studies do not identify the national multiplier: local gains can include workers moving between areas, and employment may also move between firms or among recipients of capped visas.

4. Successive cohorts have the same responses. I hold the annual participant flow and employment responses fixed as each new cohort arrives. The first five policy years use employment profiles based on five years of follow-up. To project ten policy years, I assume that each cohort's total employment effect halves every three years after its fifth year. This assumption governs the overall employment effect and is distinct from the assumed continuation of H-1B work. Appendix E also considers effects that end after year five or remain at their fifth-year level.[74]

6.3 Projected Losses and Their Scale

By the fifth policy year, the workforce is about 76,000 worker-equivalents smaller [SE 26,000] when students bear the fee and 170,000 smaller [SE 58,000] when universities do (Figure 12). Most of the rise occurs in the first three policy years, when the losses reach approximately 62,000 and 138,000. The two simple-benchmark paths have the same shape because who pays the fee changes the number of lost participants while retaining the same employment profile.[75]

Counting authorization as additional employment gives substantially larger losses. The dashed lines in Figure 12 assign one full year of work to each lost participant in OPT without the extension and three to each lost participant in OPT with the extension, holding the participation declines and multiplier fixed. This calculation assumes that every authorized month is worked and would not otherwise have been worked in the U.S., with no subsequent employment effect. It gives year-five gaps of 129,000 and 290,000, compared with 76,000 and 170,000 in the empirical benchmark, which is approximately 41% smaller whoever pays the fee. The difference reflects the distinction between authorization and work, work through other routes, and subsequent employment.

For scale, the year-five losses are equivalent to 0.4–0.9% of the 18.84 million college-educated workers in U.S. STEM occupations, or 4.5–10% of Silicon Valley's approximately 1.7 million jobs.[76]

[74]The projection treats one completion cohort as arriving at the start of each policy year and leaves cohorts graduating before the policy unaffected. Annual flows are treated as nonoverlapping entrant cohorts; Appendix E discusses repeated participation after a later degree. An additional response through enrollment, omitted here, could begin later.

[75]The standard errors reflect uncertainty in the estimated employment response, holding the participation projections, the use of the same employment profile for all lost participants, and the multiplier fixed. They do not incorporate uncertainty about the response to an implemented fee.

[76]The national benchmark is the employed civilian population ages 25–75 with a bachelor's or advanced degree in STEM occupations in the 2024 American Community Survey, as reported by National Science Board (2026). The broad STEM definition includes science and engineering, related occupations, and STEM middle-skill occupations. The Silicon Valley benchmark is the rounded 2025 jobs count reported by BW Research (2026); it covers the full Index region (Silicon Valley Institute for Regional Studies, 2026). These

Allowing different employment paths for those deterred from initial OPT and the extension gives larger initial losses, but smaller losses by year five. The richer model in Figure 12 gives year-five gaps of 62,000 and 139,000, about 18–19% below the simple calculation. The shaded portion of the figure extends both projections to ten policy years under the three-year half-life assumption. Under this assumption, the richer model reaches 70,000 and 158,000 in year ten. The loss remains substantial because new cohorts continue to forgo OPT even as the employment effects on earlier cohorts fade.

7 Conclusion

This paper asks how much Optional Practical Training adds to the U.S. skilled workforce. The answer depends on what graduates and employers would otherwise do: work that OPT authorizes adds to U.S. employment only if graduates would not otherwise have done it, and if employers would not otherwise have filled the same jobs with other workers. Linking administrative student records to professional profiles and employer immigration records, I measure both margins.

Eligibility for the OPT extension, which gives graduates more time to work after graduation, raises U.S. employment by 5.3 months [SE 1.4] per international graduate over the first three years. Some of the added work on OPT replaces employment graduates would otherwise have undertaken while studying for another degree or through another immigration route, but most represents additional U.S. work. Firms whose recruiting schools expand programs eligible for the extension hire more through OPT, without a detectable decline in other hiring, and increase their demand for H-1B workers. I use this evidence to assess a hypothetical fee of $70,000 for one year of OPT and $100,000 in total for OPT with the extension. Combining the five-year employment estimates with projected declines in participation implies substantial workforce losses even after allowing graduates to work through other routes, holding other workers' employment fixed.

For policy, the distinction between authorization and additional employment matters in both directions. In these calculations, counting all lost authorized time as additional work overstates the projected employment loss, but employment concentrated in a few years after graduation can still sustain a larger workforce as new cohorts arrive. Restrictions can therefore have lasting effects on workforce size even when an individual graduate's additional employment is temporary. The calculations also highlight the importance of who bears the cost: under the participation responses considered here, requiring universities to pay has

comparisons hold the published denominators fixed and compare worker-equivalents with counts of workers or jobs.

larger consequences than requiring students to pay. A university's willingness to finance OPT need not capture the value that a graduate and a prospective employer place on the opportunity to work together.

Eligibility rules and the alternatives to OPT may shape both who remains in the U.S. and how they do so. Restricting longer authorization to advanced-degree graduates could forgo substantial employment gains among bachelor's graduates, while shortening OPT may shift graduates toward further study or make the transition to employer sponsorship more difficult. Over a longer horizon, the prospect of U.S. employment may also influence where students enroll, which programs universities offer and where employers recruit. These responses extend beyond the calculations here, which hold enrollment fixed: a durable restriction could change not just the route graduates use to work in the U.S., but also where students study and build their careers.

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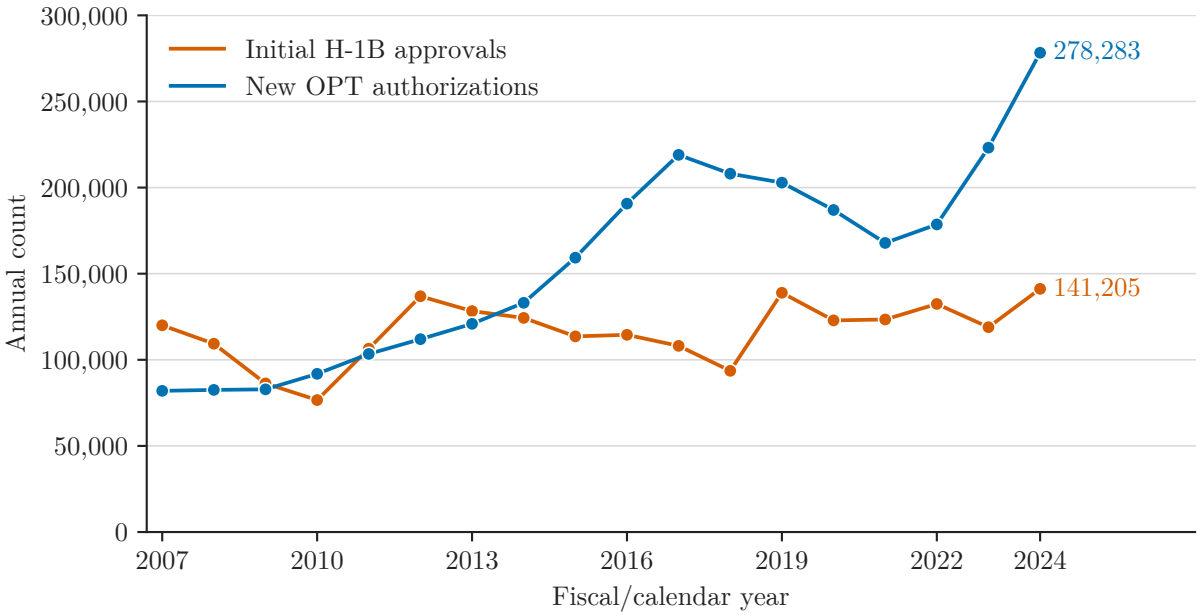
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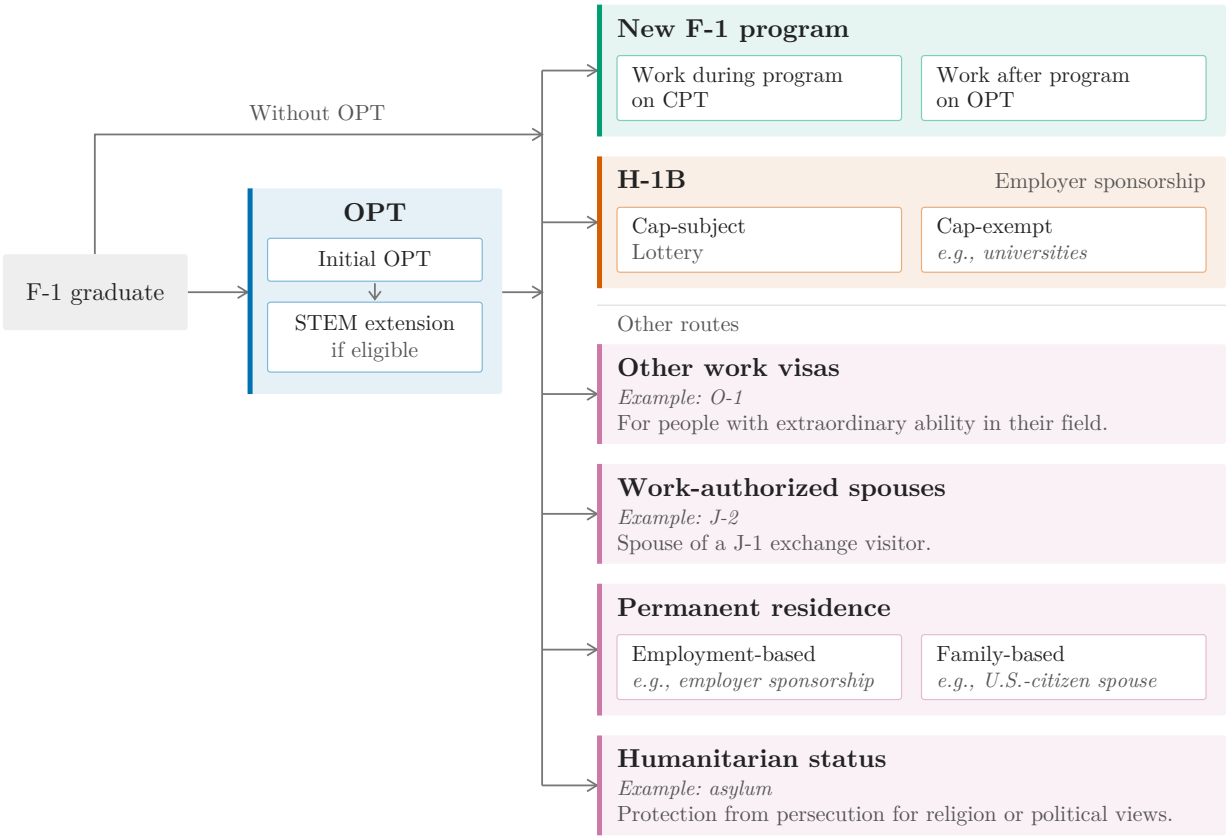
Exhibits

Figure 1: New OPT Work Authorizations and Initial H-1B Approvals, 2007–2024



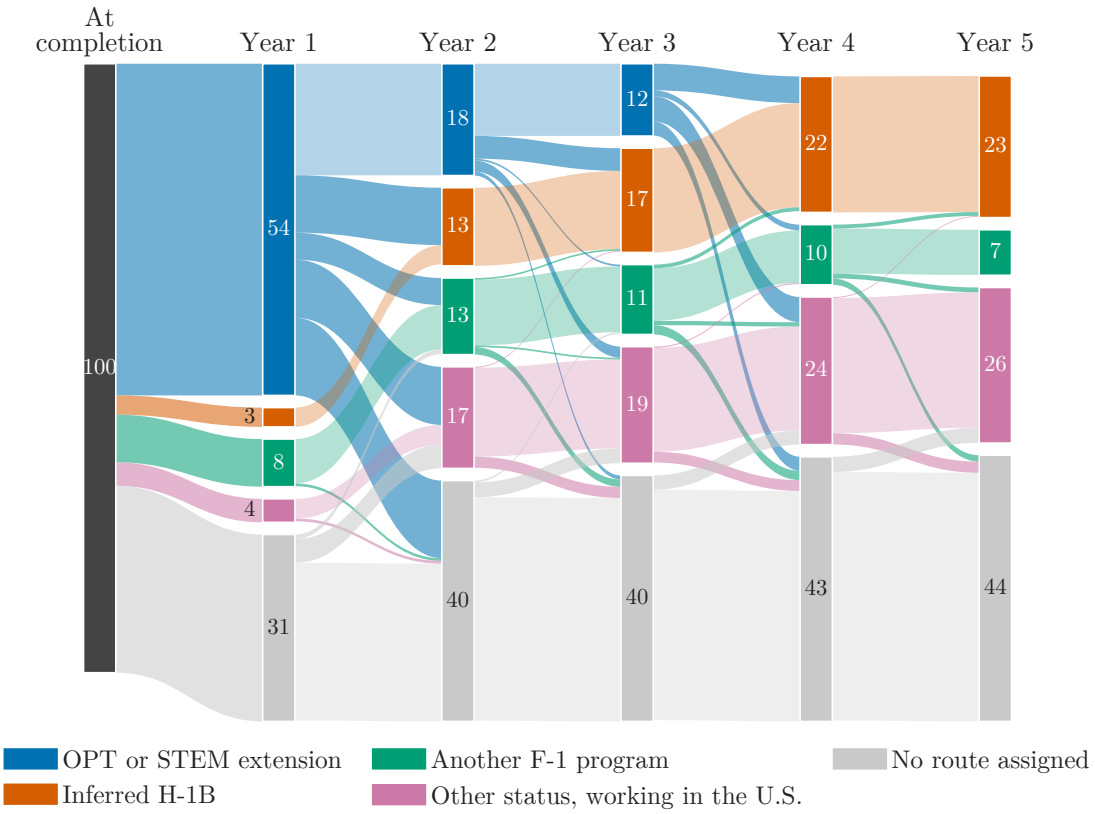
Notes: Blue: individuals issued work authorization for Optional Practical Training (OPT), including the STEM extension, by calendar year. Orange: H-1B petition approvals for initial employment, by fiscal year. Both series are annual flows. Sources: ICE (U.S. Immigration and Customs Enforcement, n.d.*a*) and USCIS annual reports (U.S. Citizenship and Immigration Services, 2010, 2013, 2015, 2018, 2020, 2021, 2022, 2023, 2024*a*, 2025).

Figure 2: Routes to U.S. Work After an F-1 Degree



Notes: Arrows show possible pathways, not observed flows. A further OPT period generally requires a qualifying higher degree; prior training use can limit eligibility. See Section 2.3 for additional information on these pathways.

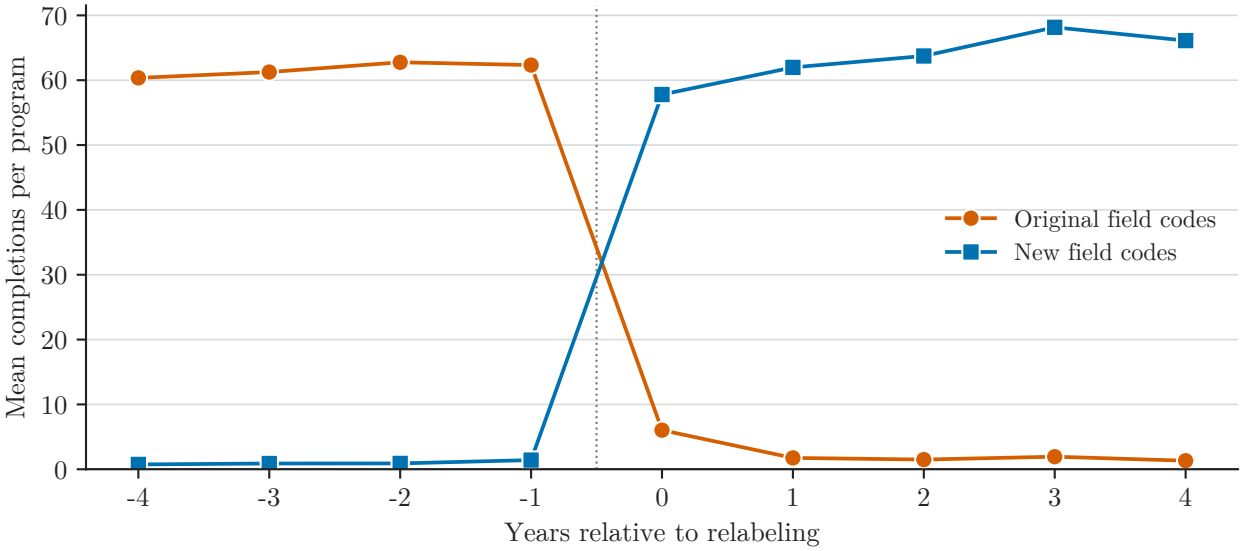
Figure 3: Post-graduation status and transitions among international graduates



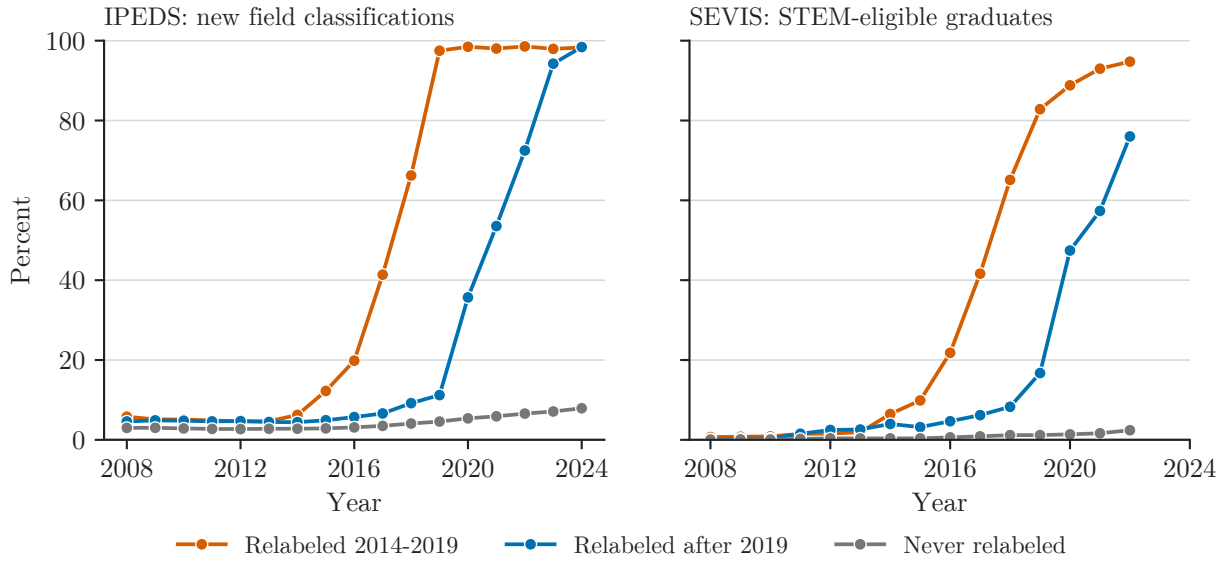
Notes: Boxes show graduates' main recorded route in each year; bands show changes between years, as shares of all graduates. I assign the route accounting for the most time, which need not exceed six months. H-1B periods are inferred from status-change requests. "No route assigned" includes education reported only in profiles and does not establish departure or absence of work. The sample contains 152,314 linked F-1 bachelor's, master's and doctoral completion records from 2010–2017, followed for five years and weighted to administrative counts within represented program-cohorts (Appendix B.2.3). Appendix C explains the monthly assignments. Bands below 0.15 percentage points are omitted. Sources: SEVIS and Revelio.

Figure 4: Program completions and eligibility around relabeling

(a) Completions reported under the old and new codes around the relabeling year



(b) Degree classifications in IPEDS and extension eligibility in SEVIS



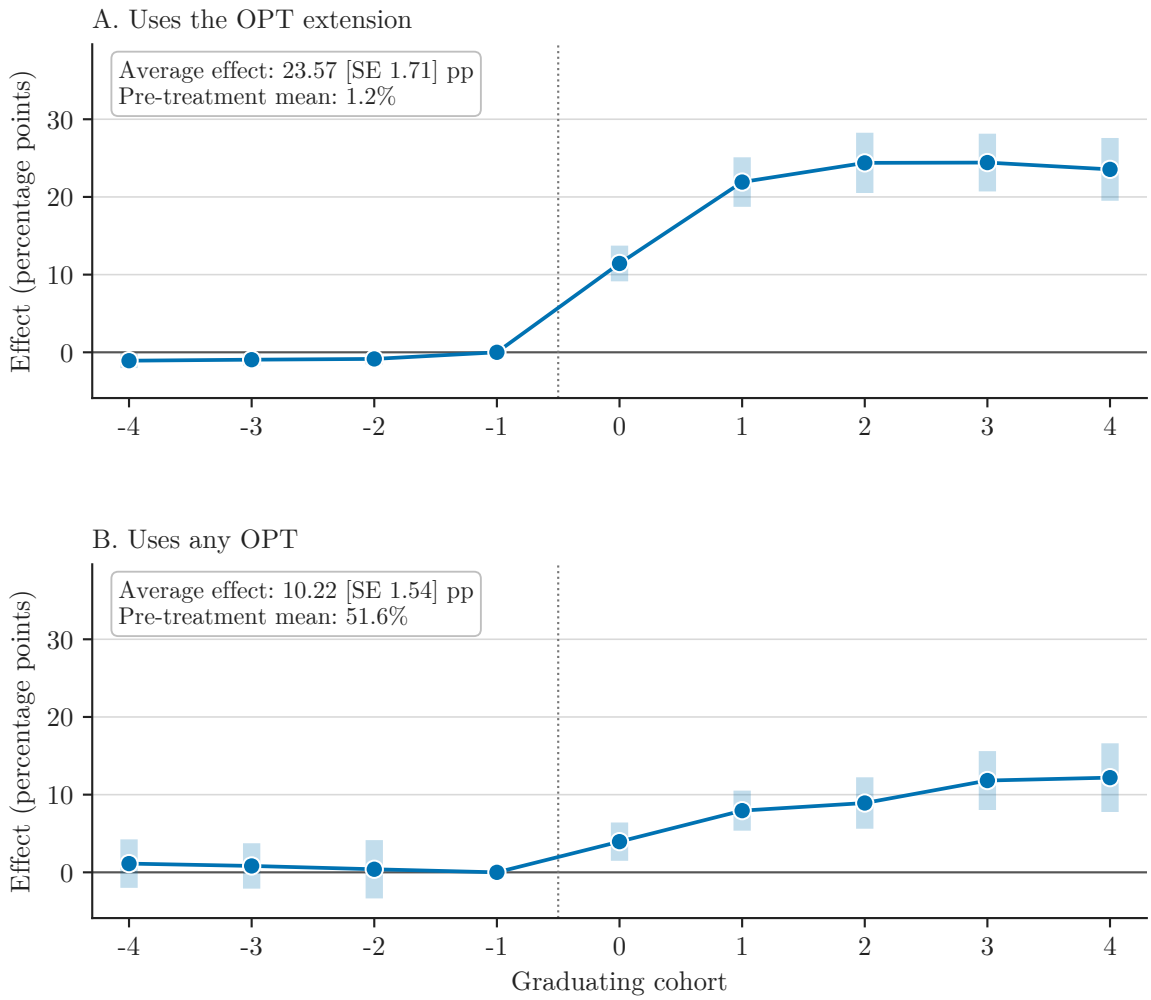
Notes: Panel (a) shows mean annual completions under the old and new field codes for programs reclassified in 2008–2024; year 0 is the first IPEDS year under the new code. In panel (b), the left chart shows degrees under the new classification as a share of degrees under either code, averaged equally across programs with positive completions. The right chart shows the share of F-1 graduates eligible for the extension in SEVIS. The two charts distinguish programs reclassified in 2014–2019, later reclassifications, and never-reclassified comparisons. Appendix D.1 describes the construction. Sources: IPEDS and SEVIS.

Table 1: Pre-treatment characteristics of relabeled and comparison programs

		Comparison programs	
	Relabeled	Unweighted	Reweighted
A. Characteristics used in reweighting			
Log program completions	4.41	4.33	4.21
International share	0.39	0.19***	0.32**
F-1 share from India	0.06	0.06	0.07
F-1 share from China	0.39	0.31***	0.35
OPT take-up	0.52	0.37***	0.53
Trend in international share	0.022	0.018	0.022
Trend in log completions	0.071	0.035**	0.089
Log real tuition	10.70	10.34***	10.67
Bachelor's program	0.51	0.71***	0.56
Master's program	0.40	0.26***	0.40
Doctoral university	0.75	0.64*	0.77
B. Additional characteristics			
F-1 share from U.K., Canada or Australia	0.06	0.06	0.07
F-1 share linked to professional profiles	0.10	0.09*	0.10
Programs	335	9,755	8,979

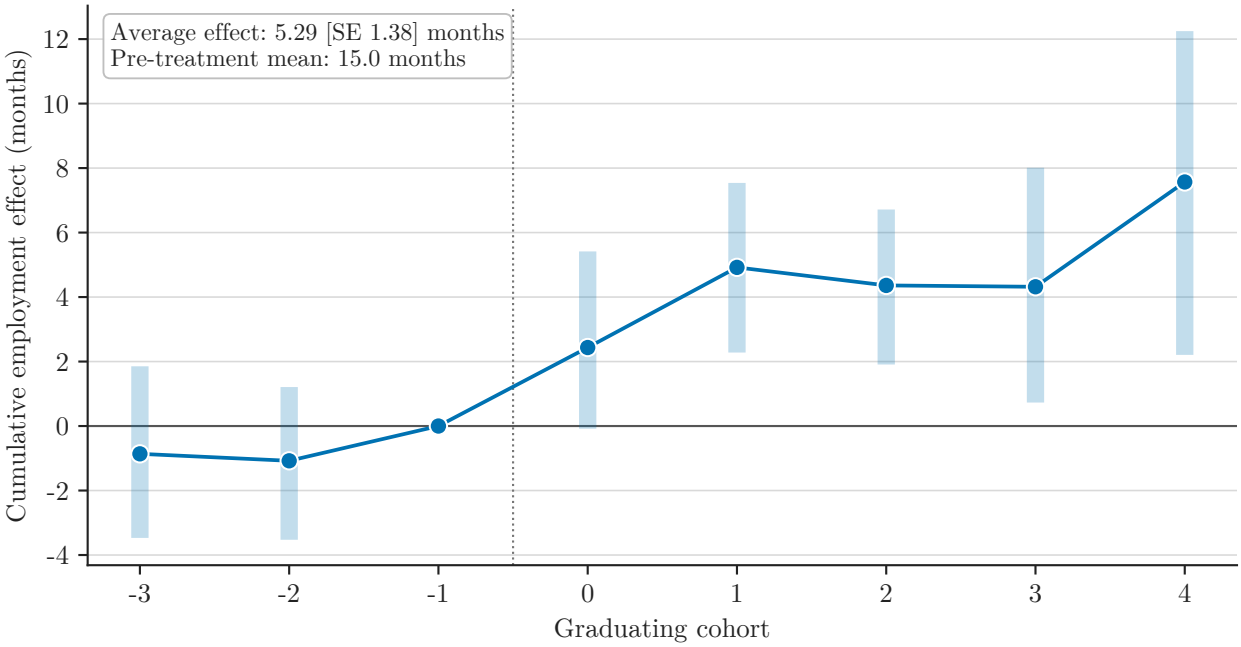
Notes: Pre-treatment characteristics of programs relabeled as STEM-eligible in 2014–2019 and their not-yet-relabeled comparisons. Panel A reports characteristics used in the comparison weights; degree and institution indicators summarize the categorical controls. Panel B reports characteristics omitted from those weights. Means use four pre-treatment graduating cohorts and weight programs by their reference-cohort F-1 graduates; relabeling years are combined by their share of treated F-1 graduates. "Unweighted" omits the additional treatment-odds weights applied to comparison programs in the reweighted column. Stars on comparison means test equality with the relabeled mean using school-clustered standard errors, holding the weights fixed: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Shares are fractions; trends are annual changes. The profile-linkage share uses the accepted links among graduates in the same pre-treatment cohorts. Counts are distinct programs with positive weight; each row omits programs with missing characteristics. Logs use positive values. See Section 4.2 and Appendix D.2. Sources: IPEDS, SEVIS and Revelio.

Figure 5: OPT participation after programs gain extension eligibility



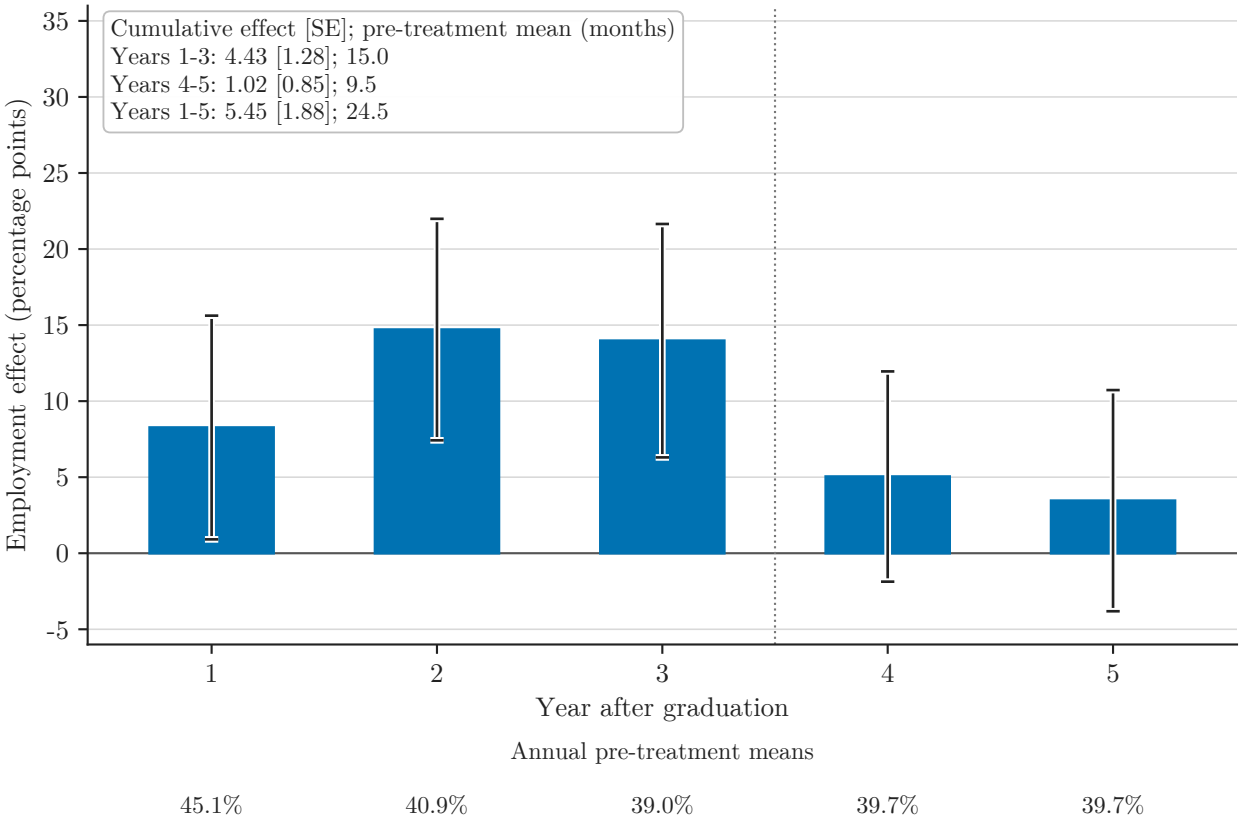
Notes: Percentage-point effects on use of the OPT extension (A) or any OPT (B) among F-1 graduates of programs reclassified as STEM-eligible in 2014–2019 and comparison programs. Estimates follow equation 1, with comparison and cohort-size weights. Cohort 1 is the first reclassified graduating class, 0 may be partly eligible, and −1 is the reference. Boxes give average effects over cohorts 1–4 [SE] and pre-treatment means. The 95% intervals use 199 school-level bootstrap draws. Sources: IPEDS and SEVIS.

Figure 6: U.S. employment after programs gain extension eligibility



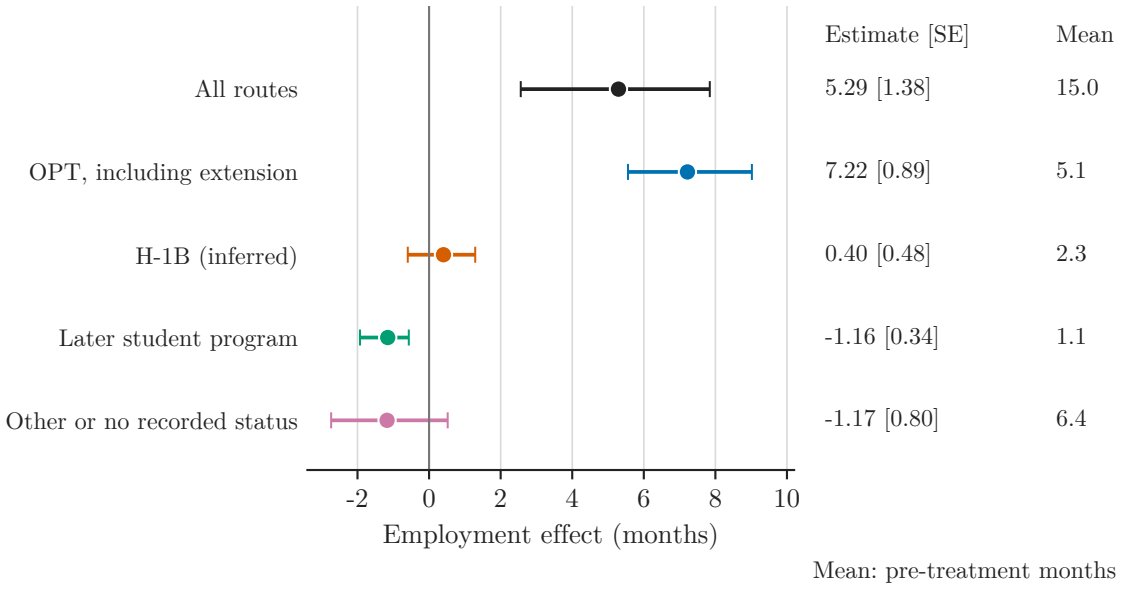
Notes: Effects on cumulative months of recorded U.S. work in the first three years after graduation, among linked F-1 graduates of programs reclassified as STEM-eligible in 2014–2019 and comparison programs. Follow-up ends by October 2023. Estimates follow equation 1, with comparison, cohort-size and inverse-linkage weights. Cohort 1 is the first reclassified class, 0 may be partly eligible, and −1 is the reference. The box gives the average effect over cohorts 1–4 and the pre-treatment mean. Work during F-1 study counts half unless full-time authorization is active. Intervals are 95% percentiles from 499 school-level bootstrap draws. Sources: IPEDS, SEVIS and Revelio.

Figure 7: Additional U.S. employment over five years



Notes: Percentage-point effects on the share of each year spent in recorded U.S. work, following the same linked F-1 graduates for five years through September 2026. Estimates compare 2014–2019 program reclassifications with comparison programs as in Section 4.2, using comparison, cohort-size and inverse-linkage weights and averaging the first four reclassified cohorts. The box gives cumulative effects and pre-treatment means in months; annual means appear below the bars. Work during F-1 study counts half unless full-time authorization is active; after SEVIS coverage ends in October 2023, work during later study counts fully. Intervals use ±1.96 school-level bootstrap standard errors from 499 draws. Sources: IPEDS, SEVIS and Revelio.

Figure 8: Additional U.S. work in the first three years, by immigration route



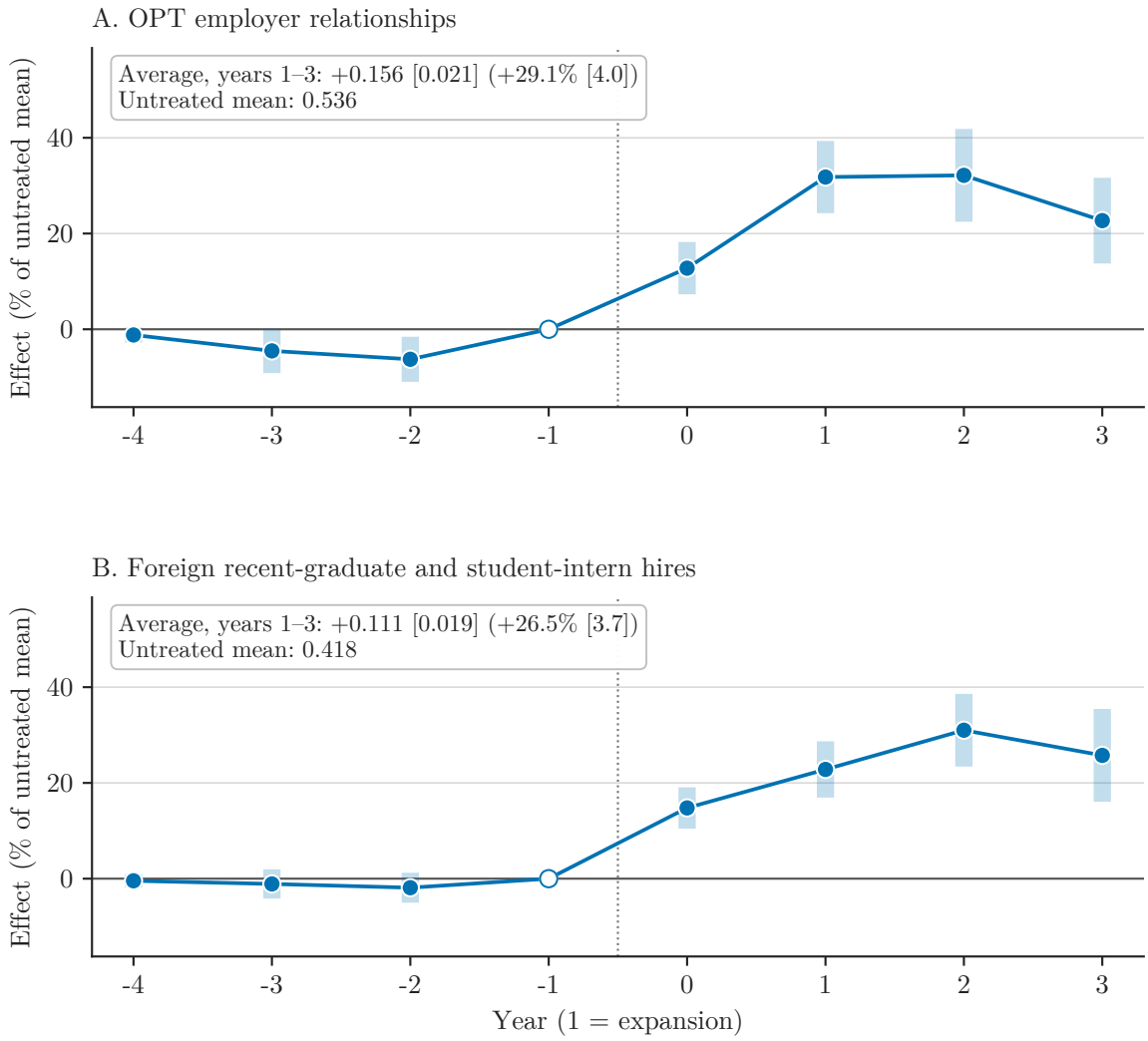
Notes: Effects on cumulative recorded U.S. work in the first three years after graduation; the four routes sum to total work. H-1B periods are inferred from undated requests. Later student programs include their work authorizations; the remaining group includes work during education recorded only in profiles. Work during F-1 study counts half unless full-time authorization is active. Linked F-1 graduates have three years of follow-up by October 2023. Estimates compare 2014–2019 program reclassifications with comparison programs as in Section 4.2, using comparison, cohort-size and inverse-linkage weights and averaging the first four reclassified cohorts. The right column gives pre-treatment means; intervals are 95% percentiles from 499 school-level bootstrap draws. Sources: IPEDS, SEVIS and Revelio.

Table 2: Pre-treatment characteristics of treated and comparison firms

		Comparison firms	
	Treated	Unweighted	Reweighted
A. Characteristics used in reweighting			
Distinct U.S. workers in year	50.8	291.4***	57.7**
OPT hiring	0.53	0.61***	0.49
Foreign graduate and intern hiring	0.43	0.58***	0.41
Native graduate and intern hiring	2.60	9.88***	2.62
H-1B demand	1.40	1.52**	1.31
Recruiting exposure (percentage points)	9.56	7.61***	9.62
B. Additional characteristics			
Other native hiring	8.13	43.44***	8.59
Other foreign hiring	1.70	4.77***	1.60
Firm-cohort observations	40,203	451,510	451,510
Unique treated firms	36,210		

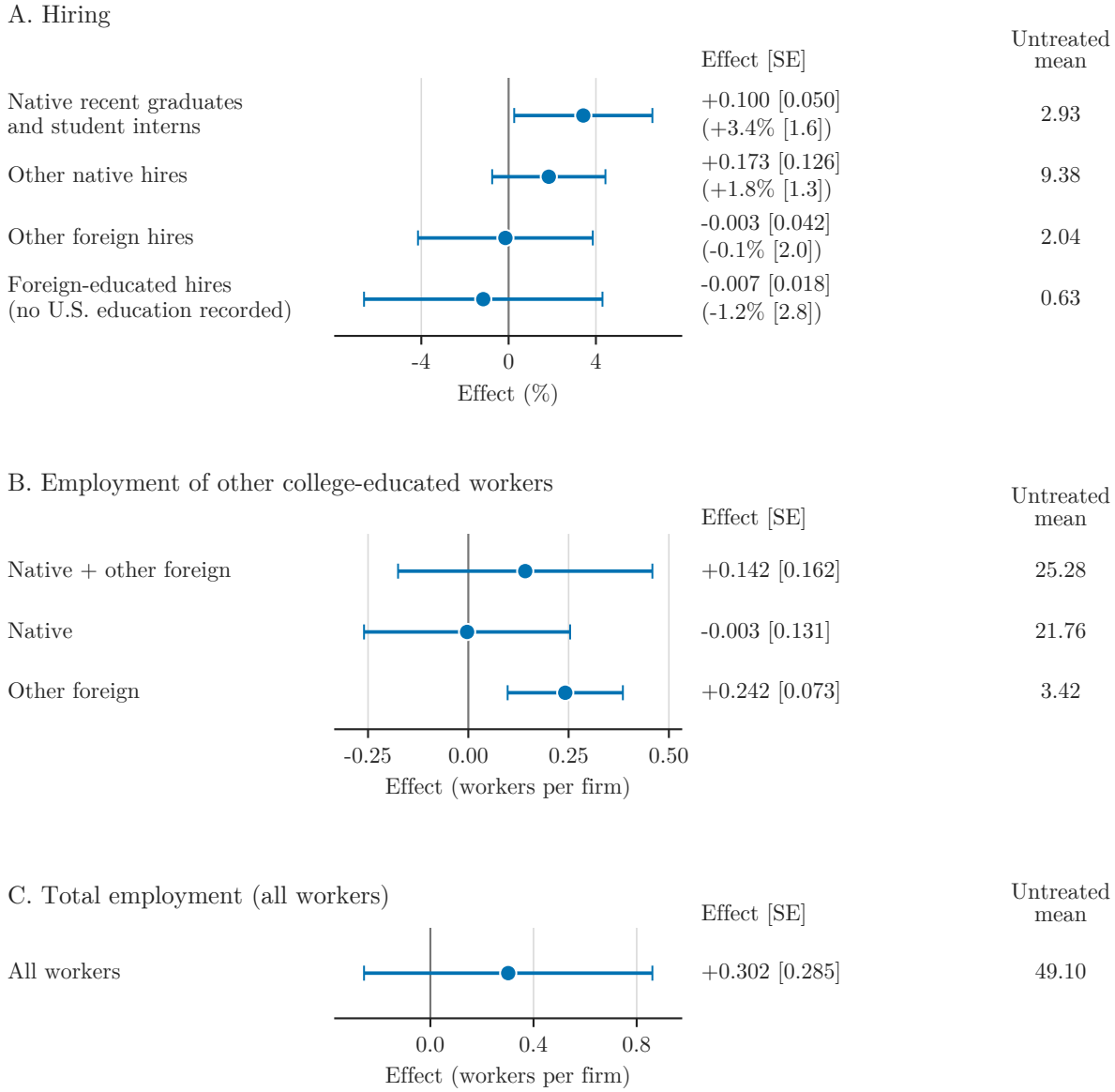
Notes: Means two years before the 2015–2019 expansions, for firms with large versus smaller increases in access to graduates of programs eligible for the OPT extension. Panel A reports characteristics whose pre-treatment histories enter reweighting, using each outcome's comparison weights; recruiting exposure uses the OPT-hiring weights. Panel B uses the OPT-hiring weights for additional hiring outcomes that do not enter that model. Cohorts are weighted by their numbers of treated firms. Worker and hiring counts are per firm; distinct workers counts anyone employed during the year. OPT hiring counts employer relationships in SEVIS, and H-1B demand counts requested positions. "Other" hiring excludes recent U.S. graduates and student interns; native and foreign origin is inferred from profiles. Recruiting exposure measures accumulated growth of eligible programs at earlier recruiting schools. Stars on comparison means test equality with the treated mean using standard errors clustered by corporate group, holding weights fixed: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. A firm can appear in several cohorts. See Section 5.2. Sources: SEVIS, Revelio, IPEDS, DOL and USCIS.

Figure 9: Graduate recruitment when eligible hiring pools expand



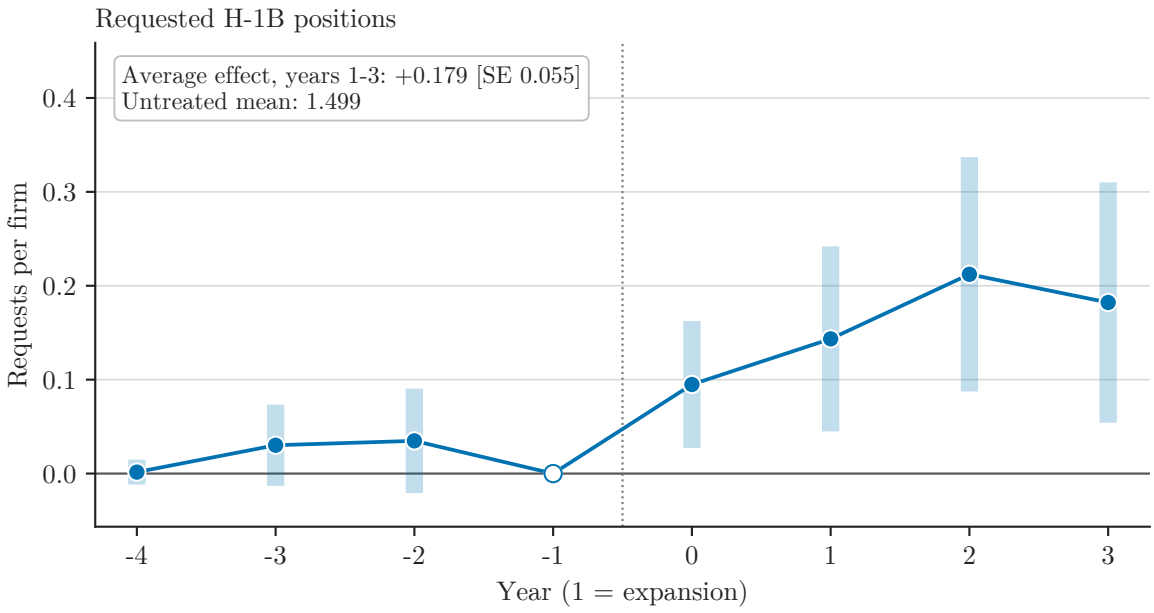
Notes: Effects of larger expansions of OPT-eligible graduating cohorts at firms' earlier recruiting schools in 2015–2019, estimated as in Section 5.2. Panel A counts OPT employer relationships by program-end year; B counts first jobs of recent U.S. graduates and student interns classified as foreign from profiles. The measures overlap. Year 1 is the measured expansion, 0 allows for earlier recruiting, and −1 is the reference. Paths show percentage effects relative to estimated hiring without the larger expansion. Boxes give average level effects over years 1–3 [SE], followed by percentages of the untreated mean. Whiskers are 95% intervals from 499 corporate-group bootstrap draws. Sources: SEVIS, Revelio and IPEDS.

Figure 10: Hiring and employment when graduate pools expand



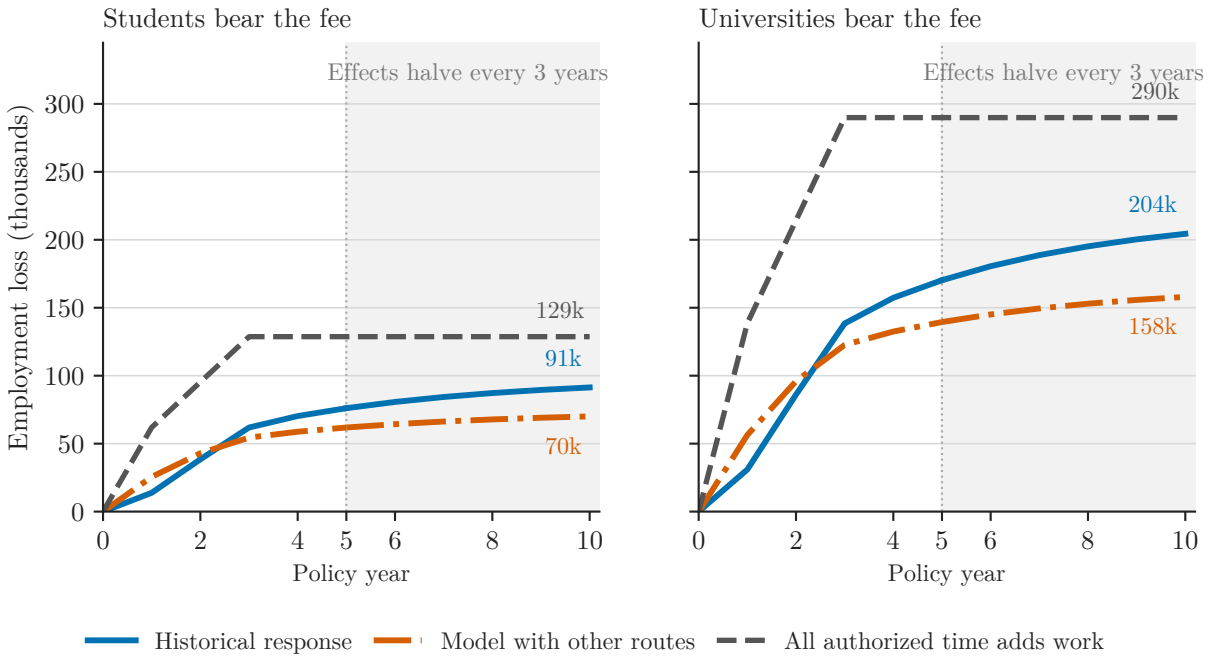
Notes: Effects of larger expansions of OPT-eligible graduating cohorts at firms' earlier recruiting schools in 2015–2019, estimated as in Section 5.2. Effects average the expansion year and next two years. Panel A counts first jobs; "other" hiring excludes recent U.S. graduates and student interns, while foreign-educated hires have no recorded U.S. education and overlap other foreign hires. Panels B–C average monthly worker counts. B includes workers with a completed bachelor's or higher degree, excluding foreign workers who entered as recent U.S. graduates or student interns; C includes all workers. Native and foreign refer to origin inferred from profiles. Labels give level effects [SE] and untreated means; A plots percentages of those means. Outcome-specific weights mean components need not add. Whiskers are 95% intervals from corporate-group bootstrap draws (499 in A; preliminary 99-draw inference in B–C). Sources: Revelio, SEVIS and IPEDS.

Figure 11: Demand for H-1B workers when eligible graduate pools expand



Notes: Effects of larger expansions of OPT-eligible graduating cohorts at firms' earlier recruiting schools in 2015–2019, estimated as in Section 5.2. The outcome counts H-1B positions requested on Labor Condition Applications and, from 2020, lottery registrations, which can include repeated requests. Counts are capped at the annual 99th percentile. Year 1 is the measured expansion, 0 allows for earlier recruiting, and −1 is the reference. Effects are requests per firm and year; the box gives the average over years 1–3 [SE] and estimated untreated mean. Whiskers are 95% intervals from 499 corporate-group bootstrap draws. Sources: DOL, USCIS, Revelio, SEVIS and IPEDS.

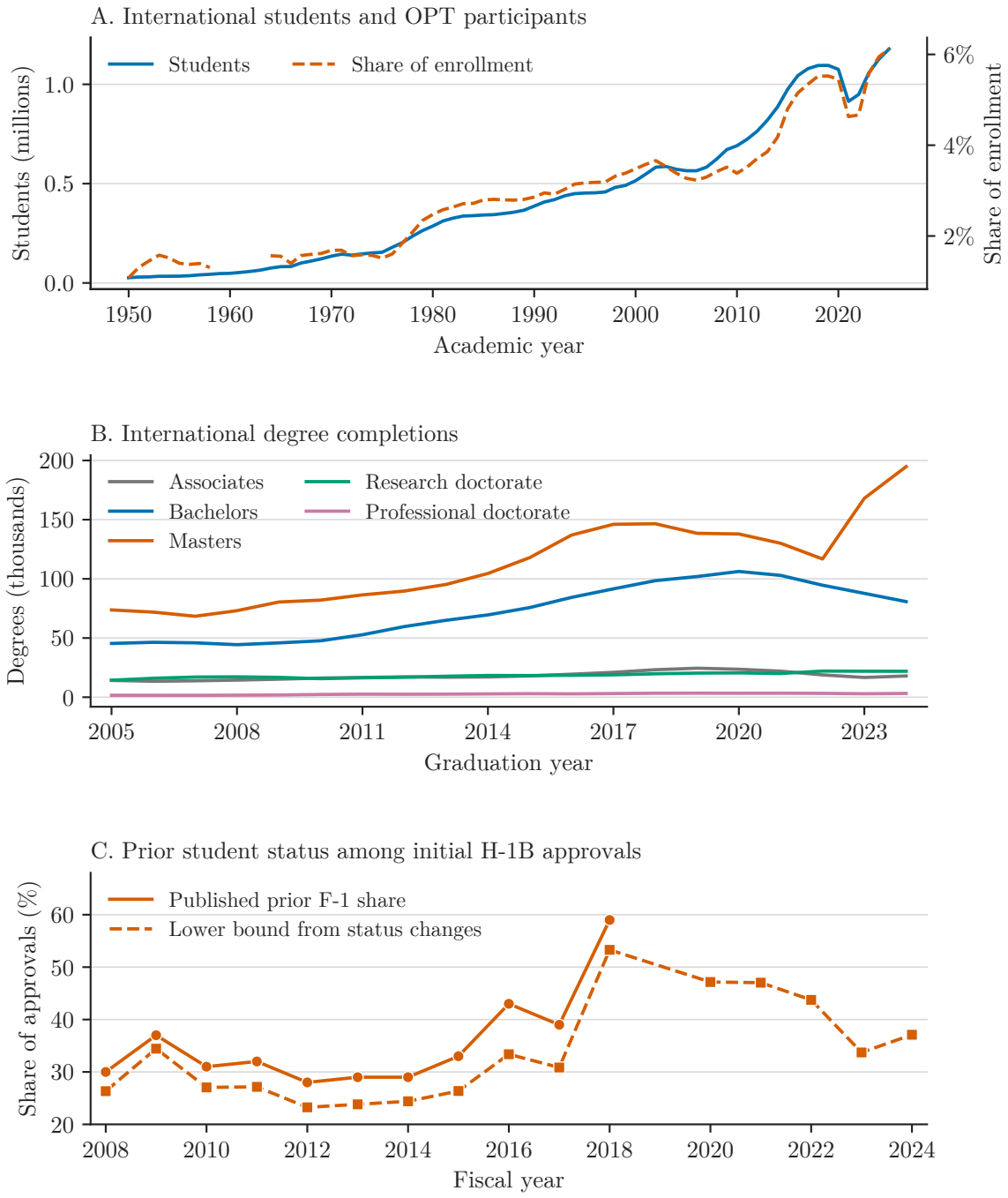
Figure 12: Projected employment losses from the proposed OPT fees



Notes: Annual employment losses under proposed fees of $70,000 for initial OPT and $100,000 for OPT with its extension (U.S. Department of Homeland Security, 2026; Clemens, 2026); panels differ in who bears the fee. A worker-equivalent is twelve months of recorded U.S. work, with study work generally counted at half weight. Policy year 1 begins with the first affected graduating cohort, and each year adds another. Blue curves apply the historical five-year employment response per additional extension user to every lost participant. Orange curves allow initial OPT, its extension and alternative immigration routes to respond separately. Gray curves count lost authorization as additional work. Shading extends the projections assuming each cohort's effect halves every three years after year five. Enrollment, annual participation responses and other workers' employment are held fixed. Appendix Figure E.2 gives sampling uncertainty for the blue curves; orange curves are conditional model projections.

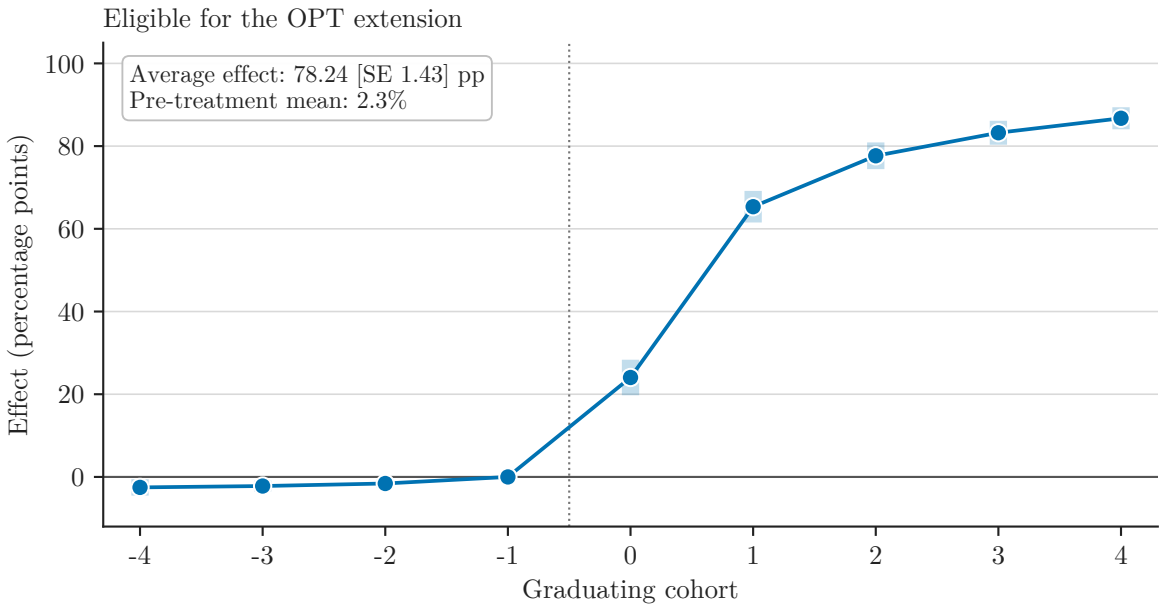
A Additional Figures and Tables

Figure A.1: International students and the transition to H-1B



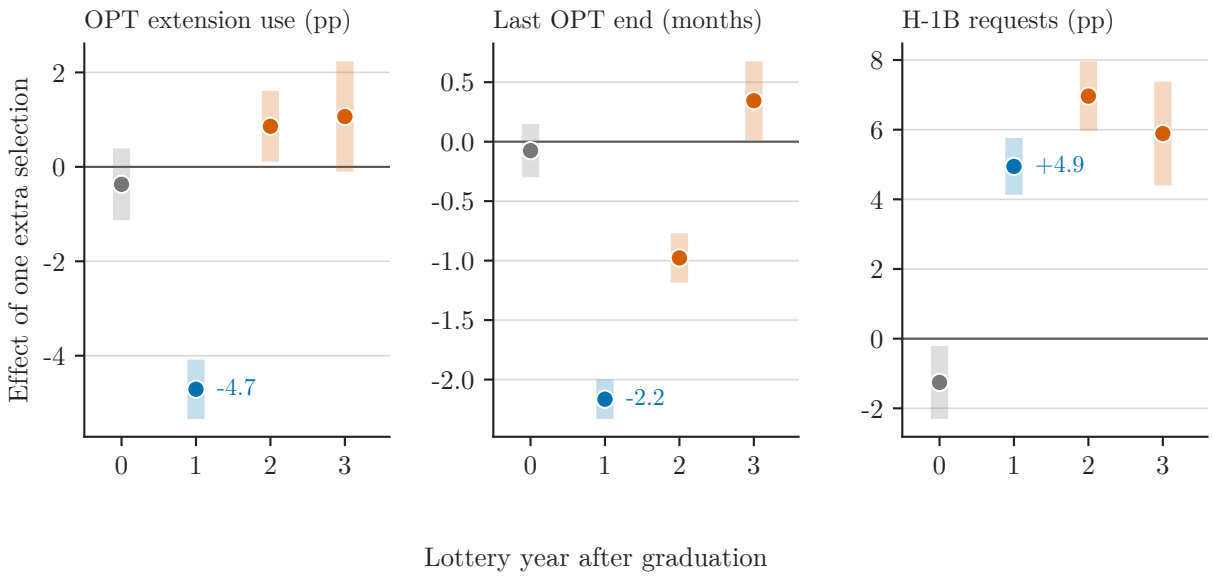
Notes: Panel A shows international students (millions, solid blue line) and their share of U.S. higher-education enrollment (dashed orange line); Open Doors includes recent graduates on OPT where reported. Panel B shows IPEDS nonresident degree completions in 2005–2024, counting the first major. In Panel C, the solid line is USCIS's published share of initial H-1B approvals with prior F-1 status through 2018. The dashed line divides changes from student status within the U.S. by all initial approvals, excluding consular processing from the numerator; it covers F-1 in 2008–2018 and F-1/F-2 in 2020–2024. Sources: Open Doors, IPEDS and USCIS (Institute of International Education, n.d.; National Center for Education Statistics, 2026*b*; U.S. Citizenship and Immigration Services, 2019, 2025).

Figure A.2: Relabeling and eligibility for the OPT extension



Notes: Percentage-point effects on OPT-extension eligibility recorded in SEVIS among F-1 graduates of programs reclassified as STEM-eligible in 2014–2019 and comparison programs. Estimates follow equation 1, with comparison and cohort-size weights. Cohort 1 is the first reclassified graduating class, 0 may be partly eligible, and −1 is the reference. The box gives the average over cohorts 1–4 [SE] and the pre-treatment mean. Whiskers are 95% intervals from 199 school-level bootstrap draws. Sources: IPEDS and SEVIS.

Figure A.3: Employer H-1B lottery selections and graduates' subsequent outcomes



Notes: Responses to one employer selection beyond the number expected given registrations and the share of graduates with a master's or doctoral degree. Extension use is measured among eligible OPT graduates; H-1B requests and elapsed months from program completion to the last authorization end use all OPT graduates. Observed elapsed months are limited to 0–48 per person before averaging and may include gaps. Shares are in percentage points. The sample pools 2020–2022 lotteries among employers with at most five registrations and graduation cohorts with at least two eligible OPT users. Year 0 is the graduation-year lottery; years 1–3 are the next three lotteries. Comparisons condition on registration count, grouped shares with a master's or doctoral degree and lottery year. Intervals are estimates plus or minus 1.96 employer-clustered standard errors. Outcomes describe employer cohorts, since individual registrants are not linked. See Section 4.4. Sources: SEVIS and USCIS H-1B registrations.

Table A.1: Relabeled programs by first relabeled cohort, degree level and field

	First relabeled IPEDS cohort						
	2014	2015	2016	2017	2018	2019	All
By degree level							
Bachelor's	1	13	12	32	45	52	155
Master's	3	7	7	32	31	41	121
Doctoral	2	6	6	13	14	18	59
By field group							
Economics	1	0	1	15	33	38	88
Architecture	0	5	2	18	16	30	71
Finance	0	1	3	9	2	6	21
Business	0	3	1	2	2	4	12
Psychology	1	7	13	14	17	19	71
Design	0	4	0	7	5	3	19
Communication	0	1	2	5	5	1	14
Other	4	5	3	7	10	10	39
All treated programs	6	26	25	77	90	111	335
Schools	4	18	24	46	63	75	169

Notes: Counts of the 335 programs relabeled from non-STEM to STEM-eligible codes in the administrative analysis. A program is a school–degree–field unit; columns date the first IPEDS graduating cohort under the new code. Field groups use the original code. School counts refer to distinct institutions within each column and need not sum across years. See Appendix D.1 for identification and sample restrictions. Sources: IPEDS and SEVIS.

Table A.2: Authorization and study choices around relabeling

A. Subsequent visa and study choices

	Requests H-1B status	Returns to F-1 study within two years	Starts a graduate program within three years
	(1)	(2)	(3)
Effect (percentage points)	3.57	-4.29	-3.02
	[0.94]	[0.79]	[0.61]
Pre-treatment mean (percent)	13.7	7.8	6.8
Program-cohort observations	207,974	198,365	186,689
Treated programs	335	335	335
Schools	1,415	1,415	1,415
Bootstrap draws	199	499	499

B. Work authorization before graduation

	Uses CPT (pp)	CPT authorization (months)	Uses pre-completion OPT (pp)
	(1)	(2)	(3)
Effect	5.30	0.20	-0.20
	[1.40]	[0.08]	[0.30]
Pre-treatment mean	18.70	0.86	2.30
Program-cohort observations	207,974	207,974	207,974
Treated programs	335	335	335
Schools	1,415	1,415	1,415
Bootstrap draws	199	199	199

Notes: Estimates compare programs relabeled as STEM-eligible in 2014–2019 with not-yet-relabeled programs, reweighted on pre-treatment characteristics. Effects average the first four relabeled graduating cohorts relative to the cohort two years before relabeling. Outcomes use all F-1 graduates in SEVIS, with pre-treatment cohort-size weights. Later-study outcomes require follow-up through the stated horizon by October 2023. H-1B requests are requests to change immigration status, not approvals. Curricular Practical Training (CPT) authorizes work that is part of the curriculum; pre-completion OPT uses the OPT allowance before graduation. Brackets give school-bootstrap standard errors. Program-cohort observations count the stacked estimation sample. pp denotes percentage points.

Table A.3: Program composition and employment of other graduates

A. Program completions

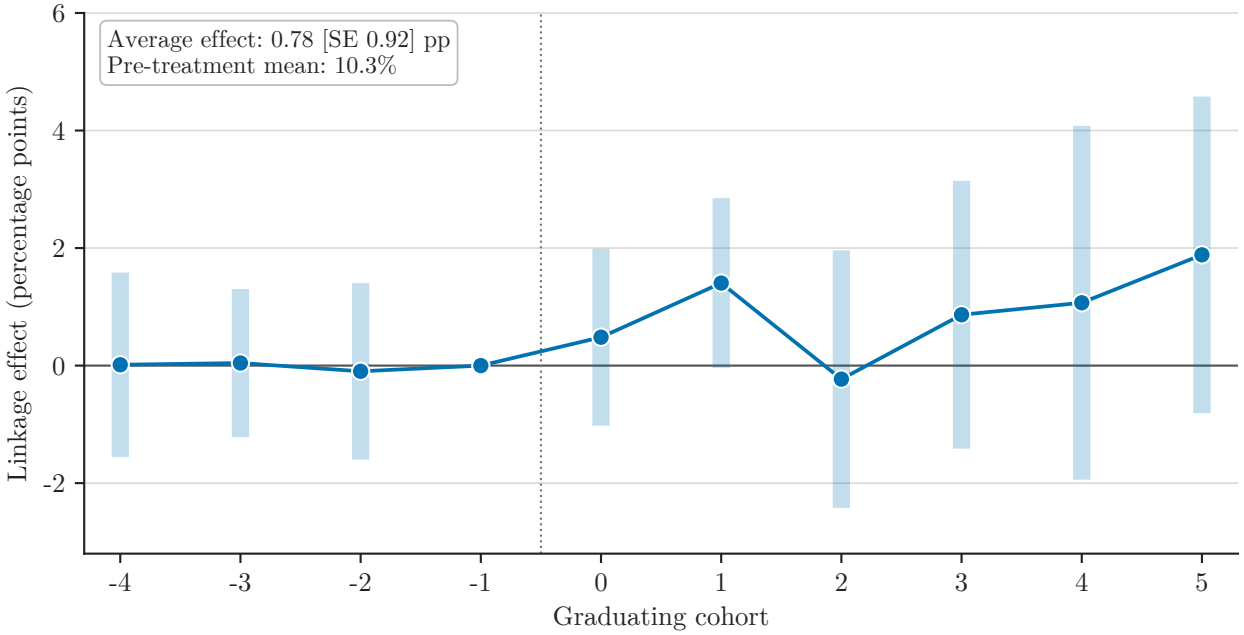
	All completions	International share (pp)
	(1)	(2)
Effect of relabeling	-0.64	4.40
	[1.69]	[0.75]
Pre-treatment mean	64.09	27.70
Program-cohort observations	266,220	256,866
Treated programs	335	335
Schools	1,415	1,412

B. Recorded U.S. employment after graduation

	Domestic year 1	Domestic year 2	Domestic year 3	Foreign year 1	Foreign year 2	Foreign year 3
	(1)	(2)	(3)	(4)	(5)	(6)
Effect (percentage points)	1.21	0.25	0.47	6.88	6.52	4.20
	[0.80]	[0.93]	[1.01]	[2.38]	[2.41]	[2.40]
Pre-treatment mean (percent)	61.6	64.1	64.0	41.1	36.6	33.3
Treated programs	271	271	271	218	218	218
All programs	24,843	24,843	24,512	12,323	12,323	12,067

Notes: Estimates compare programs relabeled as STEM-eligible in 2014–2019 with not-yet-relabeled programs, reweighted on pre-treatment characteristics. Effects average the first four relabeled graduating cohorts relative to the cohort two years before relabeling. Programs receive equal weights. Panel A uses IPEDS completions; international share is the fraction of degrees awarded to nonresident students. Panel B uses profiles matched to programs by school, degree and field; domestic and foreign groups are inferred from profiles and do not use SEVIS linkage. Employment is the percentage of months with a recorded U.S. job in each calendar year after graduation. Program-cohorts require at least one matched graduate in that group. Brackets give standard errors from 499 school-bootstrap draws. pp denotes percentage points.

Figure A.4: Relabeling and linkage to professional profiles



Notes: Percentage-point effects on linkage to a professional profile among F-1 graduates of programs reclassified as STEM-eligible in 2014–2019 and comparison programs. Estimates follow equation 1, with comparison and cohort-size weights. Cohort 1 is the first reclassified class, 0 may be partly eligible, and −1 is the reference. The box gives the average over cohorts 1–4 [SE] and the pre-treatment mean. Cohort 5 includes only 2014–2018 reclassifications. Whiskers are 95% intervals from 199 school-level bootstrap draws. Sources: IPEDS, SEVIS and Revelio.

Table A.4: Employment effects across samples and specifications

A. Employment over five years

	All cohorts			2014–2016		2017–2019	
	Main	Doubly robust	Fixed comparisons	Main	Fixed comparisons	Main	Fixed comparisons
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
U.S. work, years 1–5 (months)	5.45	6.26	4.15	12.11	11.98	4.35	2.90
	[1.88]	[2.55]	[1.78]	[2.33]	[2.66]	[2.09]	[1.94]
Years 1–3	4.43	4.67	3.23	7.18	6.89	3.93	2.62
	[1.28]	[1.50]	[1.09]	[1.24]	[1.49]	[1.44]	[1.21]
Years 4–5	1.02	1.59	0.92	4.94	5.09	0.41	0.29
	[0.85]	[1.28]	[0.86]	[1.47]	[1.56]	[0.92]	[0.92]
Pre-treatment mean, years 1–5	24.5	23.5	24.5	28.4	28.4	24.1	24.1
Program-cohort observations	55,838–60,781	—	55,838	22,564–24,992	22,564	33,274–35,789	33,274
Treated programs	248	—	248	30	30	218	218
Treatment cohorts	2014–2019	2015–2019	2014–2019	2014–2016	2014–2016	2017–2019	2017–2019

B. Additional checks over three years

	Main	Calibrated linkage weights	Two-year buffer	Three-year buffer
	(1)	(2)	(3)	(4)
U.S. work, years 1–3 (months)	5.29	4.32	4.79	4.38
	[1.38]	[1.47]	[1.31]	[1.25]
Pre-treatment mean (months)	15.0	14.7	15.0	15.0
Program-cohort observations	53,965–56,593	36,860–38,772	52,674–55,550	51,669–53,965
Treated programs	248	233	248	248
Treatment cohorts	2014–2019	2016–2019	2014–2019	2014–2019

Notes: Effects on U.S. work per F-1 graduate (months), averaged over the first four reclassified cohorts. Methods, weights and study-work adjustments follow Section 4.2. Panel A follows the same graduates for five years through September 2026; B uses three years through October 2023. Fixed comparisons exclude relabelings within six years after graduation; buffers exclude two or three years after follow-up (main: one). Calibration matches OPT-use shares. Brackets give school-bootstrap standard errors (499 draws; doubly robust: 479–484). Observation ranges span annual models. Doubly robust uses 2015–2019 cohorts; its sample counts are unavailable.

Table A.5: Potential and recorded OPT authorization

Authorization and employment benchmarks	Three-year work horizon (1)	Five-year work horizon (2)
Potential OPT authorization (months)	9.74	9.57
	[0.86]	[0.77]
Recorded OPT status (months)	7.60	—
	[0.81]	
Work / potential authorization	0.54	0.57
	[0.13]	[0.19]
Recorded status / potential authorization	0.78	—
	[0.03]	
Pre-treatment potential authorization (months)	8.0	8.0
Program-cohort observations	53,965–56,593	55,838–60,781
Treated programs	248	248
Follow-up sample	Three years	Five years

Notes: Potential authorization assigns twelve months to each Optional Practical Training (OPT) user and another seventeen or twenty-four months to each extension user, starting at graduation. Recorded status follows authorization dates in SEVIS. The first column uses linked graduates observed for three years by October 2023; the second holds graduates fixed through five years by September 2026. Authorization is measured over the first three years in both columns; the work ratio uses three and five years, respectively. Estimates compare programs relabeled as STEM-eligible in 2014–2019 with not-yet-relabeled programs, reweighted on pre-treatment characteristics. Effects average the first four relabeled graduating cohorts relative to the cohort two years before relabeling. Programs are weighted by their pre-treatment F-1 cohort size; linked graduates also receive inverse-linkage weights. Brackets give standard errors from 499 school-bootstrap draws; ratios use the paired draws. Observation ranges span annual models. Recorded OPT status is only reported for the three-year sample.

Table A.6: Employment during the original and later degree

A. Recorded U.S. work during later study (months)

	All months	Full-time CPT	Part-time CPT	Pre-completion OPT	Later program's OPT	No recorded authorization
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of relabeling	-1.16	-0.65	-0.08	0.00	-0.17	-0.26
	[0.34]	[0.22]	[0.04]	[0.00]	[0.15]	[0.15]
Pre-treatment mean (months)	1.14	0.30	0.05	0.00	0.27	0.53
Program-cohort observations	53,965–56,593	53,965–56,593	53,965–56,593	53,965–56,593	53,965–56,593	53,965–56,593
Treated programs	248	248	248	248	248	248

B. Time in later study (months)

	All months	Full-time CPT	Part-time CPT	Pre-completion OPT	Later program's OPT	No recorded authorization
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of relabeling	-1.02	-0.36	-0.04	0.00	-0.12	-0.50
	[0.21]	[0.06]	[0.01]	[0.01]	[0.04]	[0.16]
Pre-treatment mean (months)	1.81	0.24	0.05	0.00	0.25	1.27
Program-cohort observations	186,745	186,745	186,745	186,745	186,745	186,745
Treated programs	335	335	335	335	335	335

C. Recorded U.S. work in the final year before graduation

	All months, counted fully	No recorded authorization	With CPT	With pre-completion OPT	All months, part-time adjusted
	(1)	(2)	(3)	(4)	(5)
Effect (percentage points)	7.0	6.3	0.5	-0.1	3.7
	[3.2]	[3.1]	[0.8]	[0.1]	[1.6]
Pre-treatment mean (percent)	29.4	24.1	4.9	0.2	16.3
Program-cohort observations	65,874	65,874	65,874	65,874	65,874
Treated programs	248	248	248	248	248

Notes: Estimates compare programs relabeled as STEM-eligible in 2014–2019 with not-yet-relabeled programs, reweighted on pre-treatment characteristics. Effects average the first four relabeled graduating cohorts relative to the cohort two years before relabeling. Programs are weighted by their pre-treatment F-1 cohort size; linked graduates also receive inverse-linkage weights. Panels A–B cover the first three years after the original degree, with follow-up through October 2023. Panel A measures work for linked graduates; Panel B measures time for all F-1 graduates in SEVIS. Later-study months are assigned to OPT, full-time CPT, pre-completion OPT, part-time CPT, then no recorded authorization, in that order. CPT is Curricular Practical Training. Panel C covers the twelve months before graduation, using the broader sample observed over that window. Part-time adjustment counts work during F-1 study one-half unless full-time authorization is recorded. Brackets give school-bootstrap standard errors: 499 draws in A–B and 199 in C. Observation ranges span annual models.

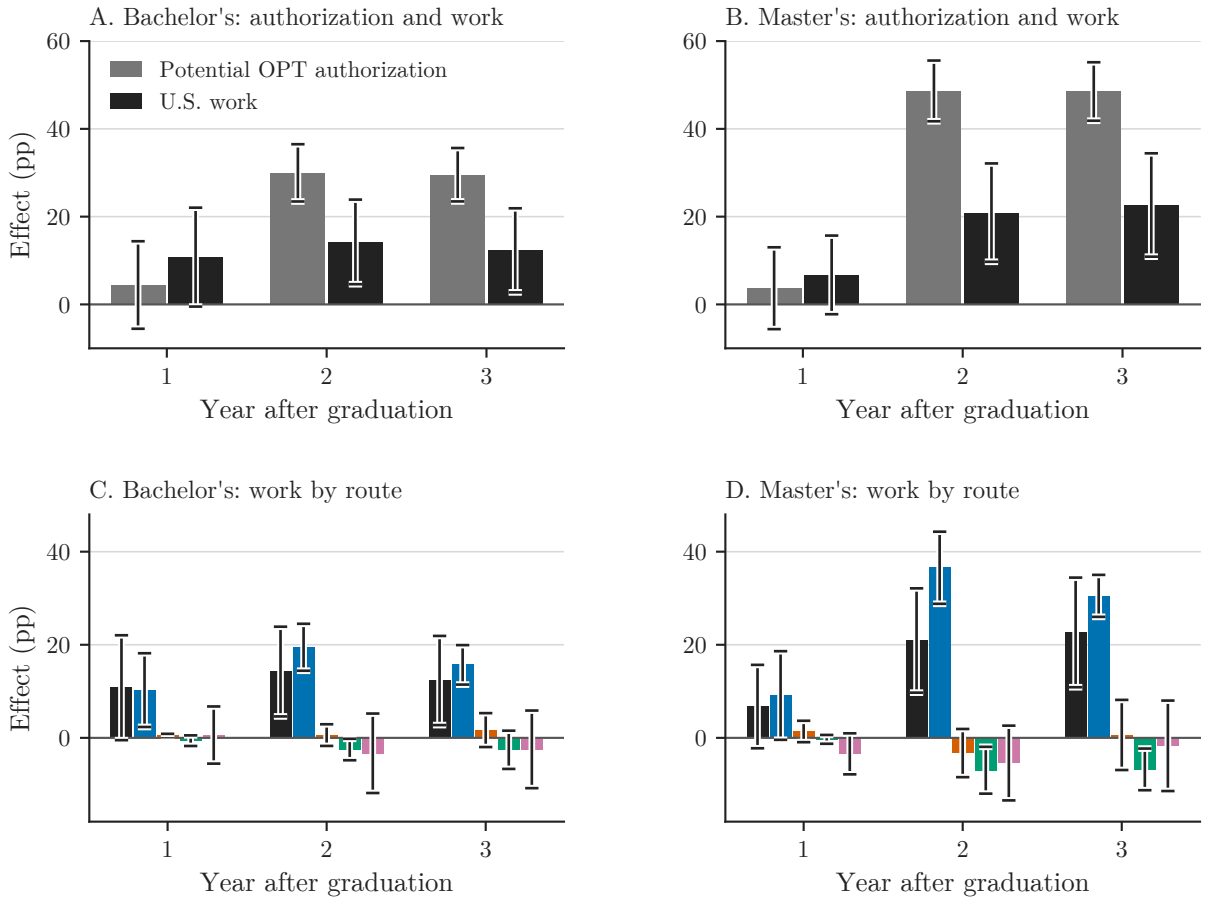
Table A.7: OPT-extension participation by degree and citizenship

A. By degree	Bachelor's	Master's	Doctoral
	(1)	(2)	(3)
Uses the STEM extension (pp)	18.1	32.8	13.2
	[1.9]	[2.4]	[2.0]
Pre-treatment mean (percent)	0.9	1.6	0.9
Program-cohort observations	133,642	63,289	11,043
Treated programs	155	121	59
Schools	1,349	894	212

B. By citizenship	India	China	English-speaking countries	Other countries
	(1)	(2)	(3)	(4)
Uses the STEM extension (pp)	44.4	18.0	24.1	24.9
	[3.6]	[2.3]	[2.8]	[1.7]
Pre-treatment mean (percent)	4.3	1.2	0.8	1.0
Program-cohort observations	23,534	79,053	22,278	160,437
Treated programs	147	241	122	295
Schools	801	982	1,040	1,386

Notes: Estimates compare programs relabeled as STEM-eligible in 2014–2019 with not-yet-relabeled programs, reweighted on pre-treatment characteristics. Effects average the first four relabeled graduating cohorts relative to the cohort two years before relabeling. The outcome is use of the STEM extension to Optional Practical Training (OPT), measured for all F-1 graduates in SEVIS. Models are fitted separately for each subgroup and weighted by pre-treatment F-1 cohort size. English-speaking countries are Australia, Canada, Ireland, New Zealand and the United Kingdom; this group uses 2015–2019 treatment cohorts. Brackets give standard errors from 199 school-bootstrap draws. pp denotes percentage points.

Figure A.5: Employment and authorization responses by degree level



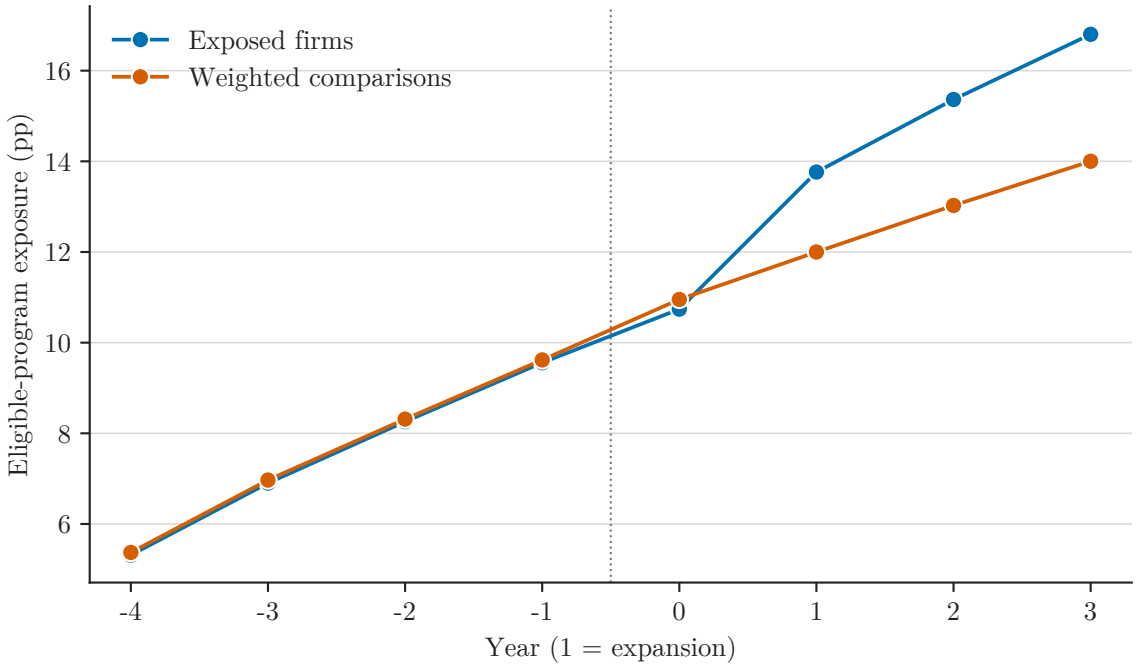
Pre-treatment means (% of months)

	B: 1	B: 2	B: 3	M: 1	M: 2	M: 3
Potential OPT authorization	58.2	1.4	1.4	76.8	1.3	1.2
U.S. work	38.5	30.3	29.5	52.0	50.6	47.0
OPT or extension	30.5	7.0	0.8	40.0	10.5	0.5
Inferred H-1B	0.2	3.3	5.2	1.3	12.0	16.5
Later F-1 program	0.8	3.8	6.0	0.6	4.5	4.8
Other or no recorded status	7.1	16.2	17.5	10.1	23.6	25.1

B: bachelor's; M: master's; numbers denote years

Notes: Panels A–B compare potential OPT authorization and recorded U.S. work; C–D divide work by immigration route. Effects are percentage points of months in years 1–3 after graduation. Potential authorization assigns twelve months per OPT user and seventeen or twenty-four more per extension user. Work during F-1 study counts half unless full-time authorization is active; H-1B status is inferred from requests. Samples hold fixed linked graduates observed through year 3 by October 2023, using 2015–2019 relabelings for bachelor's and 2014–2019 for master's graduates. Stacked difference-in-differences estimates use comparison and linkage weights and average the first four relabeled cohorts (Section 4.2). Intervals are estimates ±1.96 school-bootstrap standard errors from 499 draws. The table below the panels gives treated pre-treatment means. Sources: IPEDS, SEVIS and Revelio.

Figure A.6: Recruiting exposure of treated and comparison firms



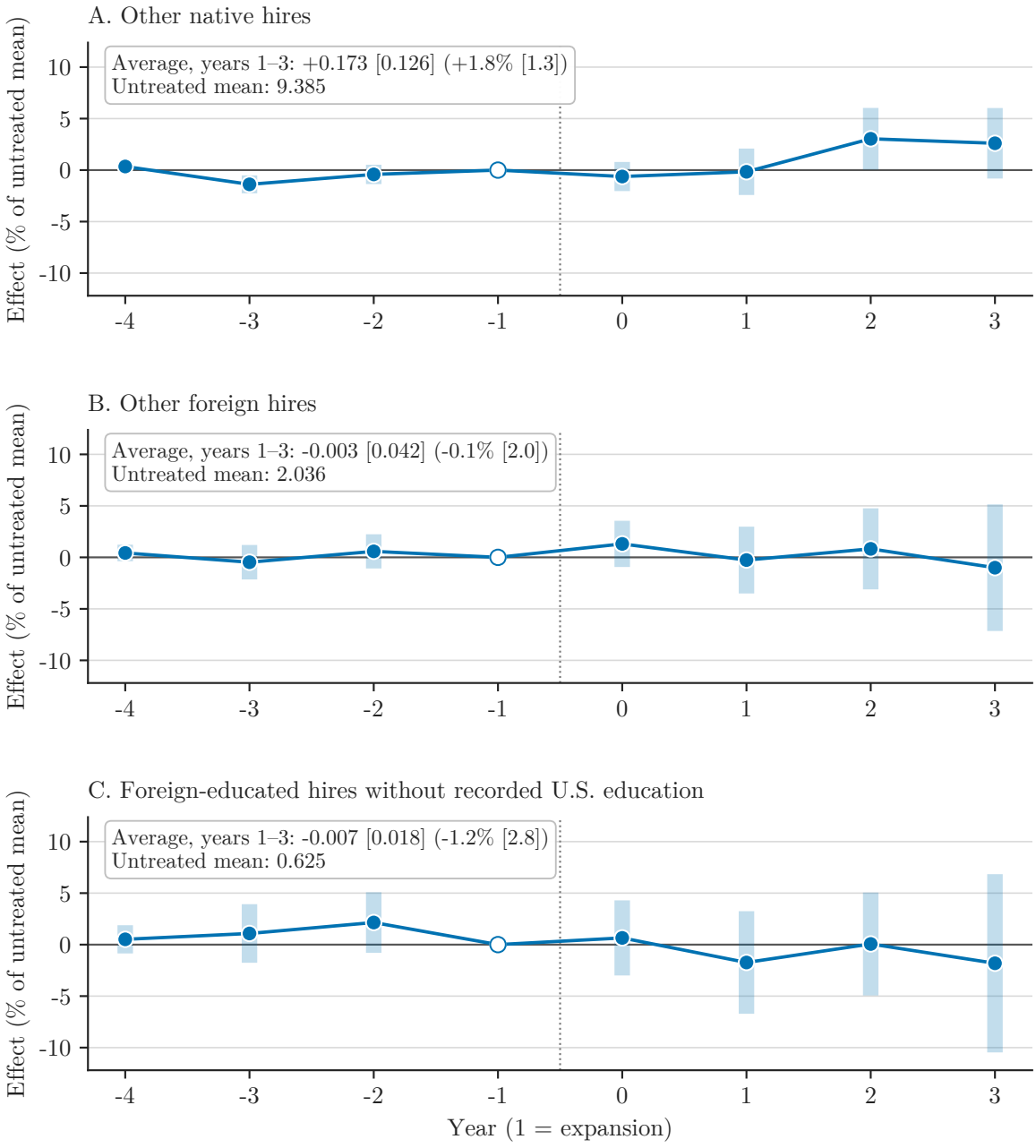
Notes: Expansion of OPT-eligible degree programs at firms' earlier recruiting schools, weighted by their shares of the firm's 2008–2013 hires (equation 3). The index adds increases in the eligible share of degrees attributable to growth of eligible programs, in percentage points. Lines compare large expansions in 2015–2019 with smaller expansions at firms weighted to have similar prior characteristics. Year 1 is the measured expansion, 0 allows for earlier recruiting, and −1 is the reference. Treatment cohorts are weighted by their number of treated firm episodes. Whiskers are 95% intervals from 499 corporate-group bootstrap draws. Sources: IPEDS, SEVIS and Revelio.

Table A.8: Employer sample and comparison weights by treatment cohort

	Treated firms	Comparison firms	Effective comparisons	Largest weight (percent)
	(1)	(2)	(3)	(4)
2015	7,956	82,159	6,520	0.39
2016	8,843	83,484	12,374	0.15
2017	9,816	89,252	9,679	0.18
2018	8,341	95,125	7,699	0.30
2019	5,247	101,490	12,822	0.16

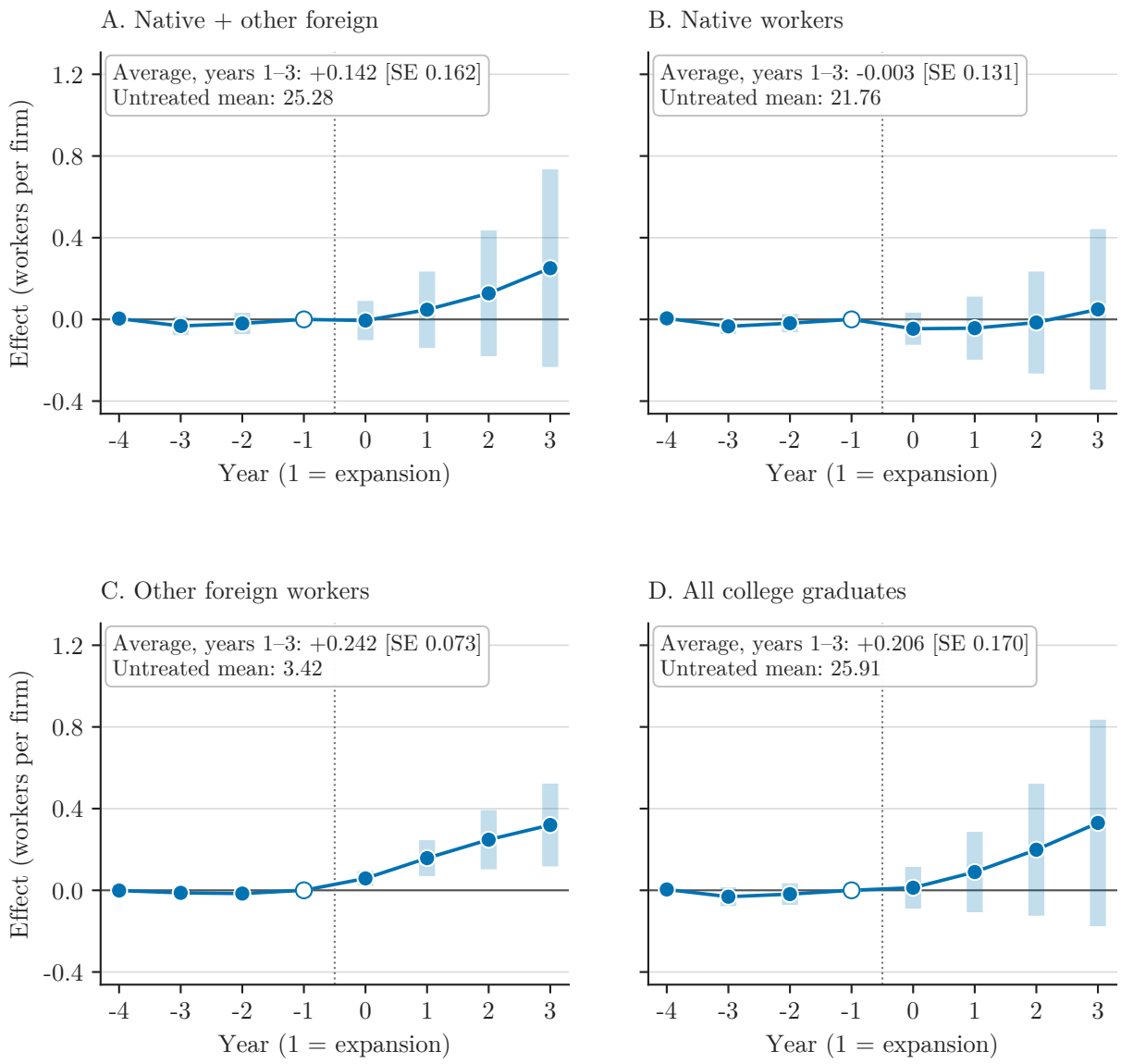
Notes: Numbers of treated and comparison firms by treatment cohort; a firm can appear in several cohorts. Treated firms experience large expansions of OPT-extension-eligible programs at their earlier recruiting schools; comparisons experience smaller expansions. The table uses the comparison weights for OPT hiring. For weights normalized to sum to one, effective comparisons equal $1/\sum_f \omega_f^2$; the final column gives the largest weight. Table 2 reports pre-treatment balance, and Section 5.2 describes the weighting.

Figure A.7: Other native and foreign hiring around exposure increases



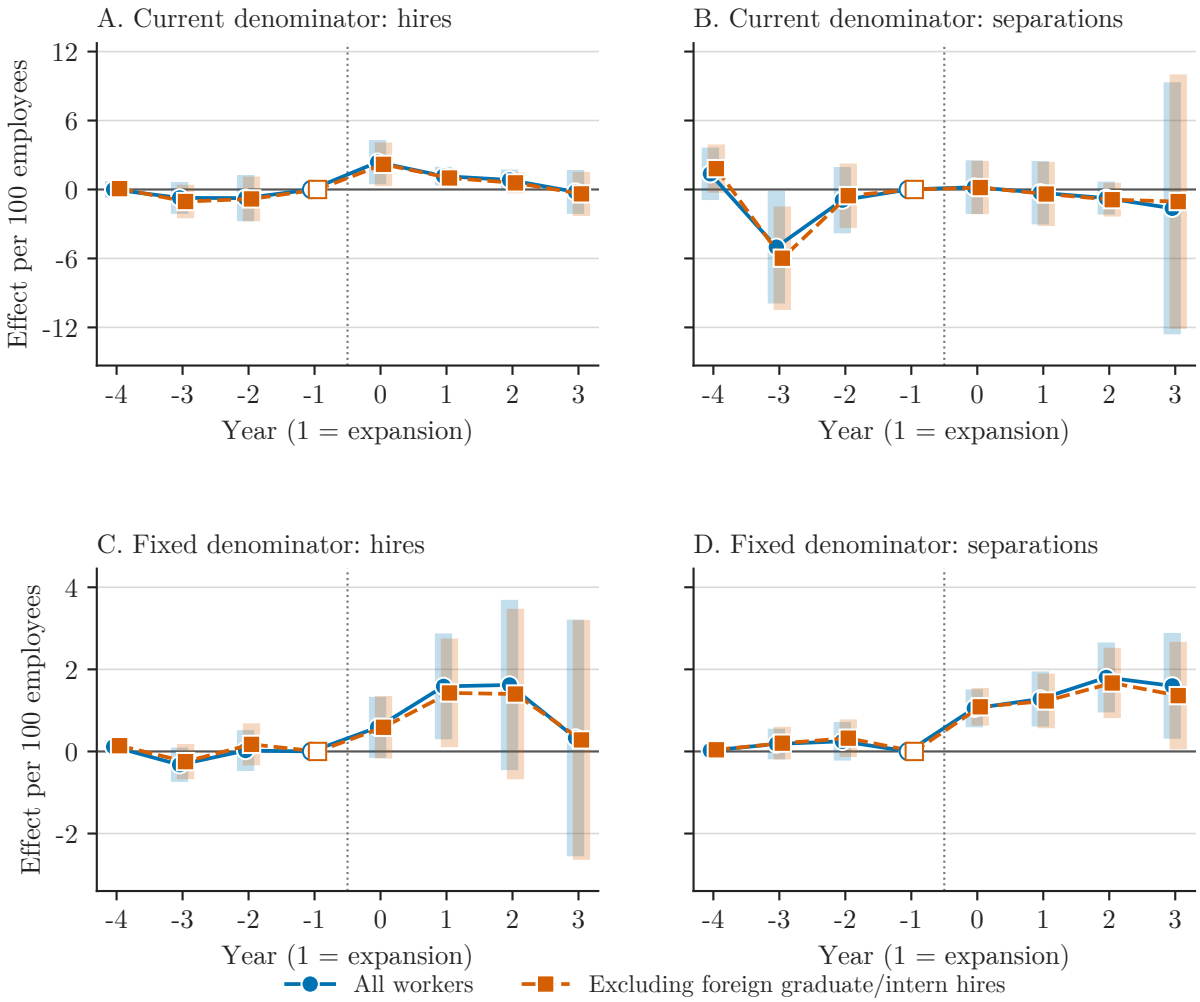
Notes: Effects of larger expansions of OPT-eligible graduating cohorts at firms' earlier recruiting schools in 2015–2019, estimated as in Section 5.2. Panels count first jobs among other native workers (A), other foreign workers (B), and foreign-educated workers with no recorded U.S. education (C). "Other" excludes recent U.S. graduates and student interns; origin is inferred from profiles. C overlaps B and can include workers already in the United States. Year 1 is the measured expansion, 0 allows for earlier recruiting, and −1 is the reference. Paths show percentages of estimated hiring without the expansion. Boxes give average level effects over years 1–3 [SE], then percentages of the untreated mean. Whiskers are 95% intervals from 499 corporate-group bootstrap draws. Sources: Revelio, SEVIS and IPEDS.

Figure A.8: College-educated employment around graduate-pool expansions



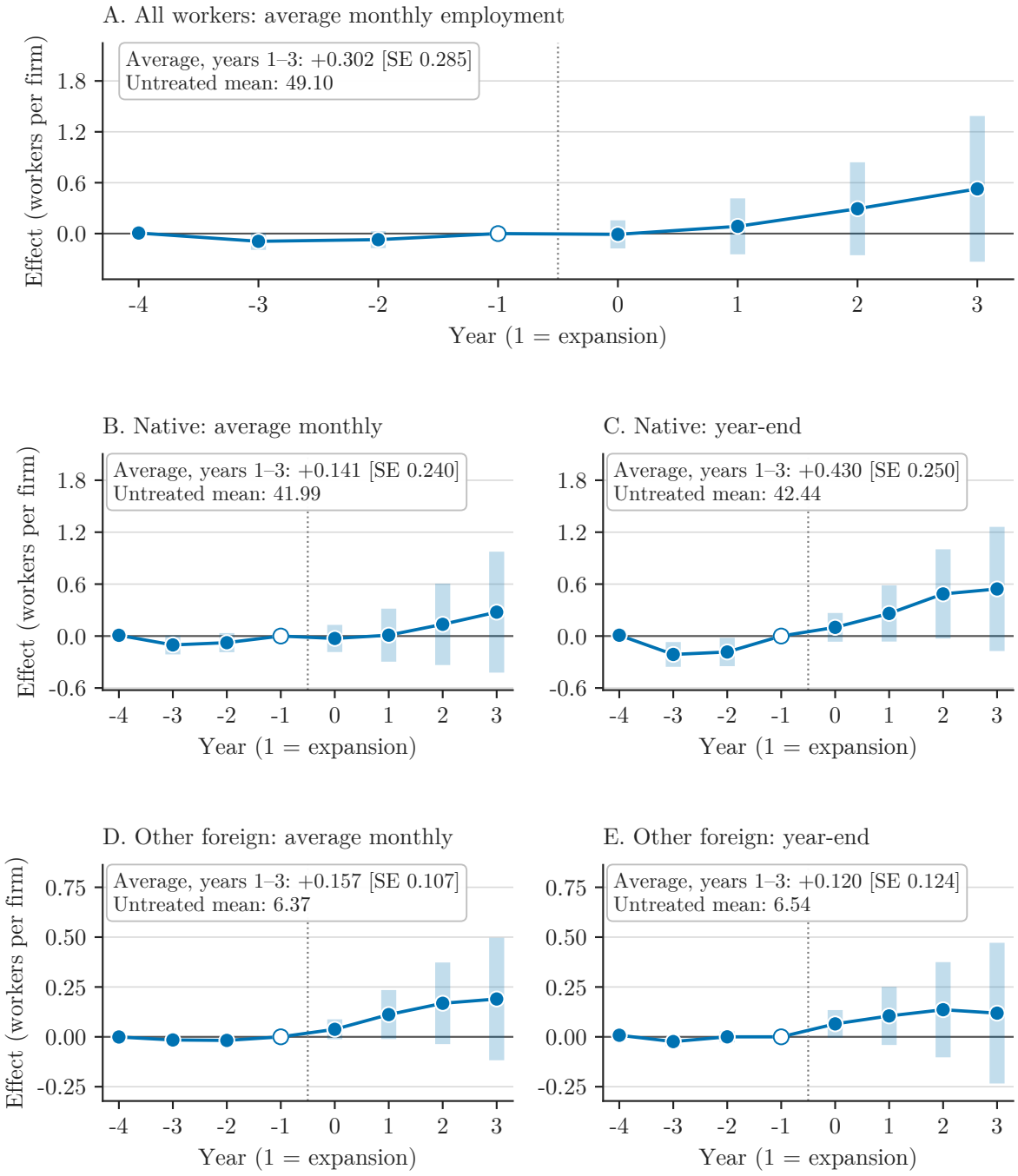
Notes: Effects of larger expansions of OPT-eligible graduating cohorts at firms' earlier recruiting schools in 2015–2019, estimated as in Section 5.2. Employment averages monthly counts of workers with a completed bachelor's or higher degree. A–C exclude foreign workers who entered as recent U.S. graduates or student interns; native recent graduates remain included. D includes the excluded group. Origin is inferred from profiles. Year 1 is the measured expansion, 0 allows for earlier recruiting, and −1 is the reference. Boxes give average effects over years 1–3 [SE] and estimated untreated means. Separate outcome weights mean components need not add. Whiskers are preliminary 95% intervals from 99 corporate-group bootstrap draws. Section 5.4 describes degree coverage. Sources: Revelio, SEVIS and IPEDS.

Figure A.9: Hiring and separation flows across the firm's workforce



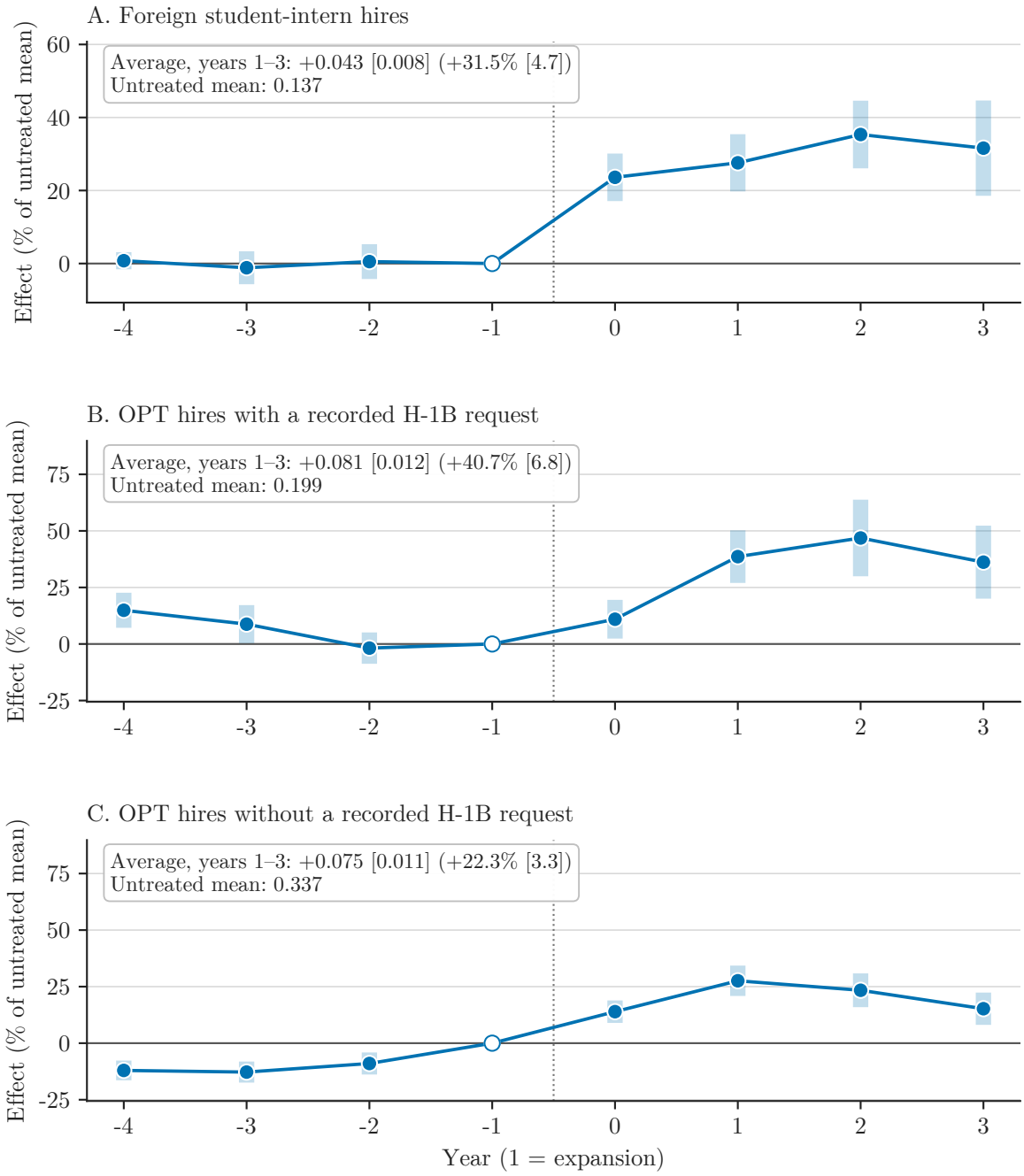
Notes: Rates count hires, including rehires, and departures on the day after a job ends. Panels A–B divide by current-year average daily employment; C–D use average daily employment over the four years before exposure. The first measure requires positive employment throughout follow-up, while the second retains later years with no employees. Scales differ across rows. "All workers" excludes identified internship spells; the second sample also excludes workers classified at first entry as foreign recent graduates or student interns. Effects of larger expansions of OPT-eligible graduating cohorts at firms' earlier recruiting schools in 2015–2019, estimated as in Section 5.2. Year 1 is the measured expansion, 0 allows for earlier recruiting, and −1 is the reference. The adjustment includes earlier hiring and departure rates; samples and weights differ between the two denominators (Appendix D.3). Whiskers are preliminary 95% intervals from 99 corporate-group bootstrap refits. Estimated rates without the larger expansion in years 1–3 are 26.4, 30.9, 53.9 and 38.4 per 100 employees in A–D for all workers, and 26.2, 30.7, 53.5 and 38.0 for the second sample. Sources: Revelio, SEVIS and IPEDS.

Figure A.10: Employment effects under monthly and year-end measures



Notes: Effects of larger expansions of OPT-eligible graduating cohorts at firms' earlier recruiting schools in 2015–2019, estimated as in Section 5.2. A shows average monthly employment at all education levels; B–E compare twelve-month averages with December 31 counts for native and other foreign workers. Origin is inferred from profiles; other foreign excludes workers entering as recent U.S. graduates or student interns. Year 1 is the measured expansion, 0 allows for earlier recruiting, and −1 is the reference. Boxes give average effects over years 1–3 [SE] and estimated untreated means, in workers per firm. Outcomes have separate comparison weights; whiskers are preliminary 95% intervals from 99 corporate-group bootstrap draws. Sources: Revelio, SEVIS and IPEDS.

Figure A.11: Student internships and OPT hiring by H-1B request status



Notes: Effects of larger expansions of OPT-eligible graduating cohorts at firms' earlier recruiting schools in 2015–2019, estimated as in Section 5.2. A counts foreign students whose first job is short and overlaps U.S. enrollment. B–C split OPT employer relationships by whether the graduate has a recorded H-1B request, assigning hiring to program-end year. Requests are undated, need not concern this employer, and do not establish approval. Year 1 is the measured expansion, 0 allows for earlier recruiting, and −1 is the reference. Paths show percentages of estimated hiring without the expansion. Boxes give average level effects over years 1–3 [SE], then percentages of the untreated mean. Whiskers are 95% intervals from 499 corporate-group bootstrap draws. Sources: SEVIS, Revelio and IPEDS.

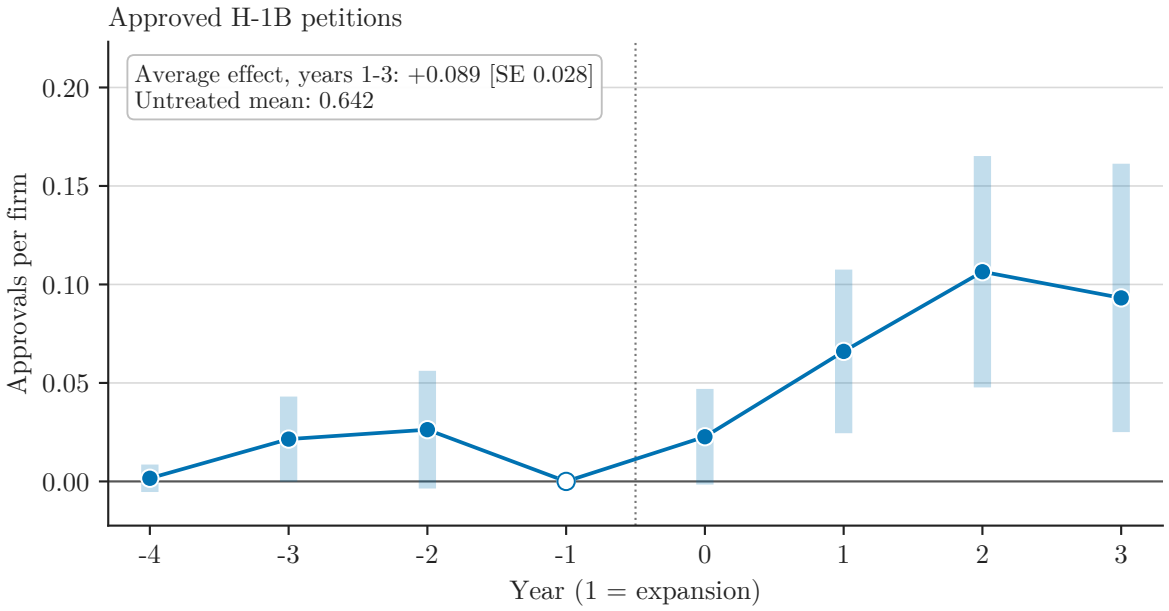
Table A.9: Timing and inference for the employer estimates

A. Timing and program eligibility of OPT hiring	Calendar-year hiring (1)	Academic-year hiring (2)	From eligible programs (3)	From other programs (4)
Three years before expansion	−0.030	−0.004	0.006	−0.010
	[0.012]	[0.011]	[0.009]	[0.005]
Year before expansion	0.074	0.056	0.042	0.014
	[0.016]	[0.014]	[0.013]	[0.005]
Expansion year and next two	0.156	0.175	0.175	−0.001
	[0.021]	[0.022]	[0.021]	[0.005]
Counterfactual mean	0.536	0.536	0.355	0.179
Treated firm-cohort observations	40,203	40,203	40,203	40,203
Comparison firm-cohort observations	451,510	451,510	451,510	451,510

B. Uncertainty under alternative clustering	OPT hiring (1)	Foreign graduate and intern hiring (2)	Native graduate and intern hiring (3)	Distinct workers in year (4)
Average effect	0.156	0.111	0.100	0.316
SE: corporate groups	[0.021]	[0.019]	[0.050]	[0.407]
SE: recruiting schools	[0.022]	[0.019]	[0.038]	[0.377]
Counterfactual mean	0.54	0.42	2.93	62.50
Treated firm-cohort observations	40,203	40,203	40,203	40,203
Comparison firm-cohort observations	451,510	451,510	451,510	451,510
Corporate groups	123,437	123,437	123,437	123,437
Recruiting schools	3,238	3,238	3,238	3,238

Notes: Effects compare firms with large versus smaller recruiting-pool expansions during 2015–2019, adjusting for prior differences; the reference is two years before expansion. Panel A counts SEVIS OPT employer relationships, assigned to the calendar or July–June academic year of degree completion. The final two columns split academic-year hiring by eligibility for the STEM extension. Panel B averages the expansion year and next two years; the last column counts distinct workers seen during a year, rather than average monthly employment. Counterfactual means estimate outcomes without the larger expansion. Brackets give standard errors from 499 multiplier-bootstrap draws that refit comparison weights. Recruiting-school clustering groups firms by their largest historical recruiting share. See Section 5.2.

Figure A.12: H-1B approvals when eligible graduate pools expand



Notes: Effects of larger expansions of OPT-eligible graduating cohorts at firms' earlier recruiting schools in 2015–2019, estimated as in Section 5.2. The outcome counts new and continuing H-1B petition approvals by fiscal decision year, capped at the annual 99th percentile. Year 1 is the measured expansion, 0 allows for earlier recruiting, and −1 is the reference. Effects are approvals per firm and year; the box gives the average over years 1–3 [SE] and estimated untreated mean. Whiskers are 95% intervals from 499 corporate-group bootstrap draws. Sources: USCIS, Revelio, SEVIS and IPEDS.

B Data Quality and Linkage

The employment analyses in Sections 4 and 5 require following graduates and employers beyond the work recorded in student-visa files. I first check the coverage of these files and the LinkedIn histories, then explain how I connect the two sources and assess the resulting links. The links between individual students and profiles support the graduate analysis; separate links between companies support the employer analysis.

B.1 Data Quality and Coverage

B.1.1 SEVIS Administrative Records

Before using the student-visa records to follow graduates over time, I need to distinguish new information from records carried forward across years. The Student and Exchange Visitor Information System (SEVIS) files are annual snapshots, so a student who remains enrolled or uses practical training can appear repeatedly with the same education and employment information. Within each file, a person identifier connects the student's records, but these identifiers change across annual files. Combining the files without accounting for this repetition would count many students and authorizations more than once.

I use the repeated information to connect records belonging to the same person. Across annual files, records are connected when their contents agree exactly apart from the regenerated identifiers; within a file, the reported person identifier connects them. I assign a common identifier to records connected in either way, excluding connections that would merge different person identifiers from the same annual file. Of the 68,104,536 rows in the 2006–2022 files, this procedure assigns 68,063,835 to 4,641,311 people; the remaining 40,701 rows have ambiguous or inconsistent connections. These are counts of repeated records and reconstructed people, before restricting to the degrees and graduating cohorts used in the analysis.

The year of a file does not necessarily indicate when the recorded activity took place. I assign graduating cohorts using the actual program-end calendar year and retain education and authorization dates even when they fall outside the year of the file containing them. I use the continuous 2006–2022 series and treat October 2023 as the last date covered by the administrative records, rather than taking future authorization endpoints as evidence that a graduate remains observed.[77]

Employers and employment dates. The records identify which firms employed a student, but do not always identify when the student worked at each firm. Beginning with the 2015

[77] Several early files require correcting their year labels, one nominal year duplicates another, and the final 2023 file is incomplete. I correct the early labels and omit the duplicate and incomplete files.

files, a student who has worked at two employers may have each employer attached to all of the student's employment intervals. Using those dates for each firm would overstate employment duration and firm-level activity. I therefore keep the set of employers separate from the authorization intervals: the former identifies firms that hired a graduate on OPT, while the latter measures authorized time, combining overlapping valid intervals so that each day is counted once. Because these records cannot reliably measure a firm's OPT headcount in a particular calendar year, I assign its graduate hires to program-end cohorts and use the linked work histories to measure individual employment.

Changes of status. To learn about immigration routes after F-1 study, I use SEVIS requests to change status, including requests associated with H-1B. These requests are recorded for the person without dates, so they establish neither approval nor the timing or duration of a subsequent status. The main text reports the requests directly; Appendix C explains how I combine them with work and education histories to assign status over time. Requests do not capture every transition, since a graduate obtaining a visa through consular processing or after the student record closes may have no corresponding request in these files.

Comparison with degree awards. I check the reconstructed graduate counts against universities' degree awards reported to the Integrated Postsecondary Education Data System (IPEDS). For the 2012–2018 cohorts, F-1 completers total 95.9% of IPEDS awards to nonresident students at the bachelor's level and 94.6% at the master's level, although both ratios fall to about 79% in 2018. At the doctoral level, SEVIS counts total 176.1% of research-doctorate awards, and including professional and other doctorates in the IPEDS denominator still leaves a substantial discrepancy.[78]

B.1.2 LinkedIn Coverage

I also compare LinkedIn education histories with IPEDS degree awards to assess how broadly the profile data cover graduates across fields and years. For these coverage comparisons, broad fields follow the two-digit categories of the Classification of Instructional Programs (CIP), which group related subjects such as engineering or business. I use all profiles, including people who cannot be linked to an F-1 student, and count distinct users within degree level, broad field, and graduation year for the 2008–2024 classes, retaining education records whose school can be matched to IPEDS and whose field can be assigned a CIP category. Graduation

[78] For this comparison, completion requires completed-program status or OPT after graduation in SEVIS. I use the latest reported program-end date and field and count each person once within degree level, field, and year. This is narrower than the program-end records used in the analysis, and F-1 students and IPEDS nonresident students are different populations; the doctoral discrepancy remains unresolved.

year is the reported end year; when that is missing, I add four, two, or six years to the start year for bachelor's, master's, or doctoral study, respectively.[79]

The profile data cover a substantial share of reported degrees and closely track differences in the size of fields and graduating classes (Table B.1). The relationship remains positive after accounting for each field's average size, although coverage within fields is not constant. A similar comparison within institution–degree groups gives a correlation of 0.55 between log profile and IPEDS counts after subtracting each group's mean. These comparisons provide evidence on the breadth of the profile data beyond the graduates used in the individual linkage.

I compare the distribution of degrees across broad fields and degree levels for the 2010–2020 classes (Figure B.1). I summarize the overlap by adding the smaller share from the two sources in each category, so 100% means identical distributions, not complete coverage of graduates. The distributions overlap by 87.2% overall and by 94.0% within the categories containing the programs used in the analysis.[80]

The count ratios should not be read as the probability that a graduate has a profile, because the sources count somewhat different objects. IPEDS counts degrees by first major, while a user reporting majors in two broad fields can appear in both profile counts; the profile doctorate category can also include professional degrees, while this IPEDS comparison uses research doctorates. I assess the links between profiles and individual F-1 records separately in Appendix B.2.2.

[79] IPEDS reporting year Y covers July of $Y-1$ through June of Y, whereas profiles use calendar years. Requiring a reported end year changes the overall ratio of profile counts to IPEDS awards from 0.55 to 0.54.

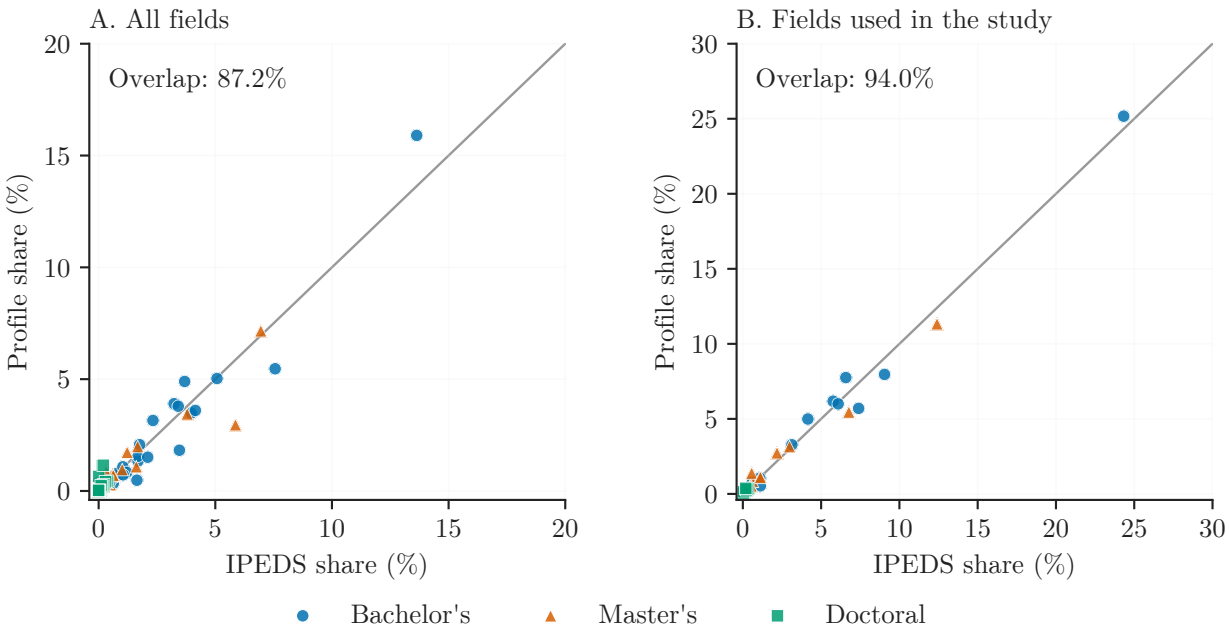
[80] I use reported graduation years for this comparison. The restricted comparison includes the national broad-field and degree categories containing each program's original or new classification, including other institutions and related subfields.

Table B.1: LinkedIn education histories relative to IPEDS degree awards

	All degrees	Bachelor's	Master's	Doctoral
Profiles per 100 degree awards	55.0	53.8	53.2	110.1
Correlation of log counts	0.95	0.96	0.95	0.88
Correlation within fields	0.75	0.77	0.63	0.43
IPEDS degree awards	47,011,135	32,193,431	13,626,486	1,191,218
Profile education counts	25,839,336	17,327,621	7,244,611	1,311,226
Positive field–year pairs	644	644	619	548
Pairs excluded from correlations	187	184	209	271

Notes: Graduating classes 2008–2024, including all origins. IPEDS counts degrees by first major; profiles count distinct users within degree level, broad field, and year. Broad fields follow the two-digit Classification of Instructional Programs. A user reporting two fields can appear in both, and doctoral profiles can include professional degrees while IPEDS counts only research doctorates, so the ratios need not equal the share of graduates with profiles. Correlations use log counts where both sources have positive counts; the within-field correlation subtracts each field's mean log count in each source before computing the correlation. Count ratios retain all groups. Sources: Revelio and IPEDS (Revelio Labs, 2026*b*; National Center for Education Statistics, 2026*b*). The all-degree profile count combines degree levels within each field and year, so it need not sum to the degree columns. The bottom rows report the underlying counts and the pairs omitted because a count is nonpositive.

Figure B.1: Profile education histories compared with U.S. degree awards



Notes: Each dot is a broad field and degree level for the 2010–2020 graduation classes, including graduates of all origins. Axes show shares of profile education records and universities' degree awards reported to IPEDS; the line indicates equal shares. Panel A includes all categories; Panel B includes the 33 national categories containing the study programs' original or new classifications. I use reported graduation years and do not require a recorded job. Profiles count once per person, field, degree and year; IPEDS counts first-major awards. The profile doctorate category can include professional degrees, while IPEDS counts research doctorates. Overlap adds the smaller source share in each category, reaching 100% for identical distributions. Sources: Revelio and IPEDS (Revelio Labs, 2026*b*; National Center for Education Statistics, 2026*b*).

B.2 Individual Linkage

B.2.1 Linkage Procedure

To follow a graduate's work after student-visa records end, I need to find that person's education and employment history in the profile data. SEVIS withholds names, but the combination of schools attended, graduation dates, fields, and employers can distinguish students from others at the same school. I first make these details comparable across the two sources, then use their agreement to select a profile when the evidence is sufficiently strong.

Schools must refer to the same institution even when names or campus identifiers differ. I standardize capitalization, punctuation, and common abbreviations, and compare names with the IPEDS catalog using alternative names and locations. Campus relationships help distinguish branches of one institution from similarly named schools, while year-specific

assignments and changes in institutional identifiers allow me to follow the same institution through a student's education history. Schools outside the IPEDS reporting universe can remain unmatched even when their names are correctly reported.

Fields require a similar translation, since profiles usually describe majors in words while SEVIS and IPEDS use CIP codes. I assign profile majors to four-digit CIP groups using the major and, when useful, the degree description: "Master's in Applied Computer Science" identifies both degree and field, whereas "Master of Science" does not identify a field. An unresolved or missing field is kept distinct from a field that disagrees with the administrative record.[81]

For each administrative student, I search among profiles reporting education at the same institution, allowing for differences in dates and incomplete degree information. Agreement in graduation timing, degree, and field narrows the candidates, and other schools in the student's history provide additional evidence. OPT employers help distinguish the remaining candidates: agreement on an uncommon employer is more informative than agreement on a company appearing in many profiles, and employment dates distinguish a plausible shared job from work at the same company in unrelated periods. I also compare administrative citizenship with evidence on country of origin from profile education, employment, locations, and names.[82]

I combine these comparisons into a score using a logit fitted to likely matches and competing candidates. A training match requires exactly one candidate to share an uncommon employer; other candidates for the same administrative identity supply the training nonmatches. I omit the employer that identifies a training match when fitting the employer coefficients, so that the fit does not simply reproduce the rule used to identify matches. The score includes education, employer and origin evidence, together with a composite of these comparisons that accounts for missing information. Training and test observations are separated by administrative identity.[83]

[81]The four-digit groups make fields comparable for linkage. To define university programs, I retain the more detailed six-digit administrative codes and follow the original and new codes together when a school changes a program's classification.

[82]Repeated entries for the same employer count once. Country evidence from a profile helps identify a link but does not itself measure citizenship.

[83]The employer identifying a training match appears in at most 25 profiles. Previously reviewed identities are excluded from this coefficient fit.

For each administrative identity, I convert the scores into matching weights between zero and one, reserving weight for the possibility that no candidate is correct. If s_{ij} is the score of candidate j for identity i, the weight is

$$w_{ij} = (1 - p_i^0)\frac{\exp(s_{ij}/\tau)}{\sum_{k \in C_i} \exp(s_{ik}/\tau)},$$

where C_i is the candidate set, p_i^0 is the weight on no match, and τ determines how strongly higher scores are favored. I fit τ using reviewed candidate lists. The no-match model uses the best score, the number of candidates and how closely they compete, comparing reviewed lists with lists from which an employer-based match has been removed. These weights summarize relative matching evidence; they are not calibrated probabilities that a link is correct.

I assign an administrative identity its leading profile if the matching weight is at least 0.45, a cutoff selected using the development review described below. When administrative identities compete for the same profile, I retain the strongest claim. I then map these assignments to the corrected administrative people used in the analysis. Because that mapping can associate a selected profile with more than one corrected record, I retain at most one record for each profile within a program and graduating cohort, giving priority to the larger matching weight.[84] The cohort and timing of outcomes follow the administrative program-end date; a profile's graduation date helps identify the person but does not replace that date.

B.2.2 Linkage Validation

I assess both whether the linked histories are consistent with belonging to the same person and how well linked graduates represent the broader population. I used a stratified review of 400 student cases to assess whether the candidate histories supported the proposed links and to choose the selection threshold. Large language model reviewers could select a candidate, reject the displayed candidates, or mark the case uncertain. This review checks whether a link is consistent with the available evidence, without an independent record of each student's identity. I assess the analysis sample separately using administrative outcomes, an external population benchmark, and sensitivity to the linkage weights.

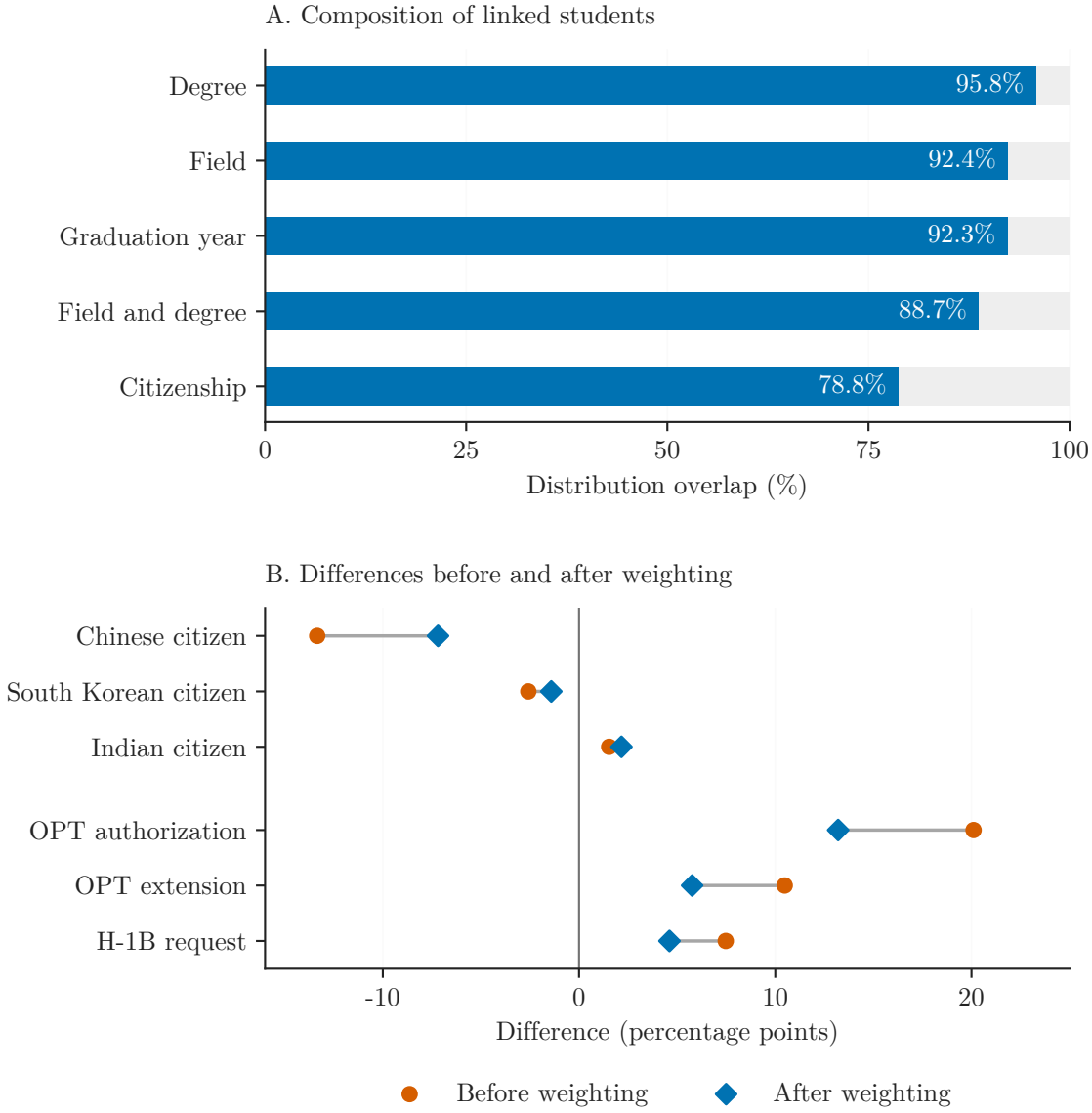
Composition of the linked sample. Before weighting, the linked sample closely resembles the administrative records in its degree and field composition: overlap is 95.8% across degree levels and 92.4% across fields (Figure B.2, Panel A). This comparison follows 51,809 program-completion records in the 335 programs reclassified in 2014–2019, with 6,830 links and three

[84] Remaining ties within a program and cohort are resolved by program-start year and person identifier.

years covered by the administrative records.[85] Within graduating classes with at least one link, I compare the linked and administrative shares while holding the mix of programs and classes fixed. Giving greater weight to linked graduates who are less likely to have a profile match reduces the excess share with recorded OPT authorization from 20.1 to 13.2 percentage points (Panel B), with smaller gaps also for extension use and H-1B requests.

[85]This descriptive comparison precedes the additional sample restrictions used in each regression. Graduating classes with no link contain 7.0% of eligible records; they remain in Panel A's administrative population but cannot enter the weighting comparison.

Figure B.2: How linked graduates compare with student-visa records



Notes: The sample contains 51,809 student-visa completion records in 335 programs whose graduates gained eligibility for the Optional Practical Training (OPT) extension in 2014–2019; 6,830 records are linked to profiles. All have three years covered by administrative records. Panel A compares unweighted shares; overlap sums the smaller share in each category, reaching 100% for identical distributions. Panel B shows linked-minus-administrative differences among graduating classes with a link, holding the program and class mix fixed. Circles weight linked students equally within each class; diamonds give more weight to those less likely to link. OPT and its extension indicate authorization; H-1B records are requests for employer-sponsored status. Classes without a link contain 7.0% of eligible records and enter only Panel A's administrative population.

Administrative outcomes. SEVIS records OPT use, later enrollment, and status-change requests for both linked and unlinked graduates, so these outcomes show how the estimates

change when I restrict to students with profiles. Table B.2 compares three versions: all administrative students, linked students, and linked students reweighted to account for observable differences using Appendix B.2.3. The programs, graduating cohorts, and weights used to compare programs that gain eligibility for the OPT extension with their comparisons are the same across columns. The sample contains 415,416 F-1 program records, including 53,471 linked records, in 248 treated programs and their comparisons.[86]

The linked sample captures the increase in OPT use and H-1B requests and the decline in later F-1 enrollment. Weighting brings the STEM OPT response closer to the estimate from all administrative records, reducing it from 45.6 to 38.1 percentage points, compared with 26.2 points in the full records; the remaining difference is 11.8 points [SE 2.9]. H-1B request estimates are closer across samples, although weighting moves the any-OPT estimate farther from its full-record counterpart. These comparisons motivate an additional adjustment that makes OPT participation in the linked sample match the administrative records, as described below. The main employment increase remains substantial with that adjustment.

Comparison with the NSCG. An independent survey provides a second way to assess how the linked graduates represent subsequent U.S. presence. I compare graduates from 2012–2018 with the 2021 National Survey of College Graduates (NSCG) (National Center for Science and Engineering Statistics, 2023). In the profiles, a graduate is present if a U.S. job or education period is recorded on February 1, 2021, the start of the survey reference week. For the survey benchmark, I count U.S. degrees earned by residents who first entered on a study or training visa, using the survey weights, and divide by IPEDS nonresident degree awards. This is a comparison with a related population, rather than a survey following the same F-1 graduates over time.

Table B.3 shows that linkage weighting brings bachelor's and master's presence closer to the survey benchmark. The weighted rates are 45.2% and 62.8%, compared with benchmarks of 36.5% and 56.0%; doctoral presence moves farther from its benchmark. The profile comparison includes all mapped F-1 programs, including those that never change classification, and holds the mix of programs and graduating cohorts fixed before and after weighting. I then weight degree, field, and cohort groups to their IPEDS distribution. A program's graduating class remains unrepresented when none of its graduates can be linked. The comparison supports the adjustment for bachelor's and master's graduates while showing that some differences in coverage remain.

[86] I require three years covered by the administrative records and a common comparison sample for all outcomes. These sample requirements differ from the main administrative analysis, so its estimates need not equal those in this table.

Table B.2: Do linked records recover the administrative responses?

	Full records	Linked, raw	Linked, weighted	Weighted − full
Uses any OPT	9.0	8.4	6.0	-3.0
	[1.8]	[3.6]	[3.9]	[3.2]
Uses the OPT extension	26.2	45.6	38.1	11.8
	[1.3]	[2.1]	[2.4]	[2.9]
H-1B request	4.5	6.1	5.4	0.9
	[0.9]	[2.2]	[2.3]	[2.1]
Later F-1 enrollment	-5.8	-12.7	-9.9	-4.1
	[0.9]	[2.5]	[2.6]	[2.9]
Uses any OPT mean	40.7	56.3	50.3	—
Uses the OPT extension mean	2.3	3.7	3.0	—
H-1B request mean	10.0	14.9	13.5	—
Later F-1 enrollment mean	10.2	21.9	17.8	—
Program records	415,416	53,471	53,471	415,416
Stacked program–cohort rows	57,672	57,672	57,672	57,672
Schools	1,220	1,220	1,220	1,220
Treated programs	248	248	248	248
Program fixed effects	Yes	Yes	Yes	Yes
Field × degree × year effects	Yes	Yes	Yes	Yes
Inverse-linkage weights	No	No	Yes	Yes

Notes: Percentage-point effects of eligibility for the Optional Practical Training (OPT) extension, comparing the first four relabeled cohorts with not-yet-relabeled programs. Columns hold programs and cohorts fixed; linked samples are unweighted or weighted by inverse linkage probabilities. Included groups cover 71.4% of eligible records and have three years after completion observed by October 2023. OPT is any recorded use by that date; H-1B requests are undated and do not establish approval; later enrollment covers three years. Sample means pool treated and comparison cohorts using administrative counts as weights. Both sets of fixed effects are specific to each treatment cohort. Bracketed standard errors cluster by school with weights fixed in columns 1–3; column 4 uses 199 school-bootstrap draws that refit comparison and linkage weights. Its record count refers to the paired full sample. Appendix B.2.2 describes this diagnostic sample. Sources: SEVIS, IPEDS and Revelio.

Recording during authorized work. Even a correctly linked profile may omit jobs, particularly work during further study. I assess this possibility by checking whether a U.S. job is active in the profile in months when SEVIS records an OPT employer or authorizes Curricular Practical Training (CPT), the work permission tied to an enrolled student's curriculum. The comparison follows 70,798 linked program records with three years covered by the administrative records and uses the same monthly dates in both sources. CPT months exclude OPT-authorized months, and the part-time CPT group excludes months with full-time CPT. In this sample, profiles record work more often during CPT during a later F-1 program than during OPT employer spells, both before and after linkage weighting (Table B.4). These rates describe the graduates observed in each route, who need not be comparable; employer information also helped identify their links.

I also examine whether recording changes after schools reclassify degrees to make their graduates eligible for the OPT extension. Among programs and graduating classes that can be compared within each route, weighted recording during OPT employer spells rises by 7.0 percentage points [SE 3.2], while recording during full-time CPT in a later program falls by 28.9 points [SE 6.6]. The latter comparison includes 47 treated programs.[87] Because eligibility can change who works through each route and which periods of work are observed, these differences cannot isolate a change in reporting.

[87] I use the fixed effects and treatment weights from the administrative validation, keeping observations with positive months in the relevant route and an observed reference cohort, and requiring both treated and comparison programs. Standard errors cluster by institution and hold the estimated treatment and linkage weights fixed. Later-program part-time CPT lacks the required treated and comparison observations in some post-treatment years.

Table B.3: U.S. presence in linked profiles and a survey benchmark

	Bachelor's	Master's	Doctoral
NSCG/IPEDS presence benchmark	36.5	56.0	88.7
Profiles, unweighted	48.2	65.3	74.3
Profiles, weighted	45.2	62.8	72.9
Administrative records represented (%)	66.9	82.0	74.7
Linked program records	62,476	65,551	28,204
Administrative program records	598,796	534,535	193,336
NSCG degree observations	582	2,580	975

Notes: Percentages for the 2012–2018 graduation cohorts. A profile records U.S. presence when a U.S. job or education period is active on February 1, 2021. The National Survey of College Graduates (NSCG) benchmark counts U.S. degrees earned by U.S. residents who first entered on a study or training visa, using survey weights, and divides by nonresident degree awards in institutional records (IPEDS). Both profile rows hold the mix of programs and graduating cohorts fixed and weight degree, field, and cohort groups to their IPEDS shares. The weighted row also weights linked graduates by the inverse of their estimated linkage probability within each program and cohort. Groups must contain a linked graduate; the bottom panel gives their share of administrative records. The sample includes all programs matched across sources, including those never reclassified for extended work authorization. This is a descriptive comparison of related populations; survey-design intervals have not been calculated. Sources: SEVIS, Revelio, IPEDS, and the 2021 NSCG (National Center for Science and Engineering Statistics, 2023).

Table B.4: Work recorded during OPT and later-study authorization

	OPT employer spell	Full-time CPT	Part-time CPT
Work recorded, unweighted (%)	74.3	88.0	83.1
Work recorded, weighted (%)	70.4	87.0	80.9
Authorized months	514,703	28,588	15,541
Linked program records in route	42,562	3,830	2,611
All linked records in sample		70,798	

Notes: The first two rows show how often a profile records a U.S. job in months covered by an OPT employer spell or CPT authorization during a later F-1 program. OPT is Optional Practical Training; CPT is Curricular Practical Training, work tied to a student's curriculum. The sample contains 70,798 linked program records with three years after completion covered through October 2023. OPT combines employer spells and authorization; CPT authorization counts as full-time unless marked part-time. Each month receives equal weight in the unweighted row; the weighted row weights months by the inverse of the graduate's estimated linkage probability. Recorded jobs count fully here, including during study. Each route contains different people and periods. Appendix B.2.2 gives the construction. Sources: SEVIS and Revelio.

B.2.3 Adjusting for Selective Linkage

Graduates with more distinctive administrative histories are easier to link to profiles. OPT employers provide particularly useful information, so simply averaging the linked graduates' outcomes would give OPT participants more influence than they have in the full cohort. I instead estimate each graduate's probability of being linked and give greater weight to linked graduates with a lower predicted probability. This adjustment lets a graduate with a less commonly linked history represent more of the students missing from the profiles.

Let R_i indicate that graduate i has a retained link, X_i denote the administrative characteristics used to predict linkage, and $\widehat{q}_i$ be the fitted probability from a logit for $\Pr(R_i = 1 \mid X_i)$. For outcome Y_i, the weighted mean in program p and graduation cohort t is

$$\overline{Y}_{pt} = \frac{\sum_{i \in (p,t)} R_i Y_i / \widehat{q}_i}{\sum_{i \in (p,t)} R_i / \widehat{q}_i}. \tag{7}$$

Thus each observed outcome receives weight $1/\widehat{q}_i$, and dividing by the sum of weights gives the cohort's average. Three weights are distinct: the matching weight used to select a candidate profile, the predicted probability of linkage defined here, and the program-level weight used to construct the treatment comparison.

The prediction uses citizenship, degree level, graduation cohort, broad field, the share of other graduates from the same country in the program–cohort, and counts of distinct employers, employment periods, and education periods in the administrative records. It does not directly include OPT use, STEM-extension use, or H-1B requests.[88]

For the weighted average to recover the full cohort's mean, linked and unlinked graduates with the same predictors must have the same average outcomes, and comparable graduates must have a positive probability of being linked. Some history counts can themselves respond to the OPT extension; here they help predict whether a work history is observed, rather than enter as controls in the employment regression. Weighting addresses differences in who is linked, but cannot correct a mistaken identity or a job missing from a correctly linked profile.

As a further check, I adjust the weights so that the linked sample reproduces administrative participation in three groups: no OPT, initial OPT only, and the STEM extension. I make this adjustment separately for programs gaining eligibility in each year and for each graduating cohort before or after the change, keeping treated and comparison programs separate. The adjustment is possible only when a participation category with administrative

[88]The logit includes the country share and its square, with indicators for missing shares and single-student groups. Employer and employment counts enter in categories of 0, 1, 2, 3, 4, 5–9, or 10 or more; education counts enter as 0–1, 2, 3, or 4 or more. I pool country and broad-field categories with fewer than 200 records and refit the model in the school-level bootstrap.

students also contains linked graduates; excluding groups that do not meet this condition leaves the 2016–2019 treatment cohorts. This check changes both the weights and the represented sample, and the employment results in Section 4 continue to show a substantial increase under the additional adjustment.

B.3 Employer Linkage

To compare firms' OPT recruitment with their broader employment and immigration sponsorship, I connect employers in the student-visa records to companies in the profile data and the employer-sponsored immigration records. The latter include H-1B petitioners and employers filing the labor applications required for H-1B visas (LCAs) and permanent residence (PERM). Names provide a starting point for these links, while alternative names, addresses, and other identifiers help distinguish employers with similar names. I standardize punctuation, spacing, legal suffixes, and common abbreviations, accept straightforward exact agreements, and use approximate name comparisons and review for spelling differences and alternative names.

The goal is to follow the same operating company across sources. I combine Revelio entries that describe the same employer, but do not combine companies solely because they share a corporate parent; an operating company also need not correspond to a single legal filing entity. Linking the administrative records to these company identifiers allows me to compare OPT recruitment with broader hiring, employment, and immigration sponsorship, without requiring successful links between individual SEVIS students and profiles. When an administrative source covers the relevant year and the reviewed link indicates that a company has no corresponding record, I assign zero activity. If the employer's identity remains unresolved, I leave the outcome missing. The set of firms therefore varies with the coverage of each administrative outcome.

C Immigration Status and Work Authorization

Section 4 asks how much Optional Practical Training (OPT) adds to graduates' U.S. employment and how much it replaces work through other immigration routes. To distinguish those responses, I combine the jobs recorded in profiles with monthly histories of study and work authorization from the student-visa records. The administrative data directly identify study on an F-1 student visa and the associated work authorization, but provide less complete information about later statuses such as the employer-sponsored H-1B visa. This appendix explains how I assign status when it is not directly observed and how I use the resulting histories to divide recorded work across routes.

C.1 Monthly Immigration Histories

For each graduate in the SEVIS data, I can directly identify the dates of their usage of OPT (both standard OPT and the STEM extension) and CPT, as well as any transitions to other programs. This lets me distinguish work authorization associated with the original degree from authorization obtained through further study. The main difficulty is assigning a status after the last recorded F-1 activity. SEVIS reports requests to change immigration status, but does not date the requests or establish approval. For someone with an H-1B request, I infer an H-1B period beginning when the last recorded F-1 program or work authorization ends and continuing for 72 months. Using the last F-1 activity across all of the person's programs prevents the request from placing a graduate on H-1B while a later F-1 program is still active.[89] When no study, authorization, or requested status applies, I categorize the work as having "no recorded status."

C.2 Recorded Work by Immigration Route

I then assign each month of U.S. work recorded in Revelio data to the graduate's immigration category in that month.[90] The main results divide work into four groups: OPT and the extension following the original degree; inferred H-1B; later F-1 study and its associated authorization; and other or no recorded status. When periods overlap, OPT from the original degree takes priority, followed by later F-1 activity and then inferred H-1B. These groups sum to total recorded work, allowing the increase during OPT to be compared with changes

[89] While H-1B requests comprise over 95% of requests, I include other requested statuses, which begin at the same assigned date and receive 72 or 24 months according to the type of request.

[90] Monthly observations begin in the SEVIS program-end month. A job dated January 1 of the graduation year and continuing past graduation is moved to the day after graduation to account for dates reported only by year.

in work through other routes.[91] Education reported only in a profile does not establish F-1 status and therefore falls in the last group.

Because profiles do not report actual hours, I allow for limits on work during study by counting some work months as one-half. This adjustment applies during the original program and during later F-1 study and its associated authorization, unless a full-time authorization is active; other work months count fully.[92] The adjustment follows administrative program and authorization dates, so inferred H-1B and other requested statuses affect the division of work across routes without changing the total. Profiles may continue to record jobs after SEVIS coverage ends (in October 2023), but comparisons requiring immigration status use only the period covered by both sources.

D Program Relabelings and Empirical Methods

D.1 Identifying Program Relabelings

In Section 4, I use changes in how schools classify existing degree programs to study the effect of longer OPT eligibility. I refer to these changes as program relabelings. To identify them, I first compile documented examples and then look for similar patterns in the annual degree counts that schools report to IPEDS.

The documented examples establish which old and new field classifications to look for. I compile them using a large language model with web search and match them to IPEDS. Common examples include Economics to Econometrics and Quantitative Economics, General Psychology to Experimental or other quantitative psychology codes, Business to Management Science, and Architecture to Architectural and Building Sciences. These field classifications are recorded as CIP codes.

In IPEDS, a relabeling should produce a sustained fall in degrees reported under the old code alongside a rise under the new code (Figure 4). A fall alone could reflect a closure, and a rise alone could reflect a new program. I combine old codes when a documented program previously reported several tracks and review related changes at the same school and degree level together to avoid counting one event more than once.

When several new codes are possible, dates conflict, or it is unclear whether records

[91] Table A.5 also distinguishes potential authorization from recorded authorization and work. Potential authorization assigns twelve months to OPT users and another seventeen or twenty-four months to extension users, beginning at program completion; extensions beginning on or after May 10, 2016 receive twenty-four months. Recorded authorization instead follows the dates in SEVIS.

[92] CPT, pre-completion OPT, and OPT authorizations count as full-time unless marked part-time in the data. For later F-1 activity, the half-time adjustment ends on October 15, 2023, the last date covered by SEVIS. Later F-1 work months beyond that date count fully, including in the five-year employment results.

describe the same program, I consult archived catalogs, department announcements and other program-specific documents. This review can confirm a change, revise the old codes or date, or leave the case unresolved. I exclude unresolved cases from both treatment and comparison groups, as well as master's changes that would duplicate the corresponding doctoral program's change. For a first relabeled IPEDS year g, the four complete pre-treatment years used to screen coverage and construct baseline characteristics are $g-5, \ldots, g-2$. The screen requires complete IPEDS coverage in those years, at least five total completions across them, positive F-1 completions in $g-2$, and an observed Carnegie classification. The main administrative take-up fit contains 335 treated programs from the 2014–2019 relabeling cohorts, 207,974 stacked program-cohort observations and 27,671 program-by-stack units; the last two are not counts of distinct programs, because comparison programs can appear in several stacks. Appendix Table A.1 counts the treated programs by first relabeled cohort, degree level and field, with examples.

All relabeling event studies date potential exposure from $g-1$, so that the reference cohort $e=-1$ is $g-2$ and event time zero is the cohort immediately before the first relabeled IPEDS cohort. This convention keeps the partly exposed cohort in the event study without using it as the baseline.

Comparison programs are drawn from the potentially eligible source-code universe. For a comparison program first relabeled in year g_c, a student outcome measured for graduation cohort t at horizon h is included only if

$$g_c > \max\{g-2, t\} + h + b, \tag{8}$$

where the buffer $b=1$ in the main specification; never-relabeled programs have no finite g_c, and administrative ever-use outcomes use $h=0$. Appendix Table A.4 reports $b=2$ and $b=3$, and a version that applies the rule with $h=5$ in every year so that the comparison programs are the same in every year.

D.2 Weights, Estimands and Inference in the Relabeling Design

To make comparison programs resemble treated programs before relabeling, I estimate the probability of treatment separately within each stack. Section 4.2 explains how these probabilities enter the comparison and how I combine the estimates. Its eight continuous pre-treatment predictors are mean log program size, mean international share, the pooled shares of F-1 graduates from India and China, pooled OPT take-up, the trend in international share, the trend in log program size, and mean log real tuition. Means and trends use the four pre-treatment years above, with at least three observed years required where

applicable; logarithms are taken of positive values only, and missing or undefined predictors receive missing-value indicators. Field-by-degree and Carnegie categories enter alongside these covariates, and categories without treatment-comparison overlap are removed. The propensity model is an unpenalized logit, and the odds are not trimmed; treated programs receive a weight of one and comparison programs $\widehat{p}/(1-\widehat{p})$. Table 1 shows how reweighting brings comparison programs closer to treated programs in prior OPT use, tuition and degree composition, while narrowing the gap in international share.

Student outcomes additionally weight programs by their reference-cohort F-1 count, in both the treatment model and the outcome regression. The estimand therefore weights programs in proportion to their pre-treatment F-1 cohorts but averages the outcomes of the graduates who actually complete the program in each year, so fixed weights do not remove changes in student composition. Program-level outcomes, including completions and their international share, use equal program weights and a separately fitted treatment propensity.

Linked program-cohort means use the inverse-linkage adjustment described in Appendix B.2.3. Confidence intervals use school-level exponential multiplier draws that refit the treatment and linkage models, and the calibration when it is used, in each of 499 draws. The doubly robust estimator also models changes in comparison programs' outcomes. Its analytic standard errors account for the estimated weights and outcome model and allow outcomes to be correlated within schools. I compare these with the school-level bootstrap; of 499 attempted draws, 483 yield finite three-year estimates and 479 yield finite five-year estimates.

The employment response need not operate only through use of the extension: initial OPT use also changes, and successive cohorts may differ in composition. The main text therefore compares the increase in total U.S. work with the increase in work on OPT, using the same graduates and follow-up period for both. Their ratio describes how much additional employment accompanies additional OPT work; it is not a local average treatment effect of extension use on a fixed group of students.

D.3 The Employer Design

Section 5 asks how employers respond when their usual recruiting schools supply more graduates from programs eligible for the extension of Optional Practical Training (OPT). These programs allow international graduates a longer period of work authorization, but also supply domestic graduates. I measure this change using degree completions at each school and firms' earlier recruiting patterns, then compare firms with large increases in access to these graduates with firms experiencing smaller increases. This appendix explains the

construction of that comparison and the additional measures of employment and worker departures.

Measuring growth in eligible programs. The share of a school's degrees awarded in programs eligible for the extension can rise because eligible programs grow or because other programs shrink. I want the first component: growth in the eligible hiring pool, which includes domestic as well as international graduates. I classify degree completions using the field codes that schools report to the Integrated Postsecondary Education Data System (IPEDS), called CIP codes, and the Department of Homeland Security's eligibility list for each year. Growth can therefore reflect additional graduates in eligible fields, a change in a program's reported field, or the addition of fields to the eligibility list.

To separate growth in eligible completions from contraction elsewhere, let A_{kt} and B_{kt} denote eligible and other completions at school k in year t. Total completions are $T_{kt} = A_{kt} + B_{kt}$ and the eligible share is $S_{kt} = A_{kt}/T_{kt}$. The change in this share decomposes exactly as

$$\Delta S_{kt} = \frac{(B_{kt} + B_{k,t-1})\Delta A_{kt}}{2T_{kt}T_{k,t-1}} - \frac{(A_{kt} + A_{k,t-1})\Delta B_{kt}}{2T_{kt}T_{k,t-1}}. \tag{9}$$

The first term is the contribution of changes in eligible completions; the second is the contribution of changes in other completions. I add up positive values of the first term from 2009 onward to obtain the school's expansion index:

$$H_{kt} = \sum_{u=2009}^{t} \max\left\{\frac{(B_{ku} + B_{k,u-1})\Delta A_{ku}}{2T_{ku}T_{k,u-1}}, 0\right\}, \qquad H_{k,2008} = 0. \tag{10}$$

The index rises when eligible completions expand, but not when other completions fall.[93]

Sample support. Section 5.1 explains the fixed recruiting shares and treatment thresholds used to connect these school changes to firms. I restrict to firms with at least three hires attributed to U.S. schools ($\sum_k N_{fk} \geq 3$). The denominator of the recruiting shares includes all of these hires, including hires from schools outside the IPEDS exposure series, so a firm's weights on the schools in the series can sum to less than one. Exposure must be observed from $g-2$ through $g+3$ and positive from 2009 through $g-2$, where g is the year defining the treatment cohort, and the firm must have positive employment from $g-5$ through $g-2$. I also require complete outcome histories, an identified firm, and administrative sources that cover the relevant years. Earlier exposure increases are permitted and enter the comparison

[93]To limit changes caused by reporting coverage, I retain school–degree combinations with positive total completions in every year from 2008 through 2023. Individual fields can still open or close within this sample.

weights. Appendix Table A.8 reports the numbers of episodes and effective comparison sample sizes; Table 2 compares firms before the expansion. All thirteen annual outcomes use 40,203 treated episodes at 36,210 distinct firms except PERM, the application process for employer-sponsored permanent residence, whose coverage supports 39,991 episodes at 36,014 firms. Observed zeros enter the outcomes as zero, and unobserved firm-years are missing.

Making firms comparable before the increase. The comparison gives more weight to firms whose earlier outcomes and characteristics resemble those of treated firms. I estimate each firm's probability of a treated episode using its earlier outcomes, size and growth, foreign-worker composition, OPT hiring, H-1B demand, exposure history, recruiting composition, industry and geography. Recruiting composition includes the share of earlier recruiting accounted for by schools with usable degree reports and the concentration of recruiting across schools. I use inverse probability tilting to estimate this probability; the resulting weights balance the means of these characteristics between treated and comparison firms. Formally, comparison firms receive normalized propensity-score odds,

$$\widehat{\omega}_{fg} = \frac{\widehat{p}_{fg}/(1-\widehat{p}_{fg})}{\sum_{j:D_{jg}=0} \widehat{p}_{jg}/(1-\widehat{p}_{jg})}, \tag{11}$$

where D_{fg} indicates a treated episode and $\widehat{p}_{fg}$ is the estimated probability.[94]

The timing follows Section 5.2: firms may begin responding in the year before the measured expansion, so I use two years before the expansion, $g-2$, as the reference. With event time $e = t-(g-1)$, the reference is $e=-1$, the partly exposed year is $e=0$, and the measured expansion is $e=1$. I compare each outcome with its value in the reference year, $\Delta Y_{fge} = Y_{f,g+e-1} - Y_{f,g-2}$, using equation 4, and average responses in $e=1,2,3$ using equation 5. Treatment cohorts receive weights proportional to their numbers of treated episodes, giving equal weight to firm-episodes rather than workers. I report hiring effects in levels and as percentages of the estimated mean without the larger exposure increase; employment and H-1B sponsorship effects are reported in levels.[95]

[94] Firm growth is the change in the logarithm of mean headcount between $g-5$ to $g-4$ and $g-3$ to $g-2$; requiring positive employment in all four years makes this measure well defined. For other nonnegative characteristics, the adjustment separates positive values from zero and includes indicators for positive and missing values where applicable; signed changes and annual outcomes enter in levels. I cap administrative demand, approvals and PERM counts at the annual 99th percentile among positive observations.

[95] Hiring and sponsorship inference uses 499 corporate-cluster multiplier bootstrap draws: firms in the same corporate group receive the same random multiplier across repeated episodes. Each draw re-estimates comparison weights, treatment-cohort shares and outcome contrasts, holding the constructed school links, graduation attributions, recruiting shares and exposure measures fixed. Percentage hiring effects account for covariance between the level effect and its estimated counterfactual mean.

Adding a model of outcome changes. As a check, I also predict comparison firms' outcome changes using a weighted linear regression on the same characteristics used to estimate the weights. Let $\widehat{m}_{0ge}(X)$ denote this prediction for characteristics X, and N_{1g} the number of treated episodes in treatment cohort g. Subtracting predicted changes before comparing the two groups gives the doubly robust estimator

$$\widehat{\tau}^{DR}_{g,e} = \frac{1}{N_{1g}} \sum_{f:D_{fg}=1} \left[\Delta Y_{fge} - \widehat{m}_{0ge}(X_{fg})\right] - \sum_{f:D_{fg}=0} \widehat{\omega}_{fg} \left[\Delta Y_{fge} - \widehat{m}_{0ge}(X_{fg})\right]. \tag{12}$$

Under conditional parallel trends and overlap, this estimator is consistent if either the probability model or the outcome model is correctly specified (Sant'Anna and Zhao, 2020). The former assumptions still require comparable firms and similar outcome changes, conditional on their earlier characteristics, absent the larger exposure increase. Here, the weights already balance the regressors in the linear outcome model, so the two estimators coincide to three decimal places for the main pooled effects. Their agreement checks the implementation rather than the identifying assumptions.

Counting employed workers. Hiring more workers need not increase employment if they replace workers who leave. I therefore count the distinct workers with U.S. jobs at each firm on the last day of each month and average the twelve counts within a calendar year. I also count employment on December 31. Both measures use the merged employment histories and firm episodes in the hiring analysis, rather than counting everyone who appears at a firm at any point during the year.[96]

For each employment measure, I adjust the comparison using its own earlier mean and trend over $g-5$ through $g-2$ in place of the outcome histories used for hiring. Other firm and exposure characteristics remain unchanged.[97] Native employment includes all workers classified as U.S.-origin from their profiles, including recent graduates. Foreign employment means other inferred origin, not observed citizenship or birthplace. The "other foreign" measure excludes workers classified at their first entry to the firm as recent U.S. graduates or qualifying U.S. student interns, using the same categories as the hiring analysis. I keep this classification fixed across later jobs at the firm, so a graduate does not enter the other-foreign group simply by staying longer. Total employment includes these graduates and interns.[98]

For the college-educated employment outcomes in Section 5.4, I count workers with a

[96]The employment histories record each job's first and last dates through July 6, 2026; a spell ending on an observation date counts as employment that day. Both measures retain observed zeros.

[97]The earlier employment mean enters as its ordinary logarithm when positive, with an indicator for a positive mean so that zero means remain in the sample; the trend enters in levels.

[98]The internship classification depends partly on the observed duration of the worker's first spell at the firm.

recorded completed bachelor's, master's, MBA or doctoral degree from any country at each month-end. A worker enters these counts when the degree is completed; a later degree does not change earlier employment counts.[99] The main outcome combines native degree holders with other foreign degree holders, excluding the foreign recent-graduate and student-intern group defined above. This entry classification remains fixed, while degree completion is assessed each month. For people hired after exposure, the entry classification need not have been determined before exposure; these are employment groups, not workers known to be unaffected by OPT. Appendix Figure A.8 reports the combined outcome, its native and other-foreign components, and total college-educated employment. All four retain 40,203 treated episodes, with weights estimated separately using each outcome's earlier employment mean and trend.

Following workers already at the firm. To examine whether an expanding graduate pool affects existing employees, I follow the workers employed at the firm at the start of the partly exposed year. I fix this roster on January 1 of $g-1$ and measure the share employed at the same firm at the ends of that year and the following three years. Workers who leave remain in the denominator and count as employed if they return and are working there on the observation date. The roster pools native and foreign workers, including workers originally hired as foreign graduates.[100]

Employment at the original firm tends to decline as a group of workers is followed for longer, even without a change in the graduate pool. I account for this by constructing an earlier roster, fixed on January 1 of $g-5$, and following it for the same one to four years. Its follow-up ends before the partly exposed year, although these workers can have experienced earlier large increases in exposure. Let R_{fgh} be the percentage of the later roster employed at the firm after h years and R^{pre}_{fgh} the corresponding percentage of the earlier roster. I apply equation 4 to the difference $R_{fgh} - R^{\text{pre}}_{fgh}$, then average $h = 2, 3, 4$ to match the December 31 endpoints in $g, g+1, g+2$. This compares the change across successive rosters at treated firms with the change at weighted comparison firms, always holding the length of follow-up fixed.

The identifying assumption is that, without the larger exposure increase, employment would have changed similarly across successive rosters in the two groups. The follow-up

[99] I use precise graduation dates when available and January 1 when only the year is recorded. Missing or undated education remains unknown. I retain observed zero counts and use the same employment histories as the unrestricted measures, without an additional exclusion of internship spells.

[100] Jobs must have begun before January 1 of $g-1$ and cover that date. I exclude identified internship spells when forming the roster and at follow-up, but retain former interns working in later regular spells. An internship is a completed company spell lasting less than six calendar months that overlaps a recorded education interval; this definition uses realized duration and need not identify every internship.

profiles cannot provide a conventional pre-trend test of this assumption: every worker is employed when each roster is formed. For the main specification, I re-estimate comparison weights using earlier headcount histories together with the firm, exposure and recruiting characteristics described above. The earlier roster supplies the outcome comparison, but its employment and departure rates do not additionally enter the model for these weights.[101]

I also measure the share of each roster whose original continuous job at the firm has ended before the observation date. Together, these measures distinguish departures from employment after a return: employment at the firm equals uninterrupted employment plus employment after a return. The employment effect plus the first-departure effect therefore equals the effect on employment after a return. I use the same observations and weights for all three measures and retain their covariance in inference. These outcomes do not distinguish dismissals from voluntary moves or show whether departing workers work elsewhere.

Results for existing workers and sensitivity to adjustment. Employment at the original firm falls by 0.27 percentage points [SE 0.18], relative to a counterfactual employment rate of 62.8%. First departures rise by 0.25 points [SE 0.17], with little change in employment after a return. Excluding workers who first joined as foreign recent U.S. graduates or qualifying foreign student interns leaves similar estimates: −0.25 points [SE 0.18] for employment and 0.24 [SE 0.17] for first departure.[102]

The adjustment for earlier employment histories matters more. Adding the earlier roster's four employment rates and four first-departure rates, in levels, changes the all-worker employment estimate to −1.26 points [SE 0.14] and the first-departure estimate to 1.33 [SE 0.14]. Within each population, the workers, firms, outcomes and follow-up periods stay the same; the additional histories change the comparison firms' weights. Balancing these histories makes the earlier rosters more similar by construction, so it cannot independently test the assumption of similar changes across successive rosters. The estimated decline in employment of existing workers therefore depends on how I adjust for earlier histories.

Hiring and departures across the whole firm. The preceding comparison follows a fixed group of existing workers. The turnover measures in Figure A.9 instead follow the firm's changing workforce, including workers hired after the exposure increase. I count the starts of

[101] Headcount histories count workers appearing at the firm during each year, rather than average monthly employment. Both starting rosters must contain workers, with no restriction on subsequent employment. The sample contains 38,827 treated episodes at 35,097 firms and gives equal weight to treated firm-episodes rather than workers.

[102] This classification stays fixed across subsequent jobs at the firm, and other foreign workers remain included. The restricted sample contains 38,785 treated episodes at 35,060 firms, with weights re-estimated for this population.

all non-intern jobs at the company, including rehires, and count a departure on the first day after each job ends. To compare firms of different sizes, I express these annual flows per 100 employees, using either current-year average daily employment or mean daily employment in the four earlier years, $g-5$ through $g-2$.

The earlier employment denominator stays fixed and therefore retains later years in which the firm has no employees. The current-year denominator requires positive average employment in all eight event years, conditioning the sample on activity after exposure. The all-worker sample has 40,203 treated episodes with the fixed denominator and 39,504 with current employment; excluding the foreign graduate and student-intern group leaves 40,199 and 39,467, respectively.[103] The comparison weights retain the headcount characteristics and add earlier means and trends in hiring and departure rates. Hiring and departures use the same weights within each population and denominator, with annual event time and averages over $e = 1, 2, 3$.

For all non-intern workers, annual departures rise by 1.56 per 100 pre-exposure employees [SE 0.39], while the estimate for the current-year rate is -0.89 [SE 2.07]. These flows include new hires and repeated jobs at the firm, so they do not identify replacement hiring or involuntary departures. Their samples and adjustments also differ from the fixed-roster and monthly employment comparisons; adding the three estimates would not decompose the employment response.[104]

Allowing firms recruiting from the same school to move together. Firms may face common changes because they recruit from the same schools, even when they belong to different corporate groups. Table A.9 accounts for this by grouping firms according to the school with their largest recruiting share and giving firms in the same group a common bootstrap multiplier. I re-estimate the comparison weights in each of 499 draws.

[103] I exclude identified internship spells throughout. The graduate and student-intern group is defined at first firm entry, and excluding this group removes its members from both the flows and the employment denominators.

[104] Monthly and year-end employment, existing-worker outcomes and whole-firm flows use 99 corporate-cluster multiplier draws, refitting the probability model in each draw, so their uncertainty estimates are preliminary. Intervals use the bootstrap covariance and a Student t critical value with the number of corporate clusters minus one degrees of freedom. Hiring and sponsorship use 499 draws.

E Policy Application: Employment Responses to an OPT Fee

In Section 6, I introduce a simple model of an OPT fee that assigns the same employment response to everyone deterred from participating. Here, I allow initial OPT participation and extension use to respond separately, and model how graduates' work changes through H-1B visas, further study, and other routes. I first explain the participation assumptions and work histories used in the calculation, then describe these adjustments and their implications for employment.

E.1 A Common Basis for Comparison

The simple calculation uses annual authorization counts to approximate two groups: graduates who use OPT without the extension and graduates who use both. In 2024, 194,554 initial OPT authorizations and 95,384 extension authorizations had employment start dates, leaving a difference of 99,170 (U.S. Immigration and Customs Enforcement, n.d.*b*). For the two groups, respectively, I apply projected participation declines of 28.8% and 35.0% when students bear the fee, and 63.5% and 79.3% when universities do (Clemens, 2026). This assumes a stable division between the groups and no switching from OPT with the extension to initial OPT alone. I use the combined bachelor's/master's rates for all degrees and hold enrollment fixed; Section E.5 allows responses to differ by degree.[105]

Figure E.1 compares several ways of translating lost participation into lost work. The first two use the estimated employment effect of extension eligibility, divided by its effect on extension use within the same sample. The three-year sample gives 1.17 work-years per additional extension user [SE 0.29], whereas the five-year sample gives 1.23 [SE 0.42], of which 1.00 occurs in its first three years. The two samples differ in both population and follow-up period, and each curve uses its own sample's annual estimates. A third calculation uses observed work on OPT and multiplies it by 0.733, the estimated share of additional OPT work that increases total U.S. work in the three-year sample. I then multiply this employment profile by the same annual number of lost participants. This assumes a common work response per OPT work-month across participants and years, while allowing authorized time to differ from time actually worked.

The richer model instead reduces work on initial OPT and on the extension, then allows work through other routes to change. The shares of initial-OPT and extension work retained

[105]The fee scenarios use 2024 dollars. The student participation declines are lower bounds within the source's model; I use them as point scenarios.

under the fee are 0.712 and 0.650 when students pay, and 0.365 and 0.207 when universities pay. Extension work falls by the higher-fee response alone, whereas the simple calculation divides graduates into separate groups using OPT without and with the extension. Both ways of assigning the fee responses require assumptions beyond the source's participation projections.

E.2 Baseline Work and Reweighting

The broader descriptive baseline used for the dynamic curves in Figures E.1, E.3 and E.4 consists of 181,381 distinct administrative participants with qualifying post-completion OPT authorization following bachelor's, master's, or doctoral completion in 2017–2018.[106] The 36,814 linked participants have four years of follow-up and remain in the sample even if no job is recorded, since participation is defined by authorization. I measure monthly work from jobs overlapping authorization for OPT after the original degree, rather than from the duration of permission alone.

Starting with inverse-linkage weights, I calibrate the linked sample to the administrative population's degree, completion year and month, broad field, and origin. The preferred weights additionally balance observed extension use and original-OPT authorization duration within extension-use groups. Figure E.4 compares these weights with the weights based on demographic and program characteristics alone and a scheme that also balances whether an OPT employer start date is recorded within those groups. These weights describe the baseline participant population and are distinct from the weights in the relabeling design. They require that the linked histories represent unobserved histories after adjustment. The four-year comparisons use the same participants throughout.

The richer curve in Figure 12 instead uses the complete five-year sample of 161,326 participants and 32,775 links, excluding December 2018 completions whose full fifth year ends after the administrative records stop. I recalibrate the weights to this population and fit the model to its five-year total of work during inferred H-1B periods, so all five annual contributions come from the same sample and calibration.

E.3 H-1B, Later Study, and Remaining Work

I assign recorded work to four nonoverlapping categories: work on OPT after the original degree, during inferred H-1B status, through a later F-1 program and its authorization, and

[106] I retain one record per person: the earliest qualifying 2017–2018 completion with post-completion OPT, provided that the associated original-program OPT episode has ended by October 15, 2023. This need not be the person's first use of OPT.

through remaining routes. A graduate who moves from OPT to H-1B work leaves the OPT category, so a reduction in H-1B entry can leave more graduates working on OPT.[107]

At each annual lottery opportunity, I divide graduates who have not entered H-1B work according to whether they are working on OPT. Within each group, the number entering H-1B work equals the group's size times the historical lottery selection rate times a coefficient that combines the likelihood of applying and starting work if selected. I fit these two coefficients jointly with the initial-OPT response to match three quantities: the first-year share of the historical OPT response, the cumulative H-1B response relative to the OPT response, and total inferred H-1B work in the baseline sample. The fitted coefficients are 0.300 on OPT and 0.161 outside OPT for the four-year sample, and 0.294 and 0.148 for the five-year sample.[108]

Lottery opportunities begin in the first March on or after completion, with work starting in October after selection. I assume that 95% of H-1B work continues each year. Graduates waiting to start H-1B work can still work on OPT; after leaving H-1B work, they do not return to OPT associated with the original degree. Each graduate enters H-1B work at most once. Matching total inferred H-1B work disciplines its overall amount, while monthly timing depends on these assumptions. Figure E.3 varies continuation with the entry coefficients either held fixed or refitted to the same historical targets.[109]

A later degree and its associated work authorization can recover some of the work lost when OPT becomes less attractive. I use the historical response to extension eligibility to estimate how much additional later study follows a reduction in OPT work, and how much work occurs during that study or its associated authorization. I also allow work through remaining routes to respond to the change in time available outside OPT work, H-1B work, and later study.

For a given month, let ℓ_{OPT} and ℓ_H be the reductions in OPT and H-1B work per baseline participant. Let ΔS and ΔF be changes in time and work associated with later study, measured as the fee scenario minus the no-fee scenario. If remaining work increases by b per additional month available outside these activities, total graduate work lost is

$$\ell = (1 - b)(\ell_{\text{OPT}} + \ell_H) - (\Delta F - b\Delta S). \tag{13}$$

[107] A recorded OPT or H-1B job-month counts as one work-month. Work associated with a later F-1 program counts as one-half unless full-time authorization is active, following the paper's measurement convention; actual hours are unobserved.

[108] Through filing year 2019, the selection rate is selected petitions divided by submitted petitions (U.S. Department of Homeland Security, 2020, Table 3); for 2020–2023, it is selected registrations divided by eligible registrations (U.S. Citizenship and Immigration Services, 2026). These rates concern petitions or registrations, rather than people, and do not distinguish advanced-degree eligibility or multiple employers.

[109] Annual selection counts can include later lottery rounds; assigning successful starts to October approximates their timing.

The first term allows other work to replace part of the lost OPT and H-1B work. The second subtracts work recovered through later study, net of other work displaced by time spent in that study or its authorization. All terms measure monthly work-equivalents per baseline participant; I aggregate them into the annual national loss below.

I set b to the historical change in remaining work divided by the change in available time, giving 0.214 work-months per additional month. Annual historical ratios determine added later-study time per OPT work-month lost and work during that time; the third-year ratios apply in subsequent years. The model retains observed baseline remaining work and uses b only for its change.[110]

To consider a fee that also discourages later study, I reduce both time and work associated with later F-1 programs and their authorizations by the same proportion. A factor of one leaves the model's return-to-school response in place; a factor below one reduces both existing later-study activity and the additional activity induced by lost OPT work. In the four-year sample, a factor of 0.75 raises fourth-year employment losses from about 58,000 to 62,000 when students bear the fee and from 131,000 to 137,000 when universities do. The increase depends on how much other work replaces the work lost through later study. The factor is assumed rather than estimated.

Figures E.3 and E.4 vary the school response, remaining work, entry timing, selection benchmarks, and the responses of initial OPT and extension use. When a variant changes H-1B work, the OPT work profile conditional on not having entered H-1B work is adjusted to reproduce observed OPT work without a fee. I omit two scenarios that imply more work than the model permits: 80% annual continuation of H-1B work when the entry responses are recalibrated, and 25% higher H-1B selection rates when those responses are held fixed. Some parameters can compensate for one another in calibration: scaling selection probabilities and inversely scaling the effective-entry coefficients preserves the reference paths, so the refitted variants cannot separate these parameters.

E.4 Aggregation, Other Workers, and Uncertainty

For each dynamic scenario, I average the monthly work gaps within postgraduation years and add the contributions of successive annual cohorts.[111] These gaps already include the

[110] Available time excludes OPT and H-1B *work*, rather than all time in those statuses, so it includes nonwork during them. Later study includes the associated work authorization. Its response follows the current loss of OPT work, without a separate model of degree progress.

[111] This treats the annual authorization flow as nonoverlapping entrant cohorts. The source flows can count a person again after a later degree. Because the work profiles include later-program OPT, repeated participation could assign the same later employment to more than one entrant cohort; a national person-level projection would need to resolve this overlap.

participation response, so I multiply them by the annual baseline flow of 194,554. Figures E.1, E.3 and E.4 use the broader four-year baseline for their dynamic comparisons. Figure 12 instead uses the complete five-year dynamic path alongside the five-year empirical profile.

To extend each profile to ten policy years, I specify the total employment effect in years six through ten after graduation. For the simple profile, the three alternatives are $e_h = 0$, $e_h = e_5\, 2^{-(h-5)/3}$, and $e_h = e_5$, for $h = 6, \ldots, 10$. Figure 12 plots the three-year half-life case. For the richer model, I apply the same factors to its fifth-year total employment gap per baseline participant, then aggregate successive cohorts. The illustrative three-year half-life halves the employment effect between years five and eight after graduation. The alternatives change only the effect beyond the observed years for earlier cohorts; each new cohort continues to follow the same five-year profile. A zero later effect gives a constant national gap after policy year five, whereas a positive later effect adds contributions from older cohorts. The shaded region in that figure combines older-cohort contributions under this half-life with the contributions of newer cohorts within their first five years. These extensions operate on the total employment gap rather than on separate H-1B lottery, schooling, or status-transition paths. The three alternatives are scenarios; effects that reverse sign or grow beyond their fifth-year level are also possible. The national entrant flow, composition, participation declines, and employment multiplier are held fixed.

Even if other workers replace one-quarter of graduates' lost employment ($m = 0.75$), projected losses by the fifth policy year remain about 57,000 worker-equivalents when students bear the fee and 128,000 when universities do (Figure E.2). The figure holds the simple employment profile and participation responses fixed while varying the multiplier: values below one allow other workers to replace some of the lost work, while values above one allow additional employment losses among other workers. These national responses are assumptions, since the firm and local evidence can include employment moving between firms or areas rather than changes in total U.S. employment.[112] The dashed limits show sampling uncertainty in the historical employment response at $m = 1$, conditional on participation and the other policy assumptions; the alternative curves show how the projection changes under different assumptions about other workers.

[112]The employer design concerns existing firms and a broader pool of domestic and international graduates; local evidence such as Kwon, Lee and Monras (2026) uses a different immigrant population. The calculations also hold H-1B selection rates fixed, without estimating employment changes among alternative recipients of capped visas. A multiplier covering all education levels would describe total employment rather than skilled employment alone.

E.5 Responses by Degree

Graduates can differ both in how readily they forgo OPT and in how much additional U.S. work OPT allows them to do. I examine these two responses in turn using bachelor's, master's and doctoral graduates in the five-year baseline sample. For each degree, I apply the participation decline at the $70,000 fee to observed work on initial OPT and the decline at $100,000 to work on the extension. I then multiply the lost OPT work by the historical ratio of additional total U.S. work to additional work on OPT. This assumes that graduates deterred from each stage would otherwise have had their degree group's average work history. I aggregate across degrees and successive cohorts, holding enrollment and other workers' employment fixed.[113]

For participation, I use the bachelor's and master's responses in Clemens (2026, Tables 2(b) and 5(b)), reported in Panels A–B of Figure E.5. For employment, I divide the estimated first-three-year effect of extension eligibility on total U.S. work by its effect on recorded OPT work. The ratio is 0.82 [SE 0.27] for bachelor's graduates and 0.66 [SE 0.17] for master's graduates, compared with 0.73 pooled. The bachelor–master difference is imprecise, at 0.16 [SE 0.30]. Applying these historical ratios to graduates deterred by a fee and to all five postgraduation years remains an assumption. For doctoral graduates, I use the combined participation rates and pooled work ratio because comparable degree-specific estimates are unavailable.

Panels C–D hold this calculation fixed while varying the two responses. Allowing participation to differ by degree raises the projected annual loss in policy year five from 52,600 to 54,900 worker-equivalents when students bear the fee and from 117,400 to 123,100 when universities do. Master's graduates face larger projected participation declines and have longer observed OPT work histories. Allowing the work response to differ offsets this increase, giving losses of 52,200 and 116,300, close to the common-response benchmark.[114] The doctoral assumption matters because doctoral graduates account for about 18% of baseline participants. Keeping their work ratio fixed but assigning them the bachelor's or master's participation rates gives losses of 48,200–54,700 when students pay and 104,400–123,200 when universities pay.

I also allow participation responses to differ by degree in the model of OPT, H-1B, further study and other work, retaining pooled work histories and common transition assumptions.

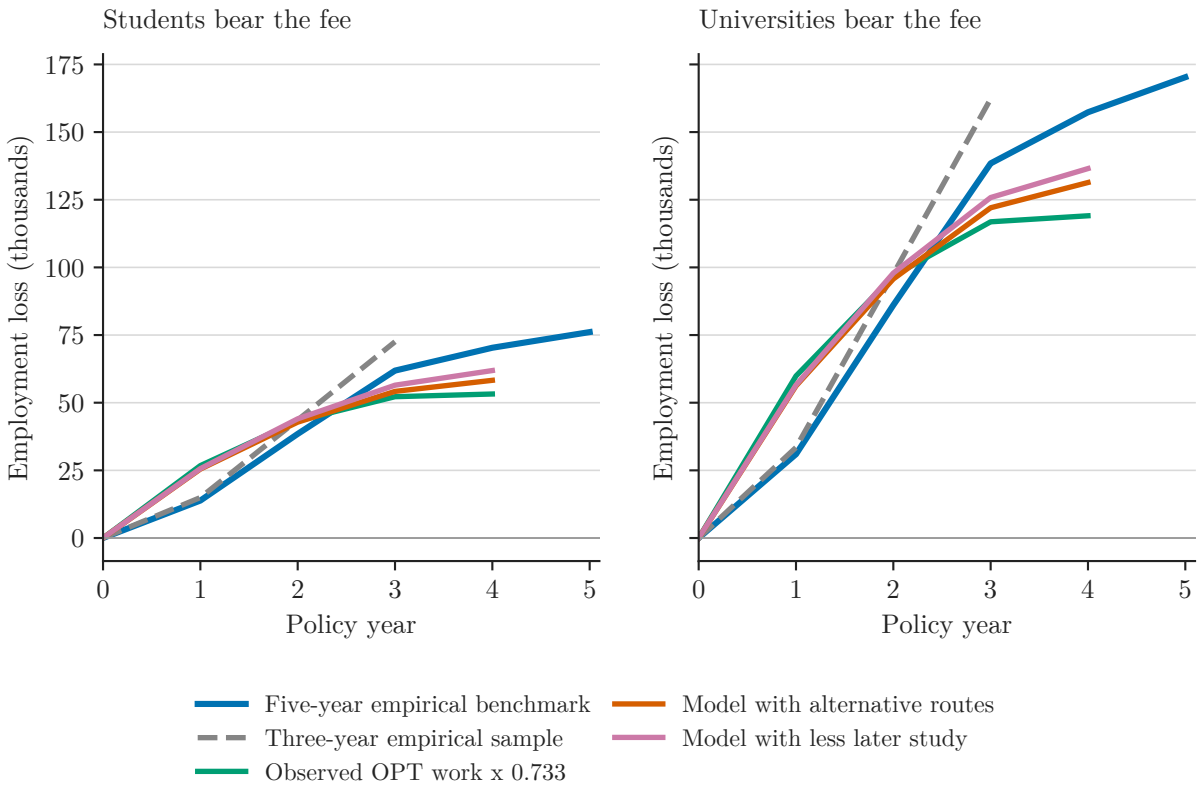
[113]This calculation uses the five-year sample of 161,326 participants and 32,775 links, with its weights and completion-date composition, and the annual flow of 194,554. The total-work response already includes work through other routes.

[114]Every curve applies the participation responses to initial-OPT and to extension work, whereas the observed-work curve in Figure E.1 scales the pooled OPT-work profile by one number of lost participants. The estimated work ratios and participation responses are held at their point estimates.

Year-five losses become 62,400 and 140,000, compared with 61,900 and 139,500 under common participation responses.[115]

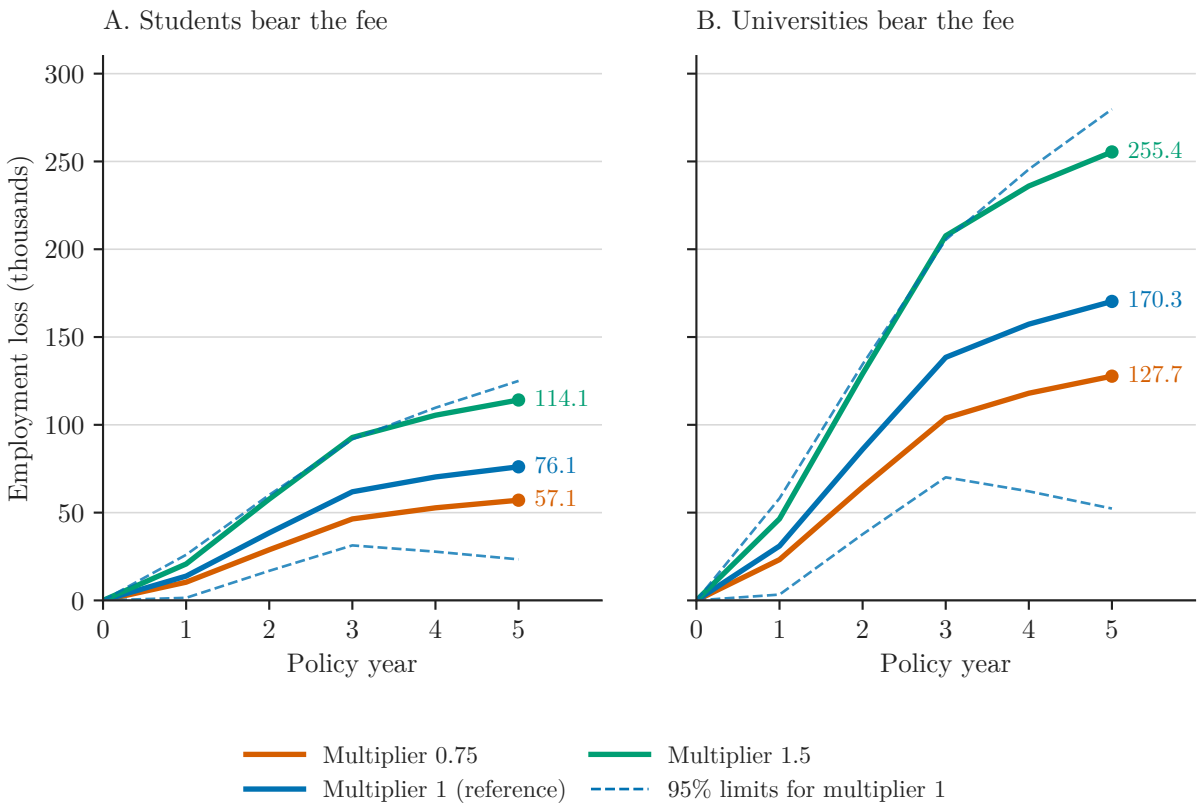
[115]With common transition and work-response assumptions, separate degree histories sometimes require more work than the model permits. I therefore use pooled histories for every degree in this comparison; the observed-work calculation above uses the separate degree histories.

Figure E.1: Employment losses under alternative graduate-work responses



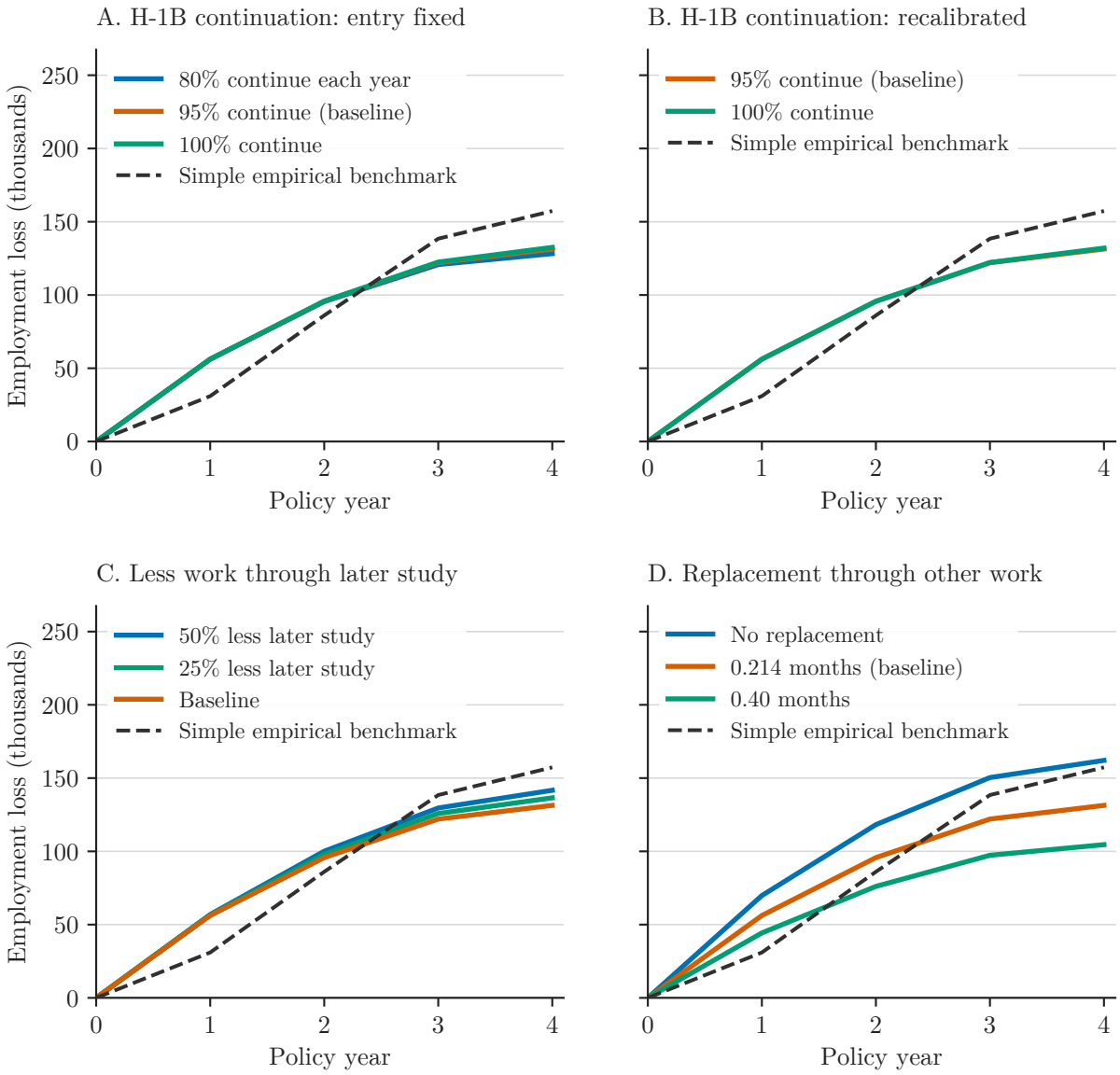
Notes: Annual employment losses under fees of $70,000 for initial Optional Practical Training (OPT) and $100,000 for OPT with its extension, holding enrollment and other workers' employment fixed. A worker-equivalent is twelve months of recorded U.S. work, with study work generally counted at half weight. Policy year 1 begins with the first affected graduating cohort. Empirical curves use the response per additional extension user from the indicated sample. The observed-work curve applies the additional-work share, 0.733, to OPT work. Model curves allow H-1B, later study and other work to adjust; the reduced-study case cuts later-study time and work by 25%. Those curves use four-year histories and an annual flow of 194,554. Curves stop at their sample's follow-up limit.

Figure E.2: Employment losses under alternative responses by other workers



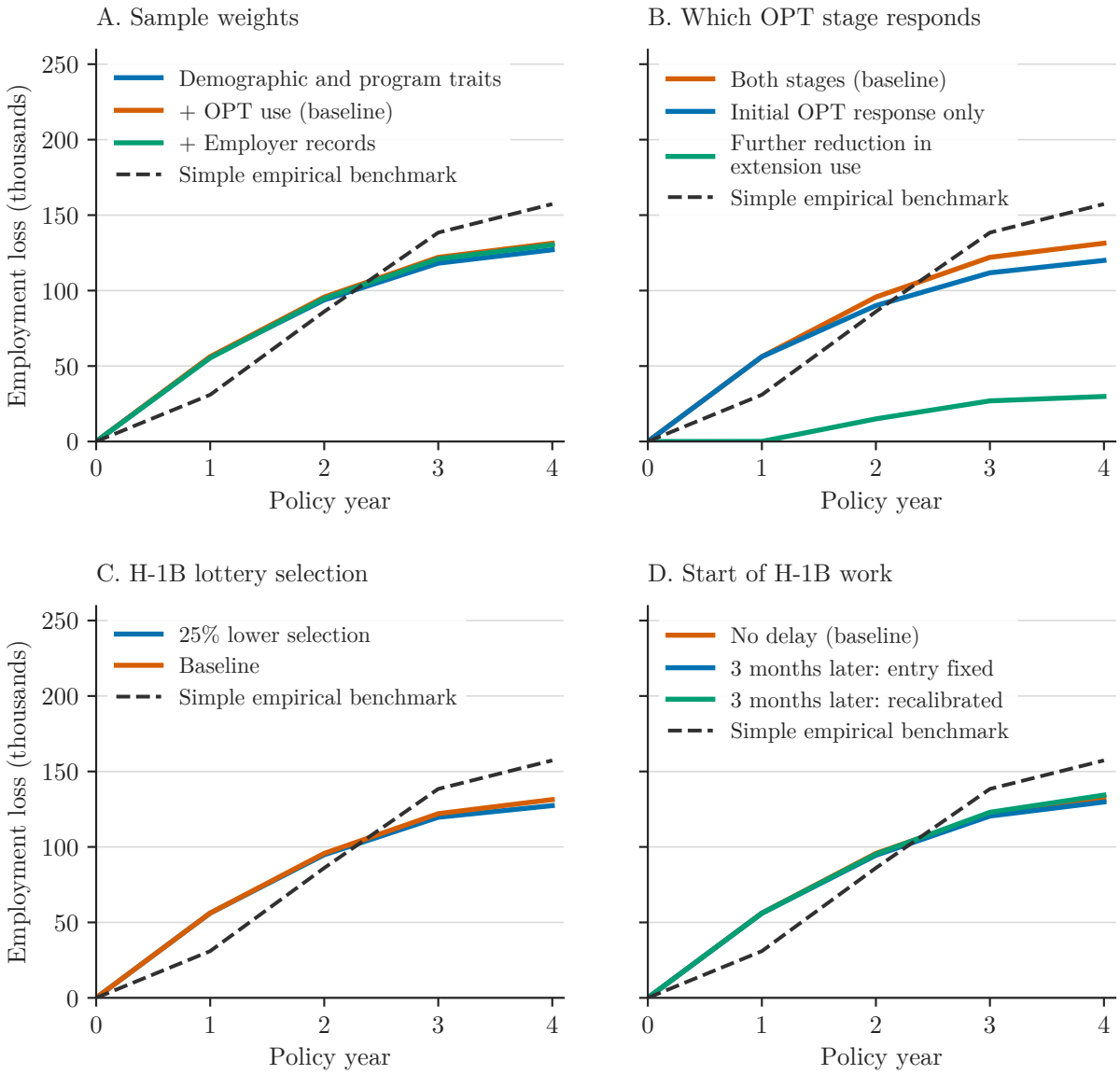
Notes: Annual employment losses under fees of $70,000 for initial Optional Practical Training (OPT) and $100,000 for OPT with its extension, holding enrollment fixed. Policy year 1 begins with the first affected graduating cohort; each year adds another. The multiplier describes other workers' responses: 0.75 offsets one-quarter of graduates' lost work, 1 leaves other employment unchanged, and 1.5 adds half as much lost work among other workers. The graduate response is held fixed. Dashed lines give pointwise 95% limits for multiplier 1 from 499 school-level bootstrap draws, conditional on participation and policy assumptions. A worker-equivalent is twelve months of recorded U.S. work, with study work generally counted at half weight. Labels give year-five losses in thousands.

Figure E.3: Sensitivity to H-1B work, later study, and remaining work



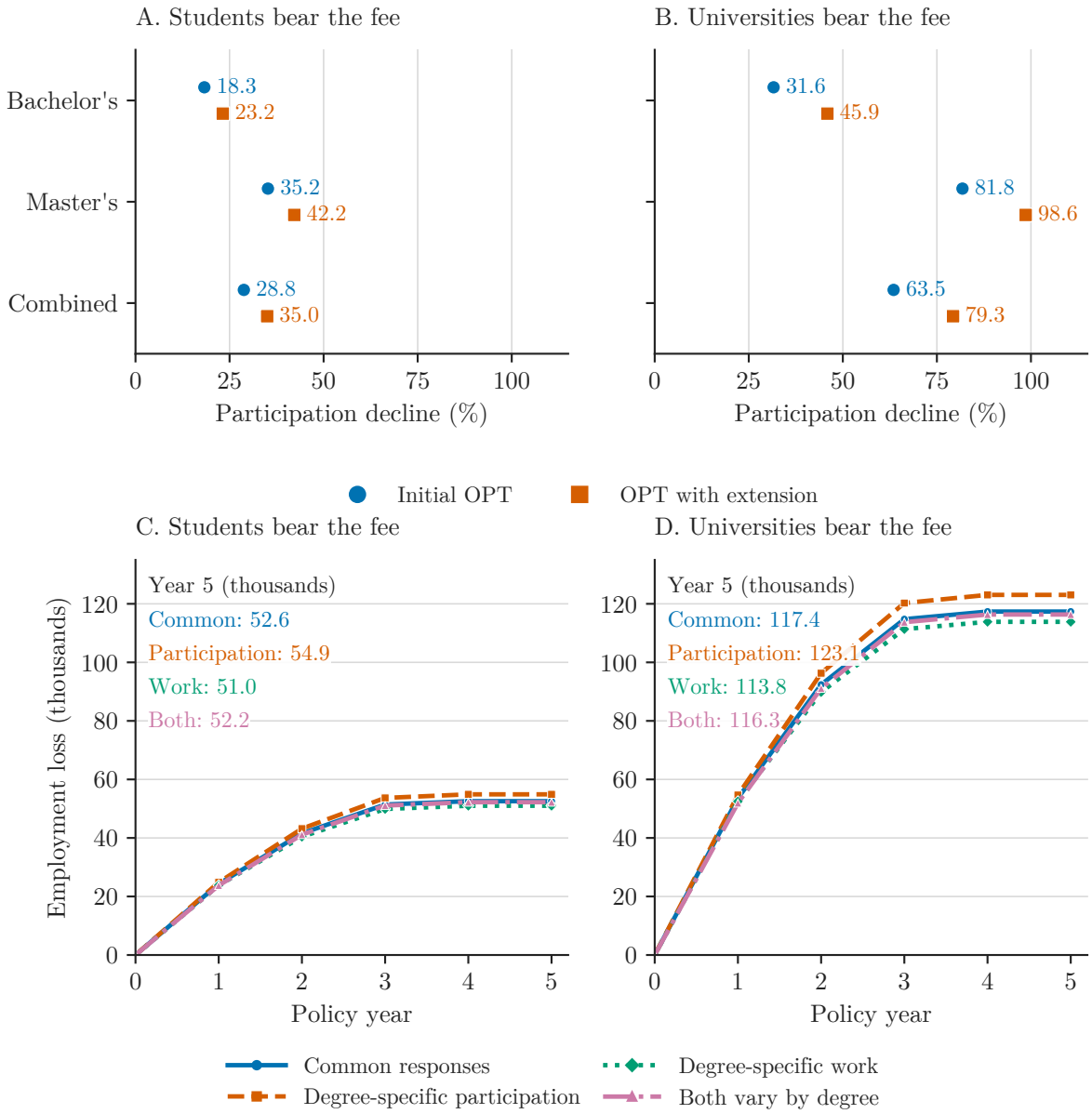
Notes: Annual employment losses when universities pay $70,000 for initial Optional Practical Training (OPT) and $100,000 for OPT with its extension, holding enrollment and other workers' employment fixed. A worker-equivalent is twelve months of recorded U.S. work, with study work generally counted at half weight. Policy year 1 begins with the first affected graduating cohort. Orange is the baseline model and the dashed line the simple empirical benchmark. A–B vary annual continuation of H-1B work: A holds fitted responses for applying and starting work fixed, while B recalibrates them to the same historical targets. C reduces later-study time and work. D varies months of other U.S. work per month outside OPT work, H-1B work and later study. Model curves use four-year histories of OPT participants and an annual flow of 194,554; Appendix E describes the assumptions and omitted scenarios.

Figure E.4: Sensitivity to weighting, OPT participation, and the start of H-1B work



Notes: Annual employment losses when universities pay $70,000 for initial Optional Practical Training (OPT) and $100,000 for OPT with its extension, holding enrollment and other workers' employment fixed. A worker-equivalent is twelve months of recorded U.S. work, with study work generally counted at half weight. Policy year 1 begins with the first affected graduating cohort. Orange is the baseline model and the dashed line the simple empirical benchmark. A adds OPT use and duration, then employer starts, to the characteristics balanced by the weights. B varies which OPT stage responds to the fee. C lowers historical H-1B lottery selection by 25%, holding graduates' fitted responses for applying and starting work fixed. D delays H-1B work by three months, either holding these responses fixed or recalibrating them to the same historical targets. Model curves use four-year histories of OPT participants and an annual flow of 194,554; Appendix E describes the assumptions and omitted scenarios.

Figure E.5: Employment losses when participation and work responses vary by degree



Notes: A–B show projected participation declines at fees of $70,000 for initial Optional Practical Training (OPT) and $100,000 for OPT with its extension (Clemens, 2026). C–D apply these responses to work in each stage, then scale lost OPT work by the historical ratio of additional U.S. work to additional OPT work. Policy year 1 begins with the first affected graduating cohort. Curves vary participation and work responses by degree, holding five-year histories, an annual flow of 194,554, enrollment and other workers' employment fixed. Doctoral graduates use combined participation rates and the pooled work ratio. A worker-equivalent is twelve months of recorded U.S. work, with study work generally counted at half weight. Responses remain at their point estimates; Section E.5 describes the calculation.