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The Effect of Retaining High-Skilled International Graduates: Evidence from the STEM OPT Extension

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Abstract

High-skilled migration programs exist around the world in the hope that immigrants complement native workers, allow firms to grow, and boost innovation. We study the effect of one such program by exploiting the 2016 extension of the Optional Practical Training (OPT) program, which significantly prolonged the work authorization period for international STEM graduates. Using a synthetic difference-in-differences approach, we find that the policy successfully increased the local supply of high-skilled immigrants in exposed Commuting Zones. This local inflow stimulated firm creation and the demand for native high-skilled workers. The program might have also boosted innovation in certain sectors and startup investment, especially in Commuting Zones hosting top-ranked universities, where, overall, the effects tend to be larger.

Keywords: Immigration, labor demand, firm dynamics, high-skilled migration.

JEL Code: F22, J31, J61, R11

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1 Introduction

High-skilled immigration policies—such as the H-1B visa program or the F-1 and J-1 student visas—are among the most important labor market policies for the development of IT giants, startup creation, and innovation (Bryan and Williams, 2021). The rationale behind legal pathways towards citizenship is that high-skilled immigrant workers, usually younger ones, are supposed to bring skills that are difficult to find in host economies. This should complement existing workers, allow firms to grow, and boost innovation. Perhaps the most important concern over this type of programs is that high-skill foreign workers may "take" the jobs of native high-skill workers. In fact, to preempt this from happening, many such programs require firms to show that no alternative native worker could be found for the position.

Reflecting these concerns, a large literature studies whether high-skilled immigrants displace native high-skill workers using three main identification strategies: (i) quasi-random variation from the H-1B lottery when annual caps bind (Doran et al., 2022; Dimmock et al., 2022; Glennon, 2024), (ii) policy-driven changes in the H-1B cap over time (Kerr and Lincoln, 2010; Mayda et al., 2018), and (iii) shift-share designs that instrument for high-skilled immigrants' location choices (Peri et al., 2015). Across approaches, evidence on crowd-out is mixed, but results more consistently point to gains in innovation and productivity, as well as adjustments along other margins.[1]

In this paper, we propose a new empirical research design to revisit this important question. We use variation generated by the extension in 2016 of the Optional Practical Training (OPT) program in the United States. The OPT program, formalized in the 1992 rule under the Immigration and Naturalization Service (INS), allows international students in the United States on F-1 visas to gain practical work experience related to their field of study. In 2016, the U.S. Department of Homeland Security (DHS) introduced the STEM OPT extension rule, published on March 11, 2016, and effective from May 10, 2016,

[1] Doran et al. (2022) find substantial within-firm substitution (roughly 1.5 fewer other workers per additional H-1B) with only modest innovation effects, while Dimmock et al. (2022) show that greater H-1B access improves startup performance (funding, exits, and patenting), and Glennon (2024) finds that tighter H-1B access shifts activity abroad via higher foreign affiliate employment. Consistent with these patterns, cap and shift-share studies generally find that higher H-1B admissions raise immigrant STEM employment and patenting with limited native displacement (Kerr and Lincoln, 2010; Mayda et al., 2018) and are associated with higher native wages and city-level productivity (Peri et al., 2015).

which allowed eligible STEM graduates to work in the U.S. for up to 36 months in total under the OPT program. The regulation also expanded the list of qualifying STEM fields and aligned work authorization with broader national goals of attracting and retaining high-skilled foreign talent in STEM occupations.

Hence, this change in policy increased, de facto, the supply of high-skilled immigrants in the US in occupations that likely complement native high-skill workers, enable firms to grow, and boost innovation. Relative to the lottery designs, our approach uses variation that reflects what happens in the local labor market, rather than at the firm. Relative to the papers using policy-driven changes in the cap and shift-share instruments, our design captures the effects of *young* and *US-educated* immigrant workers and allows us to trace dynamic effects more precisely, something that may be of particular policy relevance.

To evaluate the effects of the increase in the number of foreign-born skilled workers, we leverage cross-Commuting Zone (CZ) variation in the exposure to the OPT extension prior to the policy using a synthetic difference-in-difference strategy. This strategy allows us to deal with concerns on differential pre-trends across CZs, and hence to evaluate the effects of the policy by comparing treated CZs – defined as the top 25% CZs in terms of eligible international students to population ratios – relative to control CZs selected to match pre-treatment trends. While there are systematic level differences in a number of characteristics between treatment and control CZs, we show that trends between treatment and control during the period 2010 to 2015 track each other very well for all our outcome variables.

Using this empirical design, we first confirm that the policy significantly increased the number of high-skilled immigrants in treated CZs. The magnitude of this increase is equivalent to 0.3 percent of the working-age population relative to the control group. In terms of composition, approximately 50 percent of this inflow was driven by STEM workers, with self-employed immigrants accounting for another 8 percent. We show that these estimates are robust to controlling for potential confounders.

Consequently, the OPT extension led to a substantial increase in the number of high-skilled immigrants. This influx could increase competition for native workers, but it could also create incentives to form new businesses, expand operations, and hire natives who complement immigrant labor. Empirically, we find that the number of firms increased, along with native high-skilled employment and wages, in treated CZs relative to control areas. This evidence is consistent with the idea that young and high-skilled immigrant workers who entered the labor market complemented existing high-skilled native workers, both young and old, and allowed production to expand.

Quantitatively, the estimated effects are substantial. We find that the total number of firms increased by approximately 1%, with the effects concentrated among small (below 20 employees) and medium-sized enterprises (20 to 99 employees). Native employment increased as well, by around 0.5 percent of the working-age population, when comparing treatment and control groups. These employment effects occur among native high-skilled workers in general: both young and old, and those in STEM and non-STEM industries. Low-skilled employment, however, did not change in treated relative to control CZs. Consistent with an increase in the demand for high-skilled native labor, high-skilled native wages increased by around 1%, while wages of native low-skilled workers, if anything, decreased slightly.

Finally, we study the effects of high-skilled immigration on innovation and credit access. While we find no significant change in aggregate patenting and credit, we document concentrated growth in high-tech sectors specifically targeted by the policy. In general, however, we find smaller effects on direct measures of innovation than some of the past literature, perhaps because the 2016 OPT expansion affected mostly immigrant workers at the start of their careers.

In the last part of the paper, we investigate whether results differ as a function of the quality of the universities across CZs. In particular, we classify treated CZs between those that host a star university (top 20 university in US News' Best National Universities Rankings) and those that do not. We find stronger effects in CZs that have a star university. When focusing only on CZs with top universities, we find some evidence consistent with start-up creation, since the number of small business loans (SBLs) increased strongly relative to control CZs.

Our work primarily contributes to the literature on the local economic impact of high-skilled immigration. A number of papers identify theoretical channels through which high-skilled migration may generate positive labor market effects, many of which operate through firm or job creation (Waugh, 2018; Albert, 2021). Other papers suggest that high-skilled migration to the U.S. induces both labor competition and innovation, implying theoretically ambiguous effects on high-skilled natives (Bound et al., 2020; Khanna and Lee, 2020; Bound et al., 2015; Jaimovich and Siu, 2018). Empirically, the literature generally finds that immigrants have positive effects on labor market outcomes, innovation, and firm creation (Terry et al., 2025; Hunt, 2010; Peri, 2012; Beine et al., 2024; Moser et al., 2025), except perhaps for narrow groups of native workers (Borjas, 2005, 2009).[2] Unlike most of these studies, which are restricted to the ten-year horizons implied by decennial Census data, our time period and data allow us to trace the full adjustment to an increase in the supply of high-skilled immigrants across a range of high-skilled occupations, yielding estimates that may be more relevant for standard high-skilled labor supply shocks. Moreover, the STEM OPT extension provides a cleaner, localized, and exogenous supply shock of young, US-trained, high-skilled workers, allowing us to precisely estimate the dynamic local adjustment in the labor market, in contrast to broader immigration trends.

Our paper is also related to the literature on high-skilled immigration policy in the U.S. Much of this work focuses on the H-1B visa program—the primary route for high-skilled immigrants to work in the U.S.—and studies outcomes such as innovation and local productivity.[3] In contrast, relatively few papers examine the impact of the OPT extension, which provides eligible international students an additional attempt at securing an H-1B visa (Amuedo-Dorantes et al., 2020). Among those, Kim (2022) studies the impact on STEM-designated business programs at universities, and Khoo (2025) examines the effect on international students' major choice, but neither analyzes its impact on local economies. The only exception we are aware of is Guo et al. (2024), who study the effects of the OPT extension on the supply of foreign IT professionals in local labor markets, which corresponds closely to our first-stage result. Exploiting variation in high-

[2]Bernstein et al. (2025) use the death of immigrant scientists to study the effect of migrants on innovation and spillovers, while Moser et al. (2014), Moser and Nguyen (n.d.), and Borjas and Doran (2012) use (mostly foreign-born) star scientists fleeing Nazi-Germany and Russia in the early 1990s to study similar questions. We differ in that we consider a high-skilled labor supply shock of young, US-trained workers affecting the overall local labor market.

[3]For instance, see Hunt and Gauthier-Loiselle (2010); Kerr and Lincoln (2010); Peri et al. (2015); Kato and Sparber (2013); Clemens (2013).

skilled immigration induced by this policy change, we examine a range of local economic outcomes, including the number of firms, the effects on different types of labor, patent activity, small business lending, and capital.

2 Background: The 2016 STEM OPT Extension

The Optional Practical Training (OPT) is a program that allows international students in the United States holding an F-1 visa to gain temporary employment directly related to their field of study. The term "optional practical training" and the current regulatory structure were formalized in the 1992 rule under the Immigration and Naturalization Service (INS) (Miano, 2017).[4] While there are two types of OPT (*pre*-completion OPT and *post*-completion OPT), most international students typically participate in the *post*-completion OPT as a means to legally work and extend their stay in the U.S.[5] The post-completion OPT often serves as a transitional period before obtaining an H-1B visa, a temporary employment visa for specialty occupations. During this period, graduates can gain U.S. work experience, establish employer relationships, and apply for H-1B sponsorship, thereby increasing their likelihood of remaining in the U.S. labor market.

A major policy shift occurred in 2016, when the U.S. Department of Homeland Security (DHS) introduced the STEM OPT extension rule, published on March 11, 2016, and effective from May 10, 2016 (U.S. Department of Homeland Security, 2016).[6] The 2016 rule replaced the previous 17-month extension introduced in 2008 with a new 24-month extension, allowing eligible STEM graduates to work in the U.S. for up to 36 months in total under the OPT program. This regulation expanded the list of qualifying STEM fields and aligned work authorization with broader national goals of attracting and retaining high-skilled foreign talent in STEM occupations.

Under the 2016 regulation, eligible students must hold a degree in a DHS-designated STEM field at the bachelor's level or higher, work for an employer enrolled in the E-

[4](57 Fed. Reg. 31,954 (July 20, 1992))

[5]The *pre*-completion OPT permits international students to work part-time during academic terms or full-time during official breaks prior to graduation, but it is reported by some institutions to be used far less frequently than post-completion OPT.

[6]The DHS first announced the proposed rule for the new STEM OPT extension on October 19, 2015, and published the final rule on March 11, 2016, which became effective on May 10, 2016.

Verify system, and submit a formal training plan outlining how their employment provides practical training consistent with their academic degree. The eligible fields include major disciplines such as computer science, engineering, mathematics, biological sciences, and physical sciences, as well as newly added interdisciplinary areas such as cognitive science and environmental sciences.[7]

The 2016 STEM OPT extension has important implications for the geography of high-skilled immigration. Although F-1 graduates with OPT authorization are technically free to relocate across regions, several features of the policy and the broader visa system create incentives for them to remain in the same local labor market where they completed their studies. First, OPT participants often work in firms that sponsor H-1B visas, and the extended 36-month window allows multiple opportunities to apply for H-1B status, which is allocated through an annual lottery. As a result, remaining in the same region increases the likelihood of finding or retaining H-1B-sponsoring employers.[8]

Second, local networks between universities and employers play a central role in matching international graduates to firms. Information flows, alumni networks, and regional labor market institutions create localized channels that facilitate employment for OPT participants. For instance, technology clusters such as Silicon Valley, Boston, and Austin likely experience an increase in the local supply of high-skilled immigrant workers in areas already hosting large numbers of international students.

Consequently, the 2016 STEM OPT extension can be viewed as an exogenous policy shock that differentially affected local labor markets depending on their pre-existing exposure to international students in STEM fields. Regions with a higher share of STEM international graduate students prior to 2016 were expected to experience a larger post-policy increase in high-skilled immigrants. These differences provide a quasi-experimental setting for evaluating how an expansion of temporary high-skilled work authorization influences local labor markets, firm dynamics, and native employment.

[7] The list of eligible majors is in Appendix Table C.1 and C.2.

[8] In addition to higher sponsorship rates, local firms familiar with university graduates may provide informational and network advantages that improve the probability of transition from F-1 to H-1B status.

3 Empirical Framework

In this section, we describe the data sources and explain our estimation strategy for identifying the impact of the STEM OPT extension on local labor markets. We also present evidence on whether treated CZs experienced an increase in high-skilled immigrants following the policy change.

3.1 Data

The geographical unit of our analysis is CZ, which approximates a local labor market in the United States (Tolbert and Sizer, 1996). There are 722 CZs in the U.S., and this delineation has been widely used to study local labor market impacts of major structural changes such as trade shocks (Autor et al., 2013), automation (Acemoglu and Restrepo, 2020), and technological change (Autor and Dorn, 2013). Following this definition, we classify CZs into treated and control groups and implement the synthetic difference-in-differences (SDID).

Our main analysis focuses on the period 2010–2019, during which the STEM OPT extension was introduced in 2016, roughly at the midpoint of this window. Although some outcome variables are available outside this time range, we restrict attention to this period for two reasons. First, the global financial crisis (2008–2010) and the COVID-19 pandemic (2020–2022) are major confounding threats to our estimates. These shocks are particularly problematic for our setting because SBL—one of our outcomes—may have been heavily influenced by government relief policies and emergency credit programs during those periods. Second, international migration flows were substantially disrupted during the COVID-19 pandemic, which could obscure the effects of the STEM OPT extension.

In order to analyze the local economic impact of the 2016 STEM OPT extension, we draw on several data sources. First, to estimate the local stock of international students eligible for the STEM OPT extension, we use the American Community Survey (ACS). Because the ACS provides detailed individual-level information on birthplace, citizenship status, and field of degree, we can estimate the number of affected students across the 722

CZs with reasonable accuracy.[9] We also use ACS data to construct CZ-level wage and employment measures for native- and foreign-born workers, which allows us to examine local labor market responses.

Second, to study the impact on the number of firms in local labor markets, we utilize the annual Statistics of U.S. Businesses (SUSB) from the U.S. Census Bureau, which reports the number of firms by size during the week of March 12th. This dataset is our primary source for examining changes in the total number of firms and firm size categories, but it excludes solo self-employed firms, government establishments, and several specific industries.[10] To analyze the heterogeneous impact between STEM and non-STEM firms, we rely on the County Business Patterns (CBP) data, also from the U.S. Census Bureau. CBP is necessary for industry-specific analysis because industry information in the SUSB dataset is only consistently available starting in 2015, which restricts its utility for our pre-treatment analysis.

Third, to investigate the impact on local innovation, we utilize patent data from the U.S. Patent and Trademark Office (USPTO) for all patents granted between 2011 and 2024. The USPTO records provide comprehensive details, including application and grant dates, the identities of inventors, and their firm affiliations. Crucially, the data also includes citation counts, WIPO (World Intellectual Property Organization) technology fields, and the geographic location (county and state) of each inventor.[11] From this dataset, we construct CZ-level measures for the number of patents and citation-adjusted counts based on application years (2010–2019) to evaluate the local innovation landscape.

Fourth, for banking-related outcomes, such as SBLs and the amount of deposits, we use data from the Community Reinvestment Act (CRA). The CRA provides annual county-level, bank-level information from 2005 to 2021 on loans under $1 million. From these data, we extract both the number and the dollar amount of SBLs.[12] Since changes in local deposit supply may drive increases in SBLs, we additionally use data on total deposit

[9]Using the individual-level microdata, we first collapse the data to the Public Use Microdata Area (PUMA) level and then aggregate them to the CZ level following Autor and Dorn (2013).

[10]The SUSB dataset excludes agricultural production, railroad transportation, postal services, pension and trust funds, and private household services. Despite these exclusions, the remaining firms reasonably represent the local business landscape.

[11]County-level data are mapped to CZ level using the crosswalk developed by Autor and Dorn (2013).

[12]We translate county-level information to the CZ level using David Dorn's crosswalk (Autor and Dorn, 2013).

amounts to examine whether corresponding changes in bank deposits accompany the effects on SBLs. We also utilize firm-level capital data from Compustat North America to construct measures of capital expenditures and net property, plant, and equipment.[13]

3.2 Synthetic Difference-in-Differences

Our analysis employs the synthetic difference-in-differences (SDID) estimator developed by Arkhangelsky et al. (2021) to identify the impact of the 2016 STEM OPT extension on a broad set of CZ-level outcomes, including wages, employment, firm counts, SBLs, patents, and capital formation.

The SDID estimator combines key features of the traditional difference-in-differences (DID) framework with the synthetic control method (Abadie et al., 2010). Whereas standard DID relies on equal weighting across units and requires parallel pre-treatment trends, SDID improves identification by constructing outcome-based weights that align the treated and control groups in their pre-treatment outcome trajectories. Specifically, SDID assigns (i) *unit weights* to control CZs so that a weighted average of their pre-treatment outcomes matches those of the treated CZs, and (ii) *time weights* that reweight pre- and post-treatment periods to ensure that the synthetic control units remain comparable to the treated units throughout the sample period. Together, these weights mitigate violations of the parallel-trends assumption that arise when treated and control CZs follow different underlying trends.

Formally, we estimate the average treatment effect of the STEM OPT extension as in Clarke et al. (2023):

$$(\hat{\tau}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \arg \min_{\tau,\mu,\alpha,\beta} \left(\sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau)^2 \hat{\omega}_i \hat{\lambda}_t \right), \tag{1}$$

where $\hat{\tau}$ denotes the estimated average treatment effect, and α_i and β_t represent CZ and time fixed effects, respectively. The indicator W_{it} equals one for treated CZs in the post-2015 period and zero otherwise. The unit and time weights, $\hat{\omega}_i$ and $\hat{\lambda}_t$, are chosen to minimize discrepancies in pre-treatment dynamics between treated and control CZs

[13] We exclude financial sectors (NAICS 52) due to their distinct capital structures. Readers should interpret these results with caution, as Compustat data assigns capital to headquarters and overrepresents large, publicly traded firms, potentially obscuring local-level dynamics.

and to balance the relative influence of pre- and post-period observations. This weighting structure is the key distinction from standard DID, which implicitly applies equal weights across units and time.

The SDID framework is particularly well-suited for our empirical setting because we examine a wide array of outcomes—including immigration flows, wages, employment, firm activity, and credit supply—that may display heterogeneous pre-treatment trends across CZs. Such heterogeneity may violate the parallel-trends assumption and bias traditional DID estimates. By explicitly aligning pre-treatment outcome paths through synthetic weighting, SDID provides a more credible counterfactual and substantially strengthens identification across the diverse set of outcomes. In robustness checks, for our key outcomes, we also present simple DID estimates to see if our results are entirely driven by the SDID method.

The first step in implementing the SDID method in our setting is to assign treatment and control status to the 722 CZs. To do so, we construct a measure of local exposure to the STEM OPT extension, defined as the number of international students eligible for the extension divided by the working-age population in 2014. Specifically, we calculate this local exposure as follows. First, using the ACS, we identify individuals in 2014 who were not U.S. citizens and who were born outside the United States. Second, we restrict the sample to those enrolled in graduate programs in 2014 with a bachelor's degree.[14] Third, we retain only individuals whose field of study qualifies for the 2016 STEM OPT extension. Fourth, following Basso (2016), we exclude household dependents by keeping only those who lived alone or were not classified as dependents, and we restrict the sample further to individuals who arrived in the United States after age 16. Finally, we standardize the number of eligible students by the local working-age population in 2014 to obtain a comparable, location-level measure of potential OPT exposure.

Based on this measure, we construct the treatment and control groups. Many CZs have no eligible international students in 2014, and about 49% exhibit a positive exposure value. Because some CZs display only minimal exposure, we define treated CZs as those in the top 25% of the exposure distribution, resulting in 186 treated CZs with an average

[14]The majority of OPT recipients held graduate degrees (about 63% in 2004 to over 80% in 2017 (Neufeld, 2019)).

exposure of 0.2% (the share of eligible students out of the working-age population). Control CZs are defined as those with zero exposure, corresponding to 378 CZs. Thus, CZs with positive but below-threshold (25%) exposure are excluded. Since this classification of treatment and control status is somewhat arbitrary, we assess the robustness of our findings using an alternative definition of treatment. Specifically, we classify all CZs with a positive exposure value as treated, and all other CZs as controls, and then re-estimate the effects.

Figure 1 presents the geographic distribution of treated and control CZs. The hatched areas represent treated CZs, whereas the blue areas represent control CZs. Although the treated CZs include well-known innovative regions such as San Francisco and Boston, they are relatively evenly distributed across U.S. states. Because such areas may correlate with the presence of universities, we control for the number of native-born students in some specifications.[15]

Figure 1: Treated and Control CZs

Notes: This figure displays the geographic distribution of treated and control CZs used in the analysis. Treated CZs are defined as those in the top 25% of the 2014 graduate OPT-eligible ratio, and shown in red with diagonal hatching. Control CZs have a value of zero for the 2014 graduate OPT-eligible ratio, and are shown in blue. Gray areas denote CZs that had a positive OPT-eligible population share but did not fall within the top 25%, and are therefore excluded.

Next, in Table 1, we compare treated and control CZs in terms of their observable characteristics in 2015. Relative to control CZs, treated CZs are generally more populated, more immigrant-intensive, more highly educated, and have higher income levels, while other characteristics—such as the shares of males and whites—are relatively similar

[15]Other than the number of native-born students, we control for the number of native population and the predicted number of SBLs (Minton et al., 2024) in some specifications.

across the two groups. Although these systematic level differences raise concerns about potential differential trends between treated and control CZs, we show that trends in all outcome variables for the two groups track each other closely during the period 2010 to 2015, especially if we apply SDID method.

Table 1: Summary Statistics for Treated vs Control CZs

Panel A. Treated CZs					
Variable	Mean	SD	Min	Max	N
Population	709,320	1,457,768	2,434	12,605,711	186
Immigrant ratio	0.1030	0.0889	0.0172	0.5227	186
Male ratio	0.5046	0.0155	0.4768	0.5590	186
White ratio	0.8008	0.1235	0.3308	0.9489	186
Black ratio	0.0980	0.1132	0.0024	0.4882	186
Asian ratio	0.0298	0.0427	0.0030	0.3635	186
High-skilled ratio	0.2449	0.0722	0.1019	0.4593	186
Agriculture ratio	0.0236	0.0246	0.0013	0.1172	186
Employment rate	0.6784	0.0592	0.5229	0.8023	186
Median income	23,335	5,097	12,000	40,000	186
Panel B. Control CZs					
Variable	Mean	SD	Min	Max	N
Population	98,234	137,405	885	1,108,714	378
Immigrant ratio	0.0793	0.0604	0.0076	0.3881	378
Male ratio	0.5106	0.0171	0.4737	0.5567	378
White ratio	0.8420	0.1233	0.2903	0.9653	378
Black ratio	0.0680	0.1021	0.0011	0.5037	378
Asian ratio	0.0167	0.0343	0.0000	0.4413	378
High-skilled ratio	0.1845	0.0496	0.0944	0.3796	378
Agriculture ratio	0.0358	0.0258	0.0027	0.1128	378
Employment rate	0.6654	0.0776	0.4116	0.8336	378
Median income	21,867	5,047	8,500	36,000	378

Notes: Panel A presents summary statistics for the treated CZs, defined as those in the top 25% value of the 2014 graduate OPT eligible ratio. Panel B reports corresponding statistics for control CZs, which are CZs with a 0 value of the 2014 graduate OPT eligible ratio. All variables are measured using data from 2015, prior to the 2016 OPT extension, and therefore reflect baseline demographic and economic characteristics.

3.3 The OPT Extension and High-skilled Immigrants

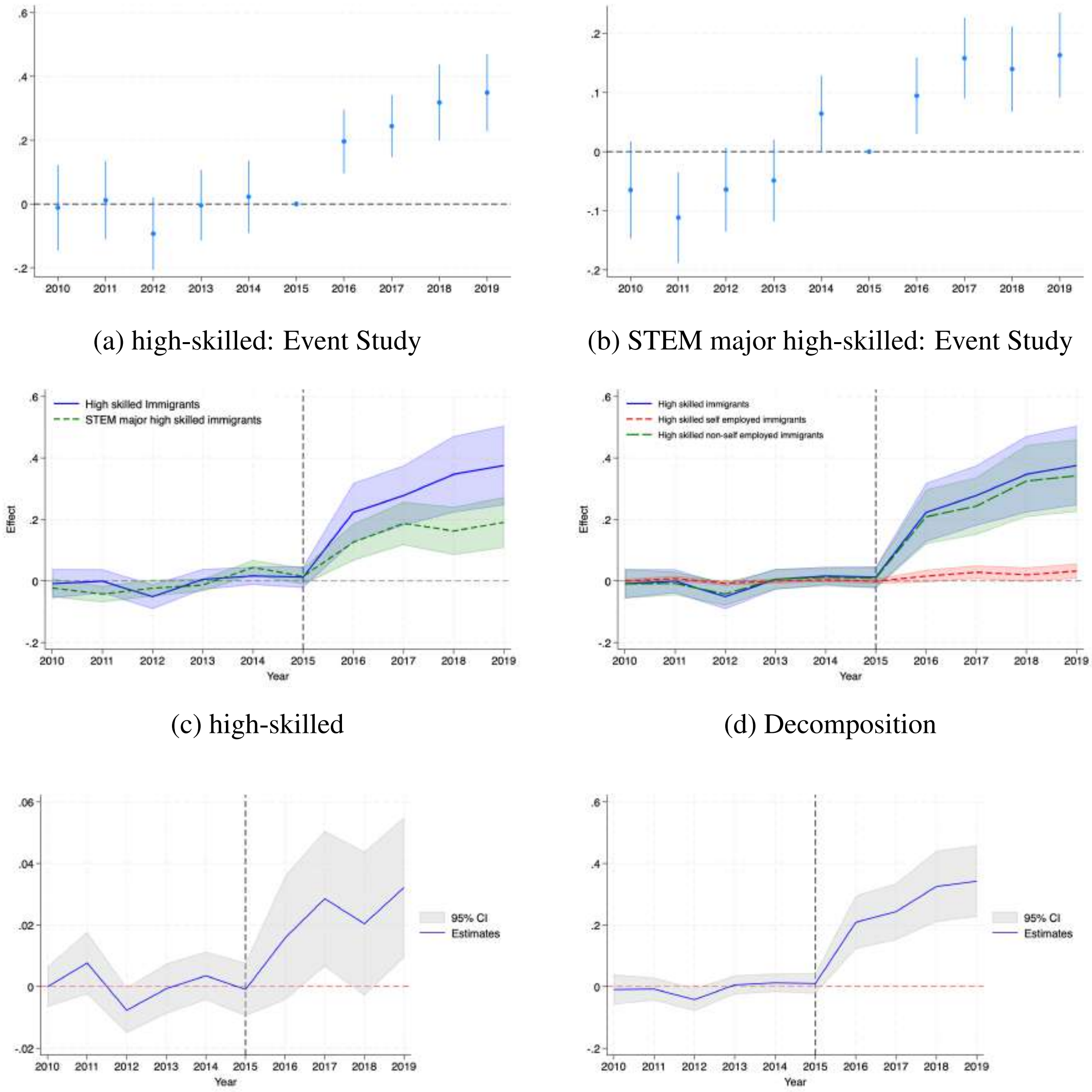
We begin our analysis by examining whether treated CZs experience an increase in high-skilled immigrants relative to control CZs. Our assignment of treated and control groups, based on the number of international students eligible for the STEM OPT extension, should predict a subsequent rise in high-skilled immigrants, particularly those with STEM majors. However, in an extreme case, it is also possible that no such increase occurs if affected immigrants relocate to different CZs to gain work experience, rather than remaining in the CZ where their universities are located. This is a plausible scenario, as the immigrants we study are skilled and thus highly geographically mobile (Malamud and Wozniak, 2012). Therefore, our first step in assessing the local economic impact of the OPT extension is to document that treated CZs indeed experience an increase in high-skilled immigrants.

We present the event-study estimates and the average SDID estimates in Figure 2 and Table 2, respectively. All figures report point estimates with 95% confidence intervals. Panels (a) and (b) of Figure 2 first show the event-study estimates from the standard DID model. These panels indicate an increase in high-skilled immigrants in treated CZs, particularly in STEM fields, but exhibit some pre-treatment trends that are not fully aligned with the control groups. To address these pre-existing differences, we rely on the event-study-style estimates from the SDID model, shown in Panels (c) through (f). As Panel (c) demonstrates, college-educated immigrants significantly increase in treated CZs relative to control CZs. Approximately half of this increase is driven by STEM-major immigrants, suggesting that the policy not only boosted the supply of STEM immigrants but also other skilled immigrants. This pattern is potentially explained by labor complementarity between workers with different majors or by labor-demand effects associated with firm creation. Regarding the composition of the inflow, Panels (d) and (e) show that self-employed immigrants account for less than 10% of the rise in college-educated immigrants, with the majority of the increase driven by wage workers (Panel (f)).

Quantifying the magnitude of this STEM-immigrant-driven labor supply shock is important for relating it to other local economic outcomes. Panel A of Table 2 shows that treated CZs, on average, experienced a 0.3% increase in college-educated immigrants relative to the 2014 working-age population. This inflow translates to an approximately 6% increase in the stock of high-skilled immigrants.[16] Panels B, C, and D report corresponding effects of 0.17%, 0.02%, and 0.28% for STEM-major immigrants, self-employed immigrants, and wage workers, respectively. While these estimates may appear modest in absolute terms (relative to the working-age population), they are highly statistically

[16]The 6% increase is calculated by dividing the estimated coefficient for the increase in college-educated immigrants (0.3%) by the initial share of high-skilled immigrants out of the working-age population in 2015 (5.1%).

Figure 2: Effect on the Number of High-skilled Immigrants



(a) high-skilled: Event Study

(b) STEM major high-skilled: Event Study

(c) high-skilled

(d) Decomposition

(e) Self employed

(f) Non-Self employed

Notes: In all subfigures, t indexes a calendar year. Subfigures 2(a) and 2(b) show event study results with controls, for outcomes of high-skilled immigrants and stem major high-skilled immigrants (individuals with degrees eligible for the 2016 OPT extension), each. Subfigures (c)-(f) plot the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. In subfigure 2(c), the outcome is the number of high-skilled immigrants and STEM-major high-skilled immigrants in year t, CZ i divided by the total population in CZ i in 2014. In subfigure 2(d), the outcome decomposes the total high-skilled immigrants into self-employed and non-self-employed groups; Subfigures 2(e) and 2(f) separately present each component. Total high-skilled immigrants and high-skilled non-self-employed immigrants include both employed and non-employed individuals. Coefficients are multiplied by 100 for readability. Treated CZs are those in the top 25% of the 2014 graduate OPT-eligible ratio distribution; control CZs have a value of zero for this measure. Regressions in panels (c)-(f) include CZ fixed effects, year fixed effects, and no additional controls.

significant and remain stable across specifications with and without controls.[17]

[17] Control variables include: (i) the native student, defined as the number of native-born graduate students in CZ i and year t, normalized by its 2014 population; (ii) the native population, defined as the total number of natives in CZ i and year t, normalized by its 2014 population; and (iii) the predicted SBL, constructed using a shift–share prediction following Minton et al. (2024).

Table 2: Effect on the High-skilled Immigrants

	Immigrants (per 2014 pop.) (1)	Immigrants (per 2014 pop.) (2)	Immigrants (per 2014 pop.) (3)	Immigrants (per 2014 pop.) (4)	Immigrants (per 2014 pop.) (5)
Panel A: Effect on high-skilled Immigrants					
Treated	0.306***	0.307***	0.304***	0.306***	0.305***
	(0.046)	(0.050)	(0.046)	(0.046)	(0.046)
Panel B: Effect on STEM major high-skilled Immigrants					
Treated	0.167***	0.168***	0.171***	0.167***	0.172***
	(0.030)	(0.029)	(0.029)	(0.029)	(0.030)
Panel C: Effect on high-skilled Self employed Immigrants					
Treated	0.024***	0.024***	0.024***	0.024***	0.024***
	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)
Panel D: Effect on high-skilled Non-Self employed Immigrants					
Treated	0.280***	0.281***	0.278***	0.280***	0.279***
	(0.042)	(0.043)	(0.045)	(0.043)	(0.043)
Controls:					
Native student		✓			✓
Native population			✓		✓
Predicted SBL				✓	✓
Observations	5,640	5,640	5,640	5,640	5,640

Notes: See Equation (1) for specification. The dependent variable is measured as the number of high-skilled immigrants in CZ i and year t, normalized by the 2014 population of CZ i. Coefficients are multiplied by 100 for readability. Treated CZs are defined as those in the top 25% of the 2014 graduate OPT-eligible ratio, while control CZs have a value of zero. STEM major high-skilled immigrants correspond to individuals with STEM degrees eligible for the 2016 OPT extension. Total high-skilled immigrants, STEM major high-skilled immigrants, and high-skilled non-self-employed immigrants include both employed and non-employed individuals. Panels A-D report effects on total high-skilled immigrants, STEM-major high-skilled immigrants, high-skilled self-employed immigrants, and high-skilled non-self-employed immigrants, respectively. Columns differ by the inclusion of control variables. Control variables include: (i) the native student, defined as the number of native-born graduate students in CZ i and year t, normalized by its 2014 population; (ii) the native population, defined as the total number of natives in CZ i and year t, normalized by its 2014 population; and (iii) the predicted SBL, constructed using a shift–share prediction following Minton et al. (2024). All regressions include CZ fixed effects and year fixed effects. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

4 Local Economic Impacts of the OPT Extension

The previous section established that the OPT extension led to a significant increase in the local supply of skilled immigrants in treated CZs. Building on this "first-stage" result, this section investigates the subsequent impact of this local skilled labor supply shock on the number of firms, native employment and wages, patent activity, and proxies of start-up formation like small business loans.

4.1 Firms

As before, we estimate the effect of the OPT extension by comparing our treated and our control group of CZs, based on our synthetic difference-in-difference strategy. We start by presenting graphical results on firm creation, which has been identified in prior literature as a key force that helps absorb high-skilled immigrant inflows.

Figure 3 presents event-study style graphs showing the effect of the OPT extension on the number of firms, disaggregated by firm size and by industry. Panel (a) displays the event-study estimates for the total number of firms using the standard DID model, which indicates a positive post-policy impact with no significant pre-trends. Panels (b) through (e) show the SDID event-study results broken down by firm size categories (less than 20, 20-99, 100-499, and more than 500 employees).[18] Crucially, all these graphs confirm that there is no evidence of differential pre-trends leading up to the policy change in 2015.

Following the OPT extension, the number of firms consistently increased in treated CZs relative to control CZs across the firm size distribution. The effect is particularly pronounced among small to medium-sized firms (less than 100 employees) and large firms (more than 500 employees) – although this latter effect disappears once we include additional controls. These results align with evidence that immigrant students attending U.S. colleges are disproportionately employed either in large firms—especially multinationals—or in small firms (Dillon et al., 2025). This suggests that high-skilled immigrants are disproportionately important for both newly created or expanding startups and, perhaps, large, consolidated enterprises.

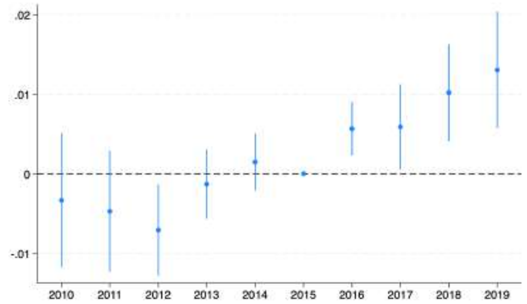
[18] The SDID event-study estimates for total firms, along with the comparative effects across firm sizes, are reported in Appendix Figure B.1.

Panel (f) shows the differential effects in STEM and non-STEM industries. Note that for this analysis, we use the number of establishments rather than firms, as detailed industry classification data is available only at the establishment level.[19] While both groups exhibit an increase in the number of establishments post-2015, the magnitude of the effect is substantially larger and more pronounced in STEM industries. This finding is consistent with the fact that the positive supply shock directly targets international STEM graduates. However, non-STEM establishments also experienced a significant increase, likely driven by positive demand spillovers from skilled immigrants and production complementarities between sectors.

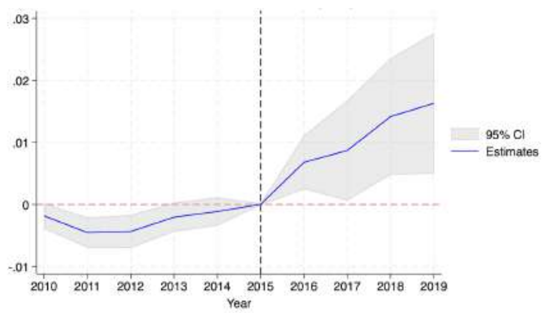
Table 3 quantifies firm creation effects and demonstrates the robustness of our findings across various specifications. The magnitude of the effect is substantial: we estimate a 1% increase in the total number of firms, coming mostly from small and medium-sized enterprises (less than 100 employees). The estimated elasticity of firm formation relative to the immigrant labor supply shock is 0.1 to 0.2. This magnitude implies that the impact on firm entry extends beyond the direct hiring of immigrants; rather, the shock appears to have encouraged firm formation by boosting demand for other productive factors, such as native workers.

[19]The STEM industry is defined as the 11 NAICS 4-digit industries based on the core concentration industries from Uhlenkott et al. (2014), which indicates industries with at least 5 times more than average concentration in STEM occupations.

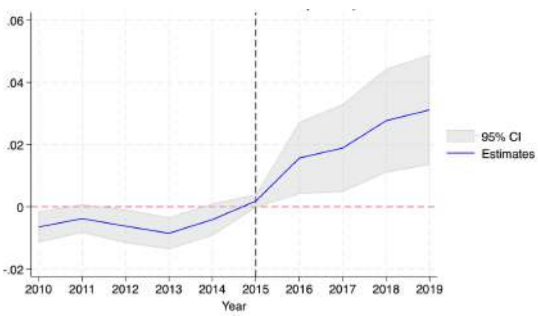
Figure 3: The Effect of High-Skilled Migration on Firm Creation



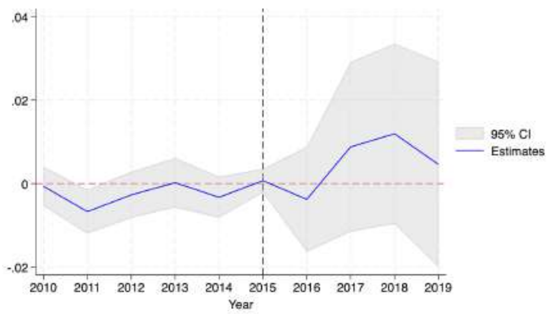
(a) Total: Event Study



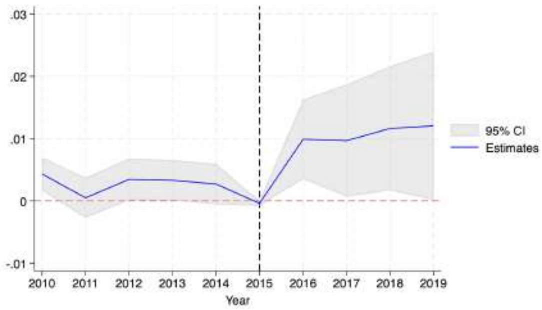
(b) Less than 20 employees



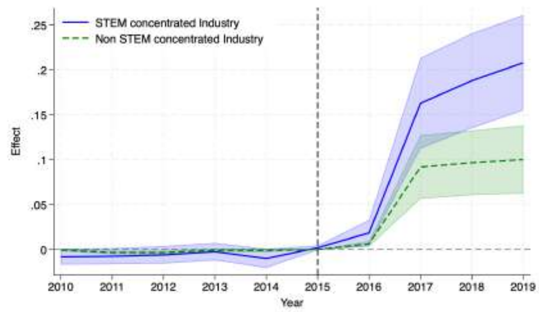
(c) 20–99 employees



(d) 100–499 employees



(e) Over 500 employees



(f) STEM vs Non-STEM establishments: Total

Notes: In all subfigures, t indexes a calendar year. Subfigure 3(a) shows event study results with controls for the total number of firms. Subfigures (b)-(f) plot the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. The outcomes in subfigures (b)-(e) correspond to the log number of firms in CZ i in year t by employment size: firms with fewer than 20 employees in Subfigure 3(b), firms with 20–99 employees in Subfigure 3(c), firms with 100–499 employees in Subfigure 3(d), and firms with 500 or more employees in Subfigure 3(e). The outcome for subfigure 3(f) is the log total number of establishments in CZ i, year t decomposed by STEM concentrated industry and non-STEM concentrated industry. The STEM concentrated industry is defined as the 11 NAICS 4-digit industries based on the core concentration industries from Uhlenkott et al. (2014), which indicates industries with at least 5 times more than average concentration in STEM occupations. Treated CZs are those in the top 25% of the 2014 graduate OPT-eligible ratio distribution; control CZs have a value of zero for this measure. Regressions in panels (b)-(f) include CZ fixed effects, year fixed effects, and no additional controls.

Table 3: Effect on the Number of Firms

	Log (Firm) (1)	Log (Firm) (2)	Log (Firm) (4)	Log (Firm) (3)	Log (Firm) (5)
Panel A: Effect on total Firms					
Treated	0.010***	0.010***	0.009***	0.010***	0.009***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Elasticity	0.17	0.17	0.15	0.17	0.15
Observations	5,640	5,640	5,640	5,640	5,640
Panel B: Effect on Firms with less than 20 employees					
Treated	0.012***	0.011***	0.010***	0.012***	0.010***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)
Elasticity	0.20	0.18	0.17	0.20	0.17
Observations	5,640	5,640	5,640	5,640	5,640
Panel C: Effect on Firms with 20-99 employees					
Treated	0.023***	0.023***	0.016***	0.023***	0.016***
	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)
Elasticity	0.38	0.38	0.27	0.38	0.27
Observations	5,590	5,590	5,590	5,590	5,590
Panel D: Effect on Firms with 100-499 employees					
Treated	0.005	0.006	-0.000	0.005	-0.000
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Elasticity	0.08	0.10	0.00	0.08	0.00
Observations	5,540	5,540	5,540	5,540	5,540
Panel E: Effect on Firms with over 500 employees					
Treated	0.011**	0.011**	0.007	0.011**	0.007
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
Elasticity	0.18	0.18	0.12	0.18	0.12
Observations	5,600	5,600	5,600	5,600	5,600
Controls:					
Native student		✓			✓
Native population			✓		✓
Predicted SBL				✓	✓

Notes: See Equation (1) for specification. The dependent variable is measured as the log number of firms in CZ i and year t. Treated CZs are defined as those in the top 25% of the 2014 graduate OPT-eligible ratio, while control CZs have a value of zero. Panels A-E report effects on the number of total firms, firms with less than 20 employees, firms with 20-99 employees, firms with 100-499 employees, and firms with over 500 employees, respectively. Columns differ by the inclusion of control variables. Control variables include: (i) the native student, defined as the number of native-born graduate students in CZ i and year t, normalized by its 2014 population; (ii) the native population, defined as the total number of natives in CZ i and year t, normalized by its 2014 population; and (iii) the predicted SBL, constructed using a shift–share prediction following Minton et al. (2024). All regressions include CZ fixed effects and year fixed effects. *** p<0.01, ** p<0.05, * p<0.1

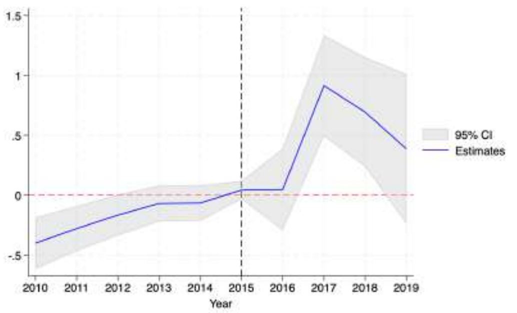
4.2 Employment and Wage Effects

Whether an immigrant-induced labor supply shock increases or decreases the demand for native labor depends on two opposing forces: substitution effects and scale effects. On the one hand, an increase in immigrant labor makes that factor relatively more abundant and, potentially, cheaper. This leads firms to substitute towards immigrant labor and, consequently, away from native labor and other factors of production. On the other hand, the abundance of immigrant labor can encourage new firm formation and allow existing firms to grow, as seen empirically in the previous section. This second force results in an increase in the overall demand for all factors of production, including native labor. If the scale effect dominates the substitution effect, we would expect to see an increase in native employment and wages.

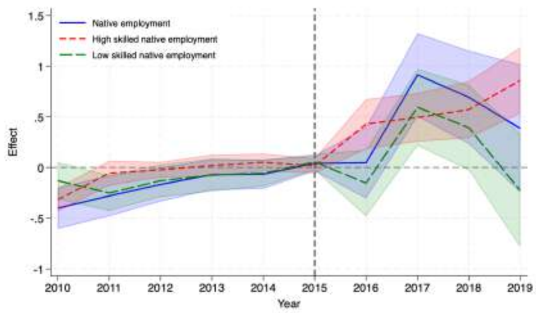
Panel (a) of Figure 4 confirms that total native employment did increase in treatment relative to control CZs. Panel (b) of the same figure shows the decomposition of the employment effects between high- and low-skilled native workers. In principle, scale effects should affect all workers relatively equally, while competition (substitution) effects should be stronger among factors of production most similar to immigrants. These graphs show a pronounced and strengthening positive effect on native high-skilled workers, particularly towards the end of the analysis period. In contrast, the effects on native low-skilled workers appear positive initially but diminish over time. These results strongly suggest that high-skilled immigrants act as complements to high-skilled native workers, implying mutual benefit and positive spillovers. Panel (c) shows that the employment effects among high-skilled natives are positive both for younger (below 40 years old) and older workers.

Panel (d) of Figure 4 further decomposes the native employment effect by industry type (STEM and non-STEM). The effects on both groups are positive and significant, but their temporal patterns differ. Similar to the impact on high-skilled natives, the positive effect on native STEM workers gradually increases over time. This trend is consistent with the progressive, long-term expansion of STEM-intensive firms and sectors driven by the immigrant shock. Conversely, the positive impact on non-STEM native workers diminishes after 2018. This decline reinforces the explanation that the local industrial structure shifts toward high-skilled intensive industries, proportionally reducing the relative demand for non-STEM labor over time.

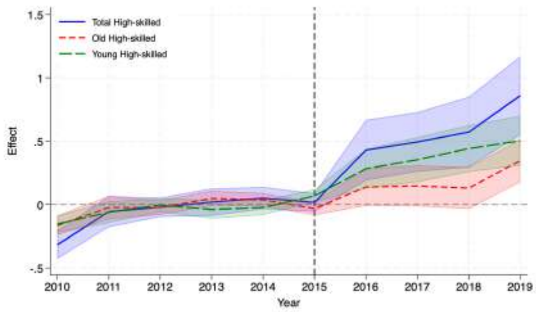
Figure 4: Effect on the Native Employment



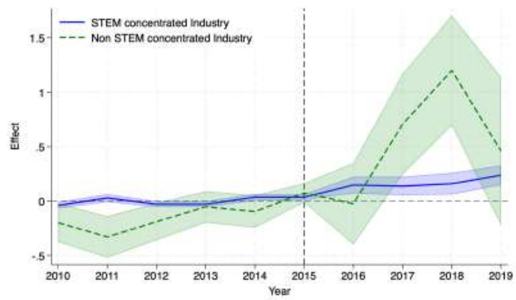
(a) Total



(b) Employment Decomposition



(c) High-skilled Employment Decomposition



(d) STEM vs Non-STEM

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. In subfigure 4(a), the outcome is the total number of natives who are employed in year t CZ i, normalized by the 2014 population of CZ i. Subfigure 4(b) decomposes the native employment into high- and low-skilled. For subfigure 4(c), it decomposes the native high-skilled employment into old and young based on the age of 40. The outcome for subfigure 4(d) is the number of natives who are employed in year t CZ i, normalized by the 2014 population of CZ i, decomposed to STEM industry and non-STEM industry. STEM industry is defined as the 11 NAICS 4-digit industries based on the core concentration industries from Uhlenkott et al. (2014), which indicates industries with at least 5 times more than average concentration in STEM occupations. Native employment is defined as wage employment, excluding self-employed individuals. Coefficients are multiplied by 100 for readability. Treated CZs are those in the top 25% of the 2014 graduate OPT-eligible ratio distribution; control CZs have a value of zero for this measure. Regressions in all panels include CZ fixed effects, year fixed effects, and no additional controls.

Table 4 quantifies the employment effects first uncovered in Figure 4. The results highlight a sharp divergence across skill groups. We estimate a positive, yet small and statistically indistinguishable from zero, elasticity of low-skilled native employment to the high-skilled immigrant inflows. In contrast, we find a large elasticity of high-skilled native employment to these immigrant inflows that exceeds one. This large elasticity is consistent with the aforementioned complementarities between immigrant and native high-skilled workers, and is consistent with the large effects on firm creation documented in the previous section. Furthermore, the positive employment effects on native workers are statistically significant for both younger and older worker (Panels D and E), and for STEM and non-STEM native workers (Panels F and G). These findings are stable and robust to the inclusion of various controls.[20]

[20]Note that in this table we do not include a column which controls for native population since this is related to the dependent variable, and would be a bad control.

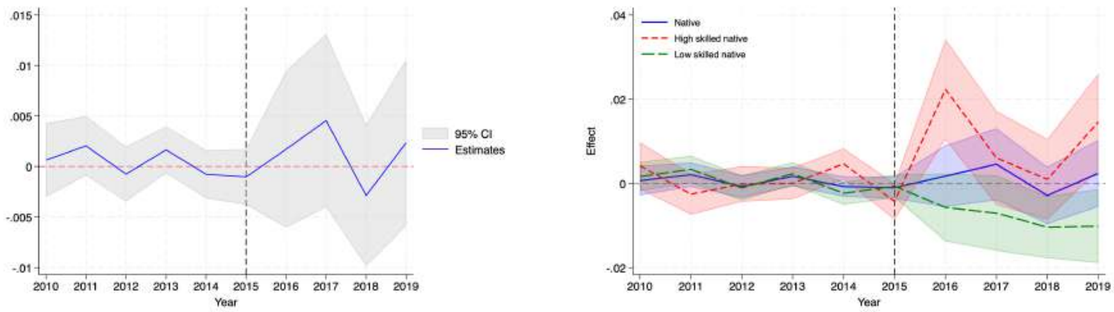
Table 4: Effect on the Native Employment

	Natives (per 2014 pop.) (1)	Natives (per 2014 pop.) (2)	Natives (per 2014 pop.) (3)
Panel A: Effect on Native Employment			
Treated	0.510***	0.448**	0.508**
	(0.193)	(0.192)	(0.198)
Elasticity	1.67	1.46	1.66
Panel B: Effect on High-skilled Native Employment			
Treated	0.589***	0.491***	0.589***
	(0.106)	(0.098)	(0.105)
Elasticity	1.93	1.60	1.93
Panel C: Effect on Low-skilled Native Employment			
Treated	0.152	0.178	0.151
	(0.161)	(0.162)	(0.160)
Elasticity	0.50	0.58	0.49
Panel D: Effect on High-skilled Old Native Employment			
Treated	0.190***	0.169***	0.190***
	(0.060)	(0.059)	(0.060)
Elasticity	0.62	0.55	0.62
Panel E: Effect on High-skilled Young Native Employment			
Treated	0.395***	0.354***	0.397***
	(0.067)	(0.063)	(0.068)
Elasticity	1.29	1.15	1.30
Panel F: Effect on STEM Industry Native Employment			
Treated	0.170***	0.167***	0.170***
	(0.036)	(0.036)	(0.037)
Elasticity	0.56	0.54	0.56
Panel G: Effect on Non-STEM Industry Native Employment			
Treated	0.585***	0.525**	0.583***
	(0.200)	(0.209)	(0.214)
Elasticity	1.91	1.71	1.91
Controls:			
Native student		✓	
Predicted SBL			✓
Observations	5,640	5,640	5,640

Notes: See Equation (1) for specification. The dependent variable is measured as the number of employed natives in CZ i and year t, normalized by the 2014 population of CZ i. Native employment includes only wage workers and excludes those who are self-employed. Coefficients are multiplied by 100 for readability. Treated CZs are defined as those in the top 25% of the 2014 graduate OPT-eligible ratio, while control CZs have a value of zero. Panels A-G report effects on total native employment, high-skilled native employment, low-skilled native employment, high-skilled old native employment, high-skilled young native employment, STEM concentrated industry native employment, and non-stem concentrated industry native employment, respectively. Columns differ by the inclusion of control variables. Control variables include: (i) the native student, defined as the number of native-born graduate students in CZ i and year t, normalized by its 2014 population; and (ii) the predicted SBL, constructed using a shift–share prediction following Minton et al. (2024). All regressions include CZ fixed effects and year fixed effects. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Figure 5 explores the effects of the expansion of high-skilled immigrants on native hourly wages.[21] Consistent with the interpretation that the demand for high-skilled natives expanded as a result of the high-skilled immigrant shock, high-skilled native wages initially increased, especially immediately on impact. However, this initial premium subsequently diminished as native high-skilled employment expanded, suggesting that the local labor supply response eventually began to dampen the initial wage effects. On the other hand, the wages of low-skilled native workers consistently decreased. This divergent pattern is consistent with a structural shift in the local economy towards high-skilled intensive sectors. The influx of skilled immigrants, particularly those in STEM fields, may have accelerated the growth of high-skilled intensive industries within the CZs.

Figure 5: Effect on the Native Log Median Hourly Wage



(a) Total

(b) High- and Low-skilled

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. In subfigure 5(a), the outcome is the weighted log median hourly wage for total employed natives, except for those who are self-employed, in year t CZ i. In subfigure 5(b), the outcome compares the weighted log median hourly wage for total native workers with the weighted log median hourly wage for high-skilled and low-skilled native workers. Treated CZs are those in the top 25% of the 2014 graduate OPT-eligible ratio distribution; control CZs have a value of zero for this measure. Regressions in all panels include CZ fixed effects, year fixed effects, and no additional controls.

Table 5 quantifies the hourly wage effects.[22] We estimate that the wages of native high-skilled workers increased by around 1% when comparing treatment and control CZs. The magnitude of this wage increase provides a useful contrast to the native employment results: Native employment increased by around 0.5%, while wages increased by 1%. This suggests an implied native labor supply elasticity of about 2, which is in line with other estimates found in the literature. Consistent with the temporal analysis in Figure 5, the wage effects for low-skilled native workers are negative, which amounts to a decline

[21] Effect on the native weekly wages can be found in Appendix figure B.2.

[22] Weekly wage effects can be found in Appendix Table C.3.

of about 0.8%. Finally, the positive wage impacts on high-skilled natives are particularly pronounced among younger workers, whereas the effects on older workers are statistically insignificant.

Table 5: Effect on the Natives' Log Median Hourly Wage

	Log (Wage) (1)	Log (Wage) (2)	Log (Wage) (3)	Log (Wage) (4)	Log (Wage) (5)
Panel A: Effect on Native Log Median Hourly Wage					
Treated	0.001	0.001	0.001	0.001	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Elasticity	0.02	0.02	0.02	0.02	0.03
Panel B: Effect on High-skilled Native Log Median Hourly Wage					
Treated	0.011***	0.012***	0.010***	0.011***	0.011***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Elasticity	0.18	0.20	0.17	0.18	0.18
Panel C: Effect on Low-skilled Native Log Median Hourly Wage					
Treated	-0.008***	-0.008**	-0.008***	-0.008***	-0.008***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Elasticity	-0.13	-0.13	-0.13	-0.13	-0.13
Panel D: Effect on High-skilled Old Native Log Median Hourly Wage					
Treated	0.004	0.003	0.002	0.003	0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Elasticity	0.07	0.05	0.03	0.05	0.02
Panel E: Effect on High-skilled Young Native Log Median Hourly Wage					
Treated	0.020***	0.020***	0.019***	0.020***	0.019***
	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)
Elasticity	0.33	0.33	0.32	0.33	0.32
Controls:					
Native student		✓			✓
Native population			✓		✓
Predicted SBL				✓	✓
Observations	5,640	5,640	5,640	5,640	5,640

Notes: See Equation (1) for specification. The dependent variable is measured as the weighted log median hourly wage in CZ i and year t. Treated CZs are defined as those in the top 25% of the 2014 graduate OPT-eligible ratio, while control CZs have a value of zero. Panels A-E report effects on the total natives' log median hourly wage, high-skilled natives' log median hourly wage, low-skilled natives' log median hourly wage, high-skilled old natives' log median hourly wage, and high-skilled young natives' log median hourly wage, respectively. Columns differ by the inclusion of control variables. Control variables include: (i) the native student, defined as the number of native-born graduate students in CZ i and year t, normalized by its 2014 population; (ii) the native population, defined as the total number of natives in CZ i and year t, normalized by its 2014 population; and (iii) the predicted SBL, constructed using a shift–share prediction following Minton et al. (2024). All regressions include CZ fixed effects and year fixed effects. *** p<0.01, ** p<0.05, * p<0.1

4.3 Small Business Loans

The evidence discussed in the previous subsections clearly indicates that the immigrant high-skilled labor supply shock facilitated firm creation and expansion, leading to a consequent increase in the demand for native labor, particularly high-skilled labor. A fundamental prerequisite for business growth is access to credit. Firms routinely require loans to capitalize on existing opportunities, expand operations, hire workers, and build up capital. Ideally, we would directly examine physical capital accumulation.[23] However, such analysis is limited by the fact that standard firm-level data assigns capital investment to headquarters locations, which obscures local investment dynamics in the specific branch locations or CZs we analyze. Therefore, in this subsection, we focus on whether the amount of loans—a more geographically precise measure of local credit access—increased in response to high-skilled immigration.

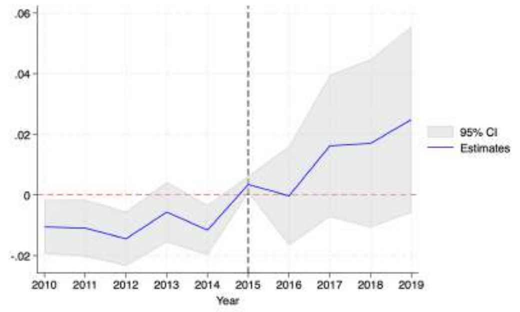
Figure 6 presents the results for SBLs. Panels (a) and (b) show that the number of SBLs increased substantially in treated CZs relative to control CZs following the policy change, although the aggregate amount of SBLs does not show a significant change. While these estimates are generally noisier compared to the firm or employment effects, both the number and amount of SBLs exhibit no systematic differential pre-trends leading up to 2015. This observed increase in the number of SBLs is consistent with the documented rise in self-employed immigrants (Table 2) and the growth of small firms (Table 3).

In Panels (c) and (d) of Figure 6, we explore the heterogeneous effects according to the type of banks: large banks versus community banks. Although the estimates in this subanalysis show considerable noise in both the pre- and post-periods, we find a statistically significant effect on the number of SBLs originating from large banks. Conversely, the amount of SBLs increased slightly in community banks. This heterogeneity may reflect differences in the loan borrowing behavior of immigrants across various bank types, as well as distinct types or reasons for firm borrowing across these channels.

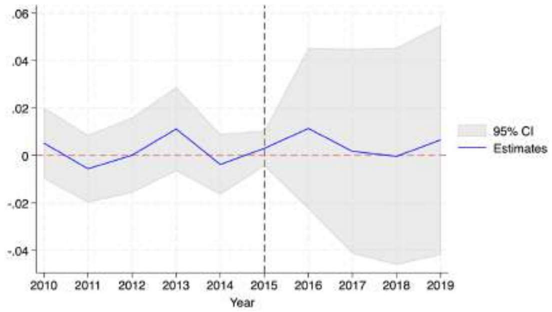
Table 6 provides estimates on the effect of high-skilled migration on loans and deposits. We estimate an increase in the number of SBLs of about 1% when comparing

[23] In the Appendix, we analyze Capital Expenditure and Net Property, Plant, and Equipment, but find no statistically significant increase in these measures.

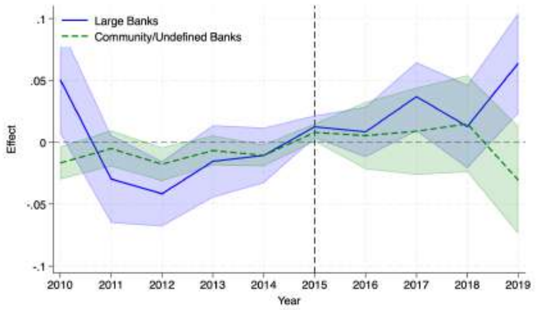
Figure 6: Effect on SBLs



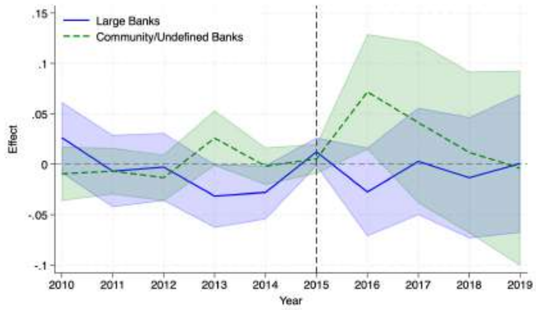
(a) Number



(b) Amount



(c) Number: by Bank Size



(d) Amount: by Bank Size

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. In subfigure 6(a), the outcome is the log number of SBLs of year t, CZ i. In subfigure 6(b), the outcome is the log amount of SBLs of year t, CZ i. Subfigures 6(c) and 6(d) decompose the effect by bank size for the outcomes number of SBLs and amount of SBLs, each. Based on the information from the Federal Reserve, we defined large banks as those with assets greater than 10 billion, community banks as those with less than 10 billion in assets, and undefined banks as those with no asset information. Treated CZs are those in the top 25% of the 2014 graduate OPT-eligible ratio distribution; control CZs have a value of zero for this measure. Regressions in all panels include CZ fixed effects, year fixed effects, and no additional controls.

treated and control CZs. While these estimates are not statistically significant, they are robust and stable across a number of specifications and controls. In contrast, we obtain a very small and insignificant estimate on the amount of SBLs, indicating a null effect on the total value. This suggests that the increase in the number of SBLs is likely driven by the formation of small firms, which require a greater number of individual, smaller loans rather than a few large loans. Finally, we do not find any effect on the amount of deposits, implying that the positive effects on SBLs, if any, are not driven by the increase in the local deposit base.[24]

[24]The SDID Figure for deposit amount is presented in Appendix figure B.3.

Table 6: Effect on the Loans/Deposits

Panel A: Effect on the Number of SBLs					
	Log (SBL) (#) (1)	Log (SBL) (#) (2)	Log (SBL) (#) (3)	Log (SBL) (#) (4)	Log (SBL) (#) (5)
Treated	0.014	0.014	0.010	0.015	0.010
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Elasticity	0.23	0.23	0.17	0.25	0.17
Observations	5,640	5,640	5,640	5,640	5,640
Panel B: Effect on the Amount of SBLs					
	Log (SBL) (amt) (1)	Log (SBL) (amt) (2)	Log (SBL) (amt) (4)	Log (SBL) (amt) (3)	Log (SBL) (amt) (5)
Treated	0.005	0.004	-0.005	0.005	-0.005
	(0.018)	(0.020)	(0.019)	(0.020)	(0.020)
Elasticity	0.08	0.07	-0.08	0.08	-0.08
Observations	5,640	5,640	5,640	5,640	5,640
Panel C: Effect on the Amount of Deposits					
	Log (Deposit) (amt) (1)	Log (Deposit) (amt) (2)	Log (Deposit) (amt) (4)	Log (Deposit) (amt) (3)	Log (Deposit) (amt) (5)
Treated	0.009	0.008	-0.005	0.007	-0.006
	(0.017)	(0.017)	(0.018)	(0.018)	(0.017)
Elasticity	0.15	0.13	-0.08	0.12	-0.10
Observations	5,100	5,100	5,100	5,100	5,100
Controls:					
Native student		✓			✓
Native population			✓		✓
Predicted SBL				✓	✓

Notes: See Equation (1) for specification. The dependent variable in Panel A is the log number of SBLs in CZ i and year t. Panel B reports results for the log amount of SBLs, and Panel C reports results for the log amount of deposits, each measured at the CZ–year level. Treated CZs are defined as those in the top 25% of the 2014 graduate OPT-eligible ratio, while control CZs have a value of zero. Columns differ by the inclusion of control variables. Control variables include: (i) the native student, defined as the number of native-born graduate students in CZ i and year t, normalized by its 2014 population; (ii) the native population, defined as the total number of natives in CZ i and year t, normalized by its 2014 population; and (iii) the predicted SBL, constructed using a shift–share prediction following Minton et al. (2024). All regressions include CZ fixed effects and year fixed effects. *** p<0.01, ** p<0.05, * p<0.1

4.4 Innovation Effects

Having established that the OPT extension significantly contributed to the growth of the local STEM workforce and firm scale, we next investigate whether these labor supply shocks generated positive spillovers into knowledge creation. Theoretically, the 2016 OPT extension expanded the supply of high-skilled STEM labor, which is traditionally associated with higher rates of innovation; however, because the policy targets early-career graduates who may not yet have accumulated the experience required for patentable inventions, it remains an empirical question whether this specific labor supply shock translates into an immediate, measurable increase in aggregate patenting activity.

To examine the impact of the OPT extension on innovation, we follow Bernstein et al. (2022). Specifically, we construct two variables: 1) the number of patents; 2) the number of adjusted citations of patents. Since patent approval typically takes several years, we use the application year rather than the grant year for our analysis. That is, we focus on patents that (i) were applied for between 2010 and 2019, (ii) have been granted as of the most recent data (through 2024), and (iii) involve inventors whose current location is the U.S. The geographic assignment of a patent is based on the inventor's location, rather than the assignee firm's location. The main reason for this is that using firm location would mechanically assign all patents to headquarters locations, which would obscure local innovation effects when patents are generated at branch locations.

We construct the number of patents and the number of adjusted citations of patents by CZ and application year by allocating each patent number and citations equally across its inventors, [25] aggregating to the CZ-year level. We then construct the number of adjusted citations by focusing on citations accrued within three years after the patent grant year. We normalize each patent's three-year citation count by dividing by the average three-year citation number within its first WIPO field, [26] and the year the patent was granted.

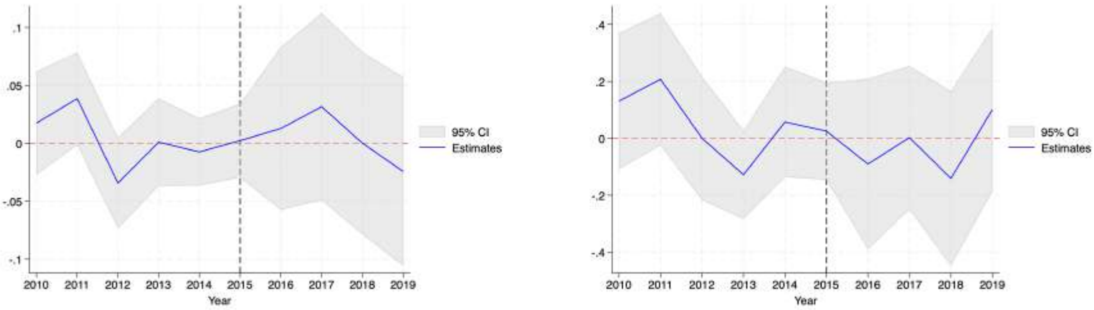
Results are shown in Figure 7 and Table 7. It is clear from both the figure and the table that the 2016 OPT extension and the subsequent increase in high-skilled immigrant and native work did not lead to a significant increase in local patent activity on average. There are many reasons why this result seems to differ from Hunt and Gauthier-Loiselle

[25] 1/n per inventor for a patent with n inventors.

[26] including NA categories

(2010), which is perhaps the most comparable piece of research in the existing literature. First, we focus on CZs, rather than states. Second, we use yearly variation, rather than ten year horizons. Third, their variation comes from a shift-share strategy that allocates immigrant inflows from various countries to the 1940 state distribution.[27] In contrast, we focus on young, U.S.-trained immigrants around 2016. While this influx induces firm growth and complements native labor, its impact on innovation is likely concentrated in specific high-tech domains, and thus may not be visible in aggregate patent indices.

Figure 7: Effect on Patents



(a) Number of Patents

(b) Adjusted Citations

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. In subfigure 7(a), the outcome is the log number of patents applied in year t, CZ i. In subfigure 7(b), the outcome is the log number of adjusted citations of patents applied in year t, CZ i. Treated CZs are those in the top 25% of the 2014 graduate OPT-eligible ratio distribution; control CZs have a value of zero for this measure. Regressions in all panels include CZ fixed effects and year fixed effects.

However, the aggregate null result masks significant heterogeneity. Given that the 2016 OPT extension was explicitly designed for STEM occupations, one would expect its impact to be concentrated in high-tech sectors rather than distributed evenly across the sectors. Table 8 confirms this hypothesis. Decomposing patent activity by WIPO sector reveals that treated CZs experienced notable gains in two distinctively high-tech categories: Electrical Engineering and Instruments. These sectors likely include the very frontiers of the digital and scientific economy—spanning semiconductors, digital communication, and IT methods (under Electrical Engineering) to medical technology and optics (under Instruments). These results suggest that the labor supply shock did indeed fuel innovation, but precisely within the tech-intensive boundaries where STEM graduates are most active.

[27]Other studies concentrate on the effects of star scientists fleeing particular countries, such as Nazi-

Table 7: Effect on Patents

Panel A: Effect on Number of Patents					
	Log (Patent) (1)	Log (Patent) (2)	Log (Patent) (3)	Log (Patent) (4)	Log (Patent) (5)
Treated	0.005 (0.030)	0.004 (0.030)	-0.010 (0.029)	0.005 (0.028)	-0.010 (0.029)
Elasticity	0.08	0.07	-0.17	0.08	-0.17
Observations	4,320	4,320	4,320	4,320	4,320
Panel B: Effect on Number of Adjusted Citations					
	Log (Adjusted Citation) (1)	Log (Adjusted Citation) (2)	Log (Adjusted Citation) (3)	Log (Adjusted Citation) (4)	Log (Adjusted Citation) (5)
Treated	-0.032 (0.113)	-0.040 (0.112)	-0.036 (0.111)	-0.032 (0.108)	-0.043 (0.113)
Elasticity	-0.53	-0.67	-0.60	-0.53	-0.72
Observations	1,470	1,470	1,470	1,470	1,470
Controls:					
Native student		✓			✓
Native population			✓		✓
Predicted SBL				✓	✓

Notes: See Equation (1) for specification. The dependent variable in Panel A is the log number of patents applied in year t, CZ i. Panel B reports results for the log number of adjusted citations of patents applied in year t, CZ i. Treated CZs are defined as those in the top 25% of the 2014 graduate OPT-eligible ratio, while control CZs have a value of zero. Columns differ by the inclusion of control variables. Control variables include: (i) the native student, defined as the number of native-born graduate students in CZ i and year t, normalized by its 2014 population; (ii) the native population, defined as the total number of natives in CZ i and year t, normalized by its 2014 population; and (iii) the predicted SBL, constructed using a shift–share prediction following Minton et al. (2024). All regressions include CZ fixed effects and year fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Germany or Russia in the early 1990s (Moser et al., 2014; Borjas and Doran, 2012).

Table 8: Effect on the number of Patents by WIPO Sector

	Log (Patent)					
Sector:	Chemistry	Electrical engineering	Instruments	Mechanical engineering	Other fields	NA
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: No controls included						
Treated	0.004	0.096**	0.116**	-0.024	0.106*	0.075
	(0.051)	(0.047)	(0.052)	(0.039)	(0.059)	(0.060)
Elasticity	0.07	1.60	1.93	-0.40	1.77	1.25
Panel B: Full controls included						
Treated	-0.002	0.086*	0.112**	-0.030	0.095	0.061
	(0.051)	(0.049)	(0.050)	(0.038)	(0.058)	(0.060)
Elasticity	-0.03	1.43	1.87	-0.50	1.58	1.02
Observations	2,760	2,530	2,460	3,140	2,260	2,240
Controls:						
Native student	✓	✓	✓	✓	✓	✓
Native population	✓	✓	✓	✓	✓	✓
Predicted SBL	✓	✓	✓	✓	✓	✓

Notes: See Equation (1) for specification. The dependent variable is the log number of patents applied in year t, CZ i for each WIPO sector. Panel A reports the results with no control variables, and Panel B reports the results with full controls. Treated CZs are defined as those in the top 25% of the 2014 graduate OPT-eligible ratio, while control CZs have a value of zero. Each column indicates the results for each WIPO sector. Control variables include: (i) the native student, defined as the number of native-born graduate students in CZ i and year t, normalized by its 2014 population; (ii) the native population, defined as the total number of natives in CZ i and year t, normalized by its 2014 population; and (iii) the predicted SBL, constructed using a shift–share prediction following Minton et al. (2024). All regressions include CZ fixed effects and year fixed effects. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

4.5 Star University Effect

It is often emphasized that star universities play a crucial role in attracting foreign talent. Hence, it is worth exploring whether the effects we uncover are driven by these types of universities. In what follows, control CZs are the same as before, however we distinguish the types of treated CZs. Based on the US News' Best National Universities Rankings,[28] we define define CZs with the top 20 universities[29] as CZs with a star university. This allows us to have two treated groups: CZs with and without a star university, which we compare to the same pool of control CZs.

Table 9 shows the main results.[30] Across columns, when we compare Panel A (which includes only Star University CZs) and Panel B (Non-star University CZs) we see that effects tend to be larger in Panel A, consistent with the idea that STEM immigrant workers graduating from highly ranked universities likely have a larger impact on local economies.

[28]See US News

[29]Universities included: Princeton University, Massachusetts Institute of Technology, Harvard University, Stanford University, Yale University, University of Chicago, Duke University, Johns Hopkins University, Northwestern University, University of Pennsylvania, California Institute of Technology, Cornell University, Brown University, Dartmouth College, Columbia University, University of California, Berkeley, Rice University, University of California, Los Angeles, Vanderbilt University, Carnegie Mellon University, University of Michigan–Ann Arbor, University of Notre Dame, Washington University in St. Louis

[30]Results on other outcomes can be found in the Appendix Figure B.12 through B.15.

Table 9: Heterogeneous Effect by Star & Non-star University CZ

	High-S. Imm. (per 2014 pop.)	Log(Firm) (20-99 employees)	Natives (per 2014 pop.)	Log(Wage)	Log(Patent)	Log(SBL) (#)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Star University CZ						
Treated	0.919***	0.013***	0.287	0.007	-0.001	0.066***
	(0.220)	(0.005)	(0.208)	(0.006)	(0.030)	(0.017)
Observations	3,960	3,910	3,960	3,920	2,860	3,960
Panel B: Non-Star University CZ						
Treated	0.241***	0.009***	-0.015	0.002	-0.011	0.004
	(0.043)	(0.003)	(0.124)	(0.004)	(0.031)	(0.011)
Observations	5,460	5,410	5,460	5,220	4,140	5,460
Controls:						
Native student	✓	✓	✓	✓	✓	✓
Native population	✓	✓	✓	✓	✓	✓
Predicted SBL	✓	✓	✓	✓	✓	✓

Notes: See Equation (1) for specification. The dependent variables are the number of high-skilled immigrants, the log number of firms, the number of employed natives, weighted log median hourly wages, log number of patents, and the log number of SBLs in year t, CZ i for each column. Panel A reports the results for the CZs with a star university, and Panel B reports the results for the CZs without a star university. A star university is defined based on the top 20 universities in the 2025 US News' Best National Universities Rankings. Treated CZs for the star university analysis are those who have a star university & in the top 25% of the 2014 graduate OPT-eligible ratio. Treated CZs for the non-star university analysis are those who do not have a star university & in the top 25% of the 2014 graduate OPT-eligible ratio. Control CZs have a value of zero for the 2014 graduate OPT-eligible ratio. Control variables include: (i) the native student, defined as the number of native-born graduate students in CZ i and year t, normalized by its 2014 population; (ii) the native population, defined as the total number of natives in CZ i and year t, normalized by its 2014 population; and (iii) the predicted SBL, constructed using a shift–share prediction following Minton et al. (2024). All regressions include CZ fixed effects and year fixed effects. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

4.6 Robustness Checks

Our primary results indicate that the increased supply of high-skilled immigrants, driven by the 2016 STEM OPT extension, robustly resulted in firm expansion, increased native employment and wages (especially for the high-skilled), and increased SBLs activity. In this subsection, we corroborate these key findings by addressing two potential alternative explanations: the choice of estimation technique and the definition of treated regions.

First, to ensure our results are not merely driven by the choice of our estimation technique, the SDID model, we provide the event study estimates from the standard DID model. We have already shown that the impacts on high-skilled immigrants (Figure 2) and the number of firms (Figure 3) are similar across both SDID and standard DID estimates. We further confirm that the impacts on other outcomes, including native employment, and SBLs are also similar to our SDID estimates, as detailed in Appendix Figure B.4.[31]

Second, to address the possibility that our initial definition of treatment and control CZs (treated CZs as top 25% exposure and control CZs as zero exposure) is arbitrary, we redefine treated CZs as those with a strictly positive OPT-eligible share in 2014 (approximately 49% of all CZs) and re-estimate the specifications under this alternative assignment. These results, available in Appendix Figure B.5 through B.10, consistently show the robustness of our main findings across different assignment thresholds.

5 Conclusion

This paper examines how a policy-driven increase in the supply of young, high-skilled immigrants affects local economic activity. Exploiting the 2016 STEM OPT extension as a plausibly exogenous labor supply shock and applying the SDID estimation method, we document several robust empirical patterns across CZs in the United States.

First, the reform led to a sizable and persistent increase in the number of high-skilled foreign workers, particularly STEM workers, consistent with the policy's intent. Second, treated local economies responded along multiple margins, exhibiting significant

[31] In our empirical specifications, in addition to the two-way fixed effects (CZ and time), we include several control variables to account for potential confounding local factors. Specifically, these controls are the native student population, the total native population, and the predicted SBLs.

increases in firm formation and native employment. Employment effects were heterogeneous: native high-skilled and STEM workers experienced substantial and strengthening employment gains, while the positive effect on low-skilled and non-STEM workers diminished over time. Third, while high-skilled native wages initially increased, low-skilled native wages slightly decreased. We also document some effects on patent activity – but only in certain sectors – and some, although less precisely estimated, effects on the number of SBLs. These effects are larger in CZs with a star university.

Our findings contribute to a growing literature that studies the economic impacts of high-skilled migration. By focusing on a large policy-induced supply shock, this paper provides novel evidence that high-skilled young, US-trained immigrants play a central role in inducing local entrepreneurship and enhancing employment of high-skilled native workers.

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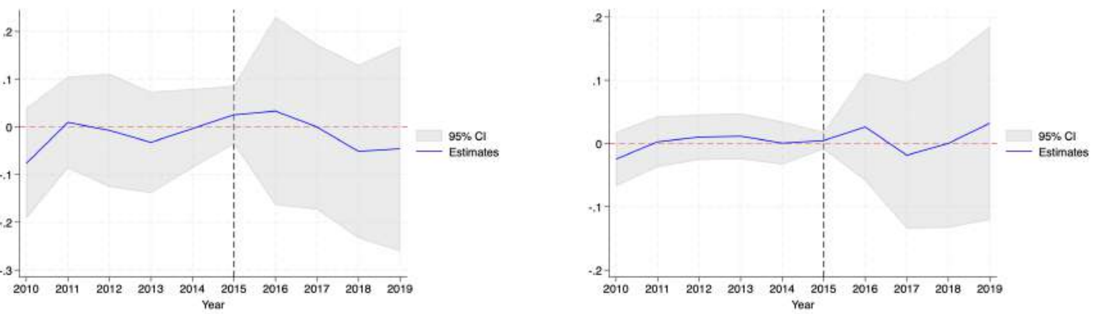
Appendix

Appendix A: Capital Effect

We analyze whether the local increase in skilled migrants affects capital investment. In this analysis, we include asset controls to account for mechanical channels through which balance-sheet size may drive capital investment. Note also that we exclude financial sectors (NAICS code 52), because their dynamics are fundamentally different due to high capital reliance and complex regulation. We use the two outcomes: Capital Expenditure (flow) and Net property, plant, and equipment (level).

Figure A.1 examines the capital investment effect.[32] The estimates show no evidence of differential pre-trends before 2015 for both measures. However, neither measure of capital investment shows a statistically significant increase in the post-periods. These results indicate that, at least within the four-year horizon of our analysis, we do not find a significant capital investment effect in response to increased high-skilled immigration.

Figure A.1: Effect on Capitals: with asset control



(a) Capital Expenditure

(b) Net property, plant, and equipment

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. In subfigure 1(a), the outcome is the log capital expenditure in year t, CZ i. In subfigure 1(b), the outcome is the log net property, plant, and equipment in year t, CZ i. We excluded financial sectors (NAICS code 52), since their dynamics are quite different due to high capital reliance and complex regulations. Treated CZs are those in the top 25% of the 2014 graduate OPT-eligible ratio distribution; control CZs have a value of zero for this measure. Regressions in all panels include CZ fixed effects, year fixed effects, and asset control.

Table A.1 reports the estimated effects of high-skilled migration on capital investment. Across various specifications, the point estimates for capital investment—both flow (Capital Expenditure) and stock (Net Property, Plant, and Equipment)—are small, sometimes

[32]For the capital, we additionally control for the asset, which is the log amount of total assets in CZ i, year t.

positive and sometimes negative depending on the controls used, and are generally not statistically significant. The insignificant capital investment effect observed within this four-year horizon may stem from several factors. First, capital adjustments are inherently sluggish: large-scale investments in property, plant, and equipment typically involve significant planning and execution lags, requiring a longer time frame than our analysis period to fully materialize. Second, data limitations likely play a role. In Compustat, capital variables are typically reported at the firm's headquarters rather than at the establishment level. This introduces measurement error, as capital investments made in local branches or plants may be incorrectly attributed to the headquarters' location, thereby obscuring the true local impact. Furthermore, Compustat primarily covers large, publicly traded firms, which may already be capital-intensive, leading to an underrepresentation of the capital response from the new, smaller firms and startups driven by the high-skilled immigrant shock. Finally, the nature of the labor supply shock itself: the influx of highly skilled labor, rich in knowledge capital, may partially substitute for physical capital in the short term, thus temporarily dampening the need for extensive new capital expenditure.

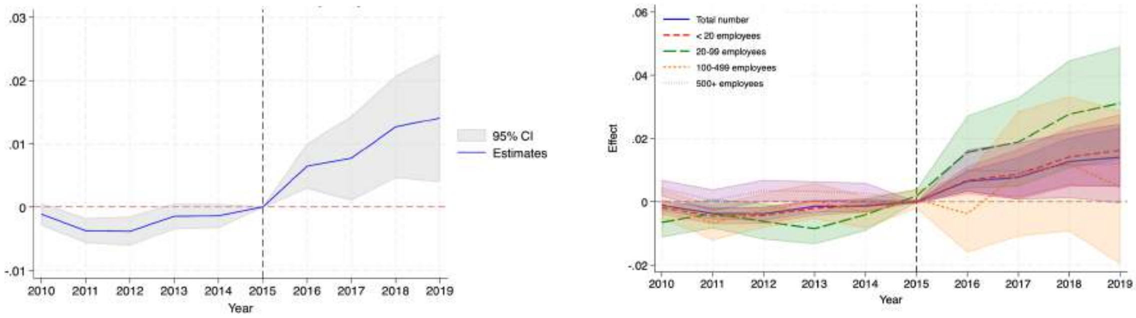
Table A.1: Effect on the Capital

Panel A: Effect on the Capital Expenditure						
	Log (Capx) (amt) (1)	Log (Capx) (amt) (2)	Log (Capx) (amt) (3)	Log (Capx) (amt) (4)	Log (Capx) (amt) (5)	Log (Capx) (amt) (6)
Treated	-0.023	-0.016	-0.009	-0.022	-0.015	-0.016
	(0.100)	(0.081)	(0.080)	(0.078)	(0.081)	(0.086)
Elasticity	-0.38	-0.27	-0.15	-0.37	-0.25	-0.27
Panel B: Effect on the Net property, plant, and equipment						
	Log (PPENT) (amt) (1)	Log (PPENT) (amt) (2)	Log (PPENT) (amt) (3)	Log (PPENT) (amt) (4)	Log (PPENT) (amt) (5)	Log (PPENT) (amt) (6)
Treated	0.024	0.010	0.010	-0.003	0.010	-0.002
	(0.078)	(0.056)	(0.057)	(0.055)	(0.055)	(0.056)
Elasticity	0.40	0.17	0.17	-0.05	0.17	-0.03
Controls:						
Asset		✓	✓	✓	✓	✓
Native student			✓			✓
Native population				✓		✓
Predicted SBL					✓	✓
Observations	1,510	1,510	1,510	1,510	1,510	1,510

Notes: See Equation (1) for specification. The dependent variable in Panel A is the log capital expenditure in CZ i and year t. Panel B reports results for the log net property, plant, and equipment at the CZ–year level. We excluded financial sectors (NAICS code 52), since their dynamics are quite different due to high capital reliance and complex regulations. Treated CZs are those in the top 25% of the 2014 graduate OPT-eligible ratio. Columns differ by the inclusion of control variables. Control variables include: (i) the native student, defined as the number of native-born graduate students in CZ i and year t, normalized by its 2014 population; (ii) the native population, defined as the total number of natives in CZ i and year t, normalized by its 2014 population; (iii) the predicted SBL, constructed using a shift–share prediction following Minton et al. (2024); and (iv) log amount of total assets in CZ i and year t. All regressions include CZ and year fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Appendix B: Figures

Figure B.1: Effect on the Number of Firms

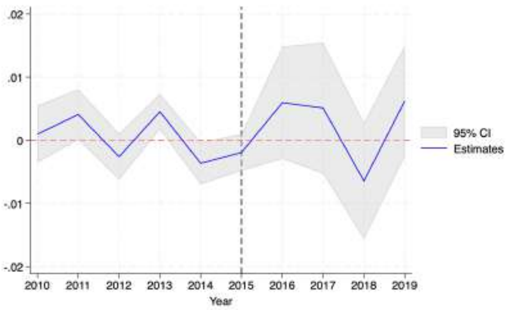


(a) Total Firms

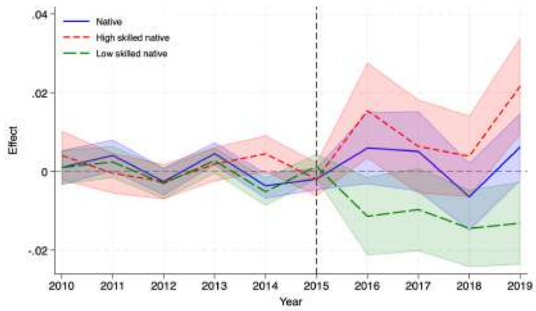
(b) Compare

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. In subfigure 1(a), the outcome is the log number of total firms in year t, CZ i. In subfigure 1(b), the outcome compares the log number of firms by employment size. Treated CZs are those in the top 25% of the 2014 graduate OPT-eligible ratio distribution; control CZs have a value of zero for this measure. Regressions in all panels include CZ fixed effects, year fixed effects, and no additional controls.

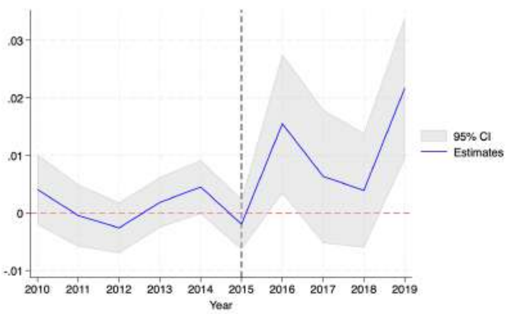
Figure B.2: Effect on the Log Median Weekly Wage



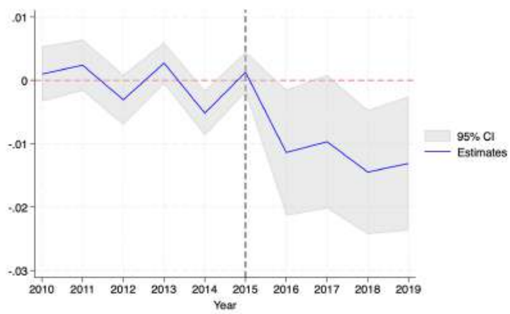
(a) Total



(b) Compare



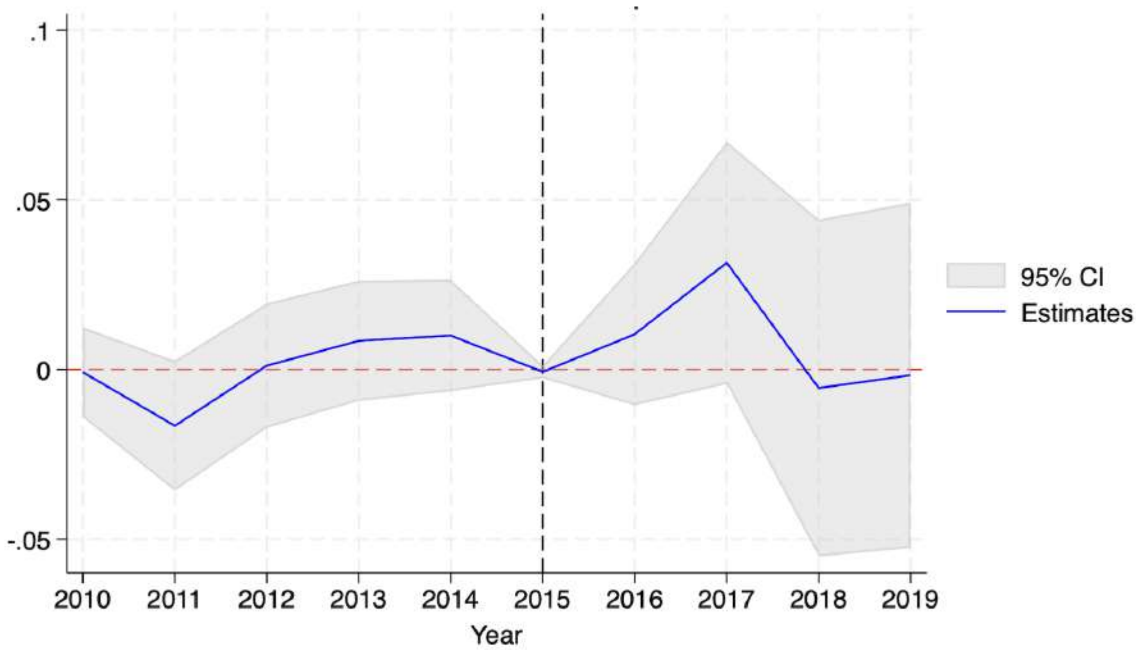
(c) high-skilled



(d) Low skilled

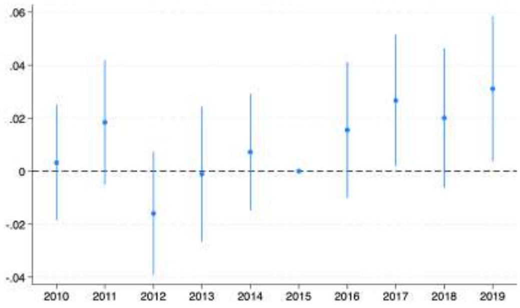
Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. In subfigure 2(a), the outcome is the weighted log median weekly wage for total employed natives, except for those who are self-employed, in year t CZ i. In subfigure 2(b), the outcome compares the weighted log median weekly wage for total native workers with those of high-skilled and low-skilled native workers. Subfigures 2(c) and 2(d) separately present each component. Treated CZs are those in the top 25% of the 2014 graduate OPT-eligible ratio distribution; control CZs have a value of zero for this measure. Regressions in all panels include CZ fixed effects, year fixed effects, and no additional controls.

Figure B.3: Effect on the Amount of Deposits

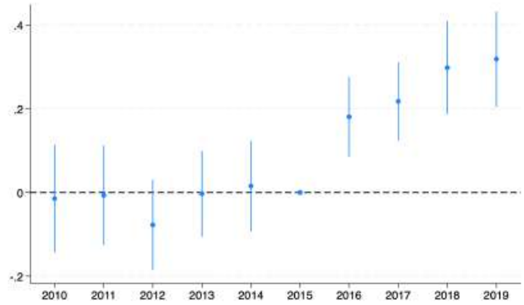


Notes: t indexes a calendar year. This figure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. The outcome is the log amount of deposits of year t, CZ i. Treated CZs are those in the top 25% of the 2014 graduate OPT-eligible ratio distribution; control CZs have a value of zero for this measure. Regressions in all panels include CZ fixed effects, year fixed effects, and no additional controls.

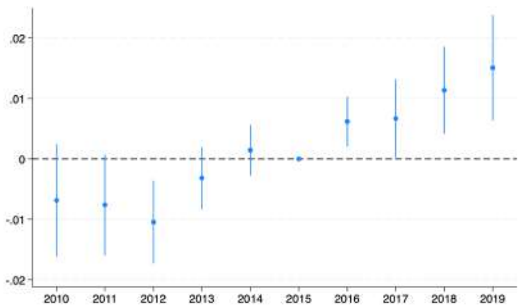
Figure B.4: Event Study Estimates (Standard DID model)



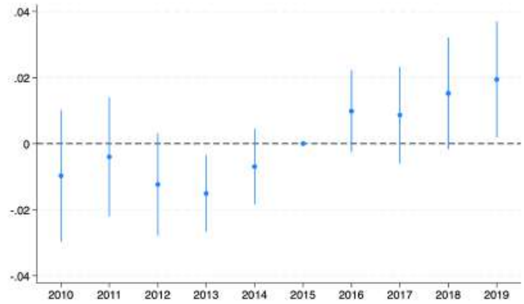
(a) Self-employed High-skilled Immigrants



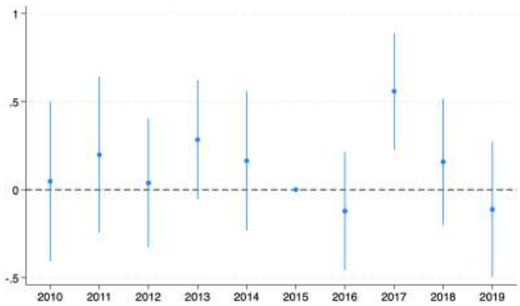
(b) Non-Self-employed High-skilled Immigrants



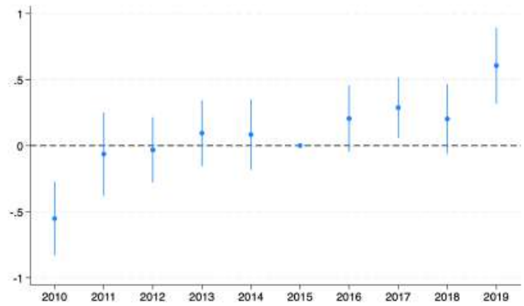
(c) Firms: Less than 20 employees



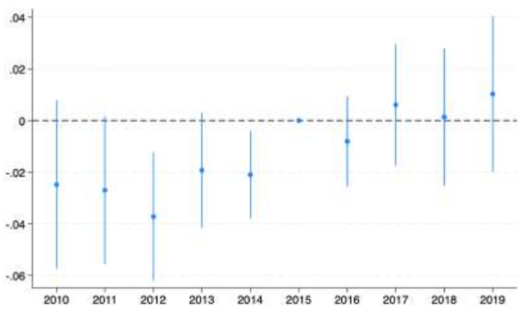
(d) Firms: 20-99 employees



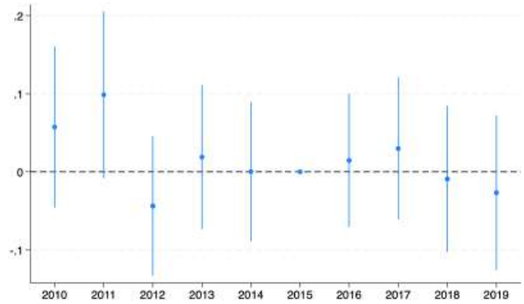
(e) Total Native Employment



(f) High-skilled Native Employment



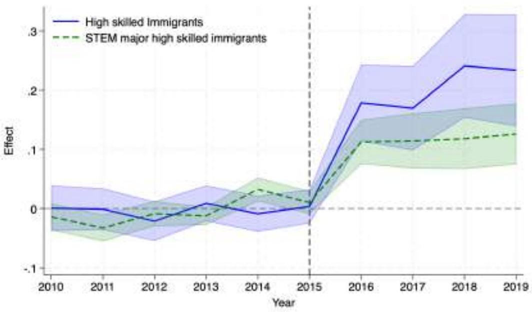
(g) Number of SBLs



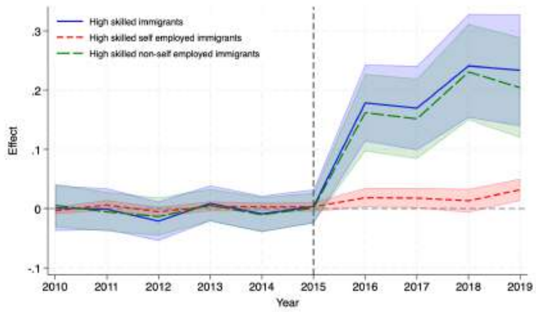
(h) Number of Patents

Notes: Treated CZs are those in the top 25% of the 2014 graduate OPT-eligible ratio distribution; control CZs have a value of zero for this measure. All subfigures show event study results with CZ fixed effects, year fixed effects, and controls. Control variables include: (i) the native student, defined as the number of native-born graduate students in CZ i and year t, normalized by its 2014 population; (ii) the native population, defined as the total number of natives in CZ i and year t, normalized by its 2014 population; and (iii) the predicted SBL, constructed using a shift–share prediction following Minton et al. (2024). Standard errors clustered at the CZ level.

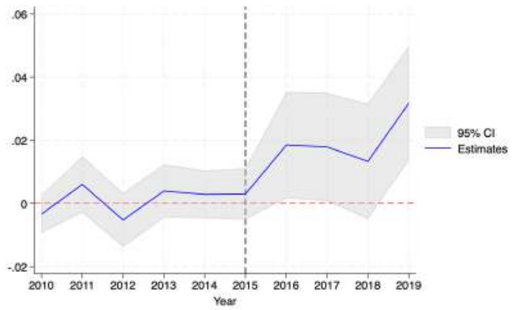
Figure B.5: Effect on the Number of High-skilled Immigrants: Robustness Check



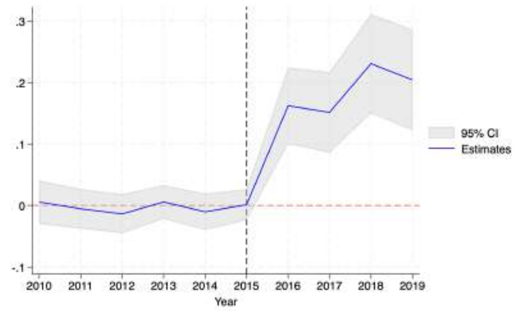
(a) high-skilled



(b) Decomposition



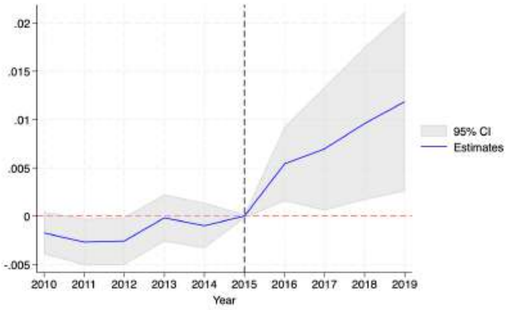
(c) Self employed



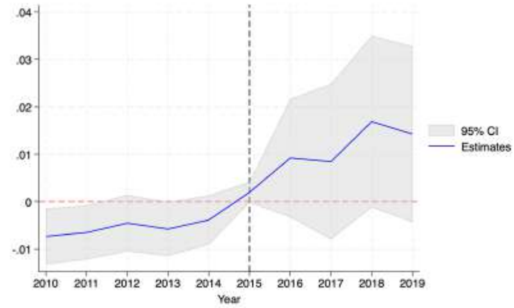
(d) Non-Self employed

Notes: In all subfigures, t indexes a calendar year. Subfigures (a)-(d) plot the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. In subfigure 5(a), the outcome is the number of high-skilled immigrants and STEM-major high-skilled immigrants in year t, CZ i divided by the total population in CZ i in 2014. In subfigure 5(b), the outcome decomposes the total high-skilled immigrants into self-employed and non-self-employed groups; Subfigures 5(c) and 5(d) separately present each component. Total high-skilled immigrants, STEM major high-skilled immigrants, and high-skilled non-self-employed immigrants include both employed and non-employed individuals. Coefficients are multiplied by 100 for readability. Treated CZs are defined as those with a strictly positive 2014 graduate OPT-eligible share, while control CZs have a value of zero for this measure. Regressions in panels (a)-(d) include CZ fixed effects, year fixed effects, and no additional controls.

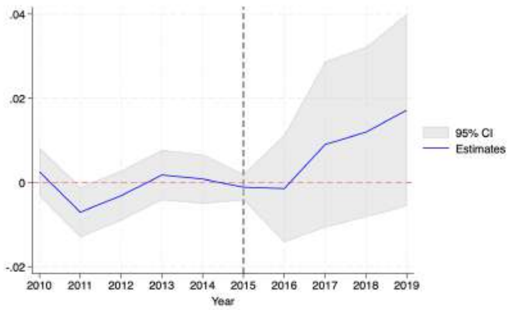
Figure B.6: The Effect of High-Skilled Migration on Firm Creation: Robustness Check



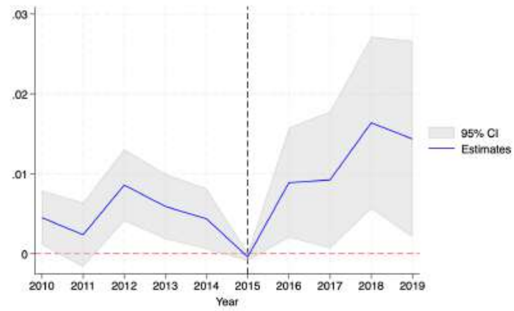
(a) Less than 20 employees



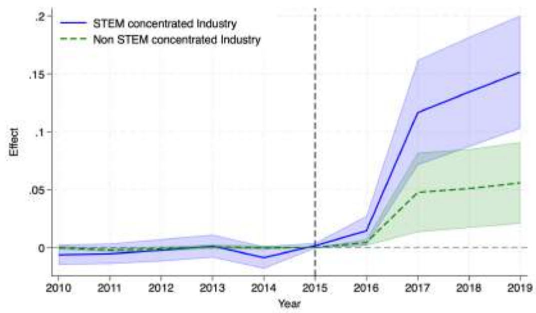
(b) 20–99 employees



(c) 100–499 employees



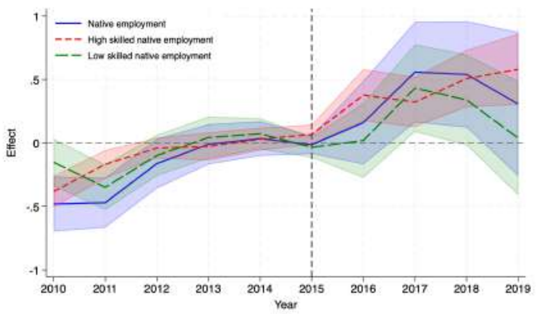
(d) Over 500 employees



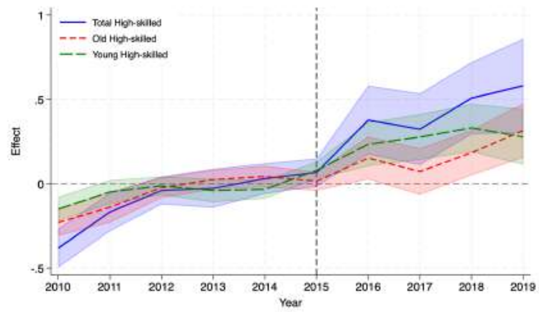
(e) STEM vs Non-STEM establishments: Total

Notes: In all subfigures, t indexes a calendar year. Subfigures (a)-(e) plot the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. The outcomes in subfigures (a)-(d) correspond to the log number of firms in CZ i in year t by employment size: firms with fewer than 20 employees in Subfigure 6(a), firms with 20–99 employees in Subfigure 6(b), firms with 100–499 employees in Subfigure 6(c), and firms with 500 or more employees in Subfigure 6(d). The outcome for subfigure 6(e) is the log total number of establishments in CZ i, year t decomposed by stem concentrated industry and non-stem concentrated industry. The STEM concentrated industry is defined as the 11 NAICS 4-digit industries based on the core concentration industries from Uhlenkott et al. (2014), which indicates industries with at least 5 times more than average concentration in STEM occupations. Treated CZs are defined as those with a strictly positive 2014 graduate OPT-eligible share, while control CZs have a value of zero for this measure. Regressions in panels (a)-(e) include CZ fixed effects, year fixed effects, and no additional controls.

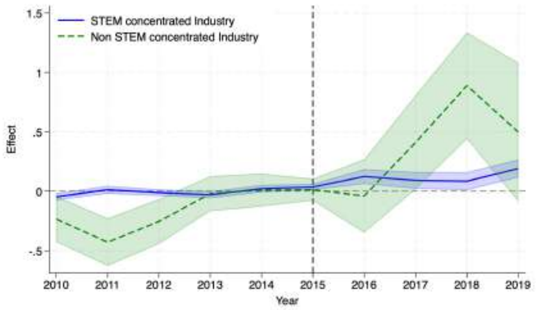
Figure B.7: Effect on the Native Employment/Log Median Hourly Wage: Robustness Check



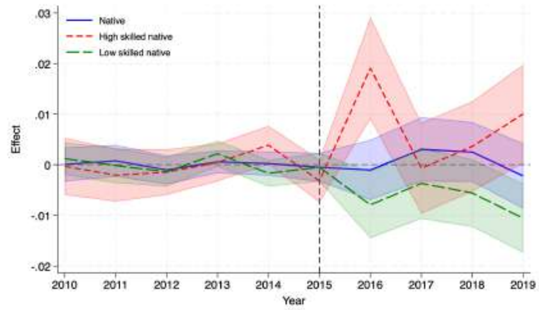
(a) Native Employment: Decomposition



(b) Native High-skilled Employment: Decomposition



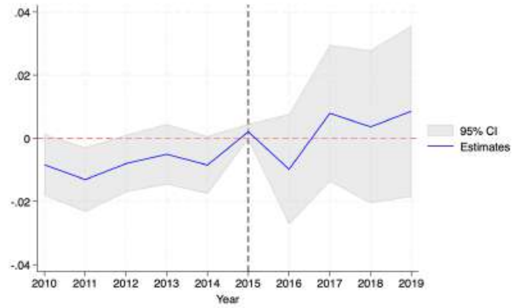
(c) Native Employment: STEM vs Non-STEM



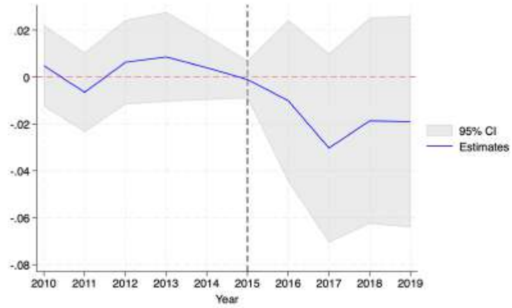
(d) Hourly Wage: Compare

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. In subfigure 7(a), the outcome decomposes the total native employment into high-skilled and low-skilled groups. Subfigure 7(b) decomposes the high-skilled native employment into young and old groups. The outcome for subfigure 7(c) is the number of natives who are employed in year t CZ i, normalized by the 2014 population of CZ i, decomposed to stem concentrated industry and non-stem concentrated industry. STEM concentrated industry is defined as the 11 NAICS 4-digit industries based on the core concentration industries from Uhlenkott et al. (2014), which indicates industries with at least 5 times more than average concentration in STEM occupations. Native employment is defined as wage employment, excluding self-employed individuals. The coefficients for native employment are multiplied by 100 for readability. In subfigure 7(d), the outcome compares the weighted log median hourly wage for total native workers with those of high-skilled and low-skilled native workers. Treated CZs are defined as those with a strictly positive 2014 graduate OPT-eligible share, while control CZs have a value of zero for this measure. Regressions in all panels include CZ fixed effects, year fixed effects, and no additional controls.

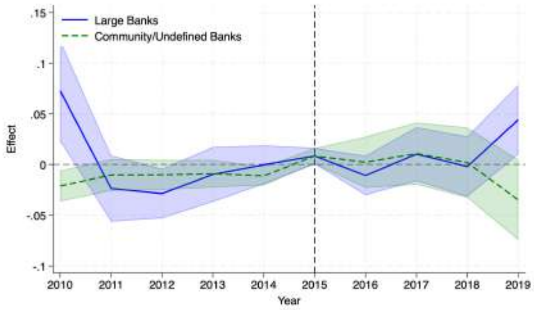
Figure B.8: Effect on SBLs: Robustness Check



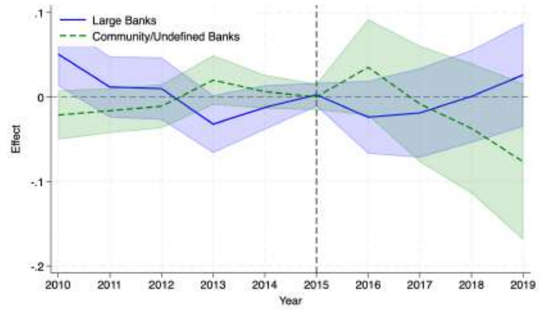
(a) Number



(b) Amount



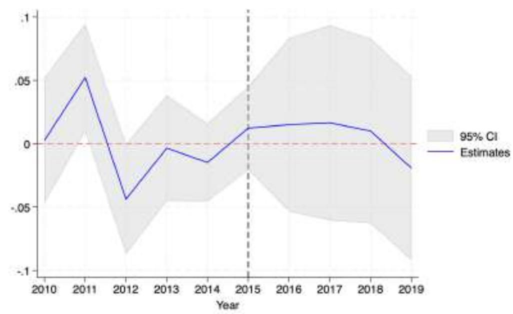
(c) Number: by Bank Size



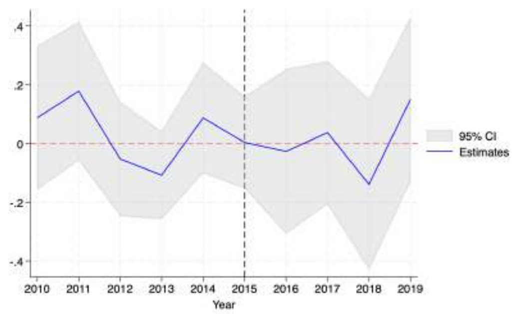
(d) Amount: by Bank Size

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. In subfigure 8(a), the outcome is the log number of SBLs of year t, CZ i. In subfigure 8(b), the outcome is the log amount of SBLs of year t, CZ i. Subfigures 8(c) and 8(d) decompose the effect by bank size for the outcomes number of SBLs and amount of SBLs, each. Based on the information from the Federal Reserve, we defined large banks as those with assets greater than 10 billion, community banks as those with less than 10 billion in assets, and undefined banks as those with no asset information. Treated CZs are defined as those with a strictly positive 2014 graduate OPT-eligible share, while control CZs have a value of zero for this measure. Regressions in panels (a)-(e) include CZ fixed effects, year fixed effects, and no additional controls.

Figure B.9: Effect on Patents: Robustness Check



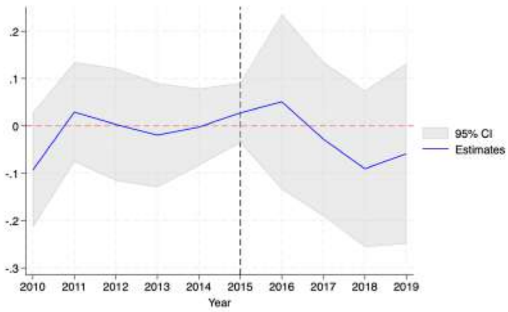
(a) Number of Patents



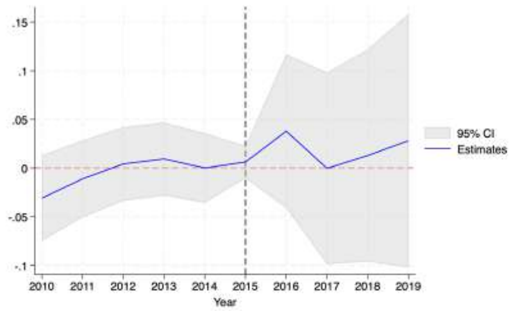
(b) Adjusted Citations

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. In subfigure 9(a), the outcome is the log number of patents applied in year t, CZ i. In subfigure 9(b), the outcome is the log number of adjusted citations of patents applied in year t, CZ i. Treated CZs are defined as those with a strictly positive 2014 graduate OPT-eligible share, while control CZs have a value of zero for this measure. Regressions in all panels include CZ fixed effects and year fixed effects.

Figure B.10: Effect on Capitals: Robustness Check



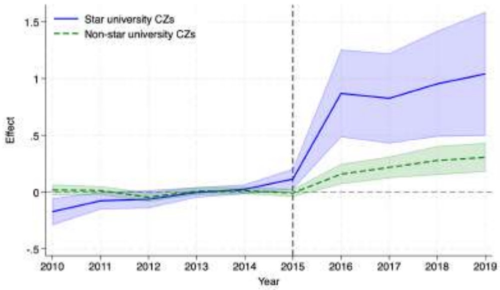
(a) Capital Expenditure



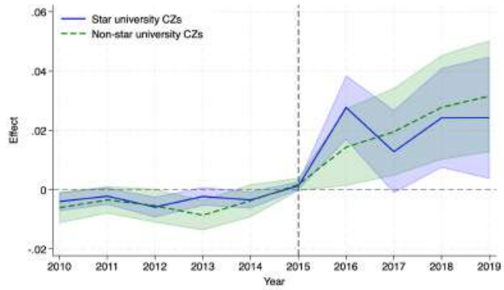
(b) Net property, plant, and equipment

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. In subfigure 10(a), the outcome is the log capital expenditure in year t, CZ i. In subfigure 10(b), the outcome is the log net property, plant, and equipment in year t, CZ i. We excluded financial sectors (NAICS code 52), since their dynamics are quite different due to high capital reliance and complex regulations. Treated CZs are defined as those with a strictly positive 2014 graduate OPT-eligible share, while control CZs have a value of zero for this measure. Regressions in all panels include CZ fixed effects, year fixed effects, and asset control.

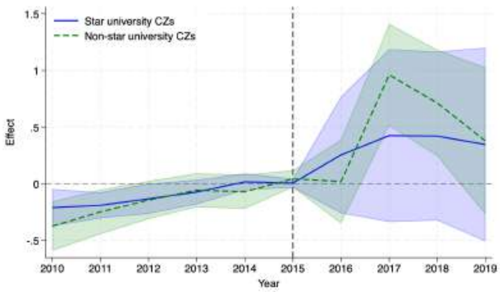
Figure B.11: Star University Effect Compare



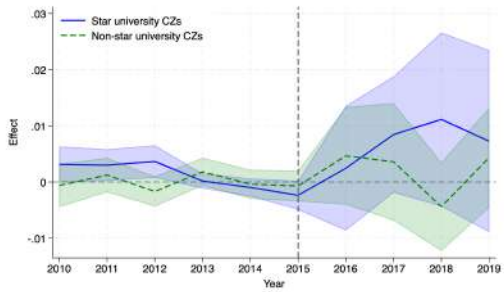
(a) high-skilled Immigrants



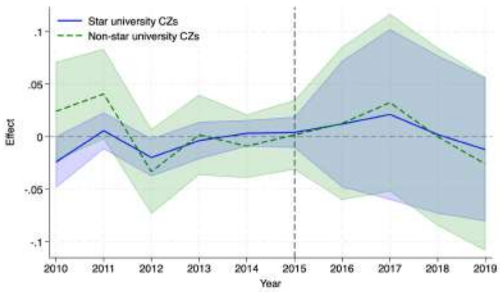
(b) Firms: 20–99 employees



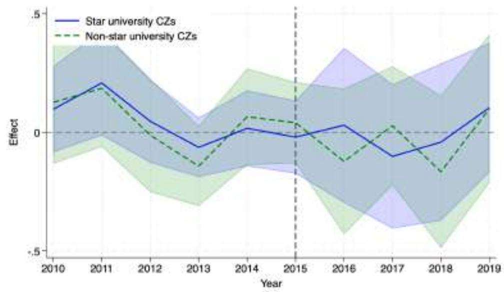
(c) Total Native Employment



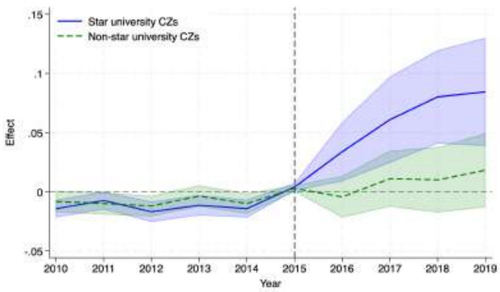
(d) Total Native Log Median Hourly Wage



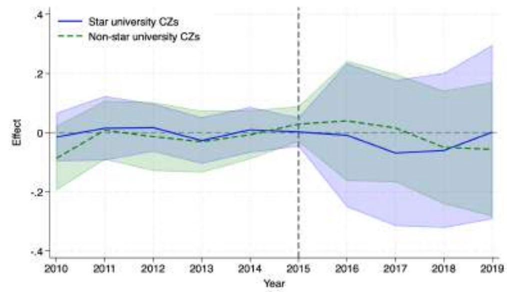
(e) Number of Patents



(f) Adjusted Citations



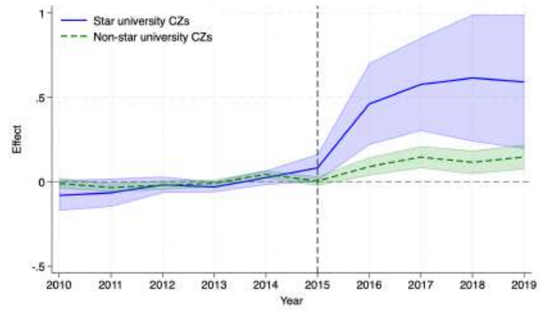
(g) SBL: Number



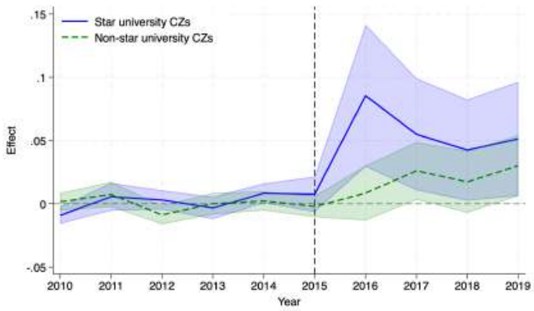
(h) Capital Expenditure

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment is the 2016 OPT extension. Subfigures decompose the effect into CZs with a star university and CZs without a star university, based on the top 20 universities in the 2025 US News' Best National Universities Rankings. The outcomes for the subfigures (a)-(h) are the number of high-skilled immigrants, number of firms with 20-99 employees, total native employment, total native log median hourly wage, number of patents, number of adjusted citations of patents, number of SBLs, and capital expenditure, each. Treated CZs for the star university analysis are those who have a star university & in the top 25% of the 2014 graduate OPT-eligible ratio. Treated CZs for the non-star university analysis are those who do not have a star university & in the top 25% of the 2014 graduate OPT-eligible ratio. Control CZs have a value of zero for the 2014 graduate OPT-eligible ratio. Regressions in all panels include CZ fixed effects, year fixed effects, and no additional controls.

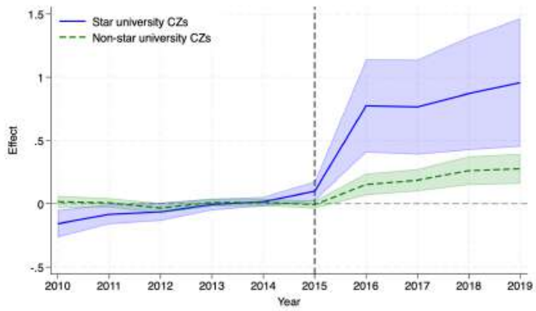
Figure B.12: Effect on the High-skilled Immigrants: Compare



(a) STEM major High-skilled



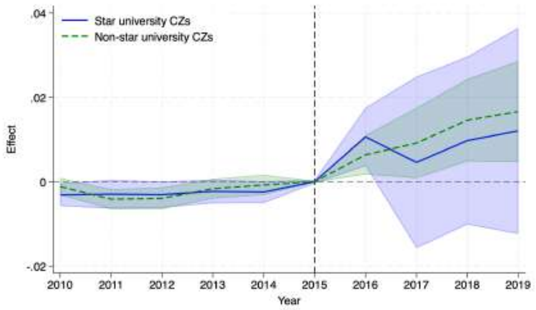
(b) Self employed



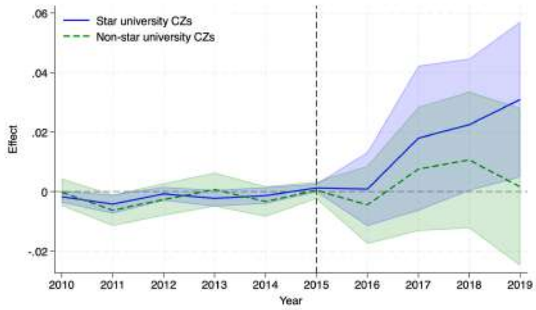
(c) Non-Self employed

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. All subfigures decompose the effect into CZs with a star university and CZs without a star university, based on the top 20 universities in the 2025 US News' Best National Universities Rankings. The outcomes for the subfigures (a)-(c) are the number of stem major high-skilled immigrants, the number of self-employed high-skilled immigrants, and the number of non-self-employed high-skilled immigrants, each. Treated CZs for the star university analysis are those who have a star university and are in the top 25% of the 2014 graduate OPT-eligible ratio. Treated CZs for the non-star university analysis are those who do not have a star university and are in the top 25% of the 2014 graduate OPT-eligible ratio. Control CZs have a value of zero for the 2014 graduate OPT-eligible ratio. Regressions in all panels include CZ fixed effects, year fixed effects, and no additional controls.

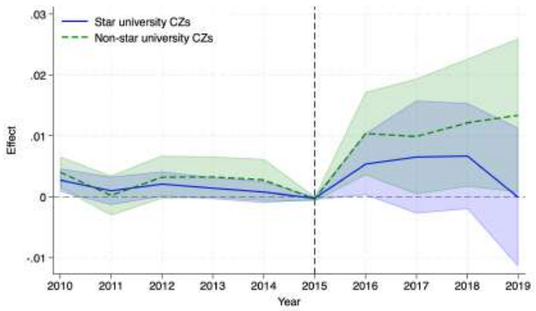
Figure B.13: The Effect of on Firm Creation: Compare



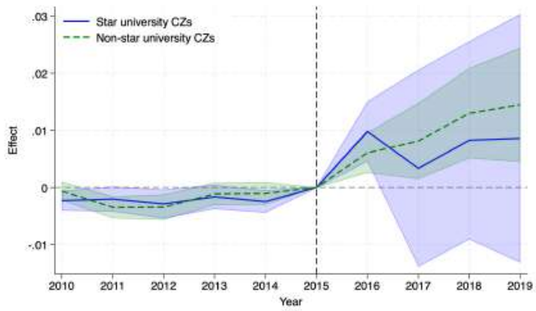
(a) Less than 20 employees



(b) 100–499 employees



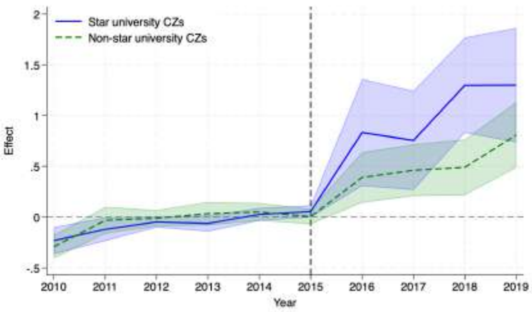
(c) Over 500 employees



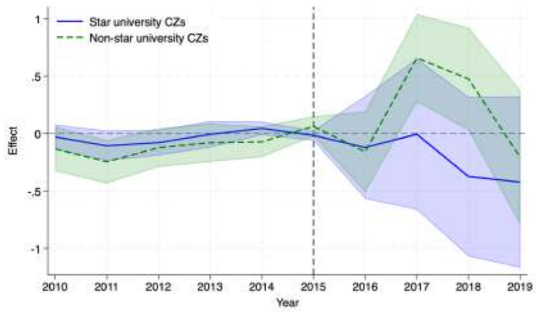
(d) Total

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. All subfigures decompose the effect into CZs with a star university and CZs without a star university, based on the top 20 universities in the 2025 US News' Best National Universities Rankings. The outcomes for the subfigures (a)-(d) are the number of firms by firm size, which are less than 20 employees, 100-499 employees, over 500 employees, and total firms, respectively. Treated CZs for the star university analysis are those who have a star university and are in the top 25% of the 2014 graduate OPT-eligible ratio. Treated CZs for the non-star university analysis are those who do not have a star university and are in the top 25% of the 2014 graduate OPT-eligible ratio. Control CZs have a value of zero for the 2014 graduate OPT-eligible ratio. Regressions in all panels include CZ fixed effects, year fixed effects, and no additional controls.

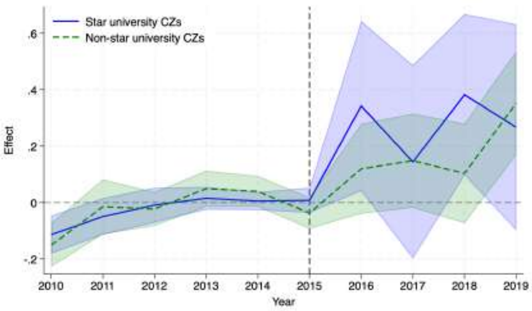
Figure B.14: Effect on the Native Employment and Log Median Hourly Wage: Compare



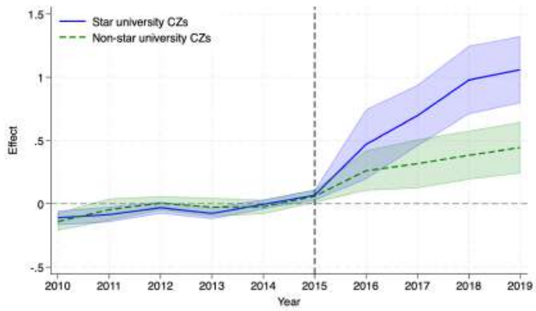
(a) High-skilled Native Employment



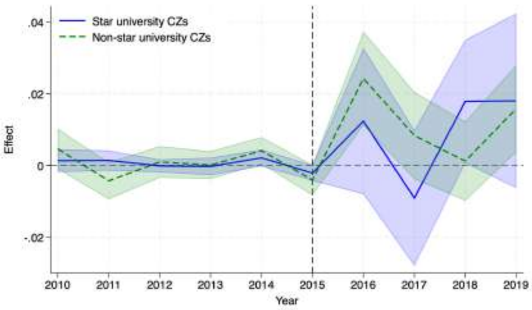
(b) Low skilled Native Employment



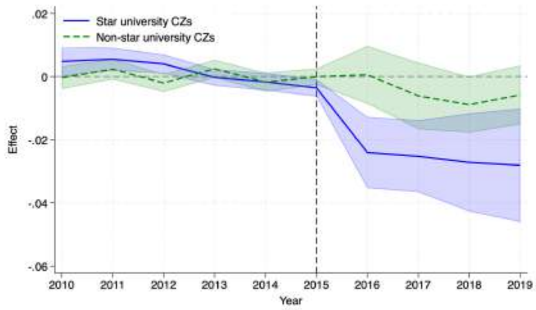
(c) High-skilled Old Native Employment



(d) High-skilled Young Native Employment



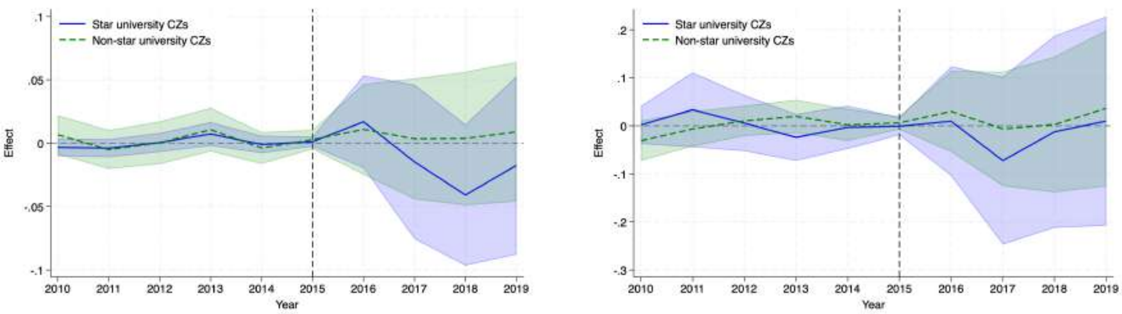
(e) high-skilled Native Hourly Wage



(f) Low skilled Native Hourly Wage

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. All subfigures decompose the effect into CZs with a star university and CZs without a star university, based on the top 20 universities in the 2025 US News' Best National Universities Rankings. The outcomes for the subfigures (a)-(f) are the high-skilled native employment, low-skilled native employment, high-skilled old native employment, high-skilled young native employment, log weighted median high-skilled native hourly wage, and log weighted median low-skilled native hourly wage, respectively. Treated CZs for the star university analysis are those who have a star university and are in the top 25% of the 2014 graduate OPT-eligible ratio. Treated CZs for the non-star university analysis are those who do not have a star university and are in the top 25% of the 2014 graduate OPT-eligible ratio. Control CZs have a value of zero for the 2014 graduate OPT-eligible ratio. Regressions in all panels include CZ fixed effects, year fixed effects, and no additional controls.

Figure B.15: Effect on SBLs and Capitals: Compare



(a) SBL: Amount

(b) Net property, plant, and equipment

Notes: In all subfigures, t indexes a calendar year. Each subfigure plots the SDID estimated coefficients τ and their 95% confidence intervals. The vertical dashed line separates pre-treatment years from post-treatment years, where treatment refers to the 2016 OPT extension. All subfigures decompose the effect into CZs with a star university and CZs without a star university, based on the top 20 universities in the 2025 US News' Best National Universities Rankings. The outcomes for the subfigures (a)-(b) are the log amount of SBLs, and the log net property, plant, and equipment, respectively. Treated CZs for the star university analysis are those who have a star university and are in the top 25% of the 2014 graduate OPT-eligible ratio. Treated CZs for the non-star university analysis are those who do not have a star university and are in the top 25% of the 2014 graduate OPT-eligible ratio. Control CZs have a value of zero for the 2014 graduate OPT-eligible ratio. Regressions in all panels include CZ fixed effects and year fixed effects. For SBL amounts, no additional controls are included. For net property, plant, and equipment, however, we include asset controls.

Appendix C: Tables

Table C.1: STEM Designated Degree Program List: Engineering, Biological & Biomedical Sciences, Mathematics & Statistics, Physical Sciences

2010 CIP Code	DEGFIELD (Detailed)	Label
14.0101	2400	General Engineering
14.0201	2401	Aerospace Engineering
14.0301	2402	Biological Engineering
14.0401	2403	Architectural Engineering
14.0501	2404	Biomedical Engineering
14.0701	2405	Chemical Engineering
14.0801-14.0805, 14.0899	2406	Civil Engineering
14.0901-14.0903, 14.0999, 14.2701	2407	Computer Engineering
14.1001	2408	Electrical Engineering
14.1101, 14.1201, 14.1301	2409	Engineering mechanics, Physics, Science
14.1401	2410	Environmental Engineering
14.3901	2411	Geological & Geophysical Engineering
14.3501, 14.3601	2412	Industrial & Manufacturing engineering
14.0601, 14.1801, 14.2801, 14.3201	2413	Materials engineering & Materials science
14.1901	2414	Mechanical Engineering
14.2001	2415	Metallurgical Engineering
14.2101	2416	Mining & Mineral engineering
14.2201	2417	Naval architecture & Marine engineering
14.2301	2418	Nuclear Engineering
14.2501	2419	Petroleum Engineering
14.2401, 14.3301, 14.3401, 14.3801, 14.9999	2499	Miscellaneous Engineering
26.0101-26.0102	3600	Biology
26.0202-26.0203, 26.021, 26.0299	3601	Biochemical Sciences
26.0301, 26.0305, 26.0307, 26.0308, 26.0399	3602	Botany
26.0204-26.0207, 26.0401, 26.0406-26.0407, 26.0499, 26.1502	3603	Molecular Biology
26.1301-26.1302, 26.1305-26.1308, 26.1399	3604	Ecology
26.0801-26.0806, 26.0899	3605	Genetics
26.0502-26.0503, 26.0507, 26.0599	3606	Microbiology
26.1001-26.1003, 26.1005-26.1007, 26.1099	3607	Pharmacology
26.0707, 26.0901-26.0905, 26.0907-26.0911, 26.0999, 26.1503	3608	Physiology
26.0701-26.0702, 26.0799	3609	Zoology
26.0208-26.0209, 26.0403-26.0404, 26.0504-26.0506, 26.0708-26.0709, 26.1004, 26.1101-26.1103, 26.1199, 26.1201, 26.1303-26.1304, 26.9999	3699	Miscellaneous Biology
27.0301, 27.0303, 27.0399	3701	Applied Mathematics
27.0501-27.0502, 27.0599, 27.9999	3702	Statistics & Decision Science
26.1501	4003	Nutrition Sciences
40.0101	5000	Physical Sciences
40.0201-40.0203, 40.0299	5001	Astronomy & Astrophysics
40.0401-40.0404, 40.0499	5002	Atmospheric Sciences & Meteorology
40.0501-40.0504, 40.0506-40.0508, 40.0599	5003	Chemistry
40.0601	5004	Geology & Earth Science
40.0601-40.0606	5005	Geosciences
40.0607	5006	Oceanography
40.0801-40.0802, 40.0804-40.0810, 40.0899	5007	Physics

Notes: The 2010 CIP codes listed in this table correspond to the STEM-designated degree eligible for the 2016 OPT extension as specified by the U.S. Immigration and Customs Enforcement ICE (2016). These CIP codes are matched to detailed DEGFIELD codes available in the American Community Survey (ACS). The DEGFIELD codes reported in this table are used to identify STEM-major individuals and to construct the OPT-eligible population using the ACS dataset. The Label column reports the corresponding field-of-degree descriptions associated with each DEGFIELD code. This table reports only the CIP codes corresponding to majors in Engineering, Biological and Biomedical Sciences, Mathematics and Statistics, and Physical Sciences. CIP codes for all remaining majors are presented in the subsequent table.

Table C.2: STEM Designated Degree Program List: Others

2010 CIP Code	DEGFIELD (Detailed)	Label
1.0901-1.0907, 1.0999	1103	Animal Sciences
1.1001-1.1002, 1.1099	1104	Food Science
1.1101-1.1106, 1.1199	1105	Plant Science and Agronomy
1.1201-1.1203, 1.1299	1106	Soil Science
3.0103-3.0104	1301	Environmental Science
3.0502, 3.0508-3.0509	1302	Forestry
3.0101, 3.0199, 3.0205, 3.0601	1303	Natural Resources Management
9.0702	1903	Mass Media
10.0304	2001	Communication Technologies
11.0101-11.0103, 11.0199	2100	Computer and Information Systems
11.0201-11.0203, 11.0299, 11.0801-11.0803, 11.0899	2101	Computer Programming and Data Processing
11.0701	2102	Computer Science
11.0401	2105	Information Sciences
11.1001-11.1004, 11.1099	2106	Computer Information Management and Security
11.0901	2107	Computer networking & Telecommunications
13.0501, 13.0601, 13.0603	2399	Miscellaneous Education
15.0000	2500	Engineering Technologies
15.1501	2501	Engineering & Industrial Management
15.0303-15.0305, 15.0399	2502	Electrical Engineering Technology
15.0607, 15.0611-15.0613, 15.0699	2503	Industrial Production Technologies
15.0801, 15.0803, 15.0805, 15.0899	2504	Mechanical Engineering Related Technologies
15.0101, 15.0201, 15.0401, 15.0403-15.0405, 15.0499, 15.0503, 15.0505-15.0507, 15.0599, 15.0701-15.0704, 15.0799, 15.0901, 15.0903, 15.0999, 15.1001-15.1103, 15.1199, 15.1201-15.1204, 15.1301-15.1306, 15.1399, 15.1401, 15.9999	2599	Miscellaneous Engineering Technologies
27	3700	Mathematics
30.1901	4002	Nutrition Sciences
30.0801	4005	Mathematics and Computer Science
30.1001, 30.2501	4006	Cognitive Science and Biopsychology
30.0101, 30.0601, 30.1701, 30.1801	4008	Multi-disciplinary or General Science
41.0204-41.0205, 41.0299, 41.0301, 41.0399, 41.9999	5102	Nuclear, Industrial Radiology, and Biological Technologies
42.2707	5206	Social Psychology
42.2701-42.2703, 42.2705-42.2706, 42.2708-42.2709	5299	Miscellaneous Psychology
43.0106	5301	Criminal Justice and Fire Protection
45.0603	5501	Economics
45.0301	5502	Anthropology and Archeology
45.0702	5504	Geography
49.0101	5901	Transportation sciences & Technologies
51.1002, 51.1005	6105	Medical Technologies Technicians
51.2003-51.2007, 51.2009	6108	Pharmacy, Pharmaceutical Sciences, and Administration
51.2202, 51.2205	6110	Community and Public Health
51.2502-51.2506, 51.251-51.2511, 51.2706	6199	Miscellaneous Health Medical Professions
52.1304	6202	Actuarial Science
52.1301-52.1302, 52.1399	6212	Management Information Systems and Statistics

Notes: The 2010 CIP codes listed in this table correspond to the STEM-designated degree eligible for the 2016 OPT extension as specified by the U.S. Immigration and Customs Enforcement ICE (2016). These CIP codes are matched to detailed DEGFIELD codes available in the American Community Survey (ACS). The DEGFIELD codes reported in this table are used to identify STEM-major individuals and to construct the OPT-eligible population using the ACS dataset. The Label column reports the corresponding field-of-degree descriptions associated with each DEGFIELD code. This table reports the remaining CIP codes not included in the previous table. For CIP codes in Engineering, Biological and Biomedical Sciences, Mathematics and Statistics, and Physical Sciences, refer to the preceding table.

Table C.3: Effect on Natives' Log Median Weekly Wage

	Log (Wage) (1)	Log (Wage) (2)	Log (Wage) (3)	Log (Wage) (4)	Log (Wage) (5)
Panel A: Effect on Native Log Median Weekly Wage					
Treated	0.003	0.003	0.003	0.003	0.003
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Elasticity	0.05	0.05	0.05	0.05	0.05
Panel B: Effect on High-skilled Native Log Median Weekly Wage					
Treated	0.012***	0.013***	0.011***	0.012***	0.012***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Elasticity	0.20	0.22	0.18	0.20	0.20
Panel C: Effect on Low-skilled Native Log Median Weekly Wage					
Treated	-0.012***	-0.012***	-0.012***	-0.012***	-0.012***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Elasticity	-0.20	-0.20	-0.20	-0.20	-0.20
Panel D: Effect on High-skilled Old Native Log Median Weekly Wage					
Treated	-0.000	0.000	-0.001	-0.000	-0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Elasticity	-0.00	0.00	-0.02	-0.00	-0.02
Panel D: Effect on High-skilled Young Native Log Median Weekly Wage					
Treated	0.022***	0.023***	0.022***	0.022***	0.023***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Elasticity	0.37	0.38	0.37	0.37	0.38
Controls:					
Native student		✓			✓
Native population			✓		✓
Predicted SBL				✓	✓
Observations	5,640	5,640	5,640	5,640	5,640

Notes: See Equation (1) for specification. The dependent variable is measured as the weighted log median weekly wage in CZ i and year t. Treated CZs are defined as those in the top 25% of the 2014 graduate OPT-eligible ratio, while control CZs have a value of zero. Panels A-E report effects on the total natives' log median weekly wage, high-skilled natives' log median weekly wage, low-skilled natives' log median weekly wage, high-skilled old natives' log median weekly wage, and high-skilled young natives' log median weekly wage, respectively. Columns differ by the inclusion of control variables. Control variables include: (i) the native student, defined as the number of native-born graduate students in CZ i and year t, normalized by its 2014 population; (ii) the native population, defined as the total number of natives in CZ i and year t, normalized by its 2014 population; and (iii) the predicted SBL, constructed using a shift–share prediction following Minton et al. (2024). All regressions include CZ fixed effects and year fixed effects. *** p<0.01, ** p<0.05, * p<0.1