The Effects of Skilled Immigration Restrictions on Corporate Investment: Evidence from H-1B Visa Application Deadlines*

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Abstract

I study how access to foreign skilled workers affects corporate investment. Restrictions on high-skilled immigration may induce firms to lower investment due to complementarity between skill and capital, and also to delay investment due to ex-ante uncertainty over the ability to hire. I exploit a temporal discontinuity in firms' ability to apply for temporary work visas ("H-1B visas") for prospective employees, and find that rationing of H-1B visas leads to lower capital expenditures, especially for firms in industries more dependent on H-1B workers for skilled labor, for years during which rationing leads to longer hiring delays, and for firms with the slack financial resources to otherwise pursue investment opportunities. Using the historical geographic distribution of immigrants to identify exposure to immigrant supply shocks, I further find that firms delay investment until uncertainty over H-1B hiring is resolved each year, consistent with hiring uncertainty increasing the option value of delaying investment.

Key words: corporate investment, skilled labor, immigration, capital-skill complementarity, hiring uncertainty, H-1B visas.

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1 Introduction

> *"In other businesses the capacity constraint is buildings, plant or equipment. In our business... it's people."*
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> —Jeff Owens, CEO of Advanced Technology Services, Inc.[1]

Access to human capital has become a binding resource constraint for the modern corporation. As Zingales (2000) noted, the balance of power within firms has shifted from physical capital to human capital in recent decades. As improvements in capital markets make it easier to finance capital-intensive assets and globalization increases the demand for innovation, employee talent has become an increasingly important driver of both firm value and investment decisions. Even in traditionally capital-intensive industries such as manufacturing, the race to automate production has made skilled labor essential.[2]

I investigate how access to skilled labor, specifically college-educated *immigrant* workers, affects corporate investment in the United States. My focus on immigration is motivated by the significant and growing population of foreign-born workers as a share of the U.S. college-educated workforce, especially in science, technology, engineering, and math (STEM) occupations.[3] Skilled immigrants in the U.S. are characterized by distinct features that make them economically important. In particular, skilled foreigners specialize in quantitative and analytical skills that complement the soft communication skills of domestic skilled workers (Peri and Sparber, 2011), and prior research has shown that skilled immigration is associated

[1] Weitzman, Hal "Skills gap hobbles US employers" *Financial Times* 13, Dec. 2011. See `https://www.ft.com/content/6d586922-21f0-11e1-8b93-00144feabdc0`.

[2] A 2011 Deloitte survey of manufacturing firms found that seventy-four percent of respondents stated that workforce shortages or skills deficiencies in skilled production roles significantly impacted their ability to expand operations or improve productivity (see `www.themanufacturinginstitute.org/~/media/A07730B2A798437D98501E798C2E13AA.ashx`).

[3] According to Kerr (2020), immigrants grew from 7% of U.S. college-educated workforce in 1980 to 17% in 2020, and make up an even greater share of the STEM workforce (29%) and doctorate degree holders (52%).

with greater levels of per capita patenting (Hunt and Gauthier-Loiselle, 2010) and productivity growth (Peri, Shih, and Sparber, 2015b). Moreover, the business community often points to a "skills mismatch" problem when arguing for less restrictive immigration policies.

Immigration restrictions create obvious frictions in labor markets, and U.S. skilled immigration policy in particular contains complex institutional features that I exploit to identify the causal effect of hiring restrictions on investment. Specifically I use the application process for the H-1B visa program, which allows U.S. employers to hire foreign college workers on a temporary basis. The U.S. government limits the submission of visa applications within a specific time period, and closes the application window once they have received a sufficient number of applications to fill their annual quota. Once the window closes, employers cannot hire foreign workers through the H-1B program for the remainder of the fiscal year. Importantly, firms have no advance knowledge of *precisely* when this deadline arrives each year. Thus, by comparing firms that become eligible to submit applications just before the deadline to firms that become eligible just after the deadline, I identify the causal effects of skilled labor rationing on firm-level outcomes.

Motivated by the literature on capital-skill complementarity (Griliches, 1969), I hypothesize that rationed firms reduce investment relative to their non-rationed peers. In support of this *skill rationing hypothesis*, I find that rationed firms reduce their rates capital expenditure relative to non-rationed firms. Specifically, one additional rationed H-1B application is associated with a 0.0012 percentage point (0.0011 percent relative to the sample) decrease in capital expenditure rates, which roughly translates to $100,000 in reduced capital expenditures per rationed H-1B worker. While the estimated *per-worker* effect is small, the overall economic impact is significant if one aggregates across all workers denied H-1B visas each year, which has consistently exceeded 100,000 in recent years.

Heterogeneity analysis shows that the effect on investment is more pronounced in industries that are more dependent on H-1B workers for skilled labor, during years in which rationing leads to longer hiring delays, and for firms with greater financial slack that would otherwise be less constrained from pursuing investment opportunities. I further find that H-1B rationing results in lower valuations as measured by Tobin's Q, which suggests that the adverse effect of losing access to skilled workers is quickly incorporated into market prices. Robustness tests show that my results do not depend on the length of the window around visa application deadlines used to define my H-1B rationing measure.

The potential effect of H-1B rationing on investment is intuitive, but immigration quotas can also affect investment through creating *uncertainty* over hiring capabilities. As the length of the visa application window is uncertain, it may be optimal from a real options perspective (under the framework of Dixit and Pindyck (1994)) to delay investment decisions until the uncertainty over the number of H-1B workers they can hire for the fiscal year is resolved. Prior research has found that policy uncertainty leads to lower investment (Baker, Bloom, and Davis, 2016; Gulen and Ion, 2016), and that *cyclical* uncertainty (about election results rather than hiring restrictions) lead firms to periodically delay investment (Julio and Yook, 2012; Jens, 2017).

Under the *hiring uncertainty hypothesis*, we expect to observe higher rates of corporate investment after the application deadline for firms that face a greater degree of *ex-ante* uncertainty over H-1B hiring. An alternative empirical strategy is required to study hiring uncertainty, however, as the *ex-post* rationed and non-rationed firms that I identify using application timing are both subject to the same temporal patterns of uncertainty resolution.

To identify variation in cyclical hiring uncertainty, I compare firms based on their ex-ante access to immigrant labor supply. To disentangle demand and supply, I follow Card (2001)

and use locations of *immigrant enclaves*, i.e., regions with historically heavy concentrations of immigrants, to construct regional measures of H-1B labor supply. Firms located in immigrant enclaves should have a natural advantage in attracting H-1B visa workers, and would thereby be more exposed to *both* the rationing effect, which predicts a decrease in investment following the closing of the application window, as well as as the hiring uncertainty effect, which predicts an increase in investment following the closing of the application window.

Since some industries rely on H-1B workers more heavily than others, I further exploit differences in industry-level H-1B usage to formulate a difference-in-difference-in-differences test of the hiring uncertainty hypothesis. I find that firms in regions with high historic immigrant populations systematically increase their capital expenditures following the application deadline relative to firms in regions with low historic immigrant populations, but only if they belong to an industry that relies more heavily on H-1B workers. This result cannot be explained by different investment cycles across industries or across regions, and indicates that the hiring uncertainty channel dominates the skill rationing channel in explaining investment cycles around H-1B visa availability. In light of uncertainty surrounding the recent suspension of the H-1B program by presidential executive order,[4] it is especially important to understand the how uncertainty stemming from immigration policy affects the economy.

2 Literature Review

My paper relates to the emerging empirical literature examining the economic effects of skilled immigration through the lens of the H-1B visa program. Prior studies have found that access to H-1B workers is positively associated with innovation (Kerr and Lincoln, 2010;

[4] See `www.nytimes.com/2020/06/22/us/politics/trump-h1b-work-visas.html`.

Ashraf and Ray, 2017) and productivity (Peri et al., 2015b; Ghosh, Mayda, and Ortega, 2016), while having minimal adverse effects on domestic employment and wages (Kerr, Kerr, and Lincoln, 2015; Peri, Shih, and Sparber, 2015a). While much of the existing literature focuses on *outcomes* (e.g., patenting, output, and wages), I study the effects on firm-level investment *decisions* in order to better understand the interplay between labor and capital accumulation.

I also contribute to the existing H-1B literature through my novel empirical design. The existing H-1B literature can broadly be separated into three methodological categories. The first approach, seen in Kerr and Lincoln (2010), Kerr et al. (2015), and Peri et al. (2015b), is to use immigrant enclaves in the manner of Card (2001) by interacting historical geographic shares of immigrants with changes in the total size of the H-1B program. While I use a similar approach in the second part of this paper, my objective is to study cyclical patterns in hiring uncertainty rather than to identify changes in the supply of H-1B workers. To the best of my knowledge, the only H-1B paper that examines the role of *uncertainty* is Sharifkhani (2018), who studies the asset pricing implications of political uncertainty over immigration policy.

The second approach, seen in Ghosh et al. (2016) and Ashraf and Ray (2017), is to use the dramatic reduction in the annual H-1B cap in 2004 as a natural experiment by comparing changes in firm outcomes between firms with differing degrees of ex-ante reliance on H-1B workers as proxied by pre-2004 LCAs submissions.[5] The potential concern with this approach is that firms hiring H-1B workers at different rates prior to 2004 may exhibit systematic differences. While this concern is mitigated to some extent by the use of empirical matching techniques, the cap drop itself is subject to endogeneity concerns, especially considering the

[5] An earlier iteration of this paper uses a similar approach based on pre-2004 petitions submitted.

proximity of 2004 to the dot-com crash. Kerr, Lincoln, and Mishra (2014), for instance, find that firms that are more reliant on skilled immigrants tend to lobby more aggressively on H-1B policy.

The third approach is to exploit the random lottery system that the USCIS uses to award H-1B visas in years when the number of petitions significantly exceeds the annual cap. Peri et al. (2015a) infers lottery winners and losers by comparing the number of certified Labor Condition Applications (LCAs) and approved USCIS petitions, assuming that the number of visas rejected by the lottery can be inferred from subtracting the latter from the former.[6] As Doran, Gelber, and Isen (2014) point out, however, petition approval conditional on LCA certification is not not completely random due to the possibility of LCA withdrawals. They, along with Wu (2015), use administrative data on lottery outcomes and both reach a different set of conclusions than that of Peri et al. (2015a). My novel empirical design relies on the random *timing* of when LCAs are certified relative to the USCIS petition deadline rather than the visa lottery, and is therefore applicable in years during which the USCIS does not run a visa lottery.

My paper also relates to the broader corporate finance literature on how labor market frictions affect firm investment, which parallels the more established literature on how financial frictions affects investment.[7] Prior research has uncovered evidence that corporate investment decisions are affected by the presence of labor unions (Connolly, Hirsch, and Hirschey, 1986), the mobility of the workforce (Garmaise, 2011; Shen, 2018; Jeffers, 2019), the rigidity of the labor market (Bai, Fairhurst, and Serfling, 2020), and labor supply shocks

[6] This method has been used to study the effects of H-1B rationing on start-up firms (Dimmock, Huang, and Weisbenner, 2019) and offshoring (Glennon, 2020).

[7] Selected works on financial frictions and investment include Fazzari, Hubbard, and Petersen (1988), Whited (1992), Kaplan and Zingales (1997), Rauh (2006), Almeida and Campello (2007), Duchin, Ozbas, and Sensoy (2010), Campello, Graham, and Harvey (2010), and Chaney, Sraer, and Thesmar (2012), among others.

induced by the opioid epidemic (Ouimet, Simintzi, and Ye, 2019). I show in this paper that immigration barriers also constitute an important labor friction that affects corporate investment decisions in multiple ways.

Lastly, my paper relates to the capital-skill complementarity literature started by Griliches (1969). The original idea of capital-skill complementarity is framed as a fundamental inverse relationship between the *relative* prices of skilled and unskilled labor and demand for capital. My paper, concerned with how the rationing of skilled labor affects investment, is motivated by this idea but not a direct test. Ben-Gad (2008) provides a more direct theoretical basis for my hypothesis; he calibrates a structural model with embedded capital-skill complementarity to show that an influx of skilled immigrants substantially raises the return of native-owned capital. Lewis (2011) finds empirical evidence that influxes of low-skilled immigration result in higher adoption rates for capital-intensive automation technology, but the causal relationship between high-skilled immigration and demand for capital is not yet well-established.

3 Empirical Strategy

I exploit institutional features of the H-1B visa program to design two complementary identification strategies. Before describing these strategies in detail, I first provide relevant background information about the H-1B visa program.

3.1 Overview of the H-1B Program

The H-1B visa program was established by Congress through the Immigration Act of 1990. The program allows employers to hire skilled foreign workers to work in the U.S. on a

temporary basis. Visas are issued for a period of up to three years, with the possibility of a one-time extension for an additional three years. According to the U.S. government's website, the program's stated intent is to allow employers to fill vacant positions for which *"the nature of the specific duties is so specialized and complex that the knowledge required to perform the duties is usually associated with the attainment of a bachelor's or higher degree"*.[8] Since the late 1990s, H-1B workers have largely consisted of workers in technical occupations related to science and technology fields, and been heavily recruited by firms in the high-technology sector.

A legislative cap limits the total number of *new* H-1B visas issued each year (there is no limit to the number of visa extensions). This H-1B cap has fluctuated throughout the history of the H-1B program, starting with 65,000 in 1992 and reaching as high as 195,000 during the early 2000s, before dropping back down to 65,000 in 2003. Before the cap reduction in 2003, employers were effectively assured of securing visas. Since 2004, however, demand for new H-1B visas has exceeded supply every year, resulting in a binding cap and a rationing of visas.

The H-1B visa program provides an economically significant source of skilled immigrant workers for U.S. firms. In comparison to the 65,000 annual cap for H-1B visas, the annual limit for employment-based legal permanent residents (i.e. "green cards") is set at 140,000. Moreover, each foreign country is allotted up to 7% of the annual green card limit. Since the H-1B program places no such per-country limit, it is often the only practical channel for firms to hire workers from countries with large emigrant populations. In particular, the country of India supplies a significant majority of H-1B visa applicants, with Indian workers

[8] More than 98% of all approved applicants in 2004 possessed at least a bachelor's degree, with the remaining 2% coming from special exempt occupation such as fashion modelling.

representing over 73% of H-1B applicants in 2017.[9]

The process of obtaining a new H-1B visa entails a two stage application process. In the first stage, a prospective H-1B employer submits a Labor Condition Application (LCA) to the Department of Labor (DOL), in which the the employer provides information about the number of workers they are looking to hire, the proposed wage, and work location. They must also attest that the prospective H-1B employee(s) will be paid a salary commensurate with prevailing wages for employees with similar experience and qualifications, and that the prospective hire(s) will not adversely affect domestic U.S. workers.

Once the DOL approves the LCA, the employer must submit an H-1B application package for each specific employee it wants to hire to the United States Citizenship and Immigration Services (USCIS). The package consists of the certified LCA, a visa petition containing details about the prospective worker and the position being filled, and supporting proof of the worker's qualifications. Since a petition is specific to one worker and an LCA can list multiple workers for a given role, a single LCA may be associated with multiple petitions. If approved, the USCIS forwards the application package to the Consular Office at the Department of State, which reviews the documents and issues the visa if no problems are found.

The USCIS's processing of visa petitions creates a significant bottleneck in the H-1B hiring process. As illustrated in Figure 1, the agency begins to accept petitions for new H-1B visas every year on April 1 for the upcoming fiscal year. The fiscal year is defined according to the agency's fiscal calendar: fiscal year (FY) t begins on October 1 of calendar year $t-1$ and ends on September 30 of calendar year t.[10] The bottleneck arises because the

[9] See `https://www.uscis.gov/sites/default/files/USCIS/Resources/Reports%20and%20Studies/Immigration%20Forms%20Data/BAHA/h-1b-2007-2017-trend-tables.pdf`. Note that the proportion of Indian applicants has grown larger over time.

[10] An application submitted and approved in June of 2005, for example, will allow the prospective H-1B worker

USCIS stops accepting new petitions at a certain point each year, when it has determined that a sufficient number of petitions have been submitted to fill the annual cap. Depending on demand for H-1B visas, the petition deadline may come just a few days after the April 1 opening of the application window, or as late as February of the following year. Uncertainty over the exact deadline for submitting petitions plays a crucial role in my identification strategy.

3.2 Identifying H-1B Rationing

To identify exogenous variation in H-1B visa rationing across firm, I exploit the timing of when hiring firms received their LCA certifications and the date at which the USCIS stops processing new petitions for the upcoming fiscal year. The identifying assumption is that firms receiving LCA certification *immediately after* the petition deadline are rationed relative to firms receiving LCA certification *immediately prior to* the petition deadline, but the timing of when LCAs are certified within a tight window around the petition deadline is otherwise unrelated to a firms' subsequent investment decisions.

This identification strategy relies on two institutional details of the application process. The first is that a certified LCA is required for employers submitting petitions to the USCIS, implying that an employer receiving LCA approval after the petition deadline will have to wait until next April to submit the accompanying visa petition and hence delay hiring for one year. The second is that the exact deadline for submitting visa petitions is uncertain. While prospective employers likely have an approximate idea when the cap will be reached given knowledge of recent history and general guidance from the USCIS, the exact date on which the USCIS stops accepting petitions is not known in advance.

to begin work on October 1, 2005 for a period of three years.

Table I lists the petition deadline for each fiscal year from 2004 to 2020. I report dates starting in 2004 because the number of H-1B applications never exceeded the cap before the cap was lowered from 195,000 to 65,000 in 2004. Note that in 2008, 2009, and every year since 2014, the USCIS stopped accepting new petitions only a few days after April 1, with no petition deadline occurring later than April 7. The high speed at which the H-1B cap was reached during these years raises endogeneity concerns over the timing of LCA processing dates: given that the April 1 application start date *is* known in advance, employers who have their LCAs certified as soon as the application window opens are likely to be fundamentally different from employers who do not.[11]

In years when the cap was not reached until well after April 1, several factors may delay the certification of LCA from immediately before to immediately after the petition deadline. First, the Department of Labor can take multiple days to process an individual LCA, which means that firms may submit an application before the deadline only to receive certification after the deadline has passed. Second, the USCIS's public announcement of the petition deadline often arrives after the deadline has passed, which *retroactively* voids the value of any LCA submitted after the deadline. Thus, a firm may submit an LCA only to find out later that the submission came after the petition deadline. Table I provides information on the delay between the petition deadline and its announcement to the public for each year. Third, inertia from bureaucratic internal hiring procedures may induce firms to continue submitting LCAs after the petition deadline has passed. Identification relies on these factors being orthogonal to firms' investment opportunities.

[11] This is exacerbated by the fact that firms began to "predate" their LCA applications in order to have them in place by April 1 for FY2008 and FY2009 hiring cycles (Peri et al., 2015a). Predating LCAs indicates a willingness to sacrifice length of employment for a higher probability of submitting a petition before the deadline. Since LCAs are effective for three years, starting no longer than six months from submission, applying for an LCA before April 1 implies the LCA will expire before the three year employment term beginning on October 1.

Figure 2 illustrates the total number visa petitions for new H-1B hires around a +/- 20-business-day window around the petition deadline for each year from FY2004 to FY2013, excluding FY2009.[12] It is clear that the chart for FY2008, with a sharp spike immediately preceding the petition deadline surrounded by near zero submissions on the other dates, displays a fundamentally different pattern than charts for other years, which show more sustained application activity before the drop-off on the petition deadline. Figure 3 further emphasizes this contrast, illustrating the average frequency of LCAs from publicly traded companies certified within a +/- 5-business-day window around the petition deadline. The relative balance between the right and left of the deadline in subfigure (a) indicates that the timing of the deadline came largely as a surprise during years other than FY2008/FY2009, while the asymmetric pattern in subfigure (b) strongly suggests a rush to obtain LCAS in FY2008/FY2009 before the closing of the petition window.

To avoid issues associated with the early arrival of petition deadlines, I exclude FY2008 and FY2009 and use the remaining years from FY2004 to FY2013. I further exclude FY2010, as the USCIS allowed firms to submit petitions without certified LCAs from November 5, 2009 through March 9, 2010 due to delays in processing times for LCAs that year.[13] As the petition deadline for FY2010 (December 21, 2009) fell within the exemption window, firms that received LCA certifications following the petition deadline were not prevented from submitting petitions before the deadline and therefore were not rationed.

I construct my measure of H-1B rationing as the *difference* between the number of LCAs certified immediately after and immediately before the USCIS petition deadline. Using this measure, I estimate the following fixed effects panel regression:

[12] The chart for FY2009 is very similar to FY2008, and excluded in order to present a more legible grid layout.
[13] See `https://www.uscis.gov/archive/archive-news/questions-and-answers-temporary-acceptance-h-1b-petition-filed-without-dols-certified-labor-condition-applications-lcas`.

$$CapEx_{it} = \alpha_i + \lambda_t + \delta PrePostDiffLCA_{it} + X_{it}\beta + \epsilon_{it}, \quad (1)$$

where i indexes firms and t indexes H-1B fiscal years (i.e., starting October 1), *CapEx* represents capital expenditures, α represents a firm-specific dummy, λ_t denotes a time-specific dummy, X represents a vector of control variables, ϵ represents the error term, and *PrePostDiffLCA* represents the difference between number of LCAs certified before and after the cap reach date for fiscal year t.

PrePostDiffLCA captures the degree to which firms are rationed from H-1B hiring in a given year. The underlying assumption is that firms receiving LCA approval just before the petition deadline are able to submit petitions in time, while firms receiving LCA certification just after the petition deadline are prevented from submitting. For example, a firm that receives LCA certification one day before the August 10, 2005 petition deadline is in time to apply for H-1B visa to allow its prospective employee to start working in FY2006, but a firm that receives certification one day after the deadline is too late to apply for a FY2006 start date. Under the skill rationing hypothesis, the rationed firm will lower its investment in FY2006, implying that δ from Eq. 1 should be negative.

It is important to note that the inclusion of firm fixed effects in Eq. 1 implies that identification of LCA rationing comes from *within-firm* variation, i.e., I am effectively comparing how LCA rationing changes over time for a given firm and relating it to its changing investment policy. This alleviates concerns over any potential relationship between a firm's tardiness in applying for LCAs and time-invariant firm characteristics.

I define the window around the petition deadline as three business days preceding and following the petition deadline. The choice of a six business day window is motivated by Figure 3(a), which shows that the average number of LCAs in the three days prior to and

three days following the petition deadline exhibits a reasonable degree of balance, but beyond this range, the number of LCAs becomes sharply lower on the right side relative to the left side. I do not include the number of LCAs certified *on* the petition deadline, as it is not clear whether the employer would have time to submit a petition on the same day its LCA is certified. In robustness tests, I examine alternative windows of varying lengths.

Although a single LCA can list multiple workers, I define *PrePostDiffLCA* by aggregating LCA *application counts* rather than aggregating LCA *worker counts*. I do this for two reasons. First, data on number of workers can be unreliable due to errors in data entry. Second, number of workers exhibits a steeper positive relationship to firm size, as LCAs from larger firms tend to list more employees per application. This is illustrated by Figure 4, which plots firm-level log employment against the number of LCAs submitted (*NearCapLCA*) and number of workers listed on LCAS submitted (*NearCapWorkers*), respectively, during the six day window around the cap reach date. The figure also shows that number of workers is more volatile than number of applications. Thus, I use the more precise application-based measure that, to a certain extent, already scales for employer size.

3.3 Identifying Regional Exposure to H-1B Visa Availability

To study how the fluctuating USCIS petition deadline generates cyclical patterns of uncertainty over H-1B hiring, I compare firms that are differentially exposed to the time-varying availability of H-1B visas. Firms that rely more heavily on H-1B workers as a supply of skilled labor should face greater ex-ante uncertainty over their hiring for the next fiscal year and also experience sharper rationing effects upon reaching the petition deadline. Whether such firms will increase or decrease investment following the petition deadline each year depends on whether the hiring uncertainty or skill rationing effect dominates.

To identify cross-sectional variation in H-1B dependence that is exogenous to cyclical patterns of investment opportunities, I construct a measure based on the "immigrant enclave" instrument from Card (2001). As new immigrants tend to congregate in regions with high concentrations of existing residents with similar ethnic and cultural backgrounds (Bartel, 1989), we should expect *historical* regional concentrations of immigrants to predict the preferred destinations of new H-1B workers. Since a significant majority of H-1B workers come from India, I use the regional proportion of residents that speak an *Indic* language at home as of 1990 as a measure of the historical Indian immigrant population.[14] Using this measure of regional exposure to H-1B visa availability, I estimate the following specification:

$$CapEx_{iyq} = \alpha_i + \lambda_{yq} + \gamma_{iy} + \delta PctIndic_i Post_{yq} + X_{it}\beta + \epsilon_{iyq}, \tag{2}$$

where i indexes firms, y indexes years, and q indexes fiscal quarters, $CapEx$ represents capital expenditures, α represents a firm-specific dummy, λ denotes a year-quarter dummy, γ denotes a firm-year dummy, X represents a vector of control variables, ϵ represents the error term, $PctIndic$ represents the proportion of Indic-language residents as of 1990 in the region of firm i's headquarters, and $Post$ represents a dummy variable indicating that quarter q comes the petition submission deadline in year y. Note that $PctIndic$ and $Post$ are not included in the specification as they are subsumed by firm and time fixed effects.

Eq. 2 represents a repeated difference-in-differences test in which the inclusion of firm-year fixed effects means identification comes from within-firm changes in quarterly investment in each year. Under the skill rationing hypothesis, firms in high $PctIndic$ regions experience

[14] Indic languages are classified as Census 3-digit language codes 662–678 (e.g., Hindi, Bengali, Gujarathi, Punjabi, Urdu, Nepali, Sinhalese, etc.–see `https://www.census.gov/hhes/socdemo/language/about/02_Primary_list.pdf`). I choose a language-based measure rather than a broader ethnicity-based measure due to the existence of American-born residents with Indian ethnicity.

more severe skill rationing following the petition deadline each year and hence lower investment. Under the hiring uncertainty hypothesis, firms in high *PctIndic* regions have a greater degree of uncertainty over skilled hiring resolved following the petition deadline and hence increase investment. The net effect on investment, and hence the sign on δ, depends on which effect dominates.

I define geographic regions using commuting zones, which form logical geographic units for defining local labor markets (Tolbert and Sizer, 1996; Autor, Dorn, and Hanson, 2013). Figure 5 provide commuting zone data maps of *PctIndic* and average annual number of H-1B petitions, respectively, in subfigures (a) and (b).[15] The geographic correlation between *PctIndic* and subsequent H-1B petitions is visually evident, suggesting that Indian enclaves predict subsequent H-1B hiring. Note that the geographic distribution of *PctIndic* may be related to other factors, such as population density and degree of urbanization, but concerns over possible omitted variables are mitigated to a large extent by the inclusion of firm-year fixed effects.

I segment firms based on their industry-level rates of H-1B hiring prior to 2004, and separately estimate Eq. 2 on the subsamples of high H-1B usage and low H-1B usage industries, respectively. In doing so, I account for the fact that not all industries employ H-1B workers at a significant rate. Moreover, it allows me to strengthen identification by estimating a difference-in-difference-in-differences ("triple-differences") test in which I include additional interaction terms involving a dummy variable for high H-1B usage industries. In the most rigorous fixed effects specification, I control for time-varying differences across commuting zones and across industries.

[15] Data on H-1B petitions comes from the USCIS via a Freedom of Information Act (FOIA) request.

4 Data

4.1 Data Sources

I construct two samples, the "LCA sample" and the "immigrant enclave sample", to implement the empirical strategies described in Sections 3.2 and 3.3, respectively. For both samples, I use firm-level data from the merged CRSP-Compustat Fundamentals Quarterly file. Following conventions of the literature, I exclude firms in the utilities, financial, and public sectors (SIC code 4900-4999, 6000-6999, and 9000-9999, respectively). Following Almeida, Campello, and Weisbach (2004) and Duchin et al. (2010)), I also discard observations where firms fall below $50 million in terms of market capitalization. This serves to exclude the smallest firms with volatile accounting data and skewed investment patterns.

I limit both samples to the FY2004–FY2013 time period, excluding FY2008 and FY2009, where fiscal years are defined according to the USCIS fiscal calendar (i.e., starting October 1). As described in Section 3, FY2008, FY2009, and every year from FY2014 onwards are excluded because the petition deadline arrived only a few days after the petition window opened in those years. Years prior to FY2004 are excluded because the H-1B cap was never reached in those years. I further exclude FY2010 from the LCA sample due to the temporary LCA exemption that year, but not from the ethnic enclave sample as my second empirical strategy does not rely on the quasi-random timing of LCA approvals.

I obtain data on H-1B petition submission deadlines from the USCIS News Archives,[16] and data on LCA filings from the U.S. Department of Labor.[17] As the raw LCA data does not include standardized firm identifiers, I match the employers listed in the LCA data to

[16] Available at `https://www.uscis.gov/archive`

[17] Data on LCAs after 2007 are found at `https://www.flcdatacenter.com/caseh1b.aspx`, and data on LCAs before 2007 are found at `https://www.foreignlaborcert.doleta.gov/performancedata.cfm`.

Compustat firms using company names and locations. Due to spelling mistakes and alternate variations of firm names in the USCIS data, I employ a name matching procedure that incorporates fuzzy string matching as well as manual inspection of matches. Importantly, the LCA data provides the certification/denial decision, as well as the exact date on which each filing was certified or denied.

I obtain historical demographic data on Indic-speaking populations from the 1990 U.S. Census Summary File 3. To map county-level information from the Census data to commuting zones, I obtain commuting zone information from the U.S. Department of Agriculture Economic Research Service.[18] Lastly, I obtain a list of NAICS4 industries from Table 13 of the Department of Homeland Security's report on the characteristics of H-1B workers for fiscal year 2002 (which immediately precedes my sample time period) to define high H-1B usage industries.[19]

4.2 Descriptive Statistics

I present descriptive statistics for my data in Table II, with Panel A displaying statistics for the LCA sample, and Panel B displaying descriptive for the immigrant enclave sample. It is immediately apparent that the former is substantially larger than the latter in size. There are three reasons for this discrepancy. First, the LCA sample excludes FY2010 due to the temporary LCA exemption that year, but the immigrant enclave sample does not. Second, quarterly Compustat observations are aggregated by year for the LCA sample but not for the immigrant enclave sample; specifically, I average quarterly measures within each USCIS fiscal year to create annual measures that align with H-1B hiring cycles for the LCA sample.

[18] See `https://www.ers.usda.gov/data-products/commuting-zones-and-labor-market-areas.aspx`.

[19] These industries consist of 4-digit NAICS 5415, 6113, 5413, 5416, 5417, 5133, 6111, 5412, 6221, 5231, 3344,6211,5419, 5410, 5239, 3342, 5141, 3254, 3341, 6213, 5411, 5414, 5613, 3340, 5112, and 5418, and account for more than 90% of all H-1B applications in 2002.

Third, firms only enter the LCA sample if they receive at least one LCA approval within the six day window around the H-1B petition deadline, while no similar restriction applies to the immigrant enclave sample.

Since a firm that submits LCAs close to the petition window one year does not necessarily do so every year, the LCA sample forms an unbalanced panel. I provide details on the panel structure of the LCA sample in Figure 6. Subfigure 6(a) illustrates how the sample breaks down by fiscal year, showing that the number of firm-year observation gradually rises from 211 in 2004 to 306 in 2007, then drops down to hover above 200 in the years following the financial crisis. Subfigure 6(b) illustrates a tabulation of distinct firms by how often a firm enters the sample, and shows that most firms only appear two or three times in the sample,[20] and that only 29 heavy users of H-1B visas apply for LCAs close to the petition deadline every year.

Returning to Table II, the Compustat variables listed in Panel A and Panel B include *CapEx* (capital expenditures scaled by lagged book assets), the primary dependent variable in my regressions, as well as firm-level control variables $ln(Assets)$ (natural log of book assets), *Tobin's Q* (market value of assets divided by book value of assets), *Cash Flow* (operating income before depreciation scaled by lagged book assets), *Leverage* (long-term debt scaled by book assets), and *Cash Holdings* (cash holdings scaled by lagged book assets). I also construct financial constraints indicators $FC\ (KZ)$, $FC\ (WW)$, and $FC\ (HP)$ according to Kaplan and Zingales (1997), Whited and Wu (2006), and Hadlock and Pierce (2010), respectively, as additional control variables. To address outliers, I winsorize all Compustat variables at the 1% level at both tails. For the purpose of legibility, all variables that are scaled by assets, with the exception of *Tobin's Q*, are expressed as percentage points.

[20] Note that the chart shows a minimum frequency of two since a firm must appear at least twice in the sample in order not to be subsumed by firm fixed effects.

A comparison of Panel A and Panel B reveals that firms in the LCA sample are on average larger in size and less financially constrained relative to firms in the immigrant enclave sample. This is explained by the fact that the LCA sample consists of firms that file at least one LCA within a tight temporal window. Since larger firms hire more workers, they are more likely to be in the process of hiring H-1B workers at any particular point in time, and hence more likely to be filing LCAs for new H-1B hires around the time of the H-1B petition deadline. The two panels also show that firms from the LCA sample are more likely to be in a high H-1B usage industry relative to firms from the immigrant enclave sample (55.3% in Panel A versus 28.5% in Panel B) as reflected by the differences in *IndH1B*.

Panel A further lists the LCA-based measures of H-1B rationing: *PreCapLCA* represents the number of LCAs certified in the three-day window before the petition deadline, *PostCapLCA* represents the number of LCAs certified in the three-day window after the petition deadline, and *PrePostDiffLCA* is defined as *PostCapLCA* minus *PreCapLCA*. The sample mean for *PrePostDiffLCA* is close to zero, indicating that LCAs certified before and after the petition deadline are evenly balanced,[21] but the large standard deviation indicates substantial variation in the relative timing of LCA certification dates across firms. While *PrePostDiffLCA* forms the main explanatory variable in my LCA rationing tests, I also include *PostCapLCA* and *PostCapLCA* as separate explanatory variables in an alternative specification.

Panel A also lists summary statistics for the average number of business days between submission and certification for LCAs submitted around the petition deadline (*ProcessTime*) and for the number of days between the petition deadline and its announcement to the public (*AnnounceDelay*). The sample mean of 1.706 for *ProcessTime* and 1.828 for *AnnounceDelay*

[21] This also corroborates the visual evidence from Figure 3.

indicates that the average firm in the sample waits approximately 3.5 days after submitting an LCA before finding out whether it was in time for the petition deadline. This further supports the choice of a 3-day window around the petition deadline.

I check for systematic differences between firms that differ on *PreCapLCA*, *PostCapLCA*, and *PrePostDiffLCA*. The first five rows of Table III presents correlations between each LCA measure with lagged $ln(Assets)$, lagged $Tobin's\ Q$, lagged *Cash Flows*, lagged *LT Leverage*, and lagged *Cash Holdings*, respectively. The first two columns show that the number of LCAs certified before and after the petition deadline are significantly correlated with firm characteristics, notably size. This is unsurprising, as the rate of hiring, and hence the number of LCA filings, is likely to scale with firm size. The third column, however, reveals that *PrePostDiffLCA*, the difference between LCAs certified before and after the petition deadline, is uncorrelated to lagged firm characteristics.

The remaining rows of Table III present additional correlations involving LCA-related variables. The sixth row shows a marginally statistically significant relationship between *PrePostDiffLCA* and *ProcessTime*, which indicates that delay in the Department of Labor's processing of LCAs contributes to firms receiving LCA certification after the petition deadline. The last two rows show that *PrePostDiffLCA* is uncorrelated with the total number of LCAs submitted during an entire application cycle (*TotApps*) and with the average number of workers listed per LCA (*WorkersPerApp*), which suggests that the longer processing delays are not driven by light users of the H-1B visa program being less familiar with the application process.

Returning to Table II, Panel B lists *PctIndic*, the proportion of Indic language speakers residing as of 1990 in the commuting zone containing firm headquarters expressed in percentage points. The mean indicates that Indic speakers in 1990 made up less than half a percent

of the total population in 1990, but the larger standard deviation suggests significant variation across commuting zones. The last variable reported in Panel B is *Post*, an indicator variable for whether a given fiscal quarter comes after the petition deadline for fiscal year y but before the April 1 opening of the petition window for fiscal year $y + 1$. The calendar quarters during which submission deadlines occur each year are listed in Table I. I drop quarters during which the petition deadline occurred, as it is unclear whether these should be counted as before or after the deadline. The reported mean for *Post* in Panel B indicates that close to 50% of observations in the immigrant enclave sample is in the post-deadline period.

5 Results

In this section, I present and discuss the results of my analyses on H-1B rationing using the two empirical strategies discussed in Section 3.

5.1 The Effects of H-1B Rationing

I estimate Eq. 1 to test the effect of H-1B rationing on corporate investment and present the results in Table IV. The first three columns show estimates from regressions that include *PreCapLCA* and *PostCapLCA* separately as explanatory variables. Regardless of whether I include industry-year fixed effects to control for time-varying industry trends at the 4-digit NAICS level (columns (2) and (3)) or firm-level control variables (column (3)),[22] *PreCapLCA* is never statistically significant while *PostCapLCA* is always significant at a 1% level. This

[22] All control variables are lagged by one period relative to the dependent variable *CapEx* except for *Cash Flows*, which is contemporaneous to account for the sensitivity of investment to cash flows, following the literature on financial constraints and investment.

indicates that only LCAs certified *after* the petition deadline are associated with decreases in capital expenditures, which is evidence in support of the skill rationing hypothesis.

Columns (4) to (6) show estimates from regressions with *PrePostDiffLCA* as the only explanatory variable of interest. Again, regardless of whether I include industry-year fixed effects (columns (5) and (6)) or firm-level control variables (column (6)), the negative estimates are all significant at the 1% level. In the most rigorous specification provided in column (6), the estimated coefficient implies that a firm with one additional LCA certified after the petition deadline, keeping the number of LCA certifications before the petition deadline constant, will lower their capital expenditure rate by 0.00124 percentage points in the coming fiscal year.

The estimated skill rationing effect on investment is precisely estimated with tight standard errors,[23] but small in economic magnitude: the point estimate from column (6) represents a 0.00114 percent drop in *CapEx* relative to its sample mean of 1.0836 percentage points. Considering that the average LCA in the sample lists 2 prospective workers, the economic magnitude is reasonable, as one would not expect losing two potential hires to have an outsized effect on investment. For the average firm in the LCA sample, with \$16.726 billion in assets, and the average LCA that lists two prospective H-1B workers, my estimate implies approximately \$100,000 ($0.0000124 \div 2 \times \$16.726B$) in reduced capital expenditures per rationed H-1B worker.

While the estimated *per-LCA* effect is small, it is important to consider the aggregate economic effect by extrapolating across all workers who are denied H-1B visa across the entire fiscal year, not just around the tight window around the petition window.[24] Thus, to

[23] I double cluster standard errors by firm and by industry-year, to account for correlated within-year industry shocks and correlated errors within firm across time. The results remain unchanged if I cluster by firm only.

[24] One can think of the narrow window around the petition deadline, which is crucial for identification, as the "tip of the iceberg" that provides a glimpse at a small part of a much larger phenomenon.

contextualize the economic importance of my results, I conduct the following back-of-the-envelope calculation based on fiscal year 2019. In that year, the USCIS received 190,098 cap-subject H-1B petition for 85,000 available visas (65,000 regular slots plus an additional 20,000 for advanced degree holders). Using 100,000 as a conservative estimate for number of rationed visas,[25] we can scale up the $100,000 estimated reduction in capital expenditures per rationed H-1B worker to arrive at an aggregate effect on the order of $10 billion per year.

I show that my main finding is robust to alternative definitions of *PrePostDiffLCA* using windows of different lengths around the petition deadline. Table V presents the estimation results of the of regressions that involve *PrePostDiffLCA* defined based on windows of +/-1 day, +/-2 days, +/-3 days, +/-4 days, and +/-5 days, respectively. The first four columns show that the negative relationship between *PrePostDiffLCA* and investment is statistically significant only up to a +/-4 day window definition. The estimate is less statistically significant in column (4) compared to the first three columns, and no longer significant in column (5). This is consistent with bias and noise being introduced by the high degree of imbalance between LCAs certified 4+ days before and 4+ days after the petition deadline, as discussed in Section 3.2 and visually illustrated in Figure 3.

While the main focus of my paper is on corporate investment in capital expenditures, i.e., tangible capital, I also investigate how skill rationing affects firm performance, financial policy, and investment in intangible capital. Table VI reports the results of estimating Eq. 1 on several firm-level variables. I include only lagged $ln(Assets)$ as a control variable in the $Tobin's\ Q$ regression reported the first column, and add lagged $Tobin's\ Q$ as a control variable in regressions reported in the remaining columns, but my results remain qualitatively

[25] Note the difference between petitions and available visas does not account for potential applicants that missed the application deadline, so the true excess demand for visas is likely to be significantly higher than 100,000.

unchanged with additional control variables.

Column (1) shows that H-1B rationing has a statistically significant negative effect on *Tobin's Q*. In terms of economic magnitude, a one standard deviation increase in *PrePostDiffLCA* is associated with a 0.0834 decrease in *Tobin's Q*, which represents a 3.57 percent decrease in *Tobin's Q* relative to its sample mean of 2.337. This indicates a modest effect on valuation, which suggests that the adverse effect of skill rationing is quickly reflected in equity prices. It is unclear, however, whether equity prices adjust from investors directly observing the rationing of H-1B workers, which is unlikely given the discrete nature of internal hiring processes, or from investors making indirect inferences from observing rationed firms cut back on investment.

The remaining columns of Table VI show that skill rationing does does not significantly affect operating performance, financial policy or investment in intangible capital. With respect to operating performance, the negative coefficient in column (2) implies a negative effect on *Cash Flows*, but it is only weakly significant at the 10% level. The insignificant estimates in columns (3) and (4) further indicate no detectable effects on leverage or cash holdings. The positive coefficient in column (5) implies a *positive* effect on research and development,[26] but this is weakly significant and the insignificant estimate for *SG&A* in column (6) does not support an effect on investment in intangible capital. Lastly, the positive estimate in column (7) indicates no detectable effect on acquisitions expenditures.[27]

[26] A potential explanation for the positive effect on R&D is that firms rationed from H-1B hiring turn to more expensive domestic workers. Another possibility is that firms spend more on accumulating intangible capital in order to compensate for the loss in human capital.

[27] The contrast between the findings on capital expenditures and acquisition expenditures may be explained by two factors. First, acquisitions are infrequent effects that are more difficult to detect in a limited sample. Second, firms may make acquisitions in order to acquire skilled labor (Ouimet and Zarutskie, 2020) from other firms in response to losing access to skilled H-1B workers.

5.2 Heterogeneous Effects of H-1B Rationing

I investigate how the negative effect of H-1B rationing on investment varies across different industries, time periods, and firms. First, I check whether the effects are more pronounced for industries that are more dependent on H-1B workers as a source of skilled labor. To this end, I estimate Eq. 1 with added indicator variables related to industry-level H-1B usage, both by themselves and interacted with *PrePostDiffLCA*, and present the results in the first two columns of Table VII. The negative and significant estimate for the interaction term involving *IndH1B*, in column (1) indicates a more pronounced effect for high H-1B usage industries. Column (2) shows a similar finding, using *IndTech*, a dummy variable indicating whether a firm belongs to a high-technology industry,[28] as an alternative indicator of H-1B dependence.

Next, I explore temporal differences in the severity of H-1B rationing. First, I compare periods in which the petition deadline arrived before the October 1 start of the fiscal year against periods in which the deadline arrived afterwards. For example, the deadline of Aug 10, 2005 arrived before the start of FY2006, while the deadline of Feb 17, 2004 arrived after the start of FY2004. The length of rationing is longer in the former case, in which hiring is delayed for at least one year until FY2007 (starting Oct 1, 2006), relative to the latter case, in which the potential wait until FY2005 (starting Oct 1, 2004) is shorter than one year. The negative coefficient in column (3) of Table VII shows a negative estimate for the interaction term involving *PostOctDeadline*, a dummy variable indicating whether the petition deadline arrived after October 1, that is consistent with the rationing effect being attenuated when the length of the hiring delay is shortened.

[28] According to the BLS 4-digit NAICS-based definition of high-technology industries from Heckler (2005): 3341, 3342, 3344, 3345, 5112, 5161, 5179, 5181, 5182, 5415, 3254, 3364, 5413, 5417.

I also check whether the negative effect on investment is related to the length of the delay in the USCIS's announcement of the petition deadline to the public. Column (4) shows the estimation results from including an interaction term involving *HighDelay*, a dummy variable indicating that the USCIS waits more than one day after it stops processing H-1B petitions before announcing it to the public. While the coefficient estimate on *HighDelay* × *PrePostDiffLCA* is indeed negative, consistent with a sharper impact associated with a greater degree of surprise, it falls just below the threshold of statistical significance. Note that if my main results are driven by disorganized firms with poor investment prospects being habitually late in submitting LCAs, then one should expect the effect to more pronounced in years with *shorter* announcement delays.

Lastly, I examine the role of financial slack, and check whether the negative effects on investment is more or less pronounced for firms with greater amounts of financial slack. To the degree that a firm with limited financial resources is financially constrained, imposing additional hiring constraints may be redundant in impeding investment. If financial and hiring constraints are "substitutable" in this manner, then we should expect the negative effects on investment to be more pronounced for firms with greater financial slack.

In column (5), the positive coefficient on the interaction term involving *HighLev*, a dummy variable indicating whether a firm's long-term leverage ratio (*LT Leverage*) is above the within-fiscal-year sample median, suggests that the negative effects on investment are attenuated for firms with high leverage ratios. In column (6) the negative coefficient on the interaction term involving *HighCash*, a dummy variable indicating whether a firm's cash holdings (*Cash Holdings*) is above the within-fiscal-year sample median, suggests that the negative effects on investment are concentrated for firms with high levels of cash holdings. Both results support the interpretation that hiring constraints become redundant when a

firm lacks the financial resources to pursue investment opportunities.

5.3 H-1B Rationing and Investment Dynamics

I study the dynamics of how LCA rationing affects investment to a) investigate the temporal persistence of my benchmark results, and b) check for the existence of pre-trends. To this end I estimate Eq. 1 with *PrePostDiffLCA* defined at different time lags with respect to *CapEx* and present the results in Table VIII. Note that I include the lagged *PrePostDiffLCA* measures in different regressions rather than in a single regression due to the unbalanced panel structure of the data.[29] Specifically, a single regression with multiple lags of *PrePostDiffLCA* will severely restrict the sample to the very few firms that receive at least one LCA certification around the petition deadline in *consecutive* fiscal years.

The first two columns of Table VIII show that both *PrePostDiffLCA* at $t-2$ and $t-1$ have significant negative effects on *CapEx* at time t. This indicates that, for example, firms that are rationed from submitting H-1B petitions around the May 26, 2006 deadline for FY2007 lower their capital expenditure rates not only for FY2007 (as confirmed in column (3)), but for FY2008 and FY2009 as well. This persistence may be explained by the permanent nature of losing a difficult-to-replace prospective H-1B employee, who takes on another employment opportunity instead of waiting an additional year to reapply. Alternatively, the persistence may be due to temporal knock-on-effects: delaying the hiring of an H-1B worker by one year means an additional H-1B petition to submit the following year, in which case the possibility of being rationed still remains.

Column (4) of Table VIII shows that *PrePostDiffLCA* at $t+1$ is also negatively related to *CapEx* at time t. While this appears to call into question the validity of my empirical

[29] I also do not include lagged control variables here due to lagged control variables such as *Tobin's Q* being affected by *PrePostDiffLCA* at large enough lags.

design, it is explained by the overlap between petition deadlines for fiscal year t with fiscal year $t-1$. For example, firms rationed from submitting H-1B petitions around the May 26, 2006 deadline for FY2007 may reduce their investment in the last quarter of FY2006, which ends on September 30, 2006. Since investment decisions are forward looking, any years in which the petition deadline arrives before the end of the previous fiscal year may trigger an "early" reaction in investment policy. When I drop such "overlap years" are from the sample,[30] the relationship between *PrePostDiffLCA* at $t+1$ and *CapEx* at time t is positive and no longer significant, as seen in column (5). As there is no concern of overlap between *PrePostDiffLCA* at $t+2$ and *CapEx* at time t, estimate in column (6) is not statistically significant.

To obtain more precise timing over the effects of H-1B rationing, I examine differences in *quarterly* firm outcomes (instead of annualized outcomes) within an annual application cycle, where application cycles are defined to start on April 1, when the USCIS opens its petition window.[31] Specifically, I estimate Eq. 1 at a quarterly frequency for two separate sub-samples: one restricted to quarters before the petition deadline within an application cycle, and one restricted to quarters after the petition deadline. For example, the petition of November 22, 2011 for FY2012 means that 2011Q2 and 2011Q3 would constitute the "pre-deadline" quarters, and 2011Q4 and 2012Q1 would constitute the "post-deadline' quarters for the FY2012 application cycle.

Table IX presents estimation results for the two subsample analyses. The first two columns show no evidence that *PrePostDiffLCA* affects quarterly *CapEx* and *Tobin's Q* before the petition deadline, but the next two columns show significant negative effects on

[30] Overlap years consist of FY2005, FY2006, and FY2012. See Table I.

[31] To be precise, the application cycle for FY t would start begin on April 1 of calendar year $t-1$ and end on March 31 of calendar year t.

both quarterly measures after the petition deadline. In columns (5) and (6), I estimate a difference-in-differences test by using all quarters in the sample and including an interaction term between *PrePostDiffLCA* and *Post*, where *Post* is a dummy variable indicating post-deadline quarters. The results again show that *CapEx* and *Tobin's Q* are unaffected by *PrePostDiffLCA* prior to the petition deadline, but are negatively affected following the deadline.

The results of this within-year analysis should alleviate concerns that *PrePostDiffLCA* is endogenous to pre-existing differences in investment opportunities. The quarterly frequency of the analysis further suggests that firms and investors react quickly to H-1B rationing after it has occurred, consistent with the forward-looking nature of investment decisions and equity valuations.

5.4 H-1B Application Cycles and Investment Dynamics

I investigate whether firms located in regions that are differentially exposed to cycles of H-1B labor availability also exhibit different cycles of investment. First, I verify that that immigrant enclaves indeed predict more pronounced cycles of H-1B hiring as described in Section 3.3. To this end, I obtain data on H-1B petitions from a Freedom of Information Act (FOIA) request filed with the USCIS, and construct quarterly measures of petition counts at the commuting zone level. I then regress these quarterly measures on the interaction between *PctIndic*, the historical concentration of Indic language speakers, and *Post*, a dummy variable indicating that the observation comes after the petition deadline within the annual application cycle. I include commuting zone and year fixed effects, where year is defined according to H-1B application cycle (i.e., beginning on April 1), in order to obtain estimates that reflect investment changes *within* an application cycle across different commuting zones.

I present the results in Table X. The negative coefficient on *Apps* reported in column (1) indicates that employers in commuting zones with higher historical concentrations of Indic language speakers submit fewer total H-1B petitions following the petition deadline relative to commuting zones with lower historical concentrations. The coefficient is statistically significant at a 1% level, and the magnitude of the point estimates indicates that a one standard deviation change in *PctIndic* (0.101 percentage points)[32] is associated with a relative decrease of 120.6 total H-1B petitions across the petition deadline each application cycle. Consistent with the binding nature of the petition deadline, this represents a substantial difference in cyclical H-1B hiring as the average commuting zones submits only 110 petitions per quarter in the sample.

Column (2) of Table X shows that the number of petitions for H-1B visa extensions (*AppsExtend* does not change following the petition deadline, but column (3) shows that petitions for initial employment (*AppsNew*) decreases following the petition deadline. Given that only visas for new H-1B hires are subject to the cap, these results confirm the differential exposure of high *PctIndic* and low *PctIndic* regions to cycles in H-1B hiring.

I next investigate within-cycle changes in investment by estimating Eq. 2, and present the results in Table XI. The first two columns show results from regressing *CapEx* on *PctIndic* $\times$ *Post* for the sample of low H-1B usage firms (*IndH1B* $= 0$) and high H-1B usage firms (*IndH1B* $= 1$), respectively. By including the necessary fixed effects, I estimate coefficients that represent within-year changes in investment for a given firm in a given industry and commuting zone.[33] Only the positive estimate in column (2) is statistically significant, which indicates that firms in high *PctIndic* regions increase investment after the petition deadline

[32] Note this is different from the standard deviation reported in Panel B of Table II as the commuting zone panel used here is different from the firm panel on which Panel B is based.

[33] Firm-year and commuting zone-industry-year fixed effects are both included because firms change industries.

within each H-1B application cycle relative to firms in low *PctIndic* regions, but *only* if they operate in an industry that hires H-1B workers at a significant rate.

This suggests that firms more exposed to annual cycles of H-1B availability *delay* investment until the temporary uncertainty over the H-1B hiring is resolved, in support of the hiring uncertainty effect dominating the skill rationing effect as discussed in Section 3.3. The point estimate of the coefficient on $PctIndic \times Post$ in column (2) indicates that a one standard deviation increase in *PctIndic* (0.259 percentage points) is associated with investment increasing by 0.04 percentage points after the petition deadline each H-1B application cycle. This represents a 2.86% increase relative to the *CapEx* sample mean of 1.412 percentage points.

In the last three columns of Table XI, I present the results of "triple differences" regressions to show that the difference in investment cycles between firms in high *PctIndic* and low *PctIndic* regions is statistically different between high H-1B usage and low H-1B usage industries. In each of the last three columns, the coefficient on $PctIndic \times IndH1B \times Post$ is significant and positive, albeit at a lower statistical significance in column (6). This last estimate, however, comes from the most rigorous fixed effects specification, in which the inclusion of industry-year-quarter and commuting zone-year-quarter fixed effects controls for all time-varying within-year factors at the regional and industry level.

6 Conclusion

In this paper, I find evidence that access to skilled immigrant workers is an important determinant of corporate investment policy. Using restrictions imposed by the H-1B visa program, I find that rationing of skilled hiring induces firms to decrease investment, consistent with

complementarity between skilled labor and capital. The effect is more pronounced for industries that are more reliant on H-1B workers for skilled labor, during times periods in which rationing lead to longer hiring delays, and for firms with the slack financial resources to pursue investment opportunities if not for hiring constraints.

I further find that firms that are more exposed to cycles of H-1B visa availability systematically increase investment after the annual government quota for visas is reached, which suggests that ex-ante uncertainty over hiring is a stronger driver of corporate investment decisions than the ex-post rationing of workers. This connection between hiring uncertainty and investment delay constitutes a novel channel linking immigration policy and economic outcomes that holds promise for further study in the future.

In addition to advancing our understanding of the relationship between labor and capital, my research also provides practical policy implications. Specifically, my findings suggest that, rather than focusing only on the immediate impacts on domestic employment and wage growth when evaluating immigration policy, policymakers should also consider the long-term impact on capital accumulation and its subsequent implications for overall economic growth. They should also consider how volatile swings in skilled immigration policy may have a large chilling effect on investment, which is especially relevant in light of recent presidential executive orders temporarily suspending the granting of H-1B visas.

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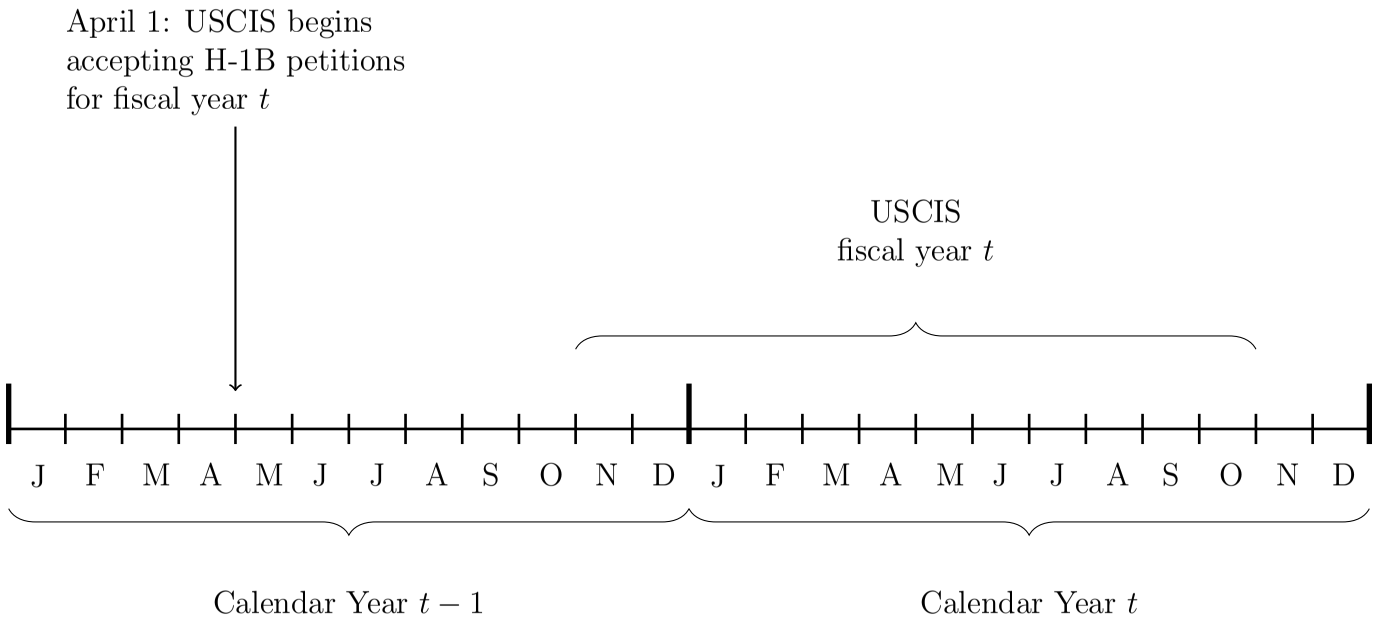
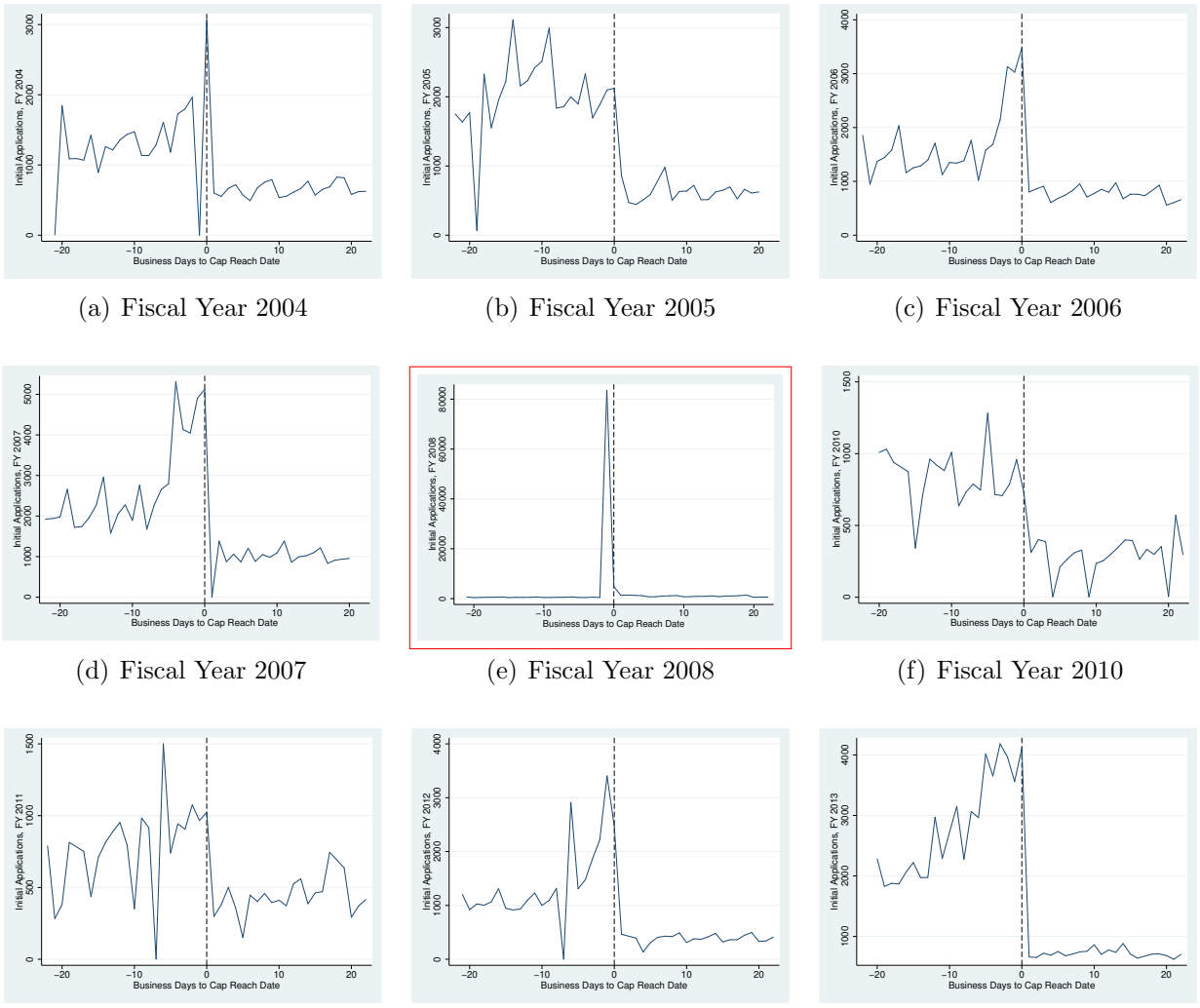


Figure 1: Timeline of H-1B application and hiring

This figure shows the timeline of when the USCIS begins accepting H-1B petitions and the corresponding fiscal year for starting employment.



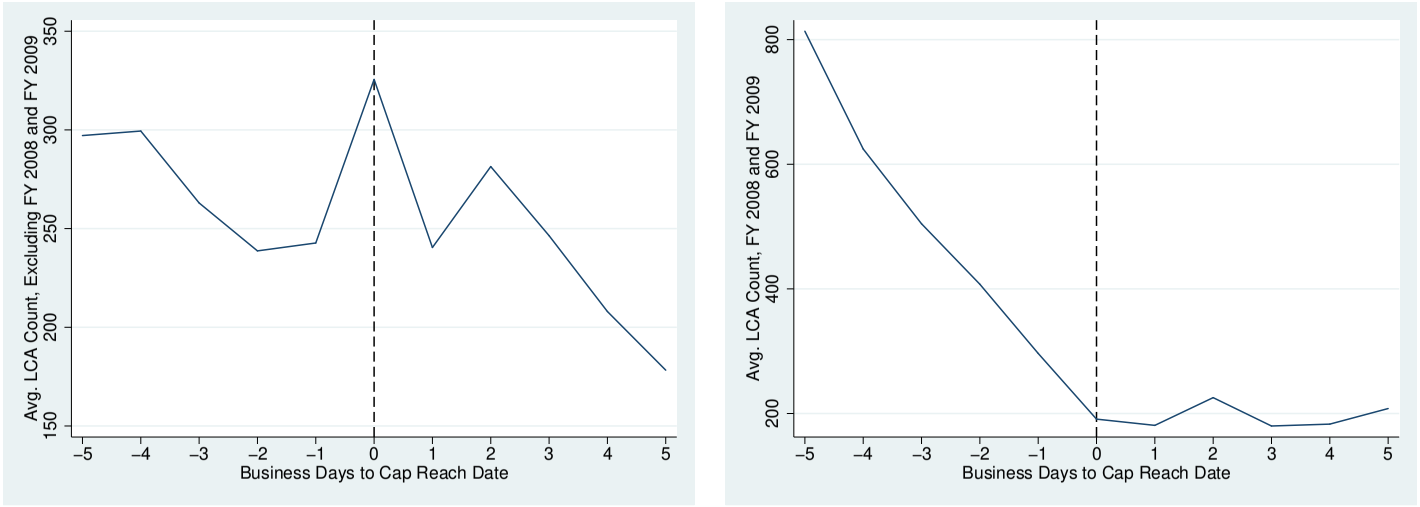
(a) Fiscal Year 2004 (b) Fiscal Year 2005 (c) Fiscal Year 2006

(d) Fiscal Year 2007 (e) Fiscal Year 2008 (f) Fiscal Year 2010

(g) Fiscal Year 2011 (h) Fiscal Year 2012 (i) Fiscal Year 2013

Figure 2: H-1B Applications for Initial Employment by Number of Days to USCIS Petition Deadline

These figures plot number of H-1B applications for initial employment by date submitted to the USCIS for each government fiscal year from FY 2004 to FY 2013, excluding FY 2009. In every plot, the horizontal axis represents the number of business days between the petition submission date and the USCIS H-1B petition deadline for that fiscal year, and the vertical axis represents the total number of H-1B petitions for initial employment submitted on that date. Data on H-1B petitions comes from the USCIS via a Freedom of Information Act (FOIA) request.



(a) LCAs Processed FY2004-FY2013 (excl. FY2008 and FY2009)

(b) LCAs Processed FY2008 and FY2009

Figure 3: Average Number of LCAs Processed by Number of Days to Petition Deadline

These figures plot the total number of H-1B labor condition applications (LCAs) processed on the vertical axis by the number of business days to the USCIS H-1B petition deadline on the horizontal axis. Figure (a) shows averages for all years between FY2004 and FY2013 excluding FY2008 and FY2009, and figure (b) shows averages for FY2008 and FY2009 only. Both subfigures represent LCAs submitted by publicly-traded companies only.

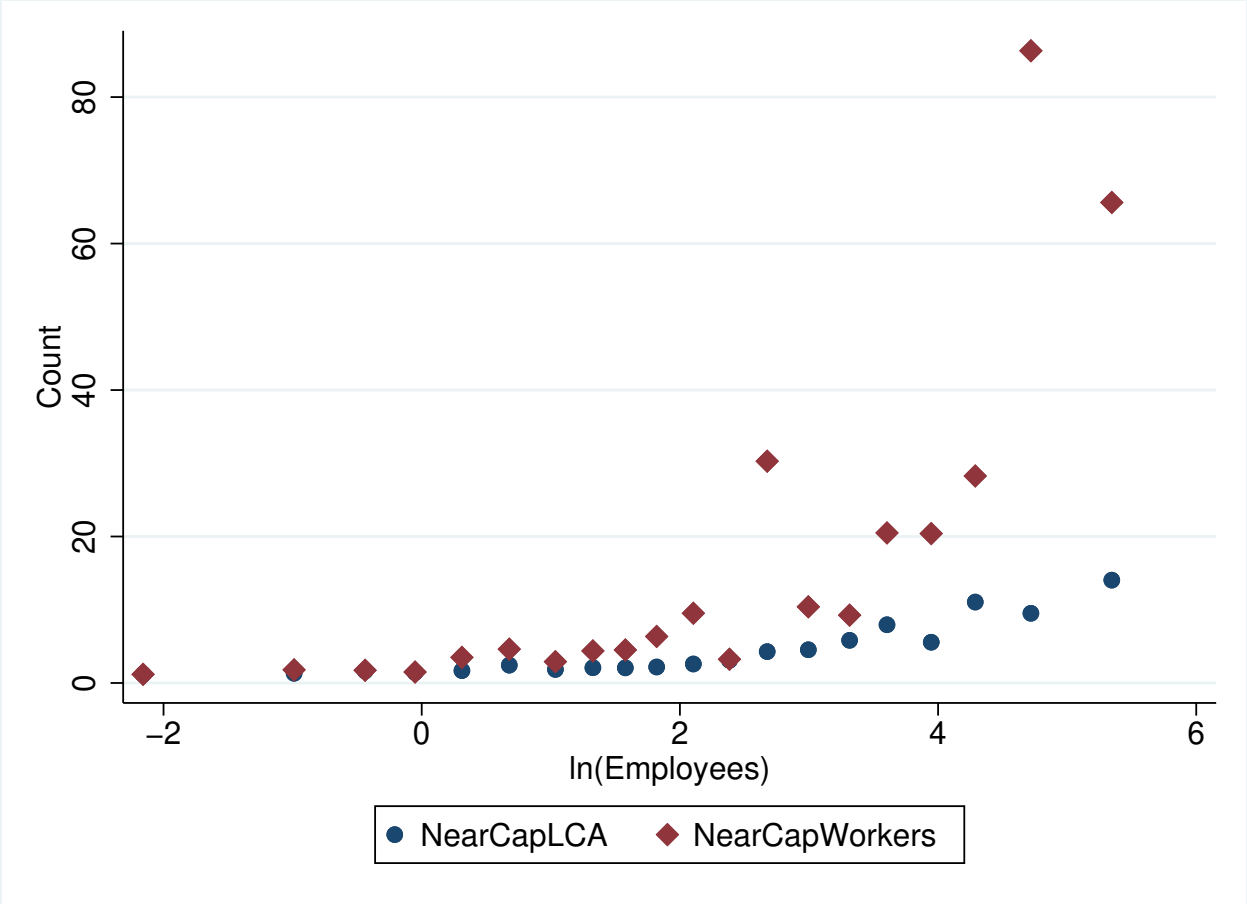
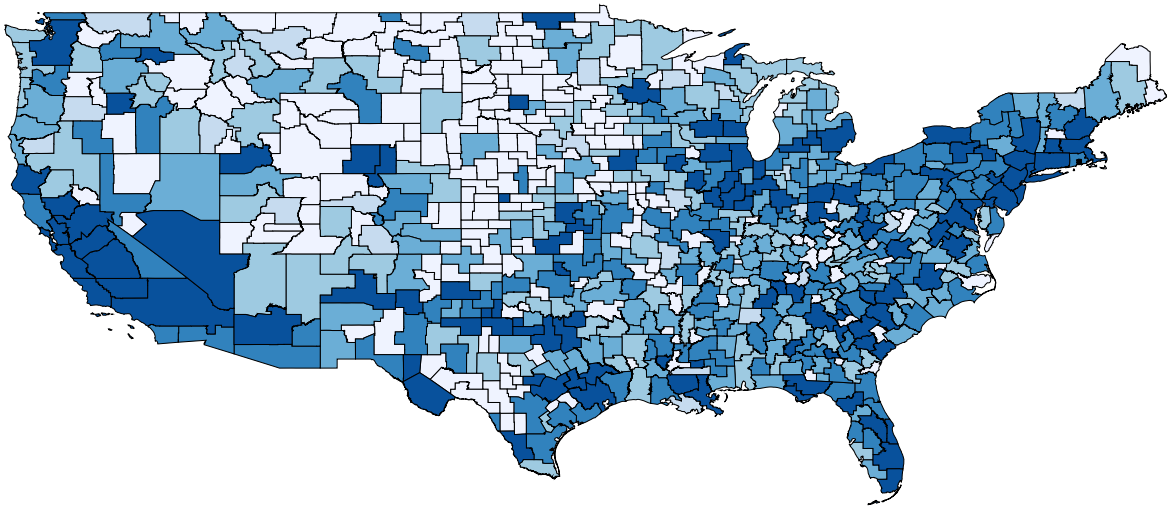
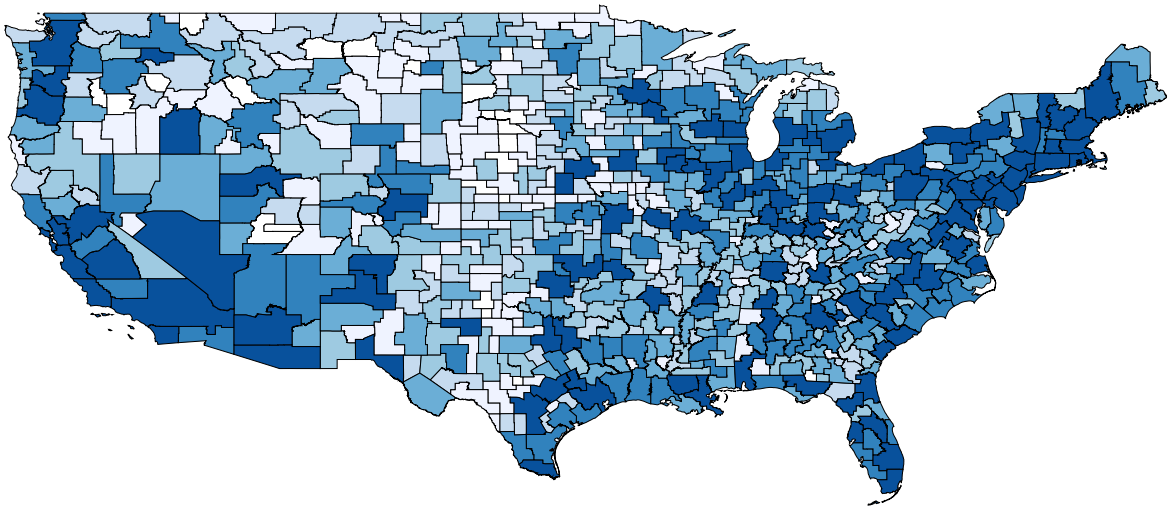


Figure 4: Log Employment vs. LCA Applications and LCA Workers

This figure is a bin scatterplot that plots the natural log of annual firm employment on the horizontal axis against the number of labor conditions applications (blue circles) and the total number of employees listed on labor condition applications (red diamonds) submitted in the six day window around the USCIS H-1B petition deadline on the vertical axis.



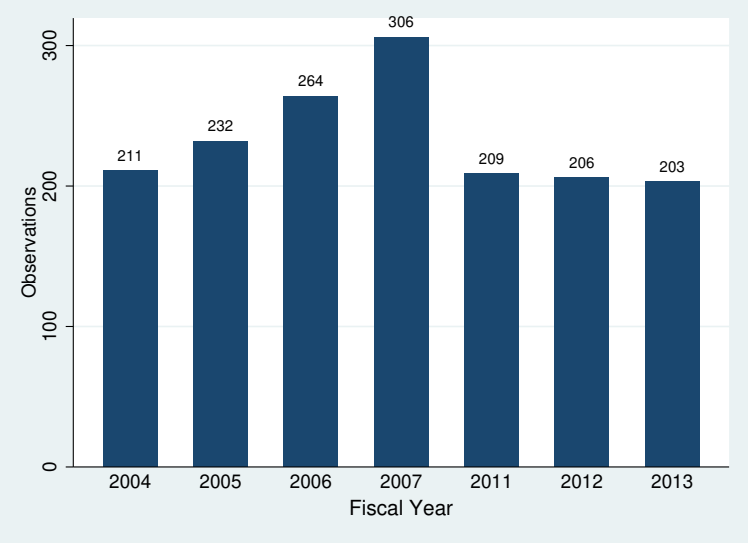
(a) Proportion of Population Speaking Indic Language in 1990 by Commuting Zone



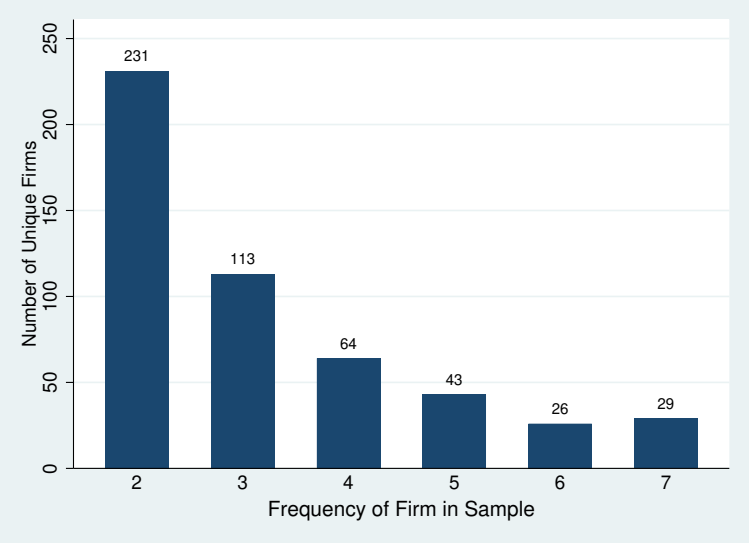
(b) Average Annual H-1B Petitions (2004-2013) by Commuting Zone

Figure 5: Geographic Distribution of Indic Language Speakers and H-1B Petitions

This figure shows commuting zone variation in *PctIndic* (the proportion of residents speaking an Indic language in 1990) in subfigure (a) and in *PetitionsNew* (the average annual number of H-1B petitions for initial employment) in subfigure (b). Darker shades represent greater concentrations of Indic speaker and higher average H-1B petitions, respectively. Data on commuting zone shapes comes from the Health Inequality Project (`https://healthinequality.org/data`).



(a) LCA Sample Size by FY



(b) Number of Firms by Frequency in LCA Sample

Figure 6: Panel Structure of LCA Sample

These figures show cross-tabulation results related to the sample structure of the LCA Sample. Subfigure (a) illustrates the number of observations by fiscal year. Subfigure (b) illustrates the number of firms by how frequent they appear in the sample.

Table I: USCIS Petition Deadline for H-1B Visas by Year

This table presents a list of annual dates and corresponding calendar quarters, for each year from FY2004 to FY2020, on which the USCIS announced they would no longer accept new cap-subject H-1B petitions for initial employment for the coming fiscal year. Data on petition deadline dates comes from the USCIS News Archives, available at `https://www.uscis.gov/archive`.

Fiscal Year	Submission Deadline Date	Announcement Delay	Submission Deadline Calendar Quarter
2004	Feb 17, 2004	0 days	2004Q1
2005	Oct 1, 2004	0 days	2004Q4
2006	Aug 10, 2005	2 days	2005Q3
2007	May 26, 2006	6 days	2006Q2
2008	Apr 3, 2007	0 days	2007Q2
2009	Apr 7, 2008	0 days	2008Q2
2010	Dec 21, 2009	1 days	2009Q4
2011	Jan 26, 2011	1 days	2011Q1
2012	Nov 22, 2011	1 days	2011Q4
2013	Jun 11, 2012	1 days	2012Q2
2014	Apr 5, 2013	3 days	2013Q2
2015	Apr 7, 2014	0 days	2014Q2
2016	Apr 7, 2015	0 days	2015Q2
2017	Apr 7, 2016	0 days	2016Q2
2018	Apr 7, 2017	0 days	2017Q2
2019	Apr 6, 2018	0 days	2018Q2
2020	Apr 5, 2019	0 days	2019Q2

Table II: Descriptive Statistics

This table presents summary statistics for the main variables in my regressions. Panel A presents statistics for the LCA sample, which consists of firm-year observations for years FY2004–FY2013, excluding FY2008–FY2010. Panel B presents statistics for the immigration which consists of firm-quarter observations for years FY2004–FY2013, excluding FY2008 and 2009. Definitions for all variables are provided in Section 4.2. All Compustat variables are winsorized at the 1% level at both tails. All Compustat variables in Panel A are annual averages of quarterly measures, in which years are defined according to the USCIS fiscal calendar (i.e., beginning Oct 1). The LCA sample in Panel A is restricted to firms that received at least one LCA certification around a six day window around the H-1B petition deadline each year.

Panel A: LCA Sample

	Observations	Mean	Std Dev	Median
CapEx	1,631	1.0836	0.9951	0.7776
IndH1B	1,631	0.5530	0.4973	1.0000
PreCapLCA	1,631	2.9203	12.5539	1.0000
PostCapLCA	1,631	2.4372	15.7938	1.0000
PrePostDiffLCA	1,631	-0.4831	13.7420	-1.0000
ProcessTime	1,623	1.7062	2.2066	0.0000
AnnounceDelay	1,631	1.8283	2.1072	1.0000
ln(Assets)	1,631	8.0967	1.8984	8.1261
Tobin's Q	1,626	2.3365	1.4046	1.8832
Cash Flows	1,603	3.5245	2.8019	3.4665
LT Leverage	1,622	13.3374	14.4444	10.4316
Cash Holdings	1,629	24.7710	20.2309	19.5622
FC (KZ)	1,629	0.1381	0.3451	0.0000
FC (WW)	1,629	0.0460	0.2096	0.0000
FC (HP)	1,629	0.0270	0.1622	0.0000

Panel B: Immigrant Enclave Sample

	Observations	Mean	Std Dev	Median
CapEx	70,843	1.4123	2.1396	0.7549
PctIndic	70,843	0.3283	0.2594	0.2501
IndH1B	70,843	0.2847	0.4513	0.0000
Post	70,843	0.4619	0.4985	0.0000
ln(Assets)	67,760	6.2854	1.9724	6.2504
Tobin's Q	62,798	2.8198	8.6287	1.6295
Cash Flows	68,466	0.9252	16.1648	2.9646
LT Leverage	67,093	20.3983	24.8854	13.1168
Cash Holdings	67,279	23.0569	27.7452	12.6489
FC (KZ)	70,665	0.2609	0.4391	0.0000
FC (WW)	70,665	0.1986	0.3989	0.0000
FC (HP)	70,665	0.1829	0.3866	0.0000

Table III: Correlations between LCA Submissions and Firm Characteristics

This table presents correlations between each LCA-based measures (*PreCapLCA*, *PostCapLCA*, *PrePostDiffLCA*, *ProcessTime*, *AnnounceDelay*, *TotApps*, *WorkersPerApp*) and select Compustat variables (*ln*(*Assets*), *Tobin's Q*, *Cash Flows*, *LT Leverage*, *Cash Holdings*). All Compustat variables are lagged with respect to the LCA measures by one fiscal year. Definitions for all Compustat variables and LCA-based measures of rationing are provided in Section 4.2. *ProcessTime* represents the average number of working days between LCA submission and LCA certification across LCAs certified within a six-day window around the petition deadline, *AnnounceDelay* represents the number of days between the petition deadline and the announcement of the petition deadline for a given fiscal year, *TotApps* represents the total number of LCAs submitted during the entire application cycle, and *WorkersPerApp* represents the average number of workers listed per LCA across LCAS certified within a six-day window around the petition deadline. P-values are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) PreCapLCA	(2) PostCapLCA	(3) PostPreDiffLCA
ln(Assets)	0.0854***	0.0622***	-0.0054
	(0.0000)	(0.0000)	(0.7805)
Tobin's Q	0.1284***	0.0715***	0.0083
	(0.0000)	(0.0000)	(0.6707)
Cash Flows	0.0870***	0.0462***	-0.0018
	(0.0000)	(0.0001)	(0.9277)
LT Leverage	-0.0722***	-0.0339**	0.0064
	(0.0002)	(0.0033)	(0.7418)
Cash Holdings	0.0695***	0.0363**	0.0107
	(0.0003)	(0.0018)	(0.5836)
ProcessTime	-0.0050	0.0496	0.0753*
	(0.8880)	(0.0687)	(0.0331)
AnnounceDelay	0.0503**	0.0315**	-0.0075
	(0.0089)	(0.0062)	(0.6988)
TotApps	0.4390***	0.3343***	-0.0400
	(0.0000)	(0.0000)	(0.0645)
WorkersPerApp	0.1854***	0.1456***	0.0479
	(0.0000)	(0.0000)	(0.1738)

Table IV: Effect of H-1B Rationing on Investment

The table below reports estimated coefficients for regressions of *CapEx* (capital expenditure scaled by lagged assets) on LCA-based measures of H-1B rationing. Columns (1) to (3) report separate coefficients for *PreCapLCA* and *PostCapLCA* (number of LCAs processed before and after the petition deadline, respectively), and columns (4) to (6) report a single estimate for *PrePostDiffLCA* (*PostCapLCA* minus *PreCapLCA*). All other coefficients listed represent firm-year control variables, and are lagged by one fiscal year relative to *CapEx*, with the exception of *Cash Flows*. Detailed definitions for all variables can be found in Section 4.2. Fixed effects are indicated by "X" markers at the bottom of the table. Reported number of observations exclude singleton observations within fixed effects groups. Standard errors, which are corrected for heteroskedasticity and double-clustered at the firm and industry-year level, are shown in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) CapEx	(2) CapEx	(3) CapEx	(4) CapEx	(5) CapEx	(6) CapEx
PreCapLCA	-0.0017	-0.0013	-0.0007			
	(0.0032)	(0.0028)	(0.0024)			
PostCapLCA	-0.0029***	-0.0025***	-0.0016***			
	(0.0001)	(0.0002)	(0.0002)			
PrePostDiffLCA				-0.0022***	-0.0019***	-0.0012***
				(0.0008)	(0.0006)	(0.0005)
ln(Assets)			-0.1322*			-0.1426**
			(0.0690)			(0.0707)
Tobin's Q			0.0994**			0.1015**
			(0.0455)			(0.0456)
Cash Flows			0.0198			0.0197
			(0.0259)			(0.0258)
LT Leverage			0.0006			0.0006
			(0.0027)			(0.0027)
Cash Holdings			-0.0030			-0.0032
			(0.0022)			(0.0023)
FC (KZ)			-0.0191			-0.0184
			(0.1401)			(0.1400)
FC (WW)			0.1128			0.1071
			(0.0962)			(0.0964)
FC (HP)			0.1498			0.1446
			(0.1543)			(0.1556)
Firm FE	X	X	X	X	X	X
Year FE	X			X		
Year × NAICS4 FE		X	X		X	X
Observations	1,631	1,314	1,263	1,631	1,314	1,263
Adjusted R-squared	0.761	0.736	0.754	0.759	0.735	0.753

Table V: Defining H-1B Rationing Using Alternative Window Lengths

The table below reports estimated coefficients for regressions of *CapEx* (capital expenditures scaled by lagged assets) on *PrePostDiffLCA* (the difference between LCAs processed after and before the petition deadline) using windows of different lengths around the petition deadline. Going from left to right in columns (1) through (5), *PrePostDiffLCA* is defined using +/-1 day, +/-2 day, +/-3 day, +/-4 day, and +/-5 day windows, respectively, not including date 0 (i.e., the date of the petition deadline). All specifications include the full set of control variables: $ln(Assets)$, *Tobin's Q*, *Cash Flow*, and *Cash Holdings*, *FC* (*KZ*), *FC* (*WW*), and *FC* (*HP*). Estimates for control variables are not reported to conserve space, and definitions for all control variables are provided in Section 4.2. Fixed effects are indicated by "X" markers at the bottom of the table. Reported number of observations exclude singleton observations within fixed effects groups. Standard errors, which are corrected for heteroskedasticity and double-clustered at the firm and industry-year level, are shown in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) CapEx	(2) CapEx	(3) CapEx	(4) CapEx	(5) CapEx
PrePostDiffLCA (W=1)	-0.0018**				
	(0.0007)				
PrePostDiffLCA (W=2)		-0.0018***			
		(0.0005)			
PrePostDiffLCA (W=3)			-0.0012***		
			(0.0005)		
PrePostDiffLCA (W=4)				-0.0007**	
				(0.0003)	
PrePostDiffLCA (W=5)					-0.0000
					(0.0001)
Control Variables	X	X	X	X	X
Firm FE	X	X	X	X	X
Year × NAICS4 FE	X	X	X	X	X
Observations	417	867	1,263	1,601	1,898
Adjusted R-squared	0.775	0.782	0.753	0.770	0.771

Table VI: Effect of H-1B Rationing on Other Outcomes

The table below reports estimated coefficients for regressions of annual firm-level variables on *PrePostDiffLCA* (the difference between LCAs processed after and before the petition deadline). *R&D* is defined as the annual average of quarterly R&D expenditures scaled by lagged assets, *SG&A* is defined as the annual average of quarterly SG&A expenditures scaled by lagged assets, and *Acquisitions* is defined as the annual average of acquisition expenditures divided by lagged assets. Detailed definitions for all other variables can be found in Section 4.2. Fixed effects are indicated by "X" markers at the bottom of the table. Reported number of observations exclude singleton observations within fixed effects groups. Standard errors, which are corrected for heteroskedasticity and double-clustered at the firm and industry-year level, are shown in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) Tobin's Q	(2) Cash Flows	(3) LT Leverage	(4) Cash Holdings	(5) R&D	(6) SG&A	(7) Acquisitions
PrePostDiffLCA	-0.0053***	-0.0024*	-0.0012	0.0088	0.0033*	-0.0012	0.0016
	(0.0012)	(0.0013)	(0.0083)	(0.0132)	(0.0020)	(0.0020)	(0.0019)
ln(Assets)	-1.2703***	-0.7865***	-0.1146	-9.2535***	-1.4516***	-2.4198***	-0.4816*
	(0.1274)	(0.2150)	(1.1698)	(1.7084)	(0.3051)	(0.3393)	(0.2521)
Tobin's Q		0.5740***	-1.7538***	1.0016	0.2072*	0.1818*	0.0200
		(0.1154)	(0.4679)	(0.8284)	(0.1072)	(0.1097)	(0.0932)
Firm FE	X	X	X	X	X	X	X
Year × NAICS4 FE	X	X	X	X	X	X	X
Observations	1,337	1,283	1,318	1,312	1,041	1,285	1,194
Adjusted R-squared	0.722	0.782	0.731	0.801	0.756	0.903	0.146

Table VII: Heterogeneity in the Effect of H-1B Rationing on Investment

The table below reports estimated coefficients for regressions of *CapEx*, capital expenditures scaled by assets, on *PrePostDiffLCA*, the difference between LCAs processed after and before the petition deadline, and interaction terms involving *PrePostDiffLCA* and various firm-level variables. The regression includes an interaction term involving *IndH1B*, a dummy variable indicating belonging to an industry listed by the USCIS as a major employer of H-1B workers, in column (1), an interaction term involving *IndTech*, a dummy variable indicating belonging to a high-technology industry according to BLS definitions, in column (2), an interaction term involving *PostOctDeadline*, a dummy variable indicating the petition deadline arriving before October 1 during the fiscal year, in column (3), an interaction term involving *HighDelay*, a dummy variable indicating an announcement delay of two or more days, in column (4), an interaction term involving *HighLev*, a dummy variable indicating *LT Leverage* being above the annual median, in column (5), and an interaction term involving *HighCash*, a dummy variable indicating *Cash Holdings* being above the annual median, in column (6). All specification include both the additional variable by itself as well as interacted with *PrePostLCA*, but only the interaction is reported to conserve space. Control variables are not included in any of the regressions across the columns. Fixed effects are indicated by "X" markers at the bottom of the table. Standard errors, which are corrected for heteroskedasticity and double-clustered at the firm and industry-year level, are shown in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) CapEx	(2) CapEx	(3) CapEx	(4) CapEx	(5) CapEx	(6) CapEx
PrePostDiffLCA (t)	0.0104	0.0100*	-0.0022***	-0.0017**	-0.0020***	0.0162**
	(0.0064)	(0.0052)	(0.0007)	(0.0008)	(0.0006)	(0.0076)
IndH1B × PrePostDiffLCA	-0.0126*					
	(0.0064)					
IndTech × PrePostDiffLCA		-0.0122**				
		(0.0052)				
PostOctDeadline × PrePostDiffLCA			0.0061*			
			(0.0032)			
HighDelay × PrePostDiffLCA				-0.0026		
				(0.0019)		
HighLev × PrePostDiffLCA					0.0117***	
					(0.0042)	
HighCash × PrePostDiffLCA						-0.0181**
						(0.0074)
Firm FE	X	X	X	X	X	X
Year × NAICS4 FE	X	X	X	X	X	X
Observations	1,314	1,314	1,314	1,314	1,303	1,310
Adjusted R-squared	0.735	0.735	0.735	0.734	0.734	0.735

Table VIII: H-1B Rationing and Investment Dynamics

The table below reports estimated coefficients for regressions of *CapEx* (capital expenditure scaled by lagged assets) on *PrePostDiffLCA* (the difference between LCAs processed after and before the petition deadline) at different time lags. Going from left to right in columns (1) through (6), *CapEx* for fiscal year t is regressed on *PrePostDiffLCA* for fiscal year $t+2$, $t+1$, t, $t-1$, and $t-2$, respectively. All columns report results based on the full LCA sample with respect to fiscal years included, except for column (5) which restricts the sample to "non-overlap years" by dropping FY2006, FY2007, and FY2012 from the sample. Fixed effects are indicated by "X" markers at the bottom of the table. Reported number of observations exclude singleton observations within fixed effects groups. Standard errors, which are corrected for heteroskedasticity and double-clustered at the firm and industry-year level, are shown in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) CapEx	(2) CapEx	(3) CapEx	(4) CapEx	(5) CapEx	(6) CapEx
PrePostDiffLCA (t-2)	-0.0011**					
	(0.0005)					
PrePostDiffLCA (t-1)		-0.0015***				
		(0.0004)				
PrePostDiffLCA (t)			-0.0019***			
			(0.0006)			
PrePostDiffLCA (t+1)				-0.0019***	0.0053	
				(0.0007)	(0.0039)	
PrePostDiffLCA (t+2)						-0.0009
						(0.0008)
Years in Sample	All Years	All Years	All Years	All Years	Non-Overlap	All Years
Firm FE	X	X	X	X	X	X
Year × NAICS4 FE	X	X	X	X	X	X
Observations	1,172	1,250	1,314	1,334	509	1,310
Adjusted R-squared	0.764	0.732	0.735	0.715	0.613	0.642

Table IX: Pre-Deadline vs. Post-Deadline Investment

The table below reports estimated coefficients for regressions of *CapEx* (capital expenditures scaled by lagged assets) on *PrePostDiffLCA* (the difference between LCAs processed after and before the petition deadline). Regressions for columns (5) and (6) also include an additional interaction term *PrePostDiffLCA*×*Post*, where *Post* is a dummy variable indicating whether the fiscal quarter comes after the petition deadline for the fiscal year t but before the April opening of the petition window for fiscal year $t+1$. Sample restrictions are indicated at the bottom of the table: *Pre* indicates *Post* = 0, *Post* indicates *Post* = 1, and *All* indicates no restrictions. Fixed effects are indicated by "X" markers at the bottom of the table. Reported number of observations exclude singleton observations within fixed effects groups. Standard errors, which are corrected for heteroskedasticity and double-clustered at the firm and industry-year level, are shown in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) CapEx	(2) Tobin's Q	(3) CapEx	(4) Tobin's Q	(5) CapEx	(6) Tobin's Q
PrePostDiffLCA	0.0006	0.0073	-0.0027***	-0.0107***	0.0022	0.0020
	(0.0033)	(0.0055)	(0.0007)	(0.0023)	(0.0018)	(0.0048)
PrePostDiffLCA × Post					-0.0042***	-0.0119***
					(0.0006)	(0.0005)
Sample	Pre	Pre	Post	Post	All	All
Firm FE	X	X	X	X	X	X
Year × Qtr × NAICS4 FE	X	X	X	X	X	X
Observations	2,536	2,542	4,698	4,689	7,499	7,503
Adjusted R-squared	0.674	0.732	0.672	0.760	0.665	0.750

Table X: H-1B Petition Cycles Across Commuting Zones

The table below report estimated coefficients for regressions of quarterly counts of H-1B petitions on *PctIndic* (the proportion of Indic-speaking residents as of 1990 in the commuting zone), *Post* (a dummy variable indicating whether the fiscal quarter comes after the petition deadline for the fiscal year t but before the April opening of the petition window for fiscal year $t+1$), and $PctIndic \times Post$. The dependent variables in columns (1), (2), and (3) represent, respectively, the commuting zone level quarterly counts of H-1B petitions of all types (*Apps*), of H-1B petitions for visa extensions (*AppsExtend*), and of H-1B petitions for initial employment (*AppsNew*). Fixed effects are indicated by "X" markers at the bottom of the table. Only $PctIndic \times Post$ is reported as all other combinations of interacted variables are subsumed by fixed effects. Reported number of observations exclude singleton observations within fixed effects groups. Standard errors, which are corrected for heteroskedasticity and clustered at the commuting zone level, are shown in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) Apps	(2) AppsExtend	(3) AppsNew
PctIndic × Post	-1.1955*** (0.2604)	0.0135 (0.0153)	-1.1973*** (0.2396)
CZ × Yr FE	X	X	X
Yr × Qtr FE	X	X	X
Observations	9,207	9,207	9,207
Adjusted R-squared	0.933	0.942	0.915

Table XI: Corporate Investment Cycles across Commuting Zones and Industries

The table below reports estimated coefficients for regressions of *CapEx* (capital expenditures scaled by lagged assets) on *PctIndic* (the proportion of Indic-speaking residents as of 1990 in the commuting zone), *IndH1B* (a dummy variable indicating whether a firm is in a high H-1B usage industry), *Post* (a dummy variable indicating whether the fiscal quarter comes after the petition deadline for the fiscal year t but before the April opening of the petition window for fiscal year $t+1$), and the interaction terms $PctIndic \times IndH1B$, $PctIndic \times Post$, $IndH1B \times Post$, and $PctIndic \times IndH1B \times Post$. Control variables $ln(Assets)$, $Tobin's\ Q$, $CashFlows$, $LTLeverage$, $CashHoldings$, $FC(KZ)$, $FC(WW)$, and $FC(HP)$ are also included in each regression but not reported to conserve spacce. Detailed definitions for all control variables can be found in Section 4.2. Fixed effects are indicated by "X" markers at the bottom of the table. Select explanatory variables are not reported across the columns as they are subsumed by fixed effects. Reported number of observations exclude singleton observations within fixed effects groups. Standard errors, which are corrected for heteroskedasticity and clustered at the commuting zone level in columns (1) through (3) and double-clustered at the NAICS4 industry and commuting zone level in columns (4) and (5), are shown in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) CapEx	(2) CapEx	(3) CapEx	(4) CapEx	(5) CapEx
PctIndic × Post	-0.0388	0.1558***	-0.0397	-0.0652	
	(0.0528)	(0.0173)	(0.0539)	(0.0786)	
IndH1B × Post			-0.0975**		
			(0.0402)		
PctIndic × IndH1B × Post			0.2084***	0.2578***	0.1790*
			(0.0587)	(0.0803)	(0.0975)
Industry Subsample	Low H-1B	High H-1B	All	All	All
Control Variables	X	X	X	X	X
Firm × Yr FE	X	X	X	X	X
NAICS4 × CZ × Yr FE	X	X	X	X	X
Yr × Qtr FE	X	X	X		
NAICS4 × Yr × Qtr FE				X	X
CZ × Yr × Qtr FE					X
Observations	41,673	17,091	58,793	57,884	55,854
Adjusted R-squared	0.535	0.530	0.574	0.531	0.486