Labor Mobility, Firm Monopsony, and Entrepreneurship: Evidence from Immigration Wait-Lines *

Abhinav Gupta†

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Abstract

This paper examines how US immigration-induced labor mobility frictions affect firm monopsony power and entrepreneurship. I exploit a natural experiment in the US immigration system that unexpectedly increased Green Card (GC) related job-switching frictions for Indian and Chinese immigrants in October 2005. Using matched employee-employer data from LinkedIn, I confirm that this shock reduced inter-firm employee mobility for Indian and Chinese employees. I rule out other explanations such as changes in employee composition, selection effects, or concurrent changes in India or China driving my results. This sudden decrease in labor mobility increased incumbent firm value, with $28.7 billion in abnormal stock returns for firms with Indian and Chinese employees in the ten days following the announcement. The slowdown of internal promotions for Indian and Chinese employees suggests monopsony power as the primary channel increasing firm value. The shock to immigrant mobility also had an adverse impact on entrepreneurship. Immigration related mobility restrictions disproportionately lowered the propensity of Indian and Chinese employees to join startups compared to incumbent firms. This distortion in labor supply to startups reduced new firm formation, with 12,000 fewer new firms founded in markets with more Indian and Chinese employees in the next five years. The distortion also decreased the funding and IPO of existing startups that had Indian and Chinese co-founders. These results reinforce the differential impact of labor mobility on incumbents and startups, providing direct causal evidence of the impact of labor mobility on business dynamism.

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†NYU Stern School of Business, ag6382@stern.nyu.edu.

1 Introduction

The last 50 years have witnessed a decrease in startup entry and an increase in incumbent firm profits (Gutiérrez et al., 2021), sparking concerns about declining US business dynamism. Understanding the underlying causes behind this decline is a central question. Concurrently, inter-firm employee mobility has also declined over the same horizon (Molloy et al., 2016). Several observers have speculated that these two trends are connected, and that labor mobility frictions have contributed to the decline in business dynamism by facilitating inefficient resource allocation (Decker et al., 2017; Davis and Haltiwanger, 2014) and limiting knowledge diffusion (Akcigit and Ates, 2019, 2021). However, it has been challenging to test this relationship directly. This paper studies the causal impact of labor mobility on business dynamism by focusing on two outcomes: incumbent firm value and entrepreneurship.

The impact of labor mobility on both firm value and entrepreneurship outcomes is not theoretically obvious, making this an empirical question. Lower labor mobility may increase firm value by increasing monopsony power, lowering adjustment costs, and limiting post-investment worker holdout. Alternatively, reduced labor mobility may erode firm value by reducing the employee's incentive to work hard. Reduced labor mobility may also limit inter-firm knowledge transfer and innovation. Mobility restrictions may increase investment into startups by allowing the entrepreneur to credibly commit human capital to the firm. However, they may limit talent supply to startups, causing an adverse impact on entrepreneurship.

There are significant issues in empirically identifying the impact of labor mobility on firm value and entrepreneurship. Labor mobility changes often correlate with underlying structural or demographic shifts,[1] capable of independently driving firm value and entry. An ideal specification would be to reduce the mobility of a subset of employees and firms and then compare outcomes for treated employees and firms with the unaffected control group. This paper comes close to this ideal by exploiting a sudden, unexpected change in the Green Card (GC) process which drastically decreased the inter-firm mobility of highly-skilled immigrant

[1]Previous literature has documented aging of the US workforce, increase in housing debt, increase in firm concentration and increase in job specialization requirements as explanations for changes in job-to-job transitions (Hyatt and Spletzer, 2013; Davis and Haltiwanger, 2014; Hyatt, 2015; Molloy et al., 2016; Azzopardi et al., 2020).

employees of Indian and Chinese origin in October 2005. I compare outcomes for Indian and Chinese employees, and firms with a higher proportion of Indian and Chinese employees, with others after the 2005 shock. I find that the decrease in immigrant labor mobility increased the value of large publicly listed firms by strengthening firm monopsony power. Immigrant mobility frictions also reduced entrepreneurship, with distortions to labor flow resulting in substantial declines in startup formation and growth.

The sudden shock to employee mobility resulted from the complexities of the US immigration process. Immigrant workers require GCs to work indefinitely in the US. Any employment-based GC application involves three sequential steps. Employee ability to switch firms improves considerably after the third step (referred to as Adjustment of Status) as employees are allowed to switch to similar jobs[2] 180 days after this step. However, the eligibility for Adjustment of Status varies by country of birth. Only 9,800 employees can apply for Adjustment of Status per country each year. Employees over this cap have to join a first-come-first-serve wait-line. Mobility is severely restricted in this wait-line as changing firms resets the employee's position in the line and may even lead to loss of legal status in the US. These restrictions are relaxed once an employee applies for Adjustment of Status.

All employees were free to apply for Adjustment of Status without any wait before 2005. Wait-lines to apply for Adjustment of Status were suddenly introduced in October 2005. This shock was especially severe for Indian and Chinese immigrants, with wait-times reaching five to seven years for India and China compared to one year for other countries. Employees faced severe mobility restrictions in these wait-lines as any job switch would reset the employee's position in the wait-line and endanger her legal status in the US. I refer to these wait-lines by their colloquial name "GC wait-lines" even though they only reduced employee mobility during the GC process but did not change the final time for GC approval. This increase in GC wait-lines was the result of internal information technology (IT) process changes at the United States Citizenship and Immigration Service (USCIS), rather than any policy or regulations updates. Anecdotal evidence suggests that these resets were unexpected for employees, firms, and even other governmental agencies such as the Department of State (DOS).

[2]Department of Labor (DOL) defines similarity as jobs under the same broad occupation category.

I implement my research design with granular employee-level data from a 2017 snapshot of LinkedIn, the world's largest online professional networking platform. LinkedIn data provides detailed online curriculum vitae (including education and job history) for all listed employees. I extract a sample of immigrant employees from overall LinkedIn data in two steps. First, I ascertain an immigrant worker's country of origin based on the worker's first educational or job location and the worker's name. Second, I limit my sample to employees eligible for employment-based GCs. I infer worker eligibility for such GCs based on employer name and the worker's country and qualifications. My final sample covers 130,090 immigrants from 2001 to 2010, of which half are of Indian or Chinese origin. I verify that my sample is representative of actual immigration by aggregating and comparing sample cross-sectional distributions (on the country of origin, location, and employer) to USCIS administrative data. I hand-merge my data to Compustat for studying incumbent firm outcomes. I use data from LinkedIn to study new firm formation and Crunchbase to study existing startup outcomes.

I start by documenting that the GC shock did indeed reduce employee mobility. Indian and Chinese immigrants experienced larger changes in wait-times compared to other nationalities after the GC shock. Correspondingly, I find a reduction of eight percent over the mean in the probability of Indian and Chinese employees changing firms relative to other immigrants after the GC shock. I provide multiple tests to ensure that my estimates cannot be explained by changes in immigrant employee quality or preferences, sample selection on immigrants who chose to stay in the US, or simultaneous structural changes impacting India and China.

Next, I focus on understanding the impact of mobility restrictions on firm value. I compare daily adjusted stock returns of firms around the date of GC announcement to quantify the change in firm returns due to GC mobility restrictions. Firms with one percentage point more of Indian and Chinese employees before 2005 witnessed 0.16 percentage point higher daily abnormal cumulative stock returns just after the GC announcement. The increase is significant in aggregate and accounted for $28.7 billion in ten day extra returns for firms with Indian and Chinese employees. These results correspond to an increase of $104,000 in firm returns for every Indian and Chinese employee, indicating that stock market participants expected firms to accrue benefits of around 15% the annual wage for each extra year the employee spent in a GC wait-line.[3] These increases persisted over longer time periods, with firms having one percentage point more Indian and Chinese employees before 2005 recording a 1.6% increase over the mean in Tobin's Q post GC shock. These results are on the lower end of previous estimates on the impact of mobility restrictions on firm value.

I document direct evidence for monopsony power as an explanation for increases in firm value. Most immigrant employees are paid close to the prevailing wage mandated by their job title (Matloff, 2013). Additionally, job titles account for over 90% of the variation in wages (Marinescu and Wolthoff, 2020). Controlling the rate of promotions is the only way for firms to extract wage rents from employees with limited mobility. Anecdotal evidence also suggests that outside offers play a significant role in the decisions of technology firms in offering employee promotions.[4] Correspondingly, I find a decline of 13% over the mean in within-firm promotions for Indian and Chinese employees, compared to other immigrants after the GC shock. I also find reductions to firm operating costs, and no changes in firm investment or innovation outcomes after the GC shock, consistent with monopsony being the primary mechanism for my results.

Immigration related mobility frictions disproportionately affect startups compared to incumbent firms. Employees in GC wait-lines are hesitant to join startups, as they face the additional risk of losing legal status in the US if either the Department of Labor (DOL) rejects the startup's filing for employee GC (citing lack of sufficient funds to pay market wages) or if the startup fails, and the employee cannot find a new job within two months. I find that the probability of Indian and Chinese employees joining startups reduced by 15 to 20% over the mean post the GC shock, both statistically and economically different from the five to seven percent decrease for incumbent firms. Unlike previously used mobility shocks such as Non-Competes (NCs) and Inevitable Disclosure Doctrines (IDDs), the GC shock only impacted the mobility of affected employees to startups while leaving the entrepreneur's ability to create a new firm unchanged.[5] This unique feature of my setting allows me to study the impact of the reduction in employee mobility cleanly.

I document that the reduction in the rate of Indian and Chinese employees joining startups

[3] \$104,000 return over seven years in the GC wait-line, 15% of the mean annual wage of \$98,000 (PERM data).

[4] Google policy of giving employees with an outside-offer a counter-offer (including promotion) in 24 hours.

[5] GC shock impacts the GC wait-line and leaves GC's actual award unchanged. As obtaining GC is a prerequisite for founding a new firm, GC mobility shock does not affect an entrepreneur's ability to start a firm.

is important in aggregate. Markets (commuting zone-industry pairs) with one percentage point more Indian and Chinese employees as of 2005 experienced a four percent decrease over the mean in new startup formation post the GC shock. These coefficients imply that GC induced mobility restrictions resulted in 12,000 fewer new startups from 2006 to 2010. The largest impact of GC restrictions is on firms in knowledge-intensive sectors (such as technology and professional services) consistent with labor mobility restrictions binding most for human-capital dependent industries. I find no signifcant impact for a placebo sample using the proportion of non-Indian and non-Chinese immigrants in a market, reinforcing that my results capture the impact of changes in immigrant mobility.

I also find that this sudden decrease in startup labor availability harmed existing startups. Kerr and Kerr (2021) document Indian (Chinese) founded startups hire 20 (40)% employees of similar ethnicity, with more funding and survival for startups with more co-ethnic hires. Any reduction in Indian and Chinese high-skilled labor should have the most significant impact on Indian and Chinese-founded startups most reliant on this labor. I find a 0.5% decrease over the mean in venture capital investment and a 0.8% drop over the mean in IPO for existing startups with one percentage point more Indian or Chinese co-founders after the GC shock.

My results show that immigration-induced changes in labor mobility have a differential impact on incumbent firms and startups. I find that a decrease in immigrant mobility boosts the value of incumbent firms by increasing their monopsony power. However, the reduction in skilled labor availability is the more important channel for startups, leading to a decline in startup formation and growth. Other systemic mobility frictions, which increase employee risk aversion, may act through channels similar to immigration mobility frictions. These results suggest that the secular decline in labor mobility may be an important factor in explaining the decrease in US business dynamism over the past fifty years.

These results also have significant policy relevance. The immigration frictions explored in this paper (GC induced mobility restrictions) are important by themselves. These restrictions currently affect over one million people (Bier, 2020) and have generated considerable interest from lawmakers.[6] However, there is little academic understanding about the overall impact of

[6]"U.S. Citizenship Act of 2021" includes provisions for increasing total GCs awarded from 140,000 to 170,000 annually, increasing per country quota from 7% to 20% of total, and capping GC wait-time at 10 years.

these restrictions (Kerr et al., 2015a). This paper reduces this gap by providing direct evidence of the impact of GC restrictions on employees, incumbent firms, and startups.

Related Literature: This paper helps identify the causal impact of changes in immigration related labor mobility frictions on business dynamism. While there has been a consensus in the literature about the secular decline in US business dynamism, the underlying causes behind this trend are unclear. Existing literature has proposed the drop in knowledge diffusion (Akcigit and Ates, 2019, 2021; Andrews et al., 2015), increase in firm concentration (Gutiérrez et al., 2021; Covarrubias et al., 2019; Gutiérrez et al., 2021; Barkai, 2020; Eggertsson et al., 2021), reduced firm responsiveness to productivity shocks (Decker et al., 2020), inefficient allocation of productive resources (Decker et al., 2017; Davis and Haltiwanger, 2014), increased dependence on intangibles (De Ridder, 2019), changes in demographics (Karahan et al., 2019), decline in interest rates (Liu et al., 2021), and worsening local housing markets (Davis and Haltiwanger, 2019) as explanations for this trend. In this paper, I find that immigration mobility restrictions causally decrease business dynamism. These results imply that other systemic mobility frictions - which impact mobility through employee risk aversion - may also be key in explaining the decline in business dynamism.

This paper adds to the existing literature on firm monopsony power by identifying a key friction to explain the firm's market power over high-skilled immigrants. Previous work has documented the importance of market concentration (Azar et al., 2020; Berger et al., 2019; Benmelech et al., 2020; Rinz, 2020; Autor et al., 2020; De Loecker et al., 2020; Barkai, 2020), informational frictions (Cardoso et al., n.d.; Starr et al., 2021; Ransom, 2021), and no-poaching and non-compete agreements (Krueger and Ashenfelter, 2018; Balasubramanian et al., 2020; Lipsitz and Starr, 2020) in explaining monopsony power. This paper documents direct evidence of the monopsony power of firms arising from immigration regulations. The high concentration of immigrant workers in the technology sector allows me to focus on large US technology firms. These firms rely heavily on human capital and have a history of inter-firm collaboration for increasing monopsony power,[7] making them a valuable setting for this study.

This paper contributes to entrepreneurship literature by being the first to document the

[7]2013 Anti-trust legislation alleges Adobe, Apple, Google, Intel, Pixar, Lucasfilm, and E-Bay enforced "no cold call" agreements to refrain from recruiting each other's employees.

importance of immigrant labor mobility as a determinant for startup labor supply and its impact on overall entrepreneurship. Previous work has shown that household wealth (Cen, 2021) and firm R&D (Babina and Howell, 2018) are critical factors in an employee's decision to work for a startup. This paper finds mobility frictions to be another crucial factor in an immigrant's decision to join a startup, reinforcing previous survey evidence from PhD students (Roach et al., 2019; Amuedo-Dorantes and Furtado, 2019). Additionally, these distortions in immigrant labor supply have aggregate effects on startup entry and growth. These results emphasize the importance of early-stage employees in startups similar to Choi et al. (2021), Bernstein et al. (2020), and Roach and Sauermann (2015). These effects are large and multiplicative, highlighting the essential and complimentary role immigrants play in the US startup ecosystem similar to findings by Kerr and Lincoln (2010), Peri et al. (2015), and Dimmock et al. (2019).

This paper adds to the literature on the financial impact of labor mobility by studying a novel natural experiment that reduced employee mobility. Existing papers have used changes in state-level regulations such as Non-Competes (NCs) and Inevitable Disclosure Doctrines (IDDs), as mobility shocks to study firm-level outcomes, such as investment and R&D (Sanati, 2018; Jeffers, 2019; Garmaise, 2011), leverage (Ysmailov, 2020; Sanati, 2018), firm acquisition and spin-outs (Younge et al., 2015; Starr et al., 2018; Chen et al., 2021a), and VC investment and new firm entry (Jeffers, 2019; Kang and Fleming, 2019; Gu et al., 2020). Immigration induced labor mobility frictions provide several key advantages. NCs and IDDs limit both the mobility of employee to startups and the ability of entrepreneur to start a new firm. However, GC mobility restrictions only impact employee mobility, allowing me to provide the first estimates of changes in labor mobility on entrepreneurship. The employee-level nature of the GC shock allows me to disentangle impacts of mobility from other macro-economic trends, a critique of NCs and IDDs which study across-state differences due to these laws.[8] The state-level implementation also makes NCs and IDDs prone to bias due to leakage of firms and employees.[9] Immigration laws apply nationally, with direct tests for reverse migration yielding no evidence of cross-country leakage following this policy change.

[8]Barnett and Sichelman (2016) argue that the differences in innovation in California and Massachusetts attributed to NCs, can be accounted for by fundamental technological and economic factors

[9]Marx et al. (2015) document the brain-drain of inventors from states that enforce NCs to those which do not.

This paper augments the previous literature on the impact of immigration policy by focusing on mobility frictions rather than immigrant supply. Existing work has focused on the effect of changes in the supply of immigrants due to exogenous shifts in H1-B visa caps (Kerr and Lincoln, 2010; Ghosh et al., 2014; Ashraf and Ray, 2017; Mayda et al., 2018; Xu, 2018), design of the visa lottery (Clemens, 2013; Doran et al., 2014; Dimmock et al., 2019; Chen et al., 2021b), or spatial settlement patterns of immigrants (Kerr et al., 2015b; Peri et al., 2015). However there is limited academic understanding of these frictions despite considerable policy interest. In fact, Kerr et al. (2015b) stress the need for future work on understanding overall implications of GC lock-ins. The only other paper studying impact of GC restrictions is Shen (2021), who focuses solely on the firm value of publicly traded firms. This paper studies the broader question of the impact of mobility restrictions on business dynamism, by focusing on both start-ups and incumbent firms. Further, Shen (2021) is unable to establish the mechanism behind the firm value increases as the lack of longitudinal employee data prohibits from studying changes in employee wages or promotions. This paper uses employee-level data on promotions to establish increased firm monopsony power as the major mechanism behind these results. Finally, Shen (2021) relies on two sources of variation to capture the impact of immigration mobility frictions: the monthly changes in GC wait-times and a temporary reset which allowed all employees to apply for adjustment of status for one month in 2007. However the monthly GC wait-time changes may capture the shifts in the number of GC applications correlated with unobserved technology or investment trends, making a causal interpretation difficult. The 2007 reset, while exogenous to such trends, was quickly reversed in a few days, leaving the question of long-term impact of mobility changes unanswered. This paper uses a much larger, persistent, and exogenous shock, which permanently increased wait-times for Indian and Chinese immigrants from zero to five to seven years, to understand the causal and long-term impact of immigration frictions. Hence this paper makes three contributions: it captures the impact of GC restrictions on business dynamism by focusing on both incumbent firms and startups, it uses within-firm promotions to pin down monopsony, and it relies on a novel yet persistent shock to GC wait-lines to understand the long-term causal impact of immigrant mobility restrictions.

This paper also helps explain the puzzling impact of GC approvals on employee mobility and wages. Hunt and Xie (2019) and Wang (2019) use employee survey data from the National Survey of College Graduates (NSCG) to study the wage impact of GCs. They find that while GC approval increases the employee's inter-firm mobility, it has no significant impact on her wages. Slower employee human capital developement and promotions during the GC wait-line may explain this result. Indeed, this paper documents a slowdown in within-firm promotions for employees trapped in the GC wait-line.

The paper proceeds as follows. Section 2 provides institutional background. Section 3 describes the data. Sections 4, 5, and 6 describe the empirical strategy and results on the impact of GC restrictions on employee mobility, incumbent firms, and startups. Section 7 concludes.

2 Institutional Details

This paper uses the quirks of the US immigration system to examine the impact of labor mobility. Accordingly, this section lays out the institutional details, including the immigration process and the natural experiment which unexpectedly raised GC wait-times.

2.1 US Immigration Process

Employment-based work visas for foreign workers (H-1 and L-1) can be extended for a maximum of five to seven years.[10] Workers wanting to stay permanently need to transition to legal permanent residency by applying for a Green Card (GC). Over 70% of these GCs are obtained based on an employer-based visa application (Jasso et al., 2010).[11]

A typical GC application involves three steps (figure C.2) which are approved sequentially and are adjudicated independently. First, the worker obtains labor certification (PERM) from the Department of Labor (DOL). Next, the employer sponsors an immigrant petition for the employee (I-140). Finally, the worker can apply for Adjustment of Status (I-485). Employee mobility improves significantly after Adjustment of Status even before getting the actual GC,

[10]H1-B visas (high-skilled workers) valid for six years and L-1 visas (managers) up to five or seven years.

[11]There are three categories for employment-based GCs: EB-1 for PhDs and managers, EB-2 for those with master's or five years of experience, and EB-3 for all other employees. I do not study EB-1 GCs, as they are not subject to GC mobility restrictions. I consolidate EB-2 and EB-3 GCs as workers switch between these categories.

as The American Competitiveness in the 21^{st} Century Act (AC21 2000) entitles the employee to switch employers to any similar job[12] 180 days after filing for Adjustment of Status.

The current US system imposes a fixed cap of 9,800 on the number of GCs granted to any country each year.[13] Employees are not allowed to apply for Adjustment of Status if demand for GCs exceeds this cap. Instead, they must join a first-come-first-served line based on the date of their labor certification. This system has created significant wait-lines for high-demand countries such as India and China.[14]

There are substantial restrictions on job mobility for an employee in these wait-lines. Firms can withdraw an employee's immigrant petition (I-140) once they switch jobs. The employee then must redo the entire GC process leading to three potential issues. First, this leads to a reset in the employee position in the wait-line, inducing significant delays. Second, the employee requires an approved immigrant petition (I-140) to renew legal status every three years, and any delay in the new petition hampers the employee's ability to stay in the US. Finally, the DOL will reject labor certification if they find the new employer cannot pay the employee market wages, leading to a loss of legal status for the employee. The frictions go away once the employee applies for Adjustment of Status, irrespective of actual GC approval.

2.2 Shock to Wait-Lines

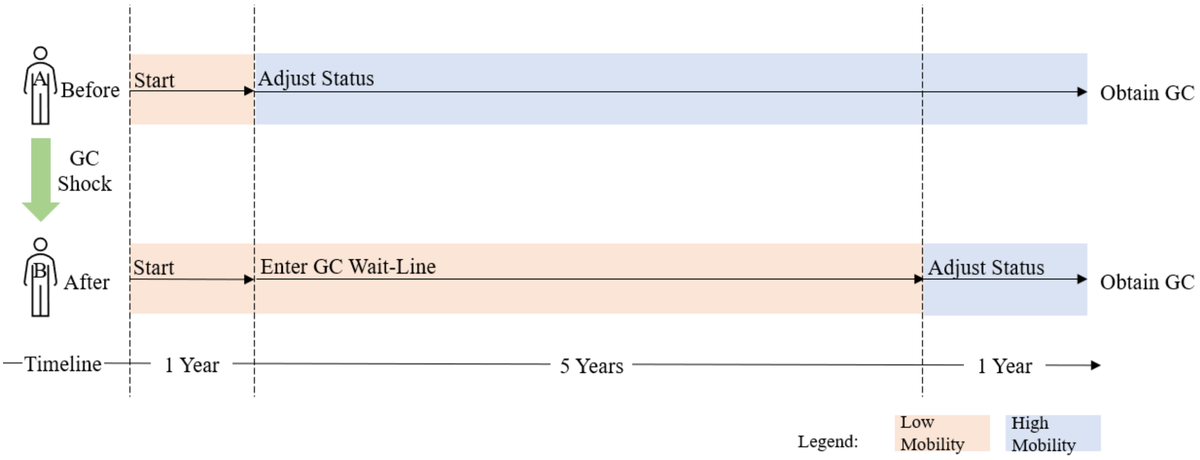
Prior to 2005, there were no wait-lines and all employees were free to apply for Adjustment of Status after completing the previous two steps. Wait-lines were suddenly introduced in October 2005 and employees from countries where applications exceeded the country-cap were not permitted to apply for Adjustment of Status. I illustrate the impact of the shock with an example in figure 1. Consider two immigrants who became eligible to apply for a GC just a day before (employee A) and after (employee B) the shock. Both employees still face a similar wait in obtaining final GC approval. However, A can directly apply for Adjustment of Status after completing the first two steps and be eligible to change firms for similar jobs throughout her subsequent wait. On the other hand, B has to enter a wait-line where any job change would

[12]DOL defines similarity as jobs under the same broad occupation category.

[13]Total cap of 140,000 GCs annually, with any country eligible for a maximum of 7% of the annual cap.

[14]Estimated wait-times of 40 years for Indians and ten years for Chinese immigrants in 2017 (Bier, 2020).

Figure 1: **Example: Impact of GC Shock**



Note: This figure presents an illustrative example to understand GC Shock. Employee A applies for GC just before the GC Shock and Employee B applies for GC post GC shock. Light red color denotes low mobility state when any change in job would need the redo of entire GC process, while light blue color represents high mobility state when employee can switch to similar job without redoing GC process. Employee A is able to apply for adjustment of status (I-485) concurrently with immigrant petition(I-140), and hence skips GC wait-line. However, employee B is forced to wait in GC wait-line in low mobility state for five years.

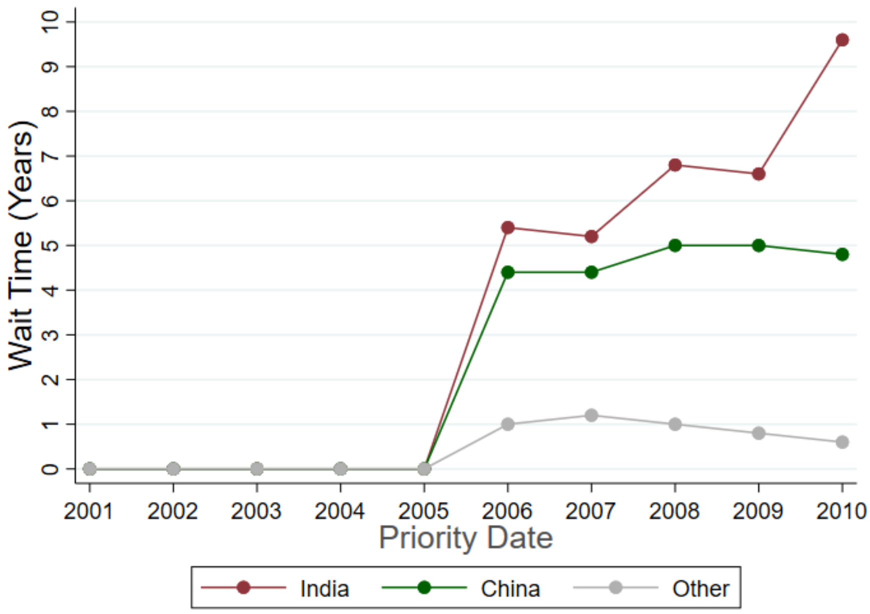
jeopardize B's position in line and legal status to work in the US. I refer to this wait-line by its colloquial name "GC wait-line" even though it only reduced employee mobility during GC process and not the final time for GC approval.

I present the impact of GC shock in figure 2, which shows the average GC wait-times faced by the employees based on their country of birth and year of GC application.[15] There was no GC wait-line for any country before 2005. However, GC wait-times increased suddenly for all applicants in October 2005. This reset was most severe for Indian and Chinese immigrants whose wait-times were increased by five to seven years, compared to around a year for others.

Changes in demand or regulation did not cause the change in wait-times. This was instead the product of internal efficiency improvements at USCIS. From 2000 to 2005, legacy IT systems at the USCIS lead to large application backlogs and failure of communication between agencies. Subsequently, the DOS could not estimate GC demand, and allowed

[15] I calculate wait-time based on the application deadlines from the Department of States' visa bulletin, https://travel.state.gov/content/travel/en/legal/visa-law0/visa-bulletin.html. The monthly visa bulletin details the cutoff dates for Green Card applications, based on country of birth and employment category. Only employees with labor certification filed before the cutoff date can apply for Adjustment of Status (I-485) in that month. I calculate an employee's wait-time as the difference between the year her labor certification was filed and the year she became eligible to apply for Adjustment of Status. I require that employees remain eligible for at least six months to allow for time to prepare employee applications. For example, suppose the cut-off date of 2006 for Indians started appearing consistently in the visa bulletin post January 2010. In that case, I take the wait-time for an Indian employee applying in 2006 as four years. I show the weighted average for EB-2 and EB-3 wait-times based on weights from Bier (2020).

Figure 2: **Green Card Wait-Times**



Note: This figure presents Green Card wait-times based on employee priority date and country of birth. The y-axis presents the average number of years EB-2/3 employees spent in the GC wait-line. The x-axis represents each employee's priority date: the year employer started the employees' GC process. The red line represents employees born in India, the green line those born in China, and the gray line all other immigrants.[15]

immigrants from all countries to apply for Adjustment of Status without any wait-line. However, the USCIS started a backlog reduction process by improving its internal IT processes in 2005. This brought to light that country-level caps were being exceeded, leading to an increase in GC wait-lines. Anecdotal evidence from immigration law websites presented in Appendix A suggests that this increase in GC wait-lines was unexpected by both employees and firms. Further, statements from the DOS claiming that it did not expect the retrogressions to be severe, help establish that the resets were a surprise to all stakeholders.

3 Data and Main Variables

3.1 LinkedIn Data

The lack of employer-employee matched data has been a significant hurdle in studying immigrant employee job mobility. Only three government agencies (the DOL, the DOS, and the USCIS) house employee immigration data. Any information about employee identity is redacted from all Freedom of Information Act (FOIA) data requests from all three regulatory agencies, making it impossible to track employees longitudinally. I overcome this hurdle by assembling data from LinkedIn. LinkedIn is the world's largest online professional networking platform, which began in 2003 and has since grown to over 740 million users worldwide. LinkedIn user profiles are online curriculum vitae meant to be a detailed resume for prospective employers. Each profile lists users' education (schools, programs attended, and graduation) and employment history (firms, location, positions held, joining, and leaving).

LinkedIn data offer two benefits for this study: First, high-skilled professionals, the target population for this paper, are the primary users of LinkedIn. Second, LinkedIn profiles are public to all users, making it difficult for individuals to make false claims about their employment. I use LinkedIn data for more than 108 million users extracted from public profiles. The dataset is a 2017 snapshot, allowing me to view complete user educational and employment history during my sample period (2001–2010). It also provides users ample time to update their employment history, minimizing concerns about data completeness.

I filter out the subset of immigrant employees from the complete LinkedIn data in three steps. First, I classify any employee who has had initial schooling or jobs outside the US as an immigrant from the corresponding country. I ensure that I am not capturing US citizens who may have started their career or education outside the US by predicting employee ethnicity based on name, and by throwing out any observations where ethnicity does not match the classified origin country. Second, I filter out immigrants who would not have applied for employment-based GCs based on country of origin and past employment history. Finally, I extract the year an employee starts working in the US, after completing the highest degree as a proxy for the year an employee begins the GC process. Limits on renewal of temporary visas and long wait-lines incentivize employees to start the GC process as early as possible and impose caps on how far employers may delay filing, making the start dates a reasonable proxy for the date of GC filing. I discuss sample construction in detail in Appendix B.

I compare my constructed LinkedIn sample with the administrative data. I use de-identified firm-level filing information available from the DOL website for comparison. Table C.1 presents the results. Cross-sectional distributions on the country of origin, locations, and firms appear similar between LinkedIn and administrative data. Approximately half the immigrants are from India and China, consistent with these countries having a high demand for GCs. California, New York, and New Jersey are the largest centers for GC applicants. Technology and consulting firms are the largest GC filers, with Microsoft, Cognizant, Qualcomm, Cisco, and Google as the primary employers in both datasets.

3.2 Employee Mobility Variables

I construct a matched employer-employee panel detailing each employee's role, location, and firm each year to track employee mobility. In cases where an employee lists multiple firms during the same duration (e.g., volunteering or part-time work while holding a primary job), I assume the position with the longest duration to be the primary one, and I ignore all others. I create an indicator variable $1(MoveFirm)$, which equals one only if the employee changes firms in that year. My final sample comprises 130,090 immigrant employees who entered the US between 2001 and 2010, encompassing 1,157,377 employee-year observations. Table 1 panel A shows that the mean probability of move is 14% for any immigrant employee, close to 12% for natives documented by Jeffers (2019). I also create an indicator for within-firm promotions $1(PromotionWithin)$ equal to one if an employee changes title within the same firm. The average probability of within-firm promotion for an employee is seven percent.

Table 1: **Descriptive Statistics**

Panel A: Employee Level Data	**Mean**	**Std. Dev.**	**P(10)**	**P(25)**	**P(50)**	**P(75)**	**P(90)**	**Count**
1(Indian/Chinese)	0.48	0.5	0	0	0	1	1	1,157,377
1(MoveFirm)	0.14	0.34	0	0	0	0	1	1,157,377
1(Promotion Within)	0.07	0.25	0	0	0	0	0	1,157,377
1(Move to Firm Age $<=$ 10 years)	0.02	0.12	0	0	0	0	0	1,157,377
1(Move to Firm Age $>$ 10 years)	0.12	0.33	0	0	0	0	1	1,157,377
1(Move to Firm Size $<=$ 200 Employees)	0.02	0.15	0	0	0	0	0	1,157,377
1(Move to Firm Age $>$ 200 Employees)	0.11	0.32	0	0	0	0	1	1,157,377
1(Masters Degree)	0.58	0.49	0	0	1	1	1	1,157,377
1(US Degree)	0.69	0.46	0	0	1	1	1	1,157,377
1(Female)	0.33	0.47	0	0	0	1	1	1,157,377
Job Experience (Years)	7.2	5.5	1	3	6	10	16	1,157,377
# LinkedIn Connections	286.7	173.1	50	131	277	500	500	1,157,347
# LinkedIn Recommendations	1.6	3.9	0	0	0	1	5	1,157,377

Note: This table presents descriptive statistics for employee-level variables. Column 1 presents sample mean, Column 2 presents standard deviations, Columns 3 to 7 present the 10th, 25th, 50th, 75th, and 90th percentile, respectively, and Column 8 presents observation counts.

I further break up the probability of employees moving into two components: startups and incumbent firms. I distinguish startups from incumbents based on both firm age and size. I use a cut-off of ten years and two hundred employees to differentiate incumbent firms and startups based on these parameters. As seen in Table 1 panel A, the probability of a move to startups is just two percent compared to 12% for incumbent firms.

I infer multiple employee-related characteristics directly from LinkedIn data as detailed in Appendix B. I present the summary statistics for these characteristics in table 1, panel A. 58% of immigrant employees hold a master's degree, in line with around 60% of applicants having a master's degree in administrative data.[16] A total of 69% of employees in my sample have some American degree, and 33% are women. Average job experience is seven years, with a quarter of the employees having less than three years of experience. Most employees are highly connected, with 270 being the mean number of LinkedIn connections. However, only the top quartile has one or more LinkedIn recommendations.

3.3 Public-Listed Firm Data

Table 1: **Descriptive Statistics**

Panel B: Publicly Listed Firms	**Mean**	**Std. Dev.**	**P(10)**	**P(25)**	**P(50)**	**P(75)**	**P(90)**	**Count**
B1: Cumulative Daily Return (31 August 2005 to 23 September 2005)								
Ratio Indian & Chinese (2005) (%)	0.42	1.10	0	0	0	0.21	1.27	62,498
Market Adjusted Return (%)	-2.27	32.66	-40.4	-22.23	-5.26	13.96	37.76	62,498
Fama French 3 Factor Adjusted Return (%)	-4.43	32.16	-43.54	-24.32	-6.79	12.07	34.38	62,498
Fama French 5 Factor Adjusted Return (%)	-2.40	32.99	-41.42	-22.44	-5.02	14.04	37.26	62,498
B2: Quarterly Data (2004 Q2 to 2007 Q4)								
Ratio Indian & Chinese (2005) (%)	0.43	1.09	0.00	0.00	0.00	0.26	1.32	50,236
Tobin's Q	2.12	1.15	1.06	1.29	1.73	2.55	3.89	50,236
Market to Book Ratio	3.07	2.28	0.96	1.49	2.37	3.84	6.42	50,236
Return On Assets	0.02	0.05	-0.05	0.01	0.03	0.05	0.07	50,236
(COGs + SGA)/ Assets	0.26	0.17	0.07	0.13	0.22	0.34	0.52	50,236
(Operating Expenses)/ Assets	0.26	0.17	0.07	0.13	0.22	0.34	0.52	50,236
Capex/ Assets	0.01	0.01	0.00	0.00	0.01	0.02	0.03	50,236
Total Patents	5.1	36.5	0	0	0	0	5	50,236

Note: This table presents descriptive statistics for variables related to firm value and firm-level controls from Compustat. Column 1 presents sample mean, Column 2 presents standard deviations, Columns 3 to 7 present the 10th, 25th, 50th, 75th, and 90th percentile, respectively, and Column 8 presents observation counts.

I need the proportion of Indian and Chinese employees in each firm to understand the impact of GC shock on firms. I achieve this by merging firm-level data from Compustat to LinkedIn as detailed in Appendix B. Table 1, panel B presents these results. I present the firm-level variables

[16]This estimate is from the 2015 to 2017 GC filings, as other years do not report this data.

used in the paper at both quarterly levels (from 2004 Q2 to 2007 Q4) and daily stock returns in the 15-day window around the GC shock date. I provide details on variable construction in table B.1. My data contains 3,680 firms, with 28% having Indian or Chinese employees as of 2005. Firms hiring Indian and Chinese employees are larger on average and represent more than 67% of market capitalization as of 2005.

3.4 New Firm Formation Data

Table 1: **Descriptive Statistics**

Panel C: New Firm Entry	Mean	Std. Dev.	P(10)	P(25)	P(50)	P(75)	P(90)	Count
Ratio Indian & Chinese (2005) (%)	0.087	0.305	0	0	0	0	0.212	395,180
Ratio Indian & Chinese (2005, Weight by Emp) (%)	0.425	0.584	0	0.036	0.176	0.524	1.453	395,180
New Firms Founded / 1000 Employees	6.18	21.76	0.00	0.00	0.00	2.68	12.20	395,180
New Firms Founded (Empployer) / 1000 Employees	4.46	16.49	0.00	0.00	0.00	1.31	8.33	395,180

Note: This table presents descriptive statistics for variables related to new firm entry at the industry-commuting zone-year level. Column 1 presents sample mean, Column 2 presents standard deviations, Columns 3 to 7 present the 10th, 25th, 50th, 75th, and 90th percentile, respectively, and Column 8 presents observation counts.

I require detailed data on location, industry, and the founding of startups to understand the impact of GC restrictions on firm formation for each industry-commuting zone (henceforth "market"). I get this data directly from LinkedIn company pages following Jeffers (2019), who also uses LinkedIn data to measure new firm formation. I drop any public listed firms to avoid any new firms formed due to spin-offs and mergers. I also drop any industries where I observe no new firm formation in the data. I use LinkedIn industry classification, which contains 114 unique industries, as it is more detailed than NAICS 4-digit classification (83 unique codes in data), especially in critical sectors, such as technology. My results are robust to using 4-digit NAICS classification. I define startup formation per employee as the ratio of total new firms founded per market each year, scaled by the total number of employees in that market in 2005.

Jeffers (2019) argues that LinkedIn data are the best way to capture startup entry for three reasons. First, LinkedIn data better captures entrepreneurial ventures than the census, which equally captures all types of businesses. As an example, while tech startups are more likely to be listed on LinkedIn than a mom-and-pop bakery, they would be given equal weight in the census. Second, the Business Dynamics Statistics (BDS) available by location-year in the census are aggregated to such broad levels that nothing would map to the technology sector.

Third, the BDS counts the number of establishments rather than firms, which would over-count multi-establishment firms, and not count startups working from home or co-working spaces.

Table 1, panel C presents the summary statistics for this data. My panel contains 114 unique LinkedIn industries and 696 commuting zones across ten years. However, 50% of these industry-commuting zone pairs have no new firm formation across all ten years, leading to a panel of 39,518 markets. A total of 15% of the markets have a non-zero share of Indian and Chinese immigrants. Markets with Indian or Chinese employees tend to have a larger employee base, evidenced by the employee weighted mean for the ratio of Indian and Chinese employees being larger than its unweighted counterpart. I observe an average of 6.2 firms formed for every 1,000 employees per market per year. The average drops to 4.5 when I exclude solo firms (firms classified as "myself only" or having a single employee).

3.5 Existing Startup Outcomes Data

Table 1: **Descriptive Statistics**

Panel D: StartUp Growth	**Mean**	**Std. Dev.**	**P(10)**	**P(25)**	**P(50)**	**P(75)**	**P(90)**	**Count**
Ratio Indian & Chinese Founders (2005) (%)	5.33	18.40	0.00	0.00	0.00	0.00	12.50	103,270
1(IPO) /1000 Firm-Years	14.52	119.60	0.00	0.00	0.00	0.00	0.00	103,270
1(Any Investment)/1000 Firm-Years	70.20	255.48	0.00	0.00	0.00	0.00	0.00	103,270
1(VC Investment)/1000 Firm-Years	41.80	200.14	0.00	0.00	0.00	0.00	0.00	103,270

Note: This table presents descriptive statistics for variables related to existing startups. Column 1 presents sample mean, Column 2 presents standard deviations, Columns 3 to 7 present the 10th, 25th, 50th, 75th, and 90th percentile, respectively, and Column 8 presents observation counts.

I use company-level data from Crunchbase[17] to understand the impact of GC restrictions on startup growth. Crunchbase is a crowd-sourced database that tracks startups, especially those in high-tech sectors. Crunchbase covers over 675,000 firms tracking firm name, address, industry, founders, and firm events, such as funding, IPOs, and acquisitions. Several studies, such as Dalle et al. (2017), Ling (2015), Block et al. (2015), and Wang (2018), have argued that Crunchbase provides comprehensive coverage comparable to other datasets for US startups. Dimmock et al. (2019) have also previously relied on Crunchbase to study the effects of changes in immigrant employee supply on startups. I limit my sample to firms founded in and before

[17] Through the CrunchBase Research Access Program.

2005 to focus on existing startups. I use name-based ethnicity prediction on founder names, as detailed in Crunchbase, to identify Indian and Chinese origin founders.[18]

Table 1, panel D presents the main variables. I report the outcome variables as normalized by 1000 firm-years to improve readability. I study 10,327 firms across ten years, of which 12% have Indian and Chinese founders. 15 out of 1000 firm-years see IPOs during the sample period. Firms receive any funding and VC investment in 70 and 42 of 1000 firm years, respectively.

4 Employee Level Mobility

I examine the impact of GC wait-lines on employee mobility in this section. While prior studies have documented increased employee mobility after obtaining GC (Hunt and Xie, 2019; Wang, 2019), the 2005 GC shock forced employees to enter GC wait-lines while leaving the actual time to obtain the GC the same. Hence, I first document the change in employee mobility after the 2005 GC shock to establish the validity of my setting. I also test if alternative explanations (except for the shift in employee mobility) can explain my results.

4.1 Empirical Specification

Figure 2 shows the sharp increase in GC wait-lines after 2005. This shift is especially severe for immigrants from India and China, for whom wait-times suddenly increased to five to seven years, compared to other immigrants who faced wait-times of around one year. I compare the differential change in mobility for Indian and Chinese immigrant cohorts who started their job in the US after 2005, using other immigrants as the control group. Most other immigration policies (such as H1-B visa caps and lotteries) are applied across all immigrants without any distinction by country of birth. Comparing within immigrants allows me to rule out any effects due to change in other immigration policies, and allows me to pinpoint the effects of GC wait-lines. I use a difference-in-differences approach with the sudden increase in GC wait-lines as my treatment, Indian and Chinese immigrants as the treated group, and all other immigrants as

[18] I use name prism API, as developed by Ye et al. (2017); https://www.name-prism.com/.

the control group. For any immigrant i and year t :

$$
\begin{aligned}
\mathbb{1}(MoveFirm_{i,t}) = & \sum_{\tau=2001}^{2010} \beta_\tau \mathbb{1}(USJobStartYear_i = \tau) * \mathbb{1}(Indian/Chinese_i) \\
& + [\gamma \mathbb{1}(Indian/Chinese_i) + \sum_{\tau=2001}^{2010} \delta_t \mathbb{1}(USJobStartYear_i = \tau) + \eta X_{i,t}] + \varepsilon_{i,t}
\end{aligned}
\tag{1}
$$

Here $\mathbb{1}(MoveFirm_{i,t})$ is an indicator variable which equals one if the employee i switches firms in year t. $\mathbb{1}(USJobStartYear_i = \tau)$ is an indicator that is one if immigrant i started first US job in year τ and zero otherwise. $\mathbb{1}(Indian/Chinese_i)$ an indicator which is equal to one if i is Indian or Chinese. β_τ captures the differential mobility impact on Indian and Chinese immigrants based on employee cohort. The omitted year is 2004, one year before the GC shock. I control for employee country and the year of the first job in the US to ensure my results do not capture differences in mobility between nationalities and cohorts. I include industry-location-year fixed effects to absorb any effect of industry and location level time trends. The industry is the self-identified LinkedIn industry (NAICS 4-digit) and remains constant for an employee even if she changes firms. Using self-identified rather than firm industry helps me proxy for employee roles accurately. For example, if an employee works in accounting in Microsoft, she will put in accounting (rather than technology) as her industry helping me compare her outcomes to other accountants. Location is the current location of the employee, defined at the commuting zone level. I control for granular employee-level characteristics including employee's highest degree, US degree, gender, previous experience, number of LinkedIn connections, and recommendations. I interact these employee-level characteristics with the year and the number of years spent by the employee in her job to control the differential impact of these characteristics by time and seniority. I present results with and without all controls to rule out concerns that changes in employee composition may drive my estimates. I cluster standard errors at industry and commuting zone.

The identifying assumption here is that Indian and Chinese employee cohorts would have continued on parallel trends to other immigrant cohorts without the GC shock. If the identifying assumption is satisfied, we should see no trends in β_τ before 2005 and then negative coefficients from 2006 onwards, indicating a relative decline in the mobility of Indian and Chinese immigrants entering the US post the GC shock. I also run the regression version of the event study as follows:

$$\begin{aligned}\mathbb{1}(MoveFirm_{i,t}) &= \beta * \mathbb{1}(USJobStartYear_i > 2005) * \mathbb{1}(Indian/Chinese_i) \\ &\quad + [\gamma \mathbb{1}(Indian/Chinese_i) + \sum_{\tau=2001}^{2010} \delta_t \mathbb{1}(USJobStartYear_i = \tau) + \eta X_{i,t}] + \varepsilon_{i,t}\end{aligned} \tag{2}$$

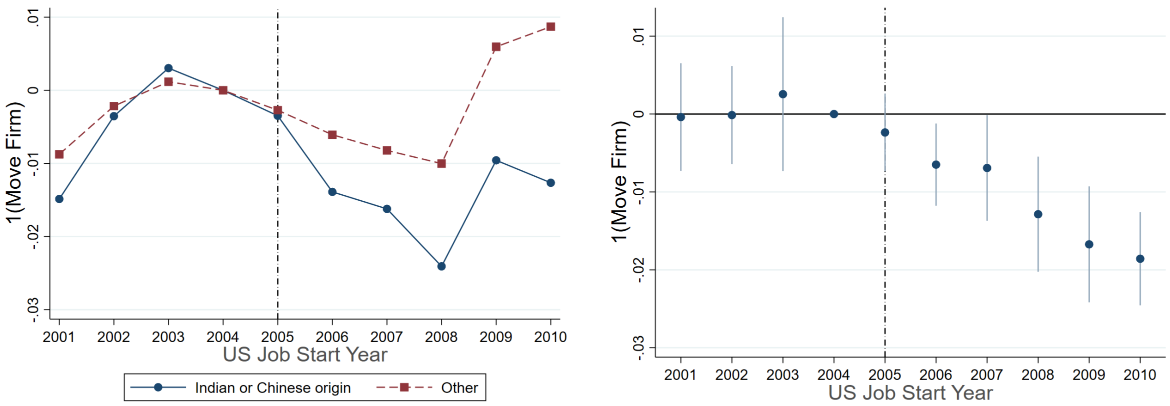
The specification is similar to Equation 1 with $\mathbb{1}(YearFirstJob_i > 2005)$ being an indicator equal to one if i starts her first job in the US after 2005.

4.2 Overall Mobility Results

I first verify the absence of any pre-trends in figure 3. I present only raw means in panel A and the specification with all controls as per equation 1 in panel B. Indian and Chinese employees are on parallel trends with other immigrants, with each pre-period coefficient statistically indistinguishable from zero. There is a sudden decline in employee mobility for Indian and Chinese employees only post the 2005 shock, consistent with a sudden change in GC wait-lines driving the results rather than any other gradual change in immigrant preferences or demographics. The precise timing of the shock also helps me rule out the effects of other changes in immigration law from different years.

Figure 3 shows that the difference in mobility for Indian and Chinese and other immigrants widens from 2006 to 2010, nearly doubling from 2006 to 2010. There are two potential explanations for this trend. First, as seen in figure 2, there was a steady increase in wait-times for Indian and Chinese cohorts from four to six years in 2005 to five to ten years in 2010. Hence, we should expect to see a secular decline in mobility of later cohorts subjected to longer wait-lines. Second, the DOS accidentally reduced GCs wait-lines to zero allowing everyone to apply for Adjustment of Status for one month in June 2007. As some employees from the 2006 and 2007 cohorts may have taken advantage of this narrow window, we should see a smaller impact on these as compared to later cohorts, consistent with my results.

Figure 3: **Event Study: Employee Mobility**



A: Raw Means (Detrended) B: Regression Coefficients

Note: This figure presents event studies on immigrant employee mobility around the introduction of GC wait-lines. The y-axis plots an indicator that equals one if the employee switches firms in that year and is zero otherwise. The x-axis plots the year an employee first started working in the US. The dashed line indicates the year 2005 when GC wait-lines were introduced. 2004 is the omitted year set equal to zero in both panels. Panel A plots the mean employee mobility without linear trends for employee cohort for Indians and Chinese immigrants (in blue) and all other immigrants (in red). Panel B plots the differential impact of GC wait-lines on Indians and Chinese compared to other immigrants as estimated by the β coefficient obtained from equation 1. I control for industry-location-year fixed effects. Industry is specified at NAICS 4-digit and location at commuting zone level. I also control for granular employee controls interacted with year and the number of years on the job for each employee. Employee controls include indicators for master's degree, any degree from US-based college, gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone).

I present the regression counterparts for event study as defined by equation 2 in table 2. I use my most granular specification with industry-location-time fixed effects and employee controls interacted with year and seniority in col. 6 as my baseline. There is a relative decline of 1.1 percentage points in labor mobility for Indian and Chinese cohorts, compared to other immigrants starting jobs in the US after 2005. This decline is economically large and constitutes eight percent of the mean sample mobility. I present several alternate specifications: from no fixed effects in col.1 to industry-location, employee controls, and job year controls in col. 2 to 4, and a combination of all these controls in col. 5. My coefficient remains stable, ranging from 1.1 to 1.3 percentage points across all specifications, reducing the concern of shifts in composition across industries or employee quality driving my results.

The lack of person-level identifiers in administrative data forces me to make two data assumptions. First, I use the year an employee starts her job in the US as a proxy for when the employee starts the GC process. Limits on renewal of temporary visas and long wait-lines

Table 2: **Effect of GC Shock on Employee Mobility**

	Dependent Variable: 1(Move Firm)					
	(1)	(2)	(3)	(4)	(5)	(6)
1(YearFirstJob > 2005) × 1(Indian/Chinese)	-0.011** (0.005)	-0.011*** (0.004)	-0.013*** (0.004)	-0.013*** (0.004)	-0.013*** (0.003)	-0.011*** (0.003)
Industry × Location FEs		Y			Y	Y × Year
Employee Controls			Y		Y	Y × Year
Job Year Controls				Y	Y	Y × Emp Cntrl
Y-Mean	0.14	0.14	0.14	0.14	0.14	0.14
Observations	1,157,377	1,157,377	1,157,377	1,157,377	1,157,377	1,157,377
Observations - Explained by FEs						32,918

Note: This table presents estimates on the impact of changes in GC wait-lines on immigrant employee mobility. The dependent variable is an indicator that equals one if the employee switches firms in that year and is zero otherwise. The independent variable is an indicator that is one if an employee is Indian or Chinese interacted with an indicator that is one if the employee starts the job in the US post 2005. The β coefficient is obtained from equation 2. Col. 1 presents the results with no controls, col. 2 with industry-location fixed effects, col. 3 with employee controls, col. 4 with job year controls, col. 5 with industry-location fixed effects, employee controls, and job year controls, and col. 6 with industry-location fixed effects interacted with year and employee controls interacted with year and job year controls. Industry is specified at NAICS 4-digit level and location at commuting zone level. Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

incentivize employees to start the GC process as early as possible and impose caps on how far employers may delay filing, making the start dates a reasonable proxy. Even if this assumption is invalidated, using start dates as a proxy reduces the probability of finding any result.[19] Second, in the absence of actual visa status, I classify all immigrants as dependent on employer-based applications. This concern is minimized because I only consider employees who have worked for companies with a track record of applying for GCs as a part of my sample. I also show that cross-sectional distributions appear similar between LinkedIn and administrative data in section 3.1. Even if I capture employees who are not on employer-based GCs, any bias arising from this assumption reduces the likelihood of finding a result.[20]

Table C.2 shows that these results are robust to changes in specifications. Recent literature has documented potential for bias in difference-in-difference coefficients estimated by two-way

[19]Consider employee A, from India, who started her job in 2004 and began her GC application in 2006. As she applied for the GC post October 2005, she was required to join the GC wait-line. However, I assume A's GC application date as 2004, and classify her as not exposed to the GC shock. This misclassification of some treated employees as control reduces my likelihood of finding treatment effects.

[20]Previous research has shown higher mobility for family-based visa immigrants than those on employment-based GCs (Mukhopadhyay and Oxborrow, 2012), as they are not subject to the same visa restrictions. Inclusion of these employees unaffected by GC wait-lines in my sample reduces the likelihood of finding any effects.

fixed effects. I re-estimate the main effect with the doubly robust estimator as per Sant'Anna and Zhao (2020) and find similar effects in panel A1. Another concern may be that commuting zone and NAIC-4 digit industry are too broad to control for changes in employee composition. However, I see similar effects with an even more granular definition of industry and location in panels A2 and A3. A concern may be that I observe different cohorts for different tenures biasing my results. Reassuringly, I find larger results on restricting my sample to seven years tenure (the maximum possible for all cohorts in my data) for all employees in panel B1. I also rule out the great financial crisis driving mobility results by restricting my sample to only years post 2010 in panel B2.

4.3 Alternate Explanations

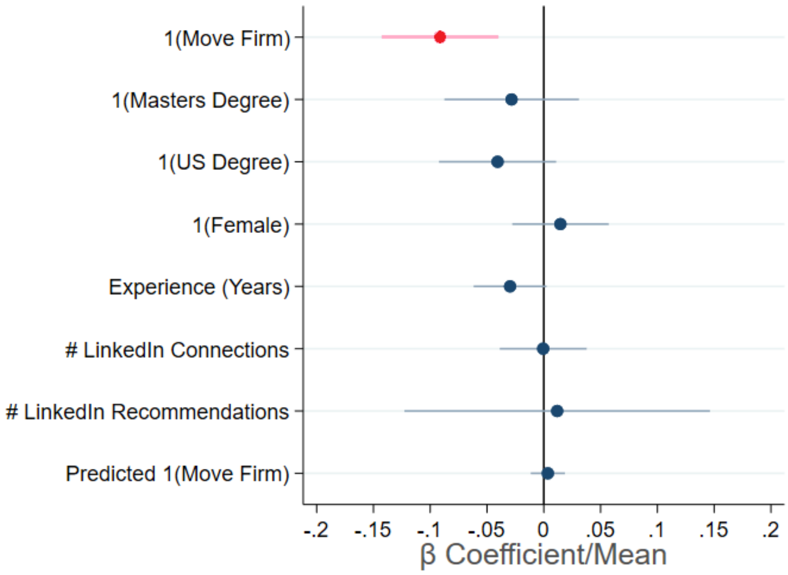
I can divide alternate explanations on the decrease in employee mobility after the 2005 GC shock into four major categories: Changes in employee composition, selection on stayers, structural changes in India and China, and changes in employee productivity. In this section, I provide robustness tests to reject each of these explanations.

4.3.1 Changes in Employee Composition

The GC shock may have changed the composition of Indian and Chinese employees entering the US due to changes in selection preferences of either employees or firms. For example, suppose the increase in GC wait-lines discouraged Indian and Chinese employees with a greater preference for mobility from immigrating into the US. In such a case, we would still observe a decrease in mobility, but it would reflect a change in immigrant quality or preferences, rather than mobility frictions. I rule out this concern using three tests.

First, I check if there is any impact of the GC shock on any observable employee characteristics. If a change in employee composition causes a decline in mobility, I should observe changes in employee attributes following the GC shock. As characteristics are constant for a given employee across time, I collapse my data from employee-year to

Figure 4: **Robustness: Change in Employee Mobility versus Composition**



Note: This figure presents the change in immigrant employee mobility and composition around the introduction of GC wait-lines. The y-axis shows the dependent variable for each regression. The x-axis plots the change in the dependent variable post 2005 GC shock as estimated by the β coefficient obtained from equation 3. I scale the estimate by sample means for the respective variables. I control for industry-location fixed effects. Industry is specified at NAICS 4-digit level and location at commuting zone level. The first row (in red) shows the average probability of an employee changing firms. Rows 2-7 (in blue) show employee variables including an indicator for master's degree, any degree from US-based college, employee gender, employee experience before job in the US (in years), number of LinkedIn connections, and number of recommendations on LinkedIn. Row 8 shows an indicator for the predicted component of employee mobility obtained by regressing mobility on the composition factors in rows 2-7. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone).

employee-level and use a modified version of equation 2 for this test:

$$\begin{aligned} Y_i = {} & \beta * \mathbb{1}(USJobStartYear_i > 2005) * \mathbb{1}(Indian/Chinese_i) \\ & + [\gamma \mathbb{1}(Indian/Chinese_i) + \sum_{\tau=2001}^{2010} \delta_t \mathbb{1}(USJobStartYear_i = \tau) + \eta X_i] + \varepsilon_i \end{aligned} \tag{3}$$

Figure 4 and table C.3 provide the estimates for this test. Row 1 (in red) shows that the results still hold up for employee mobility, with an effect of nine percent, similar to that obtained by equation 2. However, there appears to be no statistically or economically significant change in employee's highest education level, probability of having a US degree, gender, experience, number of LinkedIn connections, and recommendations. There is no consistent pattern of a positive or negative bias that might influence results in any direction. I also regress the probability of the employee moving on these observable characteristics to obtain the component of mobility of Indian and Chinese employees predicted by these factors.

I find almost zero effect on this predicted component of employee mobility. These results help establish that there are no observable changes in employee composition post GC shock.

Table 3: **Robustness: Within Employee Change in Mobility Post Immigrant Petition**

	Dependent Variable: 1(Move Firm)					
	(1)	(2)	(3)	(4)	(5)	(6)
1(YearFirstJob > 2005) × 1(Indian/Chinese) × 1(Post Immigrant Petition)	-0.011** (0.004)	-0.012*** (0.004)	-0.012*** (0.004)	-0.012*** (0.004)	-0.013*** (0.004)	-0.013*** (0.004)
Person FEs	Y	Y	Y	Y	Y	Y
Industry × Location × Year FEs		Y			Y	Y
Employee Controls × Year FEs			Y		Y	Y
Job Year Controls				Y	Y	Y × Emp Cntrl
Y-Mean	0.14	0.14	0.14	0.14	0.14	0.14
Observations	1,157,377	1,157,377	1,157,377	1,157,377	1,157,377	1,157,377
Observations - Explained by FEs	55	33,050	59	55	33,054	33,058

Note: This table presents estimates for within-employee change in mobility after GC shock after employees' immigrant petition. The dependent variable is an indicator that equals one if an employee switches firms in that year and is zero otherwise. The independent variable is an indicator which is one if an employee is Indian or Chinese interacted with an indicator which is one if the employee starts the job in US post 2005 and another indicator which turns on after the employee starts immigrant petition. I estimate the triple-diff β coefficient obtained from equation 4. I control for employee fixed effects to obtain within employee estimates. I further control different combinations of industry-location-year fixed effects and employee controls interacted with year and number of years on the job. Industry is specified at NAICS 4-digit level and location at commuting zone level. Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. I do not present the coefficient for Indian and Chinese employees who applied for GC post 2005 as it is absorbed by person fixed effects. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

Second, I show that reductions in mobility post GC shock only manifest after employees enter GC wait-lines. Employees face GC related frictions only after immigrant petition (Form I-140). Immigrants from high-demand countries, such as India and China, have to enter wait-lines to file for Adjustment of Status after this step. However, other immigrants may file immigrant petition and Adjustment of Status application simultaneously, without waiting. If GC frictions drive the change in employee mobility, I should observe the employee's mobility decrease only post employee immigrant petition application. While I am unable to observe the firm immigrant petition year for any employee directly, I approximate it using a heuristic based on institutional details of the immigration process.[21] I operationalize this test using a

[21] I base my heuristic on two facts: 1. Employees must start the GC process within six years of starting a job in the US (H1-B visa can be renewed for six years without GC). 2. Employees need to remain at a GC firm for at least two years before switching jobs (PERM process sing time of 12 to 18 months, and six months wait after immigrant petition). I infer the first firm where an employee spends at least two years in the first six years in the US as the GC firm. If no such firm exists, I assume that the employee firm in her sixth year is the GC firm. I assume the date of immigrant petition one year after the employee enters GC firm (12 months of PERM certification).

triple-difference research design to study the change in within-employee mobility post GC application. For an employee i in year t:

$$\begin{aligned}\mathbb{1}(MoveFirm_{i,t}) = \beta{*}\mathbb{1}(YearFirstJob_i > 2005) * \mathbb{1}(Indian/Chinese_i) *\\ \mathbb{1}(ImmigrantPetition_{i,t}) + [\alpha_i + \eta X_{i,t}] + \varepsilon_{i,t}\end{aligned} \quad (4)$$

$\mathbb{1}(ImmigrantPetition_{i,t})$ is an indicator that turns on after the firm has filed an employee's immigrant petition (Form I-140). All other variable definitions are similar to equation 1. The coefficient of interest is β, which identifies the effect on mobility post-immigrant petition for Indian and Chinese employees who start their US jobs after the 2005 GC shock. I control for person-level fixed effects to measure the within-individual decline in mobility, and absorb any unobservable composition changes in employees. Table 3 and figure C.3 show the results. I only show the triple difference coefficient, as employee fixed effects absorb the coefficient for Indian and Chinese employees who applied for GC after 2005. My most granular specification (col. 6), documents a decrease of 1.3 percentage points in the mobility Indian or Chinese cohorts entering post 2005 after the employee's immigrant petition. This decline is very similar to the 1.1 percentage point decrease for the baseline specification showing that the entire decrease in mobility for post 2005 Indian and Chinese cohorts comes after an employee enters the GC wait-line.

Third, I check if the selection by incoming immigrants causes my results. 70% of immigrants in my sample enter the US for education and start their US job a few years after entering the US. I limit the sample to immigrants who entered the US before or in 2005. As the increase in GC was unexpectedly announced in 2005, it is unlikely that these employees decided whether to come to the US based on changes in GC wait-lines. If my results are driven by high mobility immigrants not choosing to enter the US after 2005, I should not find any results for this sample. I present my results in table 4 and figure C.4. I find that the mobility of Indian and Chinese immigrants reduced by 0.9 percentage points for the most granular specification (col. 6) after the GC shock for this sub-sample. This reduction constitutes a seven percent reduction over the mean, similar to the eight percent estimate for the complete sample.

Table 4: **Robustness: Sub-sample of Employees Entering US Before GC Shock**

	Dependent Variable: 1(Move Firm)					
	(1)	(2)	(3)	(4)	(5)	(6)
1(YearFirstJob > 2005) × 1(Indian/Chinese)	-0.018*** (0.003)	-0.014*** (0.002)	-0.014*** (0.003)	-0.014*** (0.003)	-0.012*** (0.002)	-0.009*** (0.002)
Industry × Location FEs		Y			Y	Y × Year
Employee Controls			Y		Y	Y × Year
Job Year Controls				Y	Y	Y × Emp Cntrl
Y-Mean	0.13	0.13	0.13	0.13	0.13	0.13
Observations	765,928	765,928	765,928	765,928	765,928	765,928
Observations - Explained by FEs						29,275

Note: This table presents estimates on the impact of changes in GC wait-lines on immigrant employee mobility for a sub-sample of immigrants who entered the US before 2005. The dependent variable is an indicator that equals one if the employee switches firms in that year and is zero otherwise. The independent variable is an indicator which is one if an employee is Indian or Chinese interacted with an indicator which is one if the employee starts the job in US post 2005. I estimate by β coefficient obtained from equation 2. This table is similar to table 2, except I limit the sample to immigrants who had entered the US before 2005. I control for different combinations of industry-location-year fixed effects and employee controls interacted with the year and number of years on the job. Industry is specified at NAICS 4-digit level and location at commuting zone level. Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

4.3.2 Selection on Stayers

Another concern is that the increased GC wait-lines could encourage existing immigrants affected by wait-lines to leave the US for their home country, or any other country with a more lenient visa regime. Such changes in reverse migration behavior would bias my sample as I only observe employees who remain in the US. I use the international nature of LinkedIn data to address this concern. I create an outcome that equals one if the employee applies for a GC and moves out of the US (to any other country) and is zero otherwise. While I do not observe the GC application by employees, I assume that any employee who moves out after spending at least six years in the US to have applied for a GC. As a GC is needed to spend more than six years working continuously in the US, this filter helps approximate all potential applicants. Next, I carry out a regression to identify any changes in the probability of employees moving out of the US after 2005. As the outcome is at the employee level (and not the employee-year level), I use a specification similar to equation 3. I present results in table 5 and figure C.5. I find no significant changes in the probability of reverse migration of Indian and Chinese employees post the 2005 GC shock. The coefficient is also economically small, being 30

Table 5: **Robustness: Reverse Migration Post Shock**

	Dependent Variable: 1(Move to Other Country)			
	(1)	(2)	(3)	(4)
1(YearFirstJob > 2005) × 1(Indian/Chinese)	0.0001	0.0004	-0.0001	0.0004
	(0.003)	(0.003)	(0.003)	(0.003)
Industry × Location FEs		Y		Y
Employee Controls			Y	Y
Y-Mean	0.014	0.014	0.014	0.014
Observations	200,978	200,978	200,978	200,978
Observations - Explained by FEs		3,544		3,544

Note: This table presents the estimates for the differential change in reverse migration out of the US for Indians and Chinese compared to other immigrants. The dependent variable is an indicator that equals one if the immigrant employee leaves the US for any other country after staying in the US for at least six years. The independent variable is an indicator which is one if an employee is Indian or Chinese interacted with an indicator which is one if the employee starts the job in US post 2005. The result is estimated by the β coefficient obtained from equation 3. Data are collapsed to the employee level. I control for different combinations of industry-location fixed effects and employee controls. Industry is specified at NAICS 4-digit level and location at commuting zone level. Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

times smaller than the baseline reduction of 1.1 percentage points in employee mobility, even in the most sympathetic specification.

4.3.3 Structural Changes in India & China

A third alternative explanation is that changes in time-varying variables specific to Indian and Chinese immigrants drive my results. For example, any changes in home-country economic conditions may make Indian and Chinese immigrants more risk-averse and unwilling to switch firms after 2005. I exploit within-country GC wait-line differences to alleviate these concerns.

GC wait-lines depend on employee qualifications apart from country of birth. Employees can belong to three categories: from EB-1 to EB-3 (employment-based 1 to 3) depending on previous experience and education. PhDs belonging to the EB-1 category did not experience any GC wait-times for any country during my sample period. Hence, I should not find any results for a sample of PhD holders if GC restrictions drive my results. On the other hand, I should see an equivalent decrease in mobility in the placebo sample for PhD if confounding variables specific to Indian and Chinese immigrants explain my results. I present the results

Table 6: **Robustness: Placebo Sample of PhDs**

	Dependent Variable: 1(Move Firm)					
	(1)	(2)	(3)	(4)	(5)	(6)
1(YearFirstJob > 2005) × 1(Indian/Chinese)	0.001 (0.003)	0.001 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.001 (0.002)	-0.001 (0.001)
Industry × Location FEs		Y			Y	Y × Year
Employee Controls			Y		Y	Y × Year
Job Year Controls				Y	Y	Y × Emp Cntrl
Y-Mean	0.10	0.10	0.10	0.10	0.10	0.10
Observations	317,573	317,573	317,573	317,573	317,573	317,573
Observations - Explained by FEs		239			239	14,195

Note: This table presents estimates on the impact of changes in GC wait-lines on immigrant employee mobility for a placebo sample of PhD applicants who were not subject to GC wait-lines. The independent variable is an indicator which is one if an employee is Indian or Chinese interacted with an indicator which is one if the employee starts the job in US post 2005. The β coefficient is obtained from equation 2. This table is similar to table 2, except I use a sample of immigrants holding PhD degrees. I control for different combinations of industry-location-year fixed effects, and employee controls interacted with year and number of years on the job for both panels. Industry is specified at NAICS 4-digit level and location at commuting zone level. Employee controls include indicators for any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

for this test in table 6 and figure C.7. Reassuringly I find small and insignificant results for the placebo sample.

Next, I divide my existing sample of employees into EB-2 and EB-3 categories. EB-2 employees hold a master's degree or at least five years of relevant work experience. While I do not directly observe the GC application category for any employee, I create an indicator for EB-2 applicants based on their work experience and education at the start of their first US job. We would expect the decrease to be larger for the EB-3 category even within the same country as these applicants faced much longer wait-times from 2005 to 2010 (ten years for EB-3 as compared to five for EB-2). I interact the treatment variable identifying Indian and Chinese employees post 2005 with an indicator for the EB-2 category, and run a specification similar to equation 2. I present the results in table 7 and figure C.7. I document that reduction in mobility is more than twice in magnitude for EB-3 employees compared to EB-2 across specifications. This within-country heterogeneity is consistent with GC related frictions rather than changes in country-level variables driving my results.

Table 7: **Robustness: Heterogeneity by Employee Skill**

	Dependent Variable: 1(Move Firm)					
	(1)	(2)	(3)	(4)	(5)	(6)
1(YearFirstJob $>$ 2005) $\times$ 1(Indian/Chinese)	-0.035*** (0.009)	-0.033*** (0.007)	-0.026*** (0.006)	-0.026*** (0.006)	-0.026*** (0.005)	-0.019*** (0.004)
1(YearFirstJob $>$ 2005) $\times$ 1(Indian/Chinese) $\times$ 1(EB-2 Visa)	0.030*** (0.006)	0.028*** (0.004)	0.017*** (0.005)	0.017*** (0.005)	0.018*** (0.004)	0.010*** (0.003)
Industry $\times$ Location FEs		Y			Y	Y $\times$ Year
Employee Controls			Y		Y	Y $\times$ Year
Job Year Controls				Y	Y	Y $\times$ Emp Cntrl
Y-Mean	0.14	0.14	0.14	0.14	0.14	0.14
Observations	1,157,377	1,157,377	1,157,377	1,157,377	1,157,377	1,157,377
Observations - Explained by FEs						32,918

Note: This table presents estimates on heterogeneity in the impact of changes in GC wait-lines on immigrant employee mobility by visa category. The independent variable is an indicator which is one if an employee is Indian or Chinese interacted with an indicator which is one if the employee starts the job in US post 2005 and an indicator if employee qualifies for the EB-2 visa. Employees with more than five years of previous experience or master's degree are classified as EB-2 and the rest as EB-3. The β coefficient is obtained from equation 2. I control for different combinations of industry-location-year fixed effects and employee controls interacted with year and number of years on the job for both panels. Industry is specified at NAICS 4-digit level and location at commuting zone level. Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

4.3.4 Changes in Employee Productivity

Finally, I address the concern that the GC shock affected employee productivity or quality in turn, which caused a change in mobility. As an example, longer wait-times may increase employee stress and reduce employees' motivation to switch jobs. Gortmaker et al. (2021) report an increase in employee networking activity on LinkedIn when trying to change jobs. Hence, if a reduction in the employee motivation to switch jobs drives my results, I should see fewer LinkedIn connections and recommendations for Indian and Chinese cohorts post the 2005 shock. However, as shown in figure 4 and table C.3, I see no economically or statistically significant change in any of these outcomes.

5 Firm Value for Publicly Traded Firms

I study the impact of labor mobility on firm value for large publicly traded companies in this section. I start by documenting the competing theories of the effects of labor mobility on firm value. I then study the impact of the GC announcement on firm daily stock returns. I extend my analysis to Tobin's Q to check if the effect on firm value is transitory or persists over extended durations. I conclude by testing various channels which may drive the firm value results.

5.1 Hypothesis Development

The relation between labor mobility and firm value is not theoretically obvious. Lower labor mobility decreases the workers' ability to generate outside offers. Hence, it reduces their bargaining power, allowing firms to underpay workers and capture a more significant proportion of wage rents compared to a counter-factual scenario with full labor mobility (Eisfeldt and Papanikolaou, 2013; Donangelo, 2014). Higher employee retention may also reduce firm adjustment costs related to hiring and training new workers, or due to disruptions to existing projects post loss of critical labor (Oi, 1962; Belo et al., 2014). Finally, lower labor mobility limits the ability of essential workers to leave the firm post-investment in a project, reducing the possibility of worker holdout post-investment. Firms can now invest in positive net present value (NPV) projects that they may have ignored previously resulting from the threat of disruption due to the loss of employees (Acemoglu and Shimer, 1999).

On the other hand, reducing employee ability to generate outside options (due to lower mobility) may also reduce employee incentives to work hard. Reduced employee motivation may translate into lower productivity, reducing the overall value for the firm (Acharya et al., 2013; Fulghieri and Sevilir, 2010). Finally, previous literature has stressed knowledge spillover/transfer as a significant contributor to firm innovation and value (Audretsch and Feldman, 1996; Carlino and Kerr, 2015). The reduction in employee movement across firms reduces such spillovers across firms and hence may decrease the value generated by each firm. These competing theories make it essential to study this question empirically to understand both the direction and driving mechanisms of the impact of labor mobility on firm value.

5.2 Empirical Specification

Section 4 details the unexpected change in GC wait-lines in October 2005, which caused an eight percent decrease in mobility of Indian and Chinese immigrants. Changes in employee characteristics, selection on stayers, or other confounding variables cannot explain this increase. I now use this natural experiment to study the impact of changes in labor mobility on firm value for large publicly listed firms. I identify this effect by analyzing the changes in firms with a high proportion of Indian and Chinese workers around the 2005 GC shock. Firms that did not have Indian and Chinese workers serve as the control group. I use the proportion of Indian and Chinese workers in the firm as of 2005 to ensure that I capture the impact of GC shock on existing employee base and not changes in hiring preferences due to the shock itself. For any firm i at time t:

$$Y_{i,t} = \beta_t * \alpha_t * Ratio_i + [\alpha_i + \gamma Industry_i * \alpha_t + \eta X_{i,t}] + \varepsilon_{i,t} \tag{5}$$

Here $Ratio_i$ is the percentage of Indian and Chinese employees in GC wait-lines[22] in firm i as of 2005 and α_t is an indicator for each time period. The coefficient of interest is β_t, which captures the impact of having more Indian and Chinese employees for time t. We expect to see differential outcomes for firms with a higher proportion of Indian and Chinese employees after the GC mobility shock: post 8 September 2005 for daily data,[23] post 2005 Q2 for quarterly data, and post 2005 for annual data. I control for firm fixed effects to ensure inherent differences between firms do not drive my results. I also control for industry (defined at NAICS 4-digit level) interacted with time to make sure I do not capture any industry-level changes. I use firm controls commonly used in corporate finance literature, such as firm size, leverage, return on assets, sales growth, and cash ratio. I present my results with and without firm-level controls (interacted with time) to ensure firm-level determinants do not drive them. I cluster standard errors by industry.

The central identifying assumption is that both firms with and without Indian and Chinese employees would have continued on parallel trends in the absence of the GC shock. If the assumption is satisfied, we should see no pre-trends in β_t before the GC shock. I also estimate

[22] Indian and Chinese immigrants in GC wait-lines identified as in Appendix B. I limit to immigrants who started US job in past decade to omit immigrants having already obtained GCs.

[23] While GC wait-lines came in effect from October 2005; the announcement was made on 8 September 2005 (https://travel.state.gov/content/travel/en/legal/visa-law0/visa-bulletin/2006/visa-bulletin-for-october-2005.html).

the regression counterpart for the event study:

$$Y_{i,t} = \beta * \mathbb{1}(Shock_t) * Ratio_i + [\alpha_i + \gamma Industry_i * \alpha_t + \eta X_{i,t}] + \varepsilon_{i,t} \tag{6}$$

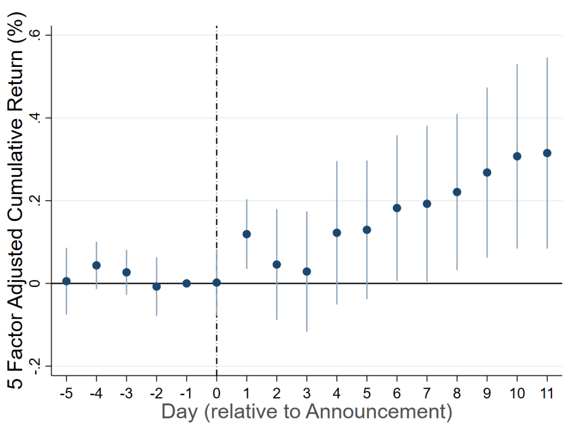
This is similar to equation 5 except I use $\mathbb{1}(Shock_t)$, an indicator that on post the GC shock.

5.3 Results

5.3.1 Firm Returns

I start by studying the effect of labor mobility changes on daily abnormal cumulative stock returns. Focusing on the differential daily stock returns around the exact date of GC wait-list announcements help to cleanly identify the effect of the GC shock rather than any other firm-level changes. These event studies also help ascertain if the stock market updates its expectations about firm value post GC shock. As the data is at a firm-day level, firm fixed effects absorb quarterly firm controls.

Figure 5: **Event Study: Daily Cumulative Return (Fama 5 Factor Adjusted)**



Note: This figure presents event studies on the change in firm returns post GC shock. The y-axis plots the Fama-French 5 factor adjusted cumulative daily return (in percentage). The x-axis plots days around GC shock. The dashed line indicates the time (September 8, 2005) when GC shock is first announced. September 7, 2005, is the omitted period set equal to zero. I plot the differential outcomes for firms with a higher percentage of Indian and Chinese employees (as of 2005) as estimated by the β coefficient obtained from equation 5. I control for firm and industry-day fixed effects. Industry is specified at NAICS 4-digit level. I winsorize all variables at a 1% level. Standard errors are clustered by industry (NAICS 4-digit).

Figure 5 shows the daily event study associated with the announcement. I plot the impact of having one percentage point more Indian and Chinese employees on the Fama-French 5 factor adjusted cumulative daily returns. There are no pre-trends up to five days before the announcement date. Returns for firms with a higher proportion of Indian and Chinese employees increase abruptly on the day after the announcement. This increase is persistent, with these firms gaining consistently in returns over the next ten days. I adjust for the five Fama-French factors and control industry-day trends, ensuring that typical asset pricing factors or industry differences cannot explain this return increase.

Table 8: **Effect of GC Shock on Firm Returns**

	Market Adjusted Returns (%)		FF 3 Factor Adjusted (%)		FF 5 Factor Adjusted (%)	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Date > Sept/08/2005) $\times$ Ratio (%)	0.151* (0.086)	0.097 (0.072)	0.230*** (0.079)	0.141** (0.068)	0.267*** (0.083)	0.164** (0.076)
Firm FEs	Y	Y	Y	Y	Y	Y
Industry $\times$ Time FEs		Y		Y		Y
Y-Mean	-2.27	-2.27	-4.43	-4.43	-2.40	-2.40
Observations	62,498	62,498	62,498	62,498	62,498	62,498
Observations - Explained by FEs	1	528	1	528	1	528

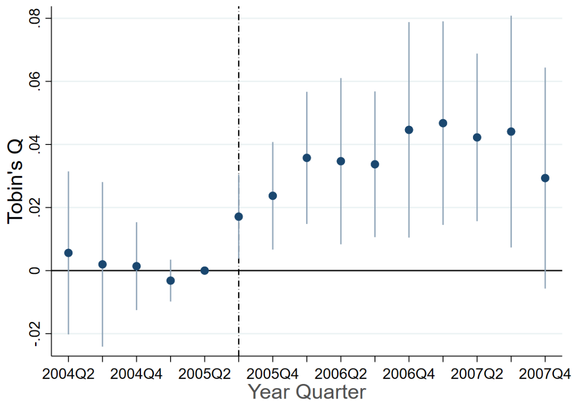
Note: This table presents estimates for the effect of labor mobility on firm returns. The independent variable is the ratio (in percentage) of Indian, Chinese employees in the firm as of 2005 interacted with an indicator which switched on for periods post GC shock: post 8 September 2005 for variables defined at a daily level. The β coefficient is obtained from equation 6. Col. 1 and 2 present results for market-adjusted returns, Col. 3 and 4 for Fama-French 3 Factor adjusted returns, and col. 5 and 6 for Fama-French 5 factor adjusted returns. All specifications include firm fixed effects and alternate specifications include industry-time fixed effects. Industry is specified at NAICS 4-digit level. I winsorize all variables at a 1% level. Standard errors are clustered by industry (NAICS 4-digit). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

I present the regression counterparts in table 8. To be conservative, I pick Fama-French 5 factor adjusted daily returns with industry-day fixed effects (col. 6) as my base specification. An increase of one percentage point in Indian and Chinese employees increases daily returns by 0.16 percentage points. This return is especially impressive since an average firm had a cumulative abnormal return of -2.4% during this period. I present market-adjusted cumulative abnormal returns in col. 1 and 2, the Fama-French 3 factor adjusted cumulative returns in col. 3 and 4, and the Fama-French 5 factor adjusted cumulative returns in col. 5 and 6. My results are larger after adjusting for Fama-French factors. As firms with Indian and Chinese employees tend to be larger, adjusting for Fama-French factors is critical for controlling for firm size premium. I also show results both with and without industry-day fixed effects in alternate columns. My results are large and significant across both types of specifications.

I estimate the overall and per employee impact to better understand the magnitude of the increase in firm returns in table C.6. The specification is similar to table 8, except I use a binary indicator for any firm having Indian and Chinese immigrant employees rather than the ratio of Indian and Chinese employees to estimate the average magnitude of effect for any treated firm. I multiply my coefficient to the total equity value for all firms having any Indian and Chinese employees at the end of 2005 Q2 to estimate the total increase in market value. I find an increase of \$28.7 billion in the market value for treated firms attributable to the GC shock, highlighting the aggregate importance of GC frictions. Further, I divide the overall increase with estimated numbers of Indian and Chinese employees in treated firms. I find that the shock led to an increase of \$104,000 in firm returns for every Indian and Chinese employee. This increase corresponds to 15% of the annual wage for each extra year the employee spends in a GC wait-line.[24] These results indicate that stock market participants are sensitive to immigration related mobility frictions and value each year of increased GC wait-line at 15% of employee salary.

5.3.2 Firm Value

Figure 6: **Event Study: Tobin's Q**



Note: This figure presents event studies on the change in firm value post the 2005 GC shock. The y-axis plots Tobin's Q for the firm. The x-axis plots quarters around GC shock. The dashed line indicates the time (2004 Q3) when GC shock is first announced. 2004 Q2 is the omitted period set equal to zero in all panels. I plot the differential outcomes for firms with Indian and Chinese employees compared to others as estimated by β coefficient (associated with percent of Indian and Chinese employees in the firm as of 2005) obtained from equation 5. I control for firm and industry-quarter fixed effects. Industry is specified at NAICS 4-digit level. I also control for time-varying firm-level variables: size, ROA, leverage, cash ratio, and sales growth interacted with the quarter. Definition for each variable construction is available in section B.1. I winsorize all variables at a 5% level. Standard errors are clustered by industry (NAICS 4-digit).

I study Tobin's Q changes over two years after the GC shock to ascertain if the increases in firm value are persistent. I do not find any significant pre-trend for up to five quarters before the

[24] \$104,000 extra return over seven years extra in GC wait-line, 15% of average annual wage of \$98,000 (from PERM data) for these employees.

GC shock. There is a large increase in firm value in 2005 Q3 (just as the USCIS announced new GC wait-lines) for firms having a higher proportion of Indian and Chinese employees. Inherent differences in firms cannot explain this increase, as I control for firm fixed effects. These effects are not the result of time trends across industries, as I control for industry-quarter fixed effects. To ensure that other firm-level changes do not drive my results, I control for firm characteristics commonly used in corporate finance literature (size, leverage, return on assets, sales growth, and cash ratio). Another concern might be that the time-varying impact of firm characteristics drives my results. For example, suppose the effect of size on firm value increases with time. As larger firms are more likely to hire Indian and Chinese workers, we would see an improvement in Tobin's Q ratio, but it would result from the change in the impact of size and worker mobility. I interact firm controls with an indicator for each quarter to address this concern.

Table 9: **Effect of GC Shock on Firm Value**

	Dependent Variable: Tobin's Q				
	(1)	(2)	(3)	(4)	(5)
1(Quarter > 2005Q2) $\times$	0.028***	0.018*	0.021**	0.035***	0.033***
Ratio (%)	(0.008)	(0.010)	(0.010)	(0.009)	0.000
Firm FEs	Y	Y	Y	Y	Y
Industry $\times$ Time FEs	Y			Y	Y
Firm Controls		Y	Y $\times$ Qtr	Y	Y $\times$ Qtr
Y-Mean	2.12	2.12	2.12	2.12	2.12
Observations	50,236	50,236	50,236	50,236	50,236
Observations - Explained by FEs	479	16	16	479	479

Note: This table presents estimates for the effect of labor mobility on firm value. The independent variable is the ratio (in percentage) of Indian, Chinese employees in the firm as of 2005 interacted with an indicator which switched on after 2005 Q2, post the GC shock. The β coefficients are obtained from equation 6. I present results on changes in Tobin's Q. All specifications include firm fixed effects. Col. 1 presents results with industry-time fixed effects, col. 2 with firm controls, col. 3 with firm-level controls interacted with time, and col. 4 with firm controls and industry-time fixed effects, and col. 5 with firm controls interacted with time and industry-time fixed effects. Industry is specified at NAICS 4-digit level. Time-varying firm controls include size, ROA, leverage, cash ratio, and sales growth. Definition for each variable construction is available in section B.1. I winsorize firm characteristics at a 5% level and the ratio of Indian and Chinese employees at a 1% level. Standard errors are clustered by industry (NAICS 4-digit). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

I next present the regression counterpart of the above results in table 9. I take my most granular specification with industry-time fixed effects and firm-level controls interacted with time (col. 5) as my base specification. Firms with one percentage point higher proportion of Indian and Chinese immigrant employees experience an increase of .033 in Tobin's Q post GC shock. This increase is 1.6% of the sample mean. These results are not driven by other firm-level determinants and are robust to including firm-level controls, with or without interaction with time fixed effects. Col. 1 presents a coefficient of 1.3% of the sample mean without firm control, and col. 4 shows a coefficient of 1.7% of the sample mean with firm-level controls not interacted with time. Industry-level time trends cannot account for my results, with my estimates being larger comparing firms within the same industry-quarter. My results increase from 1% of the sample mean in col. 2 and 3 to 1.6% of the sample mean after controlling for industry-quarter fixed effects. These estimates are on the lower end of previous results on the impact of labor mobility on firm value.[25] My results reinforce that labor mobility plays a sizeable role in determining short-term firm return and long-term firm value.

The increase in firm value is robust to a battery of specification checks as in table C.4. This increase is not driven by bias due to the two-way fixed effects specification and is robust to using the doubly robust differences-in-differences estimator as in Sant'Anna and Zhao (2020) in panel A1. Another concern may be that these results hold only for a particular functional form of Tobin's Q. This is not the case with results robust to using Market by Ratio in panel A2 and log Tobin's Q in panel A3. These estimates are robust to alternate definitions for independent variable (Ratio), with Panel A4 and C1 presenting regressions using a binary indicator, and indicators for cut-offs of one percent and two percent for Indian and Chinese employees, respectively. As most public firms in my data are multi-geographical, I do not use location-year controls in the base specification. I show that my results are not the result of location trends by controlling for commuting zone-quarter fixed effects in panel A5. Another concern might be that the treatment and control groups are un-observably different and hence not comparable. To address this concern, I match 1,241 firms out of the total 3,670 firms in the sample. Panel B1 documents that my results remain similar for such a matched sub-sample. These estimates are persistent over a longer time period of 2001 to 2010, as in panel B2. My results are also robust to alternate sample definitions, such as including financial firms, excluding small firms (less than one million in assets), removing firm-quarters with M&A activity, and excluding outsourcing firms in panels B3 to 6. Panel C2 shows that my estimates

[25] I document an increase of 1.45% over mean for one standard deviation decrease in employee mobility, comparable to 1.4 to 1.7% documented by Shen (2021).

are driven by knowledge industries, with greatest importance of human capital, consistent with estimates capturing the impact of labor mobility frictions. Panel D presents a falsification test with ratio of non Indian and Chinese immigrants to total employees as the placebo group. If firms with immigrant employees are inherently different to others, firms with a higher percentage of non Indian and Chinese immigrants should see a increase in value similar to firms with a large ratio of Indian and Chinese immigrants. However, I only observe an increase in value with firms with the ratio of Indian and Chinese immigrants and no significant results for firms with the ratio of non Indian and Chinese immigrants, consistent with changes in employee mobility driving the results.

A final concern here is that of changes in firm hiring driving these results. The shock may impact the incentives of firms to hire immigrant employees. This can then increase firm value if these employees have differential productivity or quality compared to natives. I directly verify whether this is the case by looking at changes in composition and number of immigrant employees hired after the shock in table C.5. The specification is similar to equation 6, with annual data to capture the changes in firm hiring. I do not find any significant change in the ratio of Indian and Chinese or other immigrants being hired by treated firms after the GC shock. Similarly, there are no changes in the growth of overall Indian and Chinese or other immigrants hired by treated by firms after the GC shock. More than seventy percent of GC immigrant employees come to the US as students for an educational degree. The inability of firms to directly control the number or composition of students coming to the US, may explain the null effect on immigrant hiring by firms.

5.4 Potential Mechanisms

Two potential mechanisms can account for the increase in firm value following the reduction in employee mobility. First, the increase in employee mobility restrictions may help enhance firm monopsony power and improve firm profitability, bolstering the firms' current cash flows. Second, limiting the ability of workers to leave may enable firms to invest in positive NPV projects previously impossible for fear of post-investment holdout. This increase in investment may improve the firms' future cash flows. I do not consider industry level changes (such competitive landscape) as potential mechanisms, because my estimates capture only within industry-year increases in firm value. I first use employee-level data to document direct evidence of firm monopsony power. Next, I use firm-level data to show that the GC shock reduced firm operating costs, but did not impact firm investment or innovation outcomes, consistent with monopsony power as the primary mechanism behind my results.

I directly test for enhanced firm monopsony power using employee-level data to study the effect of GC restrictions on within-firm promotions. Previous work shows that firms pay most immigrant employees near the prevailing wage mandated by the DOL for their level, industry, and location (Matloff, 2013). Additionally, job titles serve as a useful wage proxy, capturing over 90% of the variation in employee wages (Marinescu and Wolthoff, 2020), especially for technology companies where salaries are bench-marked by employee title.[26] Hence, slowing promotions is the only channel available to firms to wage discriminate against immobile employees. Anecdotal evidence supports the idea of technology firms promoting employees based on their bargaining power rather than only skill. For example, Google instituted a policy to provide employees a promotion, within an hour of receiving an offer from Facebook.[27]

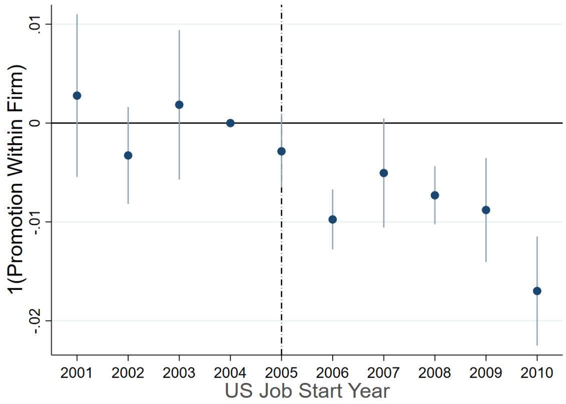
I follow my baseline specification as developed in section 4 and equation 1 to test firm monopsony. The outcome variable captures internal firm promotions and turns equal to one if an employee changes job titles within the same firm in the year. I present my results in figure 7. There are no significant pre-trends in employee promotions for Indian and Chinese employees before the 2005 GC shock. There is a sudden decrease in within-firm employee promotions for Indian and Chinese employees entering the US after the GC shock. Similar to figure 3, this decrease is persistent and greater in value for future cohorts as GC wait-times become larger. I present the regression counterpart of my results in table 10. My most granular specification (col. 6) documents a decrease of 13% over the mean in with-in firm promotions for Indian and Chinese cohorts post GC shock. Cols. 1 to 5 show that this decrease is significant across all different combinations of industry-location-year controls and employee characteristics.

I study changes in firm profitability and investment outcomes to understand which of these

[26]Levels.fyi (https://www.levels.fyi/) benchmarks salary by job designation across tech firms.

[27]https://techcrunch.com/2010/09/15/google-fights-back-in-battle-for-talent-but-may-be-creating-a-worse-problem-for-itself/?guccounter=1;https://qz.com/234455/the-eric-schmidt-email-confirming-googles-one-hour-window-to-counter-facebook-job-offers/.

Figure 7: **Mechanisms: Event Study on within-Firm Promotion**



Note: This figure presents event studies on immigrant employee's within-firm promotions around the introduction of GC wait-lines. The y-axis plots an indicator that equals one if the employee is promoted within the same firm in that year and is zero otherwise. The x-axis plots the year an employee first started working in the US. The dashed line indicates the year 2005 when GC wait-lines were introduced. 2004 is the omitted year set equal to zero in both panels. I plot the differential impact of GC wait-lines on Indians and Chinese compared to other immigrants as estimated by the β coefficient obtained from equation 2. I control for industry-location-year fixed effects. Industry is specified at NAICS 4-digit level and location at commuting zone level. I also control for granular employee controls interacted with the number of years on the job for each employee. Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone).

mechanisms drives my results. Both investment and wages are long-term outcomes and may not change immediately post GC shock. We would only see a change in firm investment rates once the firm gets an opportunity to invest. Similarly, firms may not decrease employee salary immediately after the shock, but rather reduce the rate of annual increments and new hiring to increase profit over a longer period. Therefore, I use annual data from 2001 to 2010 and a modified version of equation 6 to study these outcomes. I control for firm fixed effects and industry-year controls across all specifications. I present these results for these tests in table 11. I find a 0.67 percentage point increase in profitability for firms with one percentage point more Indian and Chinese employees post the 2005 shock. The rise in profitability stems from decreases in operating expenses. I find evidence for economically significant reductions in COGs and SGA[28] and operating expenses for firms with more Indian and Chinese employees post GC shock. Similarly, I look at firm investment ratio and patents to understand the impact of labor mobility restrictions on firm investment. We should expect the firm's investment ratio to increase, and the firm to get more patents per year if increases in firm value are driven by

[28] I exclude firm R&D costs as these include salaries and firm investment. Reductions in labor mobility decrease employee salaries but increase firm investment, making the effect on R&D unclear.

Table 10: **Mechanism: Effect on Within-Firm Employee Promotions**

	Dependent Variable: 1(Promotion Within)					
	(1)	(2)	(3)	(4)	(5)	(6)
1(YearFirstJob > 2005) × 1(Indian/Chinese)	-0.012***	-0.010***	-0.011***	-0.012***	-0.011***	-0.009***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
Industry × Location FEs		Y			Y	Y × Year
Employee Controls			Y		Y	Y × Year
Job Year Controls				Y	Y	Y × Emp Cntrl
Y-Mean	0.07	0.07	0.07	0.07	0.07	0.07
Observations	1,157,377	1,157,377	1,157,377	1,157,377	1,157,377	1,157,377
Observations - Explained by FEs						32,918

Note: This table presents estimates on the impact of changes in GC wait-lines on within-firm employee promotions. The dependent variable is an indicator that equals one if the employee gets promoted within the same firm in that year and is zero otherwise. The independent variable is an indicator which is one if an employee is Indian or Chinese interacted with an indicator which is one if the employee starts the job in US post 2005. The β coefficients are obtained from equation 2. This table is similar to table 2. I control for different combinations of industry-location-year fixed effects and employee controls interacted with years and number of years on the job. Industry is specified at NAICS 4-digit level and location at commuting zone level. Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

investment or innovation. However, I find no significant changes in the firm's capex ratio. Similarly, I do not find any economically or statistically significant results on overall firm patents or citations.[29] While it may not be possible to interpret magnitudes of these results directly, they directionally suggest that the increase in firm value coincides with increases in profitability rather than investment. Together with the effects on within-firm promotions, these results reinforce monopsony power as the primary channel behind firm value effects.

6 Startup Formation and Growth

I study the impact of labor mobility on startup outcomes. I start by documenting that GC restrictions have a more significant impact on employee mobility to startups as compared to incumbents. I next explore the competing theories of the effects of labor mobility on startup outcomes. Finally, I check for the impact of GC induced mobility restrictions on new startup entry and existing startup growth.

[29] I use overall patents, and firm patents and citations adjusted for different technology cohorts similar to Hall et al. (2001); Lerner et al. (2011); Bena and Li (2014); Seru (2014); Chang et al. (2015).

Table 11: **Mechanism: Firm Profitability and Investment**

	Firm Profitability			Firm Investment			
	(COGS+SGA)/ Assets	(Op. Exp.)/ Assets	Return On Assets	Capex/ Assets	Log (1+ Patents)	Log (1+Adj. Patents)	Log (1+Adj. Citations)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1(Year >= 2005) × Ratio	-0.0139**	-0.0139**	0.0067**	0.0004	0.0097	0.0026	0.0040
	(0.0065)	(0.0065)	(0.0026)	(0.0004)	(0.0084)	(0.0032)	(0.0093)
Firm FEs	Y	Y	Y	Y	Y	Y	Y
Industry × Time FEs	Y	Y	Y	Y	Y	Y	Y
Y-Mean	1.02	1.02	0.05	0.05	0.01	0.01	0.01
Observations	33,210	33,210	33,210	33,210	33,210	33,210	33,210
Observations - Explained by FEs	331	331	331	331	331	331	331

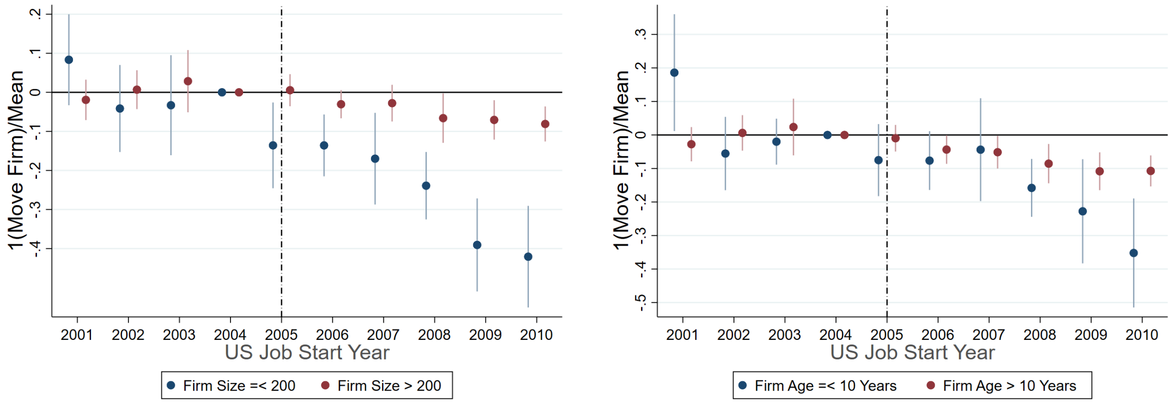
Note: This table presents tests to ascertain mechanisms for the increase in firm value. The independent variable is the ratio of Indian and Chinese employees (in percentage) in the firm as of 2005 interacted with an indicator which switched on post GC shock. The β coefficients are obtained from equation 5. Col. 1 to 3 present the results on firm profitability, and col. 4 to 5 on firm investment. Col. 1 presents results for cost of goods sold and selling, general and administrative expenses scaled by total assets, col 2. for operating expenses scaled by total assets, col. 3 for return on assets, col. 4 for capital expenditure scaled by total assets, col. 5 for the logarithm of total patents applied by firm in year, col. 6 for the logarithm of total patents adjusted for technology category, and col. 7 for citation weighted patents adjusted for technology category. Definition for each variable construction is available in section B.1. All models include firm fixed effects and industry-year fixed effects. I also control for lagged patents (from 3 years prior) for col. 5 to 7. Industry is defined at NAICS 4-digit level. I winsorize all variables at a 1% level. Standard errors are clustered by industry (NAICS 4-digit). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

6.1 Impact of GC restrictions

GC restrictions may have a larger impact on startups' labor mobility than incumbent firms as they introduce an additional downside risk of loss of legal status for immigrant employees joining startups. As discussed in section 2.1, employees need to apply for a PERM labor certification with the DOL as the first step towards a GC application. The DOL verifies that the employer has enough funds to pay prevailing market wages to the employee as a part of this process. Startups have a high amount of variability in their cash flows and are more likely to fail this step, in which case the employee would be unable to continue working legally in the US. Startups also have a higher risk of failure as compared to incumbent firms. This risk is more salient for immigrants in the GC wait-line, as firm closure would mean the automatic loss of legal status for these employees.

I first verify if GC wait-lines reduce an employee's propensity to join startups. I study the differential impact of the sudden increase in GC wait-lines on Indian and Chinese employees compared to others in a specification similar to my baseline in equation 1. I break up the probability of the employee joining a new firm ($1(MoveFirm)$) into two components: the

Figure 8: **Event Study: Employee Mobility by Firm Type**



A: Firm Size B: Firm Age

Note: This figure presents event studies on employee mobility by firm type around the introduction of GC wait-lines. The y-axis plots the probability of the employee switching to a startup (in blue) or incumbent firm (in red) scaled by mean probability for each variable. The x-axis plots the year an employee first started working in the US. The dashed line indicates the year 2005 when GC wait-lines were introduced. 2004 is the omitted year set equal to zero in both panels. Both panels plot the differential impact of GC wait-lines on Indians and Chinese as compared to other immigrants as estimated by the β coefficient obtained from equation 1. Panel B classifies firms based on size with firms with less than or equal to 200 employees being classified as startups and others as incumbents. Panel B classifies firms based on age with firms with an age of less than or equal to ten years being classified as startups and others as incumbents. I control for industry-location-year fixed effects. Industry is specified at NAICS 4-digit level and location at commuting zone level. I also control for granular employee controls interacted with the year and number of years on the job for each employee. Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone).

probability of the employee joining an incumbent firm or a startup. I define startups based on firm size (cut-off of 200 employees) and age (cut-off of ten years). Table 1 shows that the probability of an employee switching to a startup (two percent) is much lower than switching to an incumbent firm (twelve percent). I scale the probability of joining both startups and incumbent firms by their mean values to ensure the comparability of estimates.

I present results in figure 8 and table 12. There is a decrease of 21% over the mean in the probability of treated employees joining small firms post the GC shock. This decrease is quadruple the 5.6% decrease in employee mobility to incumbents. Similarly, the probability of an employee joining young firms decreases by 15% over the mean compared to seven percent to incumbent firms. T-Tests reveal that the coefficients for startups and incumbents are different at one percent and five percent significance levels for firm cuts by size and age, respectively. These results are consistent with GC related frictions having a significant detrimental effect on

Table 12: **Effect of GC Shock on Employee Mobility by Firm Type**

	Dependent Variable: 1(Move Firm) / Mean			
	A: Firm Size		B: Firm Age	
	Start-Up	Incumbent	Start-Up	Incumbent
	(1)	(2)	(3)	(4)
1(YearFirstJob $>$ 2005) $\times$ 1(Indian/Chinese)	-0.211*** (0.036)	-0.056*** (0.019)	-0.150*** (0.038)	-0.072*** (0.021)
T-Test for Equality (P Value)	0.000***		0.031**	
Industry $\times$ Location FEs	Y $\times$ Year	Y $\times$ Year	Y $\times$ Year	Y $\times$ Year
Employee Controls	Y $\times$ Year	Y $\times$ Year	Y $\times$ Year	Y $\times$ Year
Job Year Controls	Y $\times$ Emp Cntrl	Y $\times$ Emp Cntrl	Y $\times$ Emp Cntrl	Y $\times$ Emp Cntrl
Observations	1,157,377	1,157,377	1,157,377	1,157,377
Observations - Explained by FEs	32,918	32,918	32,918	32,918

Note: This table presents estimates on the impact of GC wait-lines on immigrant employee mobility by firm type. The dependent variable is an indicator that equals one if the employee switches to a startup firm in that year and is zero otherwise for col. 1 and 3 and an indicator which equals one if the employee switches to a startup firm in that year and is zero otherwise for col.2 and 4. The independent variable is an indicator which is one if an employee is Indian or Chinese interacted with an indicator which is one if the employee starts the job in US post 2005. The β coefficient is obtained from equation 2. Panel A (col. 1 and 2) classifies firms based on size with firms with less than or equal to 200 employees being classified as startups and others as incumbents. Panel B (col. 3 and 4) classifies firms based on age with firms with an age of less than or equal to ten years being classified as startups and others as incumbents. I control for industry-location-year fixed effects. I also present the p-value from t-test of equality of coefficients for startup and incumbents. Industry is specified at NAICS 4-digit level and location at commuting zone level. I also control for granular employee controls interacted with the year and number of years on the job for each employee. Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

the probability of employees joining startups.

6.2 Hypothesis Development

The impact of labor mobility on startups is not clear *ex-ante*. Previous work has shown skilled labor to be a critical component in startup growth (Bhide, 1994; Hellmann and Perotti, 2011; Babina and Howell, 2018). Choi et al. (2021) document the high significance for both founders and early joiners for startup growth. Labor mobility restrictions reduce the probability of employees joining startups. This reduction in access to critical human resources may stifle startup entry and lower the probability of success for existing startups.

On the other hand, mobility restrictions also limit the possibility of disruptions due to the departure of valuable human capital. The inability of employees to leave the firm may help entrepreneur more credibly commit human capital to the venture, increasing the ability of the startup to obtain financing, as in Bolton et al. (2019). This increase in funding may help new firm formation rates go up, despite employee availability declines. Lower employee mobility may also result in lowering salary and other adjustment costs related to employee departures. If the impact of increased investment and lower adjustment costs dominate over reduced labor access, we may observe better outcomes for treated startups.

A benefit of using the GC shock is that it allows us to study the impact of entrepreneurship due to lower mobility of non-founder employees while not directly impacting entrepreneurs' ability to found firms. Previously used shocks, such as the tightening of NCs as in Jeffers (2019) reduce entrepreneurs' ability to create firms in the same sector as their previous employer and decrease labor mobility. However, the GC shock does not directly impact the ability of entrepreneurs to found firms. Immigrant employees who have exited GC wait-lines and applied for Adjustment of Status (Form I-485) cannot legally found startups before obtaining an actual GC. As the GC shock only impacts time spent in GC wait-line and not obtaining the GC, there is no direct impact on the ability of employees' to found new firms.

6.3 New Firm Formation

6.3.1 Empirical Specification

I use the unexpected decrease in the probability of Indian and Chinese employees joining startup firms as a natural experiment to understand the aggregate impact of labor mobility on new firm formation. I collapse my data to industry-commuting zone-year level (markets) and study the differential impact on markets having high proportions of Indian and Chinese employees compared to others. Similar to section 5.2, I compute the ratio of Indian and Chinese employees in any market as of 2005 to avoid capturing any changes in hiring preferences associated with the shock. For industry i, commuting zone j and year t:

$$Y_{i,j,t} = \beta_t * \alpha_t * Ratio_{i,j} + [\gamma Ind_i * Loc_j + \delta Ind_i * \alpha_t + \eta Loc_j * \alpha_t] + \varepsilon_{i,j,t} \tag{7}$$

Here α_t is an indicator for the year and $Ratio_{i,j}$ is the percentage of Indian and Chinese employees in GC wait-lines[30] in industry i and commuting zone j as of 2005. The coefficient of interest is β_t, which measures the impact of having one percentage point more Indian and Chinese employees in market i, j, and year t. I weigh all regressions by the total 2005 employment in the market as employees rather than markets are the natural units of economic interest. I control industry-location fixed effects to ensure that any particular market does not drive my results. I control industry and location time trends, ensuring that unobservable changes in any industry or location do not drive my results. I use 114 unique industry definitions as defined by LinkedIn, as these are more detailed than NAICs 4-digit, but I also present my results with NAICs 4-digit as industry controls. Similarly, I use commuting zones, the most dis-aggregated level at which location data is available as controls and show my results are robust to using states instead of commuting zones. I cluster standard errors by location and industry. I verify the parallel trends identification assumption by checking for the absence of pre-trends using event studies. I implement the regression version of my main specification using the equation:

$$Y_{i,j,t} = \beta * \mathbb{1}(Shock_t) * Ratio_{i,j} + [\gamma Ind_i * Loc_j + \delta Ind_i * \alpha_t + \eta Loc_j * \alpha_t] + \varepsilon_{i,j,t} \quad (8)$$

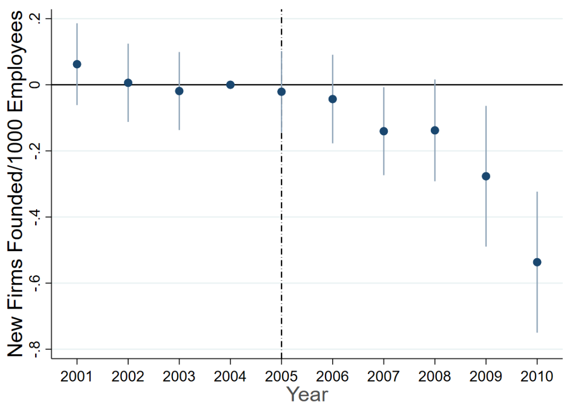
This specification is similar to equation 8, except I use $\mathbb{1}(Shock_t)$ which switches on post 2005, to estimate the impact of GC shock on new firm formation.

6.3.2 Results

I present the event study in figure 9. The outcome variable is the number of new firms formed in the industry-commuting zone pair (market) scaled by the total 2005 employment (in thousands). The pre-period coefficients are statistically indistinguishable from zero before the 2005 GC shock. There is a sudden decrease in new firm formation per employee in markets with more Indian and Chinese employees post 2005, after the introduction of GC wait-lines. The effect on new firm formation becomes more severe from 2006 to 2010. This increase in severity can be due to changes in both the external and internal margins as more cohorts enter GC

[30] Indian and Chinese immigrants in GC wait-lines identified as in Appendix B. I limit to immigrants who started US job in past decade to omit immigrants having already obtained GCs.

Figure 9: **Event Study: New Firm Formation**



Note: This figure presents an event study on the change in new firm formation post 2005 GC shock. The y-axis plots the number of new firms founded in a year in an industry-commuting zone pair. The x-axis plots years around GC shock. The dashed line indicates the year 2005, when GC wait-lines were introduced. 2004 is the omitted year set equal to zero. I plot the differential outcomes for industry-commuting zone pairs with a higher ratio of Indian and Chinese employees (in percentage) than others as estimated by the β coefficient (associated with the ratio of Indian and Chinese employees as of 2005) obtained from equation 7. Industry is specified as the 114 unique industries defined by LinkedIn. I control for industry-commuting zone, industry-year, and commuting zone-year fixed effects. I winsorize all variables at a 1% level. Regressions are weighted by 2005 total employment for each industry-commuting zone pair. Standard errors are clustered by industry and commuting zone.

wait-lines and GC related frictions increase due to the increase in GC wait-lines, as discussed in section 4.2. I control for industry-commuting zone fixed effects to ensure that I am not capturing inherent differences between markets. I control for industry-time and location-time fixed effects to absorb any time varying differences by markets.

I present the regression counterpart of my baseline specification in table 13 col. 2. There is a decline of four percent over the mean in new firms formed per employee for one percentage point increase in the ratio of Indian and Chinese employees in the market. Col. 1 presents my results without industry and location time controls. I obtain results similar to my baseline, suggesting these fixed effects do not impact my results. An important concern may be that my results capture the decrease in solo projects, which represent part-time work, rather than new firms making them susceptible to re-labeling. I drop any solo firms, repeating the same analysis in col. 3. I find a decline of three percent over the mean in employer firms in countries having one percentage point more Indian and Chinese employees post GC shock.

Concurrent changes in confounding variables may explain my results. For example, I may get similar results if markets with a high proportion of immigrants reacted differently to the great financial crisis even within the same industry-year and location-year. I conduct two tests

Table 13: **Effect of GC Shock on New Firm Formation**

	Dependent Variable : New Firm Entry / 1000 Employees				
	No Time Controls	Baseline	Employer Firms	Hetero.	Placebo
	(1)	(2)	(3)	(4)	(5)
1(Year > 2005) × Ratio (%)	-0.289***	-0.233***	-0.119***	-0.110	-0.044
	(0.068)	(0.052)	(0.043)	(0.069)	(0.031)
1(Year > 2005) × Ratio (%)				-0.173**	
X 1(Knowledge Industry)				(0.077)	
Industry × Location FEs	Y	Y	Y	Y	Y
Location × Time FEs		Y	Y	Y	Y
Industry × Time FEs		Y	Y	Y	Y
Y-Mean	6.18	6.18	4.46	6.18	6.18
Observations	395,180	395,180	395,180	395,180	395,180
Observations - Explained by FEs		60	60	60	60

Note: This table presents estimates on the impact of GC wait-lines on new firm formation. The independent variable is the ratio of Indian and Chinese employees (in percentage) in the industry-commuting zone pair as of 2005 interacted with an indicator which switches on post GC shock in 2005. The dependent variable is the number of new firms founded scaled by total employees (in 1000's) in an industry-commuting zone pair in that year. The β coefficients are obtained from equation 8. Col. 1 presents the results with only commuting zone-location fixed effects while col. 2 to 5 present results with additional industry-year and location-year fixed effects. Col 2 presents my baseline specification with industry specified as the 114 unique industries defined by LinkedIn and location specified as commuting zones. Col. 3 repeats the same specification as col. 2 except the dependent variable does not consider any solo firms (firms with "myself-on" or single employee in LinkedIn). Col. 4 repeats baseline specification in triple-difference specification with an indicator for the shock interacted with the ratio of Indian and Chinese employees interacted with an indicator for knowledge industry as the additional independent variable. Col. 5 presents baseline specification but with a placebo ratio of non Indian and Chinese immigrants as the independent variable. I control for industry-commuting zone, industry-year, and commuting zone-year fixed effects. I winsorize all variables at a 1% level. Regressions are weighted by 2005 total employment for each industry-commuting zone pair. Standard errors are clustered by industry and commuting zone. Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

to ascertain if changes in employee mobility drive my results. First, I study the heterogeneity of my results by industry. Suppose changes in labor mobility drive my results. I should observe more significant effects for knowledge industries, such as technology, information, financial services, and professional, scientific, and technical services as these require highly skilled labor, which is costlier to train and replace.[31] Col. 4 reports the results. I find a three percent higher decline over mean in new firm formation for one percentage point increase in the ratio of Indian and Chinese employees in knowledge industries post the GC shock. The coefficient is not significant for other industries, consistent with the results capturing the impact of labor mobility restrictions. Second, I conduct a placebo test using the ratio of non-Indian and non-Chinese immigrants in each market as my independent variable. If these results are the product

[31]NAICS code 51 (Information), 54 (Professional, Scientific and Technical Services), 334 (Computer and Electronic Product Manufacturing), 523 (Securities, Commodity Contracts and Other Financial Investments and Related Activities), and 525 (Funds, Trusts, and Other Financial Vehicles)

of mobility restrictions specific to Indian and Chinese immigrants, I should find much smaller results for other immigrants who experienced smaller changes in employee wait-times. Col. 5 reports the results. Reassuringly, I observe economically small and statistically insignificant results using the ratio of non-Indian and non-Chinese immigrants.

Finally, I quantify the aggregate impact of the reduction in new firm formation in table C.8. I use a similar specification to table 13, except I use an indicator that turns on for any industry-commuting zone pair having any Indian or Chinese employees rather than a continuous ratio to identify the average effect in treated markets. I then multiply the coefficient by the total number of employees in treated markets. I find that there were 12,202 fewer firms formed during the 2006 to 2010 period, compared to the counterfactual scenario with no GC restrictions.

I perform several robustness checks in table C.7. I show that my results are not driven by a specific functional form of the regression and are robust to using the doubly robust differences-in-differences estimator as in Sant'Anna and Zhao (2020) in row A1 and equal-weighted regressions (instead of population-weighted) in row A2. Another concern is that dynamic trends impacting larger markets drives these results. However, my results remain large and significant even after controlling for market size interacted with time in row B1. My results are robust to different market definitions including replacing commuting zone and LinkedIn industry by state and NAICs 4-digit industry in rows B2 and B3, respectively. Row B4 shows that the estimates are robust to controlling for total immigrants interacted with 2005 GC Shock, ruling out the possibility of capturing shifts in immigrant supply. I verify that these estimates are valid for high-growth startups by limiting to only industries with VC investment in row C1. These results are not driven by Indian outsourcing or financial firms and are robust to excluding these industries in rows C2 and C3. I verify that results remain robust to limiting to non crisis period by dropping all observations from 2008 onwards in row C4.

6.4 Existing Startup Outcomes

6.4.1 Empirical Specification

In this section, I try to understand the impact of labor mobility restrictions on growth outcomes for existing startups. I limit my sample to startups founded during or before 2005 to avoid capturing any selection effects due to change in startup entry post GC shock. Another concern is that the presence of Indian and Chinese employees may change market concentration by reducing new firm entry. This may indirectly affect firm outcomes. Hence, it becomes critical to compare firms within the same market-year (industry-commuting zone). I solve this concern by exploiting variation in co-founder ethnicity for new startups within the same market. Kerr and Kerr (2021) document a high degree of co-ethnic hiring: 20% for Indian-founded and 40% for Chinese-founded startups. They find that co-ethnic hiring is associated with greater startup survival and growth. Using this intuition, I check for differences in outcomes for firms with a higher proportion of Indian and Chinese founders post the GC shock. Due to their reliance on co-ethnic hiring networks, these firms are the most likely to suffer from any GC induced labor shortages. On the other hand, these firms are also likely to benefit from any decrease in Indian and Chinese employee bargaining power. This is because these firms have the largest proportion of such employees. I use a name-based algorithm to classify the ethnicity of startup founders, as I care about the founder's ethnicity and not their exact nationality or visa status. Using the proportion of Indian and Chinese origin founders as the instrument allows me to use the complete CrunchBase sample (compared to the matched sub-sample to LinkedIn). Using only CrunchBase data also serves as an independent check to my other results, which compute employee ratios using LinkedIn data. For firm i and year t, my event study specification is:

$$Y_{i,t} = \beta_t * \alpha_t * Ratio_i + [\alpha_i + \gamma Industry_i * Location_i * \alpha_t + \eta X_{i,t}] + \varepsilon_{i,t} \tag{9}$$

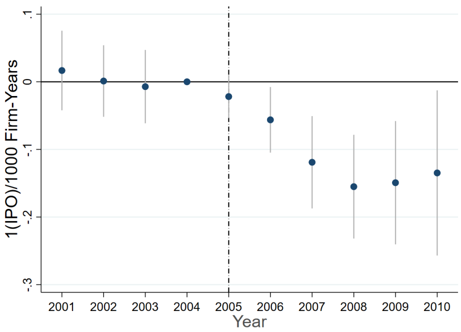
Here α_t is an indicator for time and $Ratio_{i,j}$ is the percentage of Indian and Chinese founders for firm i. The coefficient of interest is β_t, which captures the differential outcomes for startups with more Indian and Chinese founders than others. I control firm fixed effects to ensure that inherent differences between Indian and Chinese-founded startups and others do not drive my results. I remove any market-level time trends with industry-location-time fixed effects. I use industries defined by CrunchBase, as these industry groups are most applicable for startups. I cluster my standard errors by industry and location. I also implement a regression version of my event study using:

$$Y_{i,t} = \beta * \mathbb{1}(Shock_t) * Ratio_i + [\alpha_i + \gamma Industry_i * Location_i * \alpha_t + \eta X_{i,t}] + \varepsilon_{i,t} \qquad (10)$$

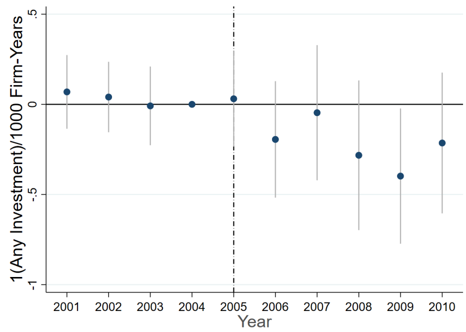
This is similar to equation 9, except I use $\mathbb{1}(Shock_t)$, an indicator that turns on post 2005.

6.4.2 Results

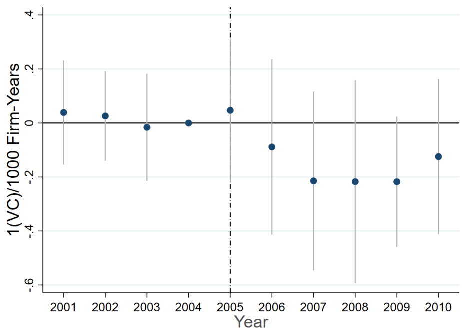
Figure 10: **Event Study: Startup Outcomes**



A: 1(IPO)



B: 1(Any Investment)



C: 1(VC Investment)

Note: This figure presents event studies on the change in startup outcomes post 2005 GC shock. The x-axis plots years around GC shock. The dashed line indicates the year 2005, when GC wait-lines were introduced. 2004 is the omitted year set equal to zero. All panels plot the differential outcomes for startups with a higher percentage of Indian and Chinese founders as compared to others as estimated by the β coefficient obtained from equation 9. Panel A plots an indicator that turns equal to one when a startup has an IPO. Panel B plots an indicator that equals one if the startup receives any investment in that year. Panel C plots an indicator which equals one if startup receives any venture capital (VC) investment in that year. I normalize outcomes by 1000 Firm-Years. I control for firm and industry-commuting zone-time fixed effects. Industry is specified as defined by CrunchBase. Location is defined at the commuting zone level. I winsorize all variables at a 1% level. Standard errors are clustered by industry and commuting zone.

Figure 10 presents my results. I look at three commonly used startup outcomes: the probability of IPO, any investment, and investment by a VC fund. I normalize outcomes by 1000 firm-years to improve the readability of estimates. I find no significant pre-trends in any of these three outcomes before 2005. There is a sudden decline in any investment and VC investment for firms with more Indian and Chinese co-founders post GC shock. This lower investment is accompanied by a reduction in the probability of firm IPO. Differences across firms cannot explain these results, as I control for firm fixed effects. Further, granular industry-location-year effects ensure that I am not capturing any market-level time trends.

I present the regression counterpart of my results in table 14. Startups with one percentage point more Indian and Chinese founders see a 0.4% reduction over the mean in the probability of receiving any subsequent investment post 2005. This reduction is even more severe for VC investment, which reduces by 0.5% over the mean for a one percentage point increase in the ratio of Indian and Chinese founders. There is also a 0.8% reduction over the sample mean in the probability of IPO after 2005 for firms with one percentage point more Indian and Chinese founders. These results are important in aggregate as 12% of all new startups have Indian and Chinese co-founders, with half the founders being Indian and Chinese for the firms with any Indian or Chinese co-founder.

Table 14: **Effect of GC Shock on Startup Outcomes**

	Dependent Variable:		
	1(IPO)/1000 Firm-Years	1(Any Inv.)/1000 Firm-Years	1(VC Inv.)/1000 Firm-Years
	(1)	(2)	(3)
1(Year > 2005) × Ratio (%)	-0.121***	-0.254***	-0.192***
	(0.039)	(0.091)	(0.068)
Firm FEs	Y	Y	Y
Location × Industry × Year FEs	Y	Y	Y
Y-Mean	14.52	70.19	41.8
Observations	103,270	103,270	103,270
Observations - Explained by FEs	11,050	11,050	11,050

Note: This table presents estimates on the impact of GC wait-lines on startup outcomes. The independent variable is the percentage of Indian and Chinese founders interacted with an indicator that switches on post GC shock in 2005. The β coefficients are obtained from equation 10. Col. 1 presents results for an indicator that turns equal to one when a startup has an IPO. Col. 2 presents an indicator that equals one if the startup receives any investment in that year. Col. 3 presents an indicator that equals one if the startup receives any venture capital (VC) investment in that year. I normalize outcomes by 1000 Firm-Years. I control for firm and industry-commuting zone-time fixed effects. Industry is specified as defined by CrunchBase. Standard errors are clustered by industry and commuting zone. Location is defined at the commuting zone level. I winsorize all variables at a 1% level. Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

6.5 Discussion

These results highlight the difference in the impact of mobility restrictions on startups and incumbents. I find that GC induced mobility restrictions enhance firm value for large publicly listed firms, but negatively impact startups, reducing new startup entry and investment and IPOs for existing startups. These dichotomous effects may result from different constraints binding for publicly listed firms and startups. Larger firms, with an emphasis on profitability, are more affected by the firm monopsony mechanism. On the other hand, startups need a steady pipeline of high-skilled talent to grow, with any disruption to talent supply having severe consequences. The negative impact of immigrant mobility restrictions on startups also reinforces how immigrant employees possess skills that are not easily replaceable and that are critical for the US startup ecosystem, as documented earlier by Kerr and Lincoln (2010), Peri et al. (2015), and Dimmock et al. (2019).

7 Conclusion

This paper studies the impact of labor mobility on firm value and startup outcomes. To study this question, I exploit a natural experiment that suddenly increased GC wait-times for Indian and Chinese immigrants by five to seven years. I first show that the increase in GC wait-times reduced the mobility of affected employee cohorts compared to the untreated employees. I further show that other changes in immigrant composition, sample bias, or other concurrent shocks to Indian and Chinese employees do not explain these results. Having demonstrated the shock to be both unexpected and large, I study its impact on publicly listed firms. I find that firms with more treated employees experienced larger increases in returns and Tobin's Q. I document direct evidence of firms' increased monopsony power driving increases in firm value. This decrease in employee mobility harms startup outcomes. Markets with more treated employees have a lower probability of startups formation post GC shock. GC restrictions also hurt the funding and IPO of existing startups.

My results stress the differential impact of immigration mobility frictions on large public firms and startups. While a welfare analysis of the impact of labor mobility frictions is beyond the scope of this paper, it highlights the importance of GC induced labor mobility frictions for incumbent firm monopsony and new firm entry. My results also imply that other systemic frictions to labor mobility, which increase employee risk aversion, may serve as an important explanation for the decrease in US business dynamism.

Previous literature has focused on the importance of immigrant entry restrictions (such as H1-B quotas) as a critical component of US immigration policy. My estimates point to immigrants' mobility as an equally important policy lever. GC wait-times severely limit the mobility of immigrant workers and increase the market power for incumbent firms at the cost of both employees and startups. Thus, any policy to boost immigrant employee mobility can reduce labor market distortions and boost startup growth. Given the large impact of GC restrictions, future research may build on this foundation to investigate additional outcomes such as firm acquisitions or innovation.

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Appendix: For Online Publication

A Anecdotal Evidence on Green Card Shock

Excerpt 1[32]

"In October 2005, the U.S. Department of State (DOS) significantly retrogressed the employment-based immigrant visa (a.k.a. employment "Green Cards") in many categories. The retrogression came as a surprise to many employers and foreign nationals because the DOS previously stated that it did not anticipate that these retrogressions would occur so early. Additionally, the DOS never indicated that the retrogressions would be so severe. However, due to the retrogressions, many foreign nationals will now be unable to commence and/or complete the last stage of the "Green Card" process (namely the adjustment of status process if completed in the United States) for many years until their priority dates become current."

Excerpt 2[33]

"The USCIS does not appear to be able to provide the DOS with exact information about the number of Form I-140 petitions that have been filed based upon these approved labor certification applications and the category (e.g. EB-2 vs. EB-3) and country of chargeability for the principal beneficiary of each of these Forms I-140. "

Excerpt 3 [34]

"Instead, this sudden retrogression is the result of USCIS' backlog of employment-based I-485s being allowed to build up for several years during which approvals of such 485s ground to a near halt, followed by the much-anticipated (and welcome) backlog elimination plan. [3] It's great that USCIS is now suddenly cranking out I-485 approvals. Getting all these 485 approvals recently feels really good, but too much of a good thing is . . . well, not good. The long dry-spell of 485 approvals followed by the recent flood of 485 approvals is what is causing these extreme cut-off dates to suddenly appear with little warning.

[32] https://www.lexology.com/library/detail.aspx?g=6a7b97df-3169-4e11-9313-84bf3baa8f5b

[33] https://www.masudafunai.com/articles/priority-date-retrogression-will-cause-a-significant-increase-in-green-card-processing-times-for-most-foreign-nationals-updated-september-2015

[34] https://www.ilw.com/articles/2005,0916-shenoy.shtm

B Data Construction

B.1 Sample Construction

I filter out a subset of immigrant employees from the complete LinkedIn data. I achieve this in three steps. First, I use employee educational and employment history to try and extract all immigrants to the US during my sample period. I classify any employee who has had initial schooling or jobs outside the US as an immigrant from the corresponding country. To avoid capturing US citizens, I filter out professions that may involve travel or deployment outside of the US, such as military work, international affairs, and maritime work. I predict employee ethnicity using name-based ethnicity predictions as in Ambekar et al. (2009),[35] and I throw out any observations where predicted ethnicity does not match the country classification based on initial education and job location. This check prevents me from wrongly classifying US citizens who began their education or careers in other countries (for example, exchange students or consultants) as immigrants.

Second, I filter out any immigrants who would not need to apply for employment-based GCs. I remove immigrants from Mexico, Canada, Chile, Singapore, and Australia. Each of these countries has special bilateral visa agreements with the US, enabling their citizens to stay in the US indefinitely without applying for GCs.[36] I further remove PhDs, doctors, and managers[37] from the sample as they qualify for the EB-1 (exceptional ability) GC, which may not be tied to the employer and does not show any GC wait-line for any country during my sample period. I also enforce that any firm with a known track record for filing GC applications (obtained from firm-level labor certification filings from the DOL[38]) must have employed the worker at least once to ensure I only capture employment-based GCs. I only keep employees with more than six years[39] of work experience in the US (and in the US as of 2017), to ensure that I capture immigrants who have applied for GCs. This limits my sample to those

[35] Operationalized through package https://pypi.org/project/ethnicolr.

[36] TN visa program for Mexico and Canada, H1B1 for Chile and Singapore, and E-3 for Australia.

[37] Managers refer to multinational executives who qualify for EB1-C GCs. These managers must have been employed in the same firm for the past three years, with at least one year being outside the US.

[38] See: https://www.dol.gov/agencies/eta/foreign-labor/performance and https://www.flcdatacenter.com/CasePerm.aspx

[39] Employees can work in the US for up to 6 years on a short-term H1-B visa without applying for GCs.

started working in the US at the latest by 2010.

Third, I extract the year an employee starts working in the US after completing the highest degree as a proxy for the year an employee starts the GC process (date firm files PERM application). Obtaining the exact date an employee starts the GC process is impossible because of the unavailability of identifiers in administrative data. Limits on renewal of temporary visas and long wait-lines incentivize employees to start the GC process as early as possible and impose caps on how far employers may delay filing, making the start dates a reasonable proxy for the date of GC filing.

B.2 LinkedIn Variables

I impute employee gender from name-based sex classification.[40] I proxy for employee age and experience by the number of years passed between employee finishing a bachelor's degree and starting her first US job. Previous research has shown education's level and location to be a strong predictor for immigrant employment outcomes (Hunt, 2011). I infer the level and location of employee education by checking if the employee has a master's degree and has any degree from a US-based college, respectively. The industry is self-classified by the employee at the time of profile creation. I merge LinkedIn industry classification to four digit North American Industry Classification System (NAICS) codes. I obtain employee commuting zones[41] from the current location in the employee's LinkedIn profile. Finally, I extract the number of employee LinkedIn connections and recommendations as of 2017 from the LinkedIn snapshot, as they predict employee mobility outcomes (Gortmaker et al., 2021).

B.3 Publicly Listed Firms

LinkedIn also collates data for firms along with employees. The firm-level data contains firm name, LinkedIn industry, headquarter location, estimates of current firm size, and company website. I aggregate my immigrant database to calculate the total number and proportion of Indian and Chinese employees, scaled by total employees, as of 2005. Next, I match these

[40] I impute gender using Genderize.io.

[41] https://www.ers.usda.gov/data-products/commuting-zones-and-labor-market-areas/.

firms from LinkedIn to Compustat. I first merge LinkedIn firms to Compustat using the firms' LinkedIn URL or website wherever available. Both these variables are unique and hence suited for the matching operation. I next use firm names to match firms that are still unmerged. I consider firm names listed in Compustat and firm subsidiary names listed in Exhibit 21 of 10-K filings.[42] Finally, I merge any remaining unmatched LinkedIn firms containing more than 0.1% or more than ten Indian and Chinese employees to Compustat firms manually. I assume all other unmatched firms in Compustat to have no Indian and Chinese employees. This assumption is not far from reality, as these firms have less than a 0.1% ratio and ten Indian and Chinese employees in total. I drop any financial services and utility firms from LinkedIn, as is standard in the literature. I also connect this data to firm-level patent data from Global Corporate Patent Dataset from Bena et al. (2017) and the daily stock price data from CRSP.

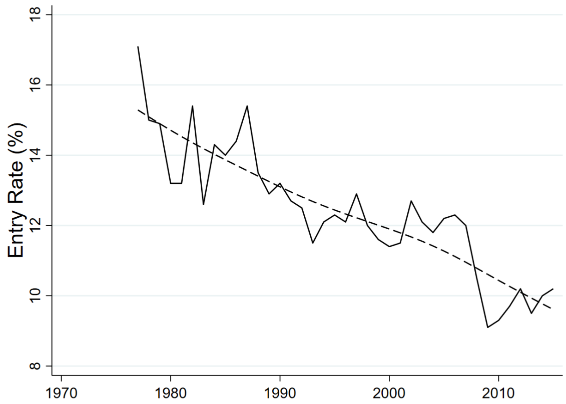
[42] I get firm subsidiary names using Corpwatch API 'http://api.corpwatch.org/companies.json.

Table B.1: **Variable Definition: Compustat Data**

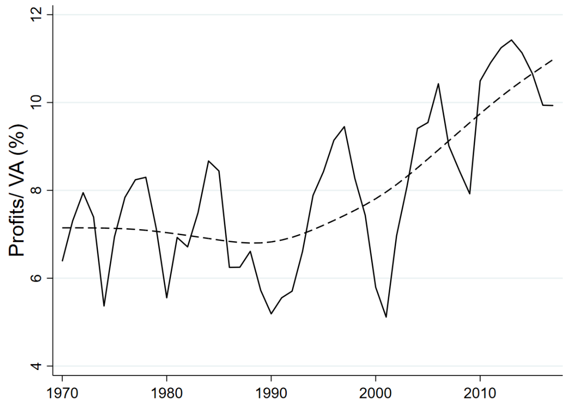
Variable	Definition
Annual Variables (Variables from Compustat, GV Key level patent variables from UVA Darden Global Patent Dataset)	
Tobin's Q	[Total Assets (at) – Common Equity (ceq) + (Common Share Price (prcc_f) * Common Shares Outstanding (csho))] / Total Assets (at)
Market to Book Ratio	(Common Share Price (prcc_f) * Common Shares Outstanding (csho)) / Common Equity (ceq)
Return On Assets	Operating Income before Depreciation and Amortization (oibdp) / Total Assets (at)
(COGs + SGA)/ Assets	[COGS (cogs)+SGA (xsga)] / Total Assets (at); Replace SGA filed as zero is missing for Firm-Year
(Operating Expenses)/ Assets	[Operating Expenses (xopr)] / Total Assets (at)
Capex/ Assets	Capital Expenditure (capx) / Total Assets (at)
Log(1 + Patents)	[Log(1+Patents filed in Year (from UVA Dataset))]; Replace Patents filed as zero is missing for Firm-Year
Log(1 + Adj. Patents)	[Log(1+ Adj. Patents filed in Year (from UVA Dataset))]; Normalize by mean patent filings in same technology cohort-year
Log(1 + Adj. Citations)	[Log(1+ Adj. Total Citations for Patents filed in Year (from UVA Dataset))]; Normalize by mean patent citations in same technology cohort-year
Size	Log(Total Assets (at))
Cash Ratio	Cash and Short-term Investments (che) / Total Assets (at)
Leverage	[Short-term Debt (dlc) + Long-term Debt (dltt)] / Total Assets (at).
Sales Growth	[Sales (sale) in year t –sales in year t-1]/ sales in year t-1.
Quarterly Variables (Variables from Compustat, GV Key level patent variables from UVA Darden Global Patent Dataset)	
Tobin's Q	[Total Assets (atq) – Common Equity (ceqq) + (Common Share Price (prccq) * Common Shares Outstanding (cshoq))] / Total Assets (atq)
Market to Book Ratio	(Common Share Price (prccq) * Common Shares Outstanding (cshoq)) / Common Equity (ceqq)
Return On Assets	Operating Income before Depreciation and Amortization (oibdpq) / Total Assets (atq)
(COGs + SGA)/ Assets	[COGS (cogsq)+SGA (xsgaq)] / Total Assets (atq); Replace SGA filed as zero is missing for Firm-Quarter
(Operating Expenses)/ Assets	[Operating Expenses (xoprq)] / Total Assets (atq)
Capex/ Assets	Capital Expenditure (capxy in quarter t - capxy in quarter t-1) / Total Assets (atq)
Size	Log(Total Assets (atq))
Cash Ratio	Cash and Short-term Investments (cheq) / Total Assets (atq)
Leverage	[Short-term Debt (dlcq) + Long-term Debt (dlttq)] / Total Assets (atq).
Sales Growth	[Sales (saleq) in quarter t –sales in quarter t-1]/ sales in quarter t-1
Daily Returns (Daily returns from CRSP, merged to Compustat using WRDS Linking Table, Factors from Kenneth French's Website)	
Market Adjusted Return	Daily Excess Return (ret -rf) - beta (using 2004 data) * Market Factor (mktrf)
Fama French 3 Factor Adjusted Return	Daily Excess Return (ret -rf) - beta vector (using 2004 data) * 3 Fama French Factors (mktrf, smb, hml)
Fama French 5 Factor Adjusted Return	Daily Excess Return (ret -rf) - beta vector (using 2004 data) * 5 Fama French Factors (mktrf, smb, hml, rmw, cma)

C Supplementary Figures and Tables

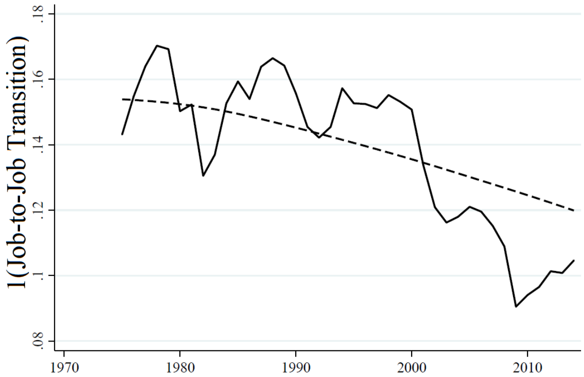
Figure C.1: **Aggregate Trends: Business and Labor Dynamism**



A: Startup Entry Rate (%)



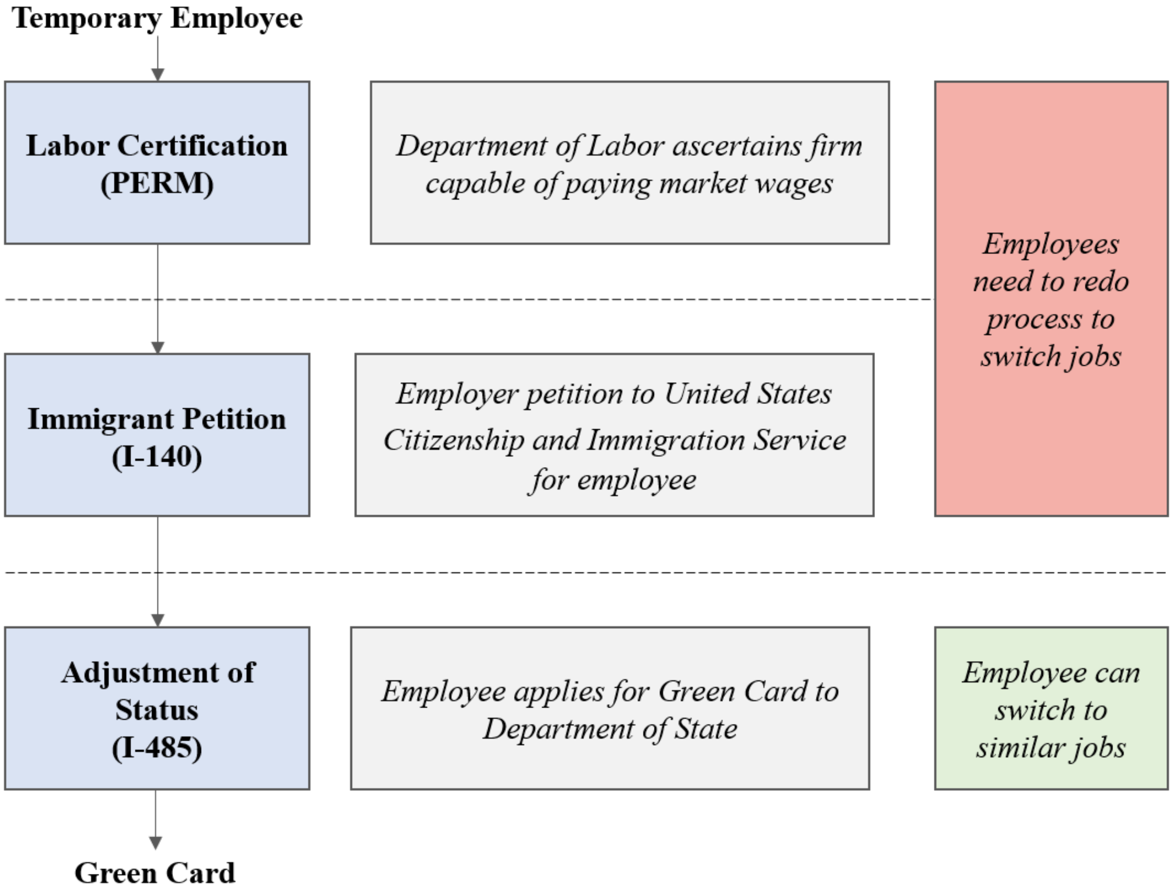
B: Incumbent Profits/ Value Added (%)



C: 1(Job-to-Job Transition Rate)

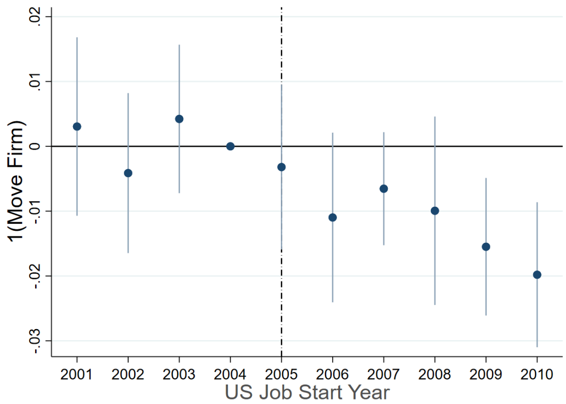
Note: This figure presents aggregate time series trends in business and labor dynamism. The x-axis plots year. Panel A plots the entry rate (in percentage) of new firm formation. Panel B plots the percentage of After Tax Corporate Profits to Value Added for Non-Financial Corporate Sector. Panel C plots an indicator for job-to-job transitions. Panel A and B are obtained from Gutiérrez et al. (2021), and Panel C from Molloy et al. (2016).

Figure C.2: **Green Card Application Process**



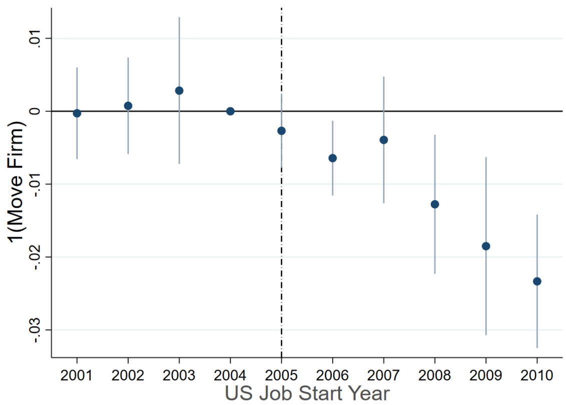
Note: This figure presents the three-step application process for obtaining Green Card. The blue boxes provide the name of the step and its associated form (in brackets). The gray boxes provide a brief description of each step. The third column shows employee mobility once the application for the corresponding step has been completed.

Figure C.3: **Robustness: Change in Mobility Post Immigrant Petition**



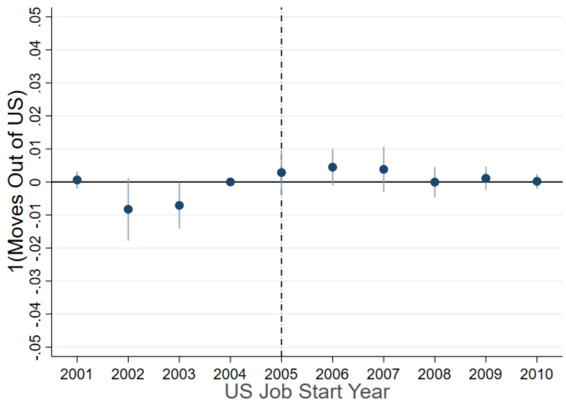
Note: This figure presents estimates of the within-employee change in mobility post immigrant petition for Indian and Chinese as compared to other immigrants. The y-axis plots an indicator that equals one if the employee switches firms in that year and is zero. The x-axis plots the year an employee first started working in the US. The dashed line indicates the year 2005, when GC wait-lines were introduced. 2004 is the omitted year set equal to zero in all panels. I estimate this effect as the triple-diff β coefficient obtained from equation 4. I include fixed effects for each employee. I control for industry-location-year fixed effects. Industry is specified at NAICS 4-digit level and location at commuting zone level. I also control for granular employee controls interacted with the year and number of years on the job for each employee. Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone).

Figure C.4: **Robustness: Mobility for sub-sample entering US pre-2005**



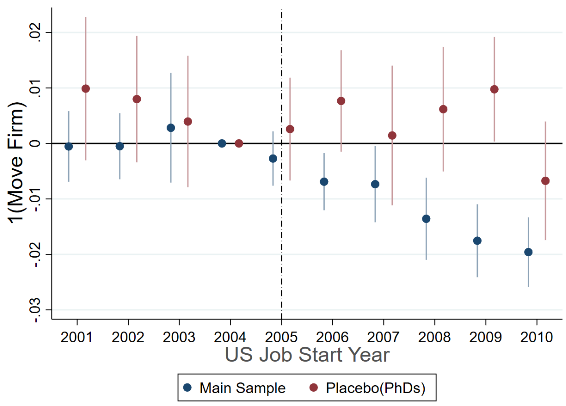
Note: This figure presents estimates on the impact of changes in GC wait-lines on immigrant employee mobility for a sub-sample of immigrants who entered the US before 2005. The y-axis plots an indicator that equals one if the employee switches firms in that year and is zero. The x-axis plots the year an employee first started working in the US. The dashed line indicates the year 2005, when GC wait-lines were introduced. 2004 is the omitted year set equal to zero in all panels. I plot the differential impact of GC wait-lines on Indians and Chinese as compared to other immigrants as estimated by the β coefficient obtained from equation 1. This is similar to figure 3 except I limit the sample to immigrants who had entered the US before 2005. I control for industry-location-year fixed effects. Industry is specified at NAICS 4-digit level and location at commuting zone level. I also control for granular employee controls interacted with the year and number of years on the job for each employee. Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone).

Figure C.5: **Robustness: Reverse Migration**



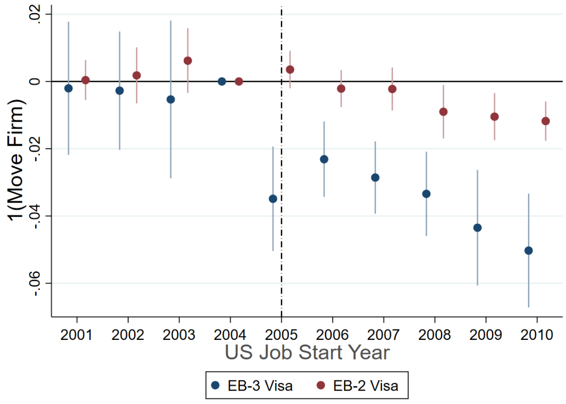
Note: This figure presents the estimates for the differential change in reverse migration out of the US for Indian and Chinese employees as compared to other immigrants. The y-axis plots an indicator that equals one if the immigrant employee leaves the US for any other country after staying in the US for at least 6 years. The x-axis plots the year an employee first started working in the US. The dashed line indicates the year 2005, when GC wait-lines were introduced. 2004 is the omitted year set equal to zero in all panels. I plot the differential change in reverse migration out of the US for Indians and Chinese as compared to other immigrants as estimated by the β coefficient obtained from equation 3. Data are collapsed to the employee-level. I control for industry-location fixed effects. Industry is specified at NAICS 4-digit level and location at commuting zone level. I also control for granular employee controls. Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone).

Figure C.6: **Robustness: Placebo Sample of PhDs**



Note: This figure presents estimates on the impact of GC wait-lines on immigrant mobility for a placebo sample of PhD applicants. The y-axis plots an indicator that equals one if the employee switches firms in that year and is zero. The x-axis plots the year an employee first started working in the US. The dashed line indicates the year 2005, when GC wait-lines were introduced. 2004 is the omitted year set equal to zero in all panels. I plot the differential impact of GC wait-lines on Indians and Chinese as compared to other immigrants as estimated by the β coefficient obtained from equation 2. I plot the coefficients for the baseline sample as in figure 3 in blue and for a placebo sample of PhD applicants who were not subject to GC wait-lines in red. I control for industry-location-year fixed effects. Industry is specified at NAICS 4-digit level and location at commuting zone level. I also control for granular employee controls interacted with the year and number of years on the job for each employee. Employee controls include indicators any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone).

Figure C.7: **Robustness: Heterogeneity by Employee Skill**



Note: This figure presents estimates on heterogeneity in the impact of changes in GC wait-lines on immigrant mobility by visa category. The y-axis plots an indicator that equals one if the employee switches firms in that year and is zero. The x-axis plots the year an employee first started working in the US. The dashed line indicates the year 2005, when GC wait-lines were introduced. 2004 is the omitted year set equal to zero in all panels. I plot the differential impact of GC wait-lines on Indians and Chinese as compared to other immigrants as estimated by the β coefficient obtained from equation 2. I divide the main sample into employment-based visa categories (EB-2 and EB-3). Employees with more than 5 years of previous experience or master's degree are classified as EB-2 (in red) and the rest as EB-3 (in blue). I control for industry-location-year fixed effects. Industry is specified at NAICS 4-digit level and location at commuting zone level. I also control for granular employee controls interacted with the number of years on the job for each employee. Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone).

Table C.1: **Comparison of Administrative and LinkedIn Data**

	Administrative Data	**LinkedIn Data**
A: Country of Origin		
India	46%	43%
China	8%	6%
Others	46%	50%
B: Location (Job State)		
California	22%	25%
New York & New Jersey	20%	16%
Texas	7%	10%
Florida	5%	5%
Others	45%	44%
C: Employer Firms		
Microsoft	1.1%	1.0%
Cognizant	1.1%	0.3%
Qualcomm	0.6%	0.5%
Cisco	0.6%	0.5%
Google	0.4%	0.3%

Note: This table presents a comparison of administrative data to LinkedIn data for Green Card applicants. Administrative data is obtained from Department of Labor certification (PERM) filings. LinkedIn data is constructed as detailed in section 3.1. Both LinkedIn and Administrative data are limited to the years 2007-2010, as complete administrative data is only available for this period. I apply similar filters to administrative data as applied while constructing LinkedIn data (remove immigrants from Mexico, Canada. Chile, Singapore, and Australia and remove any doctors and PhDs) to make both data comparable. Panel A shows the distribution by country of origin, Panel B by US state for immigrant employee, and Panel C by largest employers filing for Green Cards. I combine New York and New Jersey in Panel B as LinkedIn data are available as consolidated for New York- Newark and Jersey City.

Table C.2: **Alternate Specifications: Employee Mobility**

	Dependent Variable: 1(Move Firm)			
	β (1)	Std. Error (2)	Y-Mean (3)	Observations (4)
Panel A: Alternate Specifications				
A1: Doubly Robust Diff-in-Diff Estimator (Sant'Anna and Zhao)				
1(YearFirstJob > 2005) × 1(Indian/Chinese)	-0.012***	(0.003)	0.14	1,157,377
A2: Alternate Definition for Industry				
1(YearFirstJob > 2005) × 1(Indian/Chinese)	-0.011***	(0.003)	0.14	1,116,929
A3: Alternate Definition for Location				
1(YearFirstJob > 2005) × 1(Indian/Chinese)	-0.012***	(0.003)	0.14	1,053,264
Panel B: Alternate Sample Cuts				
B1: Restrict Sample to 7 Years Post Job				
1(YearFirstJob > 2005) × 1(Indian/Chinese)	-0.017***	(0.003)	0.14	835,187
B2: Restrict Sample to Post 2010				
1(YearFirstJob > 2005) × 1(Indian/Chinese)	-0.007**	(0.003)	0.14	706,040

Note: This table presents estimates similar to table 2 with alternate specifications. The dependent variable is an indicator that equals one if the employee switches firms in that year and is zero otherwise. The independent variable is an indicator that is one if an employee is Indian or Chinese interacted with an indicator that is one if the employee starts the job in the US post 2005. The β coefficient is obtained from equation 2. The specification is similar to the baseline specification in col 6 in table 2. Panel A1 implements the doubly robust differences-in-differences approach as per Sant'Anna and Zhao (2020). Panel A2 controls for 114 unique LinkedIn industries instead of NAICS 4-digit industry classification. Panel A3 uses metro areas as defined by LinkedIn instead of commuting zone as the location definition. Panel B1 restricts the sample for each immigrant to a maximum of seven years post entry into the US. Panel B2 restricts the sample to only years after 2010. Industry is specified at NAICS 4-digit level (except panel A2), and location at commuting zone level (except panel A3). Employee controls include indicators for master's degree, any degree from US-based college, employee gender, quartiles of employee experience, above median number of LinkedIn connections, and non-zero number of recommendations on LinkedIn. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

Table C.3: **Comparison: Change in Employee Mobility versus Composition**

	Independent Variable: 1(YearFirstJob > 2005) × 1(Indian/Chinese)				
	β (1)	Std. Error (2)	Y-Mean (3)	β/Y-Mean (4)	Observations (5)
Baseline:					
1(Move Firm)	-0.013***	(0.004)	0.14	-0.09	130,090
Composition Outcomes:					
1(Master's)	-0.016	(0.017)	0.58	-0.03	130,090
1(US Degree)	-0.028	(0.018)	0.69	-0.04	130,090
1(Female)	0.005	(0.007)	0.34	0.02	130,090
Experience (Years)	-0.213*	(0.117)	7.18	-0.03	130,090
# LinkedIn Connections	-0.158	(5.560)	285	0.00	130,090
# LinkedIn Recommendations	0.018	(0.106)	1.54	0.01	130,090
Predicted 1(Move Firm)	0.001	(0.001)	0.14	0.00	130,090

Note: This table presents estimates for change in immigrant employee mobility and composition around the introduction of GC wait-lines. The independent variable is an indicator that is one if an employee is Indian or Chinese interacted with an indicator that is one if the employee starts the job in the US post 2005. The β coefficients are obtained from equation 3. I control for industry-location fixed effects. Industry is specified at NAICS 4-digit level and location at commuting zone level. The first row shows the average probability of an employee changing firms. Rows 2 to 7 show employee composition variables including an indicator for master's degree, any degree from US-based college/ university, employee gender, employee experience before the job in the US (in years), number of LinkedIn connections, and number of recommendations on LinkedIn. Row 8 shows an indicator for the predicted component of employee mobility obtained by regressing employee mobility on the composition factors in rows 2 to 7. Standard errors are clustered by industry (NAICS 4-digit) and location (commuting zone). Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

Table C.4: **Robustness: Firm Value**

	Dependent Variable: Tobin's Q			
	β (1)	Std. Error (2)	Y-Mean (3)	Observations (4)
Panel A: Alternate Specifications				
A1: Doubly Robust Diff-in-Diff Estimator (Sant'Anna and Zhao)				
1(Quarter > 2005Q2)×1(Indian/Chinese)	0.044**	(0.018)	2.12	40,695
A2: Dependent Variable: M/B Ratio				
1(Quarter > 2005Q2)×Ratio (%)	0.085***	(0.022)	3.07	49,757
A3: Dependent Variable: Log (Tobin's Q)				
1(Quarter>2005Q2)×Ratio (%)	0.017***	(0.004)	3.07	49,757
A4: Indicator for Treated Firms				
1(Quarter>2005Q2)×1(Indian/Chinese)	0.058**	(0.028)	2.12	49,757
A5: Location-Time Controls				
1(Quarter>2005Q2)×Ratio (%)	0.035***	(0.013)	2.12	44,361
Panel B: Alternate Sample Cuts				
B1: Coarsened Exact Match Sample				
1(Quarter>2005Q2)×Ratio (%)	0.028**	(0.011)	2.02	16,184
B2: Extended Time Period (2001Q1 - 2010Q4)				
1(Quarter>2005Q2)×Ratio (%)	0.030**	(0.014)	1.94	124,726
B3: Including Finance Firms				
1(Quarter>2005Q2)×Ratio (%)	0.030***	(0.010)	1.93	67,032
B4: Excluding Small Firms (< 1 Mn)				
1(Quarter>2005Q2)×Ratio (%)	0.035***	(0.009)	2.12	49,757
B5: Remove Any M\&A Quarters				
1(Quarter>2005Q2)×Ratio (%)	0.034***	(0.009)	2.12	49,285
B6: Excluding Indian Outsourcing Firms				
1(Quarter>2005Q2)×Ratio (%)	0.033***	(0.010)	2.12	49,699
Panel C: Heterogeneity				
C1: Across Ratio of Indian & Chinese				
1(Quarter>2005Q2)×1(Ratio>1%)	0.107***	(0.034)	2.12	49,699
1(Quarter>2005Q2)×1(Ratio>2%)	0.110***	(0.035)	2.12	49,699
C2: Across Industries				
1(Quarter>2005Q2)×1(Indian/Chinese)	0.012	(0.031)	2.12	49,699
1(Quarter>2005Q2)×1(Indian/Chinese)×1(Knowledge Industry)	0.112***	(0.038)	2.12	49,699
Panel D: Falsification test				
D1: Impact across Indian/Chinese vs. Other Immigrants				
1(Quarter>2005Q2)×Ratio - Indian/Chinese (%)	0.029***	0.011	2.12	49,755
1(Quarter>2005Q2)×Ratio - Other Immigrants (%)	0.010	0.018	2.12	49,755

Note: This table presents robustness tests for the impact of labor mobility on firm value. The independent variable is the ratio of Indian, Chinese employees (in percentage) in the firm as of 2005 interacted with an indicator which switches on post the GC shock. All regressions are on the quarterly level and correspond to the baseline specification in col. 5 in table 9. The β coefficients are obtained from equation 6. Col. 1 presents the β coefficient value, col. 2 the standard error, col. 3 the value of the mean, and col. 4 the number of observations. Panel A shows results under alternate specifications. Panel A1 presents results with the doubly robust differences in differences estimator as per Sant'Anna and Zhao (2020). As per requirements of the doubly robust estimator, the outcome variable is a binary indicator for firms having Indian and Chinese employees, I keep only firms with all observations across all quarters (balanced sample), and I do not control for industry-year fixed effects due to high dimensionality. Panel A2 shows the outcomes for an alternate definition of firm value as the M/B ratio. Panel A3 presents results for the log of Tobin's Q. Panel A4 presents results with a binary indicator for firms having Indian or Chinese employees. Panel A5 presents results with location (commuting zone)-quarter controls. Panel B shows results with alternate sample cuts. Panel B1 includes matched sample based on firm controls. I subset to similar firms by matching firms with Indian and Chinese immigrants to others based on size, leverage, ROA, and cash ratio before GC shock using Coarsened exact match (CEM) as per Iacus et al. (2012). I match 1,214 out of 3,670 firms in my sample. Panel B2 shows the same result over an extended time period from 2001 to 2010. Panel B3 shows results including finance firms. Panel B4 shows results excluding small firms whose maximum size in sample is less than I million in assets, panel. Panel B5 removes any firm-quarters with M&A activity by dropping observations with more than 100% growth in total sales or assets. Panel B6 excludes any Indian outsourcing firms. All models include firm fixed effects, industry-quarter fixed effects, and firm-level controls interacted with quarter fixed effects. Panel C provides heterogeneity of results across sample cuts. Row C1 shows effect across ratio of Indian and Chinese employees by creating new indicators at cutoff's of 1% and 2%. Row C2 presents heterogeneity across a triple-diff specification by industry by interacting indicator for any Indian and Chinese employees with Indicator for Knowledge Industry: Information (NAICS Code: 51), Professional and Services (NAICS Code 54), and Computer Manufacturing (NAICS Code 334). Panel D presents falsification test with ratio of non-Indian and Chinese immigrants as the placebo sample. Firm-level controls include size, ROA, leverage, cash ratio, and sales growth. Definition for each variable construction is available in section B.1. Industry is defined at NAICS 4-digit level. I winsorize all firm characteristics at a 5% level and the ratio of Indian and Chinese employees in a firm at a 1% level. Standard errors are clustered by industry (NAICS 4-digit). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

Table C.5: **Robustness: Impact on firm hiring**

	Firm Hiring Ratios			
	Indian & Chinese / Total Emp (%)	Other Immigrants / Total Emp (%)	Log(Indian & Chinese)	Log(Other Immigrants)
	(1)	(2)	(3)	(4)
1(Year>2005)×Ratio (%)	0.051	0.051	0.000	-0.002
	(0.050)	(0.064)	(0.011)	(0.009)
Firm FEs	Y	Y	Y	Y
Industry×Time FEs	Y	Y	Y	Y
Y-Mean	1.88	3.16	0.66	1.04
Observations	12,714	12,714	12,714	12,714

Note: This table presents tests to check for changes in firm hiring of immigrants as a result of the GC Shock. The independent variable is the ratio of Indian and Chinese employees (in percentage) in the firm as of 2005 interacted with an indicator which switched on post GC shock. The β coefficients are obtained from equation 5. Col. 1 presents results for the ratio of Indian and Chinese employees hired in a year to total employees hired that year (in percentage), col. 2 for the ratio of other non Indian and Chinese immigrant employees hired in a year to total employees hired that year (in percentage), col. 3 for the logarithm of the number of Indian and Chinese immigrant employees hired in a year, and col. 4 for the logarithm of the number of non Indian and Chinese immigrant employees hired in a year. All models include firm fixed effects and industry-year fixed effects. Industry is defined at NAICS 4-digit level. I winsorize all variables at a 1% level. Standard errors are clustered by industry (NAICS 4-digit). Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

Table C.6: **Aggregate Estimates: Impact of GC Shock on Firm Returns**

Aggregate Estimates: Extra Firm Returns	
1: Coefficient for Firm Returns (%)	
1(Date>Sept/08/2005) $\times$ 1(Indian/Chinese)	0.343**
	(0.169)
2: Total Market Value For Firms with India/Chinese (in $ Billion)	8,373.6
3: Total Increase in Market Value (in $ Billion)	**28.7**
4: Total Number of Indian and Chinese Employees (000s)	**275.6**
5: Total Increase in Market Value Per Employee ($)	**104,231**

Note: This table presents calculations for aggregate impact of the GC mobility shock on abnormal firm returns. Row 1 presents the estimate for the increase in firm daily cumulative abnormal returns due to GC shock. The independent variable is an indicator which equals one if any Indian and Chinese immigrant employees were present in the firm as of 2005 interacted with an indicator which switched on post 8 September 2005 for variables. The outcome variable is Fama-French 5 factor adjusted returns. The β coefficients are obtained from equation 5. The specification is similar to table 8 col. 6 and includes firm fixed effects and industry-time fixed effects. Industry is specified at NAICS 4-digit level. I winsorize all variables at a 1% level. Row 2 presents the total market value of all firms which have any Indian and Chinese employees at the end of 2005Q2. Row 3 estimates the total increase in the market value of treated firms by multiplying the coefficient in the first row with the market value estimate in the second row. Row 4 presents the total number of Indian and Chinese employees in GC waitlines (Indian and Chinese immigrants in GC wait-lines identified as in Appendix B. I limit to immigrants who started US job in past decade to omit immigrants having already obtained GCs.) as of 2005 in US firms. I obtain this number by multiplying the per-firm ratio of Indian and Chinese employees (from LinkedIn) with total firm employees (from Compustat). Row 5 presents the $ value increase in firm value per employee by dividing Row 3 and Row 4.

Table C.7: **Robustness: New Firm Formation**

	Dependent Variable: New Firm Entry / 1000 Employees			
	β (1)	Std. Error (2)	Y-Mean (3)	Observations (4)
Panel A: Alternate Specifications				
A1: Doubly Robust Diff-in-Diff Estimator (Sant'Anna and Zhao)				
1(Year>2005) × 1(Indian/Chinese)	-0.174***	(0.052)	6.18	395,180
A2: Large Industry-Locations Only; Unweighted Regressions				
1(Year>2005) × Ratio (%)	-0.176**	(0.082)	6.18	98,330
Panel B: Alternate Controls				
B1: Time Varying Market Size				
1(Year>2005) × Ratio (%)	-0.178***	(0.057)	6.18	395,120
B2: State Controls				
1(Year>2005) × Ratio (%)	-0.165***	(0.047)	6.18	395,180
B3: NAICS-4 Industry Controls				
1(Year>2005) × Ratio (%)	-0.231***	(0.071)	6.18	395,120
B4: Total Immigrantion Controls				
1(Year>2005) × Ratio (%)	-0.220***	(0.066)	6.18	393,550
Panel C: Alternate Sample Cuts				
C1: Industries with VC Investments				
1(Year>2005) × Ratio (%)	-0.230**	(0.099)	6.83	72,050
C2: Without Finance Related Firms				
1(Year>2005) × Ratio (%)	-0.235***	(0.054)	6.23	370,400
C3: Without Outsourcing Related Firms				
1(Year>2005) × Ratio (%)	-0.234***	(0.053)	6.18	393,550
C4: Without 2008 Onwards Time Period				
1(Year>2005) × Ratio (%)	-0.097**	(0.044)	6.18	276,584

Note: This table presents robustness checks for the impact of GC wait-lines on new firm formation. The independent variable is the percentage of Indian, Chinese employees in the industry-commuting zone pair as of 2005 interacted with an indicator which switches on post GC shock in 2005. The dependent variable is the number of new firms founded per 1000 employees in the industry-commuting zone pair in that year. The specification is similar to the baseline (col. 2) in table 13. The β coefficients are obtained from equation 8. Col. 1 presents the β coefficient value, col. 2 the standard error, col. 3 the value of the mean, and col. 4 the number of observations. Panel A presents alternate specifications for the main regression. Row A1 presents results with a doubly robust differences-in-differences estimator similar to Sant'Anna and Zhao (2020). I use a binary indicator identifying markets with Indian and Chinese immigrants instead of the continuous measure as my dependent variable. I am unable to control for the high-density fixed effects (industry-time and location-time) for this specification. Row A2 presents results for equal-weighted regressions for all counties for a subset of industry-commuting zone pairs with more than 200 employees (top quintile). Panel B presents results with alternate controls. Row B1 presents results with additional market size interacted with year as control. Row B2 presents results with state instead of commuting zone controls. Row B3 presents results with NAICS 4 instead of LinkedIn industry controls. Row B4 presents results with controls for total number of immigrants in each market interacted with 2005 Shock. Panel C presents results on alternate sample cuts. Row C1 presents results for industries with VC investment (obtained from CrunchBase). VC industries include nanotechnology, semiconductors, biotechnology, medical devices, wireless, computer software, internet, computer games, pharmaceuticals, computer networking, renewable & environment, computer & network security, computer hardware, consumer electronics, e-learning, online media, telecommunications, market research, electrical/electronic manufacturing, information technology & services, information services, research, and outsourcing/offshoring. Row C2 presents results without any finance-related firms (NAICS code 52). Row C3 presents results without any outsourcing-related firms. Row C4 presents results excluding 2008 and all subsequent years. I control for industry-commuting zone, industry-year, and commuting zone-year fixed effects. I winsorize all variables at a 1% level. Standard errors are clustered by industry and commuting zone. Regressions are weighted by 2005 total employment for each industry-commuting zone pair. Observations that are fully explained by the fixed effects are dropped before the estimation. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01).

Table C.8: **Aggregate Estimates: Impact of GC Shock on New Firm Formation**

Aggregate Estimates: New Firm Formation	
1: Coefficient for New Firm Formation / 1000 Employees	
1(Year $>$ 2005) $\times$ 1(Indian/Chinese)	-0.184***
	(0.036)
2: Total Employees for Markets with India/Chinese (000s)	13,263
3: Total Firms Not Founded in 5 Years	**12,202**

Note: This table presents back-of-the-envelope calculations for total number of new firms not formed due to GC shock. Row 1 presents the estimate of the reduction in new firm formation per 1000 employees due to GC shock. The independent variable is an indicator which is one if industry-commuting zone pair has any Indian and Chinese immigrant employees as of 2005 interacted with an indicator which switches on post GC shock in 2005. The dependent variable is the number of new firms founded scaled by total employees (in 1000's) in an industry-commuting zone pair in that year. The β coefficients are obtained from equation 6. The specification is similar to the one presented in table 13 col. 2. I control for commuting zone-location, industry-year, and commuting zone-year fixed effects. Industry is specified as the 114 unique industries defined by LinkedIn. Regressions are weighted by 2005 total employment for each industry-commuting zone pair. Row 2 presents an estimate of total employment in industry-commuting zone pairs with any Indian and Chinese employees as of 2005. Row 3 presents an estimate of the total number of firms not founded from 2006-2010 by multiplying the coefficient in row 1, with an estimate for total employees in row 2, with 5 (for 5 years).