The Silicon Divide: High-Technology IPOs and the Widening Socioeconomic Gap*

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Abstract

High-technology IPOs lead to greater inequality among workers with different skills and gentrification in local neighborhoods. Using spatial causal inference and withdrawn IPOs as counterfactuals, I document positive wage effects for incumbent workers but displacement of low-skilled workers, mainly due to rising housing prices and rents. Both excess liquidity and agglomeration brought by high-technology IPOs contribute to the differentials. Further, I also develop a discrete-choice spatial equilibrium model that demonstrates that welfare changes diverge for high- and low-skilled workers, with an average IPO increasing the productivity of surrounding high-skilled workers by 20%.

JEL classification: R11, R13, G14

Keywords: IPO, Inequality, Spatial equilibrium, Spillover effects

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1 Introduction

Initial Public Offerings (IPOs) have been extensively studied for their impacts on corporate finance and investor returns, but their effects on surrounding communities remain poorly understood. Given that equity markets significantly influence the real economy (Morck et al., 1990), these IPO events can meaningfully transform local neighborhoods as well. Uber's 2019 IPO in San Francisco illustrates this dynamic: while the company celebrated its financial milestone and contributions to innovation, local residents protested rising inequality, prompting city leaders to consider special taxes on tech IPOs.

Within this broader context, high-technology firms offer a particularly compelling setting to examine these neighborhood effects. These companies receive substantial public support—between 2013 and 2018, U.S. local governments provided over $9.3 billion in subsidies to high-technology firms, anticipating economic growth and job creation benefits (Rushe, 2018). Unlike traditional companies, high-technology firms focus on knowledge-intensive innovation that primarily benefits high-skilled workers. When these firms go public, they not only inject substantial wealth into communities through their typically high valuations but also accelerate local innovation through increased R&D investment and knowledge spillovers. Understanding how these combined effects influence community welfare represents an important area for research.

This paper examines how high-technology IPOs influence local economic inequality by analyzing their differential impacts on high-skilled versus low-skilled workers in nearby neighborhoods. Adjusted for the living cost, the real wage of high-skilled workers increases while the real wage of low-skilled workers decreases, within neighborhoods close to the headquarters of the IPO firms.[1] I also document that low-skilled workers are displaced from their original neighborhoods, and relatedly, homelessness rates increase. In the second part of the paper, I incorporate welfare measures into a structural model and show that high-technology IPOs indeed result in a welfare increase for high-skill workers at the expense of low-skill workers. Moreover, high-technology IPOs lead to significant productivity gains for incumbent high-skilled workers.

To understand how high-technology IPOs affect local inequality, this paper tests three

[1] I segregate skill groups by referring to workers with at least a four-year college-level education as high-skilled workers and other workers as low-skilled workers. Admittedly, a worker's educational level is not equivalent to their skills, but education is still the single best proxy, especially when other measures like work experience or field of study are unavailable.

distinct mechanisms. First, IPOs may catalyze local entrepreneurial activity and attract new high-technology establishments (Babina et al. (2017), Babina and Howell (2019), Gagliardi and Sorenson (2023)). This entrepreneurial channel could amplify the demand for skilled labor and reshape neighborhood composition. Second, IPOs create a liquidity shock as founders and early investors cash out, generating substantial local wealth that flows into housing markets and local services (Hartman-Glaser et al., 2022). This liquidity channel can drive up living costs, potentially displacing lower-income residents. Third, and perhaps most significantly, high-technology IPOs may serve as skill-biased technological shocks through knowledge spillovers (Bernstein (2015), Matray (2021)). Public firms, compared to their private counterparts, typically excel at attracting talent, acquiring innovative outputs, and investing in growth (Brav (2009), Bernstein (2015), Maksimovic et al. (2022)). Given that over 70% of high-technology firm employees are college graduates—compared to 37% in the general population—these innovation spillovers disproportionately enhance high-skilled worker productivity.

My empirical analysis combines reduced-form evidence and structural estimation to evaluate these mechanisms. Using detailed neighborhood-level data, I find limited support for the entrepreneurial channel, as treatment areas show no significant increase in high-technology establishments. However, the evidence strongly supports both the liquidity and skill-biased technological shock channels. VC-backed IPOs, which typically generate larger wealth effects, produce stronger welfare differentials. Moreover, incumbent firms near IPO headquarters demonstrate increased patent output and value, suggesting substantial knowledge spillovers. These findings help explain why high-technology IPOs might simultaneously boost local economic growth while exacerbating inequality.

Establishing causality in this analysis requires addressing several key identification challenges. First, IPO decisions and timing may be anticipated by local residents and firms. Since the IPO process often extends over months and generates public speculation, local actors might adjust their behavior preemptively. For instance, potential homebuyers might accelerate purchases to exploit price advantages before an expected IPO-driven appreciation. To address this anticipation effect, I employ dynamic difference-in-differences tests that examine pre-trends in all outcome variables. The absence of pre-trends across specifications helps alleviate this concern. Second, macroeconomic shocks concurrent with IPOs could confound the treatment effect estimates. This is particularly concerning given that IPOs tend to cluster during favorable market conditions. I address this challenge through two approaches: leveraging the staggered nature of IPO events across time and location, and implementing a comprehensive set of fixed effects to absorb unobserved temporal and spatial dynamics.

Finally, the most critical identification challenge stems from the non-random location choices of high-technology firms. These firms systematically select areas with high concentrations of skilled workers and amenities that appeal to them, creating fundamental differences between neighborhoods near and far from IPO headquarters. To address this selection bias, I develop an identification strategy following Bernstein (2015) that uses withdrawn IPO issuers as counterfactuals. By comparing neighborhoods at similar distances to withdrawn issuers but different distances to successful IPO headquarters, I can better isolate the IPO effect from underlying location preferences. I further validate this approach through two additional tests: a triple difference-in-differences specification comparing neighborhoods around actual versus withdrawn IPO issuers, and a propensity score matching strategy that directly matches treated and control units based on observable characteristics. This comprehensive identification approach allows me to measure how high-technology IPOs causally affect local inequality through their differential impacts on high-skilled versus low-skilled workers.

The reduced form evidence provides an answer to the question of how much each welfare measure changes because of the IPO shock, but it does not fully explain how workers with different skills benefit from IPOs in nearby areas. Examining welfare measures in isolation would distort the conclusion because higher living costs can offset the benefit of wage increases. I propose a discrete choice spatial equilibrium model to identify workers' utility changes and the strength of the spillover effects as a complement to the reduced-form estimation. I use the random utility model proposed by Berry (1994) and Berry et al. (1995) with data on workers' origin–destination commuting flow to directly estimate the utility changes without knowing specific individual choices. Consistent with the reduced-form results, the utility changes are positive for high-skilled workers and negative for low-skilled workers. This implies that the burden on low-skilled workers of higher living costs overwhelms their wage increase and forces marginal workers to move to more affordable neighborhoods. Going one step further, I design the IPO shock as a multiplier for high-skilled-worker productivity, modeled as a function of distance from IPO firms and their local neighborhood. Finally, I apply shift-share instrumental variables on skill wages to account for endogeneity stemming from unobserved amenity changes. The estimated structural parameters in the model show that the spillover effect on productivity is economically and geographically extensive: A representative IPO would typically raise the productivity of high-skilled workers by 21.7% after five years.

Several limitations should be highlighted as well. First, the comparison of neighborhoods at different distances to firm headquarters only captures the relative effects of high-technology IPOs. Like other studies using the spatial approach, this study cannot identify the absolute treatment effect relative to the case of no shock but uses the results for less-affected neighborhoods as the benchmark. In the presence of general equilibrium adjustments, all participants in all labor markets will adjust and arrive at a new equilibrium, and it is impossible to identify a perfect control group. Moreover, even though workers are very immobile, policies and shocks can have ripple effects as local labor markets overlap (Manning and Petrongolo, 2017). The inability to separate these markets makes it impossible to measure the total effect.

Another limitation related to the estimated treatment effect is that it is not possible to incorporate all aspects of an IPO's impact on resident welfare, such as convenience and new products. The structural model alleviates but does not resolve the concern, by defining utility of workers narrowly as a function of real wages and (unobserved) amenity changes. Although being useful in highlighting the local welfare changes, the definition fails to capture many other potential impacts of IPOs beyond the local markets. For instance, Google Cloud, which was introduced shortly after the Google IPO, streamlined collaboration and reduced communication costs in business, bolstering the productivity of workers. Obviously, this positive effect is left out of the analysis. Hence, focusing on real wages may lead to an underestimation of the contribution made by high-technology IPOs to economic growth and social welfare. Nevertheless, the analysis of local welfare and productivity remains valid as long as workers have equal access to the service.

This paper bridges two distinct strands of literature while offering novel insights into the distributional effects of high-technology IPOs. The first strand examines IPOs from a corporate finance perspective, focusing primarily on their financial impacts on firms and investors (Ritter and Welch (2002), Ljungqvist (2008), and Jenkinson and Jones (2009)). These studies typically analyze how going public affects firm performance, innovation capacity, and shareholder wealth. A growing subset of this literature examines spillover effects on local economies (Hartman-Glaser et al. (2022), Bulut (2023)), though largely without considering distributional consequences. The second strand emerges from labor and urban economics, where scholars investigate local inequality and productivity through various lenses: endogenous neighborhood changes (Guerrieri et al., 2013), direct corporate investment (Greenstone et al. (2010), Qian and Tan (2021), Matray (2021)), and place-based government policies (Kline and Moretti (2014), Tian and Xu (2022)). However, both streams of literature have largely overlooked how equity market events like IPOs might systematically affect local economic inequality.[2]

Recent attempts to connect IPOs with local economic outcomes (Butler et al. (2019),

[2]Corporate finance research also examines welfare gaps, but primarily through the lens of individuals as either equity market investors or company employees (Li et al. (2021), Pan et al. (2022)). While equity market participation could indirectly affect local inequality through income effects, such transmission channels are less direct and harder to identify.

Cornaggia et al. (2019)) have yielded conflicting results, in part because they treat workers as a homogeneous group. This paper makes three distinct contributions to this emerging literature. First, it explicitly examines heterogeneous effects across skill groups, revealing how high-technology IPOs may exacerbate local inequality even while promoting overall economic growth. Second, it identifies and empirically tests specific mechanisms through which IPOs affect local labor markets, including liquidity effects and knowledge spillovers. Third, it develops a structural approach to measuring welfare effects that overcomes a key limitation in existing research: the challenge of determining net welfare changes when studying wages and living costs separately with unobserved amenities.

The remainder of the paper proceeds as follows. In Section 2, I describe the data construction and present summary statistics, and in Section 3, I discuss the empirical strategy for identifying the spillover effect of high-technology IPOs on local neighborhoods and present the estimation results. In Section 4, I introduce welfare outcomes into a structural model and estimate the differential worker-utility effects. Section 5 presents the robustness check on reduced-form results. Finally, Section 6 concludes.

2 Data

In this section, I describe the data sources and construction of key variables used in the analysis. The study combines granular demographic and business variables at the census tract and ZIP code levels with comprehensive IPO data from financial databases. The empirical analysis employs both reduced-form estimation using tract-level observations and structural estimation incorporating worker commuting patterns between ZIP codes to measure changes in utility.

2.1 High-Technology IPOs and Establishments

The primary sample consists of IPO events from 2003 to 2017, collected from Audit Analytics and Thomson databases. These databases provide detailed information about IPO proceedings and firm headquarters locations. Given that firms may relocate their headquarters over time, I manually verify business addresses at the time of IPO using 10-K filings, as the databases typically backfill current locations to historical records.[3] Using the Google Geocoding service, I convert these verified addresses to geographical coordinates and map them to corresponding 2010 Census ZIP codes and tracts. To identify high-technology firms,

[3] I do not use headquarters location information from the databases because they record the current location and backfill this to all previous years.

I use the U.S. National Science Foundation's (NSF) classification of high-technology industries, converting their NAICS codes to 1987 SIC codes.[4] I manually verify that the business activities of these firms primarily involve high-skilled workers. Financial data, including assets and revenue, are obtained from Compustat. To ensure sample quality and focus on economically significant IPOs, I apply several filters: (1) excluding Asset-Backed Securities (ABS) and Real Estate Investment Trusts (REITs), (2) requiring minimum IPO share prices of $5, and (3) restricting to firms with assets exceeding $100 million at the IPO year-end.

After conducting the necessary filtering, I identify a total of 396 high-technology IPOs that occurred during the sample period. This number is consistent with the figures reported by Bernstein (2015) and on Jay Ritter's website[5]. Of these, 31 firms ever relocated their headquarters in the sample period. However, with the exception of five firms, the distance between the new and old headquarters of the remaining 26 firms is less than 20 miles, and the headquarters relocation is not a serious concern. Additionally, I manually verify the address and use the location of the firm's headquarters in the year of the IPO as the center of the treatment area.[6]

The geographic distribution of high-technology firm headquarters, shown in Figure (2), reveals significant spatial clustering. Tables (1) and (2) provide detailed breakdowns by geography and industry. These firms predominantly cluster in affluent, densely-populated areas along the U.S. coasts and major inland cities. The leading metropolitan areas for completed IPOs are New York (17 firms), Houston (13), and Austin (11). At the county level, Middlesex, Massachusetts leads with 24 firms, followed by Santa Clara (20) and San Mateo (18), both in California. State-level concentration shows California dominating with 93 firms, followed by Texas (46) and Massachusetts (29). The industry distribution reveals that services-computer programming, data processing, and other computer services (SIC 7370) represents the largest segment with 68 firms, followed by biological products (SIC 2836) with 37 firms and services-prepackaged software (SIC 7372) with 29 firms. As shown in Figure (3), the temporal distribution of high-technology IPOs follows a cyclical pattern similar to overall IPO activity, with peak activity in 2014 (54 IPOs) and a trough in 2008 (7 IPOs).

Table (4) Panel A presents summary statistics of IPO characteristics and firm financial

[4]The list is available at https://www.nsf.gov/statistics/seind14/index.cfm/chapter-8/tt08-a.htm

[5]The data are available at https://site.warrington.ufl.edu/ritter/ipo-data/

[6]Some technology giants, such as Amazon and Facebook, have established locations besides their headquarters, but in most cases, these were only constructed several years after the company had already been listed. For most firms in the sample, only their headquarters are recorded. Unlike manufacturing firms, the headquarters of high-technology firms host a significant portion of their business activities, particularly innovation and R&D. For these reasons, the analysis focuses on firm headquarters and excludes other establishments.

positions. While the high-technology sample firms broadly align with typical public firms, there is substantial heterogeneity in both IPO proceeds and total assets, driven by outsized offerings from major technology companies such as Facebook and Google. To account for this heterogeneity in firm size and offering characteristics, I include case-year fixed effects in subsequent analyses.

To establish a credible control group, I construct a sample of withdrawn high-technology IPOs from Thomson/Refinitiv using the same filtering procedures described above. This process identifies 118 withdrawn IPOs during the sample period. To operationalize the analysis, I define an "IPO Zone" as the collection of neighborhoods within 30 miles of a firm's headquarters. These zones are time-invariant and may geographically overlap, with each IPO firm mapping uniquely to its corresponding zone.[7] For each census tract in the sample, I calculate its distance to the nearest withdrawn IPO firm headquarters and categorize these distances into five bins relative to each actual IPO.

2.2 Local Neighborhood Characteristics

The analysis employs two distinct geographical units: census tracts for difference-in-differences (DiD) estimation and ZIP codes for structural estimation. Census tracts, which are stable subdivisions of census blocks, offer advantages over ZIP codes for DiD analysis due to their consistent boundaries and rich demographic data. The sample encompasses 70,004 census tracts nationwide, harmonized to 2010 Census boundaries to ensure geographical consistency across census waves. For structural estimation, I aggregate data to the ZIP code level to enhance computational efficiency.

Demographic and economic variables come from the American Community Survey (ACS) 5-year estimates, accessed through the Integrated Public Use Microdata Series (IPUMS) (Manson et al. (2021)).[8] I map the 2006–2010 ACS data to 2008 and follow this pattern for subsequent years. To address missing values, I implement a two-step procedure: first interpolating using population-weighted averages from tracts within a 5-mile radius, then applying average growth rates to remaining missing observations. All wage data are converted to 2010 dollars using the GDP deflator.[9] To mitigate the influence of outliers, all numerical variables are winsorized at the 1st and 99th percentiles. Variable definitions and census-tract level descriptive statistics are presented in Tables (3) and (4), respectively. The evolution of skill-

[7]Given this one-to-one correspondence, I use the terms "IPO firm" and "IPO Zone" interchangeably throughout the analysis.

[8]The structural estimation in Section 4 also employs a 5% population sample from the 1990 Census to construct shift-share instrumental variables. The data structure closely parallels the ACS data and is detailed in that section.

[9]Results remain robust when using the Consumer Price Index as an alternative deflator.

based wage differentials, illustrated in Figure (4), reveals a persistent pattern. While nominal wages show steady growth, real wages remain relatively stable over time. Most notably, there exists a substantial and persistent wage gap of approximately $20,000 between high-skilled and low-skilled workers, underscoring the importance of understanding factors that drive this skill premium for policy considerations. An essential dimension of neighborhood welfare is homelessness, which I measure using data from the U.S. Department of Housing and Urban Development's Annual Homeless Assessment Report (AHAR) for 2007–2017. These data are mapped to census tracts using the crosswalk developed by Glynn et al. (2021). While AHAR data do not directly report educational attainment, research indicates that only 7% of homeless individuals hold bachelor's degrees or higher (Glassman (2024)), making this measure particularly relevant for assessing low-skilled worker welfare.

To analyze housing market dynamics, I combine median rent data from IPUMS with the Federal Housing Finance Agency's (FHFA) house price index (HPI). The HPI, a weighted repeat-sales index tracking single-family home prices, enables isolation of household consumption effects from broader housing market trends. The correlation structure of neighborhood characteristics, depicted in Figure (A1), reveals strong associations between worker compensation, educational attainment, living costs, and poverty rates. Higher-wage neighborhoods typically exhibit higher educational attainment, elevated living costs, and lower poverty rates. In contrast, demographic factors such as age and race show weaker correlations with these economic measures.

Panel B of Table (4) documents substantial cross-neighborhood inequality. High-skilled worker wages in the most affluent neighborhoods exceed those in the poorest areas by a factor of ten. Similarly, poverty rates range from less than 1For propensity score estimation, I construct predictors using the 2000 ACS Census data, which predates all sample IPOs. Given the granular nature of census tracts, IPO effects likely extend beyond the immediate tract containing the firm headquarters. Therefore, I calculate variables within three concentric rings (5, 10, and 15-mile radii) around each tract to capture these spatial relationships.

2.3 Commuting Patterns of Workers

The structural model assumes workers can freely choose their residential and workplace locations to maximize utility, abstracting from migration costs. To empirically capture these spatial labor market interactions, I employ the Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES), which provides comprehensive data on worker commuting flows between census blocks. The LODES dataset, collected from 2002 to 2015 across most U.S. states (U.S. Census Bureau, 2022), includes detailed information on both residential and workplace characteristics, with workers categorized by income level and industry. Consistent with the structural estimation approach, I aggregate these block-level flows to ZIP codes to maintain computational tractability.

To distinguish between high-skilled and low-skilled worker commuting patterns, I augment the LODES data with the 2009 National Household Travel Survey (U.S. Department of Transportation, 2022). This survey provides rich individual-level data, including trip purposes and educational attainment for a representative sample of travelers. Following Qian and Tan (2021), I implement a least absolute shrinkage and selection operator (LASSO) model to estimate the proportion of high-skilled workers in commuter flows between ZIP code pairs, with the methodology detailed in the subsequent section.

3 Identification Strategy

3.1 Construct counterfactuals by Withdrawn Issuers

In spatial economics, researchers commonly employ the "ring method" to evaluate place-based policies by comparing outcomes across neighborhoods at varying distances from a policy's focal point. This approach provides an intuitive framework for estimating the effects of high-technology IPOs by examining differences between neighborhoods near and far from firm headquarters. However, implementing this method requires addressing several identification challenges beyond those outlined in the introduction.

The primary identification challenge arises from high-technology firms' non-random location choices. Although these firms rely less on local economic conditions than manufacturing firms due to their globally tradable inputs and outputs, they systematically select locations—such as city centers or areas near transportation hubs—that correlate with unobserved determinants of worker welfare. To address this selection bias, I follow Bernstein (2015) by controlling for each neighborhood's distance to the nearest withdrawn-IPO high-technology firm. This approach enables comparison of census tracts that share similar location characteristics (proxied by their distance to withdrawn issuers) but differ in their exposure to successful IPOs, thereby isolating the IPO effect from underlying location preferences.

The credibility of this identification strategy depends on two key assumptions. First, withdrawn IPOs must represent valid counterfactuals for successful IPOs. Busaba et al. (2001) documents that withdrawing issuers closely match successful IPOs in both size and profitability across U.S. markets. I verify that this similarity extends to industry distribution within my sample. Second, while firms' decisions to file for IPOs may correlate with local characteristics, the ultimate IPO outcome must be orthogonal to these characteristics.

This assumption is particularly plausible for high-technology firms, whose success primarily depends on human capital rather than local economic conditions. Table (5) supports this assumption by demonstrating comparable local economic characteristics between successful and withdrawn issuers.

To illustrate this identification strategy, Figure (5) presents a case study of Open Solutions Inc's 2003 IPO. Panel (a) demonstrates the basic ring design, where the red point indicates the firm's headquarters. Shaded areas represent the treatment group (neighborhoods within 15 miles), while blank areas denote the control group (neighborhoods 15-30 miles from headquarters). Panel (b) introduces the withdrawn issuer dimension, with the dark yellow point marking China Bull Management Inc.'s headquarters (withdrawn November 17, 2011). Census tracts shown in dark and light blue fall into identical distance bins relative to the withdrawn issuer, enabling more precise treatment-control comparisons by controlling for location-specific characteristics.

To further strengthen identification, Section 5 presents two complementary tests. First, I implement a triple difference-in-differences specification comparing neighborhoods near IPO headquarters with those near withdrawn issuers. This approach addresses potential concerns about differential distances between paired treatment-control units by ensuring that estimated effects stem from listing status rather than proximity differences. Second, I develop a propensity score model to match census tracts based on their likelihood of hosting high-technology firms, providing an alternative validation of the withdrawn-IPO counterfactual approach.[10]

3.2 Treatment Effect of High-Technology IPOs

To estimate how high-technology IPOs affect neighborhood outcomes, I implement a difference-in-differences (DiD) approach. For each IPO, I define the treatment group as census tracts within 15 miles of firm headquarters[11] and the control group as tracts between 15 and 30 miles away.[12] Following Greenstone et al. (2010), I collapse observations into a single panel where census tracts can belong to multiple IPO zones and serve as treatment or control units

[10]While previous studies have employed NASDAQ market returns as an instrumental variable (Bernstein (2015), Cornaggia et al. (2019)), this approach presents several limitations for our setting. These include weak first-stage predictions and potential violation of the exclusion restriction through correlation with broader equity market events. Moreover, this approach would necessitate outcome aggregation to the IPO Zone level, substantially reducing statistical power and altering the interpretation of results.

[11]I exclude tracts containing IPO headquarters to focus on spillover effects.

[12]While the average U.S. worker commutes 41.6 miles according to ACS estimates, technology workers likely have shorter commutes given their firms' typical urban locations. The 15-mile treatment radius balances capturing spillover effects on local firms with maintaining treatment-control separation. Results remain robust to alternative radii such as 10 versus 20 miles.

across different cases. Section 5 confirms that case-by-case estimation yields fundamentally similar results.

The baseline specification is as follows:

$$Y_{it} = \alpha + \beta Treat_{ik} \times \mathbf{1}\{t > t_0^k\} + \theta Treat_{ik} + \mathbf{X}_{\mathbf{it}}^{\intercal}\mathbf{\Pi} + \delta_i + \gamma_{ct} + \eta_{kht} + \epsilon_{ikt}, \tag{1}$$

where Y_{it} are welfare outcomes related to census tract i in year t, including wage and employment by skill groups and housing market outcomes; $Treat_{ik}$ is an indicator that tract i belongs to treatment group of IPO case k, and $\mathbf{1}\{t > t_0^k\}$ denotes the dummy for year t is after the year of the IPO by firm k. The term $Treat_{ik}$ accounts for the level difference between the treatment and control groups, thus allowing us to consistently estimate β, which is the DiD estimator, and identifies the average effect of high-technology IPOs. The optional time-variant covariates for census tracts are denoted by $\mathbf{X}_{it}^{\intercal}$. I control for various fixed effects. First, the tract-fixed effect, δ_i, absorbs time-invariant characteristics of each census tract, such as geography and climate. Alternatively, it can be modified as interacted with the IPO case so that the interacted fixed effect further absorbs the dummy $Treat_{ik}$. Second, γ_{ct} is the county-year-fixed effect, controlling for unobserved dynamic confounders on the county level, such as amenity measures and regulations. Finally, by treatment status and IPO cases, I divide census tracts into five distance bins as illustrated before, denoted by $h = 1, .., 5$. Therefore, the final ingredient is the firm-distance-year fixed effect, η_{kht}, which absorbs time-variant firm-level characteristics, such as financial positions and investments, and also controls for heterogeneity related to IPOs across neighborhoods. After controlling for all fixed effects, the comparison is between tracts in the treatment group and their counterparts with similar propensity scores, given the IPO case, k. Standard errors are clustered at the firm level to adjust for potential correlation between observations treated by the same IPO event.

Table (12) shows the result. For Columns (3), (4), (7), and (8), the case-tract-fixed effects replace the tract-fixed effects, and Columns (2), (4), (6), and (8) add covariates in addition to fixed effects. Note that the panel is collapsed from repeated-observation level, so each of the census tracts can relate to more than one high-technology IPO because of overlapped IPO zones. The interacted fixed effect absorbs the dummy for treatment and accounts for possible heterogeneous treatment effects by different IPO cases in the same neighborhood. Neighborhood characteristics are only measured by tracts and years.[13] Nevertheless, the two specifications would yield the same results if the treatment effect were homogeneous

[13]However, for the same reason, assuming different outcome levels for the same census tract when it belongs to another IPO Zone does not reflect the reality. The side effect of the interacted fixed effect is that excessive variation is removed.

and stable over the years. Any disparity in coefficient estimates would mirror potentially heterogeneous and dynamic effects, and I explore the features below in this section and in Section (5).

For most outcomes, the point estimate of the effect remains stable after including further controls. In determining robustness, Oster (2019) suggests that the importance of unobservables is jointly determined by point estimates and R^2. I compute the bounds, and they are well above 1 in all specifications.[14] Moreover, R^2 is sufficiently large that it is unlikely that further unobserved characteristics could be driving the results.

Overall, high-technology IPOs generate substantial but heterogeneous effects on local labor markets. In wage outcomes, IPOs increase wages for both skill groups but widen the skill wage premium. Using log-transformed wages to enable percentage interpretation, results show that IPOs increase high-skilled worker wages by 1.16% and low-skilled worker wages by 0.52%, leading to a 0.73% expansion in the skill wage premium. Given the large population in both treatment and control areas, these percentage changes translate to economically significant aggregate impacts.

The employment effects reveal a striking pattern of worker displacement, particularly pronounced for low-skilled workers who experience a 1.36% decline in employment near IPO headquarters. High-skilled workers also face some displacement, though the effect is smaller in magnitude and becomes statistically insignificant without covariates.[15] The simultaneous increase in wage premium and changes in relative labor supply align with theoretical predictions from Welch (1973) and Katz and Murphy (1992), as well as empirical evidence on increasing skill returns from Acemoglu and Autor (2011), suggesting that high-technology IPOs exacerbate local inequality.

To disentangle whether worker displacement occurs through migration or unemployment, I examine two potential mechanisms. The migration channel operates through increased living costs driven by high-skilled worker consumption, pushing marginal low-skilled workers to relocate. Alternatively, the unemployment channel works through job destruction in sectors like manufacturing due to rising local input prices, disproportionately affecting low-skilled

[14] The bound δ is approximated by $\beta^* \approx \tilde{\beta} - \delta \times (\mathrm{o}\,\beta - \tilde{\beta}) \times (R^{max} - \tilde{R})/(\tilde{R} - \mathrm{o}\,R)$ where o denotes the specification without covariates, and ˜ denotes the specification with covariates; R is the shorthand of R^2. I set R^{max} to 1, which is the most conservative. δ is calculated by assuming $\beta^* = 0$, which implies no treatment effect. The criterion $\delta = 1$ means that unobservables are as important as observables in driving estimation results.

[15] This finding appears to contradict Butler et al. (2019), who document increased local employment following IPOs. However, this discrepancy likely stems from differences in scope - while they examine overall employment effects, I focus specifically on differential impacts across skill groups. Additionally, since not all high-skilled workers are employed in technology-related industries, some may face higher living costs without corresponding wage increases, potentially driving out marginal high-skilled workers. Data limitations on occupational composition prevent more granular analysis of within-skill-group changes.

workers. A supplementary analysis regressing unemployment rates on treatment indicators yields insignificant coefficients for both skill groups[16], supporting the migration mechanism documented in Cornaggia et al. (2019), though their analysis does not differentiate by worker skill levels. The displacement effects extend to more severe outcomes, as demonstrated by increased homelessness rates following high-technology IPOs. Using longitudinal HUD data from the Continuum of Care (CoC) surveys mapped to census tracts via Glynn et al. (2021)'s crosswalk[17], I analyze tracts reporting at least one homeless person during 2007-2017 (reflecting HUD data availability). Results reported in Table (B3) reveal significant increases in homelessness rates post-IPO.

Consistent with the displacement effect on low-skilled workers, it also becomes evident that high-technology IPOs increase the number of homeless people significantly. Compared with controlled neighborhoods, the treatment group has fewer homeless people, as shown by the coefficient of $Treat$. However, following a high-technology IPO, the number of homeless people (in logarithmic form) increases by around 0.006. Although the demographic characteristics or homeownership status of the homeless people are unavailable, the majority are likely to have previously been renters living in the same area. They may become unemployed following the IPO, or they were already unemployed before the shock. Therefore, the continuously rising rent is likely an important driving force for the higher rate of homeless people. Being homeless harms a person's physical and mental health. Numerous studies report the prevalence of health issues among the homeless population, and such an effect usually persists over the entire life cycle. Thus, based on the evidence alone, it is far from obvious that high-technology IPOs are positive or at least neutral to everyone in their locality.

Next, high-technology IPOs have a prominent positive effect on house net worth and rents. HPI excludes commercial land use, so I can separate household consumption from firm investment. In line with Mian et al. (2013) and Butler et al. (2019), IPOs stimulate local housing markets through firm and household consumption channels. On average, high-technology IPOs result in 1.9% higher housing prices and a 0.8% increase in rental prices; these magnitudes are comparable to the results in Butler et al. (2019) and Hartman-Glaser et al. (2022). As an increase in housing value is strongly correlated to higher living costs, the change in the real wage is not apparent. In parallel, one cannot infer the change in welfare from estimating outcomes separately, which is the limitation of reduced-form results.

Besides the baseline DiD, in order to incorporate the dynamic effect of high-technology IPOs and test for confounders by people's expectations, I adapt Equation (1) to the dynamic

[16] Results are available upon request.

[17] Since CoCs typically span multiple census tracts, I allocate homeless counts proportionally to tract population, with an alternative weighting based on poverty population given the strong correlation between poverty and homelessness.

DiD by estimating the exact specification.

$$Y_{it} = \alpha + \sum_{\substack{s \neq -1 \\ s=-3}}^{6} \beta_s Treat_{ik} \times \mathbf{1}\{t - t_0^k = s\} + \mathbf{X}_{it}^{\intercal}\mathbf{\Pi} + \delta_i + \gamma_{ct} + \eta_{kht} + \epsilon_{iks} \tag{2}$$

All notations follow the baseline regression (1), except for s, which denotes the relative period from the year of the IPO. I normalize the effect in the year before the IPO to zero. Here, β_s measures the effect in period s relative to period -1, and periods $s < 0$ are falsification tests for a pre-trend. In the regression tables, I denote s as $Period$.

Figures (6)–(7) plot the estimate over time. It includes the simultaneous 95% confidence intervals (Montiel Olea and Plagborg-Møller, 2019). Advocated by Freyaldenhoven et al. (2019), the simultaneous confidence intervals are designed to contain the true path over time and are thus more useful for detecting a pre-trend and identifying post-treatment effects.

The signs of the post-IPO coefficients align with the static DiD estimates. There is a notable positive treatment effect on high-skilled wages and a negative effect on low-skilled employment. One can see that the treatment effects are long-lasting, with no reversal of effects, as evidenced by the fact that the local economy continues to absorb the influence of IPOs even after ten years. In contrast, the reduction in high-skilled employment following the IPO is small and reverses after five years. In the long term, the benefits resulting from higher wages and local amenities for high-skilled workers outweigh the burden of increased living costs. Finally, the pre-IPO estimates support the common trend assumption of DiD, as the confidence intervals contain zero. The common trend also rules out potential anticipation effects of IPOs, reinforcing the finding that the changes in outcomes are solely due to the actual IPO events.

Table (7) provides coefficient estimation results. The coefficient measures the average effect of high-technology IPOs in the given period relative to period -1. As before, critical values and confidence intervals are simultaneous instead of pointwise. Similarly, coefficients for years prior to the year of the IPO are not significantly different from zero. However, we see strong effects after IPOs on all outcome variables except for the wages of low-skilled workers. Strikingly, the effect accumulates over time as the magnitude of coefficients is monotonically increasing. Taking high-skilled wages as an example, one can see the effect rise to 0.72% log points in six years from 0.5% log points over the first three years. Likewise, the decrease in low-skilled employment is about -0.68% log points initially but dampens to -1.02% log points afterward.

The dynamic treatment effect has policy implications that are as important as the average treatment effect. While the average treatment effect on labor and housing outcomes reveals that exacerbating inequality may be a concern for policymakers, the deepening magnitude of effects indicates that such policies should target long-term rather than short-term outcomes. Typically, related policies involve improving the quality or quantity of labor or housing supply, for example, through job training programs or the construction of affordable housing. Other policies, like rent control, may be effective for short-term outcomes but can be unsustainable or have side effects over a longer period, thus affecting the prosperity of the local economy.

3.3 Possible Mechanisms

The impact of high-technology IPOs on local economies potentially operates through three distinct channels identified in the literature: (1) local expansion through direct hiring and entrepreneurial spawning (*expansion channel*), (2) wealth effects from founders and investors monetizing their stakes (*liquidity channel*), and (3) knowledge spillovers enhancing worker productivity (*productivity channel*). I empirically evaluate each channel's contribution to the observed inequality effects. The expansion channel appears limited in importance. Despite theoretical predictions from Babina et al. (2017) about entrepreneurial spawning, baseline results show no significant increase in high-skilled employment. To investigate more precisely, I examine both intensive and extensive margins. For the intensive margin, I use three-year post-IPO employment growth rates as a proxy for firm expansion.[18] As shown in Figure 8 (a), even rapidly expanding firms generate only marginally stronger effects on local wages and housing markets compared to slower-growing firms. On the extensive margin, regressions of high-technology establishment counts on the DiD indicator reveal no significant changes.[19]

The liquidity channel, in contrast, shows stronger evidence of local economic impacts, particularly in housing markets. I exploit variation between VC-backed and non-VC-backed IPOs to identify this channel, as VC-backed IPOs typically generate larger immediate wealth effects through substantial share sales at IPO. Using classifications from Jay Ritter's database, Figure 8 (b) demonstrates that VC-backed IPOs generate significantly larger housing market effects, consistent with Hartman-Glaser et al. (2022)'s findings on IPO-driven housing price appreciation. While this channel increases both high- and low-skilled wages, likely through increased local consumption, employment effects remain limited.

The productivity channel emerges as the most significant mechanism, particularly through industry-specific knowledge spillovers. Using firm-level patent data geocoded to headquar-

[18]Due to data limitations, I use firm-level rather than establishment-level employment as a proxy for headquarters growth.

[19]Results remain insignificant even when restricted to small establishments more likely to represent startups. Regression tables available upon request.

ters locations, I analyze innovation outcomes in census tracts with at least one registered patent during the sample period. To capture industry-specific effects, I separately examine patents within and across SIC industries relative to the IPO firm.

Implementing specification (1) with an additional indicator for zero-patent observations[20], Table (8) shows significant increases in both quantity and quality of industry-specific patents near IPO firms.[21] Figure (9) confirms this pattern in dynamic specifications. Importantly, these spillover effects appear confined within industries, with no significant cross-industry impacts, helping explain the observed pattern of increasing inequality. A particularly intriguing finding emerges from heterogeneity analysis based on pre-IPO (year 2000) patent productivity. Figure (A4) reveals that productivity gains concentrate in initially less-productive areas, suggesting that while IPOs may exacerbate skill-based inequality, they might simultaneously promote convergence in high-skilled worker productivity across locations, which leaves space for further research.

4 Estimating Changes of Welfare

While the reduced-form analysis demonstrates that high-technology IPOs widen skill-based disparities in wages and employment, quantifying overall welfare effects requires addressing several empirical challenges. First, observed real wages may not fully capture changes in worker welfare due to unobserved amenity improvements—for instance, increased local tax revenues from newly public and peer firms might fund enhanced infrastructure benefiting all residents. Second, the spatial equilibrium effects of IPOs likely operate through complex interactions between productivity spillovers, housing markets, and worker location choices. Third, the magnitude of these effects may vary systematically with distance from IPO headquarters.

To address these challenges, I develop a structural model that quantifies changes in worker utility following high-technology IPOs. The framework builds on the spatial equilibrium models of Rosen (1979) and Roback (1982), incorporating heterogeneous workers while adopting estimation procedures from Berry et al. (1995), Diamond (2016), and Qian and Tan (2021). In this model, workers differentiated by skill level make optimal residence and work-

[20]Recent literature highlights econometric challenges with log-transformed discrete variables containing zeros, suggesting zero-inflated Poisson models as alternatives (Cohn et al. (2022); Chen and Roth (2023)). Given computational constraints with our complex fixed effects structure, I address this concern by running level specifications, which yield stronger statistical significance.

[21]To address the many-zeros issue, I complement the DiD analysis with a Tobit model (clustered at IPO zones). While the Tobit model suggests larger treatment effects, it cannot include fixed effects due to the incidental parameter problem (Neyman and Scott (1948)). Results available upon request.

place choices while sharing local housing markets, with each worker supplying one unit of labor inelastically. This approach enables estimation of mean utility changes using aggregate worker data while accounting for endogenous sorting and general equilibrium effects.

4.1 Model Setup

4.1.1 Firm Production and Local Labor Demand

Consider an economy with a finite number of independent neighborhoods indexed by j. Each neighborhood has a homogeneous representative firm producing a single output with a mixture of high-skilled and low-skilled labor. Assume firms have a production function with constant elasticity of substitution:

$$Y_{jt} = [(A_{jt}^H H_{jt})^\rho + (A_{jt}^L L_{jt})^\rho]^{\frac{1}{\rho}}, \tag{3}$$

where H_{jt} and L_{jt} denotes the number of high-skilled and low-skilled workers in neighborhood j in time t, respectively. A_{jt}^H and A_{jt}^L are time-specific local productivity shifters. I define

$$\sigma = \frac{1}{1-\rho} \tag{4}$$

as the elasticity of substitution between high-skilled and low-skilled workers.

In a competitive labor market, skill wages are paid on their marginal products. Therefore,

$$W_{jt}^H = \frac{\partial Y_{jt}}{\partial H_{jt}} = (A_{jt}^H)^{\frac{\sigma-1}{\sigma}} \left[(A_{jt}^L)^{\frac{\sigma-1}{\sigma}} \left(\frac{H_{jt}}{L_{jt}} \right)^{-\frac{\sigma-1}{\sigma}} + (A_{jt}^H)^{\frac{\sigma-1}{\sigma}} \right]^{\frac{1}{\sigma-1}} \tag{5}$$

$$W_{jt}^L = \frac{\partial Y_{jt}}{\partial L_{jt}} = (A_{jt}^L)^{\frac{\sigma-1}{\sigma}} \left[(A_{jt}^L)^{\frac{\sigma-1}{\sigma}} + (A_{jt}^H)^{\frac{\sigma-1}{\sigma}} \left(\frac{H_{jt}}{L_{jt}} \right)^{\frac{\sigma-1}{\sigma}} \right]^{\frac{1}{\sigma-1}} \tag{6}$$

I denote $w_{jt}^s = log\left(W_{jt}^s\right), s \in \{H, L\}$. Next, the logarithm of the wage gap is

$$w_{jt}^{diff} := w_{jt}^H - w_{jt}^L = \frac{\sigma-1}{\sigma} log\left(\frac{A_{jt}^H}{A_{jt}^L} \right) - \frac{1}{\sigma} log\left(\frac{H_{jt}}{L_{jt}} \right) \tag{7}$$

Next, I consider a neighborhood firm going public in period t. The event influences adjacent neighborhoods through its spillover effect on productivity, as shown in the reduced form. I incorporate the scenario by modeling the evolution of local productivity shifters as a decreasing function of the distance to the IPO firm. For simplicity, I assume that the shock only affects the productivity of high-skilled workers, so low-skilled workers serve as the benchmark. I thus parameterize the productivity shifters as follows:

$$A^H_{jt} = cA^H_{j,t-1} exp(\lambda_0 + \lambda_1 d_{j \leftarrow IPO}) \tag{8}$$

$$A^L_{jt} = cA^L_{j,t-1}, \tag{9}$$

where c is some constant. The function $exp(\lambda_0 + \lambda_1 d_{j \leftarrow IPO})$ serves as a multiplier and measures the spillover effect on neighborhood j as a function of distance; $d_{j \leftarrow IPO}$ denotes the geographical distance from j to the location of the IPO issuer. I expect $\lambda_0 > 0$ and $\lambda_1 < 0$ because closer neighborhoods absorb larger positive spillover effects, and λ_0 and λ_1 measure the total effect and the decay rate, respectively.

Introducing A^H_{jt} and A^L_{jt} into Equation (7) and then taking the first difference, I calculate the change in wage gap as

$$\Delta w^{diff}_{jt} = \frac{\sigma - 1}{\sigma}(\lambda_0 + \lambda_1 d_{j \leftarrow IPO}) - \frac{1}{\sigma} \Delta log \left(\frac{H_{jt}}{L_{jt}} \right). \tag{10}$$

For the case $\sigma > 1$ (high-skilled and low-skilled labor are substitutes), the first part on the right-hand side accounts for the IPO spillover effect; this should be positive as the IPO is in favor of high-skilled workers. The second part measures the relative changes in the local labor supply and is inversely related to the wage gap. The reduced-form results show that both Δw_{jt} and $\Delta log \left(\frac{H_{jt}}{L_{jt}} \right)$ increase over time, so this must imply that the increase in the productivity differential overwhelms the increase in the relative labor supply. In the equation above, the structural estimators of interest are $(\sigma, \lambda_0, \lambda_1)$

4.1.2 Workers' Utility and Local Labor Supply

Next, I model the utility function of workers and derive the labor supply curve. The utility function is primarily a function of residential and workplace location, and it will determine the local labor supply and thus can be used for welfare analysis in equilibrium. Workers enjoy a utility gain from wages, which depend on their workplace location, and dis-utility from rents, which depend on their residential location. Commuting costs and amenities also enter the utility function in a linear manner. For heterogeneity, I let high-skilled and low-skilled workers have different preferences for such characteristics and estimate the parameters in the next section.

I consider the same finite number of neighborhoods as before. Each worker w, with skill level $s \in \{H, L\}$, chooses residential location i and work location j to maximize their utility. The worker consumes both local goods and tradable goods. In time t, their (indirect) utility is given by

$$V^s_{ijwt} = w^s_{jt} - \theta^s r_{it} - \gamma^s \tau_{ij} + a^s_{ijt} + \zeta^s \epsilon_{ijwt} \tag{11}$$

[22]

w^s_{jt} denotes the log wage for people working in neighborhood j at time t by their skill group; r_{it} is the log of spending on housing. I assume that high-skilled and low-skilled workers face the same housing market. In equilibrium, the annualized spending on housing is the same across homeowners and renters. θ^s is the spending share of income on local goods; τ_{ij} measures the commuting cost from home location i to work location j, and the elasticity can vary by skills; a^s_{ijt} is the endogenous amenity measures differing by skill. In addition to real wages, worker utility depends on local amenities directly, and different types of workers can have heterogeneous tastes with respect to amenities. Finally, I assume that the error term ϵ_{ijwt} follows the Type-1 Extreme Value (T1EV) distribution, scaled by location preference ζ_s. High-technology IPOs are modeled as exogenous and unexpected to local workers. The assumption is justified by analysis in the preceding section. Therefore, workers can re-optimize their utility by choosing (i, j) after the IPO event.

The above equation can then be divided by ζ^s with $\beta^s := \frac{1}{\zeta^s}$ to obtain the transformed mean utility δ^s_{ijt} equal to

$$\delta^s_{ijt} = \beta^s (w^s_{jt} - \theta^s r_{it} - \gamma^s \tau_{ij} + a^s_{ijt}) \tag{12}$$

This is the average utility across workers living in i and working in j. The setting enables me to focus on the shift in mean utility without knowing the idiosyncratic taste of workers. With T1EV distribution of error, the model is the conditional logit model in McFadden (1973). Therefore, in each skill group, the share of people choosing the combination (i, j) is the average probability that (i, j) maximizes the utility of workers. Therefore,

$$\hat{\pi}^H_{ijt}(\delta) := \frac{H_{ijt}}{\sum_{i'} \sum_{j'} H_{i'j't}} = \frac{exp(\delta^H_{ijt})}{\sum_{i'} \sum_{j'} exp(\delta^H_{i'j't})} \tag{13}$$

[22] The utility function is transformed from Cobb-Douglas utility assuming bounding budget constraint. Specifically, worker w maximizes utility by choosing their spending on local good M_{wt} and nationally tradable good C_{wt} subject to the workplace payroll W^s_{jt}. They enjoy gains from amenities and incur a commuting cost from home to work as a function of distance. Their utility is

$$\max_{M,C} \quad log(M^{\theta^s}_{wt}) + log(C^{1-\theta^s}_{wt}) + a^s_{ijt} + \zeta^s \epsilon_{ijwt}$$

subject to

$$R_{it} M_{wt} + P_t C_{wt} + exp(\gamma^s \tau_{ij}) \leq W^s_{jt}$$

I take the national good as numeraire so $P_t = 1$.

$$\hat{\pi}^L_{ijt}(\delta) := \frac{L_{ijt}}{\sum_{i'}\sum_{j'} L_{i'j't}} = \frac{exp(\delta^L_{ijt})}{\sum_{i'}\sum_{j'} exp(\delta^L_{i'j't})} \tag{14}$$

The estimation consists of two steps following the standard practice. The first step is to treat δ^s_{ijt} as parameters to estimate, and the effect of a high-technology IPO on welfare is the difference in δ^s_{ijt} before and after the event. In equilibrium, the predicted share of workers choosing (i, j) equalizes the actual share of workers. Equations (13)(14) are contraction mapping from the vector of mean utility to the share of workers by skill group (Berry et al., 1995). I can solve for mean utility numerically by non-linear least squares, as long as π^s_{ijt} is observed. Supplied with some starting values of δ^s_{ijt}, the model can solve for optimized mean utilities that minimize the following:

$$\sum_i \sum_j (\hat{\pi}^s_{ijt} - \pi^s_{ijt})^2 \tag{15}$$

The second interest is the utility response to changes in wages and rents, characterized by β_s and $\beta_s\theta_s$, respectively. As in Diamond (2016), I parameterize amenity changes as a function of the (log) employment ratio of high-skilled and low-skilled workers:

$$\Delta a^s_{ijt} = \eta^s log\left(\frac{H_{it}}{L_{it}}\right) + \Delta\epsilon^{a,s}_{ijt} \tag{16}$$

By Plugging Δa^s_{ijt} in Equation (12) and taking the first difference, the mean utility can be decomposed into real wages and amenities:

$$\Delta\delta^s_{ijt} = \beta^s(\Delta w^s_{jt} - \theta^s\Delta r_{it}) + \beta^s\eta^s\Delta log\left(\frac{H_{it}}{L_{it}}\right) + \beta^s\Delta\epsilon^{a,s}_{ijt} \quad , \qquad s \in \{H, L\} \tag{17}$$

The structural parameters of interest are $(\beta^H, \beta^L, \theta^H, \theta^L, \eta^H, \eta^L)$. I observe workplace wage Δw^s_{jt}, rent Δr^s_{it}, and employment ratio $\Delta log\left(\frac{H_{it}}{L_{it}}\right)$, but not the residual $\Delta\epsilon^{a,s}_{ijt}$. To separate variation in real wages that is exogenous to amenities, I adopt the shift-share IV on wages using 1990 as the base period. The instrument identifies shifts in local demand and thus correlates with contemporaneous changes in real wages. However, both the local industry composition in 1990 and national wage trends are orthogonal to current amenities changes, so the instrument satisfies the exclusion restriction. I discuss the IV construction and application below.

4.1.3 Labor Market Equilibrium

In equilibrium, the labor demand in each neighborhood equalizes the number of workers choosing to work in the neighborhood, and the utility of all workers is maximized. We can thus express the spillover effect of high-technology IPOs as a function of all the structural parameters above.

$$\begin{aligned}\underbrace{\Delta\delta^H_{ijt} - \Delta\delta^L_{ijt}}_{\text{double diff.}} = &\overbrace{\frac{\beta^H(\sigma-1)}{\sigma}(\lambda_0 + \lambda_1 d_{j\leftarrow IPO})}^{\text{high-skilled productivity}} - \underbrace{\frac{\beta^H}{\sigma}\Delta log\left(\frac{H_{jt}}{L_{jt}}\right)}_{\text{labor supply}} + \overbrace{(\beta^H - \beta^L)\Delta w^L_{jt}}^{\text{skill complementarities}} \\ &- \underbrace{(\beta^H\theta^H - \beta^L\theta^L)\Delta r_{it}}_{\text{rent}} + \underbrace{(\beta^H\eta^H - \beta^L\eta^L)\Delta log\left(\frac{H_{it}}{L_{it}}\right) + \Delta\epsilon^{a,H,L}_{ijt}}_{\text{amenities}}\end{aligned} \tag{18}$$

where

$$\Delta\epsilon^{a,H,L}_{ijt} := \Delta\epsilon^{a,H}_{ijt} - \Delta\epsilon^{a,L}_{ijt} \tag{19}$$

and the geographical distribution of workers is given by

$$H_{ijt} = \frac{exp(\delta^H_{ijt})}{\sum_{i'}\sum_{j'} exp(\delta^H_{i'j't})}\sum_{i'}\sum_{j'} H_{i'j't} \tag{20}$$

$$L_{ijt} = \frac{exp(\delta^L_{ijt})}{\sum_{i'}\sum_{j'} exp(\delta^L_{i'j't})}\sum_{i'}\sum_{j'} L_{i'j't} \tag{21}$$

The left-hand side of Equation (18) represents the net benefit of IPO on high-skilled workers using low-skilled workers as the reference group. Because σ is greater than 1, the welfare gap is positively related to high-skilled productivity and the IPO spillover effect. Next, it declines with the relative labor supply. The third term is derived as the residual term of relative wage, and the direction of skill complementarities depends on the sign of $\beta^H - \beta^L$. (β^H, β^L) can be interpreted as the (inverse) preference of location. If high-skilled workers have greater labor mobility, the difference would be positive, and the residual term would also be positive. Besides changes in relative wages (represented by the first three terms), the welfare differential is also exposed to rent and amenity changes, and the direction depends on the sign of the difference in structural parameters. We build on our intuition here by considering the simplest case that $\beta^H = \beta^L$, $\theta^H = \theta^L$ and $\eta^H = \eta^L$ where workers with different skills are homogeneous in their tastes for location, housing, and amenities. The welfare effect only consists of changes in productivity and local labor supply. Furthermore, for $\lambda_1 < 0$, the exposure to IPO shock is negatively related to distance.

Challenges for coefficient estimation arise from the fact that the residual term $\Delta\epsilon_{ijt}^{a,H,L}$ is unlikely to be exogenous to the utility differentials introduced by IPOs. There are many channels for IPOs to influence local amenities. For example, the development of local neighborhoods can attract more businesses in the service sector, such as banks, restaurants, and private schools. Firms can also invest in local infrastructure to meet their social responsibility and branding needs. In reduced-form estimation, the variation in amenities generated by the above channels is mainly absorbed in dynamic-case and county-fixed effects. In the structural estimation, I use the shift-share IV as an identification strategy.

4.1.4 Housing Market Equilibrium

Finally, I assume a perfectly inelastic housing supply, and all residents are renters who consume one unit of housing inelastically. The simplifying assumption enables me to focus on the labor-market commuting decision. Also, a data limitation is I do not observe the commuting flow by renters and homeowners separately. With the above assumption, the equilibrium rent is equal to the total expenditure on housing divided by the total population living in the neighborhood as follows:

$$r_{it} = log\left(\frac{\sum_j(\theta^H W_{jt}^H H_{ijt} + \theta^L W_{jt}^L L_{ijt})}{\sum_j(H_{ijt} + L_{ijt})}\right) \tag{22}$$

4.2 Estimation

The estimation consists of several steps. First, I treat the mean utilities of workers as parameters to estimate and then recover these from commuting flows. This step allows me to reveal the effect of IPOs as changes in utility. Next, to estimate the elasticity of utility with repsect to real wages and amenities, I apply the shift-share IV defined later in the section. Finally, I can uncover the relationship between the spillover effect and geographical distance by identifying λ_0 and λ_1. More details for data imputation come in Appendix A.

To ease any computational difficulty, I crosswalk data from the census-tract level to the ZIP-code level and still define the treatment group as ZIP codes within 0–15 miles of an IPO firm's headquarters and the control group as those within 15–30 miles. I use the subsample of high-technology IPOs during the period from 2005 to 2010 for the estimation. The sample consists of 194 out of the 396 high-technology IPOs in the previous analysis, and the estimation is run over each IPO event. For computational convenience, I restrict ZIP codes in the sample to those in IPO zones.

The outlined model can be regarded as a two-period model for empirical estimation: period $t = 0$ is year [-3, -1] before the year of the IPO, and period $t = 1$ is year [0, 5] after the IPO. I take the average of commuting flow measures for each period. The change in mean utility is the difference in (log) utility between the two periods and should be interpreted as a percentage change in untransformed utility. Given that the actual share of workers commuting between each pair of neighborhoods is observable, their mean utility can be recovered from Equation (15) by contraction mapping, and therefore calculating the difference in mean utility between $t = 0$ and $t = 1$.

As in Table (9), I find that a high-technology IPO increases the welfare of high-skilled workers but has the opposite effect on low-skilled workers. On average, a high-technology IPO is related to a 0.26% increase in the utility of high-skilled workers and a 0.91% decrease in the utility of low-skilled workers if using the full sample of neighborhoods in IPO zones (within 30 miles). If the sample of neighborhoods is restricted to the treatment group (within 15 miles), then there is a larger positive effect on high-skilled workers, as knowledge transfer and productivity spillover are more concentrated in the closer neighborhoods and decline with distance. Overall, the changes are economically and statistically significant.

For visualization purposes, I combine ZIP codes into ten bins $k = 1, ..., 10$. $k = 1$ representing ZIP codes that are within 0–3 miles, and $k = 2$ for 3–6 miles and so forth, and thus there should be 200 possible combinations of (i, j) for each IPO event.[23] I re-estimate the vector of mean utility on bins for each IPO event. Figure (10) displays the estimation result. On average, the change in welfare is positive for high-skilled workers and negative for low-skilled workers. As before, both utility changes are significant at the 1% level.

Next, I bring Equation (17) to the data to estimate the structural estimators and address the endogeneity concern by using the shift-share IV. In Appendix A, I detail the imputation of the workplace wage and construction of the shift-share IV.

Equation (17) is estimated by two-stage least squares estimation. I calibrate $\theta^H = 0.63$ and $\theta^L = 0.68$ for the share of income spent on local goods (Diamond, 2016). For robustness, I also use $\theta^H = \theta^L = 0.62$ (Moretti, 2013). The results are not sensitive to the choice of θ^s. In practice, I add an IPO-event-fixed effect since there is one representative IPO firm in the model. The fixed effect absorbs firm heterogeneity, which is not captured by the model.

Finally, Equation (18) enables the empirical estimation of spillover effect parameters (λ_0, λ_1). In addition to (θ^H, θ^L), I calibrate $\sigma = 1.4$ following Katz and Murphy (1992) and use the estimated values of $(\beta^H, \beta^L, \eta^H, \eta^L)$. The identifying assumption is that the distance from a workplace to the IPO firm is orthogonal to amenity changes in the neighborhood.

[23] Note that the step is for visualization purpose only. In subsequent estimations, each ZIP code remains an independent neighborhood.

The above discussion is abstracted from the commuting cost $-\gamma^s\tau_{ij}$ in workers' utility, as it drops out when taking the first-differenced utility. As part of the estimation, I can also estimate the semi-elasticity of commuting cost by a gravity equation. By taking logarithm for equation (13)(14) and substituting the mean utility by equation (12), I can yield the following reduced-form relationship:

$$log(\pi^s_{ij}) = \xi^s\tau^s_{ij} + \phi^s_{it} + \phi^s_{jt} + \epsilon^s_{ij} \tag{23}$$

where the coefficient $\xi^s := \beta^s\gamma^s$ characterizes the semi-elasticity of workers' decisions on commuting distance; ϕ^s_i and ϕ^s_j are skill-specific home-period and work-period fixed effects. I augment Equation (23) by adding case-by-period fixed effects and clustering standard errors at the IPO-zone level.

Although the above equation is estimated separately by skill groups, I yield $\xi^s \approx -0.09$ for both groups. Hence, the probability of commuting is negatively related to the commuting distance, but the relationship does not vary much with the skill of workers. Corresponding results are in Table (10). With $\xi^H = \xi^L = -0.09$, I can calculate the commuting parameter (γ^H, γ^L) with estimated $(\hat{\beta}^H, \hat{\beta}^L)$.

Table (11) provides the estimation result for the parameters. First, the shift-share IV returns a very strong first stage. Second, the values $\beta^H = 3.712$ versus $\beta^L = 3.428$ mean that high-skilled workers have a slightly less heterogeneous preference for locations. This is consistent with the empirical finding on greater mobility of high-skilled workers than low-skilled workers. Importantly, I find that the spillover effect on productivity is far-reaching, given the low value of $\lambda_1 = 0.0008$.[24] Meanwhile, $\lambda_0 \approx 0.2$ implies that a representative IPO would raise the productivity of high-skilled workers in its local neighborhood by 21.7%. The magnitude is very close to the estimate of innovation spillover strength by Matray (2021), who estimates that the number of patents in an area has an elasticity of 0.2 on innovation activities by listed firms in the locality.

To shed more light on policy, I perform a counterfactual exercise by modifying the magnitude of the productivity shock λ_0. Other structural parameters are calibrated according to the values in Table (11). I assume the economy is in equilibrium at period 0 and thus use all observed demographic characteristics as inputs. In period 0, ten thousand workers are assigned to neighborhoods, and these are collected in bins as before. In the simulation, I begin by recovering the productivity fundamentals $\{A^H_0, A^L_0\}$ in period 0 from Equations (5) and (6), and amenity fundamentals a^H_0, a^L_0 from Equation (12).[25] I then model the evolu-

[24] As the distance to IPO firms is truncated by up to 30 miles, I cannot infer the boundary of the spillover effect.

[25] To keep it simple, I assume exogenous amenity, which implies that amenity fundamentals do not change

tion of productivity based on productivity shocks. The second step involves simulating the commuting flow in period 1. For this purpose, wages and rents are expressed as functions of $\{H_{ij1}, L_{ij1}\}$, reflecting the mean utility of high-skilled and low-skilled workers for each pair (i, j). Consequently, Equations (13) and (14) form a system of equations used to numerically solve for $\{H_{ij1}, L_{ij1}\}$.

Figure A5 provides the simulation results for the number of workers and residents, wages, and rents with respect to the strength of the productivity shock. All figures are expressed as a ratio relative to no productivity shock. As the productivity shock increases, wages for high- and low-skilled workers rise due to productivity gains and the skill complementarity effect, respectively. Workers in locations near the IPO headquarters benefit more significantly than those located further away. However, rents also increase with productivity, and neighborhoods close to the headquarters experience higher rents. Nevertheless, the overall effect on wages and rents is evenly distributed across neighborhoods near and far from the headquarters, as the decay of the productivity shock is relatively slow.

5 Robustness Check

This section revisits the reduced-form estimation to address potential concerns related to sample selection and identification. First, I apply alternative identification strategies to enhance credibility. Concerns in this area may stem from two factors: (1) the validity of using withdrawn high-technology issuers as a counterfactual and (2) the staggered adoption of IPOs. I address these concerns by analyzing the treatment assignment and substituting the counterfactual IPOs with a propensity score model, which includes a comprehensive set of covariates. The estimation supports the baseline findings. Additionally, I refine the sample of IPO events by excluding certain metropolitan areas in which high-technology firms are concentrated. By evaluating the remaining IPO events, I confirm that the observed effects are not driven by labor and housing market trends in major cities or counties. Finally, in the online appendix, I show that the gentrification effect is unique to high-technology IPOs by running the same DiD regressions on non-high-technology IPOs.

5.1 Validate Counterfactual by Triple Difference-in-Differences

While controlling for distance to withdrawn IPO issuers helps identify spillover effects, a remaining concern is the potentially systematic differences between treated and control census

in period 1. However, relaxing the assumption with Equation 16 does not change the effect of the simulated productivity shock.

tracts due to their varying proximities to successful IPO headquarters. Consider a city with two Central Business Districts (CBDs), each generating its own local spillover effects. If a successful IPO issuer locates in CBD A while a withdrawn issuer chooses CBD B, neighborhoods might share similar distances to CBD B but differ fundamentally due to their varying proximity to CBD A. These underlying differences could confound our estimates of IPO effects.

To address this concern, I implement a triple difference-in-differences (DiD) approach that compares neighborhoods near successful IPO issuers with those near withdrawn issuers. This strategy ensures that treatment and control groups have similar proximity characteristics, differing primarily in their exposure to actual versus withdrawn IPOs. Specifically, I estimate:

$$Y_{it} = \alpha + \beta Treat_{ik} \times \mathbf{1}t > t_0^k \times successful_k + \beta Treat_{ik} \times \mathbf{1}t > t_0^k + \mathbf{X}_{\mathbf{it}}^{\intercal}\mathbf{\Pi} + \delta_{ik} + \gamma_{ct} + \eta_{kht} + \epsilon_{ikt} \tag{24}$$

where $successful_k$ indicates whether firm k's IPO was completed. The specification includes census tract-by-IPO case fixed effects (δ_{ik}), extends the sample to include census tracts within 30 miles of withdrawn IPO issuers, and clusters standard errors at the case level. This approach helps isolate the pure effect of IPO completion from location-specific characteristics, providing a more robust test of our main hypotheses.

Table (13) presents the results of Triple DiD estimation. The coefficients of $TreatXsuccessfulXpost$ identify the treatment effect. For most of key outcomes, such as high-skilled wage and housing price, the coefficients remain significant, which assures the robustness of the baseline estimation.

5.2 Validate Counterfactual by Propensity Score Model

While the triple DiD approach addresses concerns about differential proximity effects, it still relies critically on the assumption that withdrawn IPO issuers serve as valid counterfactuals. Although prior literature suggests that IPO withdrawals are primarily driven by market timing rather than local economic conditions, there may be systematic differences in location choices between successful and withdrawn issuers. For example, more capable founding teams might select gentrifying urban neighborhoods for their headquarters, while less successful teams choose to locate in declining areas. In such cases, both our standard and triple DiD approaches could capture these underlying location quality differences rather than the pure IPO effect. To address this potential selection bias, I complement the previous analyses with a propensity score matching approach. This strategy directly matches treated and control census tracts based on observable characteristics that predict high-technology firm locations, without relying on withdrawn IPOs as counterfactuals. By constructing comparison groups with similar ex-ante probabilities of hosting high-technology firms, this approach provides an alternative test of our main hypotheses that is robust to concerns about the validity of withdrawn IPO counterfactuals.

To be specfic, I validate the identification strategy and enhance the credibility of the findings by estimating a propensity score model using the IPO events in the sample as the response. I employ characteristics from the 2000 Census, as they were collected before all IPO events. To account for nearby census tracts, I calculate variables for each tract within 0–5, 5–10, and 10–15 mile ranges and include rich interactions between these variables. Table B2 provides definitions for all variables in the model. The outcome is a binary variable that takes the value of one if there is an IPO event in the census tract. To avoid overfitting, I utilize the LASSO-Logit model, which minimizes the following:

$$\sum_{i=1}^{N}(Y_i - \alpha - G(\mathbf{X_i^\intercal}\beta))^2 + \lambda||\beta||, \tag{25}$$

where G(·) is the logistic function. The optimal λ is selected by ten-fold cross-validation. Next, I estimate the predicted propensity score for each census tract and divide the tracts into quintiles by treatment and control group. In this way, I can balance the panel by rich covariates.

Figure (5) (c) illustrates the identification strategy. The shaded and blank areas are as before. For neighborhoods, the dark blue area and the light blue area share a similar propensity of holding a high-technology IPO and thus belong to the same propensity score bin, while only the former is close to the real IPO issuer. Therefore, the difference in outcomes between the two groups is again attributable to the effect of going public.

The underlying assumption is that the location choice and timing of IPOs are independent of neighborhood characteristics, conditional on all observed variables in the propensity score model and all fixed effects added to the regressions. These fixed effects absorb dynamic changes at the county and IPO-zone levels. Although I cannot entirely rule out the influence of unobserved characteristics—a fundamental limitation of matching methods—the concern should be alleviated by the identification strategy offered by withdrawn IPOs. By combining the two strategies above, the results are expected to be highly credible.

I re-run Equation (1) and substitute the distance-fixed effect with the propensity-score fixed effect. This approach allows me to compare tracts with similar local characteristics potentially correlated with IPOs that only differ in whether they receive the actual IPO treatment. The estimation result is presented in Table (12). The effects remain significant after altering the identification strategy through fixed effects.

5.3 Validate Assignment of Treatment

Section 3 considers all high-technology IPO events, although some IPO zones overlap, especially for firms headquartered in metropolitan areas such as Boston, Houston, and San Francisco. A direct problem for identification is that a census tract in the treatment group of an IPO event can also belong to the treatment or control group of another IPO. This leads to a concern that different treatments can interfere with each other.[26] Unfortunately, like other issues in staggered DiD, there is no perfect solution. Nevertheless, I provide two additional tests to alleviate the concern.

First, I run a DiD similar to specification (1) for each IPO event. The purpose is to identify the case-specific treatment effect and compare this with the average treatment effect, as in Greenstone et al. (2010). The case-by-case DiD specification is as follows:

$$Y_{it}^k = \alpha^k + \beta^k Treat_i^k \times \mathbf{1}\{t > t_0^k\} + \mathbf{X_{it}^\intercal}^k \mathbf{\Pi}^k + \delta_i^k + \gamma_{ct}^k + \eta_h^k + \epsilon_{it}^k \tag{26}$$

The superscript k means that the regression is specific to the IPO case k. The only substantial change is that the propensity-score-fixed effect no longer interacts with the IPO- or year-fixed effect. Since each IPO zone contains a small share of observations, interacted fixed effects would reduce excessive variation. The equation above compares the outcome of census tracts in the same county with similar propensity scores each year. Standard errors are clustered at the county-by-year level.[27]

Figure (11) plots the treatment effect for various outcomes from low to high, including the 95% confidence interval. In line with the main result, the majority of IPO events have differential effects on the wages and employment of workers with different skills and a positive impact on housing value and rents.

However, the analysis still has limitations regarding identification since a census tract that has been treated by an IPO can also serve as a control unit for another. Consequently, if the treatment group of IPO firm A constitutes the control group of IPO firm B, then the estimate for firm B would actually represent the treatment effect given by B minus the effect by A.

Therefore, the second robustness check considers only the first time that census tracts are

[26] Consider an artificial example where the whole area consists of two IPO events, A and B, with A happening before B. If part of the treatment group of event A serves as the control group of event B, then a researcher who adopts DiD may underestimate the treatment effect of event B due to contamination by prior event A.

[27] If clustering at the county level, there would be too few clusters in the regression.

treated by IPO events. Once a census tract falls within 15 miles of an IPO, the DiD dummy variable takes the value of one from then on, and I do not consider subsequent treatments for this tract. In this approach, each tract can only be treated by one IPO event.

The control group is comprised of census tracts that are never included in the 15-mile ring of IPOs but are in the 30-mile ring of at least one IPO. As with the treatment group, they are attached to the IPO for which they first fall into the 30-mile ring. The specification is exactly the same as that for Equation (1), and standard errors are still clustered at the IPO-zone level.

Table (15) presents similar results to Table (12). The downside is that the assignment places higher weight on earlier IPOs since they include more observations than later IPOs. For the same reason, the magnitude of coefficients is larger than the baseline estimation. As with other technology-related changes, high-technology IPOs can exhibit decreasing marginal returns. In the early 2000s, the U.S. had far fewer high-technology firms, but there has been substantial growth in the sector in recent years. For the same additional IPO, the effect is more considerable when local neighborhoods have fewer firms that are similar. Therefore, all other things remaining equal, earlier high-technology IPOs should have a greater impact than those that occur later.

5.4 Exclude Metropolitan Areas

High-technology firms typically prefer to be located in metropolitan areas, thereby contributing to the development of large cities. For instance, Boston's city development is closely associated with the influx of biotechnology and pharmaceutical firms, and a similar pattern is observed in cities in California and Texas. This raises two concerns for the research. First, the effect of high-technology IPOs may coincide with trends in big cities, which I cannot precisely control. Second, there may be a concern that the estimation is driven by IPOs in big cities rather than being a general case across the nation.

These two issues can be addressed by excluding IPOs in (economically) large metropolitan areas. According to summary statistics, the top three cities with the highest number of high-technology IPOs are New York (17), Houston (13), and Austin (11), while the top three counties are Middlesex, Massachusetts (21), Santa Clara, California (20), and San Mateo, California (18). After excluding IPOs in these cities and counties, the sample size reduces from 396 to 292. However, the sample remains sufficiently large for valid inferences.

Using a subset of data, I re-run Equation (1) and obtain comparable results, as shown in Table (14). For skill wages and the skill wage premium, the estimated treatment effects are slightly smaller. The consistency in wage results suggests that large cities benefit from increased labor productivity due to their geographical and demographic characteristics. However, the skill wage premium grows moderately, which may imply that the development of large cities plays some role in gentrification and inequality, but the effect of high-technology IPOs remains prominent. As for housing prices and rent, the effect also appears slightly smaller. Large cities are usually characterized by relatively high property prices. Given that prices and rent are in logarithmic form, coefficients should be interpreted as the percentage increases or decreases in prices and rent, which helps explain the magnitude of the coefficients.

5.5 Non High-technology IPOs

To further validate the skill-biased technological shock channel, I examine the local economic effects of non-high-technology IPOs. These IPOs provide an ideal comparison group as they generate similar liquidity shocks through wealth creation but typically involve less technological innovation and knowledge spillover. If the differential impacts on high-skilled versus low-skilled workers primarily stem from technological spillovers rather than general IPO effects, we should observe distinct patterns between high-technology and non-high-technology IPOs.

Table (B13) presents the estimation results for non-high-technology IPOs using the same empirical specification as in the main analysis. The results reveal a markedly different pattern from high-technology IPOs. Following non-high-technology IPOs, low-skilled worker wages in nearby neighborhoods increase, and this wage growth is accompanied by increases in both housing prices and rents. However, I find no significant effects on high-skilled worker wages or the wage differential between high and low-skilled workers. Moreover, the relative labor supply of high to low-skilled workers remains unchanged.

These findings support three key insights about the mechanisms through which IPOs affect local economies. First, the liquidity channel appears to operate similarly across both types of IPOs, as evidenced by the consistent increases in housing costs. When firms go public, regardless of their technological intensity, the wealth created through the IPO flows into local real estate markets, driving up housing prices and rents. This aligns with previous findings by Hartman-Glaser et al. (2022) on the relationship between IPO-generated wealth and local housing markets.

Second, the absence of wage differentials in non-high-technology IPOs suggests that the skill-biased effects observed in high-technology IPOs are not inherent to the IPO event itself. Rather, they likely stem from the specific nature of technological innovation and knowledge spillovers characteristic of high-technology firms. This interpretation is consistent with literature documenting how public status enables technology firms to better attract talent and invest in innovation (Bernstein (2015), Maksimovic et al. (2022)).

Third, the positive effect on low-skilled wages following non-high-technology IPOs suggests that these events can benefit local workers through traditional channels such as increased local demand for goods and services. The wealth effect from the IPO may boost local consumption, creating job opportunities and wage growth for low-skilled workers. However, unlike high-technology IPOs, these benefits appear to be more evenly distributed across skill groups, as evidenced by the absence of significant changes in wage differentials or relative labor supply.

The contrasting results between high-technology and non-high-technology IPOs provide compelling evidence for the skill-biased technological shock channel. While both types of IPOs generate local wealth effects that influence housing markets, only high-technology IPOs produce significant wage differentials between skill groups. This pattern suggests that the inequality-enhancing effects of high-technology IPOs stem primarily from their role in accelerating technological change and knowledge spillovers, rather than from general IPO-related wealth effects.

These findings also help explain why previous studies examining the local economic effects of IPOs have yielded mixed results (Butler et al. (2019), Cornaggia et al. (2019)). By treating all IPOs as homogeneous events, prior research may have obscured the distinct channels through which different types of IPOs influence local economies. The heterogeneous effects documented here underscore the importance of distinguishing between high-technology and non-high-technology IPOs when studying their impact on local economic outcomes.

6 Concluding Remark

This paper connects equity markets and local economies, identifying IPOs by high-technology firms as an important but less observable source of inequality within neighborhoods. The results suggest that the effect of high-technology IPOs favors high-skilled workers through knowledge spillovers on productivity, as these workers primarily experience a net increase in welfare due to receiving higher real wages. Low-skilled workers living in the same area bear the brunt of IPOs and are more likely to be displaced from their residences and workplaces by higher living costs. Other indicators of gentrification, such as homelessness rates, also increase significantly following high-technology IPOs. In sum, the pre-existing welfare gap between different types of workers is exacerbated by high-technology IPOs.

Overall, the aggregate impact of a typical high-technology IPO on local neighborhoods is substantial and long-lasting. The causal evidence is not limited to metropolitan areas or smaller economies and remains robust when using identification strategies such as withdrawn issuers or the propensity score model as counterfactuals.

Considering the ubiquity of large-scale IPOs by technology firms, this paper emphasizes the need for policymakers to monitor the increasing inequality when funding high-technology firms and promoting their IPOs. Further research could focus on designing optimal social subsidy schemes for low-skilled residents vulnerable to displacement. For instance, increasing the supply of amenities or facilitating job searches can help mitigate the side effects of these IPOs.

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7 Figures

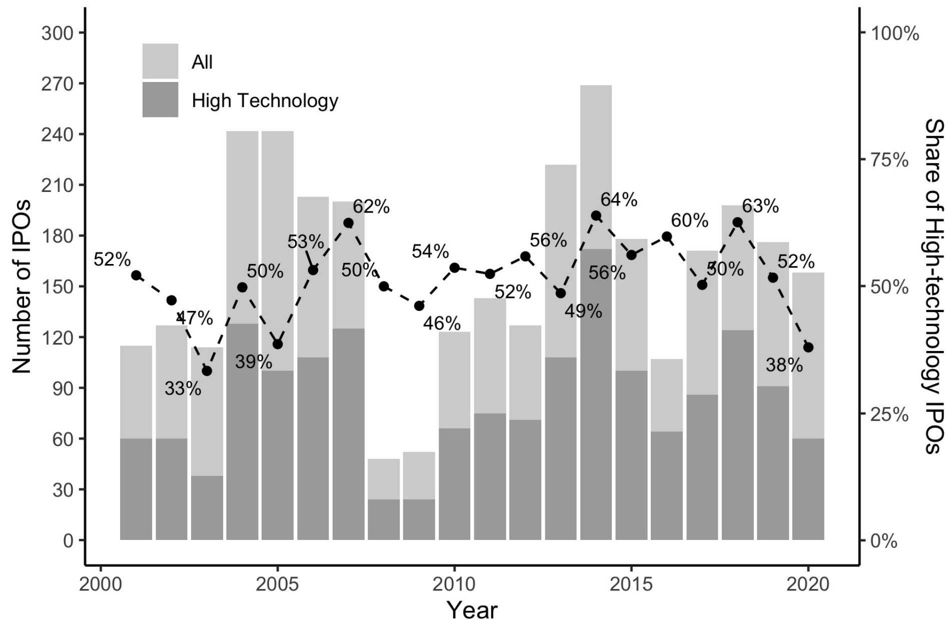


Figure 1: Number of High-technology IPOs

Notes: The figure presents number of IPOs by year. IPO Data are from Audit Analytics, and classification of high-technology follows list of NAICS codes issued by NSF. The bars indicate for number of cases and correspond to the left Y-axis. The dotted line describes share of high-technology IPOs and corresponds to the right Y-axis.

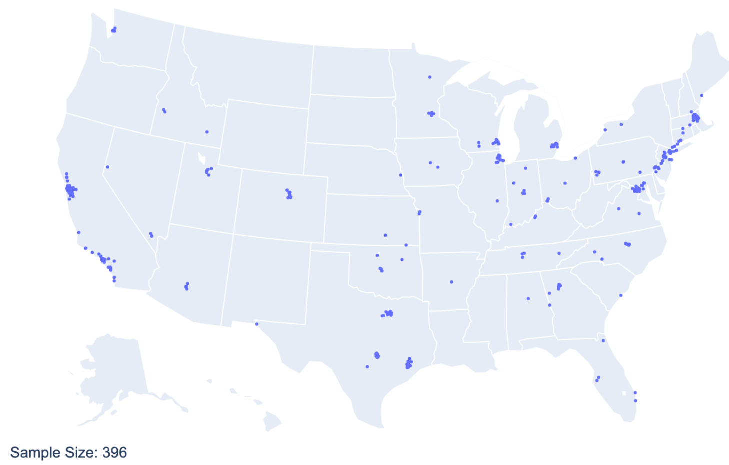


Figure 2: Location of Headquarter of IPO firms

Notes: The figure presents geographical distribution of the headquarter of high-technology IPO firms across the U.S. from 2003 to 2017. To ensure a high-quality sample and focus on influential IPOs , I exclude ABS & REIT and further restrict to firms with IPO share price no less than $5 and assets larger than $100 million by the year-end of IPO. In total, the sample consists of 396 high-technology firms.

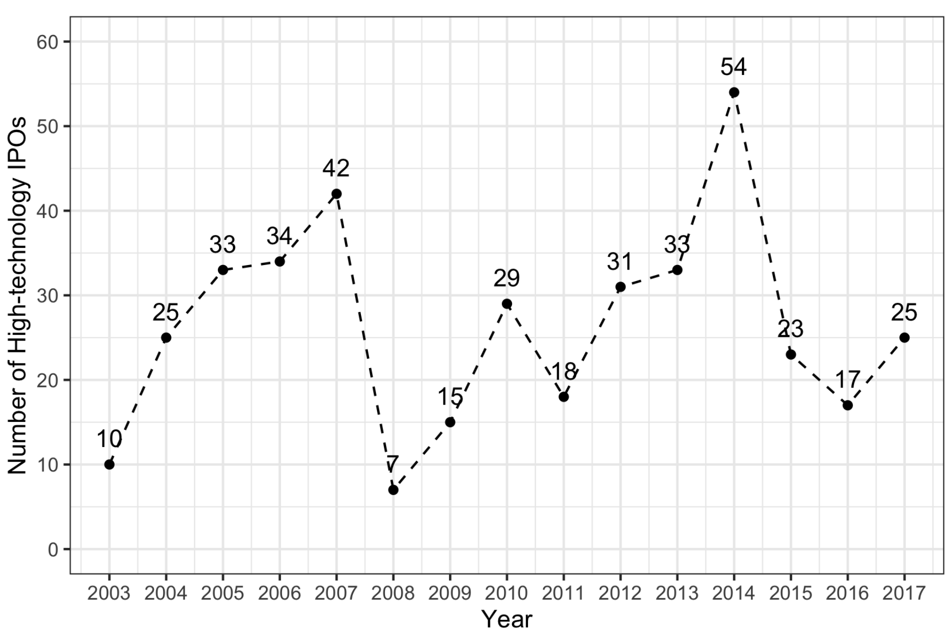


Figure 3: Number of High-technology IPOs in the Sample by Year

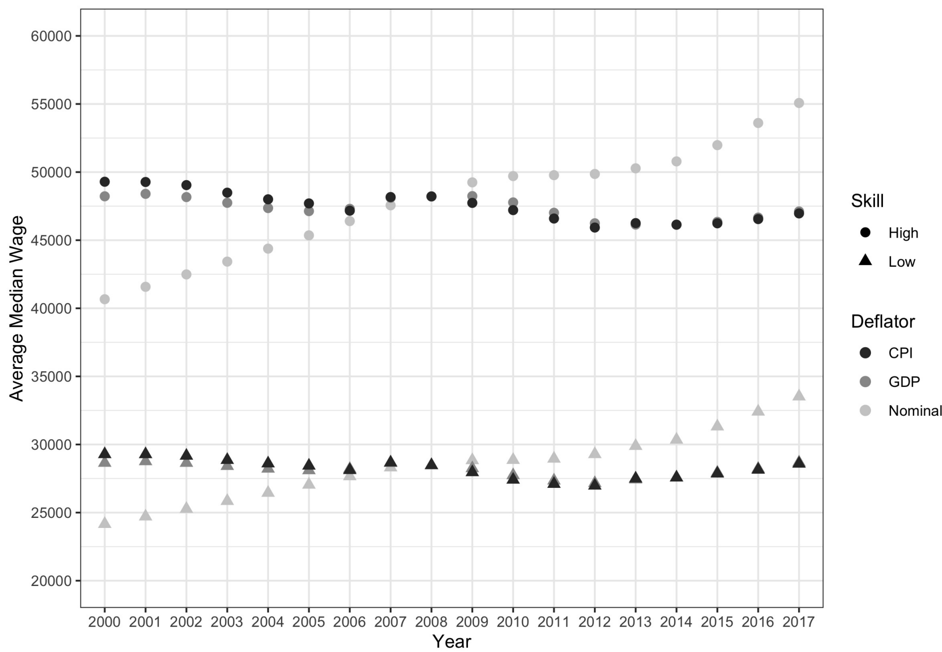
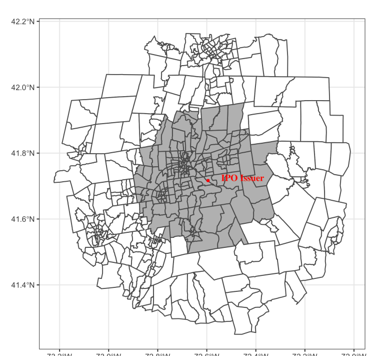
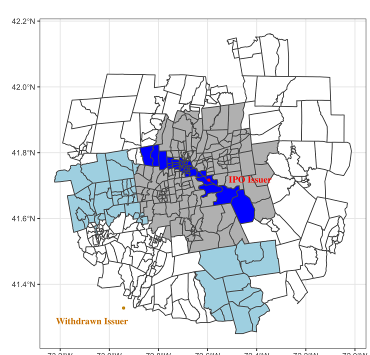


Figure 4: Average Wage by Year

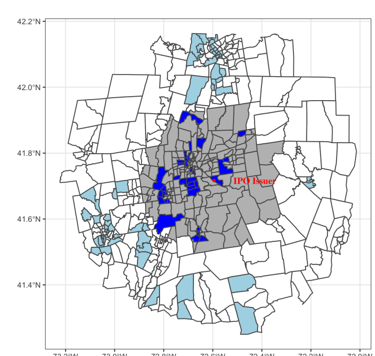
Notes: Wages are median wages in the census tract level from ACS 5-year data. Each point represents the average of all observations in a given year. To balance the panel, I restrict to census tracts with population greater than 100 in 2010, and with complete time series wage observations after imputing missing values. Wages are adjusted to 2010 dollars. Results show that the two adjusted wages are very similar and constant over time, and I use the GDP-adjusted wages for analysis throughout the paper.



(a) By IPO Proximity only



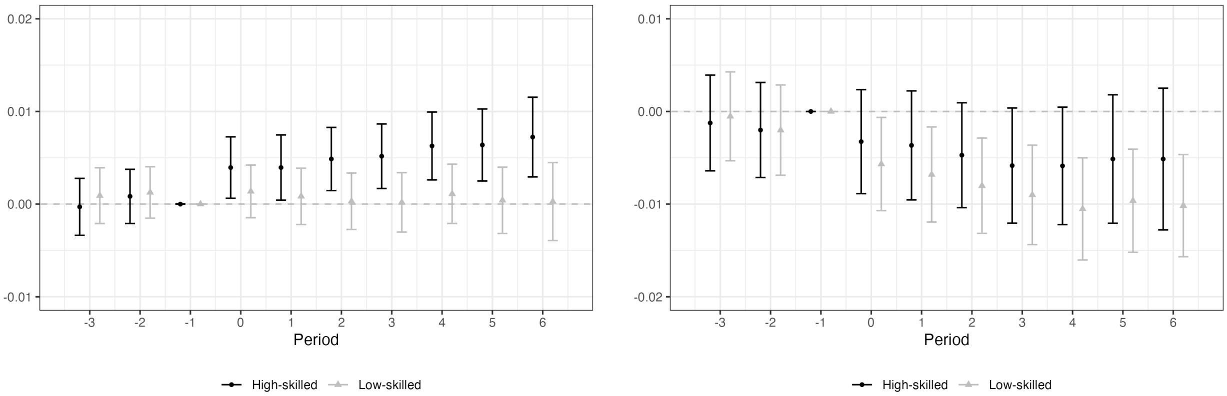
(b) By Counterfactual IPO



(c) By Propensity Score

Figure 5: Visualization of Identification Strategy

Notes: The figure visualizes the identification strategies by using IPO of Open Solutions Inc as an example. In graph (a), the centering red point identifies location of headquarter of the IPO firm. Shaded area consists of census tracts within 15 miles, and blank area is tracts within 15-30 miles. In graph (b), the dark yellow point in the left bottom indicates the headquarter of closest withdrawn issuer. While the dark blue area and the light blue area share similar proximity to the withdrawn issuer, only the former is close to the real IPO issuer. tracts colored dark blue within the shaded area have similar distance to the withdrawn issuer as tracts colored light blue in the blank area. In graph (c), the only difference is that tracts colored dark blue within the shaded area have similar estimated propensity score as tracts colored light blue in the blank area. The identification strategy compares outcomes of the dark blue area with the light blue area.



(a) Wage

(b) Employment

Figure 6: Estimation of Dynamic Treatment Effect on Labor Market with Withdrawn IPO
Notes: The figure plots the dynamic treatment effect on wage and employment estimated by dynamic difference-in-differences with covariates. Results are by skill group of workers. The horizontal axis is the relative period to the year of IPO. The vertical axis is the magnitude of effect relative to effect in period -1, which is normalized to 0. The 95% confidence intervals are simultaneous confidence intervals calculated with covariates. Notice that periods before -1 are falsification tests, and the results indicate that assumption of parallel trend is satisfied, because confidence intervals of estimates contain 0. Standard errors are clustered at the IPO firm level.

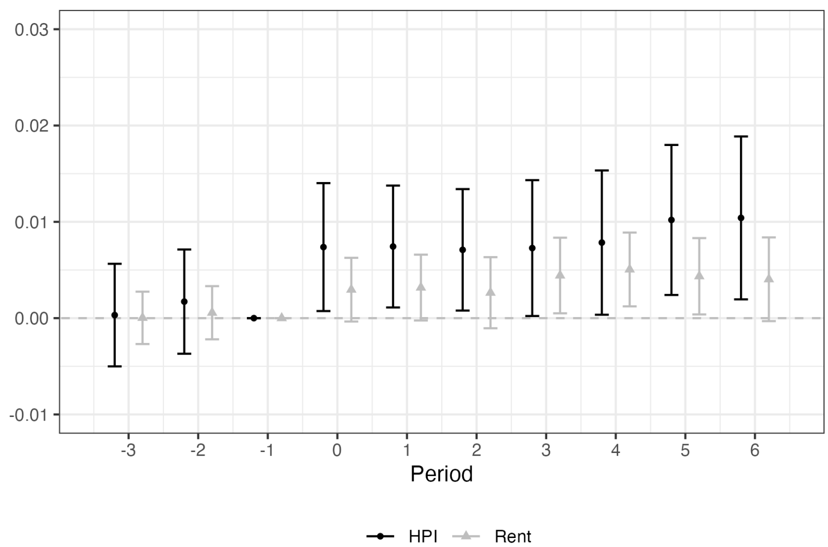
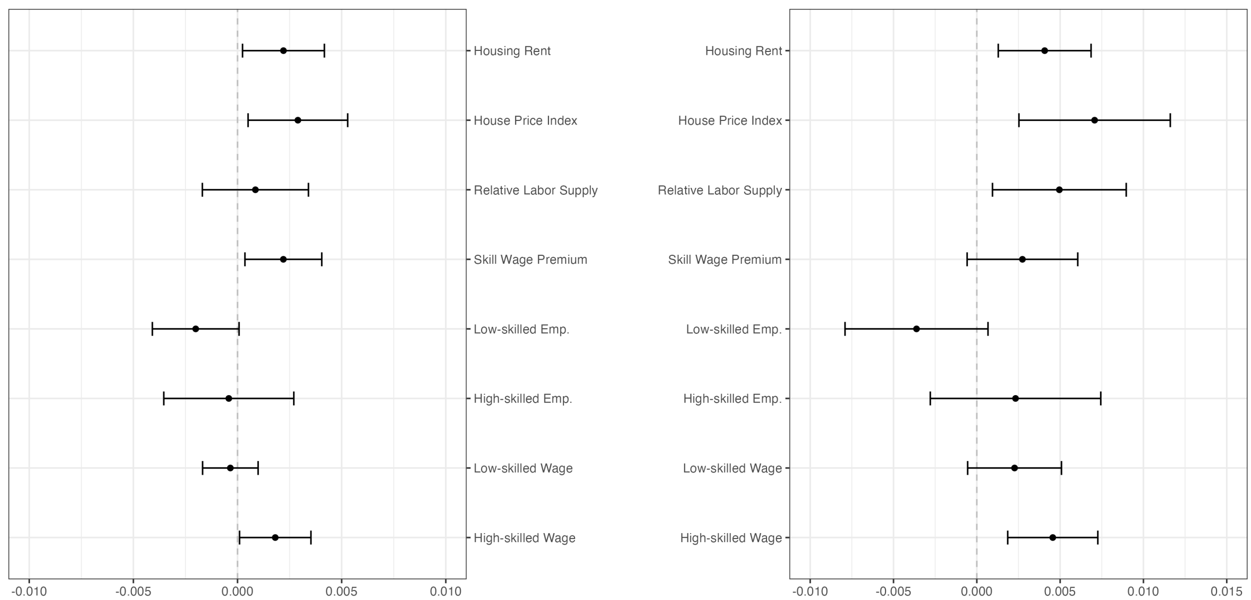


Figure 7: Estimation of Dynamic Treatment Effect on Housing Market with Withdrawn IPO

Notes: The figure plots the dynamic treatment effect on outcomes of housing markets estimated by dynamic difference-in-differences with covariates. The horizontal axis is the relative period to the year of IPO. The vertical axis is the magnitude of effect relative to effect in period -1, which is normalized to 0. The 95% confidence intervals are simultaneous confidence intervals calculated with covariates. Notice that periods before -1 are falsification tests, and the results indicate that assumption of parallel trend is satisfied, because confidence intervals of estimates contain 0. Standard errors are clustered at the IPO firm level.

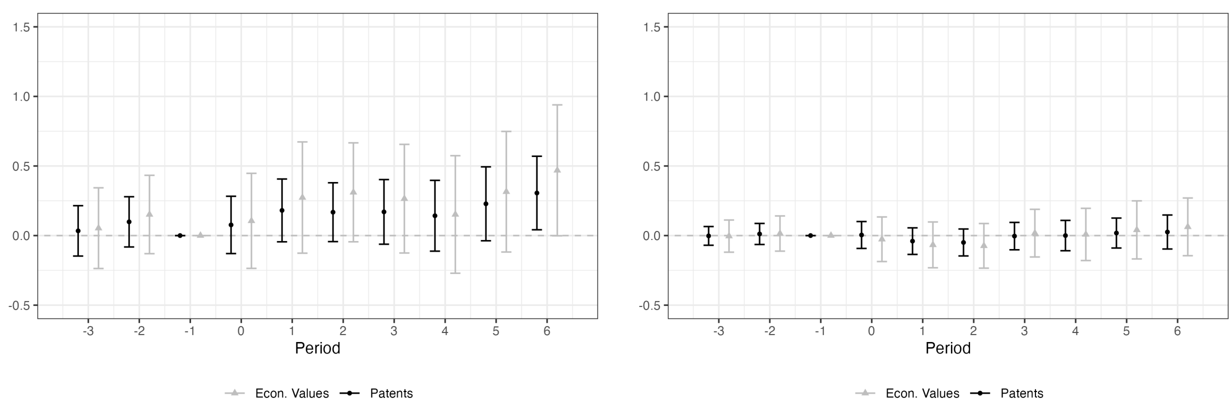


(a) Employment Growth

(b) VC-backed IPOs

Figure 8: Heterogeneity Tests for Expansion and Liquidity Channels

Notes: Figure (a) plots the incremental treatment effect with 95% confidence intervals on different outcomes of housing markets by employment growth of IPO firms, and Figure (b) plots by VC-backed versus non-VC-backed IPOs. Coefficient estimates are the interaction between the DiD dummy (*TreatXpost*) and the proxy for relative channel (employment growth or VC). The benchmark is the treatment effect estimated in Table (6). Standard errors are clustered at the IPO firm level.



(a) Same Industry

(b) Different Industry

Figure 9: Estimation of Dynamic Treatment Effect on Patenting Activity with Withdrawn IPO

Notes: The figure plots the dynamic treatment effect on the number and economic values of patents estimated by dynamic difference-in-differences with covariates. Results are by skill group of workers. The horizontal axis is the relative period to the year of IPO. The vertical axis is the magnitude of effect relative to effect in period -1, which is normalized to 0. The 95% confidence intervals are simultaneous confidence intervals calculated with covariates. Notice that periods before -1 are falsification tests, and the results indicate that assumption of parallel trend is satisfied, because confidence intervals of estimates contain 0. Standard errors are clustered at the IPO firm level.

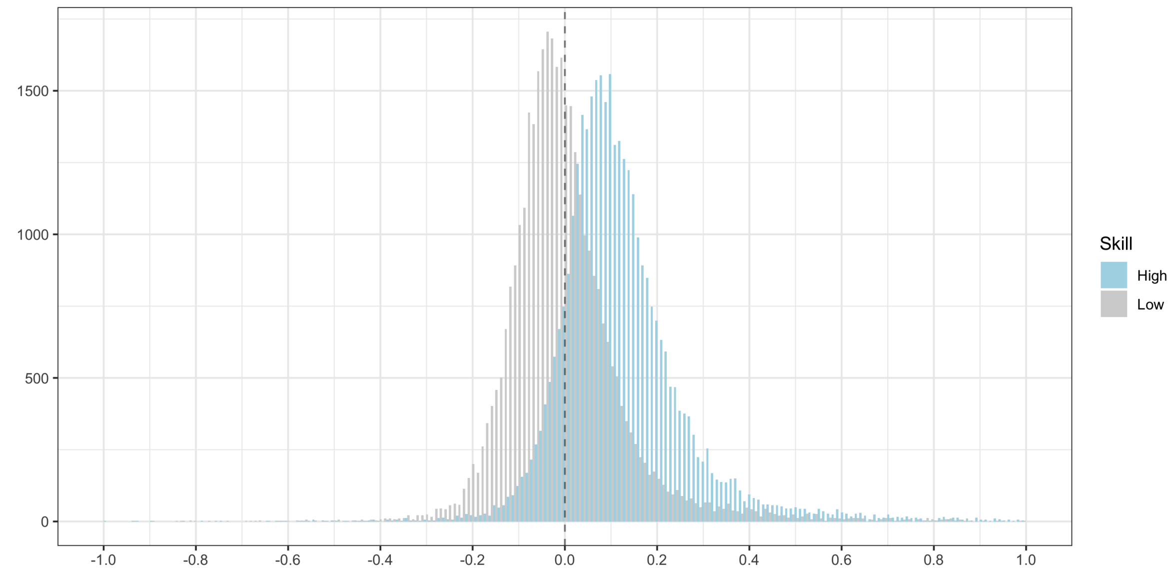
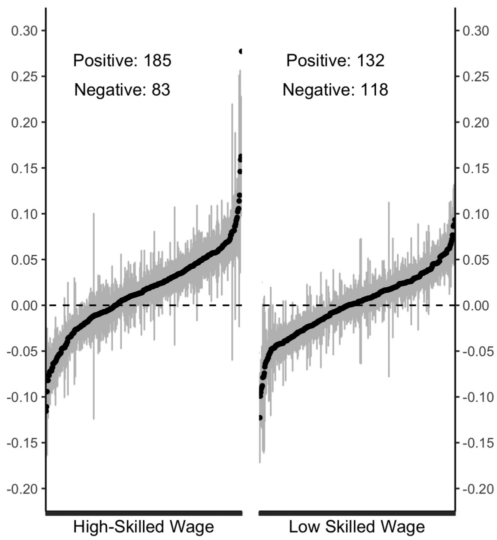
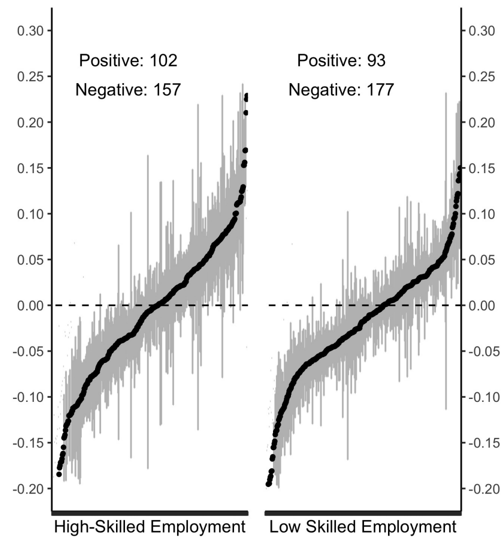


Figure 10: Change in Mean Utility across all Workers by Skill

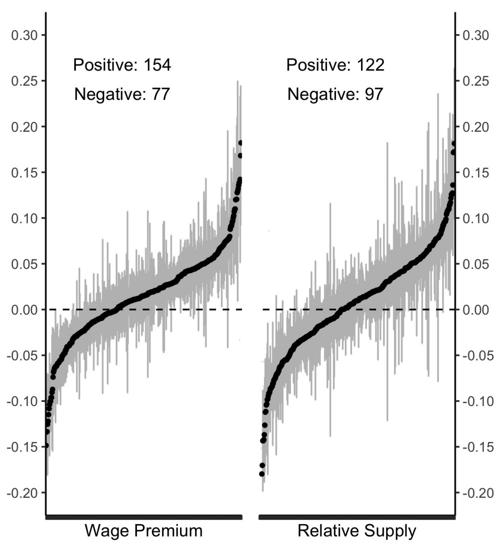
Notes: The histogram represents estimation of changes in utility across all workers by skill in IPO Zones. Mean utility before and after IPO events are estimated by contraction mapping of conditional logistic model on home-workplace commuting flows. Each observation is a three-mile ring of zip codes. On average, utility of high-skilled workers increases after IPOs, but low-skilled workers are hurt. The estimation corresponds with the reduced-form results on welfare outcomes.



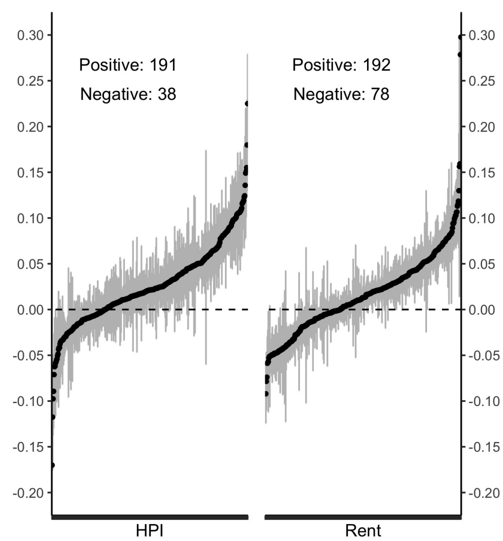
(a) Wage



(b) Employment



(c) Wage and Employment Gap



(d) Housing

Figure 11: Case-by-Case Treatment Effect

Notes: The figure plots the treatment effects by estimating each IPO event separately, a robustness test similar to Greenstone et al. (2010). In each figure, coefficient estimates are ranked from lowest to highest, and error bars represent for the 95% confidence interval. For each outcome variables, *Positive (Negative)* refers to number of cases in which coefficient is significantly greater (less) than zero. Due to much fewer observations, the estimation is more noisy. However, one can still easily observe that the majority of IPOs have effects on labor market and housing market in the same direction of previous estimation based on full sample.

8 Tables

Table 1: Areas with Highest Number of High-technology IPO Firms

City	N	County	N	State	N
New York	17	Middlesex, MA	21	CA	93
Houston	13	Santa Clara, CA	20	TX	46
Austin	11	San Mateo, CA	18	MA	29
Cambridge	8	New York, NY	17	NY	24
Dallas	8	Los Angeles, CA	15	IL	18
San Francisco	8	Harris, TX	13	VA	18
Arlington	6	Cook, IL	12	NJ	15
Chicago	6	Alameda, CA	11	PA	15
Seattle	6	Dallas, TX	11	CO	11
Los Angeles	5	Travis, TX	11	MD	11

Note: The table outlines geographical areas with the highest number of high-technology IPOs in the sample by city-, county- and state-level.

Table 2: Top SIC Codes in the Sample of High-Technology IPOs

SIC Code	N	SIC Description
7370	68	Computer Programming, Data Processing, Etc
2836	37	Biological Products, Except Diagnostic Substances
7372	29	Prepackaged Software
1311	25	Crude Petroleum and Natural Gas
3674	21	Semiconductors and Related Devices
2834	17	Pharmaceutical Preparations
7373	13	Computer Integrated Systems Design
4899	12	Communications Services, Not Elsewhere Classified
6282	12	Investment Advice
3845	10	Electromedical and Electrotherapeutic Apparatus
4922	10	Natural Gas Transmission
7374	10	Computer Processing and Data Preparation and Processing Services
7389	8	Business Services, Not Elsewhere Classified
2911	6	Petroleum Refining
8731	6	Commercial Physical and Biological Research

Note: The table outlines four-digit SIC industries with the highest number of high-technology IPOs in the sample. The original list for the definition of high-technology industry is in NAICS Codes(National Science Foundation, 2020).

Table 3: Definition of Local Neighborhood Characteristics

Variable	Description
College	Share of four-year college graduates in total population
Poverty	Share of people in poverty in total population
Unemployed	Share of unemployed people in total population
Asian Hispanic Black White	Share of people with the specific race in total population
Age under 19 Age 20 to 44 Age 45 to 64	People in the age group
Rental	Share of rental housing units in total units
Vacant	Share of vacant housing units in total units
Multiple	Share of housing units with multi-structure in total units
Ten-years	Share of household heads moving into units less than 10 years

Notes: The table includes definition of covariates in the difference-in-difference specification. Variables are calculated from the ACS Data at the census tract level, and are in percentage of the population. Demographic characteristics, such as college graduates and unemployed people, are divided by the total number of residents in the census tract. Housing units is divided by the total number of housing in the census tract. All variables are winsorized at 1% and 99% level.

Table 4: Summary Statistics for IPO Firms and Neighborhoods

	Mean	Median	Min	Max	S.D.
Panel A Firms					
IPO Price	16.35	16.00	5.85	44.00	5.91
IPO Proceeding	389.92	132.00	3.87	17 864.00	1469.61
Current Assets	422.63	148.46	0.14	11 267.00	1159.90
Total Assets	1746.18	315.39	100.17	138 898.00	8029.29
Liability	1032.99	114.44	1.09	101 739.00	5590.30
Revenue	1107.69	191.81	0.00	135 592.00	7071.10
EBIT	63.47	10.89	−3485.58	5955.00	409.71
Net Income	6.05	1.48	−3445.07	6172.00	392.21
Panel B Neighborhoods					
High-skilled Wage	49021.64	47299.07	10530.08	101987.49	16970.84
Low-skilled Wage	29028.13	28109.61	11152.84	56046.71	8558.45
High-skilled Employment	542.24	384.00	0.00	2388.44	493.62
Low-skilled Employment	1055.37	982.88	12.00	2838.00	581.90
Housing Rent	914.80	819.00	27.12	5920.85	410.21
House Price Index	235.73	203.93	95.63	791.08	119.83
White	0.66	0.76	0.01	0.98	0.30
Black	0.14	0.04	0.00	0.97	0.22
Asian	0.05	0.02	0.00	0.49	0.08
Hispanic	0.14	0.06	0.00	0.92	0.20
Age 19 Under	0.26	0.26	0.06	0.44	0.07
Age 20 to 44	0.34	0.33	0.16	0.66	0.09
Age 45 to 64	0.25	0.26	0.07	0.39	0.06
Age 65 Up	0.14	0.13	0.01	0.40	0.07
College	0.27	0.22	0.03	0.79	0.18
Unemployment	0.08	0.06	0.01	0.28	0.05
Poverty	0.15	0.12	0.01	0.55	0.12
Rental	0.35	0.29	0.03	0.98	0.23
Vacant	0.11	0.08	0.00	0.53	0.10
Multiple	0.26	0.17	0.00	0.98	0.26
Ten-years	0.37	0.35	0.01	0.84	0.18

Notes: The table presents summary statistics of financial position of high-technology IPO firms in the year of IPO and neighborhood characteristics. In Panel A, data come from Compustat and Audit Analytic. All variables except for the IPO Price are in million dollars. In Panel B, data are from tract-level ACS data and FHFA for House Price Index. Observations in different years are collapsed together. Wages, housing values and rents are adjusted to 2010 dollars by GDP. Other variables measure the share of race, age group and type of housing in total population or housing units. All variables are winsorized at 1% and 99% percentile.

Table 5: Neighborhood Characteristics by Outcome of IPOs

	Complete IPO (N=396)		Withdrawn IPO (N=118)		
	Mean	S.D.	Mean	S.D.	Diff.
Panel A Year 2000					
White	0.672	0.231	0.648	0.249	-0.024
Black	0.101	0.149	0.103	0.149	0.002
Asian	0.099	0.100	0.095	0.108	-0.005
Hispanic	0.107	0.136	0.135	0.163	0.027*
Age 19 Under	0.221	0.098	0.241	0.095	0.020**
Age 20 to 44	0.445	0.118	0.437	0.112	-0.007
Age 45 to 64	0.219	0.060	0.209	0.060	-0.010
Age 65 Up	0.112	0.069	0.108	0.067	-0.004
College	0.437	0.207	0.365	0.194	-0.072***
Unemployment	0.052	0.050	0.057	0.059	0.005
Poverty	0.114	0.115	0.127	0.126	0.013
Rental	0.493	0.283	0.476	0.279	-0.017
Vacant	0.071	0.069	0.071	0.076	0.000
Multiple	0.494	0.336	0.469	0.319	-0.025
Ten-years	0.719	0.121	0.710	0.123	-0.008
Panel B Year of IPO					
White	0.622	0.229	0.607	0.235	-0.015
Black	0.100	0.134	0.104	0.140	0.004
Asian	0.131	0.113	0.122	0.123	-0.009
Hispanic	0.132	0.149	0.149	0.161	0.017
Age 19 Under	0.207	0.090	0.222	0.096	0.015
Age 20 to 44	0.421	0.137	0.418	0.124	-0.003
Age 45 to 64	0.243	0.070	0.243	0.068	0.000
Age 65 Up	0.123	0.071	0.113	0.070	-0.010
College	0.503	0.210	0.444	0.199	-0.059**
Unemployment	0.060	0.042	0.066	0.043	0.006
Poverty	0.122	0.108	0.123	0.112	0.002
Rental	0.506	0.273	0.463	0.264	-0.043
Vacant	0.096	0.078	0.104	0.102	0.008
Multiple	0.516	0.330	0.471	0.325	-0.045
Ten-years	0.289	0.160	0.331	0.147	0.042**

Notes: The table presents summary statistics of neighborhood characteristics by the outcome of IPOs. Panel A draws data from the 2000 Census, which is surveyed prior to all IPO events. Data for Panel B are from the year of IPO. On average, there is no significant difference between neighborhoods with withdrawn IPOs and with complete IPOs, implying that correlation between outcome of high-technology IPOs and local economy is not a primary concern.

Table 6: Estimation on Market Outcomes by Using Withdrawn Issuers as Counterfactual

	Log (High Skilled Wage)				Log (Low Skilled Wage)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0151***	0.0118***	0.0192***	0.0150***	0.0051***	0.0016	0.0065***	0.0020
	(0.0013)	(0.0011)	(0.0017)	(0.0014)	(0.0016)	(0.0012)	(0.0020)	(0.0015)
Treat	-0.0066***	-0.0051***			-0.0022***	-0.0007		
	(0.0006)	(0.0005)			(0.0007)	(0.0005)		
Covariates		✓		✓		✓		✓
Observations	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853
R^2	0.78163	0.78535	0.78166	0.78537	0.70304	0.71621	0.70305	0.71621
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Distance-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

	Log (High Skilled Employment)				Log (Low Skilled Employment)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0076***	-0.0016	0.0097***	-0.0020	-0.0112***	-0.0140***	-0.0142***	-0.0179***
	(0.0028)	(0.0022)	(0.0035)	(0.0028)	(0.0024)	(0.0018)	(0.0031)	(0.0022)
Treat	-0.0033***	0.0007			0.0049***	0.0062***		
	(0.0012)	(0.0009)			(0.0011)	(0.0008)		
Covariates		✓		✓		✓		✓
Observations	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853
R^2	0.91605	0.93362	0.91605	0.93362	0.91585	0.93168	0.91586	0.93169
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Distance-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

	Log (Wage Premium)				Log (Relative Labor Supply)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0106***	0.0108***	0.0135***	0.0138***	0.0157***	0.0094***	0.0199***	0.0120***
	(0.0015)	(0.0014)	(0.0019)	(0.0018)	(0.0031)	(0.0017)	(0.0039)	(0.0022)
Treat	-0.0046***	-0.0047***			-0.0069***	-0.0041***		
	(0.0007)	(0.0006)			(0.0014)	(0.0008)		
Covariates		✓		✓		✓		✓
Observations	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853
R^2	0.52854	0.53220	0.52856	0.53222	0.93766	0.95971	0.93766	0.95971
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Distance-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

	Log (House Price Index)				Log (Housing Rent)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0219***	0.0196***	0.0278***	0.0250***	0.0068***	0.0044***	0.0086***	0.0056***
	(0.0022)	(0.0018)	(0.0027)	(0.0023)	(0.0014)	(0.0012)	(0.0018)	(0.0015)
Treat	-0.0096***	-0.0086***			-0.0030***	-0.0019***		
	(0.0010)	(0.0008)			(0.0006)	(0.0005)		
Covariates		✓		✓		✓		✓
Observations	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853
R^2	0.94909	0.95025	0.94911	0.95027	0.83384	0.83825	0.83385	0.83825
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Distance-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Sample consists of tract-level ACS 5-year data on workers by their educational attainment and 396 high-technology IPO firms. All observations are collapsed into a single panel for estimation. The coefficient of *TreatXpost* identifies treatment effect of IPO on welfare outcomes. Wage premium is the ratio of high-skilled wage by low-skilled wage, and relative supply is the ratio of high-skilled employment by low-skilled employment. Definition of covariates are same as before. I provide the coefficients of covariates in Appendix. All specifications include IPO case-distance-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replaces census tract fixed effect with case-tract fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on distance to closest headquarter of withdrawn issuers. Standard errors are clustered at the IPO case level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 7: Estimation of Dynamic Effect with Withdrawn IPO

	Log (High Skilled Wage) (1)	Log (Low Skilled Wage) (2)	Log (High Skilled Employment) (3)	Log (Low Skilled Employment) (4)	Log (House Price Index) (5)	Log (Housing Rent) (6)
Treat X Period=-3	-0.0003	0.0009	-0.0012	-0.0005	0.0003	3.08×10^{-5}
	(0.0010)	(0.0010)	(0.0017)	(0.0016)	(0.0018)	(0.0009)
Treat X Period=-2	0.0008	0.0013	-0.0020	-0.0020	0.0017	0.0006
	(0.0010)	(0.0009)	(0.0017)	(0.0017)	(0.0018)	(0.0009)
Treat X Period=0	0.0039^{***}	0.0014	-0.0033^{*}	-0.0057^{***}	0.0074^{***}	0.0030^{***}
	(0.0011)	(0.0009)	(0.0019)	(0.0017)	(0.0022)	(0.0011)
Treat X Period=1	0.0040^{***}	0.0008	-0.0037^{*}	-0.0068^{***}	0.0074^{***}	0.0032^{***}
	(0.0012)	(0.0010)	(0.0020)	(0.0017)	(0.0021)	(0.0011)
Treat X Period=2	0.0049^{***}	0.0003	-0.0047^{**}	-0.0080^{***}	0.0071^{***}	0.0026^{**}
	(0.0011)	(0.0010)	(0.0019)	(0.0018)	(0.0021)	(0.0012)
Treat X Period=3	0.0052^{***}	0.0002	-0.0058^{***}	-0.0090^{***}	0.0073^{***}	0.0044^{***}
	(0.0012)	(0.0011)	(0.0021)	(0.0018)	(0.0024)	(0.0013)
Treat X Period=4	0.0063^{***}	0.0011	-0.0059^{***}	-0.0105^{***}	0.0078^{***}	0.0051^{***}
	(0.0012)	(0.0011)	(0.0021)	(0.0019)	(0.0025)	(0.0013)
Treat X Period=5	0.0064^{***}	0.0004	-0.0051^{**}	-0.0096^{***}	0.0102^{***}	0.0043^{***}
	(0.0013)	(0.0012)	(0.0023)	(0.0019)	(0.0026)	(0.0013)
Treat X Period=6	0.0072^{***}	0.0003	-0.0051^{**}	-0.0102^{***}	0.0104^{***}	0.0040^{***}
	(0.0014)	(0.0014)	(0.0026)	(0.0019)	(0.0028)	(0.0015)
Treat	-0.0017^{***}	-0.0003	0.0016^{**}	0.0027^{***}	-0.0026^{***}	-0.0012^{***}
	(0.0004)	(0.0003)	(0.0007)	(0.0006)	(0.0007)	(0.0004)
Covariates	✓	✓	✓	✓	✓	✓
Observations	4,848,277	4,848,277	4,848,277	4,848,277	4,848,277	4,848,277
R^2	0.78704	0.72059	0.92942	0.92734	0.95343	0.84354
Tract fixed effects	✓	✓	✓	✓	✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓
Case-Distance-Year fixed effects	✓	✓	✓	✓	✓	✓

Notes: The table presents estimation result on dynamic effect of high-technology IPOs. *Treat* is the dummy for indicating census tracts belong to the treatment group. The effect on $Period = -1$ is normalized to zero. Critical values and confidence intervals are calculated by the simultaneous method by Montiel Olea and Plagborg-Møller (2019) to account for serial correlation. Definition of covariates follows Table 3. I provide the coefficients of covariates in Appendix. All specifications include census tract fixed effect, IPO case-distance-year fixed effect and county-year fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on distance to the closest withdrawn issuer. Standard errors are clustered at the IPO case level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 8: Estimation on Market Outcomes by Using Withdrawn Issuers as Counterfactual

Panel A: Same Industry	Patents		Log (Patents)		Economic Value		Log (Economic Value)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	13.51**	12.80**	0.1887***	0.1768***	569.6***	547.8***	0.3100***	0.2885***
	(5.393)	(5.184)	(0.0631)	(0.0612)	(200.1)	(200.4)	(0.0979)	(0.0970)
Treat	-0.4238	-0.1277	0.0123	0.0165	-117.6	-104.4	0.0209	0.0273
	(4.673)	(4.673)	(0.0413)	(0.0416)	(146.0)	(145.7)	(0.0668)	(0.0669)
Covariates		✓		✓		✓		✓
Observations	61,688	61,688	61,688	61,688	61,688	61,688	61,688	61,688
R^2	0.81145	0.81407	0.87261	0.87454	0.75608	0.76062	0.87690	0.87866
Within R^2	0.00345	0.01728	0.29643	0.30712	0.00355	0.02211	0.34492	0.35427
Tract fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Distance-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Panel B: Different Industry	Patents		Log (Patents)		Economic Value		Log (Economic Value)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	-0.6152	-0.3365	-0.0050	-0.0044	-19.29	-39.94	-0.0344	-0.0240
	(1.612)	(1.626)	(0.0240)	(0.0241)	(59.33)	(60.18)	(0.0371)	(0.0373)
Treat	-2.492	-2.583	-0.0370*	-0.0372*	-28.87	-23.21	-0.0341	-0.0376
	(1.933)	(1.915)	(0.0209)	(0.0208)	(54.65)	(54.57)	(0.0294)	(0.0294)
Covariates		✓		✓		✓		✓
Observations	160,409	160,409	160,409	160,409	160,409	160,409	160,409	160,409
R^2	0.73898	0.74058	0.82294	0.82373	0.69162	0.69314	0.82651	0.82747
Tract fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Distance-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The tables show the treatment effect of high-technology IPOs on local patenting activities by incumbent firms. The coefficient of *TreatXpost* identifies treatment effect of IPO on patent outcomes. Definition of covariates are same as before. All specifications include IPO case-distance-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replaces census tract fixed effect with case-tract fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on distance to closest headquarter of withdrawn issuers. Standard errors are clustered at the IPO case level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 9: Mean Test of Difference in Mean Utility

	High (N=5486272)		Low (N=5486272)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
Full Sample	0.0026	0.4186	-0.0091	0.4092	0.0117***	0.0000
< 15 Miles Only	0.0173	0.4116	-0.0033	0.3980	0.0206***	0.0000

Notes: The table compares the changes in the mean utility of high-skilled workers and low-skilled workers in each ZIP code neighborhood. The first row includes samples from all neighborhoods within 30 miles distance to high-technology IPO headquarters, while the second line includes only neighborhoods within 15 miles, which consist of the treatment group in the difference-in-difference specification. There is strong evidence that there is a net increase in the utility of high-skilled workers but a net decrease for low-skilled workers. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 10: Estimation of Gravity Equation

	$log(\pi^L)$		$log(\pi^H)$	
	(1)	(2)	(3)	(4)
ξ^s	-0.0908***	-0.0930***	-0.0908***	-0.0929***
	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Observations	4,734,754	4,734,754	4,734,710	4,734,710
R^2	0.88799	0.88072	0.88782	0.88075
Home-Period FE	✓	✓	✓	✓
Work-Period FE	✓	✓	✓	✓
Case-Period FE	✓		✓	

Notes: The table presents the estimation of the semi-elasticity of the probability of commuting on commuting distance. The gravity equation (23) is augmented by Case-Period fixed effect and has standard error clustered on the IPO Zone level. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table 11: Estimation of Structural Parameters

	High-skilled (1)	Low-skilled (2)	High-skilled (3)	Low-skilled (4)
Real wage elasticity ($\beta^s = \frac{1}{\zeta^s}$)	3.712***	3.428***	3.721***	3.466***
	(1.406)	(1.149)	(1.411)	(1.165)
Preference on amenities (η^s)	1.163***	0.1507	1.157***	0.1190
	(0.1526)	(0.1418)	(0.1507)	(0.1312)
Spillover effect on productivity (λ_0)	0.1965***		0.2006***	
	(0.0061)		(0.0063)	
Spillover effect on productivity (λ_1)	-0.0008***		-0.0008***	
	(0.0003)		(0.0003)	
Calibrated Parameters				
Share of spending on local goods (θ^s) (Diamond (2016) and Moretti (2013))	0.63	0.68	0.62	
Elasticity of substitution of skills (σ) (Katz and Murphy, 1992)	1.4		1.4	

Notes: Estimation includes sample of high-technology IPOs from 2005 to 2010 and ZIP codes within each IPO zone. Real wage elasticity and preference on amenities are identified by shift-share IV on wages using 1900 as the base year, conditional on IPO case fixed effect. Standard errors are clustered at the IPO case level. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table 12: Estimation on Outcomes of Labor and Housing Markets

	Log (High Skilled Wage)				Log (Low Skilled Wage)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0116***	0.0088***	0.0146***	0.0111***	0.0052***	0.0022**	0.0065***	0.0027**
	(0.0012)	(0.0010)	(0.0016)	(0.0013)	(0.0013)	(0.0010)	(0.0017)	(0.0013)
Treat	-0.0050***	-0.0038***			-0.0023***	-0.0010**		
	(0.0006)	(0.0005)			(0.0006)	(0.0004)		
Covariates		✓		✓		✓		✓
Observations	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853
R^2	0.78398	0.78807	0.78400	0.78808	0.70965	0.72313	0.70965	0.72313
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

	Log (High Skilled Employment)				Log (Low Skilled Employment)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	-0.0018	-0.0085***	-0.0023	-0.0107***	-0.0136***	-0.0145***	-0.0172***	-0.0182***
	(0.0027)	(0.0022)	(0.0035)	(0.0028)	(0.0022)	(0.0018)	(0.0028)	(0.0023)
Treat	0.0008	0.0037***			0.0060***	0.0063***		
	(0.0012)	(0.0010)			(0.0010)	(0.0008)		
Covariates		✓		✓		✓		✓
Observations	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853
R^2	0.91084	0.92989	0.91084	0.92989	0.91203	0.92740	0.91204	0.92740
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

	Log (Wage Premium)				Log (Relative Labor Supply)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0073***	0.0076***	0.0092***	0.0096***	0.0097***	0.0039***	0.0122***	0.0050***
	(0.0012)	(0.0011)	(0.0015)	(0.0014)	(0.0027)	(0.0015)	(0.0034)	(0.0019)
Treat	-0.0032***	-0.0033***			-0.0042***	-0.0017**		
	(0.0005)	(0.0005)			(0.0012)	(0.0007)		
Covariates		✓		✓		✓		✓
Observations	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853
R^2	0.53151	0.53504	0.53152	0.53505	0.93584	0.95929	0.93584	0.95929
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

	Log (House Price Index)				Log (Housing Rent)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0178***	0.0161***	0.0225***	0.0203***	0.0086***	0.0071***	0.0108***	0.0090***
	(0.0018)	(0.0016)	(0.0022)	(0.0020)	(0.0012)	(0.0011)	(0.0015)	(0.0014)
Treat	-0.0078***	-0.0070***			-0.0037***	-0.0031***		
	(0.0008)	(0.0007)			(0.0005)	(0.0005)		
Covariates		✓		✓		✓		✓
Observations	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853
R^2	0.95470	0.95557	0.95472	0.95558	0.84438	0.84891	0.84439	0.84891
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Sample consists of tract-level ACS 5-year data on workers by their educational attainment and 396 high-technology IPO firms. All observations are collapsed into a single panel for estimation. The coefficient of *TreatXpost* identifies treatment effect of IPO on welfare outcomes. Wage premium is the ratio of high-skilled wage by low-skilled wage, and relative supply is the ratio of high-skilled employment by low-skilled employment. Definition of covariates are same as before. I provide the coefficients of covariates in Appendix. All specifications include IPO case-pscore-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replace census tract fixed effect with case-tract fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on prediction of propensity score model. Standard errors are clustered at the IPO case level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 13: Triple DiD Estimation on Outcomes of Labor and Housing Markets

	Log (High Skilled Wage) (1)	(2)	Log (Low Skilled Wage) (3)	(4)
TreatXsuccessfulXpost	0.0084***	0.0073***	-0.0008	-0.0025
	(0.0028)	(0.0026)	(0.0023)	(0.0018)
TreatXpost	0.0052**	0.0028	0.0049***	0.0033**
	(0.0024)	(0.0023)	(0.0018)	(0.0013)
Covariates	✓	✓	✓	✓
Observations	6,131,451	6,131,451	6,131,451	6,131,451
R^2	0.81196	0.81563	0.75030	0.76267
Case-Tract fixed effects	✓	✓	✓	✓
County-Year fixed effects	✓	✓	✓	✓
Case-Year fixed effects	✓	✓	✓	✓

	Log (High Skilled Employment) (1)	(2)	Log (Low Skilled Employment) (3)	(4)
TreatXsuccessfulXpost	-0.0064	-0.0079*	-0.0121***	-0.0139***
	(0.0049)	(0.0044)	(0.0038)	(0.0030)
TreatXpost	0.0060	0.0004	-0.0022	-0.0013
	(0.0038)	(0.0036)	(0.0028)	(0.0021)
Covariates	✓	✓	✓	✓
Observations	6,131,451	6,131,451	6,131,451	6,131,451
R^2	0.92161	0.93756	0.91922	0.93305
Case-Tract fixed effects	✓	✓	✓	✓
County-Year fixed effects	✓	✓	✓	✓
Case-Year fixed effects	✓	✓	✓	✓

	Log (Wage Premium) (1)	(2)	Log (Relative Labor Supply) (3)	(4)
TreatXsuccessfulXpost	0.0093***	0.0098***	0.0047	0.0051
	(0.0030)	(0.0029)	(0.0046)	(0.0033)
TreatXpost	0.0008	0.0002	0.0063*	-0.0002
	(0.0026)	(0.0026)	(0.0033)	(0.0028)
Covariates	✓	✓	✓	✓
Observations	6,131,451	6,131,451	6,131,451	6,131,451
R^2	0.60482	0.60797	0.94327	0.96331
Case-Tract fixed effects	✓	✓	✓	✓
County-Year fixed effects	✓	✓	✓	✓
Case-Year fixed effects	✓	✓	✓	✓

	Log (House Price Index) (1)	(2)	Log (Housing Rent) (3)	(4)
TreatXsuccessfulXpost	0.0197***	0.0185***	0.0107***	0.0098***
	(0.0032)	(0.0032)	(0.0021)	(0.0021)
TreatXpost	-0.0007	-0.0017	-0.0019	-0.0029*
	(0.0024)	(0.0025)	(0.0016)	(0.0016)
Covariates	✓	✓	✓	✓
Observations	6,131,451	6,131,451	6,131,451	6,131,451
R^2	0.95361	0.95477	0.85956	0.86387
Case-Tract fixed effects	✓	✓	✓	✓
County-Year fixed effects	✓	✓	✓	✓
Case-Year fixed effects	✓	✓	✓	✓

Notes: Sample consists of tract-level ACS 5-year data on workers by their educational attainment and 396 high-technology IPO firms. All observations are collapsed into a single panel for estimation. The coefficient of *TreatXsuccessfulXpost* identifies treatment effect of IPO on welfare outcomes. Wage premium is the ratio of high-skilled wage by low-skilled wage, and relative supply is the ratio of high-skilled employment by low-skilled employment. Definition of covariates are same as before. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. Standard errors are clustered at the IPO case level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 14: Estimation on Outcomes for Non-metropolitan IPOs only

	Log (High Skilled Wage)				Log (Low Skilled Wage)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0104***	0.0068***	0.0129***	0.0085***	0.0069***	0.0025**	0.0086***	0.0031**
	(0.0016)	(0.0013)	(0.0020)	(0.0016)	(0.0015)	(0.0012)	(0.0018)	(0.0014)
Treat	-0.0044***	-0.0029***			-0.0030***	-0.0011**		
	(0.0007)	(0.0006)			(0.0006)	(0.0005)		
Covariates		✓		✓		✓		✓
Observations	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996
R^2	0.77467	0.77904	0.77469	0.77904	0.72577	0.73968	0.72578	0.73968
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

	Log (High Skilled Employment)				Log (Low Skilled Employment)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	-0.0009	-0.0070**	-0.0010	-0.0086**	-0.0180***	-0.0163***	-0.0222***	-0.0202***
	(0.0038)	(0.0030)	(0.0047)	(0.0037)	(0.0024)	(0.0020)	(0.0029)	(0.0025)
Treat	0.0004	0.0030**			0.0077***	0.0070***		
	(0.0016)	(0.0013)			(0.0010)	(0.0009)		
Covariates		✓		✓		✓		✓
Observations	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996
R^2	0.90708	0.92608	0.90708	0.92608	0.90779	0.92365	0.90780	0.92366
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

	Log (Wage Premium)				Log (Relative Labor Supply)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0042***	0.0050***	0.0052***	0.0062***	0.0141***	0.0068***	0.0175***	0.0084***
	(0.0013)	(0.0013)	(0.0017)	(0.0017)	(0.0033)	(0.0019)	(0.0040)	(0.0023)
Treat	-0.0018***	-0.0021***			-0.0060***	-0.0029***		
	(0.0006)	(0.0006)			(0.0014)	(0.0008)		
Observations	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996
R^2	0.52442	0.52779	0.52442	0.52780	0.93602	0.95932	0.93603	0.95932
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

	Log (House Price Index)				Log (Housing Rent)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0188***	0.0160***	0.0233***	0.0198***	0.0101***	0.0078***	0.0126***	0.0097***
	(0.0020)	(0.0018)	(0.0025)	(0.0022)	(0.0012)	(0.0012)	(0.0015)	(0.0014)
Treat	-0.0080***	-0.0068***			-0.0043***	-0.0033***		
	(0.0009)	(0.0008)			(0.0005)	(0.0005)		
Covariates		✓		✓		✓		✓
Observations	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996	3,500,996
R^2	0.95691	0.95784	0.95692	0.95785	0.85109	0.85645	0.85110	0.85645
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Sample consists of tract-level ACS 5-year data on workers by their educational attainment and 396 high-technology IPO firms. All observations are collapsed into a single panel for estimation. The coefficient of *TreatXpost* identifies treatment effect of IPO on welfare outcomes. Wage premium is the ratio of high-skilled wage by low-skilled wage, and relative supply is the ratio of high-skilled employment by low-skilled employment. Definition of covariates are same as before. I provide the coefficients of covariates in Appendix. All specifications include IPO case-pscore-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replace census tract fixed effect with case-tract fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on prediction of propensity score model. Standard errors are clustered at the IPO case level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 15: Estimation of First-time Treatment on Outcomes of Labor and Housing Markets

	Log (High Skilled Wage) (1)	(2)	Log (Low Skilled Wage) (3)	(4)
TreatXpost	0.0258***	0.0207***	0.0116*	0.0056
	(0.0055)	(0.0047)	(0.0069)	(0.0061)
Covariates		✓		✓
Observations	536,817	536,817	536,817	536,817
R^2	0.76469	0.76877	0.73683	0.75073
Tract fixed effects	✓	✓	✓	✓
County-Year fixed effects	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓

	Log (High Skilled Emp.) (1)	(2)	Log (Low Skilled Emp.) (3)	(4)
TreatXpost	-0.0018	-0.0263*	-0.0453***	-0.0430***
	(0.0148)	(0.0139)	(0.0130)	(0.0105)
Covariates		✓		✓
Observations	536,817	536,817	536,817	536,817
R^2	0.89942	0.92137	0.90288	0.91830
Tract fixed effects	✓	✓	✓	✓
County-Year fixed effects	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓

	Log (Wage Premium) (1)	(2)	Log (Relative Labor Supply) (3)	(4)
TreatXpost	0.0154**	0.0166**	0.0413***	0.0146*
	(0.0067)	(0.0064)	(0.0125)	(0.0088)
Covariates		✓		✓
Observations	536,817	536,817	536,817	536,817
R^2	0.52837	0.53162	0.92888	0.95525
Tract fixed effects	✓	✓	✓	✓
County-Year fixed effects	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓

	Log (House Price Index) (1)	(2)	Log (Housing Rent) (3)	(4)
TreatXpost	0.0417***	0.0385***	0.0158**	0.0133**
	(0.0068)	(0.0055)	(0.0067)	(0.0065)
Covariates		✓		✓
Observations	536,817	536,817	536,817	536,817
R^2	0.96053	0.96151	0.85525	0.86046
Tract fixed effects	✓	✓	✓	✓
County-Year fixed effects	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓

Notes: Sample consists of tract-level ACS 5-year data on workers by their educational attainment. For each census tract, only the first treatment by IPO is considered. The coefficient of *TreatXpost* identifies treatment effect of IPO on welfare outcomes. Wage premium is the ratio of high-skilled wage by low-skilled wage, and relative supply is the ratio of high-skilled employment by low-skilled employment.. Definition of covariates are same as before. I provide the coefficients of covariates in Appendix. All specifications include Tract fixed effect, IPO case-pscore-year fixed effect and county-year fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on prediction of propensity score model. Standard errors are clustered at the IPO case level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

A Internet Appendix: Data Imputation for Structural Estimation

A.1 Imputation for Commuting Patterns

In the structural model, the first step is to estimate the mean utility for each pair of neighbourhoods from the commuting pattern of workers. Hence, the actual share of workers commuting between each pair of neighbourhoods should be calculated from observed data. Therefore, I start with calculating π^s_{ijt} as the actual share of workers with skill s living in i and working in j in time t i.e.

$$\pi^s_{ijt} = \frac{n^s_{ijt}}{\sum_{i'} \sum_{j'} n^s_{i'j't}} \tag{27}$$

Hence, it just needs to know n^s_{ijt} as the number of workers with specified commuting pattern. Due to data availability, the measure is not directly observed, so I predict it by the method in Qian and Tan (2021). First, it can be decomposed into two parts

$$n^s_{ijt} = n_{ijt} p^s_{ijt} \tag{28}$$

n_{ijt} is the total commuting flow from i to j, and p^s_{ijt} is the share of workers with skill s in the flow. The LEHD Origin-Destination Employment Statistics (LODES) datasets record the commuting flow of workers from one census block to another. They are available for most states during 2002 - 2015, so n_{ijt} can be directly observed. However, as the dataset does not disclose educational attainment of workers, p^s_{ijt} is unobserved. To overcome the challenge, I complement the data with 2009 NHTS, which is a travel survey on individual level including the education of participants. I run a LASSO model using the 2009 NHTS as the training sample, and predict them based on characteristics from census, RAC and WAC data. Finally, I can calculate π^s_{ijt} for each (i, j).

A.2 Construction for Workplace Wage and IV

Estimation of Equation (17) requires variables on the right-hand sides to be observed. However, the census data include wages of residents in one area, but provide no information on the wage of workers who work in the area. To address the limitation, the workplace wage is imputed by the weighted average of the residential wage

$$w^H_{jt} = log(W^H_{jt}) = log\left(\frac{\sum_i H_{ijt} W^H_{it}}{\sum_i H_{ijt}}\right) \tag{29}$$

$$w_{jt}^L = log(W_{jt}^L) = log\left(\frac{\sum_i L_{ijt} W_{it}^L}{\sum_i L_{ijt}}\right) \tag{30}$$

Second, the shift-share IV for log wage based on 1990 is constructed by the weight average of industry wage

$$\Delta B_{jt\leftarrow 1990}^H = \sum_{ind} \left(w_{ind,t}^H - w_{ind,1990}^H\right) \frac{H_{ind,j,1990}}{H_{j,1990}} \tag{31}$$

$$\Delta B_{jt\leftarrow 1990}^L = \sum_{ind} \left(w_{ind,t}^L - w_{ind,1990}^L\right) \frac{L_{ind,j,1990}}{L_{j,1990}} \tag{32}$$

where $w_{ind,t}^s$ represents for the average log wage of workers with skill s in industry ind in year t. $H(L)_{ind,j,1990}$ measures the number of high-skilled (low-skilled) people working in ZIP code j in industry ind and in 1990, while $H(L)_{j,1990}$ is the total number of high-skilled (low-skilled) workers in ZIP code j in 1990.

By its design, the shift-share IV links with contemporaneous real wages by the "shift" part, and thus satisfies the relevance condition. Meanwhile, it is able to identifies shift of labor demand by its industry-level weight average part. For example, since 1990 the reduction in communication cost leaded to boom in the financial services sector, so we shall see greater rising wage in neighborhoods in which employees of financial services sector concentrate. Furthermore, the underlying assumption for exclusion restriction is that geographical distribution of industry in 1990 does not drive residualized amenity changes. To my best knowledge, there is no such evidence in pointing the correlation.

Besides the main data sources, I merge them with a sample of 1990 census data to calculate employment share in 1990. The sample covers 5% U.S. population and information on workers' educational attainment and industry. I crosswalk 1990 industry in census to ACS three-digit industry identifier, and use the latter as industry classification. The 1990 sample doesn't contain ZIP codes as geographical level, and the 1990 ACS data only provide employment in each industry but no information on educational attainment. I calculate the share of high-skilled workers in each industry on the county level by census sample, and then multiply employment in each industry on the ZIP code level by ACS in order to predict ($H_{ind,j,1990}$, $L_{ind,j,1990}$). Formally,

$$H_{ind,j,1990} \approx \underbrace{N_{ind,j,1990}}_{\text{ACS}} * \underbrace{\frac{H_{ind,c(j),1990}}{H_{ind,c(j),1990} + L_{ind,c(j),1990}}}_{\text{sample of 5\% population}} \tag{33}$$

$$L_{ind,j,1990} \approx \underbrace{N_{ind,j,1990}}_{\text{ACS}} * \underbrace{\frac{L_{ind,c(j),1990}}{H_{ind,c(j),1990} + L_{ind,c(j),1990}}}_{\text{sample of 5\% population}} \tag{34}$$

where $c(j)$ represents for the county containing ZIP code j.

By using the IV to estimate Equation (17), one can separate variation in real wages from unobserved amenity changes. Formally, the exclusion restriction for shift-share IV reads

$$\mathbb{E}[\mathbf{\Delta B_{jt\leftarrow 1990}} \times \mathbf{\Delta}\epsilon^{\mathbf{a}}_{\mathbf{ijt}}] = \mathbf{0} \tag{35}$$

with

$$\mathbf{\Delta B_{jt\leftarrow 1990}} := (\Delta B^{H}_{jt\leftarrow 1990}, \Delta B^{L}_{jt\leftarrow 1990})^{\intercal} \quad \mathbf{\Delta}\epsilon^{\mathbf{a}}_{\mathbf{ijt}} := (\Delta\epsilon^{a,H}_{ijt}, \Delta\epsilon^{a,L}_{ijt})$$

B Internet Appendix: Further Exploration of Treatment Effects

This section contains two parts and further explores the uniqueness and heterogeneity of treatment effects with an additional data source. First, I show that the gentrification effect uniquely links with high-technology firms, as there is no enlarging inequality when studying a different sample of non-high-technology IPOs. In the following section, I briefly discuss potential mechanisms for this difference. Second, I explore heterogeneous treatment effects based on local neighborhood characteristics to enrich the policy implications of the study.

B.1 Non-high-technology IPOs

After identifying the effect on inequality caused by high-technology IPOs, a natural extension is to examine whether the other half of the IPO universe generates similar impacts on the local economy. In contrast to high-technology IPOs, I do not observe that non-high-technology IPOs change the local wage premium or relative labor supply, finding only a similar increase in housing value and rent in nearby neighborhoods. Like high-technology IPOs, I collect a sample of IPO issuers whose SIC codes are not classified as high-technology by NSF. To reduce IPO Zones overlapping with high-technology IPOs, I restrict the sample to counties that did not host any high-technology issuers during 2003-2017. I select 107 cases of non-high-technology IPO firms. Table (B12) compares IPO proceedings and financial positions of selected firms and other non-high-technology firms, confirming their similarity in these aspects. This addresses concerns about selection bias. I construct a similar propensity score model using these non-high-technology IPOs as the outcome variable and then estimate the static DID. Interestingly, results sharply contrast with the effects of high-technology IPOs, as shown in Table (B13). First, I observe only a small positive effect on wages of low-skilled workers of around 0.47% log points, but virtually no effect on high-skilled workers. Second, there is no evidence of any employment changes related to these IPOs. As a result, the preexisting gap in wage and employment between differently skilled workers remains stable before and after IPO shocks. Finally, housing values and rents rise by 0.99% and 0.57% log points respectively. The magnitude is much smaller compared with the 1.78% and 0.71% log points observed for high-technology IPOs. Different specifications yield highly similar results.

The findings suggest that the effect spreads evenly across different workers, with low-skilled workers benefiting slightly more. Compared to high-technology issuers, while these firms' shock fuels the housing market, it does not appear to be skill-biased. One possible explanation lies in the personnel structure of firms. Consider a typical manufacturer: while assembly line workers are often less educated, administrative staff and firm managers more likely have college degrees and thus are classified as high-skilled. It is reasonable to expect that not only the issuer has such a personnel structure, but so do all similar local firms. Consequently, high-skilled and low-skilled workers share the productivity increase in each firm equally, leading to no aggregate effect on inequality. The analysis thus far investigates labor and housing market outcomes separately. As noted, the welfare implications are more complicated, given that both wages and local living costs have increased. Even though there is some displacement effect on low-skilled workers, this does not necessarily imply impaired welfare. For example, landowners can sell their appreciated property and voluntarily move away to enjoy a quieter life. In the next section, I model utility and structurally estimate it to confirm the heterogeneous effects of high-technology IPOs on different workers.

B.2 Heterogeneity Analysis on Treatment Effects

Until now, I have identified the average effect of high-technology IPOs. However, one can easily imagine that individual-specific effects vary across firm and local characteristics. Which types of high-technology IPOs benefit/hurt local residents most, and which types of local economies absorb the most significant impact of IPOs? These questions remind us to interpret the results cautiously, and their answers are closely related to local policymaking. In this final part, I present suggestive evidence on how the treatment effects of high-technology IPOs depend on different types of local economies. This analysis builds on comparative static analysis of theoretical models, such as Moretti (2011). First, I study the relationship between labor force agglomeration and IPO effects on local labor market outcomes. To measure agglomeration, I split samples into three bins based on the population density of the treatment group in each IPO Zone. Population density comes from the 2000 Census, and I use it as a cross-sectional variable. Next, I estimate equation (1) separately for cases in each bin. Due to fewer observations, year fixed effects only intersect with county fixed effects from then on. In Figure (A6) (a), I plot the DID estimate results.

One can observe that more positive effects on wages and employment coincide with each other in areas with higher population density. Therefore, agglomeration amplifies wage increases and mitigates employment displacement effects. This finding aligns with literature on agglomeration economics. Pioneered by Marshall (1890) and Fujita and Thisse (2002), researchers have identified knowledge spillover as a key component of economic growth. Furthermore, several studies find a positive relationship between population size and wages and productivity. Even today, substantial knowledge transfer occurs through face-to-face contacts, so more densely populated neighborhoods could facilitate this process (Head and Mayer (2004), Glaeser and Resseger (2010)). In such areas, high-technology IPOs serve as a multiplier in benefiting wages and productivity. As shown in the figure, the (log) wage increase of high-skilled workers in areas with the highest density is around 0.8Meanwhile, areas with higher population density attract more high-skilled workers through IPOs. The simultaneous increase in wages and employment of high-skilled workers aligns with findings in Moretti (2012), where he coins "the Great Convergence." In this setting, it is further fueled by local IPOs. One possible channel is through endogenous local amenities. Additionally, if skill premium can increase with average human capital levels because of skill complementarities, it would prompt high-skilled workers to migrate to richer communities (Giannetti, 2013). Another essential component affecting magnitude is local labor elasticity. Moretti (2011) assumes people work and live in the same city and provides theoretical results showing that the wage effect on high-skilled workers from a skill-biased demand shock is negatively related to their own labor elasticity, since firms' ability to recruit new workers is more constrained when labor supply is inelastic. In reality, however, since people commute between home and work locations, distinguishing between employment elasticity and resident elasticity is necessary. In this paper, I focus on employment elasticity's impact and confirm that IPO effects on wages and employment are more significant when local employment elasticity is high.

To construct employment elasticity, I use the same measure of commuting openness as in Monte et al. (2018). Specifically, the commuting openness of one local labor market is[28]

$$openness_i = \frac{\sum_{j \neq i} n_{ij}}{\sum_j n_{ij}} \tag{36}$$

It equals the share of workers not working in their residential place relative to the total number of workers who live there. A more open commuting market implies a lower likelihood that workers both work and live in the same place. Thus, the local employment elasticity is higher.

I calculate this measure at the IPO Zone level using 2000 ACS Data. Following the same estimation process as with population density, I study the impact on high-technology IPO effects. In Figure (A6) (b), neighborhoods with the highest commuting openness show the

[28]In the original paper, they construct the measure on commuting Zone and county level. I use the county-level ACS data but calculate the measure on the IPO Zone level. The process may include additional noise: consider two counties in the same IPO Zone, then the commuting flow between them should be considered as flow within the local market, but is included in the numerator when using county-level data. However, the problem is mitigated because I break samples into bins, rather than directly including the measure in regressions.

greatest effects on wages and employment. This corresponds with the model and empirical evidence from Monte et al. (2018). All else equal, greater commuting openness in local labor markets enables workers to adjust their location more quickly in response to demand shocks, thus increasing worker welfare.

Beyond demographic characteristics, housing values and rents can strongly correlate with local land elasticity. In studying heterogeneous treatment effects, I adopt the land availability measure from Saiz (2010) and the Wharton Residential Land Use Regulatory Index (WRLURI) developed by Gyourko et al. (2006) to measure land elasticity.[29] In Figure (A7), higher land supply elasticity correlates with larger treatment effects on housing prices and rents. This somewhat contradicts theory, as a higher portion of the effect should accrue to the sector with lower elasticity. If land supply is inelastic, then new housing construction is limited by land availability, so prices and rents should increase more. To interpret this result, I find that land availability is usually higher in underdeveloped areas versus metropolitan areas where housing prices and rents are already high prior to high-technology IPOs. Thus, the coefficient estimates may be smaller because the regression takes the logarithm of housing prices and rents as dependent variables. Moreover, there may be reverse causality as land availability is measured in 2006. Hence, these results should not be interpreted as causal. Clearly, more research is needed.

[29] I am aware of the new version of WRLURI as in Gyourko et al. (2019), but stick to the old version since it is less subject to reverse causality in this setting.

C Internet Appendix: Additional Figures

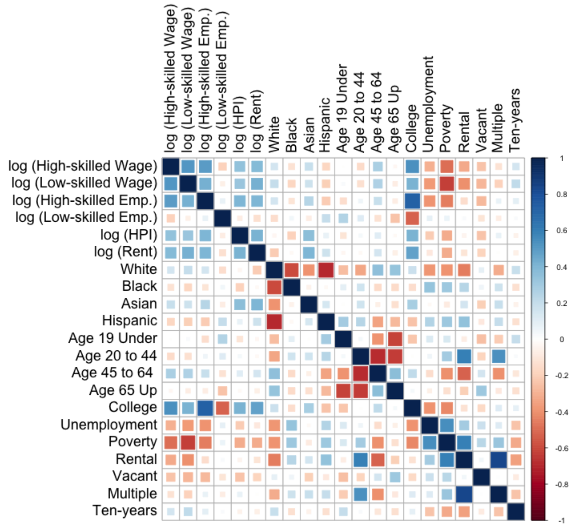
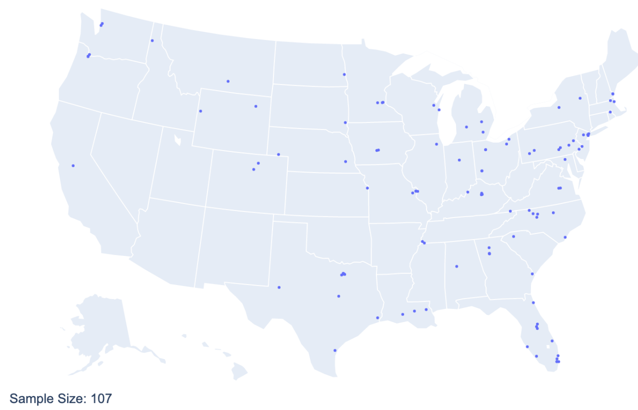
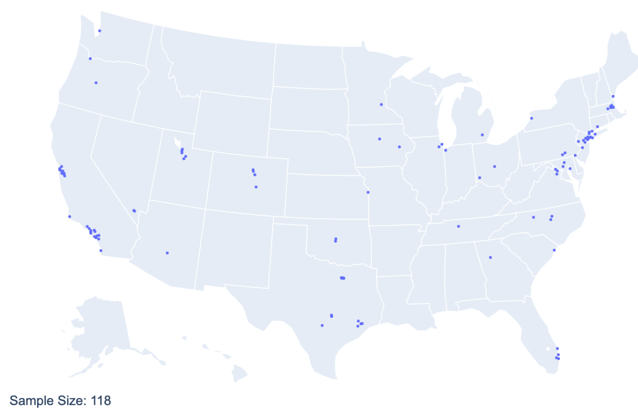


Figure A1: Correlation between Census Characteristics

Notes: The figure plots correlation coefficients between census characteristics. Observations from different years are collapsed into a single panel. Wages and Rents are adjusted to 2010 dollars. Definition of variables follows Table 3.



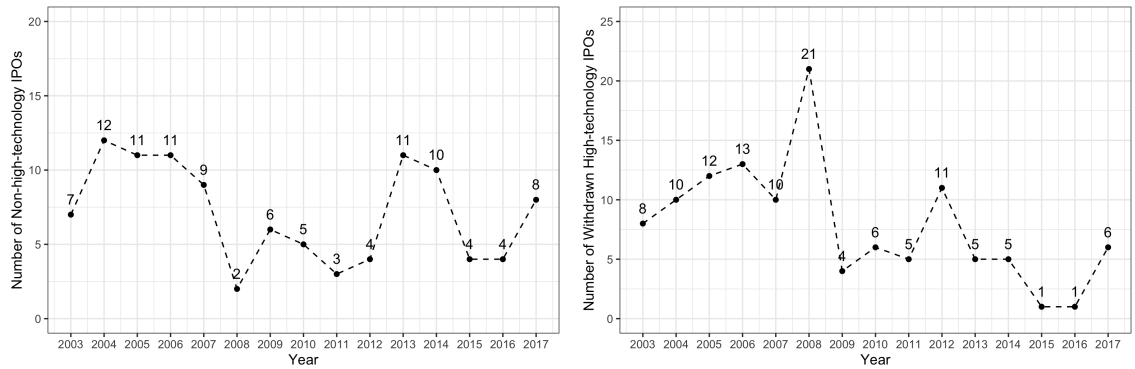
(a) Non-high-technology Firms



(b) Withdrawn High-technology IPO Issuers

Figure A2: Location of IPO Issuers

Notes: The figure plots headquarter location of non-high-technology IPO issuers and high-technology withdrawn high-technology IPO issuers. Data are from Audit Analytics and Thomson/Refinitiv. Non-high-technology IPO issuers are issuers whose SIC codes are not classified as high-technology by NSF, and are restricted to those located in counties that never host any high-technology IPOs during the sample period 2003 - 2017.



(a) Non-high-technology Firms (b) Withdrawn High-technology IPO Issuers

Figure A3: Number of IPOs by Year

Notes: The figure plots year of non-high-technology IPO issuers and high-technology withdrawn high-technology IPO issuers. Data are from Audit Analytics and Thomson/Refinitiv. Non-high-technology IPO issuers are issuers whose SIC codes are not classified as high-technology by NSF, and are restricted to those located in counties that never host any high-technology IPOs during the sample period 2003 - 2017.

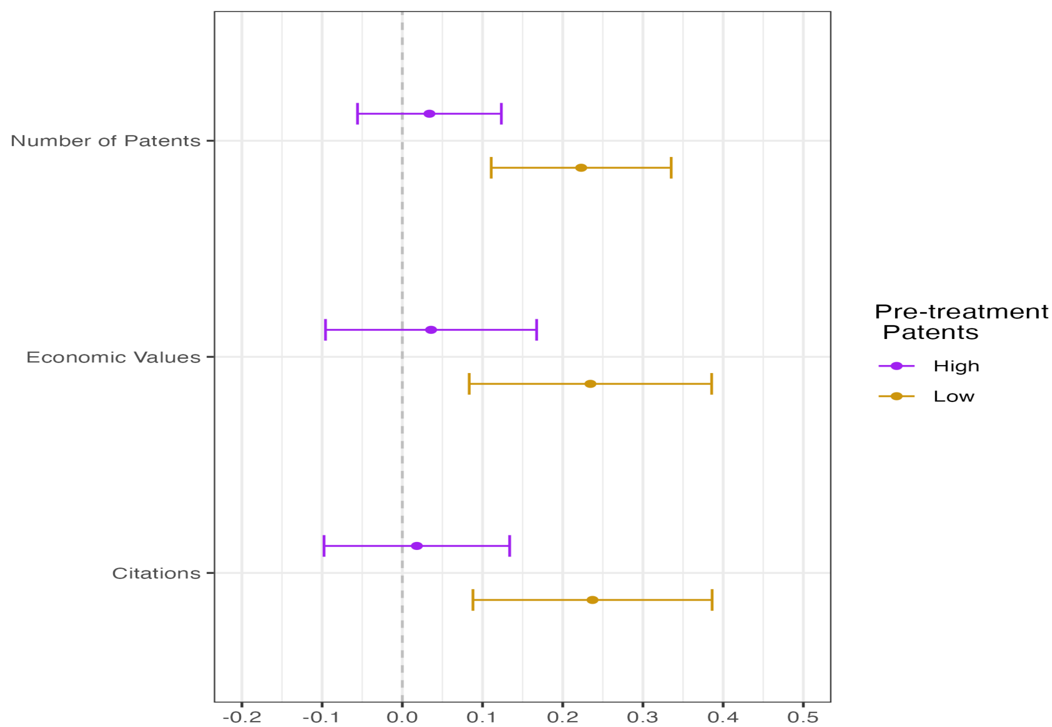
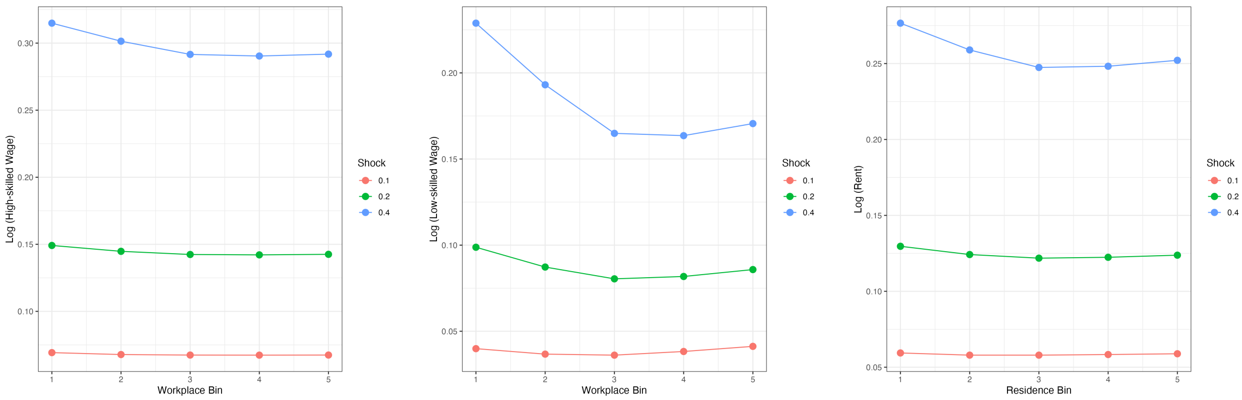


Figure A4: Heterogeneity of Patent Growth by Pre-treatment Productivity

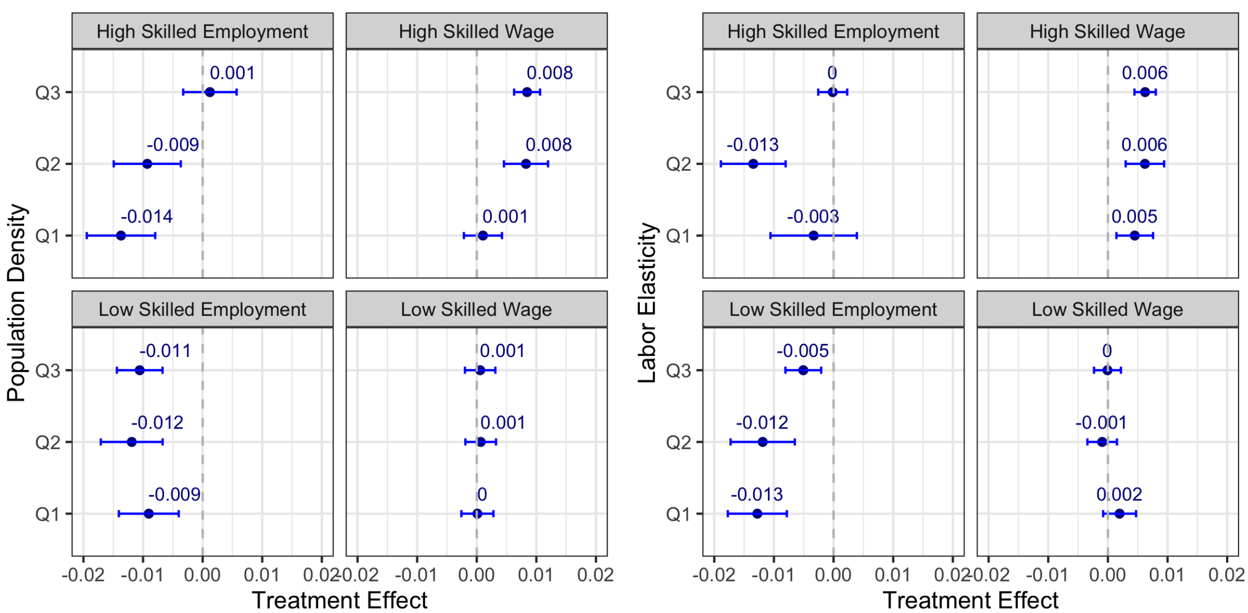
Notes: The figures provides coefficient estimate for the effect of high-technology IPOs on patent outcomes, by the pre-treatment productivity of each census tract. Economic values are stock market reaction to patents as calculated by Kogan et al. (2017). Sampled patents are in the same industry as the high-technology IPO firms, A census tract has High (Low) productivity of patent outputs if it has number of patents above (below) the average in year 2000, which is prior to all sampled high-technology IPOs. The error bars represent for 95% confidence interval. Standard errors are clustered on the IPO Zone level.



(a) High-skilled Wage (b) Low-skilled Wage (c) Rents

Figure A5: Simulation Results for Spatial Equilibrium Model

Notes: The figure plots the simulation results for spatial equilibrium model in Section 4. Structural Parameters are calibrated and estimated values as in Table (11) and (10). Ten thousands workers are assigned to neighborhoods proportionally based on the real commuting flow. Wages and rents are observed in period 0, while productivity and amenity are estimated. In each figure, the horizontal axis represents bin of neighborhoods, while smaller number indicates neighborhoods closer to the centriod of productivity shock (IPO Headquarter). The vertical axis represents the difference of values in logarithm. Each colored line indicates magnitude of productivity shock on high-skilled workers in period 1.



(a) Population Density (b) Labor Elasticity

Figure A6: Heterogeneous Treatment Effect on Labor Market

Notes: The figure plots the heterogeneous treatment effect on wage and employment by local population density and labor elasticity. The horizontal axis is the average treatment effect within bins, and the vertical axis is bins of variables ranking from highest to lowest. Population density is measured within 15 miles of headquarter of IPO firms. Proxy of labor elasticity is the commuting openness measure in the same area. I plot the 95% confidence interval as error bars. Standard errors are clustered at the IPO firm level.

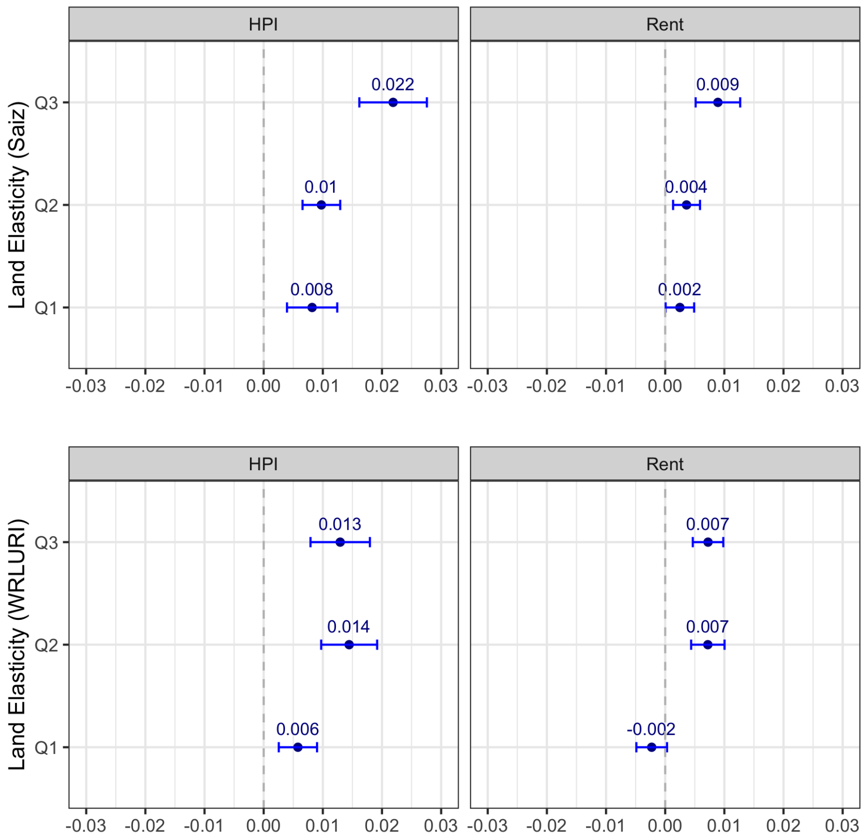


Figure A7: Heterogeneous Treatment Effect on Housing Market

Notes: The figure plots the heterogeneous treatment effect on housing values and rents by land elasticity. The horizontal axis is the average treatment effect within bins, and the vertical axis is bins of variables ranking from highest to lowest. Land elasticity is measured by land availability measure by Saiz (2010) and Wharton Land Regulatory Index by Gyourko et al. (2006). I plot the 95% confidence interval as error bars. Standard errors are clustered at the IPO firm level.

D Internet Appendix: Additional Tables

Table B1: Correlation Coefficients between Census Characteristics

	log (High-skilled Wage)	log (Low-skilled Wage)	log (High-skilled Emp.)	log (Low-skilled Emp.)	log (HPI)	log (Rent)	White	Black	Asian	Hispanic	Age 19 Under	Age 20 to 44	Age 45 to 64	Age 65 Up	College	Unemployment	Poverty	Rental	Vacant	Multiple	Ten-years
log (High-skilled Wage)	1.000	0.511	0.494	-0.194	0.340	0.390	0.152	-0.130	0.200	-0.175	-0.004	-0.156	0.276	-0.031	0.534	-0.304	-0.495	-0.322	-0.240	-0.126	0.150
log (Low-skilled Wage)	0.511	1.000	0.429	-0.064	0.321	0.424	0.206	-0.175	0.170	-0.202	0.030	-0.140	0.262	-0.073	0.418	-0.376	-0.625	-0.404	-0.289	-0.192	0.217
log (High-skilled Emp.)	0.494	0.429	1.000	0.080	0.403	0.400	0.150	-0.111	0.250	-0.215	-0.143	0.052	0.191	-0.078	0.722	-0.395	-0.457	-0.144	-0.277	0.072	0.095
log (Low-skilled Emp.)	-0.194	-0.064	0.080	1.000	-0.075	-0.136	-0.144	0.049	-0.079	0.201	0.257	0.113	-0.124	-0.245	-0.529	0.081	0.070	0.028	-0.116	-0.075	0.022
log (HPI)	0.340	0.321	0.403	-0.075	1.000	0.412	-0.039	-0.199	0.363	0.079	-0.117	0.038	0.107	-0.024	0.413	-0.212	-0.318	-0.049	-0.247	0.075	-0.054
log (Rent)	0.390	0.424	0.400	-0.136	0.412	1.000	-0.220	-0.027	0.404	0.178	-0.035	-0.028	0.152	-0.070	0.471	-0.095	-0.312	-0.112	-0.188	-0.010	-0.068
White	0.152	0.206	0.150	-0.144	-0.039	-0.220	1.000	-0.595	-0.395	-0.698	-0.260	-0.331	0.355	0.332	0.186	-0.390	-0.396	-0.452	0.083	-0.313	0.193
Black	-0.130	-0.175	-0.111	0.049	-0.199	-0.027	-0.595	1.000	-0.056	-0.029	0.098	0.166	-0.153	-0.156	-0.148	0.336	0.313	0.248	0.059	0.171	-0.136
Asian	0.200	0.170	0.250	-0.079	0.363	0.404	-0.395	-0.056	1.000	0.145	-0.008	0.167	-0.061	-0.138	0.287	-0.028	-0.089	0.155	-0.204	0.199	-0.057
Hispanic	-0.175	-0.202	-0.215	0.201	0.079	0.178	-0.698	-0.029	0.145	1.000	0.293	0.245	-0.342	-0.274	-0.249	0.258	0.314	0.345	-0.096	0.205	-0.132
Age 19 Under	-0.004	0.030	-0.143	0.257	-0.117	-0.035	-0.260	0.098	-0.008	0.293	1.000	0.035	-0.391	-0.616	-0.230	0.129	0.132	-0.078	-0.256	-0.218	0.206
Age 20 to 44	-0.156	-0.140	0.052	0.113	0.038	-0.028	-0.331	0.166	0.167	0.245	0.035	1.000	-0.690	-0.634	-0.007	0.052	0.292	0.596	-0.186	0.530	0.025
Age 45 to 64	0.276	0.262	0.191	-0.124	0.107	0.152	0.355	-0.153	-0.061	-0.342	-0.391	-0.690	1.000	0.363	0.220	-0.146	-0.403	-0.522	0.153	-0.406	-0.108
Age 65 Up	-0.031	-0.073	-0.078	-0.245	-0.024	-0.070	0.332	-0.156	-0.138	-0.274	-0.616	-0.634	0.363	1.000	0.031	-0.063	-0.137	-0.200	0.319	-0.095	-0.137
College	0.534	0.418	0.722	-0.529	0.413	0.471	0.186	-0.148	0.287	-0.249	-0.230	-0.007	0.220	0.031	1.000	-0.394	-0.429	-0.125	-0.168	0.112	0.087
Unemployment	-0.304	-0.376	-0.395	0.081	-0.212	-0.095	-0.390	0.336	-0.028	0.258	0.129	0.052	-0.146	-0.063	-0.394	1.000	0.549	0.293	0.183	0.115	-0.244
Poverty	-0.495	-0.625	-0.457	0.070	-0.318	-0.312	-0.396	0.313	-0.089	0.314	0.132	0.292	-0.403	-0.137	-0.429	0.549	1.000	0.600	0.238	0.328	-0.323
Rental	-0.322	-0.404	-0.144	0.028	-0.049	-0.112	-0.452	0.248	0.155	0.345	-0.078	0.596	-0.522	-0.200	-0.125	0.293	0.600	1.000	0.045	0.839	-0.356
Vacant	-0.240	-0.289	-0.277	-0.116	-0.247	-0.188	0.083	0.059	-0.204	-0.096	-0.256	-0.186	0.153	0.319	-0.168	0.183	0.238	0.045	1.000	0.007	-0.088
Multiple	-0.126	-0.192	0.072	-0.075	0.075	-0.010	-0.313	0.171	0.199	0.205	-0.218	0.530	-0.406	-0.095	0.112	0.115	0.328	0.839	0.007	1.000	-0.249
Ten-years	0.150	0.217	0.095	0.022	-0.054	-0.068	0.193	-0.136	-0.057	-0.132	0.206	0.025	-0.108	-0.137	0.087	-0.244	-0.323	-0.356	-0.088	-0.249	1.000

Notes: The table provides correlation coefficients between census characteristics corresponding with Figure A1. Observations from different years are collapsed into a single panel. Wages and Rents are adjusted to 2010 dollars. Definition of variables follows Table 3.

Table B2: Definition of Variables in Propensity Score Model

Category	Variable	Description	Interaction
Gender	Male	Percentage of males in total population	
Household	Urban Rural Poverty Housing Units	Percentage of population living in urban areas Percentage of population living in rural areas Percentage of population in poverty The number of housing units per capita	
Race	White Black Asian Native	Percentage of population with the specific race	
Age	Age 16 under Age 20 to 44 Age 45 to 64	Percentage of population in the specific age group	
Vehicle	Car No Car	Percentage of people commuting to work by car or public transportation Percentage of people commuting to work by bicycle or walk	
Education	High school Some college or associate degree Bachelor Graduate	Percentage of people with the specific educational attainment	Commuting time & Establishment
Employment	Unemployment High-tech Employment	Percentage of unemployed population Percentage of employment in high-technology industry	Commuting time & Establishment
Commuting time	Time 15 under Time 15 to 29 Time 30 to 59	Percentage of people with the specified commuting time	Education & Employment
Establishment	Establishment 10 under Establishment 10 to 49 Establishment 50 to 249	Percentage of establishments with the specified number of employees	Education & Employment

Notes: The table presents definition of variables and their categories in the propensity score model for predicting IPO events. Values are in percentage and based on 2000 Census, while census tracts are adjusted to 2010. For each census tract, each variable is calculated by summing and averaging all census tracts within 0-5, 5-10 and 10-15 miles respectively. Distance between census tracts is from the centriod of one tract to another. If two categories are interacted, it means a fully cross combination of all variables with the same distance zone.

Table B3: Estimation on Homeless People by Using Withdrawn Issuers as Counterfactual

	Log ($Homeless_{pop}$)				Log ($Homeless_{pov}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0063***	0.0063***	0.0097***	0.0097***	0.0045**	0.0047**	0.0070**	0.0073**
	(0.0018)	(0.0018)	(0.0028)	(0.0027)	(0.0019)	(0.0019)	(0.0029)	(0.0029)
Treat	-0.0035***	-0.0035***			-0.0025**	-0.0026**		
	(0.0010)	(0.0010)			(0.0011)	(0.0010)		
Covariates		✓		✓		✓		✓
Observations	2,803,711	2,803,711	2,803,711	2,803,711	2,803,711	2,803,711	2,803,711	2,803,711
R^2	0.99578	0.99579	0.99579	0.99580	0.99687	0.99688	0.99688	0.99689
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Distance-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The number of homeless people in each census tract is constructed by the 2007 - 2017 HUD data and the crosswalk by Glynn et al. (2021). $Homeless_{pop}$ uses total population in census tracts as weights, and $Homeless_{pov}$ uses the number of people in poverty as weights. The coefficient of *TreatXpost* identifies treatment effect of IPO on the number of homeless people. Definition of covariates are same as before. I provide the coefficients of covariates in Appendix. All specifications include IPO case-distance-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replaces census tract fixed effect with case-tract fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on distance to closest headquarter of withdrawn issuers. Standard errors are clustered at the IPO case level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table B4: Estimation on Wage by Skill Groups with Withdrawn IPO

	Log (High Skilled Wage)				Log (Low Skilled Wage)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0151***	0.0118***	0.0192***	0.0150***	0.0051***	0.0016	0.0065***	0.0020
	(0.0013)	(0.0011)	(0.0017)	(0.0014)	(0.0016)	(0.0012)	(0.0020)	(0.0015)
Treat	-0.0066***	-0.0051***			-0.0022***	-0.0007		
	(0.0006)	(0.0005)			(0.0007)	(0.0005)		
College		0.0384***		0.0385***		0.1891***		0.1891***
		(0.0131)		(0.0131)		(0.0056)		(0.0056)
Asian		0.0074		0.0071		0.3064***		0.3063***
		(0.0205)		(0.0205)		(0.0274)		(0.0273)
Hispanic		-0.1220***		-0.1216***		-0.1805***		-0.1805***
		(0.0045)		(0.0045)		(0.0060)		(0.0060)
Black		-0.4178***		-0.4173***		0.0769***		0.0770***
		(0.0327)		(0.0326)		(0.0266)		(0.0265)
White		0.0684***		0.0678***		0.4711***		0.4709***
		(0.0204)		(0.0203)		(0.0240)		(0.0240)
Poverty		-0.1483***		-0.1482***		-0.5475***		-0.5475***
		(0.0090)		(0.0089)		(0.0069)		(0.0068)
Unemployed		-0.1523***		-0.1521***		-0.2567***		-0.2567***
		(0.0064)		(0.0063)		(0.0085)		(0.0085)
Age under 19		0.4507***		0.4486***		0.4254***		0.4251***
		(0.0159)		(0.0159)		(0.0104)		(0.0103)
Age 20 to 44		0.2151***		0.2130***		-0.1003***		-0.1006***
		(0.0197)		(0.0197)		(0.0153)		(0.0153)
Age 45 to 64		0.3290***		0.3281***		0.0289*		0.0287*
		(0.0181)		(0.0181)		(0.0168)		(0.0168)
Rental		-0.2195***		-0.2193***		-0.1435***		-0.1434***
		(0.0078)		(0.0078)		(0.0067)		(0.0067)
Vacant		0.0117		0.0115		-0.0530***		-0.0531***
		(0.0092)		(0.0092)		(0.0069)		(0.0069)
Multiple		-0.1146***		-0.1147***		-0.0283***		-0.0283***
		(0.0086)		(0.0085)		(0.0037)		(0.0037)
Ten-years		-0.0244***		-0.0242***		0.0574***		0.0575***
		(0.0044)		(0.0044)		(0.0039)		(0.0038)
Observations	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853
R^2	0.78163	0.78535	0.78166	0.78537	0.70304	0.71621	0.70305	0.71621
Tract fixed effects	✓	✓			✓	✓		
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Distance-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Tract fixed effects			✓	✓			✓	✓

Notes: Sample consists of tract-level ACS 5-year data on workers by their educational attainment and 396 high-technology IPO firms. All observations are collapsed into a single panel for estimation. The coefficient of *TreatXpost* identifies treatment effect of IPO on welfare outcomes. Definition of covariates follows Table 3. All specifications include IPO case-distance-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replace census tract fixed effect with case-tract fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on distance to the closest withdrawn IPO issuer. Standard errors are clustered at the IPO case level. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table B5: Estimation on Employment by Skill Groups with Withdrawn IPO

	Log (High Skilled Employment)				Log (Low Skilled Employment)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0076***	-0.0016	0.0097***	-0.0020	-0.0112***	-0.0140***	-0.0142***	-0.0179***
	(0.0028)	(0.0022)	(0.0035)	(0.0028)	(0.0024)	(0.0018)	(0.0031)	(0.0022)
Treat	-0.0033***	0.0007			0.0049***	0.0062***		
	(0.0012)	(0.0009)			(0.0011)	(0.0008)		
College		2.688***		2.688***		-1.858***		-1.858***
		(0.0202)		(0.0201)		(0.0146)		(0.0146)
Asian		-0.4366***		-0.4365***		0.7304***		0.7309***
		(0.0901)		(0.0899)		(0.0588)		(0.0587)
Hispanic		-0.1025***		-0.1027***		0.1851***		0.1846***
		(0.0218)		(0.0217)		(0.0083)		(0.0083)
Black		-0.8649***		-0.8649***		-0.0310		-0.0316
		(0.0822)		(0.0820)		(0.0451)		(0.0450)
White		-0.9433***		-0.9431***		-0.3869***		-0.3860***
		(0.0430)		(0.0428)		(0.0257)		(0.0257)
Poverty		-0.3146***		-0.3146***		-0.4877***		-0.4878***
		(0.0139)		(0.0138)		(0.0117)		(0.0117)
Unemployed		-0.7948***		-0.7948***		-0.9368***		-0.9371***
		(0.0206)		(0.0205)		(0.0105)		(0.0105)
Age under 19		0.6081***		0.6085***		1.429***		1.432***
		(0.0423)		(0.0422)		(0.0197)		(0.0196)
Age 20 to 44		1.945***		1.946***		2.378***		2.381***
		(0.0534)		(0.0532)		(0.0230)		(0.0228)
Age 45 to 64		1.917***		1.917***		2.008***		2.009***
		(0.0526)		(0.0525)		(0.0226)		(0.0226)
Rental		-0.1260***		-0.1260***		0.0457***		0.0454***
		(0.0137)		(0.0137)		(0.0105)		(0.0104)
Vacant		-0.9874***		-0.9873***		-0.9830***		-0.9827***
		(0.0293)		(0.0293)		(0.0146)		(0.0145)
Multiple		0.2088***		0.2088***		0.2613***		0.2614***
		(0.0121)		(0.0121)		(0.0108)		(0.0107)
Ten-years		-0.0768***		-0.0769***		-0.0554***		-0.0557***
		(0.0103)		(0.0103)		(0.0099)		(0.0099)
Observations	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853
R^2	0.91605	0.93362	0.91605	0.93362	0.91585	0.93168	0.91586	0.93169
Tract fixed effects	✓	✓			✓	✓		
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Distance-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Tract fixed effects			✓	✓			✓	✓

Notes: Sample consists of tract-level ACS 5-year data on workers by their educational attainment and 396 high-technology IPO firms. All observations are collapsed into a single panel for estimation. The coefficient of *TreatXpost* identifies treatment effect of IPO on welfare outcomes. Definition of covariates follows Table 3. All specifications include IPO case-distance-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replace census tract fixed effect with case-tract fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on distance to the closest withdrawn IPO issuer. Standard errors are clustered at the IPO case level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table B6: Estimation on Wage and Employment Gap with Withdrawn IPO

	Log (Wage Premium)				Log (Relative Labor Supply)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0106***	0.0108***	0.0135***	0.0138***	0.0157***	0.0094***	0.0199***	0.0120***
	(0.0015)	(0.0014)	(0.0019)	(0.0018)	(0.0031)	(0.0017)	(0.0039)	(0.0022)
Treat	-0.0046***	-0.0047***			-0.0069***	-0.0041***		
	(0.0007)	(0.0006)			(0.0014)	(0.0008)		
College		-0.1682***		-0.1681***		4.592***		4.592***
		(0.0103)		(0.0103)		(0.0137)		(0.0137)
Asian		-0.1660***		-0.1663***		-1.040***		-1.040***
		(0.0293)		(0.0293)		(0.0662)		(0.0661)
Hispanic		0.0465***		0.0469***		-0.2735***		-0.2732***
		(0.0089)		(0.0089)		(0.0207)		(0.0207)
Black		-0.4447***		-0.4442***		-0.7762***		-0.7758***
		(0.0407)		(0.0406)		(0.0607)		(0.0606)
White		-0.3580***		-0.3585***		-0.5439***		-0.5444***
		(0.0283)		(0.0282)		(0.0379)		(0.0379)
Poverty		0.3942***		0.3942***		0.1516***		0.1516***
		(0.0108)		(0.0107)		(0.0139)		(0.0139)
Unemployed		0.0835***		0.0837***		0.1944***		0.1946***
		(0.0109)		(0.0108)		(0.0197)		(0.0196)
Age under 19		0.0485***		0.0466**		-0.8264***		-0.8280***
		(0.0186)		(0.0186)		(0.0295)		(0.0295)
Age 20 to 44		0.2995***		0.2976***		-0.4938***		-0.4955***
		(0.0244)		(0.0243)		(0.0430)		(0.0428)
Age 45 to 64		0.2917***		0.2909***		-0.1519***		-0.1527***
		(0.0242)		(0.0242)		(0.0484)		(0.0483)
Rental		-0.0612***		-0.0610***		-0.1562***		-0.1560***
		(0.0112)		(0.0112)		(0.0090)		(0.0090)
Vacant		0.0493***		0.0490***		0.0168		0.0167
		(0.0078)		(0.0078)		(0.0169)		(0.0169)
Multiple		-0.0828***		-0.0828***		-0.0483***		-0.0483***
		(0.0093)		(0.0093)		(0.0085)		(0.0085)
Ten-years		-0.0542***		-0.0540***		-0.0255***		-0.0253***
		(0.0056)		(0.0055)		(0.0070)		(0.0070)
Observations	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853
R^2	0.52854	0.53220	0.52856	0.53222	0.93766	0.95971	0.93766	0.95971
Tract fixed effects	✓	✓			✓	✓		
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Distance-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Tract fixed effects			✓	✓			✓	✓

Notes: Sample consists of tract-level ACS 5-year data on workers by their educational attainment and 396 high-technology IPO firms. All observations are collapsed into a single panel for estimation. The coefficient of *TreatXpost* identifies treatment effect of IPO on welfare outcomes. Wage premium is the ratio of high-skilled wage by low-skilled wage, and relative supply is the ratio of high-skilled employment by low-skilled employment. Definition of covariates follows Table 3. All specifications include IPO case-distance-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replace census tract fixed effect with case-tract fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on distance to the closest withdrawn IPO issuer. Standard errors are clustered at the IPO case level. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table B7: Estimation on Housing Market Outcomes with Withdrawn IPO

	Log (House Price Index)				Log (Housing Rent)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0219***	0.0196***	0.0278***	0.0250***	0.0068***	0.0044***	0.0086***	0.0056***
	(0.0022)	(0.0018)	(0.0027)	(0.0023)	(0.0014)	(0.0012)	(0.0018)	(0.0015)
Treat	-0.0096***	-0.0086***			-0.0030***	-0.0019***		
	(0.0010)	(0.0008)			(0.0006)	(0.0005)		
College		0.0371***		0.0373***		0.2031***		0.2031***
		(0.0056)		(0.0055)		(0.0090)		(0.0090)
Asian		0.7780***		0.7774***		0.4938***		0.4936***
		(0.0229)		(0.0228)		(0.0256)		(0.0256)
Hispanic		-0.0474***		-0.0467***		0.0062		0.0064
		(0.0033)		(0.0033)		(0.0061)		(0.0061)
Black		0.3334***		0.3342***		0.2792***		0.2793***
		(0.0321)		(0.0321)		(0.0245)		(0.0244)
White		0.4849***		0.4838***		0.5858***		0.5857***
		(0.0179)		(0.0179)		(0.0221)		(0.0221)
Poverty		-0.1330***		-0.1329***		-0.1533***		-0.1533***
		(0.0089)		(0.0089)		(0.0052)		(0.0052)
Unemployed		-0.0703***		-0.0699***		0.0149*		0.0149*
		(0.0114)		(0.0114)		(0.0079)		(0.0078)
Age under 19		0.3737***		0.3704***		0.2483***		0.2477***
		(0.0192)		(0.0191)		(0.0129)		(0.0129)
Age 20 to 44		-0.0414***		-0.0448***		0.1945***		0.1939***
		(0.0138)		(0.0138)		(0.0155)		(0.0155)
Age 45 to 64		-0.0748***		-0.0761***		0.0093		0.0091
		(0.0163)		(0.0163)		(0.0166)		(0.0166)
Rental		-0.0715***		-0.0711***		0.1489***		0.1490***
		(0.0071)		(0.0071)		(0.0084)		(0.0084)
Vacant		0.0148		0.0145		0.0352***		0.0351***
		(0.0101)		(0.0100)		(0.0074)		(0.0073)
Multiple		0.0775***		0.0775***		-0.3873***		-0.3873***
		(0.0049)		(0.0049)		(0.0090)		(0.0090)
Ten-years		0.0427***		0.0431***		0.1508***		0.1509***
		(0.0038)		(0.0038)		(0.0076)		(0.0076)
Observations	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853	4,354,853
R^2	0.94909	0.95025	0.94911	0.95027	0.83384	0.83825	0.83385	0.83825
Tract fixed effects	✓	✓			✓	✓		
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Distance-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Tract fixed effects			✓	✓			✓	✓

Notes: Sample consists of tract-level ACS 5-year data on workers by their educational attainment and 396 high-technology IPO firms. All observations are collapsed into a single panel for estimation. The coefficient of *TreatXpost* identifies treatment effect of IPO on welfare outcomes. Definition of covariates follows Table 3. All specifications include IPO case-distance-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replace census tract fixed effect with case-tract fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on distance to the closest withdrawn IPO issuer. Standard errors are clustered at the IPO case level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table B8: Estimation on Wage by Skill Groups with Propensity Score

	Log (High Skilled Wage)				Log (Low Skilled Wage)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0116***	0.0088***	0.0146***	0.0111***	0.0052***	0.0022**	0.0065***	0.0027**
	(0.0012)	(0.0010)	(0.0016)	(0.0013)	(0.0013)	(0.0010)	(0.0017)	(0.0013)
Treat	-0.0050***	-0.0038***			-0.0023***	-0.0010**		
	(0.0006)	(0.0005)			(0.0006)	(0.0004)		
College		0.0673***		0.0674***		0.2018***		0.2019***
		(0.0119)		(0.0119)		(0.0055)		(0.0054)
Asian		-0.0207		-0.0212		0.2438***		0.2437***
		(0.0176)		(0.0175)		(0.0271)		(0.0270)
Hispanic		-0.1140***		-0.1137***		-0.1810***		-0.1809***
		(0.0042)		(0.0042)		(0.0056)		(0.0056)
Black		-0.4351***		-0.4347***		0.0451*		0.0453*
		(0.0309)		(0.0307)		(0.0250)		(0.0249)
White		0.0754***		0.0750***		0.4663***		0.4663***
		(0.0207)		(0.0206)		(0.0213)		(0.0212)
Poverty		-0.1359***		-0.1359***		-0.5247***		-0.5247***
		(0.0084)		(0.0084)		(0.0062)		(0.0062)
Unemployed		-0.1525***		-0.1524***		-0.2577***		-0.2577***
		(0.0071)		(0.0071)		(0.0077)		(0.0076)
Age under 19		0.4931***		0.4919***		0.4392***		0.4389***
		(0.0153)		(0.0152)		(0.0105)		(0.0104)
Age 20 to 44		0.2345***		0.2335***		-0.0453***		-0.0455***
		(0.0175)		(0.0174)		(0.0136)		(0.0136)
Age 45 to 64		0.3638***		0.3634***		0.0795***		0.0794***
		(0.0162)		(0.0161)		(0.0146)		(0.0146)
Rental		-0.2256***		-0.2254***		-0.1566***		-0.1566***
		(0.0070)		(0.0069)		(0.0062)		(0.0061)
Vacant		0.0204**		0.0202**		-0.0387***		-0.0387***
		(0.0083)		(0.0083)		(0.0069)		(0.0069)
Multiple		-0.1039***		-0.1040***		-0.0261***		-0.0261***
		(0.0079)		(0.0079)		(0.0036)		(0.0036)
Ten-years		-0.0206***		-0.0204***		0.0519***		0.0519***
		(0.0039)		(0.0039)		(0.0037)		(0.0037)
Observations	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853
R^2	0.78398	0.78807	0.78400	0.78808	0.70965	0.72313	0.70965	0.72313
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Sample consists of tract-level ACS 5-year data on workers by their educational attainment and 396 high-technology IPO firms. All observations are collapsed into a single panel for estimation. The coefficient of *TreatXpost* identifies treatment effect of IPO on welfare outcomes. Definition of covariates follows Table 3. All specifications include IPO case-pscore-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replaces census tract fixed effect with case-tract fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on prediction of propensity score model. Standard errors are clustered at the IPO case level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table B9: Estimation on Employment by Skill Groups with Propensity Score

	Log (High Skilled Employment)				Log (Low Skilled Employment)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	-0.0018	-0.0085***	-0.0023	-0.0107***	-0.0136***	-0.0145***	-0.0172***	-0.0182***
	(0.0027)	(0.0022)	(0.0035)	(0.0028)	(0.0022)	(0.0018)	(0.0028)	(0.0023)
Treat	0.0008	0.0037***			0.0060***	0.0063***		
	(0.0012)	(0.0010)			(0.0010)	(0.0008)		
College		2.806***		2.806***		-1.794***		-1.795***
		(0.0226)		(0.0226)		(0.0158)		(0.0158)
Asian		-0.0429		-0.0423		0.9868***		0.9878***
		(0.0810)		(0.0808)		(0.0622)		(0.0621)
Hispanic		-0.1068***		-0.1070***		0.1757***		0.1753***
		(0.0182)		(0.0181)		(0.0080)		(0.0080)
Black		-0.6562***		-0.6566***		0.1093**		0.1087**
		(0.0742)		(0.0740)		(0.0456)		(0.0454)
White		-0.8289***		-0.8285***		-0.3637***		-0.3630***
		(0.0464)		(0.0463)		(0.0247)		(0.0247)
Poverty		-0.3271***		-0.3272***		-0.5295***		-0.5296***
		(0.0120)		(0.0120)		(0.0119)		(0.0119)
Unemployed		-0.7802***		-0.7804***		-0.9448***		-0.9451***
		(0.0182)		(0.0181)		(0.0112)		(0.0112)
Age under 19		0.7950***		0.7962***		1.474***		1.476***
		(0.0431)		(0.0430)		(0.0207)		(0.0207)
Age 20 to 44		1.929***		1.930***		2.299***		2.300***
		(0.0473)		(0.0471)		(0.0219)		(0.0218)
Age 45 to 64		1.772***		1.773***		1.829***		1.829***
		(0.0446)		(0.0444)		(0.0232)		(0.0232)
Rental		-0.1296***		-0.1298***		0.0599***		0.0595***
		(0.0126)		(0.0125)		(0.0104)		(0.0103)
Vacant		-0.9706***		-0.9704***		-0.9967***		-0.9964***
		(0.0233)		(0.0232)		(0.0139)		(0.0138)
Multiple		0.2369***		0.2369***		0.2842***		0.2843***
		(0.0121)		(0.0120)		(0.0112)		(0.0111)
Ten-years		-0.0561***		-0.0563***		-0.0220**		-0.0223**
		(0.0104)		(0.0104)		(0.0106)		(0.0105)
Observations	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853
R^2	0.91084	0.92989	0.91084	0.92989	0.91203	0.92740	0.91204	0.92740
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Sample consists of tract-level ACS 5-year data on workers by their educational attainment and 396 high-technology IPO firms. All observations are collapsed into a single panel for estimation. The coefficient of *TreatXpost* identifies treatment effect of IPO on welfare outcomes. Definition of covariates follows Table 3. All specifications include IPO case-pscore-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replace census tract fixed effect with case-tract fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on prediction of propensity score model. Standard errors are clustered at the IPO case level. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table B10: Estimation on Wage and Employment Gap with Propensity Score

	Log (Wage Premium)				Log (Relative Labor Supply)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0073***	0.0076***	0.0092***	0.0096***	0.0097***	0.0039***	0.0122***	0.0050***
	(0.0012)	(0.0011)	(0.0015)	(0.0014)	(0.0027)	(0.0015)	(0.0034)	(0.0019)
Treat	-0.0032***	-0.0033***			-0.0042***	-0.0017**		
	(0.0005)	(0.0005)			(0.0012)	(0.0007)		
College		-0.1528***		-0.1528***		4.647***		4.647***
		(0.0095)		(0.0094)		(0.0136)		(0.0136)
Asian		-0.1562***		-0.1567***		-0.9282***		-0.9284***
		(0.0274)		(0.0273)		(0.0498)		(0.0497)
Hispanic		0.0569***		0.0571***		-0.2711***		-0.2710***
		(0.0081)		(0.0080)		(0.0174)		(0.0173)
Black		-0.4380***		-0.4377***		-0.7410***		-0.7409***
		(0.0377)		(0.0376)		(0.0495)		(0.0493)
White		-0.3492***		-0.3495***		-0.4770***		-0.4772***
		(0.0263)		(0.0262)		(0.0343)		(0.0342)
Poverty		0.3847***		0.3847***		0.1823***		0.1823***
		(0.0099)		(0.0099)		(0.0128)		(0.0127)
Unemployed		0.0890***		0.0891***		0.2156***		0.2157***
		(0.0108)		(0.0107)		(0.0175)		(0.0175)
Age under 19		0.0656***		0.0645***		-0.6984***		-0.6990***
		(0.0184)		(0.0183)		(0.0277)		(0.0276)
Age 20 to 44		0.2675***		0.2666***		-0.4296***		-0.4301***
		(0.0224)		(0.0223)		(0.0375)		(0.0374)
Age 45 to 64		0.2825***		0.2822***		-0.1126***		-0.1127***
		(0.0235)		(0.0234)		(0.0409)		(0.0408)
Rental		-0.0527***		-0.0525***		-0.1746***		-0.1746***
		(0.0096)		(0.0096)		(0.0082)		(0.0082)
Vacant		0.0478***		0.0476***		0.0381***		0.0380***
		(0.0078)		(0.0078)		(0.0130)		(0.0130)
Multiple		-0.0763***		-0.0763***		-0.0421***		-0.0421***
		(0.0085)		(0.0085)		(0.0079)		(0.0078)
Ten-years		-0.0453***		-0.0452***		-0.0359***		-0.0358***
		(0.0053)		(0.0053)		(0.0063)		(0.0063)
Observations	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853
R^2	0.53151	0.53504	0.53152	0.53505	0.93584	0.95929	0.93584	0.95929
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Sample consists of tract-level ACS 5-year data on workers by their educational attainment and 396 high-technology IPO firms. All observations are collapsed into a single panel for estimation. The coefficient of *TreatXpost* identifies treatment effect of IPO on welfare outcomes. Wage premium is the ratio of high-skilled wage by low-skilled wage, and relative supply is the ratio of high-skilled employment by low-skilled employment. Definition of covariates follows Table 3. All specifications include IPO case-pscore-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replace census tract fixed effect with case-tract fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on prediction of propensity score model. Standard errors are clustered at the IPO case level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table B11: Estimation on Housing Market Outcomes with Propensity Score

	Log (House Price Index)				Log (Housing Rent)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0178***	0.0161***	0.0225***	0.0203***	0.0086***	0.0071***	0.0108***	0.0090***
	(0.0018)	(0.0016)	(0.0022)	(0.0020)	(0.0012)	(0.0011)	(0.0015)	(0.0014)
Treat	-0.0078***	-0.0070***			-0.0037***	-0.0031***		
	(0.0008)	(0.0007)			(0.0005)	(0.0005)		
College		0.0232***		0.0232***		0.2126***		0.2126***
		(0.0044)		(0.0044)		(0.0085)		(0.0085)
Asian		0.6663***		0.6653***		0.5222***		0.5218***
		(0.0232)		(0.0232)		(0.0270)		(0.0269)
Hispanic		-0.0472***		-0.0467***		0.0117**		0.0119**
		(0.0034)		(0.0034)		(0.0052)		(0.0052)
Black		0.2323***		0.2330***		0.2549***		0.2552***
		(0.0290)		(0.0289)		(0.0220)		(0.0219)
White		0.4721***		0.4715***		0.5540***		0.5537***
		(0.0176)		(0.0176)		(0.0211)		(0.0211)
Poverty		-0.1159***		-0.1158***		-0.1560***		-0.1559***
		(0.0075)		(0.0075)		(0.0052)		(0.0051)
Unemployed		-0.0568***		-0.0566***		0.0156**		0.0157**
		(0.0099)		(0.0098)		(0.0072)		(0.0072)
Age under 19		0.3178***		0.3155***		0.2059***		0.2049***
		(0.0160)		(0.0160)		(0.0126)		(0.0126)
Age 20 to 44		0.0163		0.0145		0.1495***		0.1487***
		(0.0116)		(0.0115)		(0.0155)		(0.0155)
Age 45 to 64		-0.0101		-0.0107		0.0284*		0.0281*
		(0.0134)		(0.0134)		(0.0155)		(0.0155)
Rental		-0.0555***		-0.0551***		0.1391***		0.1392***
		(0.0051)		(0.0051)		(0.0080)		(0.0079)
Vacant		0.0053		0.0050		0.0061		0.0059
		(0.0099)		(0.0099)		(0.0075)		(0.0074)
Multiple		0.0712***		0.0711***		-0.4146***		-0.4146***
		(0.0041)		(0.0041)		(0.0091)		(0.0090)
Ten-years		0.0508***		0.0511***		0.1437***		0.1439***
		(0.0031)		(0.0032)		(0.0069)		(0.0069)
Observations	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853	5,038,853
R^2	0.95470	0.95557	0.95472	0.95558	0.84438	0.84891	0.84439	0.84891
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Sample consists of tract-level ACS 5-year data on housing market outcomes and 396 high-technology IPO firms. All observations are collapsed into a single panel for estimation. The coefficient of *TreatXpost* identifies treatment effect of IPO on welfare outcomes. Definition of covariates follows Table 3. All specifications include IPO case-pscore-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replace census tract fixed effect with case-tract fixed effect. An IPO case corresponds with an IPO Zone, which is the collection of census tracts within 30 miles of headquarter of IPO firms. IPO Zone is further split into bins $h \in \{1, 2, 3, 4, 5\}$ based on prediction of propensity score model. Standard errors are clustered at the IPO case level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table B12: Balance of Selected versus Not-selected Non-high-technology Firms

	Not Selected (N=320)		Selected (N=107)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
IPO Price	16.645	4.953	16.809	6.586	0.164	0.821
IPO Proceedings	279.888	353.300	244.591	259.849	-35.297	0.283
Current Assets	388.366	699.023	310.897	400.343	-77.470	0.177
Total Assets	1492.058	3233.463	1402.094	3812.525	-89.964	0.833
Liability	1103.347	3055.110	1072.591	3157.446	-30.756	0.932
Revenue	1355.619	3524.301	1264.932	2129.577	-90.687	0.756
EBIT	112.256	299.306	110.188	151.258	-2.068	0.927
Net Income	23.845	176.478	46.498	186.228	22.654	0.289

Notes: The table presents summary statistics for selected non-high-technology firms versus those not selected. Variables are from Audit Analytics and Compustat and by the year end of IPOs. The result indicates that firms in the sample are very similar in their IPO and financial position to all firms. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table B13: Estimation on Outcomes of Labor and Housing Markets by Non-High-Technology IPOs

	Log (High Skilled Wage)				Log (Low Skilled Wage)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0031	0.0035	0.0038	0.0043	0.0042*	0.0047**	0.0051*	0.0057**
	(0.0026)	(0.0024)	(0.0031)	(0.0029)	(0.0025)	(0.0021)	(0.0030)	(0.0025)
Treat	-0.0015	-0.0017			-0.0021	-0.0023**		
	(0.0013)	(0.0012)			(0.0012)	(0.0010)		
Covariates		✓		✓		✓		✓
Observations	669,013	669,013	669,013	669,013	669,013	669,013	669,013	669,013
R^2	0.75970	0.76477	0.75970	0.76477	0.73480	0.75264	0.73481	0.75264
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
	Log (High Skilled Employment)				Log (Low Skilled Employment)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0013	-0.0023	0.0015	-0.0028	-0.0009	-0.0029	-0.0011	-0.0035
	(0.0068)	(0.0045)	(0.0082)	(0.0054)	(0.0052)	(0.0041)	(0.0062)	(0.0050)
Treat	-0.0006	0.0011			0.0005	0.0014		
	(0.0033)	(0.0022)			(0.0025)	(0.0020)		
Covariates		✓		✓		✓		✓
Observations	669,013	669,013	669,013	669,013	669,013	669,013	669,013	669,013
R^2	0.89440	0.91994	0.89440	0.91994	0.89907	0.91639	0.89907	0.91640
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
	Log (Wage Premium)				Log (Relative Labor Supply)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	-0.0015	-0.0015	-0.0018	-0.0019	0.0016	6.77×10^{-5}	0.0019	9.1×10^{-5}
	(0.0031)	(0.0030)	(0.0037)	(0.0036)	(0.0060)	(0.0032)	(0.0072)	(0.0039)
Treat	0.0007	0.0008			-0.0008	-5.46×10^{-5}		
	(0.0015)	(0.0015)			(0.0029)	(0.0016)		
Covariates		✓		✓		✓		✓
Observations	669,013	669,013	669,013	669,013	669,013	669,013	669,013	669,013
R^2	0.53372	0.53753	0.53372	0.53753	0.92038	0.95132	0.92038	0.95132
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
	Log (House Price Index)				Log (Housing Rent)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatXpost	0.0099***	0.0098***	0.0120***	0.0120***	0.0047	0.0057*	0.0056	0.0070*
	(0.0034)	(0.0033)	(0.0040)	(0.0039)	(0.0031)	(0.0029)	(0.0037)	(0.0035)
Treat	-0.0048***	-0.0048***			-0.0023	-0.0028*		
	(0.0017)	(0.0016)			(0.0015)	(0.0014)		
Covariates		✓		✓		✓		✓
Observations	669,013	669,013	669,013	669,013	669,013	669,013	669,013	669,013
R^2	0.95006	0.95143	0.95006	0.95143	0.82740	0.83529	0.82740	0.83530
Tract fixed effects	✓	✓			✓	✓		
Case-Tract fixed effects			✓	✓			✓	✓
County-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Case-Pscore-Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Sample consists of tract-level ACS 5-year data on workers by their educational attainment and 107 non-high-technology IPO firms. All observations are collapsed into a single panel for estimation. Same as above, the coefficient of *TreatXpost* identifies treatment effect of IPO on welfare outcomes. I provide the coefficients of covariates in Appendix. All specifications include tract fixed effect, IPO case-pscore-year fixed effect and county-year fixed effect, while columns (3)(4)(7)(8) replaces census tract fixed effect with case-tract fixed effect. Standard errors are clustered at the IPO Zone level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.