Local Economic Spillover Effects of Stock Market Listings

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Abstract

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Abstract

We show that IPOs have non-trivial positive spillover effects on local labor markets, business environments, consumer spending, real estate, and migration. We mitigate endogeneity concerns about unobserved heterogeneity with restrictive geographic fixed effects coupled with a matching procedure. We show that it is the listing decision, which encompasses both a wealth and liquidity shock, that induces economic spillovers. Conditional on an IPO occurring, we estimate that an additional $10 million in IPO proceeds is associated with an extra 41 jobs and 0.7 new establishments locally.

I. Introduction

Anecdotal evidence suggests that the change in the listing status of a firm and the concomitant liquidity it provides its shareholders can significantly affect local economies. An example is Facebook. The Facebook initial public offering (IPO) was enormous, raising over $16 billion and making liquid the paper wealth that many Facebook employees had in the form of previously illiquid stock. As the author of one article about the Facebook IPO states, "When Facebook CEO Mark Zuckerberg rang the NASDAQ opening bell on Friday [May 18, 2012] to mark Facebook's public debut, he also rang in a crop of new millionaires and billionaires."[1]

Without providing exact numbers, the authors of that article and others from around the same time suggest that thousands of Facebook employees became millionaires in the transaction. The author of another article from the time speculates that the Facebook IPO could create more than $1 billion in property value in the San Francisco Bay area near the company's headquarters in Palo Alto, California, due to the number of millionaires competing to buy homes.[2] This phenomenon is not unique to Facebook; more than two decades earlier, Dell went public, creating liquidity for numerous employee-shareholders in the Austin, Texas, area near Dell's headquarters. These employees, sometimes referred to as "Dellionaires," got rich off their shareholdings and bought and built homes in the area, thereby popularizing the term "McMansion," which describes their ubiquitous domiciles.[3]

[1] http://money.cnn.com/2012/05/21/technology/facebook-ipo-millionaires/index.htm.
[2] http://www.huffingtonpost.com/david-j-cross/san-francisco-real-estate_b_1527172.html.
[3] Austin, TX has residential design ordinances colloquially known as "McMansion regulations." See http://www.austintexas.gov/department/residential-design-compatability-standards.

We study whether IPOs are associated with positive economic spillover effects near the firms' headquarters. We find that IPOs have a significant and broad effect on local real estate outcomes (home prices and mortgage originations), labor market outcomes (employment growth), and other measures of economic growth (establishments' growth and individuals' credit card spending). But why? An IPO does not create a new firm, but it does create significant liquidity for the firm and the employees and other shareholders who own shares of the newly listed firm. The IPO also affects investor wealth if the firm's stock price increases after listing and it affects firm "wealth" by raising new capital. We provide evidence that both liquidity and wealth effects on investors are important channels for these local impacts from the IPO.

Identification problems arising from reverse causality and omitted variable bias are possible. To address these identification challenges, we compare outcomes for the specific locations—their ZIP codes—within a county where IPO firms have their headquarters to other areas of the same county in the same year. Our main identifying assumption is that the choice of the location of the headquarters *ZIP code* within a county when the firm is founded—eight years before the firm files for an IPO for the median in our sample—is independent of the long-run economic development of that specific ZIP code relative to other similar ZIP codes nearby. We construct plausible counterfactuals using matched no-IPO ZIP codes in the same county and year with similar ZIP code level characteristics and past IPO activity.

This approach to estimating an IPO's effect on the local economy forces identification from differences in geographic proximity of ZIP codes to the headquarters of a newly listed firm. The design mitigates concerns over omitted variables bias because

an offending omitted variable would have to be correlated with our outcome variables and our explanatory IPO activity variable and it would have to vary by proximity to the IPO firm headquarters within the same county and year after conditioning on our control variables. The approach is nuanced, and we explain it in more detail below.

We use this empirical design to study how IPOs affect the local economy around the IPO headquarters. We start with an examination of real estate outcomes and find home price growth increases by more than one-third, from 2.8% to 3.7%, in ZIP codes located within two miles from the IPO headquarters, but only for expensive houses. This home price growth declines with a ZIP code's distance from the IPO headquarters. Consistent with the possibility that home price growth is driven by new purchases, we find that the average and total mortgage amount growth is higher in ZIP codes near the IPO headquarters.

Do the real estate effects reflect that IPO shareholders now have more liquid stock in the firm due to its listing? Or do the results stem from a change in the wealth of those investors? We find evidence of both. Home price growth accelerates after the expiration of the lockup period, when shareholders can sell their stock. Moreover, this home price growth is higher for listings in which the firm's stock market price increases in the post-IPO period. A placebo test further supports the intuition: *Buyout-backed* IPOs are owned mainly by private equity firms and their limited partners, who are primarily institutional investors, rather than IPO-firm employees. These institutional investors are less likely to live close to the company headquarters and less likely to affect local housing demand. Using buyout-backed IPOs as placebo events, we confirm that the lack of local investors mutes the IPO-spillover effects.

Is the economic spillover effect driven by its employees and investors or by the firm itself—i.e., raising external capital? To assess whether the spillovers are due to a capital-raising effect, we include in our tests a variable that reflects seasoned equity offering (SEO) activity in the area. When we do so, the SEO activity variable is statistically insignificant. We interpret this finding as being consistent with changes in firms' listing status, rather than simply a capital raising event, explaining the economic spillover effects.

We examine possible channels of the IPO spillover effects. IPOs could affect local labor markets and business development through either direct or indirect channels. For instance, Kenney, Patton, and Ritter (2012) show that firm employment grows at an annual rate of 6% to 7% after the firm goes public—a direct effect. Consistent with a direct effect, we find that annual employment growth in IPO headquarters ZIP codes increases from an average of 1.4% to an average of 2%.

An indirect channel through which IPOs may affect the local economy is through investors' higher home prices and spending (Mian, Rao, and Sufi (2013), Mian and Sufi (2014)). Recent studies use changes in home prices to proxy for local wealth shocks and find an increase in employment growth (Schmalz, Sraer, and Thesmar (2017)), and establishment growth (Adelino, Schoar, and Severino (2015)). We find evidence consistent with a local demand-driven IPO effect on business development. IPOs have a large effect on the growth of retail establishments and construction near the IPO headquarters ZIP code but do not affect establishments that depend on national, rather than local, demand—i.e., businesses in the tradable sector. We also find that local consumers' credit card spending increases near the IPO headquarters, providing further support for the hypothesis that IPO effects on the local economy are demand-driven. Finally, we

document changes in post-IPO migration patterns: low-income people are 0.7-1.5% more likely to move away to ZIP codes with lower average income or lower housing costs increases.

In addition to these extensive margin tests, we quantify the intensive margin. Conditional on a change in listing status, IPOs with larger proceeds may have larger effects on local economic outcomes. Holding other factors equal, we estimate the incremental effect of an extra $10 million in IPO proceeds is, on average, 41 new employees in the county, of which approximately 69% are in the IPO firm's headquarters ZIP code and 31% are not. Our intensive margin calculations also show an increase in the number of business establishments in the county by 0.7 per additional $10 million proceeds.

Our paper adds to the finance-growth nexus literature (Jayaratne and Strahan (1996); Levine (2005)), particularly on how equity market access affects local economies. We show firm-level changes in listing status affect the surrounding geographic area's real estate, labor, and business development, and thus we provide some micro-foundation evidence for the country-level results in the literature and some macro-implications consistent with the company-level results in the literature. This study fits between the literature that examines how a firm's equity issues (Brown, Fazzari, and Petersen (2009); Brown and Floros (2012)) and listing status (Acharya and Xu (2016)) affect company activity and growth and the literature on how country-level equity market features affect country-level growth (Bekaert, Harvey, and Lundblad (2005); Beck and Levine (2004); Brown, Martinsson, and Petersen (2013)). The positive local economic spillover effects of IPOs we document here are consistent with studies that link household wealth with employment (Mian, Rao, and Sufi (2013); Mian and Sufi (2014)), employee wealth with

increases in entrepreneurship (Cagetti and De Nardi (2006); Babina, Ouimet, and Zarutskie (2017)), and entrepreneurship with job creation (Haltiwanger, Jarmin, and Miranda (2013)).

II. Methods and Data

A. Empirical design and identifying assumptions

Our main tests use a restrictive fixed effects approach to mitigate concerns of unobservable factors driving our results. Specifically, we use county-year fixed effects and examine the variation in economic outcomes across ZIP codes within the county and year of a company going public. Like all empirical designs, this approach has strengths and weaknesses. It does not address reverse causality (we have different tests, in the online appendix, that address this issue). But our empirical design does control for a wide variety of time- and geography-varying factors. For an omitted variable to bias our coefficient estimates, it would have to vary only across ZIP codes within county and within year, and to be correlated with IPO activity but not our control variables. Our main identifying assumption is the following: in the absence of a significant local shock such as an IPO, the change in the economic development of nearby ZIP codes in a given year should be similar on average.

Here we note some data management choices, and we relegate a detailed discussion of this empirical design and its interpretation to the online appendix. We exclude ZIP codes that never had an IPO from 1990 to 2015, making our treated and control samples more homogeneous. Over time, IPOs are dispersed across several different ZIP codes in a given

county; therefore, a ZIP code with an IPO in one year (treated) may instead serve as a control ZIP code in another year.

To induce homogeneity of our treated and control ZIP-year observations, we use a matching procedure. Within a given county and year, we match ZIP codes that had an IPO with ZIP codes that have no IPOs based on the lagged number of establishments and employment. To balance inclusiveness of observations with closeness of match along these dimensions, we use coarsened exact matching (see Blackwell, Iacus, and Porro (2009)). The procedure is similar to characteristic matching, but rather than trying to match well on multiple continuous dimensions, such as the characteristic matching in Barber and Lyon (1997), we match exactly on discretized bins of these variables. The result is a categorical match on every dimension for every observation that we use. We cut (or coarsen) these covariates into five groups based on the yearly distribution of their values. This process creates a number of dimensional strata, or bins. Finally, we put all our observations in these bins and make sure that each bin has at least one observation from the treated and control groups. We drop all observations that do not satisfy the above criteria. We repeat this matching process for each outcome variable we study. Once we match, the standardized differences suggest that our matched sample contains only ZIP codes with well-balanced characteristics, with all the standardized differences close to zero.

B. Structure and interpretation of tests

Our empirical model allows us to estimate IPO spillover effects within a county-year based on each ZIP code's distance from the IPO. Most of our tests are structured as follows.

$$(1)\quad (\overline{Y}\ \text{growth})_{i,j,t+1} = \beta_1(\text{IPO Headquarters Zip Code})_{i,t} + \beta_2(\text{Within 2 miles of IPO Headquarters})_{i,t} + \beta_3(\text{Between } 2-5 \text{ miles from IPO Headquarters})_{i,t} + \beta_4(\text{Between } 5-10 \text{ miles from IPO Headquarters})_{i,t} + \gamma(\text{SEO}>0)_{i,t} + \alpha_{j,t} + X_{i,t} + \epsilon_{i,j,t+1}.$$

In regression (1), the dependent variable $(\overline{Y}\ \text{growth})_{i,j,t+1}$ is the two-year average annual growth of economic variables that reflect activity in real estate, economic development and growth, and consumer behavior (home price index, home value for top-tier, middle-tier, and bottom-tier houses, mortgage amounts, employment, business establishments, and credit card spending). We use the two-year average growth rate because the effect of the IPO in the local economy may take longer than one year to show in the data, and many of our variables are measured at an annual frequency. The pair *i*,*t* indicates ZIP code-year, which is the main unit of observation in our sample. $X_{i,t}$ is a vector of lagged ZIP code control variables (Ln(*population*), Ln(*population density*), Ln(*establishments*), Ln(*employment*), Ln(*wage income*), and the lagged dependent variable). The regressions include county-year fixed effects ($\alpha_{j,t}$).[4]

The coefficient β_1 reflects the difference in the annual growth rate of *Y* between the IPO headquarters ZIP code and all ZIP codes with no IPO activity in the same county and year. The coefficient β_2 captures the difference in growth rate of *Y* between ZIP codes

[4] We use restrictive county-year fixed effects for our baseline specifications because they force identification from IPO activity across ZIP codes in the same county *and* year. When we apply a fixed effect for each ZIP code and a fixed effect for each year we find economically larger IPO spillover effects. These results are in our online appendix, section A.II.

located within two miles from the IPO and ZIP codes outside this radius.[5] Similarly, estimators β_3 and β_4 capture the difference in growth rate of *Y* between ZIP codes located two to five miles and five to 10 miles from ZIP codes in the same county and year located outside their respective radius. Importantly, every ZIP code in our sample serves both as a treated observation at least once and as a control observation—at varying distances—potentially in many different years. This cross-ZIP code heterogeneity in IPOs over time and the ZIP code's distance from an IPO allows us to identify a plausible counterfactual for what the change in outcome *Y* would have been without the IPO.[6]

C. Data

We gather IPO data on US firms from Securities Data Corporation (SDC) from 1998 to 2015 and use the address of each firm's headquarters to determine its associated ZIP code. Some firms may have operations in geographic areas distant from their headquarters, especially large firms; this geographic dispersion should bias our estimates towards zero because we treat them as control ZIP code-years, when in fact they could be considered treated. We exclude firms with missing information on ZIP code or filing amount and, following Ritter and Welch (2002), and Gao, Ritter, and Zhu (2013), also remove from the sample foreign issuers, ETFs, REITS, non-stocks, and financials.[7] We retain spinoffs and IPOs whose offer price is less than $5, but when we exclude them our regression estimates are generally 10% to 30% larger in magnitude. We also include data

[5] ZIP codes have irregular boundaries (or shapes), so to calculate the distance between two ZIP codes we compute the mile-distance of a straight line between their centroids, or the center of the mass of their area.
[6] We provide a more extensive illustration of our identification approach in the online appendix (see section A.I, Figures A.1-A.3).
[7] Specifically, we exclude the following security types: unit offers, trust units, stock units, limited partnerships, master limited partnerships, and security types that appear in SDC as: "Beneficial Ints", "Shs Benficl Int", and "Ltd Liab Int". We also exclude closed-end funds.

on ZIP code-year seasoned equity offerings (SEOs) and use these as placebo events because, unlike IPOs, they do not affect the firm's listing status, although they do involve raising capital and providing liquidity.

Even though SDC provides details about the timing, location, and characteristics of IPOs in the US since the 1970s, our need for economic data at the ZIP code level limits the time series of the panel. Our sample is from 1998 to 2015, excluding years 1999, 2000, and 2003 because of the lack of income data at the ZIP code level. For tests that examine credit card spending and migration patterns, our sample period is 2005 to 2015. We also have tests, relegated to the online appendix, that expand the external validity of our main results by using larger geographic units and a longer sample period.

We construct a ZIP code-year panel dataset of annual income, employment, business, real estate, and credit card spending data from various datasets. Data on population and population density (population divided by ZIP code land area) are from the US Census files. We calculate ZIP code per capita income from the IRS's Statistics of Income data. US Census ZIP Code Business Patterns data add annual information on employment and the total number of establishments. The Census business pattern datasets break down the annual number of establishments in the ZIP code by NAICS codes and by establishment size (based on the number of employees). Following the NAICS classifications from Mian and Sufi (2014), we calculate the total number of establishments in three sectors: the tradable sector, the non-tradable sector, and construction. The tradable sector includes establishments whose growth depends on national or global demand for the firm (i.e., they have imports or exports). Local IPO investors are less likely to affect this

sector. On the other hand, the non-tradable sector includes business in retail and restaurants, and construction includes businesses in real estate development.

The Federal Housing Finance Agency (FHFA) provides annual ZIP code home price index data. The FHFA does not break down home prices by size category, so we complement our home price data using Zillow. Based on how expensive the house is, Zillow provides home values for top-, middle-, and bottom-tier homes. Our real estate variables also include annual data on mortgage origination from the Home Mortgage Disclosure Act (HMDA) data. HMDA provides individual-level data on mortgage originations that we aggregate at the ZIP code-level to calculate the total number of mortgage applications, the total amount originated, and the average mortgage amount in the ZIP code.

Finally, we use a large sample of individual-level consumer finance data from Experian.[8] The credit bureau dataset includes annual data on credit card consumption and the associated ZIP code of the person's location. Using this dataset, we construct the average annual credit card spending in the ZIP code. For our sample of people in the Experian dataset, we locate the ZIP code of each person's residence and trace migration patterns across different ZIP codes around the timing of local IPOs.

Although the results are insensitive to doing so, we winsorize all variables at the first and 99th percentiles of their empirical distributions. Winsorizing mitigates the effects

[8] Our sample from Experian for 2005 to 2015 is a representative random sample of 1% of the US population. It contains information on all people with credit reports whose social security number ends in a particular two-digit number.

of outliers, such as from local economies that may experience large changes in our outcome variables because of exogenous events, e.g., natural disasters.

D. Summary statistics

Table I presents summary statistics for the main variables. Our final sample consists of 1,365 ZIP codes that have at least one IPO. The vast majority of ZIP codes have no more than one IPO in a given year, which suggests that there is significant heterogeneity within a county related to the location of IPOs. The average proceeds (not adjusted for inflation) from all IPOs in a given ZIP-year are approximately $130 million ($266 million for large IPOs), conditional on there being at least one IPO.

[Insert Table I Here]

Panel B shows the average ZIP code characteristics. The average per capita ZIP code wage income in the sample is approximately $60,000, which is larger than the overall US average. Counties with more business establishments have higher income, which explains why our ZIP code income, employment, and business establishment averages are higher after we exclude ZIP codes that never had an IPO during the sample period. The average ZIP code in our sample has approximately 24,000 people employed and 1,200 business establishments. The average home price index is 138 (with year 2000 as a basis of 100), and the average mortgage amount is approximately $280,000. Finally, between 2005 and 2015, people in these ZIP codes spent an average of $18,200 per year on their credit cards.

In Table I, Panel C, we divide the ZIP codes into two groups based on whether they had at least one IPO (column 1), or no IPOs (column 2) in a given year and present the group averages in the prior year. On average, for every treated ZIP code-year with at least

one IPO, we have five similar ZIP codes in the same county-year as controls. This table shows that we have a balanced set of treated and control ZIP codes. In the third column, we show univariate differences between ZIP codes with IPO activity and no IPO activity. In the last column, we express these differences normalized by their standard deviation; all of these normalized differences are close to zero, suggesting good covariate balance.

III. Empirical Results

In our empirical analysis, we focus on estimating cross-sectional differences in the real estate and business development of ZIP codes within a given county-year as a function of their distance from the ZIP code of the IPO headquarters.

A. IPO activity and local real estate markets

Motivated by numerous articles about the IPO effects in the popular press, we begin our analysis by studying whether an IPO has an important effect on local real estate markets. The average number of homes sold in a ZIP code in a given year is fewer than 1,200, and the median firm in our sample has approximately 600 employees in the year it goes public. Therefore, if the change in the listing status of the company is an important liquidity event for its shareholders and employees, we expect this shock to have a significant effect on demand for the local housing market even if just a fraction of the employees buy new homes.

Our empirical design allows us to estimate not only the magnitude but also the geographic scope of IPO spillover effects on neighboring economies. The regression (1) in Table 2 suggests that an IPO does not significantly affect prices of houses for that ZIP code. However, neighborhoods adjacent to the IPO ZIP code—within a two-mile range

(*within 2 miles from IPO HQ*)—experience an increase in home price growth 31 basis points larger than the mean (3.2%). The difference in home price growth in ZIP codes that are farther away (between two to five or five to 10 miles) is indistinguishable from the average ZIP code home price growth in that county. The finding that post-IPO home price growth in the ZIP code of the IPO headquarters is similar to the average home price growth in the county suggests that our results are not driven by selection bias of the IPO ZIP code.

[Insert Table 2 Here]

We hypothesize that larger IPOs may have larger effects on home prices, other things equal. Consistent with this hypothesis, we find that the magnitude of the effect of IPO activity in the top quartile of the yearly distribution on home price growth is larger and geographically more extensive. In Table 2, column (2), home price growth in neighborhoods within a two-mile radius of the IPO ZIP code increases by 88 basis points more than it would without the IPO. Home prices in the ZIP codes in our sample grow at a rate of 3.2% per year. Therefore, a 31-88 basis point increase in home price growth is economically large and represents a 10% to 28% increase over the mean.

The geographic reach of the spillover effect is large, too. Not only do home prices within two miles of the IPO firm's headquarters increase, but we find that home prices in ZIP codes two to five miles from the IPO firm's headquarters experience an increase in home price growth of 64 basis points. Consistent with the hypothesis that the spillover effects gradually diminish for ZIP codes farther away from the location of the IPO, we find a modest and statistically insignificant increase in home prices for ZIP codes located more than five miles from the headquarters of the listed firm.

Finally, our results suggest that the change in home prices is because of the stock's *listing*, distinct from just raising capital. To separate these two hypotheses, we focus on SEOs because they resemble IPOs in terms of raising equity capital for the firm without involving a change in listing status. Even though the average equity proceeds from an SEO are approximately 47% larger than the proceeds of an average IPO, we do not find any evidence that SEOs have a direct impact on home prices.

If the increase in home prices we observe is due to the IPO, we suspect the effect should be more pronounced for expensive houses in the area. It is. In Table 3, we find evidence that IPOs have a positive impact on the price of expensive homes only. From column 3 in regression (1), we find that after an IPO, houses in a ZIP code that is centered within a two-mile radius from the ZIP code of the IPO increase by 67 basis points. This translates to an additional $3,900 in the price for an expensive house (the average price is $590,000). The IPO does not affect the prices of low- and mid-tier homes within zero to two miles. The geographic reach of large IPOs (top quartile of proceeds in that year) is extensive; the value of expensive homes increases by 92 basis points ($5,400) if located within two miles of the IPO, or 65 basis points ($3,800) if located between two and five miles. Even though we find that IPOs have a significant impact on local real estate markets, we do not find evidence that they affect home prices in the IPO headquarters' ZIP code.

[Insert Table 3 Here]

The increase in demand for housing in neighborhoods close to a newly listed firm should also lead to an increase in demand for mortgages to finance those homes.[9] To test

[9] Even Mark Zuckerberg, the CEO of Facebook, after the IPO refinanced his $5.95 million mortgage for his house located 3 miles from Facebook's headquarters (https://www.cnbc.com/id/48220824).

this hypothesis, we calculate the growth in the number of mortgage applications, the total mortgage amount, and the average mortgage amount in ZIP codes close to the IPO firm's headquarters. The results in Table 4 show that an IPO leads to a 0.6% to 1.3% ($1,700-$3,700) increase in the average mortgage amount for new mortgages. Taken together, the evidence in this section suggests that IPO activity has a meaningful effect on local housing markets.

[Insert Table 4 Here]

B. IPO activity, local employment, and local business development

The large effect of IPO activity on local housing markets suggests that IPOs likely have a broader effect on local economies. Recent literature suggests that houses, which can be used as collateral, affect local employment and business development by easing financial constraints. In two recent papers, researchers show the effect of house prices on employment. Schmalz, Sraer, and Thesmar (2017) show that an 11% increase in home prices in France is associated with a 4% increase in employment for firms in their sample. Mian and Sufi (2014) show that counties with the largest home price declines during the financial crisis experience the largest decrease in employment. Adelino, Schoar, and Severino (2015) show that MSAs with the highest increase in home prices during the 2002 to 2007 housing boom experienced a larger increase in establishment growth. Babina, Ouimet, and Zarutskie (2017) find a positive association between IPO activity and subsequent firm creation by employees who move to startup companies.

We test whether IPO activity is associated with increases in local employment and business establishments. We start by regressing employment growth and establishment growth of a ZIP code on its proximity to an IPO. The results in columns 1 and 2 in Table

5 suggest that IPOs have a positive impact on the employment growth in the ZIP code of the IPO headquarters. The effect is economically large: employers create roughly 150-190 new jobs per year (an increase of 65 to 78 basis points) close to the firm's headquarters. This result is consistent with Kenney, Patton, and Ritter (2012), who find that post-IPO firm employment increases by approximately 200 people per year. However, the IPO firm is unlikely to be responsible for all of the increase in local employment growth. Large firms also have employees outside their headquarters, suggesting that the IPO firm is not responsible for all of the 190 additional employees in the ZIP code as we find. According to Borisov, Ellul, and Sevilir (2017), the causal effect of an IPO on firm employment (compared to similar firms that do not go public) is approximately 50 new employees. Taken together, these results suggest that although the IPO firm contributes significantly to the ZIP code's abnormal increase in employment, a significant portion comes from IPO spillovers.[10]

[Insert Table 5 Here]

An IPO's effect on the local growth of employment or new businesses could be because the new businesses support the newly listed IPO firm or because they support people who live nearby. Growth of establishments in the tradable sector more likely reflects a response to the IPO firm and its growth. Local demand from individual people is more likely to affect the growth of establishments in the retail sector (non-tradeable), such as restaurants or in construction. We classify the tradable, non-tradable, and

[10] When we exclude spinoffs from our sample, employment grows in nearby ZIP codes as well. In terms of numbers, in ZIP codes 0-2 miles from the ground-zero ZIP code where the IPO firm's headquarters is, employment growth is on average 60 basis points or, roughly, an additional 148 people when we aggregate over all ZIP codes in that distance for the non-spinoff sample. We argue this employment increase is coming from outside the IPO firm.

construction sectors as Mian and Sufi (2014) do and calculate the change in the total number of establishments of each sector. The results in Table 6 suggest that IPO activity has a large positive effect on non-tradable business establishments and construction—those that likely serve demand from individual people—but the effect of the IPO on local growth of tradable sector establishments—those that might support business operations—is indistinguishable from zero.

Our results are consistent with previous studies that find significant spillover effects from manufacturing plant openings or closures. For instance, Bernstein, Colonnelli, Giroud, and Iverson (2018) find that corporate bankruptcies have a negative spillover effect on local employment, especially in the non-tradable sector and services. Large industrial plants have a positive effect on local economic development and productivity, despite the size of government subsidies to attract investment (see Greenstone and Moretti (2003), and Greenstone, Hornbeck, and Moretti (2010)). Moretti (2010) estimates that for each additional job in manufacturing (skilled professions), there are 1.6 to 2.5 new jobs in the non-tradable sector. More studies report similar estimates from other public expenditures, such as building NFL stadiums (Carlino and Coulson (2004)) or sponsoring the Olympic games (Kavetsos (2012)).

We, too, find spillover effects, but we view our contribution as inherently distinct from the above. Opening or closing manufacturing plants, stadia, or other establishments reflects direct investment (or disinvestment) and employment in the local economies, whereas the change in listing status we study is a financial transformation that does not involve the creation (or dissolution) of a new (existing) company. In this sense, our study

relates more to the debate on whether equity markets are a sideshow (Morck, Shleifer, Vishny, Shapiro, and Poterba (1990) suggests they are not).

[Insert Table 6 Here]

Mian, Rao, and Sufi (2013) find a strong positive relation between changes in housing net worth and consumption. Using Experian data on credit card spending, we show that IPOs affect local consumer demand. In Table 7, we regress credit card spending growth on each ZIP code's proximity to an IPO. The results in columns (1) and (2) suggest that, even though credit card spending does not increase in ZIP codes where IPOs take place, ZIP codes within two miles and within two to five miles experience a large increase in spending. The increase in spending is not trivial; growth of credit card spending increases by 1.3% to 3.6%, which is equivalent to a 10% to 26% increase in the average annual growth, or $240 to $640 in spending per year.

[Insert Table 7 Here]

C. Liquidity and wealth effects of IPOs

The change in the listing status of the firm is a significant market liquidity event for the employees and shareholders of a firm. After the change in its listing status, however, fluctuations in the stock price of the firm also affect the wealth of its employees and early investors. We test whether changes in liquidity, changes in wealth, or change in both factors are associated with an IPO spillover effect.

To test this hypothesis, we exploit the heterogeneity of the characteristics of an IPO. To identify a liquidity effect from an IPO, we exploit the timing of the expiration of the lock-up period, allowing investors to sell their shares. If changes in investor liquidity cause the spillover effects, the effect should be larger after the lockup period expires. If investor

wealth causes the spillover effects, an increase in the firm's stock price above its offer price should also have a significant effect on the local economy.

There are important technical challenges in testing these hypotheses empirically. Most IPOs have a six-month lockup period, but three- or nine-month or staggered lockup periods are not rare. To exploit the timing of the lockup expiration, we need an outcome variable that is measured at a granular time-series frequency. Zillow's monthly data on ZIP code level home prices satisfy this requirement. We construct a dependent variable that measures the abnormal house price growth close to each IPO firm's headquarters. Specifically, we calculate the difference in the monthly average home price growth of ZIP codes within five miles of the IPO headquarters and ZIP codes between five and 20 miles of the IPO headquarters.

The treatment effect in this sample comes from the IPO timing and the timing of the expiration of the lockup. We define four time periods: the first period, before the IPO filing date, serves as our control period; the second (*Post Filing Date*) is a dummy variable that equals 1 for months after the IPO filing date; the third (*Post Issue Date*) is a dummy that equals 1 for months after the first trading date; the fourth (*Post Lockup*) is a dummy that equals one for months after the lockup expiration. We collapse the time-series information of the monthly differences in home price growth into these four periods to address the issue that differences-in-differences regressions tend to over-reject the null of no effect, especially when the outcome variables are serially correlated (Bertrand, Duflo, and Mullainathan (2004)).

In Table 8, the dependent variable is the difference in average top-tier home price growth in ZIP codes located within five miles of the IPO headquarters' ZIP code from that

of ZIP codes between five and 20 miles of the IPO headquarters (the control). The results in column 1 suggest that after the IPO filing date, prices in ZIP codes within five miles of the IPO headquarters' ZIP code increase by an additional five basis points per month (60 basis points annually) compared to ZIP codes in the same county and year located between five and 20 miles from the IPO headquarters. Consistent with the hypothesis that investor liquidity causes IPO spillover effects, home price growth increases by five basis points per month after the expiration of the lockup period.

[Insert Table 8 Here]

To test the hypothesis that changes in investors' wealth drive the change in spillover effects, we identify firms whose stock price in the 12-month period after the lockup expiration is, on average, above the offer price (*Price Increase*). The results in column (2) suggest that, if after the lockup period the stock price exceeds (on average) the offer price, the difference in home price growth of ZIP codes located within five miles is additionally eight basis points higher compared to ZIP codes between five and 20 miles from the IPO headquarters' ZIP code. Including the interaction term *Post Lockup(X)Price Increase* has no effect on the coefficients of *Post Filing Date* or *Post Lockup*. Taken together, these results suggest that both investor liquidity and wealth help explain the local economic spillover effects.

As a placebo test, we compare the effects of regular IPOs to buyout-backed IPOs. The latter have investors who are less likely to be company employees and/or local to the company headquarters. In column (3), we regress differences of home price growth on an indicator variable that identifies whether the IPO was buyout-backed and its interaction with post-filing, post-issue, and post-lockup period variables. The interaction term *Post*

File(X)Buyout is negative, suggesting that the lack of local investors in buyout-backed IPOs mutes the spillover effects and supports our interpretation that local investors drive IPO effects.

D. Gentrification effect of IPOs

Our findings suggest that IPOs create positive spillover effects to real estate markets and local economies. Even though the spillover effects are broad and have a positive effect on local economies as a whole, the effect may be asymmetric across individual people with different income levels. For example, our baseline results suggest that IPOs affect only the market for high-priced homes in the area. This result suggests that lower-income people who own low-priced houses do not benefit from IPOs as much as higher-income individuals.[11] Therefore, we hypothesize that IPO activity could crowd out low-income people, especially tenants, inducing them to move to more affordable neighborhoods.

We test whether IPO activity increases the likelihood that people move out of the IPO area to more affordable neighborhoods in the next two years. The Experian panel dataset (2005 to 2015) allows us to observe the ZIP code where people reside each year and to infer when they move from one ZIP code to another. The median person in our sample has lived in two different ZIP codes during this time. We believe that for people living in apartments or multi-unit complexes, it is easier to move in response to local housing market conditions than it is for those who own their homes. Thus, we focus on these people and use homeowners of single family residences as a placebo.

[11] See the following article from the *Wall Street Journal*, "San Francisco Has a People Problem" https://www.wsj.com/articles/san-francisco-has-a-people-problem-1521691260.

To create the dependent variables in our regressions, we divide all ZIP codes in the US into four groups based on average household income or median rent prices. The resulting dependent variable (migration) is an indicator that equals 1 if a person moves to a lower-rent or lower-income ZIP code following an IPO and 0 otherwise. The probability that a person will move to a different ZIP code in a given year is 12.4%, which is consistent with migration statistics from the US Census Bureau.[12] The unconditional probability that low-income people living in apartments or complexes will move to ZIP codes with lower average income is 11%, and with lower rent is 3.5%.

Our findings in Table 9, column (1) show that IPOs increase the probability that people in the lowest quartile of the income distribution move to lower-income ZIP codes. Our findings in column (2) also suggest that low-income people move to neighborhoods with lower average rents. We find no evidence that heavy SEO activity reliably affects the tendency to migrate to other ZIP codes.

[Insert Table 9 Here]

IV. Incremental Impact of IPO Activity: Intensive Margin Tests

In our main empirical analysis, we focus on extensive margin tests that allow us to test whether IPOs create significant economic spillover effects on their local economies. To examine the intensive margin of IPO activity, we focus on economies with non-zero IPO activity. Because of the geographical spillovers we document above and because the geography of a county is large enough to include the spillover effects, the most

[12] "U.S. Mover Rate Remains Stable at About 12% Since 2008, Census Bureau Reports" Release Number: CB15-47.

straightforward way to estimate the intensive margin of IPOs is a county-year panel. Using the non-zero IPO activity county-years, our sample size is between 1,300 and 1,900 county-years depending on the test. In all these tests, we retain our control variables and include county fixed effects and year fixed effects.

We take two approaches to quantifying IPO activity in these tests—natural log of proceeds, and decile of proceeds—each of which offers a slightly different interpretation. Specifically, in the regressions below, $\Delta(\bar{Y})$ is the average yearly difference in the outcome variable Y in the two-year period after an IPO in the county, and X is a vector of county level controls (population, population density, per capita income, and unemployment).

(2) $$\Delta(\bar{Y})_{i,t+1} = \beta(Ln(IPO\ Proceeds))_{i,t} + X_{i,t} + \alpha_i + a_t + \epsilon_{i,t+1}$$

(3) $$\Delta(\bar{Y})_{i,t+1} = \beta(Decile\ of\ IPO\ Proceeds)_{i,t} + X_{i,t} + \alpha_i + a_t + \epsilon_{i,t+1}$$

The results from regressing outcome variables on Ln(proceeds), regression (2), suggests that doubling IPO proceeds from say, 180 (the mean) to 360 million, will have an average effect of increasing employment in the county by 628, establishments by 10, and home values by $1,228 per home for high-end homes.[13] Thus, when evaluated at the mean, the average number of new employees per $1 million of IPO proceeds is about 628/180 = 3.5, and the average number of new establishments is 10/180 = 0.06. Continuing with these intensive margin tests, in regression (3), we rank each county with non-zero IPO activity within a year and put them in decile bins, such that the 10% of counties with the

[13] These dollar figures are not inflation adjusted, but our time fixed effects absorb inflation effects.

highest activity in terms of aggregate IPO proceeds are in bin 10, and the 10% of counties with the lowest (but still non-zero) IPO activity are in bin 1.[14]

Using regression (3), we regress our outcome variables on this 1-to-10 variable of IPO intensity. The estimates suggest that moving from proceeds bin *i* to bin *i*+1 on average in the county increases employment by 323, establishments by 5.6, and the per-home value of high-end homes by $797. When evaluated at the bin 7 (mean proceeds = $111 million) to bin 8 (mean proceeds = $179 million, closest to the overall county average) transition, the average number of new employees per $1 million of IPO proceeds is about 323/68 = 4.7, and the average number of new establishments is 5.6/68 = 0.08.

We interpret these calculations to suggest that for "normal" IPO activity (i.e., near the mean level of proceeds), each additional $1 million of proceeds creates between 3.5 and 4.7 new employees county-wide and between 0.06 and 0.08 new establishments. Based on our estimates from our ZIP code-year regressions, on average, approximately 69% of these new employees are in the IPO firm's ZIP code.[15]

[Insert Table 10 Here]

Coupled with the previous tests in the paper, this finding suggests that there is not only a direct effect of an IPO on the local economy, but there is also a greater effect the larger is the IPO. Intensive margin tests that use change in building permits or change in

[14] We find similar results using proceeds, but prefer a logged transformation because logged proceeds is less subject to problems that could arise from outliers and the skewness of the distribution. In addition, large IPOs may have employees split among multiple locations, making our estimates from local spillover effects more conservative.

[15] We measure the share of employment growth in the county across different ZIP codes, using as an independent variable the total amount of proceeds in the closest ZIP code. We provide the details of the regression and the results for these estimates in the online appendix, section A.III.

mortgage amounts are not reliably distinguishable from zero, suggesting that the IPO effect is only at the extensive margin for those variables.

V. Placebo Tests and Instrumental Variables Approaches

A. Placebo IPOs

Our identifying assumption is that cross-sectional differences in real estate, employment, and establishment growth of ZIP codes in the same county-year should be approximately the same if there is no IPO activity in that county. By performing the following placebo tests, we evaluate whether the relationship we observe in the data is spurious. We take all our treated ZIP code-years (those in which there was an IPO) and we (counterfactually) assign a random (placebo) year to each ZIP code; we retain the matched control ZIP codes, which we assign the same placebo year, and we re-estimate our regressions. Because these ZIP codes experience only placebo IPOs, there should not be a significant IPO effect on the counterfactually assigned dates.

We present the regression results from the placebo tests of real estate variables and economic development in the online appendix. The results suggest that placebo IPOs do not create statistically significant changes in home price growth, employment growth, establishment growth (tradable, non-tradable, or construction) or credit card spending between ZIP codes in a given county-year. We conclude that our results are unlikely to be a spurious result of ZIP code-specific characteristics.

B. An off-the-shelf instrumental variable approach is unsatisfying

One approach to establishing causality would be to use an instrumental variable (IV) to generate quasi-random variation in the IPO process. Busaba, Benveniste, and Guo

(2001) show that poor market returns during the thirty days after an IPO's filing period are a useful predictor of whether the company will withdraw the IPO after the filing. Between 15% and 20% of IPOs filed are ultimately withdrawn. Bernstein (2015), studying whether public/private status impacts a firm's innovation, uses 60-day market returns from the filing date to instrument for IPO activity: predicting IPO completion (as opposed to withdrawal) with market returns in a first-stage regression, he can generate plausibly exogenous variation in whether a given company becomes public or stays private. Likewise, Babina, Ouimet, and Zarutskie (2017) use the same instrument to examine whether an IPO firm's employees leave for start-up firms.

But at least in a geographic, rather than firm-level setting, the instrument fails several criteria for validity. A study by Cornaggia, Gustafson, Kotter, and Pisciotta (2018) uses this instrument in a geographic setting and concludes that IPOs have a startlingly large negative effect on employment and income growth in their county. We conclude, though, that these large negative instrumental variable estimates stem from econometric, rather than economic, reasons, primarily due to a weak instrument that violates the necessary exclusion restriction. One source of the econometric problem is that the first-stage-adjusted R-squared is quite low (at a maximum, 3.2% according to Table 3 in Cornaggia, et al. (2018)). We independently confirm a low first-stage-adjusted R-squared.[16] Perhaps the fact that the instrument fails to predict IPO withdrawals in the period after the dot-com bubble causes this lack of explanatory power. Numerous researchers suggest that a non-

[16] We include regressions of IPO completion on 2-month NASDAQ returns for various periods in section A.IV found in the online appendix.

exogenous instrument combined with a low R-squared in the first stage can yield economically implausible estimates.[17]

More importantly, however, the instrument likely fails the exclusion restriction and amplifies a negative bias in OLS estimates. First, NASDAQ returns do not affect local economies only through completed IPOs; they also affect them through acquisition activity and the wealth of shareholders in general. More than 50% of the firms that withdraw from an IPO are acquired within a few years (Cooney, Moeller, and Stegemoller (2009)). Second, there are significant factors that affect the decision to complete or withdraw an IPO—such as an active IPO market, venture-capital backing, and underwriter reputation—that are also correlated with the firm's location (Busaba, Benveniste, and Guo (2001) and Dunbar and Foerster (2008)). Moreover, fewer than 10% of the firms that withdraw an IPO file again, suggesting that market/macroeconomic conditions explain only a small fraction of a firm's decision to withdraw and that the instrument uses variation that is not as good as random. These findings cast further doubt of using withdrawn IPOs as a valid counterfactual.

No identification is perfect; however, as we argue throughout the paper, an empirical design that relies on constructing a careful counterfactual by exploiting nuanced differences in proximity to an IPO is more compelling in our economic setting than applying an off-the-shelf IV that does not satisfy the exclusion restriction. Moreover, our

[17] Jiang (2017) notes that F-statistics easily exceed the critical values in Stock and Yogo (2005) in large samples, raises concerns over the IV when R^2 is less than 2%, and stresses that weak IV tests are not informative when the IV violates exogeneity. Hahn and Hausman (2003) formalize this argument and argue that if the IV is even slightly correlated with the disturbance, a low R^2 leads to a large amount of bias. They conclude: "Our findings highlight the result that when R^2 is low (below 0.1) OLS may do better than 2SLS". See also Atanasov and Black (2017).

findings are intuitive and consistent with previous theory and empirical literature that discusses the positive effects of liquidity or wealth shocks on local real estate markets, employment, and business development.

VI. Conclusion

We estimate the spillover effects from an IPO to the local economy in the form of increases in local labor, business, and real estate outcomes. We exploit nuanced geographical distances of a ZIP code from the IPO headquarters to identify the extensive margin and the intensive margin of the IPO spillover effects. We find that each additional $10 million of proceeds leads to between 35 and 47 new jobs and between 0.6 and 0.8 new businesses in the county; IPO firms, which raise on average $180 million, create approximately 630 to 850 new jobs and 10 to 15 new businesses per year in the county over the subsequent two years. Some of the new jobs are at the IPO firm itself, and some are created from the "multiplier effect." These economic spillovers are non-trivial and translate into economically significant increases in the annual growth rate of establishments and employment of 7% and 15%, respectively. There may be additional effects if an IPO firm has operations in other counties or uses proceeds to expand geographically.[18]

Our findings relate to previous studies that document economic spillovers from opening a new manufacturing plant or other forms of corporate investment. However, this

[18] We note that our empirical design of using county-year fixed effects absorbs geographic and time series variation in the nature of the firms that go public. Our tests do not speak to across-industry variation in the effect of a local firm going public. Whether listings of firms in tradable sectors, like restaurant chains, or non-tradable sectors, like biotech firms, have different effects on the local economy and on local businesses (Kutsuna, Smith, Smith, and Yamada (2016)) is a question we leave for further research.

literature and our study differ in an important way: an IPO does not create a new firm; an IPO simply changes a firm's listing status, provides access to the equity market, and injects money into the firm. There appears to be something special about the change in listing status, because we do not find measurable economic spillover effects following seasoned equity offerings. Thus, it is unlikely that most of the effect we find arises from merely capital raising. In this respect, this paper relates to studies that investigate the role of financial markets on economic wealth and growth and contributes to the debate on whether the stock market is a sideshow. Some studies argue that the development of equity markets does not affect local economic development (Karolyi (2004)) or even has a large negative effect (Cornaggia, et al. (2018)). In contrast, our evidence suggests that when firms gain better access to financial markets, there are positive spillover effects to the local economy. Our evidence on increased credit card spending suggests a plausible channel for this positive spillover effect: employees are able to convert illiquid paper wealth into cash and then spend some of their wealth, boosting the local economy.

Moreover, our results on the effects of an IPO on consumer spending, real estate prices, and gentrification are, to our knowledge, the first in the literature. Our evidence, therefore, suggests that IPOs affect local economies through an increase in demand for local goods and services in retail and construction. Academics and regulators have increasingly paid attention to the broad issue of the real effects of IPOs (and their declining number) as well.[19] Finally, our findings also provide empirical support to popular press

[19] For example, see: https://www.sec.gov/news/speech/opening-remarks-sec-nyu-dialogue-securities-market-regulation-reviving-us-ipo-market.

articles that discuss the ongoing debate over the local economic benefits of attracting a firm's headquarters.

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Table 1: Summary Statistics. This table presents the summary statistics for variables related to ZIP code and IPO. In the list of IPO variables in Panel A, we calculate the total number of IPOs (IPO #) and sum of all the proceeds from IPOs in the ZIP code (IPO Proceeds) in a given year, conditional on having at least one IPO. We also calculate the total number of large IPOs (Large IPO #) and sum of all the proceeds from large IPOs (Large IPO Proceeds) in the ZIP code (large IPO is an IPO whose proceeds are in the top quartile of proceeds of all IPOs in a given year). Panel A includes the number of seasoned equity offerings (SEO #) and the sum of the proceeds of the SEO issues (SEO Proceeds) in a given year. Our sample period is 1998 to 2015, excluding years 1999, 2000, and 2003. Panel B shows the set of economic variables we use as controls or dependent variables in our sample. Wage income is the average per capita wage income reported in individual tax returns from the IRS. Employment is the number of employed people in the ZIP code, and establishments is the total number of businesses in the ZIP code. The home price index is from the Federal Housing Finance Agency and is based on 2000 (=100) home prices for each ZIP code. Mortgage amount is from HMDA and represents the total amount of mortgages originated in the ZIP code divided by the total number or approved applications. Credit card spending represents the average dollar amount that people living in the ZIP code spent using their credit cards. In panel C, columns (1) and (2) present the average characteristics (in the prior year) for ZIP codes with no IPO activity (IPO=0) and with at least one IPO (IPO HQ ZIP Code), respectively. The third column ((2)-(1)) shows univariate differences for the economic variables, and the last column shows univariate differences in terms of their standard deviation (normalized).

Panel A: ZIP Code IPO Characteristics (for ZIP Codes with IPO HQ ZIP Code)

	N	Mean	SD	P10	P50	P90
IPO #	2,426	1.24	0.72	1.00	1.00	2.00
IPO Proceeds	2,426	130.63	283.64	20.00	70.59	279.30
Large IPO #	826	1.36	0.90	1.00	1.00	2.00
Large IPO Proceeds	826	266.57	450.61	68.00	170.19	510.00
SEO #	2,759	1.59	1.22	1.00	1.00	3.00
SEO proceeds	2,759	192.48	236.37	17.42	90.75	587.76

(Table 1 – continued)

Panel B: ZIP Code Economic Characteristics

	N	Mean	SD	P10	P50	P90
Population (thous.)	10,661	30.06	14.60	12.93	28.43	49.03
Wage Income (thous.)	10,661	60.24	29.40	31.13	53.81	96.65
Employment (thous.)	10,661	24.61	16.64	6.99	20.79	47.72
Establishments (thous.)	10,661	1.20	0.64	0.44	1.12	2.10
Home Price Index	10,651	138.99	34.46	99.21	135.10	184.54
Mortgage Amount (thous.)	9,720	280.03	145.05	128.97	253.74	455.28
Cr. Card Spending (thous.)	6,642	18.22	12.53	7.26	14.45	33.82
%Δ(HPI)	9,850	3.19	7.19	-5.47	3.07	12.69
%Δ(Employment)	9,851	1.39	6.79	-5.72	1.16	8.04
%Δ(Establishments)	9,886	0.97	3.02	-1.96	0.71	3.92
%Δ(Income)	8,395	3.17	5.13	-2.48	3.04	8.92
%Δ(Wage Income)	8,395	2.78	3.19	-0.50	2.60	6.52
%Δ(Mortgage Amount)	7,927	3.91	8.91	-5.57	3.38	13.47
%Δ(Cr. Card Spending)	5,456	13.27	21.94	-8.92	8.10	44.20

Panel C: IPO vs non-IPO ZIP-years: Univariate Differences

	(1) Treated: At least one IPO in the ZIP-year	(2) Matched Controls: No IPOs in the ZIP-year	(2)-(1) Difference	(2)-(1) Difference (normalized)
Employment	24,749	24,596	-153	-0.011
Establishments	1,201	1,205	4	0.001
Wage Income	60,710	60,180	-530	-0.029
Home Price Index	136.81	139.50	2.69	0.062
Cr. Card Spending	19,123	18,177	-946	-0.079
Mortgage Amt.	276,930	281,220	4,290	0.022

Table 2: Regressions of home price growth on ZIP code distances from an IPO. The dependent variable is the average yearly growth rate of the home price index in the two-year period after an IPO in the ZIP code (source Federal Housing Finance Agency). A coefficient of 1 indicates a 1% change in the dependent variable. *IPO HQ ZIP Code* is a dummy variable indicating if the headquarters (HQ) of the IPO firm is in that ZIP code. *Large IPO HQ ZIP Code* is a dummy variable that indicates if the headquarters of the IPO firm is in that ZIP code and its proceeds are in the top quartile of the yearly distribution of proceeds. The IPO proximity variables indicate ZIP codes with no IPO activity in that year but are between either zero and two, two and five, or five and 10 miles away from the closest ZIP code with at least one IPO in the same county-year. We control for SEO activity in the ZIP code and include the first lag of the dependent variable, the number of establishments, employment, ZIP code population, population density, and wage income. The regressions include county-year fixed effects. We cluster at the ZIP code and county-year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1) Home Price Index Growth	(2) Home Price Index Growth
IPO HQ ZIP Code	0.052	
	(0.121)	
<0-2 miles of IPO HQ	0.311*	
	(0.181)	
2-5 miles from IPO HQ	0.168	
	(0.112)	
5-10 miles from IPO HQ	0.042	
	(0.081)	
Large IPO HQ ZIP Code		0.083
		(0.154)
<0-2 miles from Large IPO HQ		0.884***
		(0.241)
2-5 miles from Large IPO HQ		0.649***
		(0.153)
5-10 miles from Large IPO HQ		0.151
		(0.093)
ZIP SEO>0	0.028	0.024
	(0.082)	(0.083)
Ln(Population)	-0.238***	-0.221**
	(0.088)	(0.092)
Ln(Establishments)	0.263	0.244
	(0.173)	(0.180)
Ln(Employment)	0.042	0.032
	(0.102)	(0.104)
Ln(Wage Income)	0.821***	0.801***
	(0.143)	(0.142)
Lagged Dependent Variable	7.453***	7.242***
	(2.740)	(2.753)
Population Density	0.130***	0.123***
	(0.012)	(0.012)
Observations (ZIP-years)	9318	9318
Adjusted R^2	0.917	0.918
County-year Fixed Effects	Yes	Yes

Table 3: Regressions of home price growth on ZIP code distances from an IPO. The dependent variable is the average yearly growth rate of home values in the two-year period after an IPO in the ZIP code (source Zillow). A coefficient of 1 indicates a 1% change in the dependent variable. Bottom-, middle-, and top-tier houses are houses in the lowest, middle, and top tercile of house prices in the ZIP code. *IPO HQ ZIP Code* is a dummy variable indicating if the headquarters (HQ) of the IPO firm is in that ZIP code. *Large IPO HQ ZIP Code* is a dummy variable that indicates if the headquarters of the IPO firm is in that ZIP code and its proceeds are in the top quartile of the yearly distribution of proceeds. The IPO proximity variables indicate ZIP codes with no IPO activity in that year but are between either zero and two, two and five, or five and 10 miles away from the closest ZIP code with at least one IPO in the same county-year. We control for SEO activity in the ZIP code and include the first lag of the dependent variable, the number of establishments, employment, ZIP code population, population density, and wage income. The regressions include county-year fixed effects. We cluster at the ZIP code and county-year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	Home Price Growth – All IPOs			Home Price Growth – Large IPOs		
	Bottom-Tier	Middle-Tier	Top-tier	Bottom-Tier	Middle-Tier	Top-tier
IPO HQ ZIP Code	-0.201	-0.068	0.092			
	(0.139)	(0.137)	(0.141)			
<0-2 miles of IPO HQ	0.014	0.082	0.674***			
	(0.242)	(0.224)	(0.209)			
2-5 miles from IPO HQ	-0.049	0.034	0.188			
	(0.133)	(0.129)	(0.121)			
5-10 miles from IPO HQ	-0.052	-0.053	0.053			
	(0.101)	(0.010)	(0.095)			
Large IPO HQ ZIP Code				-0.158	-0.012	0.293
				(0.232)	(0.211)	(0.201)
<0-2 miles from Large IPO HQ				0.301	0.184	0.922***
				(0.504)	(0.357)	(0.346)
2-5 miles from Large IPO HQ				0.225	0.363**	0.649***
				(0.210)	(0.158)	(0.171)
5-10 miles from Large IPO HQ				0.091	0.081	0.260**
				(0.144)	(0.110)	(0.112)
ZIP SEO>0	-0.021	0.088	0.128	-0.022	0.080	0.123
	(0.104)	(0.082)	(0.082)	(0.104)	(0.078)	(0.091)
Ln(Population)	-0.124	-0.175	-0.152	-0.107	-0.167	-0.145
	(0.132)	(0.111)	(0.092)	(0.128)	(0.113)	(0.094)

Ln(Establishments)	0.145	0.233	0.453**	0.142	0.224	0.434**
	(0.202)	(0.181)	(0.180)	(0.198)	(0.185)	(0.184)
Ln(Employment)	-0.014	-0.050	-0.141	-0.012	-0.054	-0.136
	(0.121)	(0.103)	(0.099)	(0.119)	(0.102)	(0.101)
Ln(Wage Income)	0.815***	0.802***	0.153	0.802***	0.794***	0.149
	(0.192)	(0.211)	(0.181)	(0.191)	(0.209)	(0.178)
Lagged Dependent Variable	5.862**	6.552**	9.653***	5.824**	6.441**	9.501***
	(2.770)	(3.181)	(3.178)	(2.784)	(3.192)	(3.187)
Population Density	0.093***	0.092***	0.109***	0.093***	0.094***	0.106***
	(0.022)	(0.022)	(0.015)	(0.017)	(0.023)	(0.021)
Observations (ZIP-years)	7577	7831	7784	7577	7831	7784
Adjusted R^2	0.920	0.927	0.918	0.920	0.927	0.918
County-year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table 4: Regressions of mortgage origination growth on ZIP code distances from an IPO. In regression (1), the dependent variable is the yearly growth of average mortgage amount, defined as the total mortgage amounts in the ZIP code divided by the number of accepted applications. In regression (2), the dependent variable is the growth of total mortgage amounts in the ZIP code. In regression (3), the dependent variable is the growth in the total number of accepted mortgage applications in the ZIP code. Growth rates are over the two-year period after an IPO in the ZIP code. A coefficient of 1 indicates a 1% change in the dependent variable. *Large IPO HQ ZIP Code* is a dummy variable that indicates if the headquarters of the IPO firm is in that ZIP code and its proceeds are in the top quartile of the yearly distribution of proceeds. The IPO proximity variables indicate ZIP codes with no IPO activity in that year but are between either zero and two, two and five, or five and 10 miles away from the closest ZIP code with at least one IPO in the same county-year. We control for SEO activity in the ZIP code and include the first lag of the dependent variable, the number of establishments, employment, ZIP code population, population density, and wage income. The regressions include county-year fixed effects. We cluster at the ZIP code and county-year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** ($p<0.01$), ** ($p<0.05$), and * ($p<0.10$).

	(1) Avg. Mortgage Amount Growth	(2) Total Mortgage Amount Growth	(3) Mortgage Applications Growth
Large IPO HQ ZIP Code	1.147**	1.119	-0.132
	(0.501)	(1.657)	(1.391)
<0-2 miles from Large IPO HQ	1.333**	4.055*	2.800
	(0.582)	(2.321)	(2.019)
2-5 miles from Large IPO HQ	0.774**	2.449**	1.884*
	(0.331)	(1.168)	(1.007)
5-10 miles from Large IPO HQ	0.598**	0.289	-0.132
	(0.292)	(0.778)	(0.674)
ZIP SEO>0	-0.229	-0.467	-0.439
	(0.177)	(0.542)	(0.472)
Ln(Population)	-0.302*	-0.789	-0.305
	(0.167)	(0.641)	(0.536)
Ln(Establishments)	0.632*	0.523	0.189
	(0.321)	(1.312)	(1.178)
Ln(Employment)	-0.154	-0.120	-0.256
	(0.223)	(0.801)	(0.709)
Ln(Wage Income)	-2.443***	4.244*	4.634**
	(0.844)	(2.456)	(2.218)
Lagged Dependent Variable	-43.482***	-42.925***	-40.390***
	(2.064)	(4.663)	(4.514)
Population Density	0.184***	0.211*	0.053
	(0.028)	(0.111)	(0.094)
Observations (ZIP-years)	7421	7421	7421
Adjusted R^2	0.506	0.784	0.799
County-year Fixed Effects	Yes	Yes	Yes

Table 5: OLS Regressions of employment growth on ZIP code distances from an IPO. The dependent variable is the average yearly growth rates of employment in the two-year period after an IPO in the ZIP code. A coefficient of 1 indicates a 1% change in the dependent variable. *IPO HQ ZIP Code* is a dummy variable indicating if the headquarters (HQ) of the IPO firm are in that ZIP code. *Large IPO HQ ZIP Code* is a dummy variable that indicates if the headquarters of the IPO firm is in that ZIP code, and its proceeds are in the top quartile of the yearly distribution of proceeds. The IPO proximity variables indicate ZIP codes with no IPO activity in that year but are between either zero and two, two and five, or five and 10 miles away from the closest ZIP code with at least one IPO in the same county-year. We control for SEO activity in the ZIP code and include the first lag of the dependent variable, ZIP code population, population density, establishments, and wage income. All regressions include county-year fixed effects. We cluster at the ZIP code and county-year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** ($p<0.01$), ** ($p<0.05$), and * ($p<0.10$).

	(1) Employment Growth	(2) Employment Growth
IPO HQ ZIP Code	0.651**	
	(0.321)	
<0-2 miles of IPO HQ	-0.056	
	(0.377)	
2-5 miles from IPO HQ	-0.190	
	(0.219)	
5-10 miles from IPO HQ	-0.041	
	(0.191)	
Large IPO HQ ZIP Code		0.784*
		(0.437)
<0-2 miles from Large IPO HQ		0.598
		(0.701)
2-5 miles from Large IPO HQ		0.184
		(0.358)
5-10 miles from Large IPO HQ		0.279
		(0.261)
ZIP SEO>0	0.101	0.130
	(0.235)	(0.241)
Ln(Population)	0.036	0.053
	(0.182)	(0.168)
Ln(Establishments)	-1.184***	-1.171***
	(0.191)	(0.180)
Ln(Wage Income)	1.107***	1.084***
	(0.215)	(0.234)
Lagged Dependent Variable	-1.032	-0.918
	(2.552)	(2.559)
Population Density	-0.054***	-0.061***
	(0.021)	(0.020)
Observations (ZIP-years)	9284	9284
Adjusted R^2	0.162	0.161
County-year Fixed Effects	Yes	Yes

Table 6: Regressions of establishment growth (by trade sector) on ZIP code distances from an IPO. The dependent variables are, respectively, the average yearly growth rates of establishments in the two-year period after an IPO in the ZIP code (source Census ZIP code business patterns). A coefficient of 1 indicates a 1% change in the dependent variable. We calculate the number of establishments in tradable, non-tradable, and construction sector using their NAICS industry classification following Mian and Sufi (2014) (see Appendix Table A.VI). *IPO HQ ZIP Code* is a dummy variable indicating if the headquarters (HQ) of the IPO firm is in that ZIP code. The IPO proximity variables indicate ZIP codes with no IPO activity in that year but are between either zero and two, two and five, or five and 10 miles away from the closest ZIP code with at least one IPO in the same county-year. We control for SEO activity in the ZIP code and include the first lag of the dependent variable, the number of establishments, employment, ZIP code population, population density, and wage income. The regressions include county-year fixed effects. We cluster at the ZIP code and county-year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1) Establishments Growth Tradable Sector	(2) Establishments Growth Non-Tradable Sector	(3) Establishments Growth Construction
IPO HQ ZIP Code	-0.182	0.274	0.574**
	(0.284)	(0.219)	(0.240)
<0-2 miles of IPO HQ	0.110	0.824***	0.134
	(0.671)	(0.261)	(0.367)
2-5 miles from IPO HQ	-0.289	-0.150	-0.012
	(0.371)	(0.122)	(0.389)
5-10 miles from IPO HQ	-0.030	-0.025	0.602***
	(0.432)	(0.204)	(0.190)
ZIP SEO>0	0.177	0.501	0.431
	(0.506)	(0.302)	(0.279)
Ln(Population)	0.454	0.022	-0.162
	(0.329)	(0.188)	(0.284)
Ln(Establishments)	-1.956**	-1.494**	-1.671***
	(0.785)	(0.631)	(0.372)
Ln(Employment)	1.754**	0.412	0.714
	(0.654)	(0.310)	(0.516)
Ln(Wage Income)	1.401***	0.757**	0.383
	(0.360)	(0.246)	(0.376)
Lagged Dependent Variable	-18.580***	0.810	-21.011***
	(3.271)	(2.429)	(2.791)
Population Density	-0.142*	-0.014	-0.062
	(0.067)	(0.032)	(0.044)
Observations (ZIP-years)	8411	8586	8587
Adjusted R^2	0.055	0.114	0.224
County-year Fixed Effects	Yes	Yes	Yes

Table 7: Regressions of credit card spending growth on ZIP code distances from an IPO. The dependent variable is the average yearly growth rate of credit card spending in the two-year period after an IPO. A coefficient of 1 indicates a 1% change in the dependent variable. We construct ZIP code spending growth data from a large sample of people who live in the ZIP code (source Experian). *IPO HQ ZIP Code* is a dummy variable indicating if the headquarters (HQ) of the IPO firm is in that ZIP code. *Large IPO HQ ZIP Code* is a dummy variable that indicates if the headquarters of the IPO firm is in that ZIP code, and its proceeds are in the top quartile of the yearly distribution of proceeds. The IPO proximity variables indicate ZIP codes with no IPO activity in that year but are between either zero and two, two and five, or five and 10 miles away from the closest ZIP code with at least one IPO in the same county-year. We control for SEO activity in the ZIP code and include the first lag of the dependent variable, the number of establishments, employment, ZIP code population, population density, and wage income. The regressions include county-year fixed effects. We cluster at the ZIP code and county-year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** ($p<0.01$), ** ($p<0.05$), and * ($p<0.10$).

	(1) Credit Card Spending Growth	(2) Credit Card Spending Growth
IPO HQ ZIP Code	-1.077	
	(0.811)	
<0-2 miles of IPO HQ	3.071***	
	(1.069)	
2-5 miles from IPO HQ	1.368**	
	(0.651)	
5-10 miles from IPO HQ	0.194	
	(0.560)	
Large IPO HQ ZIP Code		0.473
		(1.224)
<0-2 miles from Large IPO HQ		3.214*
		(1.801)
2-5 miles from Large IPO HQ		3.561***
		(1.117)
5-10 miles from Large IPO HQ		1.763*
		(1.010)
ZIP SEO>0	0.004	-0.214
	(0.679)	(0.682)
Ln(Population)	-1.042**	-1.101**
	(0.499)	(0.481)
Ln(Establishments)	3.420***	3.336***
	(1.171)	(1.142)
Ln(Employment)	-0.980	-1.072
	(0.810)	(0.811)
Ln(Wage Income)	9.178***	9.130***
	(1.084)	(1.065)
Lagged Dependent Variable	-27.261***	-27.421***
	(1.741)	(1.729)
Population Density	0.345***	0.343***
	(0.094)	(0.093)
Observations (ZIP-years)	3729	3729
Adjusted R^2	0.614	0.614
County-year Fixed Effects	Yes	Yes

Table 8: Investor Liquidity versus Investor Wealth. The dependent variable is the difference in home price (top-tier) growth per month of ZIP codes within a five-mile IPO-range and the home price growth of ZIP codes within five to 20 miles IPO-range in a given county-year. A coefficient of 1 indicates a 1% change in the dependent variable. *Post Filing Date* is a dummy that equals 1 if the date is greater than the IPO filing date and zero otherwise. *Post Issue Date* (*Post Lockup*) is a dummy that equals 1 if the date is greater than the lockup expiration. *Price Increase* is a dummy that equals 1 if the (12-month) average stock market price is above the offer price. *Buyout IPO* is a dummy that equals 1 if the IPO was buyout-backed. We include as controls the natural log the following ZIP code control variables: population, population density, establishments, employment, and wage income. All regressions include industry (SIC-2) and year fixed effects. We cluster at industry (SIC-2) and year and report robust standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** ($p<0.01$), ** ($p<0.05$), and * ($p<0.10$).

	(1) Top-Tier Home Price Growth	(2) Top-Tier Home Price Growth	(3) Top-Tier Home Price Growth
Post Filing Date	0.052**	0.053**	0.053**
	(0.023)	(0.022)	(0.021)
Post Issue Date	0.013	-0.028	0.011
	(0.023)	(0.031)	(0.019)
Post Lockup	0.049***	0.044*	0.058***
	(0.019)	(0.023)	(0.021)
Price Increase		0.003	
		(0.021)	
Post Issue(X)Price Increase		0.024	
		(0.029)	
Post Lockup(X)Price Increase		0.081**	
		(0.034)	
Buyout IPO			0.022
			(0.054)
Post File(X)Buyout IPO			-0.045**
			(0.021)
Post Issue(X)Buyout IPO			0.030
			(0.068)
Post Lockup(X)Buyout IPO			-0.041
			(0.030)
Ln(Population)	-0.032***	-0.033***	-0.031***
	(0.010)	(0.011)	(0.010)
Ln(Establishments)	-0.011	-0.012	-0.014
	(0.021)	(0.021)	(0.022)
Ln(Employment)	0.042**	0.042**	0.044**
	(0.019)	(0.020)	(0.018)
Ln(Wage Income)	0.131***	0.134***	0.133***
	(0.022)	(0.020)	(0.021)
Population Density	0.089***	0.092***	0.093***
	(0.014)	(0.014)	(0.013)
Observations (Firm-period)	8973	8973	8973
Adjusted R^2	0.072	0.073	0.072
Industry, Year Fixed Effects	Yes	Yes	Yes

Table 9: Regressions estimating migration patterns. The ZIP code of a person's residence is from Experian (2005 to 2015). In columns (1) and (2), the dependent variable is an indicator variable that takes the value of 1 if within two years after the IPO the person moves to: (1) a ZIP code with a lower average income or (2) a ZIP code with lower average rent prices. A coefficient of 1 indicates a 1% change in the dependent variable. *Large IPO (SEO)* is an indicator variable that takes the value of 1 if the proceeds from the IPO (SEO) are in the top quartile of the distribution of IPO (SEO) proceeds. *Low Income* is an indicator variable that takes the value of 1 if the person's income is in the bottom quartile of the yearly distribution of incomes in that ZIP code. The sample is people living in an apartment, condo, or another multi-family unit. We include controls for the person's credit score and marital status. Median ZIP code rental prices are from Zillow (where available). All regressions exclude the years of the financial crisis (2007 to 2009). The regressions include ZIP code and year fixed effects. We cluster at the individual and year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** ($p<0.01$), ** ($p<0.05$), and * ($p<0.10$).

Dependent variable = 1 if:	(1) Individual moves to lower income ZIP Code	(2) Individual moves to lower rent ZIP Code
Large IPO HQ ZIP Code	-1.343	-0.974***
	(0.831)	(0.191)
Large SEO	-0.602	-0.321
	(0.621)	(0.360)
Large IPO * Low Income	1.489***	0.743*
	(0.545)	(0.391)
Large SEO * Low Income	1.054	-0.344
	(0.681)	(0.633)
Low Income	4.542***	1.050***
	(0.191)	(0.109)
Credit Score	-3.281***	-0.630***
	(0.229)	(0.061)
Single	2.438***	1.324***
	(0.321)	(0.132)
Observations (Individual-year)	644027	463256
Adjusted R^2	0.125	0.051
Year FEs	Yes	Yes
ZIP code FEs	Yes	Yes

Table 10: Intensive margin regressions based on a county-year panel of IPO activity. The dependent variables in the regressions are the average yearly difference (in levels) in employment (columns 1 and 4), establishments (columns 2 and 5), and home value for top-tier houses (columns 3 and 6) in the two-year period after an IPO in the county. *Ln*(*IPO Proceeds)* is the natural logarithm of the total amount (in $ millions) of IPO proceeds in the county in a given year. *IPO Proceeds deciles* is an integer from 1 to 10 and is equal to 1 (10) if IPO proceeds in the county are in the lowest (highest) decile of the yearly distribution of IPO proceeds. We include controls for county population (in thousands), population density, per capita income, and the number of unemployed people (in thousands). All regressions include county and year fixed effects. We cluster at the county level and present robust standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1) Δ(Employment)	(2) Δ(Establishments)	(3) Δ(Home Value-Top)	(4) Δ(Employment)	(5) Δ(Establishments)	(6) Δ(Home Value-Top)
Ln(IPO Proceeds)	627.899***	10.022**	1227.743***			
	(235.698)	(5.010)	(350.183)			
IPO Proceeds Deciles				327.682***	5.604**	796.989***
				(124.047)	(2.628)	(261.659)
Population	-0.687	-0.019	-2.285	-0.881	-0.022	-2.695
	(-0.361)	(-0.402)	(-0.822)	(-0.464)	(-0.456)	(-0.972)
Population Density	0.954	-0.092*	2.697	1.781	-0.069	2.991
	(2.203)	(-0.051)	(3.225)	(2.565)	(0.055)	(3.278)
Per Capita Income	-0.136	-0.008*	-0.755***	0.011	-0.004	-0.681***
	(-0.095)	(-0.004)	(-0.239)	(0.088)	(0.004)	(0.245)
Unemployed	52.403	1.099	59.708	38.891	1.223	69.482
	(1.376)	(0.509)	(0.857)	(1.631)	(0.566)	(1.180)
Observations (County-year)	1796	1796	1322	1796	1796	1322
Adjusted R^2	0.467	0.581	0.559	0.421	0.531	0.558
County, Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Online Appendix for:

"Local Economic Spillover Effects of Stock Market Listings"

Section A.I: Empirical design and methods

Section A.II: Reverse causality and omitted variables bias tests

Section A.III: Intensive margin – ZIP level

Section A.IV: 2-month NASDAQ returns as an IV to IPO completion

Section A.V: External Validity – MSA level regressions

Section A.VI: Placebo Tests

A.I: Empirical design and methods

In this section, we extend our discussion on our identifying assumptions and empirical strategy. Our main tests use a restrictive fixed effects approach to mitigate concerns of unobservable factors driving our results. Specifically, we use county-year fixed effects and examine the variation in economic outcomes across ZIP codes within the county and year of a company going public. Like all empirical designs, this approach has strengths and weaknesses. It does not address reverse causality (we have different tests, below, that address this issue). But our empirical design does control for a wide variety of time- and geography-varying factors. For an omitted variable to bias our coefficient estimates, it would have to vary only across ZIP codes, within county and within year, and be correlated with IPO activity but not our control variables. Our main identifying assumption is the following: in the absence of a significant local shock such as an IPO, the change in the economic development of nearby ZIP codes in a given year should be similar on average.

The design merits further explanation. First, we note that most counties have no IPOs, but some counties have lots of IPOs. It is not useful to study the local economic impact of an IPO in rural areas that have little economic activity and hence, no meaningful opportunity to have a company go public. Contrast Harris County, Texas and Brewster County, Texas. Harris County is the largest by population (4.5 million) in Texas; Brewster is the largest by land area (6,183 square miles). But Brewster County has only 280 establishments, and only one town with more than 1,000 people, Alpine. The likelihood of a company headquartered in Brewster County going public is quite small. We exclude from our sample such counties that never have an IPO. This design

choice of dropping counties like Brewster creates a bias: an IPO happening, albeit improbably, in a rural area might have a much larger effect on the local business environment than what we estimate with our sample. Moreover, this choice limits the external validity of our analysis to apply only to areas where a company going public is a real possibility. Thus, our results speak most to the effects of an IPO in counties ranging from a population of 70,000 and 1,200 establishments (10^{th} percentile in our sample), to a population of 1,000,000 and 29,000 establishments (90^{th} percentile). This approach leaves us with 295 counties, 2,219 county-years. We extend our tests, below, to regain some external validity by using a more inclusive sample that uses larger geographic units with less restrictive fixed effects and reach the same conclusions.

In a given county-year we examine the differential economic impact of the IPO across different ZIP codes, the most granular geographic unit for which we have ample data. ZIP codes are small: the typical county in a US state has more than a dozen ZIP codes. We exclude ZIP codes that never had an IPO from 1990-2015, making our treated and control samples more homogeneous. Our tests are best explained with a series of figures; we use Harris County, Texas as an example. Figure A.1 shows Harris County and its ZIP code boundaries. ZIP codes that are shaded are those that are in our sample; ZIP codes that are unshaded are "never treated" ZIP codes, and they, like our never-treated counties such as Brewster, are excluded from our tests. Matching treated ZIP codes to control ZIPs from our donor pool further mitigates concerns about unobserved heterogeneity.

[Insert Figure A.1 Here]

Our tests compare, within county-year, differences in outcomes for the shaded ZIP codes as a function of their proximity to a ZIP code that has at least one IPO in that county-year. Figure A.2 illustrates. The map highlights two ZIP codes that in 2004 had at least one IPO; ZIP code 77042 (west) had one firm going public, TODCO, and ZIP code 77056 (east) had two firms going public (WCA Waste Corporation and Westlake Chemical Corporation). The inner ring includes all ZIP codes whose center is within a two-mile radius from the center of the ZIP code that had an IPO, the middle ring includes ZIP codes between two and five miles, and the outer ring includes ZIP codes between five and ten miles from the center of the IPO. If a ZIP code is within different radiuses from different IPOs, we assign it into the smallest ring. For instance, ZIP code 77401 in Houston had no IPOs in 2004 and it is within 7 miles from ZIP code 77042 but only 3 miles from ZIP code 77056. Therefore, in this example, we include ZIP code 77401 in the 2-5 mile group and the dummy variable *Between 2-5 miles from IPO Headquarters* is set to one for 77401 in 2004. Our empirical model estimates IPO local economic effects as a function of each ZIP code's distance from the IPO. ZIP codes have irregular boundaries (or shapes), so to calculate the distance between two ZIP codes we compute the mile-distance of a straight line between their centroids, or the center of the mass of their area. The regression model below uses ZIP codes in the same county-year as a counterfactual.

[Insert Figure A.2 Here]

Figure A.3, again using Harris County, Texas as an example, illustrates how ZIP codes in given county-year provide a plausible counterfactual. Different ZIP codes in a county have IPOs at different times. This feature of our panel allows us to use similar ZIP

codes in the county that had an IPO in a different year (but not in the current year) as a counterfactual. Harris County, though large, is by no means unique. On average, IPOs in our sample are dispersed across fifteen different ZIP codes in a given county; therefore, a ZIP code with an IPO in one year (treated) may serve as a control for another year. Figure A.3, for example, shows that IPOs are scattered across ZIP codes and years.

[Insert Figure A.3 Here]

To further induce homogeneity of our treated and control ZIP-year observations, we use a coarsened exact matching procedure (see Blackwell, et al. (2009)) that we explain in the main text of the paper. Finally, if firms choose to locate their headquarters in a specific ZIP code in a county in expectation of the ZIP code's future economic development, we could have selection bias contributing to our estimates. This possibility seems doubtful, because firms likely choose the specific ZIP code in a county to locate when they are founded based on criteria other than future economic growth several years in the future.[1] Moreover, it is important to emphasize that, because we also examine the spillover effect on neighboring ZIP codes, too, the selection would have to be on the growth of economic conditions not only in the headquarters ZIP code, but also in neighboring ZIP codes where the headquarters is not. Therefore, heterogeneity in the location of corporate headquarters within a county seems plausibly exogenous to the economic development of the ZIP code more than a decade later.

A.II: Reverse causality and omitted variables bias tests

[1] Founding date to IPO date is roughly fifteen years, on average (Field and Karpoff (2002), Loughran and Ritter (2004)) and fewer than 10% of IPOs happen within one year of a firm's foundation date. Very few firms change the location of the headquarters between founding and IPO. Jay Ritter provides the data on his website

In this section, we empirically investigate whether past income growth and other measures of local economic activity affect the timing of IPOs in different ZIP codes. We start by regressing an indicator variable that equals one when a ZIP code has at least one IPO in a given year on lagged home price growth, mortgage origination growth, employment growth, establishment growth, and credit card spending growth. The results in Table A.II.a suggest none of the lags of past economic activity have the ability to predict IPO activity. That is, even though local IPO activity appears to predict future local economic outcomes, local economic progress does not predict future local IPO activity. This conclusion also reflects the lack of empirical and theoretical literature suggesting *local* economic conditions drive the timing of a firm's IPO.[2]

[Insert Table A.II.a Here]

Our matched sample analysis mitigates concerns over whether omitted variables bias drives our result, and our county-year fixed effects absorb location- and time-varying unobserved heterogeneity. When, instead, we use a ZIP code fixed effects and year fixed effects, we find economically larger spillover effects from an IPO (see Table A.II.b). In addition, our results are qualitatively similar when we exclude from the regressions ZIP codes with the highest number of IPOs in the county.

We also examine whether other unobserved factors drive the relation between IPO activity and local economic outcomes. We quantify how large the effect of selection on time-varying unobservable characteristics has to be to explain our results. Altonji, et al. (2005) formalize the procedure to estimate how the coefficients would change if

[2] Most studies suggest that firms time their IPO decision to exploit (successfully or not) a "window of opportunity". See for example Ritter (1991), Lerner (1994), Baker and Wurgler (2000), Schultz (2003), Butler, et al. (2005), and Brau and Fawcet (2006), among others. Colak, Durnev, and Qian (2016), find that state-level political instability affects an IPO decision.

selection on unobservable factors were equal to the treatment effect. Oster (2017) generalizes this methodology. We estimate the degree of selection on unobservables relative to selection on observables that would be necessary to explain away the estimated effect of IPO activity on income. In untabulated tests we find that the selection on unobservables has to be at least 2-6 times larger than selection on observables for the treatment effect of IPO activity on ZIP code economic development to be zero. Moreover, if the selection on unobservables and observables were equally important, the treatment effects of IPO activity on local economic outcomes drop, on average, by one quarter, but the effect would still be statistically significant. Taken together, the empirical results in this section are inconsistent with reverse causality or unobserved factors driving the effect of IPO activity on per capita income. Using ZIP code and year fixed effects we get similar (economically larger) results.

[Insert Table A.II.b Here]

A.III: Intensive margin ZIP-level regressions

In this section, we measure the intensive margin of IPO activity on local economic development. Specifically, to identify the intensive margin of the IPO spillovers we need to estimate the effect of an additional one million dollars in proceeds on the local economy. However, the vast majority of ZIP codes in a given year have no IPOs (zero proceeds), although we still expect the IPO to affect their economic development. To address this issue and estimate the effect of an additional one million dollars of proceeds for ZIP codes that had no IPOs (but are located near an IPO), we assign to them the total amount of IPO proceeds from the closest ZIP code. For instance, a ZIP code with no IPO activity that is located within 0-2 miles from a ZIP that had a 100

million dollars IPO will also take the value of 100.

Our regressions resemble regression model (1), but instead of an indicator that measures the distance from an IPO, we use three different proxies that capture the size (by proceeds) of the closest ZIP code that experiences IPO activity. Our first (and simplest) regression uses the actual dollar amount from IPO proceeds in the ZIP code-year. Moreover, we address the possibility that the relationship between IPO proceeds and changes in employment is non-linear using two more proxies. For our second proxy we use the natural log of proceeds; for our third proxy we group ZIP-years with at least one IPO in deciles based on the yearly distribution of the proceeds across all ZIP codes in a given year. We exclude spinoffs from the regressions and present the results in Table A.III.

[Insert Table A.III Here]

A.IV: 2-month NASDAQ returns as an IV to IPO completion

In this section, we provide empirical support for section 5.b of the main text, which argues that 2-month NASDAQ returns is not a valid instrumental variable (IV) for IPO completion in the context of this study. Specifically, we show that using the 60-day stock market returns from the filing date as an instrument for IPO completion has weak explanatory power in the first stage of the IV regression. In column (1) of Table A.IV, we regress an indicator variable that equals one if the firm completes its IPO (instead of withdrawing it) on NASDAQ returns in the two-month period after the IPO filing. The small R^2 in the first regression (approximately 1%, similar to Cornaggia, et al. (2018)) reflects the weak explanatory power of the IV. Furthermore, we also find that market

returns do not predict IPO completion in the period after year 2000 (see column (5)). This structural break during the dot com crisis, coupled with the low explanatory power of stock market conditions in predicting IPO completion suggest that the IV does not satisfy the relevance condition.

A weak IV poses important identification challenges that we discuss in detail in section 5.b of the paper. The paper also provides more details about why the IV violates the only through (or exclusion) restriction when used in geographic rather than firm-level setting (such as Berstein (2015)).

[Insert Table A.IV Here]

A.V: Extensive margin – MSA level analysis

In this section, we investigate the impact of IPO activity on development of US metropolitan areas (MSA). The baseline empirical strategy we use in the paper exploits cross-ZIP code variation of IPO activity in a given county-year to identify an IPO effect on local real estate and economic development. We choose this empirical design as our basis for two reasons: first, because it strengthens the internal validity of our estimates; second, because it allows us to trace the geographical extent of the IPO-spillover effects on local economic development. Nevertheless, this approach may limit our ability to draw inferences for larger economies, such as large metropolitan areas.

In Table A.V, we study the effect of IPOs on real estate outcomes (mortgage originations, new housing starts, and home prices), labor market outcomes (employment growth, job creation), and other measures of business development (new business starts)

for US metro areas. Specifically, we use data on IPO listing decisions over 1980-2011 to examine the effect of an IPO on the listing firm's MSA's economy.

[Insert Table A.V Here]

We use various measures of IPO activity, a matching procedure combined with MSA-level fixed effects, and various subsamples to draw inferences about the effects we estimate. Broadly speaking, we find that IPOs on average are associated with positive economic outcomes in an MSA. How big is the average effect of an IPO on these outcome variables? Our matched sample results suggest that following years of heavy IPO activity (i.e., top quartile of IPO proceeds over that last two years), relative to their matched sample counterpart MSAs, mortgage originations increase by 6.0% (one-sixth of a standard deviation), and new housing starts increase by 3.7% (one-twelfth of a standard deviation). Housing prices increase by 2.2%, (one-third of a standard deviation) but only for the highest priced homes. Labor markets improve as well, with employment growth increasing 33 basis points (one-ninth of a standard deviation) and job creation rates increasing 46 basis points (one-eleventh of a standard deviation). Finally, a result of a large IPO, the rate of new business starts increases by 18 basis points (one-fifteenth of a standard deviation).

[Insert Tables A.V.a-A.V.i Here]

A.VI: Placebo IPOs

Our identifying assumption is that cross-sectional differences in real estate, employment, and establishment growth of ZIP codes in the same county-year should be approximately the same if there is no IPO activity in that county. By performing the

following placebo tests, we evaluate whether the relationship we observe in the data is spurious. We take all our treated ZIP code-years and we (counterfactually) assign a placebo year to each ZIP code; we retain the matched control ZIP codes, which we assign the same placebo year, and we re-estimate our regressions. Because these ZIP codes experience only placebo IPOs, there should not be a significant IPO effect on the counterfactually assigned dates.

We present the regression results from the placebo tests of real estate variables and economic development in Table XI. The results suggest that placebo IPOs do not create statistically significant changes in home price growth, employment growth, establishment growth (tradable, non-tradable, or construction) or credit card spending between ZIP codes in a given county-year. We conclude that our results are unlikely to be a spurious result of ZIP code-specific characteristics

[Insert Table A.VI Here]

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Tables and Figures

Figure A.1: Identification strategy – Example from Harris County (Houston)
The shaded areas in this map represent all ZIP codes in Harris County, Texas that had at least one IPO from 1990-2015.

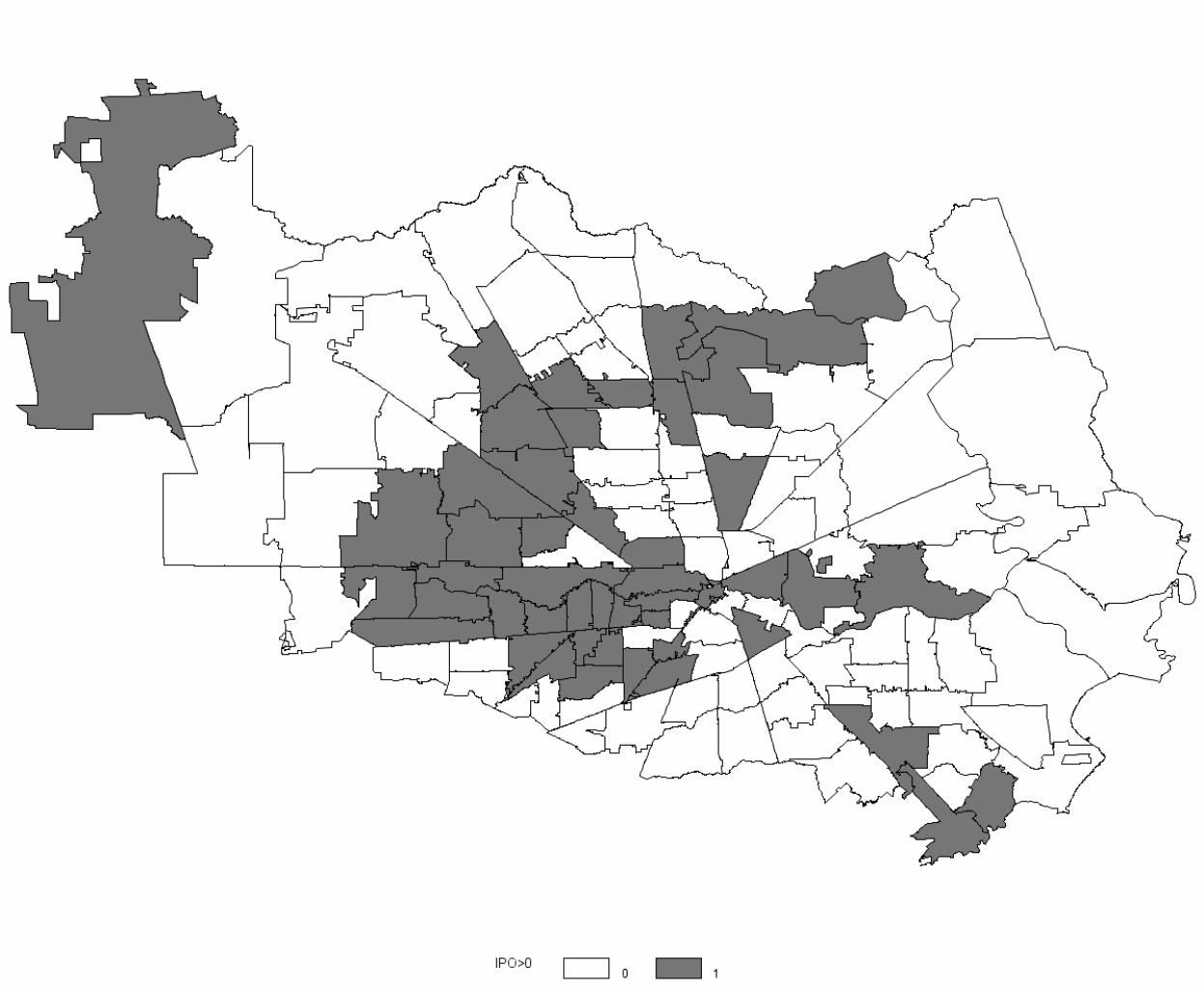

Figure A.2: Empirical design – IPOs in Harris County (year=2004).
The map shows the land area of two ZIP codes in Harris County, Texas. The shaded ZIP code to the west is 77042, and to the east is 77056. The smallest circle represents a 2-mile radius from the center of the ZIP code that the IPO took place. In our regressions, the indicator *within 2-miles from IPO HQ* identifies ZIP codes outside the shaded area but inside the 2-mile radius. Similarly, the indicator *2-5 miles from IPO HQ* identifies all ZIP codes that are at least two miles away but within a five-mile radius from the IPO ZIP code. Finally, the indicator *5-10 miles from IPO HQ* identifies all ZIP codes that are between 5-10 miles from the center of the closest ZIP code with at least one IPO.

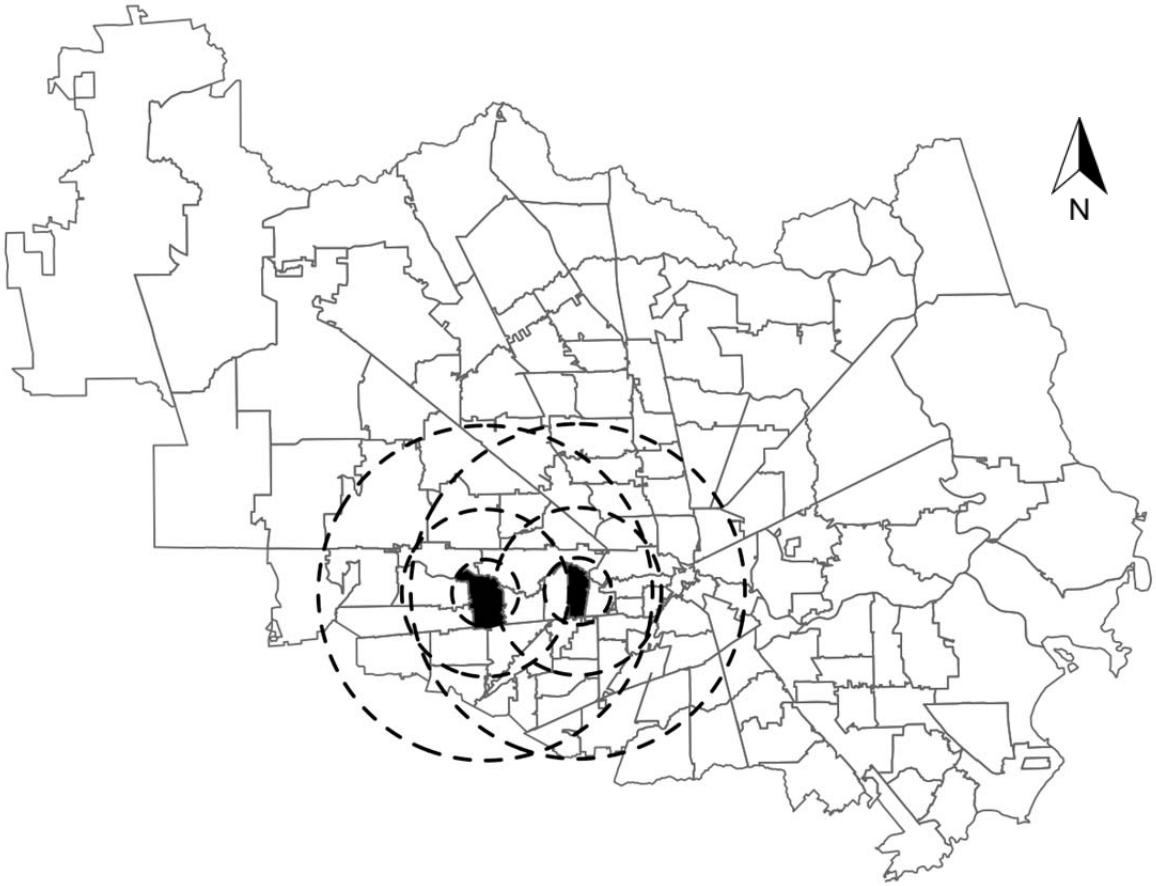

Figure A.3: ZIP codes with at least one IPO in Harris county.
The figures represent ZIP codes with at least one IPO in a given year. The Harris county maps represent IPO activity in the following years (from top left to bottom right): 1992, 1996, 2000, 2004, 2008, and 2012.

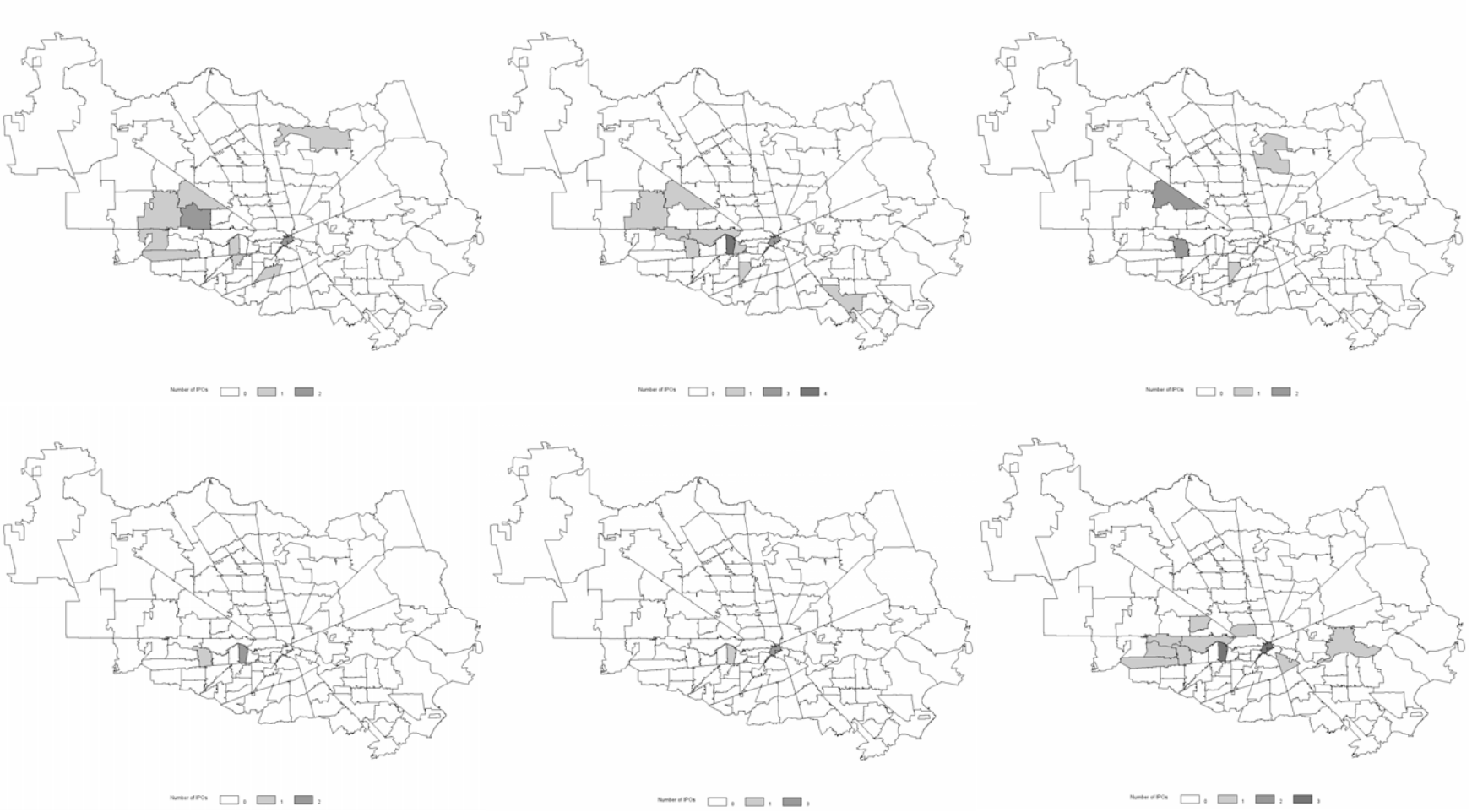

Table A.II.a: ZIP code regressions of IPO activity on lagged ZIP code real estate and economic development. The dependent variable is a dummy variable that indicates whether the ZIP code had at least one IPO in that year. In columns (1)-(5) the independent variable of interest is, respectively, the first lag of the: home price index, average mortgage amount, employment growth, establishment growth, and credit card spending growth. In all regressions we include as controls the natural logs of establishments, employment, ZIP code population, and population density. All regressions include county-year fixed effects. We cluster at the ZIP code and county-year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1)	(2)	(3)	(4)	(5)
	IPO>0	IPO>0	IPO>0	IPO>0	IPO>0
Lag-%Δ(HPI)	0.1330				
	(0.2826)				
Lag-%Δ(Avg. Mortgage Amount)		-0.1454			
		(0.1273)			
Lag-%Δ(Employment)			0.0018		
			(0.0993)		
Lag-%Δ(Establishments)				0.1650	
				(0.1649)	
Lag-%Δ(Cr. Card Spending)					-0.0272
					(0.0646)
Ln(Population)	-0.0638***	-0.0435***	-0.0593***	-0.0595***	-0.0422*
	(0.0171)	(0.0158)	(0.0173)	(0.0172)	(0.0215)
Ln(Establishments)	-0.0121	-0.0199	-0.0131	-0.0115	-0.0708*
	(0.0259)	(0.0271)	(0.0263)	(0.0263)	(0.0418)
Ln(Employment)	0.1211***	0.1237***	0.1197***	0.1195***	0.1634***
	(0.0219)	(0.0233)	(0.0220)	(0.0219)	(0.0359)
Ln(Wage Income)	0.0411**	0.0625***	0.0459**	0.0436**	0.0741***
	(0.0199)	(0.0204)	(0.0203)	(0.0199)	(0.0282)
Observations (ZIP-years)	10165	9075	10122	10176	4535
Adjusted R^2	0.323	0.326	0.322	0.322	0.283
County-Year FEs	Yes	Yes	Yes	Yes	Yes

Table A.II.b: Fixed effect regressions of local real estate and economic development on ZIP code distances from the IPO headquarters ZIP code. In regressions (1)-(6) the dependent variable is the annual growth rate in the 2-year period post-IPO for ZIP code: (1) home prices index, (2) top-tier homes tier home values, (3) average mortgage amount, (4) employment, (5) establishments in the non-tradable sector, (6) establishments in construction, and (7) credit card spending. *Large IPO HQ ZIP Code* is a dummy variable that indicates if the headquarters of the IPO firm are in that ZIP code, and its proceeds are in the top quartile of the yearly distribution of proceeds. The IPO proximity variables indicate ZIP codes with no IPO activity in that year but are between either 0-2, 2-5, or 5-10 miles away from the closest ZIP code with at least one large IPO. In all regressions we include the first lag of the dependent variable, and the natural logarithm of lagged establishments, employment, population, population density, and wage income. All regressions include ZIP code and year fixed effects. We cluster at the ZIP code and county-year level, and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1) HPI Growth	(2) Home Value Growth	(3) Average Mortgage Size Growth	(4) Employment Growth	(5) Establishments Non-Tradable Growth	(6) Establishments Construction Growth	(7) Credit Card Spending Growth
Large IPO HQ ZIP Code	0.0036	0.0095**	0.0094	0.0085**	-0.0009	0.0051	0.0304*
	(0.0035)	(0.0038)	(0.0064)	(0.0034)	(0.0041)	(0.0049)	(0.0177)
<0-2 miles from Large IPO HQ	0.0098**	0.0135***	0.0159**	0.0055*	0.0056*	0.0131**	0.0535***
	(0.0039)	(0.0046)	(0.0073)	(0.0029)	(0.0032)	(0.0063)	(0.0141)
2-5 miles from Large IPO HQ	0.0059*	0.0084***	0.0110**	0.0037**	0.0041**	-0.0007	0.0385***
	(0.0033)	(0.0032)	(0.0050)	(0.0017)	(0.0020)	(0.0031)	(0.0084)
5-10 miles from Large IPO HQ	0.0033	0.0047*	0.0089**	0.0024*	0.0032	0.0029	0.0204***
	(0.0028)	(0.0028)	(0.0044)	(0.0013)	(0.0020)	(0.0026)	(0.0069)
Observations (ZIP-years)	27714	23685	24577	31305	27998	27985	11990
Adjusted R^2	0.643	0.676	0.180	0.319	0.243	0.197	0.404
ZIP Code Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ZIP Code FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.III Intensive margin regressions based on ZIP code-year panel of IPOs. The dependent variable is the average yearly difference in employment during the two-year period after an IPO in the ZIP code. *Proceeds (IPO HQ ZIP)* is the total amount of proceeds from IPOs in that ZIP code-year. *Ln(Proceeds) (IPO HQ ZIP)* is the natural log of the total proceeds from IPOs in that ZIP code-year. *Proceeds Decile (IPO HQ ZIP)* takes integer values between 1-10, and equals 1(10) if the proceeds of IPOs in that ZIP code are in the lowest (highest) decile of IPO proceeds across all ZIP codes in that year. The IPO proximity measure the amount of proceeds (regression (1)), the natural of proceeds (regression (2)), and the IPO proceed decile (regression (3)) for ZIP codes with no IPO activity in that year but are between either 0-2, 2-5, or 5-10 miles away from the closest ZIP code with at least one IPO in the same county-year. In all regressions we control for SEO activity in the ZIP code and include the first lag of the dependent variable, the number of establishments, employment, ZIP code population, population density, and wage income. All regressions include county-year fixed effects. We cluster at the ZIP code and county-year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1) Δ(Employment)	(2) Δ(Employment)	(3) Δ(Employment)
Proceeds (IPO HQ ZIP Code)	0.663***		
	(0.152)		
Proceeds (<0-2 miles from IPO HQ)	0.103**		
	(0.051)		
Proceeds (2-5 miles from IPO HQ)	0.008		
	(0.025)		
Proceeds (5-10 miles from IPO HQ)	0.033		
	(0.059)		
Ln(Proceeds) (IPO HQ ZIP Code)		64.454***	
		(18.656)	
Ln(Proceeds) (<0-2 miles from IPO HQ)		33.168**	
		(15.527)	
Ln(Proceeds) (2-5 miles from IPO HQ)		8.728	
		(17.349)	
Ln(Proceeds) (5-10 miles from IPO HQ)		6.757	
		(16.947)	
Proceeds Decile (IPO HQ ZIP Code)			43.654***
			(9.021)
Proceeds Decile (<0-2 from IPO HQ)			16.535*
			(8.959)
Proceeds Decile (2-5 miles from IPO HQ)			0.744
			(7.768)
Proceeds Decile (5-10 miles from IPO HQ)			1.933
			(5.068)
Observations (ZIP-years)	8383	8383	8383
Adjusted R^2	0.201	0.203	0.203
County-year FEs	Yes	Yes	Yes

Table A.IV: Regressions of IPO Completion on NASDAQ 2-month returns and county-level information. The regressions use firm filing year observations from 1986 until 2015, excluding year 2000 and 2008 (dot-com and financial crisis). IPO completed is an indicator variable that equals one of the firm files for an IPO and does not withdraw. The control variables are lagged natural logarithms of economic characteristics in the county (employment, establishments, income, population, and income growth). Regressions (1)-(3) include observations from the full sample period; regression (4) includes only years before 2000, and regression (5) includes only years after 2000. In regressions (2)-(5) we include filing year and industry (SIC-2) fixed effects. We cluster at the filing year and county level, and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1)	(2)	(3)	(4)	(5)
IPO Completed	Full Period	Full Period	Full Period	Pre-2000	Post-2000
NASDAQ 2-m return	1.116***	0.775***	0.776***	0.944***	0.415
	(0.133)	(0.217)	(0.216)	(0.288)	(0.241)
Ln(Employment)			-0.016	0.006	-0.044
			(0.022)	(0.026)	(0.048)
Ln(Establishments)			-0.057	-0.083	-0.022
			(0.044)	(0.058)	(0.083)
Ln(Income)			0.097***	0.100**	0.106**
			(0.029)	(0.037)	(0.049)
Ln(Population)			-0.026	-0.026	-0.041
			(0.018)	(0.016)	(0.039)
Income Growth			-0.127	-0.427	-0.039
			(0.257)	(0.293)	(0.315)
Observations	8594	8579	8579	5592	2981
Adjusted R^2	0.010	0.062	0.063	0.047	0.074
Industry-year FEs	No	Yes	Yes	Yes	Yes
F-statistic	69.96	12.79	5.42	6.76	.

Table A.V: Regression estimates for MSA-level sample**.** This table includes the estimates of regressions of outcome variables that describe local economic activity at the MSA level on measures of IPO activity. Each cell of the table reports a coefficient estimate for a different regression, varying the outcome variable, IPO activity measure, and/or subsample. Significance at the 1%, 5%, and 10% is indicated respectively with *** ($p<0.01$), ** ($p<0.05$), and * ($p<0.10$). All regressions contain MSA and year fixed effects and a vector of control variables; we suppress the coefficients of the control variables but report the full regression output in the appendix. Columns (A)-(G) contain the dependent variables. Some economic variables, like per capita income, are persistent, so we use growth rates as dependent variables. For consistency, we use growth rates across all outcome variables as follows: (A) Mortgage origination growth: *Ln(Mortgage Origination(t)/Mortgage Origination(t-1))*. (B) Housing start growth: *Ln(Housing Start(t)/Housing Starts(t-1))*. (C) Home price growth (low-priced): *Ln(Median home price low(t)/Median home price low(t-1))*. (D) Home price growth (high-priced): *Ln(Median home price high(t)/Median home price high(t-1))*. (E) Employment growth: *Ln(Employment(t)/Employment(t-1))*. (F) Net job creation rate: the count of jobs created minus the jobs destroyed within the MSA in the last 12 months divided by the average employment between years t and t-1. (G) Business starts growth: *Ln(Estab.(t)/Estab.(t-1))*. (H) Per capita income (PCI): *Ln(PCI(t)/PCI(t-1))*. In regression (1), the independent variable of interest is an indicator variable for MSA-years that belong in the top quartile of distribution of IPO proceeds over the previous two years; the sample is limited only to MSA years with similar population, employment, private firms, public firms, and the lagged dependent variable. In regression (2), the independent variable of interest is an indicator variable for MSA years that had at least one IPO in the previous year; this regression uses the full sample of MSA-years. In regression (3), the independent variable of interest is an indicator variable for MSA-years that belong in the top quartile of distribution of IPO proceeds over the previous two years; this regression uses the full sample of MSA-years. In regression (4), the independent variable of interest is an indicator variable for MSA years that belong in the top quartile of distribution of IPO proceeds over the previous two years; this regression excludes MSA years with no IPO activity. In regression (5), the independent variable of interest is an indicator variable that identifies MSA years with exactly one IPO; this regression excludes all MSA years with more than one IPO. In regression (6), the independent variable of interest is an indicator variable for MSA years that belong in the top quartile of distribution of IPO proceeds over the previous two years; this regression excludes MSAs that never experience an IPO during the sample period. In regression (7), the independent variable of interest is an indicator variable for MSA years that belong in the top quartile of distribution of IPO proceeds normalized by population over the previous two years; this regression uses the full sample of MSA-years. In regression (8), the independent variable of interest is an indicator variable for MSA years that belong in the top quartile of distribution of IPO proceeds over the previous two years; this regression excludes from the sample the top 20 MSAs by population. In regression (9), there are two independent variables of interest: an indicator variable for MSA years that belong in the top quartile of distribution of IPO proceeds over the previous two years, and an indicator variable for MSA years that belong in the top quartile of distribution of SEO process over the previous two years; this regression includes the full sample of MSA years. In regression (10), the dependent variable is the natural log of IPO proceeds; the independent variables of interest are the first three lags of the dependent variable (A)-(H); this regression also includes the first three lags of IPO proceeds and uses the full sample of MSA-years.

		(A)	(B)	(C)	(D)	(E)	(F)	(G)
Description of test and sample	**Rationale for / interpretation of test**	**Mortgage origination growth**	**Housing starts growth**	**Low-priced home price changes**	**High-priced home price changes**	**Employment growth (bps)**	**Job creation rate (bps)**	**Business starts growth (bps)**
Mean		17.6%	3.1%	2.3%	3.2%	150	1170	140
Standard deviation		36.6%	44.5%	9.0%	7.1%	240	490	270
(1) Heavy IPO activity; matched sample	Our baseline test. Matching reduces omitted variables bias and balances the covariates.	6.0%***	3.7%**	Insignif.	2.2%***	22.2*	46**	18.1*
(2) Any IPO; full sample	Comparing MSA-years with an IPO of any size to MSA-years with no IPO activity establishes the extensive margin.	Insignif.	3.7%**	Insignif.	Insignif.	29.1***	19.8*	15.4**
(3) Heavy IPO activity; full sample	Comparing MSA-years with heavy IPO activity to MSA-years with modest or no IPO activity partially establishes the intensive margin.	5.7%**	5.1%***	Insignif.	Insignif.	29.7***	26.6**	16.9**
(4) Heavy IPO activity; sample of only MSA-years with non-zero IPO activity	Comparing MSA-years with heavy IPO activity to only MSA-years with modest IPO activity establishes the intensive margin and mitigates concerns of omitted variables bias, as all observations had some treatment.	5.3%**	3.7%**	Insignif.	Insignif.	23.6**	49.6***	15.3*
(5) Any IPO; sample of singleton IPO and no IPO observations	Comparing MSA-years with one IPO to MSA-years with no IPO activity is a weaker extensive margin test; treated observations are more similar ex ante to control observations than in the full sample.	Insignif.	3.2%***	Insignif.	Insignif.	18.5***	Insignif.	17.4**
(6) Heavy IPO activity; sample excludes MSAs that never had an IPO during our sample	An intensive margin test analogous to (4) but omitting only the MSAs that never had an IPO in our sample.	5.5%**	4.7%***	Insignif.	Insignif.	28.4***	25.8**	15.7**
(7) Heavy IPO activity on a per capita basis; full sample	An intensive margin test analogous to (3) but heavy activity is characterized on a per capita basis so that large MSAs do not skew results.	4.3%**	5.4%***	Insignif.	Insignif.	23.9**	Insignif.	Insignif.
(8) Heavy IPO activity; sample excludes 20 largest MSAs	An intensive margin test analogous to (3) but omitting the largest MSAs by population so that large MSAs do not skew results.	4.4%***	4.4%*	Insignif.	2.9%*	27.8*	27.2*	18.7**
(9) Heavy IPO activity; matched sample; add SEO activity variable	Analogous to (1), but adds a variable for MSA-year SEO activity to allow us to differentiate between change of listing status and equity capital raising	IPO: 5.9%*** SEO: Insig	IPO: 3.7%* SEO: Insig	IPO: Insig SEO: Insig	IPO: 2.2%** SEO: Insig	IPO: 23.7* SEO: Insig	IPO: 46.2** SEO: Insig	IPO: 18.0* SEO: Insig
(10) Reverse causality tests	Full sample Granger-causality style tests to determine if the outcome variable Granger-causes IPO activity	Insignif.	Insignif.	Insignif.	Insignif.	Insignif.	Insignif.	Insignif.

Table A.V.a: The effect of IPO activity on Mortgage Origination Growth. This table corresponds to column A of the meta-table presented as Table A.V. The dependent variable is mortgage origination growth Ln[*Mortg.origination*(*t*)/ *Mortg.origination* (*t*-1)]. In regression (1), the independent variables of interest are two indicator variables for MSAs that had at least one IPO in the last year and whether the MSA had at least one IPO in two years. In regressions (2)-(3) and (6)-(10), the independent variable of interest is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of proceeds from IPOs in the last two years across all MSAs. Regression (2) includes the full sample; regression (3) includes only MSA years with at least one IPO; regression (6) excludes largest 20 MSAs; regression (7) excludes MSAs that never had IPOs; regression (8) includes an indicator variable that identifies whether the MSA belongs in the top quartile of SEO activity in that year. In regression (4) we include only MSA-years with exactly one IPO. The independent variable of interest in regression (5) is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of IPO proceeds normalized by MSA population. Regression (9) includes only MSAs with similar characteristics (matched on population, employment, the number of public firms, the number of public firms, and lagged dependent variable). Regression (10) is the same as (9) but includes the high SEO activity indicator. In all regressions we control for MSA characteristics such as population, the number of private firms, the number of people employed, and the number of public firms. We include MSA- and year-fixed effects. We cluster at the MSA and year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IPOs>0 - Lag 1	0.021									
	(0.013)									
IPOs>0 - Lag 2	0.016									
	(0.015)									
1-IPO				0.024						
				(0.015)						
IPO Proceeds - High (quartile)		0.057**	0.053**			0.044***	0.055**	0.057**	0.060***	0.059***
		(0.025)	(0.020)			(0.013)	(0.022)	(0.025)	(0.020)	(0.019)
IPO Proceeds/Capita - High (quartile)					0.043**					
					(0.016)					
SEO Proceeds High								-0.037*		0.054
								(0.018)		(0.045)
Ln(Population)	0.475	0.464	-0.676	0.674*	0.470	0.537	-0.028	0.466	-0.372	-0.333
	(0.335)	(0.335)	(0.396)	(0.346)	(0.336)	(0.346)	(0.281)	(0.335)	(0.549)	(0.522)
Ln(Private firms)	-0.134	-0.132	-0.261	-0.084	-0.133	-0.125	-0.031	-0.133	0.066	0.060
	(0.089)	(0.089)	(0.222)	(0.090)	(0.090)	(0.089)	(0.090)	(0.089)	(0.447)	(0.450)
Ln(Employment)	1.148***	1.151***	2.125***	1.083***	1.145***	1.172***	1.404***	1.150***	1.303	1.270
	(0.277)	(0.274)	(0.489)	(0.259)	(0.277)	(0.270)	(0.317)	(0.274)	(0.923)	(0.904)
Ln(Public firms)	0.045**	0.047**	0.024	0.045**	0.045**	0.045**	0.027	0.047**	-0.199	-0.210
	(0.020)	(0.021)	(0.048)	(0.021)	(0.021)	(0.021)	(0.026)	(0.021)	(0.118)	(0.121)
Observations	5584	5584	926	5062	5584	5424	4176	5584	573	573
Adjusted R^2	0.695	0.695	0.901	0.681	0.695	0.694	0.737	0.695	0.906	0.906
MSA, Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.V.b: Changes in new housing starts. This table corresponds to column B of the meta-table presented as Table A.V. The dependent variable is the change in new housing starts Ln[*housing starts*(*t*)/ *housing starts* (*t*-1)]. In regression (1), the independent variables of interest are two indicator variables for MSAs that had at least one IPO in the last year and whether the MSA had at least one IPO two years. In regressions (2)-(3) and (6)-(10), the independent variable of interest is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of proceeds from IPOs in the last two years across all MSAs. Regression (2) includes the full sample; regression (3) includes only MSA years with at least one IPO; regression (6) excludes largest 20 MSAs; regression (7) excludes MSAs that never had an IPO; regression (8) includes an indicator variable that identifies whether the MSA belongs in the top quartile of SEO activity in that year. In regression (4) we include only MSA-years with exactly one IPO. The independent variable of interest in regression (5) is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of IPO proceeds normalized by MSA population. Regression (9) includes only MSAs with similar characteristics (matched on population, employment, the number of public firms, the number of public firms, and lagged dependent variable). Regression (10) is the same as (9) but includes the high SEO activity indicator. In all regressions we control for MSA characteristics such as population, the number of private firms, the number of people employed, and the number of public firms. We include MSA and year fixed effects. We cluster at the MSA and year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IPOs>0 - Lag 1	0.037**									
	(0.014)									
IPOs>0 - Lag 2	0.022*									
	(0.012)									
1-IPO				0.032***						
				(0.011)						
IPO Proceeds - High (quartile)		0.051***	0.037**			0.044*	0.047***	0.050***	0.037**	0.037*
		(0.018)	(0.017)			(0.023)	(0.017)	(0.018)	(0.017)	(0.018)
IPO Proceeds/Capita - High (quartile)					0.054***					
					(0.017)					
SEO Proceeds High								0.025		0.011
								(0.032)		(0.023)
Ln(Population)	-1.920***	-1.927***	-2.425***	-1.859***	-1.922***	-1.916***	-2.294***	-1.928***	-0.557	-0.552
	(0.353)	(0.355)	(0.578)	(0.343)	(0.354)	(0.349)	(0.393)	(0.355)	(0.714)	(0.712)
Ln(Private firms)	-0.600***	-0.601***	-0.586**	-0.553***	-0.600***	-0.576***	-0.642***	-0.600***	-0.926*	-0.925*
	(0.108)	(0.108)	(0.258)	(0.105)	(0.108)	(0.110)	(0.132)	(0.108)	(0.456)	(0.457)
Ln(Employment)	2.926***	2.939***	3.837***	2.785***	2.936***	2.908***	3.296***	2.939***	2.911***	2.903***
	(0.399)	(0.401)	(0.604)	(0.378)	(0.401)	(0.397)	(0.463)	(0.401)	(0.972)	(0.972)
Ln(Public firms)	-0.032	-0.029	0.045	-0.031	-0.030	-0.026	-0.014	-0.029	0.084	0.083
	(0.024)	(0.024)	(0.073)	(0.024)	(0.024)	(0.024)	(0.027)	(0.024)	(0.154)	(0.153)
Observations	8632	8632	1559	7749	8632	8150	6474	8632	572	572
Adjusted R^2	0.432	0.432	0.557	0.427	0.432	0.429	0.453	0.432	0.644	0.644
MSA, Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.V.c: House Price Growth (Bottom-Tier). This table corresponds to column C of the meta-table presented as Table A.V. The dependent variable is the change in house price growth of houses whose value belongs in the lowest tercile in the MSA Ln[*home price*(*t*)/*home price*(*t*-1)]. In regression (1), the independent variables of interest are two indicator variables for MSAs that had at least one IPO in the last year and whether the MSA had at least one IPO two years. In regressions (2)-(3) and (6)-(10), the independent variable of interest is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of proceeds from IPOs in the last two years across all MSAs. Regression (2) includes the full sample; regression (3) includes only MSA years with at least one IPO; regression (6) excludes largest 20 MSAs; regression (7) excludes MSAs that never had IPOs; regression (8) includes an indicator variable that identifies whether the MSA belongs in the top quartile of SEO activity in that year. In regression (4) we include only MSA-years with exactly one IPO. The independent variable of interest in regression (5) is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of IPO proceeds normalized by MSA population. Regression (9) includes only MSAs with similar characteristics (matched on population, employment, the number of public firms, the number of public firms, and lagged dependent variable). Regression (10) is the same as (9) but includes the high SEO activity indicator. In all regressions we control for MSA characteristics such as population, the number of private firms, the number of people employed, and the number of public firms. We include MSA and year fixed effects. We cluster at the MSA and year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IPOs>0 - Lag 1	0.004									
	(0.008)									
IPOs>0 - Lag 2	0.004									
	(0.006)									
1-IPO				0.003						
				(0.008)						
IPO Proceeds - High (quartile)		-0.004	-0.000			0.021	-0.007	-0.004	0.005	0.004
		(0.015)	(0.019)			(0.015)	(0.014)	(0.015)	(0.010)	(0.010)
IPO Proceeds/Capita - High (quartile)					-0.007					
					(0.012)					
SEO Proceeds High								-0.025*		0.038**
								(0.012)		(0.012)
Ln(Population)	-0.153	-0.154	-1.605***	0.046	-0.152	-0.059	-0.535*	-0.148	-1.888***	-1.929***
	(0.252)	(0.254)	(0.320)	(0.269)	(0.255)	(0.262)	(0.274)	(0.255)	(0.470)	(0.453)
Ln(Private firms)	-0.268***	-0.269***	-0.104	-0.298***	-0.269***	-0.296***	-0.163**	-0.269***	-0.280	-0.292
	(0.063)	(0.063)	(0.112)	(0.065)	(0.063)	(0.065)	(0.063)	(0.063)	(0.207)	(0.208)
Ln(Employment)	1.273***	1.277***	2.421***	1.153***	1.275***	1.195***	1.484***	1.272***	2.998***	3.043***
	(0.150)	(0.151)	(0.399)	(0.162)	(0.151)	(0.153)	(0.204)	(0.150)	(0.462)	(0.421)
Ln(Public firms)	0.017	0.017	-0.039	0.020	0.018	0.019	0.037*	0.018	-0.145	-0.147
	(0.014)	(0.014)	(0.046)	(0.015)	(0.014)	(0.015)	(0.018)	(0.014)	(0.110)	(0.109)
Observations	2391	2391	438	2113	2391	2203	1950	2391	181	181
Adjusted R^2	0.512	0.512	0.535	0.517	0.512	0.503	0.524	0.512	0.802	0.804
MSA, Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.V.d: House Price Growth (Top-Tier). This table corresponds to column D of the meta-table presented as Table A.V. The dependent variable is the change in house price growth of houses whose value belongs in the top tercile in the MSA Ln[*home price*(*t*)/*home price*(*t*-1)]. In regression (1), the independent variables of interest are two indicator variables for MSAs that had at least one IPO in the last year and whether the MSA had at least one IPO two years. In regressions (2)-(3) and (6)-(10), the independent variable of interest is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of proceeds from IPOs in the last two years across all MSAs. Regression (2) includes the full sample; regression (3) includes only MSA years with at least one IPO; regression (6) excludes largest 20 MSAs; regression (7) excludes MSAs that never had an IPO; regression (8) includes an indicator variable that identifies whether the MSA belongs in the top quartile of SEO activity in that year. In regression (4) we include only MSA-years with exactly one IPO. The independent variable of interest in regression (5) is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of IPO proceeds normalized by MSA population. Regression (9) includes only MSAs with similar characteristics (matched on population, employment, the number of public firms, the number of public firms, and lagged dependent variable). Regression (10) is the same as (9) but includes the high SEO activity indicator. In all regressions we control for MSA characteristics such as population, the number of private firms, the number of people employed, and the number of public firms. We include MSA and year fixed effects. We cluster at the MSA and year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IPOs>0 - Lag 1	0.007									
	(0.007)									
IPOs>0 - Lag 2	0.004									
	(0.005)									
1-IPO				0.008						
				(0.007)						
IPO Proceeds - High (quartile)		0.008	0.010			0.029*	0.006	0.008	0.022***	0.022**
		(0.011)	(0.013)			(0.015)	(0.011)	(0.011)	(0.007)	(0.007)
IPO Proceeds/Capita - High (quartile)					0.007					
					(0.008)					
SEO Proceeds High								-0.027***		-0.003
								(0.008)		(0.018)
Ln(Population)	-0.090	-0.091	-1.164***	0.020	-0.093	-0.039	-0.444*	-0.085	-1.142***	-1.136***
	(0.197)	(0.199)	(0.281)	(0.215)	(0.198)	(0.202)	(0.207)	(0.200)	(0.236)	(0.257)
Ln(Private firms)	-0.191***	-0.191***	-0.077	-0.200***	-0.192***	-0.204***	-0.118*	-0.192***	-0.218*	-0.217*
	(0.047)	(0.047)	(0.105)	(0.050)	(0.047)	(0.049)	(0.053)	(0.047)	(0.109)	(0.107)
Ln(Employment)	1.099***	1.101***	2.025***	1.014***	1.102***	1.044***	1.331***	1.096***	2.263***	2.253***
	(0.118)	(0.119)	(0.366)	(0.137)	(0.118)	(0.121)	(0.142)	(0.118)	(0.261)	(0.311)
Ln(Public firms)	0.014	0.014	-0.012	0.017	0.014	0.015	0.013	0.014	-0.054	-0.053
	(0.012)	(0.012)	(0.035)	(0.012)	(0.012)	(0.013)	(0.016)	(0.012)	(0.031)	(0.031)
Observations	2602	2602	444	2324	2602	2422	2033	2602	349	349
Adjusted R^2	0.543	0.542	0.601	0.538	0.542	0.535	0.552	0.543	0.846	0.846
MSA, Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.V.e: Employment Growth. This table corresponds to column E of the meta-table presented as Table A.V. The dependent variable is employment growth Ln[*employment*(*t*)/*employment*(*t*-1)]. In regression (1), the independent variables of interest are two indicator variables for MSAs that had at least one IPO in the last year and whether the MSA had at least one IPO two years. In regressions (2)-(3) and (6)-(10), the independent variable of interest is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of proceeds from IPOs in the last two years across all MSAs. Regression (2) includes the full sample; regression (3) includes only MSA years with at least one IPO; regression (6) excludes largest 20 MSAs; regression (7) excludes MSAs that never had an IPO; regression (8) includes an indicator variable that identifies whether the MSA belongs in the top quartile of SEO activity in that year. In regression (4) we include only MSA-years with exactly one IPO. The independent variable of interest in regression (5) is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of IPO proceeds normalized by MSA population. Regression (9) includes only MSAs with similar characteristics (matched on population, employment, the number of public firms, the number of public firms, and lagged dependent variable). Regression (10) is the same as (9) but includes the high SEO activity indicator. In all regressions we control for MSA characteristics such as population, the number of private firms, the number of people employed, and the number of public firms. We include MSA and year fixed effects. We cluster at the MSA and year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IPOs>0 - Lag 1	0.291***									
	(0.069)									
IPOs>0 - Lag 2	0.056									
	(0.063)									
1-IPO				0.185***						
				(0.059)						
IPO Proceeds - High (quartile)		0.297***	0.236**			0.278*	0.284***	0.290***	0.222*	0.237*
		(0.098)	(0.094)			(0.153)	(0.093)	(0.099)	(0.112)	(0.116)
IPO Proceeds/Capita - High (quartile)					0.239**					
					(0.097)					
SEO Proceeds High								0.234*		-0.527
								(0.128)		(0.368)
Ln(Population)	-4.255***	-4.237***	-2.786***	-4.327***	-4.229***	-4.265***	-4.698***	-4.241***	-3.398*	-3.191*
	(0.710)	(0.711)	(1.003)	(0.721)	(0.710)	(0.719)	(0.820)	(0.710)	(1.733)	(1.709)
Ln(Private firms)	1.149**	1.148**	0.796	1.166**	1.147**	1.193**	1.169**	1.148**	0.250	0.207
	(0.481)	(0.485)	(0.770)	(0.482)	(0.485)	(0.486)	(0.451)	(0.485)	(1.612)	(1.597)
Ln(Public firms)	0.036	0.057	0.556**	0.031	0.054	0.057	0.134	0.057	-0.783*	-0.789*
	(0.093)	(0.092)	(0.254)	(0.092)	(0.093)	(0.091)	(0.094)	(0.092)	(0.452)	(0.455)
Observations	11346	11346	1911	10264	11346	10695	8525	11346	2857	2857
Adjusted R^2	0.457	0.456	0.526	0.452	0.456	0.449	0.507	0.456	0.734	0.737
MSA, Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.V.f: Net Job Creation Rate. This table corresponds to column F of the meta-table presented as Table A.V. The dependent variable is net job creation rate (job creation rate – job destruction rate). In regression (1), the independent variables of interest are two indicator variables for MSAs that had at least one IPO in the last year and whether the MSA had at least one IPO two years. In regressions (2)-(3) and (6)-(10), the independent variable of interest is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of proceeds from IPOs in the last two years across all MSAs. Regression (2) includes the full sample; regression (3) includes only MSA years with at least one IPO; regression (6) excludes largest 20 MSAs; regression (7) excludes MSAs that never had an IPO; regression (8) includes an indicator variable that identifies whether the MSA belongs in the top quartile of SEO activity in that year. In regression (4) we include only MSA-years with exactly one IPO. The independent variable of interest in regression (5) is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of IPO proceeds normalized by MSA population. Regression (9) includes only MSAs with similar characteristics (matched on population, employment, the number of public firms, the number of public firms, and lagged dependent variable). Regression (10) is the same as (9) but includes the high SEO activity indicator. In all regressions we control for MSA characteristics such as population, the number of private firms, the number of people employed, and the number of public firms. We include MSA and year fixed effects. We cluster at the MSA and year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IPOs>0 - Lag 1	0.198*									
	(0.113)									
IPOs>0 - Lag 2	-0.044									
	(0.146)									
1-IPO				0.097						
				(0.130)						
IPO Proceeds - High (quartile)		0.266**	0.496***			0.272*	0.258**	0.253**	0.460**	0.462**
		(0.120)	(0.123)			(0.150)	(0.116)	(0.118)	(0.192)	(0.192)
IPO Proceeds/Capita - High (quartile)					0.115					
					(0.122)					
SEO Proceeds High								0.466		-0.043
								(0.270)		(0.413)
Ln(Population)	-15.668***	-15.676***	-15.929***	-15.528***	-15.674***	-15.691***	-17.834***	-15.677***	-10.514***	-10.488***
	(2.001)	(2.002)	(2.761)	(1.980)	(2.001)	(2.018)	(2.320)	(1.998)	(2.895)	(2.950)
Ln(Private firms)	-3.118***	-3.124***	-2.327	-3.237***	-3.125***	-3.269***	-2.795***	-3.117***	-7.265***	-7.276***
	(0.827)	(0.830)	(1.536)	(0.842)	(0.829)	(0.854)	(0.805)	(0.830)	(1.706)	(1.775)
Ln(Employment)	15.713***	15.731***	17.016***	15.474***	15.736***	15.899***	16.387***	15.722***	16.183***	16.167***
	(2.750)	(2.756)	(3.360)	(2.659)	(2.751)	(2.781)	(2.830)	(2.755)	(3.445)	(3.417)
Ln(Public firms)	-0.170	-0.161	0.624	-0.179	-0.162	-0.174	-0.066	-0.162	-0.351	-0.348
	(0.175)	(0.175)	(0.422)	(0.180)	(0.175)	(0.180)	(0.169)	(0.175)	(0.419)	(0.424)
Observations	10202	10202	1646	9276	10202	9631	7629	10202	2309	2309
Adjusted R^2	0.263	0.263	0.417	0.253	0.263	0.253	0.316	0.263	0.604	0.604
MSA, Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.V.g: Business Establishment Growth. This table corresponds to column G of the meta-table presented as Table A.V. The dependent variable is the growth of new business establishments Ln[*establishments*(*t*)/*establishments*(*t*-1)]. In regression (1), the independent variables of interest are two indicator variables for MSAs that had at least one IPO in the last year and whether the MSA had at least one IPO two years. In regressions (2)-(3) and (6)-(10), the independent variable of interest is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of proceeds from IPOs in the last two years across all MSAs. Regression (2) includes the full sample; regression (3) includes only MSA years with at least one IPO; regression (6) excludes largest 20 MSAs; regression (7) excludes MSAs that never had an IPO; regression (8) includes an indicator variable that identifies whether the MSA belongs in the top quartile of SEO activity in that year. In regression (4) we include only MSA-years with exactly one IPO. The independent variable of interest in regression (5) is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of IPO proceeds normalized by MSA population. Regression (9) includes only MSAs with similar characteristics (matched on population, employment, the number of public firms, the number of public firms, and lagged dependent variable). Regression (10) is the same as (9) but includes the high SEO activity indicator. In all regressions we control for MSA characteristics such as population, the number of private firms, the number of people employed, and the number of public firms. We include MSA and year fixed effects. We cluster at the MSA and year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IPOs>0 - Lag 1	0.154**									
	(0.074)									
IPOs>0 - Lag 2	0.154**									
	(0.070)									
1-IPO				0.174**						
				(0.081)						
IPO Proceeds - High (quartile)		0.169**	0.153*			0.187**	0.157**	0.166**	0.181*	0.180*
		(0.070)	(0.083)			(0.071)	(0.067)	(0.069)	(0.102)	(0.101)
IPO Proceeds/Capita - High (quartile)					0.061					
					(0.047)					
SEO Proceeds High								0.106		0.201
								(0.122)		(0.228)
Ln(Population)	-8.570***	-8.594***	-9.121***	-8.674***	-8.592***	-8.656***	-9.850***	-8.596***	-9.072***	-9.126***
	(1.587)	(1.583)	(1.873)	(1.618)	(1.585)	(1.606)	(1.732)	(1.582)	(3.094)	(3.038)
Ln(Private firms)	-1.842***	-1.856***	-2.060**	-1.935***	-1.862***	-1.934***	-1.804***	-1.856***	-2.503	-2.502
	(0.532)	(0.534)	(0.950)	(0.521)	(0.535)	(0.514)	(0.562)	(0.534)	(1.699)	(1.706)
Ln(Employment)	9.433***	9.490***	11.723***	9.483***	9.500***	9.568***	10.517***	9.489***	13.492***	13.500***
	(1.173)	(1.175)	(2.483)	(1.101)	(1.177)	(1.164)	(1.392)	(1.174)	(3.178)	(3.188)
Ln(Public firms)	-0.038	-0.020	0.217	-0.045	-0.020	-0.032	0.011	-0.020	-0.804*	-0.799*
	(0.101)	(0.099)	(0.264)	(0.098)	(0.099)	(0.096)	(0.117)	(0.099)	(0.399)	(0.397)
Observations	10527	10527	1820	9478	10527	9917	7917	10527	2651	2651
Adjusted R^2	0.450	0.449	0.516	0.445	0.449	0.446	0.473	0.449	0.640	0.640
MSA, Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.V.h: Per Capita Income Growth. This table corresponds to column H of the meta-table presented as Table A.V. The dependent variable is growth in per capital income Ln[*per capita income*(*t*)/per capita income (*t*-1)]. In regression (1), the independent variables of interest are two indicator variables for MSAs that had at least one IPO in the last year and whether the MSA had at least one IPO two years. In regressions (2)-(3) and (6)-(10), the independent variable of interest is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of proceeds from IPOs in the last two years across all MSAs. Regression (2) includes the full sample; regression (3) includes only MSA years with at least one IPO; regression (6) excludes largest 20 MSAs; regression (7) excludes MSAs that never had an IPO; regression (8) includes an indicator variable that identifies whether the MSA belongs in the top quartile of SEO activity in that year. In regression (4) we include only MSA-years with exactly one IPO. The independent variable of interest in regression (5) is an indicator variable that identifies if the MSA belongs in the top quartile of the distribution of IPO proceeds normalized by MSA population. Regression (9) includes only MSAs with similar characteristics (matched on population, employment, the number of public firms, the number of public firms, and lagged dependent variable). Regression (10) is the same as (9) but includes the high SEO activity indicator. In all regressions we control for MSA characteristics such as population, the number of private firms, the number of people employed, and the number of public firms. We include MSA and year fixed effects. We cluster at the MSA and year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IPOs>0 - Lag 1	0.293***									
	(0.075)									
IPOs>0 - Lag 2	0.001									
	(0.061)									
1-IPO				0.228***						
				(0.065)						
IPO Proceeds High		0.351***	0.236**			0.493***	0.321***	0.350***	0.326**	0.332**
		(0.113)	(0.114)			(0.156)	(0.104)	(0.113)	(0.140)	(0.139)
IPO Proceeds/Capita - High					0.228**					
					(0.092)					
SEO Proceeds High								0.029		-0.158
								(0.130)		(0.135)
Ln(Population)	-5.219***	-5.239***	-6.214***	-5.155***	-5.229***	-5.280***	-6.919***	-5.239***	-8.427***	-8.402***
	(1.319)	(1.319)	(2.229)	(1.330)	(1.316)	(1.355)	(1.292)	(1.319)	(1.849)	(1.857)
Ln(Private firms)	-1.995***	-2.003***	-2.825***	-1.896***	-2.005***	-2.005***	-1.816***	-2.003***	-2.725**	-2.752**
	(0.557)	(0.558)	(0.937)	(0.519)	(0.558)	(0.545)	(0.586)	(0.558)	(1.162)	(1.154)
Ln(Employment)	5.783***	5.826***	8.625***	5.536***	5.826***	5.811***	6.928***	5.826***	11.453***	11.468***
	(1.413)	(1.413)	(2.295)	(1.388)	(1.414)	(1.426)	(1.389)	(1.413)	(1.955)	(1.953)
Ln(Public firms)	-0.012	0.005	0.205	-0.025	0.003	0.002	0.013	0.005	-0.328	-0.320
	(0.085)	(0.085)	(0.247)	(0.085)	(0.085)	(0.086)	(0.095)	(0.085)	(0.365)	(0.365)
Observations	11346	11346	1911	10264	11346	10695	8525	11346	2047	2047
Adjusted R^2	0.566	0.566	0.647	0.566	0.565	0.555	0.619	0.565	0.669	0.669
MSA, Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table. A.V.i: The Effect of Local Economic Activity on Future IPO Activity. This table corresponds to row (10) of the meta-table presented in Table A.V. The dependent variable in each regression is Ln(1+IPO Proceeds$_t$). In each of the regressions in (1)-(10), the independent variables of interest are the first three lags of the following real estate and economic outcome variables X: (1) mortgage origination; (2) new housing starts; (3) median home price for houses in the bottom tercile of home values in the MSA; (4) median home price for houses in the top tercile of home values in the MSA; (5) employment; (6) net job creation; (7) business establishment starts; (8) per capita income. All regressions include the first three lags of the dependent variable. We also control for MSA characteristics such as population, the number of private firms, the number of people employed, and the number of public firms. All regressions include MSA and year fixed effects. We cluster at the MSA and year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Y = Ln(1+IPO Proceeds)							
	X= Mortgage origination	*X= Housing starts*	*X=Home price (Low)*	*X=Home price (High)*	*X=Employment*	*X=Net job creation*	*X=Business starts*	*X=Per capita income*
Lag 1 of X	-0.016	-0.016	-0.262	-0.229	-0.341	-0.155	0.000	0.003
	(0.021)	(0.027)	(0.192)	(0.171)	(0.410)	(0.131)	(0.267)	(0.004)
Lag 2 of X	-0.012	-0.015	0.293	0.161	-0.360	-0.249*	-0.246	0.001
	(0.019)	(0.020)	(0.321)	(0.270)	(0.407)	(0.141)	(0.258)	(0.003)
Lag 3 of X	-0.027	-0.016	-0.174	-0.074	-0.473*	-0.033	-0.238	-0.006*
	(0.017)	(0.023)	(0.219)	(0.181)	(0.246)	(0.102)	(0.193)	(0.003)
Observations	5235	8300	3144	3411	10248	7198	10164	10248
Adjusted R^2	0.932	0.926	0.947	0.945	0.928	0.925	0.928	0.928
MSA, Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.VI: Placebo regressions. The ZIP code year observations in our sample are from counties with no IPO activity in a given year that we treat as if there were IPOs (placebo) using the distribution of IPOs in the county from a different year. In regressions (1)-(6), the dependent variable is the annual growth rate in the two-year period post-IPO for ZIP code: (1) home prices index, (2) top-tier homes values, (3) employment, (4) establishments in the non-tradable sector, (5) establishments in construction, and (6) credit card spending. *IPO HQ ZIP Code* is a dummy variable indicating if the headquarters (HQ) of the IPO firm is in that ZIP code. The IPO proximity variables indicate ZIP codes with no IPO activity and are between either zero and two, two and five, or five and 10 miles away from another ZIP code with at least one IPO in the same county-year. In all regressions, we include the first lag of the dependent variable, the number of establishments, employment, ZIP code population, population density, and wage income. All regressions include county-year fixed effects. We cluster at the ZIP code and county-year level and report standard errors in parentheses. Significance at the 1%, 5%, and 10% is indicated respectively with *** (p<0.01), ** (p<0.05), and * (p<0.10).

	(1) HPI Growth	(2) Top-Tier Home Value Growth	(3) Employment Growth	(4) Establishments (Non-Tradable) Growth	(5) Establishments (Construction) Growth	(6) Credit Card Spending Growth
IPO HQ ZIP Code>0	0.0005	-0.0011	0.0030	-0.0063	-0.0010	0.0118
	(0.0015)	(0.0019)	(0.0052)	(0.0044)	(0.0059)	(0.0167)
0< miles from IPO <=2	0.0040	0.0014	-0.0052	-0.0064	0.0075	0.0378
	(0.0026)	(0.0031)	(0.0081)	(0.0081)	(0.0137)	(0.0385)
2< miles from IPO <=5	0.0013	-0.0002	0.0006	-0.0066	-0.0014	0.0220
	(0.0014)	(0.0021)	(0.0055)	(0.0045)	(0.0067)	(0.0168)
5< miles from IPO <=10	-0.0007	-0.0007	-0.0024	-0.0028	-0.0057	0.0200
	(0.0013)	(0.0018)	(0.0048)	(0.0050)	(0.0055)	(0.0140)
Ln(Population)	-0.0030***	-0.0025***	0.0001	-0.0002	-0.0033	-0.0302***
	(0.0009)	(0.0010)	(0.0035)	(0.0030)	(0.0028)	(0.0084)
ZIP Pop. Density	0.0009***	0.0009***	-0.0014***	-0.0001	-0.0010**	0.0043***
	(0.0002)	(0.0001)	(0.0004)	(0.0004)	(0.0004)	(0.0014)
Ln(Wage Income)	0.0082***	0.0071***	0.0092**	0.0148***	-0.0002	0.0792***
	(0.0015)	(0.0016)	(0.0041)	(0.0039)	(0.0039)	(0.0090)
Observations (ZIP-years)	6775	5708	6728	6431	6430	2555
Adjusted R^2	0.929	0.926	0.165	0.080	0.224	0.583
County-year FEs	Yes	Yes	Yes	Yes	Yes	Yes

Table A.VII: List of industries in non-tradable, tradable, and construction sectors

Non-Tradable Sectors:	Construction:
Grocery stores	Logging
Specialty food stores	Residential building construction
Beer wine and liquor stores	Nonresidential building construction
Health and personal care stores	Utility system construction
Gasoline stations	Land subdivision
Clothing stores	Highway street and bridge construction
Shoe stores	Foundation structure and building exterior contractors
Jewelry luggage and leather goods stores	Building equipment contractors
Sporting goods hobby and musical instrument stores	Building finishing contractors
Book periodical and music stores	Other specialty trade contractors
Department stores	Veneer plywood and engineered wood product manufacturing
Other general merchandise stores	Cement and concrete product manufacturing
Florists	Architectural and structural metals manufacturing
Office supplies stationery and gift stores	Household and institutional furniture and kitchen cabinet manufacturing
Used merchandise stores	Lumber and other construction materials merchant wholesalers
Other miscellaneous store retailers	Building material and supplies dealers
	Lawn and garden equipment and supplies stores
Automobile dealers	Lessors of real estate
Other motor vehicle dealers	Offices of real estate agents and brokers
Automotive parts accessories and tire stores	Activities related to real estate
Furniture stores	Architectural engineering and related services
Home furnishings stores	Sawmills and wood preservation
Electronics and appliance stores	Other wood product manufacturing
Tradable Sectors:	
Aerospace product and parts manufacturing	Metal ore mining
Agriculture construction and mining machinery manufacturing	Metalworking machinery manufacturing
Alumina and aluminum production and processing	Motor vehicle body and trailer manufacturing
Animal food manufacturing	Motor vehicle manufacturing
Animal slaughtering and processing	Motor vehicle parts manufacturing
Apparel accessories and other apparel manufacturing	Navigational measuring electromedical and control instruments manufacturing
Apparel knitting mills	Nonferrous metal (except aluminum) production and processing
Audio and video equipment manufacturing	Nonmetallic mineral mining and quarrying
Bakeries and tortilla manufacturing	Office furniture (including fixtures) manufacturing
Basic chemical manufacturing	Oil and gas extraction
Beverage manufacturing	Other chemical product and preparation manufacturing

Boiler tank and shipping container manufacturing	Other electrical equipment and component manufacturing
Clay product and refractory manufacturing	Other fabricated metal product manufacturing
Coal mining	Other food manufacturing
Commercial and service industry machinery manufacturing	Other general purpose machinery manufacturing
Communications equipment manufacturing	Other leather and allied product manufacturing
Computer and peripheral equipment manufacturing	Other miscellaneous manufacturing
Converted paper product manufacturing	Other nonmetallic mineral product manufacturing
Cut and sew apparel manufacturing	Other textile product mills
Cutlery and handtool manufacturing	Other transportation equipment manufacturing
Dairy product manufacturing	Paint coating and adhesive manufacturing
Electric lighting equipment manufacturing	Pesticide fertilizer and other agricultural chemical manufacturing
Electrical equipment manufacturing	Petroleum and coal products manufacturing
Engine turbine and power transmission equipment manufacturing	Pharmaceutical and medicine manufacturing
Fabric mills	Plastics product manufacturing
Fiber yarn and thread mills	Printing and related support activities
Fishing	Pulp paper and paperboard mills
Footwear manufacturing	Railroad rolling stock manufacturing
Forest nurseries and gathering of forest products	Resin synthetic rubber and artificial synthetic fibers and filaments manufacturing
Foundries	Rubber product manufacturing
Fruit and vegetable preserving and specialty food manufacturing	Seafood product preparation and packaging
Glass and glass product manufacturing	Semiconductor and other electronic component manufacturing
Grain and oilseed milling	Ship and boat building
Hardware manufacturing	Soap cleaning compound and toilet preparation manufacturing
Household appliance manufacturing	Software publishers
Industrial machinery manufacturing	Spring and wire product manufacturing
Iron and steel mills and ferroalloy manufacturing	Sugar and confectionery product manufacturing
Leather and hide tanning and finishing	Textile and fabric finishing and fabric coating mills
Machine shops; turned product; and screw nut and bolt manufacturing	Textile furnishings mills
Manufacturing and reproducing magnetic and optical media	Tobacco manufacturing
Medical equipment and supplies manufacturing	Ventilation heating air-conditioning and commercial refrigeration equipment manufacturing