Initial Public Offerings and the Local Economy

Jess Cornaggia
Pennsylvania State University

Matthew Gustafson
Pennsylvania State University

Jason Kotter
Brigham Young University

Kevin Pisciotta
University of Kansas

March 2020

Abstract

After accounting for endogeneity in the IPO decision, areas hosting large companies that go public experience muted growth in employment, establishments, and population, relative to areas where firms remain private. These effects are most pronounced in low income areas. Establishment-level analyses and tests of IPO-filer acquisition activity reveal that transitioning to public ownership causes firms to geographically diversify their establishments and employee base. These findings are consistent with public ownership reducing a firm's reliance on local agglomeration economies, to the detriment of the local community.

* Jess Cornaggia (jnc29@psu.edu) and Matthew Gustafson (mtg15@psu.edu) are at the Smeal College of Business, Pennsylvania State University. Jason Kotter (jasonkotter@byu.edu) is at Brigham Young University. Kevin Pisciotta (kpisciotta@ku.edu) is at the University of Kansas. Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed. For helpful comments we thank Ramin Baghai, David Becher, Dan Bradley, John Chalmers, Emanuele Colonnelli, Kimberly Cornaggia, Ran Duchin, Laura Field, Laurent Fresard, Sabrina Howell, Peter Iliev, Michelle Lowry, Jing Wang, Fei Xie, conference participants at the 2018 FRA, 2019 FIRS, 2019 EFA, and 2020 AFA conferences, and seminar participants at Drexel University, Hong Kong University of Science and Technology, Michigan State University, Pennsylvania State University, the University of Oregon, the University of Delaware, the University of Hong Kong, the University of Houston, the University of Kansas, and the University of South Florida.

Introduction

Policymakers, industry practitioners, and academics frequently argue that initial public offerings (IPOs) promote new business creation, job opportunities, and investor interest. While there is debate regarding the magnitude of these effects, few would argue against the idea that a vibrant IPO market is a net positive for economic growth. It is less clear, however, whether the transition of a large firm from private to public ownership positively affects the local economy where the firm originates.

Firms undergo several substantial changes when transitioning to public ownership. Two of the largest are increased access to capital and increased visibility, which may accrue via a variety of channels such as media attention, public disclosures, due diligence, or underwriter certification (see e.g., Roell, 1995; Brau and Fawcett, 2006). These changes expand firms' investment opportunity sets by increasing bargaining power for capital, labor, and other inputs to production (see e.g., Pagano, Panetta, and Zingales, 1998; Turban and Cable, 2003). These effects, in turn, may lead to an increase in local investment and local economic growth. This growth can accrue both directly via increased demand for local inputs, and indirectly via enhanced agglomeration economies, as the area becomes more attractive for other businesses looking to shares goods, people, or ideas. Empirical evidence supports this claim, as Bernstein, Colonnelli, Giroud, and Iverson (2018), Greenstone, Hornbeck, and Moretti (2010), and Dougal, Parsons, and Titman (2015) show the importance of local agglomeration economies for fostering growth and vibrancy among co-located businesses.

Alternatively, a local firm going public may stunt local production, and consequently local economic growth, if IPOs disproportionately lower the cost of non-local inputs and lead firms to expand outside their home county.[1] This effect may occur if, for instance, going public mitigates costly information frictions associated with contracts between geographically distant parties (see e.g., Bonte, 2008; Costello, 2013; Knyazeva and Knyazeva, 2012; Hollander and Verriest, 2016). Going public may also lead to less local economic growth if the enhanced access to capital allows newly public firms to undertake large non-local projects at the expense of more marginal

[1] Local production may also be stunted if the negative effect of increased agency costs on long-term investment (see e.g., Bernstein, 2015 and Asker, Farre-Mensa, and Ljungqvist, 2015) more than offsets the aforementioned increase in the IPO firm's investment opportunity set.

local projects. Both of these channels predict that reductions in local economic growth will be accompanied by geographical expansion of firms going public.

We propose an empirical strategy to identify the causal effect of going public on local economic growth, and then provide descriptive evidence on the mechanism underlying any observed effect. The central challenge to identifying the effect of going public on local economic growth is that prosperous local economies are more likely to host the types of firms that ultimately go public. Indeed, we find that past and future county-level employment and establishment growth rates are positively associated with hosting a firm that goes public. To address this selection problem, we first restrict the sample to county-years with IPO filings. We then exploit quasi-random variation in whether a firm completes its IPO to identify the causal effect of going public on local growth. Thus, the idea behind our empirical approach is to compare future local economic growth in areas where private firms randomly complete their IPOs to growth in areas where similar firms randomly withdraw their IPO filings.

Similar to Bernstein (2015), we use two-month market returns following an IPO filing to instrument for IPO completion in a two-stage least squares (2SLS) framework.[2] The rationale for this instrument is that during the initial two months after filing for an IPO (i.e., the book building phase) a firm needs to drum up investor support for its stock. Market fluctuations during the book building phase can affect investors' appetite for the firm's stock, and consequently influence the likelihood that the IPO is completed. However, random two-month stock market fluctuations are unlikely to affect long-term local growth for other reasons. To judge the appropriateness of these assumptions, we first corroborate that market returns during the book building phase are a significant predictor of IPO completion, with first stage F-statistics that exceed 20 in all of our main specifications. Next, we consider the exclusion restriction, which assumes that (after controlling for economic conditions with year fixed effects and a variety of time-varying measures of local economic growth) market returns in the two months following an IPO filing are unrelated to future economic outcomes, except through their effect on IPO completion. Consistent with this assumption, a) returns during the two-month period immediately following an IPO filing significantly predict local economic growth in the same

[2] Our baseline specification uses value-weighted CRSP market returns, however our main results are insensitive to using two-month NASDAQ returns, as in Bernstein (2015).

direction for all three of our outcomes, but returns during placebo two-month periods fail to predict economic growth in the same direction in 95 out 96 cases, indicating that there is no unconditional relationship between market returns and future local economic growth; and b) there is no relation between market returns during the book building phase and local economic growth prior to the IPO filing. This evidence indicates that our findings are unlikely to be driven by a general relation between market returns during a random two-month period and future local economic outcomes.[3]

We begin the empirical analysis by comparing employment growth rates in counties with quasi-randomly completed IPOs to counties with quasi-randomly withdrawn IPOs. Our estimates indicate an insignificant negative relation between the average IPO and future employment growth in the county. Partitioning on median IPO size, we find that large IPOs significantly reduce local county-level employment growth relative to what would have occurred had the firm remained private, while small IPOs have no significant effect on future local economic growth. During the five years after the average IPO (large IPO), we estimate that employment growth is approximately 39 (115) basis points less per year than areas with withdrawn IPOs. We find similar effects, with slightly lower magnitudes, for county-level establishment growth. Consistent with our identifying assumptions, counties hosting exogenously completed and withdrawn IPOs have parallel trends in economic activity prior to IPO filings.

We estimate that the average IPO slows job growth by between 570 and 830 fewer jobs per year in the county where the firm is located relative to areas with withdrawn IPOs, and that the magnitude of this effect is increasing the importance of the IPO firm to its local economy, measured by IPO size relative to county income. We expect the IPO to directly affect local employment growth at the IPO firm – particularly when it is an important part of the local economy – as well as at other local firms that are hurt due to the IPO firm shifting business away from the area. In later results, we find evidence suggesting that between 40-60% of these job losses come from the IPO firm shifting employment outside of its headquarter county in the

[3] For instance, these placebo tests make it unlikely that the effect of two-month post-filing market returns is driven by heterogeneous sensitivity to market conditions across counties (and thus year-fixed effects insufficiently absorbing the effect of market conditions on future local economic growth). Given the placebo test results, for such a story to represent a violation of our identifying assumption it would have to also be the case that the only time a county's future growth is sensitive to market returns is in exactly the two-months after a local firm files to go public and that this temporary increase in sensitivity is not due to the firm's probability of completing their IPO.

years after going public. Applying a spillover multiplier of around 1 – which is at the lower end of the range estimated by Moretti (2010) – fully accounts for the observed annual job reduction.

We next decompose the post-IPO decline in local employment growth into tradable and non-tradable sectors to investigate how the effect of going public propagates through the local economy. We find that employment growth in the tradable sector declines almost immediately, bottoming out within three years. Consistent with the literature on local multipliers (Moretti, 2010), we also find that the loss in tradable sector jobs is followed by losses in the non-tradable sector over the longer-run (i.e., 7 years). This chain of events suggests that IPOs first lower agglomeration benefits among goods-producing firms; this effect then spills over into non-tradable industries as there are fewer tradeable sector workers to purchase services. It also suggests that it can take local economies nearly a decade to reach a new equilibrium after a large firm goes public.

We next study whether the reduction in local employment is driven by changes in population or unemployment. We find that large IPOs reduce county population growth by about 3 percentage points over the subsequent five years, but have no significant effect on county unemployment rates and a marginally significant negative effect on county-level per-capita personal income growth.

In our final set of tests, we examine whether post-IPO changes in local economic growth are at least partially driven by firms geographically expanding after they go public. To investigate this question, we explore within-firm geographic dispersion of operations before and after IPO filings using establishment-level data obtained from the U.S. Census Bureau's Longitudinal Business Database (LBD). We find that firms more aggressively expand their labor force and establishments outside of their local economy after going public and that this expansion is most pronounced in poorer counties. We corroborate this result using publicly available data and show that firms going public in poorer counties expand operations to new states at a faster rate (as measured by state mentions in post-IPO public filings), relative to firms going public in wealthier areas. To further support the idea of geographic expansion, we show that firms significantly increase non-local acquisition activity and pursue more geographically distant targets following an IPO compared with firms that file and exogenously withdraw.

Our results contribute to several strands of literature. First, we contribute to the agglomeration economics literature. Greenstone, Hornbeck, and Moretti (2010) provide evidence of agglomeration economies by showing that new firms entering a local economy improve the productivity of incumbent establishments. Analogously, Bernstein, Colonnelli, Giroud, and Iverson (2018) find that establishment bankruptcies adversely affect surviving establishments. Dougal, Parsons, and Titman (2015) show that agglomeration economies impact firm policies, as firm investment is highly sensitive to the investment of firms in other industries located nearby. Ma, Murfin, and Pratt (2019) suggest that local agglomeration economies play an important role in determining what type of local capital is available for firms to invest in. We extend this literature by introducing ownership structure as an important determinant of local agglomeration economies that significantly affects how large firms interact with the local economy.

Our paper also relates to the literature on IPOs and the consequences to going public.[4] This literature has advanced substantially in recent years due to the empirical framework we employ that was pioneered by Bernstein (2015), who shows that increased agency costs after going public reduces firms' internal innovation. Borisov, Ellul, and Sevilir (2017) and Babina, Ouimet, and Zarutskie (2017) use similar empirical methods to provide evidence that going public increases IPO firm-level employment and provides an avenue for employees to leave and start their own businesses, respectively. Our paper extends this literature by providing evidence that not only is there a geographic element to how issuers shift their business operations after going public, but such changes negatively impact business activity in issuers' local economies.

On the surface, our findings contrast somewhat with Butler, Fauver, and Spyridopoulos (2019) who show, using OLS analysis and a careful within county-year matching procedure, that the housing market heats up in zip codes closely surrounding IPO firms' headquarters after an IPO occurs. They find mixed results when examining establishment and employment growth (their estimates are more often negative than positive when examining growth in zip codes between 2 and 10 miles from IPO headquarters). We also find a positive association between IPO completion and economic growth in our OLS analysis, which suggests that differences in our findings might be explained by the differences in our identification strategies. However, an

[4] See Lowry, Michaely, and Volkova (2017) for a recent survey of this literature.

important difference between our analyses is that we study aggregate effects at the county level, while Butler et al. (2019) study within-county effects at the zip-code level.[5] Thus, Butler et al. (2019) cannot identify county-level effects, and our approach cannot identify within-county, cross-zip code effects. A story whereby IPOs generate a wealth shock that allows employees to move closer to work, but has negative aggregate effects on employment and establishments at the county level is consistent with both our findings and those in Butler et al (2019).[6]

Finally, we contribute to the literatures on how capital markets facilitate economic growth. One stream of the literature examines the relation between stock market development and macroeconomic growth (e.g., King and Levine, 1993; Levine and Zervos, 1998; and Wurgler, 2000), while another examines the relation between access to bank finance and growth (e.g., Schumpeter, 1912; Jayaratne and Strahan, 1996; Cornaggia and Li, 2018). A unique feature of our work is that it examines the effects of stock market development at the local level. Our results do not refute evidence that stock market development stimulates macroeconomic development. But they do suggest that these gains may come at the expense of agglomeration economies in the areas where firms originate.

I. Conceptual Framework

In November of 2005, Under Armour raised nearly $160 million in its IPO. Over the next five years, the company significantly deepened its investment near its headquarters in Baltimore, MD. Under Armour spent $63 million to purchase additional headquarter space, built a 20,000 square foot retail store, and grew its Baltimore workforce from around 500 employees to 2,000. As a result, the company has been a key "growth engine in a very depressed area."[7]

In contrast, shortly after its $241 million IPO, Massachusetts-based Rubius Therapeutics made a five-year $155 million investment to purchase their own manufacturing plant in Rhode Island.[8] Similarly, the Farmington, Utah based Pluralsight, which went public in May 2018,

[5] Other differences between our empirical setting and that in Butler et al (2019) include our focus on large IPOs, our longer sample period, and our examination of five-year instead of two-year economic outcomes.
[6] Given that the typical U.S. worker commutes 25 minutes to work and most zip codes within a county are well within this typical commuting distance, it is plausible that IPO firm workers live in all zip codes within the county.
[7] See "Under Armour gets serious" published in Fortune, available at http://fortune.com/2011/10/26/under-armour-gets-serious/.
[8] See https://www.wpri.com/business-news/biotech-firm-rubius-buys-former-alexion-factory-in-smithfield/1322970442

almost immediately announced that its existing headquarters was too small and that it planned to move to Draper, Utah (two counties away from the original headquarters).[9] These anecdotes illustrate that although some firms increase their local investment after going public, other firms spread their post-IPO investment to different geographic regions.

To better understand how the change from private to public ownership affects an IPO issuer's incentives to invest locally, we develop a simple conceptual framework centered on two of the primary benefits to going public: increased access to capital and increased visibility (see e.g., Roell, 1995; Brau and Fawcett, 2006).[10]

As a result of an IPO, a firm receives a public stock price, is certified by underwriters and institutional investors, and is subjected to increased disclosure requirements, among other things, which improves firm visibility in the marketplace. Improved visibility reduces asymmetric information between the firm and suppliers of goods, labor, and funding, which in expectation allows the firm to negotiate better deals (Roell, 1995). Consistent with visibility improving bargaining power, Turban and Cable (2003) find that enhanced firm visibility increases the size and quality of employee applicant pools, and Pagano, Panetta, and Zingales (1998) and Schenone (2009) find that going public reduces the cost of bank financing. In addition to these benefits, an IPO allows a firm to raise a substantial amount of capital which can then be invested.

To formalize the possible effects that these changes may have on a firm's local investment, consider a private firm that produces a single output using two sets of inputs: local and non-local. Figure 1 illustrates a hypothetical production function. Prior to going public, the firm minimizes the costs of production by choosing to produce using input bundle A. The IPO relaxes the firm's budget constraint for two reasons: first, the additional capital raised through the IPO expands the investment opportunity set; and second, the increased visibility of being public reduces information asymmetry and thus lowers input costs (i.e., lower cost of capital, better terms on trade credit, more productive employees, etc.). Together, these effects result in an outward shift in production, which is consistent with existing evidence that firms expand their

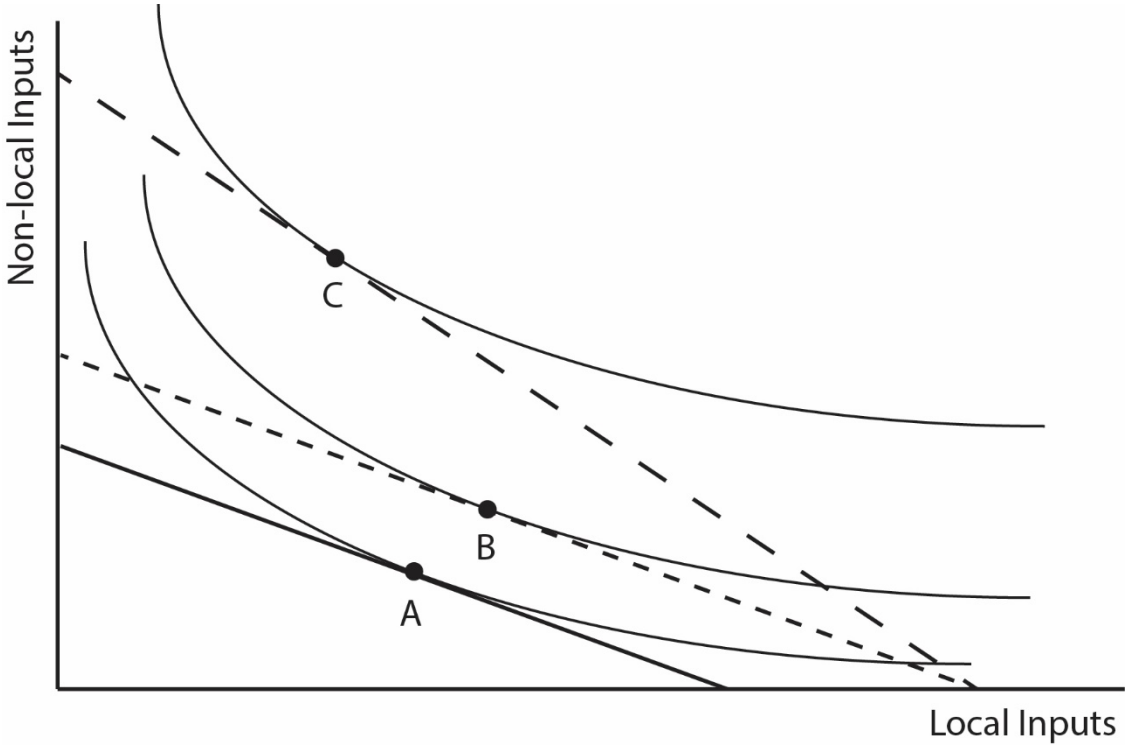
[9] See https://www.deseretnews.com/article/900018299/upcoming-pluralsight-ipo-aims-to-harvest-dollar228m-grow-valuation-to-dollar15b.html

[10] There is also empirical evidence that visibility is an important driver of the decision to go public. Mehran and Peristiani (2009) show that newly public firms that do not receive analyst coverage are much more likely to go private again.

total number of employees after going public (see e.g., Kenney, Patton, and Ritter, 2012, and Borisov, Ellul, and Sevilir, 2017).[11]

How does this newly relaxed budget constraint affect the relative demand for local vs. non-local inputs? Figure 1 illustrates two possibilities. If the reduction in information asymmetry equally affects the costs of local and non-local inputs, there is a parallel shift outward to the short-dashed isocost line. In this case, the firm shifts production to input bundle B and demands more of both local and non-local inputs. This shift will likely benefit the local economy, both directly and via spillover effects, including human capital (Moretti, 2004), labor (Moretti, 2011), and technology spillovers (Greenstone, Hornbeck, and Moretti, 2010).

Figure 1: Production if going public is an input cost shock



Notes: This figure plots a hypothetical firm's isoquants for optimal output.

In contrast, if the visibility and certification associated with going public disproportionately reduces the cost of non-local inputs, the isocost curve shifts to the long-dashed line. In this case, the firm moves production to input bundle C, substitutes non-local

[11] This outward shift in production may be mitigated by the increased agency costs of going public for certain types of firms. For instance, Bernstein (2015) finds that innovative firms reduce innovation after going public.

inputs for local inputs, and consequently boosts non-local demand at the expense of local demand. This substitution away from the local economy reduces potential agglomeration economies for other businesses and generates negative spillovers in business activity throughout the IPO firm's local economy. These spillovers will be larger when the IPO firm makes up a more significant part of the local agglomeration.

This framework cannot speak to which of these outcomes is more likely, but previous work argues that non-local contracting is particularly sensitive to information asymmetry. For example, Costello (2013) finds that geographic distance is related to shorter and more covenant-laden contracts between customers and suppliers, and Knyazeva and Knyazeva (2012) and Hollander and Verriest (2016) document similar evidence of distance-based frictions in the market for bank loans. The idea that information frictions constrain business relationships has also been framed as one of trust (see e.g., Arrow, 1974; Jones, 1995; Mayer, Davis, and Schoorman, 1995; Korsgaard, Schweigar, and Sapienza, 1995). Trust allows firms to invest less in information acquisition (see e.g., Wicks, Berman, and Jones, 1999; Tomkins, 2001), and has been shown to be decreasing in geographical distance (Bonte, 2008). To the extent that the typical IPO firm faces distance-based information asymmetries, it is plausible that the IPO disproportionately lowers the cost of non-local inputs.

In sum, whether a firm transitioning to public ownership positively or negatively affects the local economy is an empirical question. The effect will depend on the firm's production function, the relative change in local and non-local input costs, and the importance of the firm to the local agglomeration. The main contribution of this paper is to empirically identify the aggregate effect of going public on the local economy.

II. Sample Description

Our sample begins with all U.S. IPOs filed between 1986 and 2010 from Thomson One's New Equity Issues database, excluding financial firms, unit trusts, closed-end funds, blank check offerings, ADRs, and special purpose vehicles. Our main measure of county-level economic activity – number of employees – is obtained from the Bureau of Economic Analysis (BEA), which is provided as an annual figure after averaging monthly data. We also collect population and income per capita data from the BEA.[12] We obtain county-level establishments data from the

[12] BEA uses the Census Bureau's annual midyear (July 1) for population estimates.

County Business Patterns (CBP) as of March 12 each year, annual unemployment data from the Bureau of Labor Statistics (BLS), and employment data disaggregated by industry from the Quarterly Census of Employment and Wages (QCEW). The most restrictive of these data series are QCEW industry level data and unemployment data, which begin in 1990 and result in somewhat smaller sample sizes for the accompanying analyses.[13]

We use IPO filing proceeds to classify IPO issuers based on firm size, primarily because other measures of firm size (e.g., employees, sales, etc.) are not available for all firms that withdraw their IPO and remain private. Consequently, we exclude IPOs without reported filing proceeds. When examining our prediction that large IPOs will have a larger effect on the local economy, we define large IPOs as those with filing proceeds (in real terms) greater than the sample median. This definition is noisy when small firms raise substantial amounts of IPO proceeds as was the case during the 1998-1999 IPO tech bubble when many young firms went public at very high valuations. Thus, we exclude these years in our main sample, however in the Appendix we present qualitatively similar results including the tech bubble. [14]

Our sample includes 6,205 IPO filings in 329 counties across 23 filing-years. Seventy-nine percent of these IPOs are completed and 21 percent are withdrawn. Unreported statistics reveal that the portion of IPOs withdrawn per year is higher in the second half of the sample, but there does not appear to be excessive temporal clustering in withdrawn deals, as no two-year period comprises more than 16% of the sample of withdrawn filings.

In Panel A of Table 1, we present descriptive statistics for the IPO characteristics that we control for throughout our analysis. Because our sample begins in 1986, approximately 10 years before the SEC's EDGAR database consistently catalogs IPO prospectuses, our set of control variables is limited to those that are (1) comprehensively covered by SDC or other databases for both completed and withdrawn IPOs, and (2) unlikely to change throughout the IPO filing

[13] We define IPO filing years from March 12 through March 11. For example, when examining the effect of IPO completion on 5-year post-IPO economic outcomes, we merge IPO filings between March 12, 2002 and March 11, 2003 with the five-year change in an economic outcome from 2002 through 2007. This filing year definition aligns with the CBP data, which is the earliest reported economic data within the calendar year. Defining the calendar year so that the end of the market return period aligns with the start of the CBP data year, or the (approximated) BEA/QCEW data year on July 1st, produces qualitatively similar results.

[14] We define the tech bubble period as IPOs filed in 1998 or 1999. Lowry, Officer, and Schwert (2010) define the bubble period as IPOs issued between September 1998 and August 2000. Given our use of withdrawn IPOs, we cannot perfectly mimic their sample restriction.

process. We enforce these requirements so that our control variables are uniformly measured at the initial filing for completed and withdrawn deals. Panel A shows that the completed and withdrawn IPOs in our sample are similar in terms of venture capital or private equity backing, underwriter reputation, and the number of lead underwriters employed. However, withdrawn IPOs are somewhat larger. Specifically, the issuers that complete their offerings file for approximately $81 million in proceeds (in 2011 dollars), compared with $106 million for ultimately withdrawn offerings. Within the subset of large IPOs, however (of which approximately 27% are withdrawn), the filing proceeds are quite similar ($149 vs. $154 million). The industry distribution of completed and withdrawn IPOs is also nearly identical (for both the full sample and the subset of large IPOs) with the three most frequent sectors being manufacturing (NAICS 31-33, approximately 41% of the sample), Information (NAICS 51, approximately 12% of the sample), and Professional, Scientific, and Technical Services (NAICS 54, approximately 8% of the sample).

Figure 2 illustrates the geographical dispersion of the IPOs in our sample. Notably, the IPOs in our sample are spread across much of the United States. While there is a concentration of IPOs in Silicon Valley, our results are not driven by these IPOs. In the Appendix, we show that our results are robust to dropping California from our sample.

III. Identification Strategy

III.A Identification Challenges

There are at least two challenges to identifying the causal effect of IPOs on the local economy, which guide our sample construction and empirical design. First, private firms select where they locate. Comparing counties with IPO filings to counties without IPO filings is problematic because areas that host IPO filers differ from counties that do not host IPO filers.

Panel B of Table 1 illustrates several differences between these two types of counties. The top three rows of Panel B of Table 1 show that county-years with an IPO filing are around fifteen times larger than other county-years in terms of population and total employees. Inflation adjusted per capita income is approximately 40% higher in IPO county-years. More importantly, IPO county-years exhibit significantly higher past and future growth in employment and population compared with county-years without IPO filings. This pattern highlights the need for comparing county-years that do not differ on whether they host IPO filers.

Our IPO-centric sample eliminates this problem by restricting the sample to county-years with at least one IPO filing. A remaining issue is the possibility that, conditional on filing, private firms select whether to complete their IPO based in part on the future prospects of the local economy. Panel C of Table 1 suggests that this occurs. Although county-years with exclusively completed IPOs are similar to counties with exclusively withdrawn IPOs in terms of employment and population levels, counties with completed IPOs have significantly higher lagged and future growth rates in employees and population. Because firms that complete an IPO are more likely to be located in counties that are growing faster, naively comparing the outcomes of completed and withdrawn IPOs will tend to overstate the positive impact of IPOs on local economic conditions.

We examine the relation between IPO completion and county growth more formally in Table 2, which reports OLS regressions that estimate the conditional association between completing an IPO (as opposed to withdrawing the IPO) and the annual geometric average of five-year county employment growth following an IPO filing. The regressions in all three columns are estimated at the IPO level. The positive coefficients on IPO completion suggest that counties with completed IPOs experience higher future employment and establishment growth than counties with withdrawn IPOs, after controlling for lagged economic conditions.

While these coefficients are not statistically significant, the positive association between completed IPOs and future employment growth is consistent with evidence in Butler, Fauver, and Spyridopoulos (2019). However, the statistical methods used thus far are not sufficient to claim that the relation is causal. A causal interpretation would likely predict the effects of an IPO on local economic growth to be even stronger for large IPOs, which should contribute more to local agglomerations. However, comparing Columns 2 and 3 we find no evidence that the relation between local economic growth and IPO completion is increasing in IPO size.

III.B Empirical Specification

Identifying how a firm going public affects the local economy requires a setting that can compare future local economic growth in counties hosting firms that complete their IPO with

those same outcomes in similar counties hosting otherwise similar firms that do not complete IPOs. In this section, we discuss our identification strategy, which approximates such a setting.

We begin, as stated above, by restricting the sample to county-years that experience a local firm filing for an IPO. This means that all firms in our sample are at a similar point in their life cycle. To address the endogeneity of the IPO completion decision, we use an instrumental variable approach. Similar to Bernstein (2015), we use fluctuations in the two-month market returns following an IPO filing to instrument for IPO completion. We use broad market returns, as opposed to NASDAQ returns as in Bernstein (2015), because (unlike Bernstein who focuses on innovative firms) our sample contains a representative set of IPO issuers.[15] Nonetheless, our findings are similar when we use two-month post-filing NASDAQ returns as the IV (see Appendix B for these results).

Our first stage model regresses an indicator for IPO completion on two-month post-filing market returns, in addition to controls for economic conditions:

$$\begin{aligned} IPO\ Completion_{it} = {} & \alpha_1 Market\ Ret._t + \alpha_2\ Emp.\ Growth_{kt-1} + \alpha_3\ Pop.\ Growth_{kt-1} + \\ & \alpha_4\ Income\ Growth_{kt-1} + \alpha_5\ Ln(IPOs)_{kt} + \alpha_6\ Ln(IPO\ Size)_{it} + \\ & \alpha_7\ Leads_{it-1} + \alpha_8\ PE\ \ or\ VC_{it} + \alpha_9\ Underwriter\ Rep._{it} + \delta_k + \lambda_j + \gamma_t + \varepsilon_{it} \end{aligned} \quad (1)$$

, where *IPO Completion* equals one for a completed IPO and zero for a withdrawn IPO. *Market Ret.* is the two-month CRSP value-weighted return following the IPO filing. We control for nationwide economic conditions with year fixed effects (γ_t). We further control for local economic conditions with the pre-IPO filing annual percentage growth in the number of employees, population, and income per capita in a county, along with county fixed effects (δ_k). We control for the natural log of the number of IPOs in the county-year, as well as several IPO characteristics including IPO size (i.e., real proceeds filed), the number of lead managers, private

[15] In addition, the geographic concentration of firms driving fluctuations in the total market index is much more dispersed, and movements in this index are likely to be less sensitive to valuation shocks for firms co-located with our IPO firms. Thus, the use of broad market returns also helps mitigate the possibility that IPO firms are co-located with firms that drive fluctuations in NASDAQ returns.

equity or venture capital backing, and underwriter reputation.[16] Finally, we also include 2-digit NAICS industry(λ_j) fixed effects. See Appendix A for variable definitions and data sources.

Under the identifying assumptions of 2SLS, which we discuss in detail below, the following second stage regression will estimate the causal effect of IPO completion on local economic activity:

$$\Delta\ Econ.\ Outcome_{kt,t+5} = \beta_1 Instrumented\ IPO\ Completion_{it} + \beta_2 Emp.\ Growth_{kt-1} + \beta_3 Pop.\ Growth_{kt-1} + \beta_4 Income\ Growth_{kt-1} + \beta_5 Ln(IPOs)_{kt} + \beta_6 Ln(IPO\ Size)_{it} + \beta_7 Leads_{it-1} + \beta_8 PE\ or\ VC_{it} + \beta_9 Underwriter\ Rep._{it} + \delta_k + \lambda_j + \gamma_t + \varepsilon_{it} \quad (2)$$

, where Δ *Econ. Outcome* represents the annualized (geometric average) percent change in economic activity in county *k* over the five years beginning at time *t*, i.e., the year of the IPO filing. Our primary measure of economic activity is county-level employment growth, though we also examine changes in establishments, population, unemployment, and income. *Instrumented IPO Completion* is the predicted value from Equation 1. Because economic activity is both persistent within a county and correlated across counties in a given year, we double cluster our standard errors at the county and year levels. Results are similar without clustering or clustering only at the county level.

III.C Identifying Assumptions

Our identifying assumption is that, after controlling for other determinants of IPO completion and county-level economic conditions, two-month CRSP market index fluctuations following an IPO filing are a significant predictor of IPO completion, but are otherwise unrelated to a county's future economic growth.

In Table 3, we estimate the first stage regression (i.e., Equation 1) to examine the relevance condition, which requires that our instrument, $Market\ Ret._{it}$, is a significant predictor of IPO completion. We estimate the first stage for both the entire sample of IPOs (Columns 1 and 2) and for the subset of small (Column 3) and large (Column 4) IPOs. Consistent with prior

[16] As we discuss throughout the paper, the choice of IPO-level control variables has little effect on our findings. Because our sample begins before the coverage of the Securities and Exchange Commission's Electronic Data Gathering and Retrieval System, the IPO-level control variables are limited to variables that SDC consistently populates for withdrawn IPOs.

evidence linking market fluctuations during the book-building period to IPO completion, the coefficient on market returns is positive and highly statistically significant across all four columns.[17] It is also economically meaningful; for example, the estimate of 0.75 in Column 4 suggests that a 10% increase in market returns in the two months after an IPO filing predicts a 7.5 percentage point increase in the probability of IPO completion (approximately a 10% increase relative to the sample mean). Across all four columns of Table 3, we see that post-filing market fluctuations are a strong predictor of IPO completion. Our first stage Kleibergen-Paap F-statistic is 46 for the full sample and 24 for the subsample of large IPOs, which easily exceeds the threshold of 16, which Stock and Yogo (2005) note limits the potential bias of instrumental variable (IV) estimates attributable to weak instruments to at most 10%.[18] The small IPO subsample is slightly weaker with an F-statistic of 11. However, this is still above the Stock and Yogo (2005) threshold of 9, which limits the potential bias to at most 15%.

The second half of our identifying assumption, the exclusion restriction, requires that two-month post-IPO market fluctuations are unrelated to future economic growth, except through their effect on IPO completion. Although this condition is unlikely to be satisfied unconditionally, the condition is plausible after including year and county fixed effects (and other controls for current local economic conditions) in the regression. A violation of this assumption would require that market returns during a seemingly arbitrary two-month period predict future changes in economic growth, after controlling for current macro-economic conditions via time fixed effects, persistent differences across counties via county fixed effects, and current local economic conditions via measures of lagged local economic growth. Although we cannot rule out such a possibility entirely, a strength of our setting is that we can conduct a series of reduced-form placebo tests to examine the likelihood that such a violation of the exclusion restriction exists.

These placebo tests take the following form:

[17] See, e.g., Dunbar (1998), Busaba, Benveniste, and Guo (2001), Benveniste, Ljungqvist, Wilhelm, and Yu (2003), Edelen and Kadlec (2005), Brau and Fawcett (2006), Dunbar and Foerster (2008).

[18] Additionally, both the Anderson-Rubin Wald test and Stock-Wright LM test reject the null of weak instruments at the 1% level, and our R-squared is greater than 13% (contrary to the 3.2% that Butler et al. (2019) reference). Finally, following Hahn and Hausman (2003) to impute the potential bias of our estimates, our partial R-squared estimates indicate that any 2SLS bias attributable to weak instruments is at most 4%.

$$\Delta \text{ Econ. Outcome}_{kt,t+5} = \delta_1 \text{Two-Month Market Ret.}_{it} + \delta_2 \text{ Emp. Growth}_{kt-1} + \delta_3 \text{ Pop. Growth}_{kt-1} + \delta_4 \text{ Income Growth}_{kt-1} + \delta_5 \text{ Ln(IPOs)}_{kt} + \delta_6 \text{ Ln(IPO Size)}_{it} + \delta_7 \text{ Leads}_{it-1} + \delta_8 \text{ PE or VC}_{it} + \delta_9 \text{ Underwriter Rep.}_{it} + \delta_k + \lambda_j + \gamma_t + \varepsilon_{it} \quad (3)$$

, where *Two-Month Market Ret.*$_{it}$ is measured at a variety of time periods surrounding the firm's IPO filing date. To the extent that our exclusion restriction is valid, we expect no relation between *Two-Month Market Ret.*$_{it}$ and future economic outcomes when *Two-Month Market Ret.*$_{it}$ is measured after the book building period of a local firm filing to go public, since by this point market returns cannot affect the probability of the firm completing their IPO. We also expect little relation between *Two-Month Market Ret.*$_{it}$ and future local economic outcomes when measuring market returns prior to the IPO filing, although market returns during this period could affect the realized population of IPO filings we observe, and therefore may not be entirely independent.

Panel A of Figure 3 presents estimates of δ_1 from Equation 3 using five-year employment growth as the dependent variable. Each point on the solid line represents estimates from a regression in which *Two-Month Market Ret.*$_{it}$ is measured starting in the month indicated on the x-axis. The vertical lines represent 95% confidence intervals for the point estimate of δ_1. Consistent with our exclusion restriction, none of the twenty-five event-time windows over which *Two-Month Market Ret.*$_{it}$ is computed load in the same direction as the event-time window measured immediately following a local IPO filing, and only one loads positively. As would be expected by random chance, twelve of the twenty-five estimates are positive and thirteen are negative. Panels B, C, and D present similar results estimating Equation 3 with future establishment, population, and per capita income growth as dependent variables. Out of these 100 total regressions, market returns are only negatively correlated with future economic growth in 5 regressions, 4 of which are when market returns are measured during the book building phase of the IPO.

Bernstein (2015) – the first paper to employ a similar instrument – conducts several additional tests that are consistent with our exclusion restriction when estimated at the firm-level, as opposed to the county-level as in our setting. Given this existing evidence, we defer additional placebo tests and other robustness analyses to Appendix B.

IV. Main Results: The Effects of IPOs on Local Economic Growth

In this section, we estimate the effect of IPOs on local economic activity using the 2SLS procedure outlined in Section III. Our main measure of local economic activity is the number of employees in an IPO filer's headquarter county.

Table 4 presents second-stage 2SLS estimates, using a county's average annual employment growth rate over the five years following an IPO filing as the dependent variable. The explanatory variable of interest, *Instrumented IPO Completion*, is the fitted value from the corresponding column in Table 3. The coefficient can be interpreted as the effect of an IPO being randomly completed (i.e., nudged to completion due to two-month post-filing market returns that were just favorable enough) in the county in year zero relative to what would have happened had the IPO been randomly withdrawn.[19]

In Column 1, we estimate the effect of IPO completion over the full sample of IPOs. The estimated coefficient is negative, but statistically insignificant with a t-statistic of -1.2. In Column 2, we limit the sample to the subset of small IPO filings (below median proceeds) and again we find no significant relation between IPO completion and future local employment growth. Column 3 restricts the sample to the larger half of IPO filings, which will tend to be more important to local business agglomerations. The significantly negative coefficient on *Instrumented IPO Completion* in Column 3 of Table 4 indicates that a large firm going public results in less county-level employment growth over the succeeding five years, compared to what would have happened had the firm remained private. The effect is substantial; Column 3 suggests that a county that hosts a large IPO grows about 1.15 percentage points slower per year than it would have grown if the large firm had stayed private.

In Figure 4 we explore the evolution of the effect that a large IPO has on local economic growth by examining the post-IPO employment decline in event time surrounding the IPO filing year. Specifically, in Panel A we plot the *Instrumented IPO Completion* coefficient from a series of regressions using the change in employment from year zero through the year indicated on the x-axis as the dependent variable. All explanatory variables are identical to those used in Column

[19] Firms can be nudged to completion even when market returns are bad. All that is necessary is that realized market returns put the firm close to their indifference point as to whether or not they are willing to complete their IPO.

3 of Table 4.[20] This figure shows that employment growth begins to decline in the year following the IPO filing, and continues to decline at a steady rate over the five-year post-filing window, with the effect becoming significant at the 5% level two years after the IPO filing. Unreported results indicate that the employment growth reduction persists through the eighth post-filing year, albeit at a slower rate. In addition to providing evidence on the timeline of the IPO effect, Figure 4 provides descriptive support for our identifying assumptions. *Instrumented IPO Completion* is not significantly related to pre-filing employment growth, indicating similar pre-trends between counties hosting exogenously completed and withdrawn IPOs. This pattern mitigates the possibility that *Instrumented IPO Completion* is spuriously correlated with local economic conditions.[21]

In Figure 5, we decompose the five-year post-IPO employment growth into two broad industry sectors – tradable and non-tradable – to better understand how going public impacts economic linkages within an IPO firm's agglomeration. The literature on local multipliers (Moretti, 2010) typically characterizes agglomeration dynamics as initiating from an increase or decrease in tradable sector jobs, followed by a spillover to non-tradable jobs, as the non-tradable sector depends on having local consumers to purchase services. We find evidence that IPOs first affect the tradable sector and then spill over into non-tradable industries. Figure 5 shows that employment growth within the tradable sector immediately declines following the IPO and continues to fall until leveling off in the third year at a total decline of about 10 percentage points. Employment then recovers somewhat to level off at a total decline of about 5 percentage points by the eighth year after the IPO. In contrast, employment growth in non-tradable industries is flat in the year immediately following the IPO but begins to slowly decline in the second year. Employment in non-tradeable industries then steadily declines until leveling off in the eighth year at a total decline of about 5 percentage points. This suggests that it takes nearly a decade for a local economy to rebalance after a large firm goes public, but once it does, the

[20] Lagged local economic conditions are measured as of year -4 when measuring pre-IPO filing economic growth.
[21] In unreported results, we provide further support for orthogonality by comparing lagged and future employment growth between counties exposed to extreme high and low post-filing market fluctuations (e.g., top vs. bottom quartile). While the "positive" shock group has similar one, two, and three-year lagged growth as the "negative" shock group, the positive shock group has significantly lower long-run future county employment growth.

county has a similar proportion of employees in tradable and non-tradable sectors as it did prior to the IPO.

In Table 5, we examine heterogeneity in this effect by interacting *Instrumented IPO Completion* with both *IPO size*, and measures designed to capture the vibrancy of the local economy, using the full sample of IPOs.[22] Consistent with the evidence in Table 4, the insignificant coefficient on *Instrumented IPO Completion* in Column 1 indicates no significant relation between the smallest IPOs and future local employment growth. The negative and significant interaction between *Instrumented IPO Completion* and *IPO size* suggests that larger IPOs generate more substantial negative effects on future local employment growth.

With the caveat that 2SLS estimates represent local average treatment effects and that the estimates in Table 5 are accompanied by large standard errors and strong functional form assumptions, we can use the results in Column 1 of Table 5 to generate a back of the envelope estimate of the number of jobs lost due to an average sized IPO (which raises approximately $86 million in proceeds). The estimates suggest that job growth slows by approximately 570 (830) jobs per year in the median (mean) county following the average sized IPO, compared to what would have happened had the IPO been quasi-randomly withdrawn.[23] This effect is large relative to the size of the average IPO firm in our sample (which has around 3,000 employees, based on pre-IPO Compustat estimates), suggesting that there are substantial spillover effects from the IPO firm to other businesses in the local economy.

To determine the plausibility of such spillovers, we use U.S. Census Bureau establishment-level data to estimate how much of the job loss can be directly explained by the IPO firm shifting employment away from the headquarter county after going public. The results, reported in Section V.A, suggest that the average IPO firm moves around 330 employees per year out of its headquarters county after going public. Considering only these job losses at the IPO firm as the direct effect of the IPO and applying a spillover multiplier of between .7 and 1.5 – which is at the lower end of the range estimated by Moretti (2010) – explains all of the observed annual job reduction. However, it is likely that the job losses at the IPO firm itself understate the direct

[22] Because we interact the instrumented variable, there are technically 2 separate first-stage regressions. We report the K-P F-statistic for the combined significance of our instruments in the bottom row of Table 5.
[23] We compute these estimates in the following way: [0.00321455-.053386773*0.08660424]*393,908 (570,237), where .08660424 is the sample mean filing proceeds (in $B) from Panel A of Table 1, and 393,908 (570,237) is the sample median (mean) lagged county employees from Panel B of Table 1.

effect of the IPO. For example, local suppliers are likely to be directly affected when the IPO firm geographically expands. Accounting for these other direct effects and allowing for larger multipliers within the range suggested by Moretti (2010) can explain the total estimated number of jobs impacted even if the IPO firm moves substantially fewer than 330 employees outside of the county. This bottom-up estimate indicates that, though large, the total number of lost jobs is quite plausible. Importantly, these estimates suggest that a large IPO results in slower local economic growth than the county could have had if the firm had stayed private, but this does not necessarily imply that people are fired from their jobs or that businesses disappear.

The conceptual framework introduced in Section I provides a starting point for thinking about why an IPO might disrupt local agglomeration economies and curb aggregate county-level economic growth. If an IPO disproportionately lowers the cost of non-local inputs, IPO firms may shift to non-local production after going public, which can create negative externalities for other businesses and reduce growth in the local economy. The negative externalities generated by a shift in production by an important local firm are likely to be particularly severe when the local economy is less vibrant. In Column 2 of Table 5, we proxy for the vibrancy of the local economy with per-capita income, and examine whether the relation between IPO completion and employment growth depends on local economic vibrancy by interacting *Instrumented IPO Completion* with county per-capita income.

Column 2 indicates that the effects of IPO completion on local employment growth are most negative when the county has low per-capita income. In Column 3, we examine the combined effect of IPO size and county income. The negative and significant coefficient on this interaction indicates that the negative effects of an IPO on local economic growth are particularly concentrated in poor counties that host large IPOs. The estimates in Column 3 suggest that in the typical median (mean) county following an average sized IPO, employment growth slows by approximately 350 (500) jobs per year, which is somewhat smaller than the magnitude implied by the estimates in Column 1.[24] Interpreting the results in Table 5 in the context of our conceptual framework in Section I suggests that one reason IPOs occurring in less vibrant areas have such a strong effect on local economic growth is because firms located in these areas are

[24] The average size to county income in our sample is 0.0020257 million. Based on this average, the effect is [0.00546073 – 3.217621*0.0020257], multiplied by the median (mean) number of employees in the county of 393,908 (570,237).

particularly likely to expand outside of their local economy after going public. We provide additional evidence that this is the case in Section V.

In Table 6, we further examine how IPOs affect local economic growth. Panel A indicates that a decline in establishment growth following large IPOs is of similar magnitude to the employment growth decline documented in Table 4, which can also be seen in event time in Panel A of Figure 6. Panels B and C of Table 6 shed light on the potential sources of the reduced employment growth. The two most intuitive sources for the reduced employment growth are a decrease in population or an increase in the unemployment rate. The evidence in Table 6 is most consistent with a population decline driving the employment decline. The estimates in Column 3 indicate that a county's population growth declines by approximately 54 basis points per year during the five years following a completed large IPO. Panel B of Figure 6 indicates that, like the reduction in post-completion employment and establishment growth, the population growth rate declines steadily for five years, and there is no significant relation between *Instrumented IPO Completion* and pre-filing county-level growth. We find no evidence of a significant change in unemployment rates or significant changes in population surrounding small IPOs.

Although we have documented a significant effect of IPO completion on net population and employment flows, it remains unclear how labor costs are affected. To investigate this question in Panel D of Table 6, we use annual average changes in per-capita personal income over the five years after an IPO filing as second-stage dependent variables in our 2SLS framework. We find that IPO completion results in a marginally significant decline in income growth. Five years after the completion of a large IPO, the average county resident has personal income that is approximately 5% less than if the issuer had withdrawn its IPO.

In the Table B2 of the Appendix, we show that the results in this section are robust to including IPOs filed during the stock bubble of 1998-1999 and excluding IPO filings from California. Additionally, the results are robust to instrumenting for the completion decision using NASDAQ, rather than value-weighted CRSP, returns.

Our results thus far consistently suggest that local agglomerations are disrupted when an incumbent large firm goes public: aggregate growth in employees, establishments, population, and income are weakened in the aftermath of a local firm transitioning from private to public ownership, as opposed to if the firm had remained private. In the following sections, we perform

additional analyses to better understand the channel through which the IPO transition generates these negative and persistent effects on local economic growth.

V. Discussion and Descriptive Evidence on Mechanism

The analysis thus far exploits exogenous variation in IPO completion to provide evidence that large IPOs disrupt economic growth at the county level, especially in poorer areas. Although our empirical framework is well-suited to identify the consequences of completed IPOs, pinpointing the exact mechanism through which IPOs affect the local economy requires additional exogenous variation, which we do not have. In this section, we descriptively examine the economic drivers behind the effect of IPOs on local economic growth. In particular, we test the story motivated by our conceptual framework in Section I in which an IPO adversely affects the local economy because it leads the IPO firm to shift toward non-local investment.

V.A More Direct Evidence on IPO Firms' Non-local Growth

To more directly examine the geographic expansion mechanism, we use establishment-level data to study the extent to which IPO firms geographically diversify their operations following their IPO. We then examine whether geographic diversification is related to the average income of the IPO firm's headquarter county. We begin by matching our sample of large IPOs with the U.S. Census Bureau's Longitudinal Business Database (LBD). The LBD records the number of employees at each separate physical location (establishment) of all U.S. businesses with paid employees, which enables us to track the geographic dispersion of firm operations over time. We match firms to establishments in the LBD based on name, county, zip code, and industry using the Business Register (BR).[25]

Specifically, we use a fuzzy text matching algorithm to compare the name of the firm with the name of the establishment and then match based on name, year of the IPO filing, 1-digit SIC industry code, and zip code.[26] We then relax the zip-code constraint and match at the county level. Finally, we attempt to hand-match the remaining observations based on the set of all establishments operating within the same county-year. We successfully match 1,800 large IPO

[25] The BR was formerly called the Standard Statistical Establishment List (SSEL).
[26] We use the generalized Levenshtein edit distance to compare names and match observations with scores less than 200.

firms in our sample.[27] The primary reason that we are unable to match all firms is that the establishment name is often a division or subsidiary name that does not closely correspond to the firm name.

After matching each IPO-filing firm with at least one establishment located in the same county, we use the enterprise identifier contained in the BR to identify and track the location of all of the firm's establishments in the LBD. One limitation of this approach is that establishments of small or new firms are often not correctly grouped together until the next Economic Census (which occurs every 5 years). When the correction is made, the Census changes the enterprise identifier, causing the firm to drop out of our sample.[28] This correction, along with the fact that some firms exit the sample via merger, leads our sample size to shrink, especially when examining employee and establishment growth over long horizons.

Given these constraints, we focus on the two-year window after the filing of an IPO. The results are broadly similar for longer windows, though smaller sample sizes reduce the power of the tests. The LBD allows us to construct two separate measures of the geographic dispersion of firm production: the number of establishments and the number of employees. We define *Employee Growth* as the percentage change in the number of employees in the firm's home county less the percentage change in the number of employees outside of the firm's home county, measured from the year prior to 2 years after the IPO filing. *Establishment Growth* is defined analogously, using the count of the number of establishments within/outside of the home county.

We use a two-stage specification similar to Column 3 in Table 4 to estimate the effect of going public on the geographic dispersion of firm activity and report the results in Table 7.[29] The instrumented effect of IPO completion on relative local firm production is negative and statistically significant. In the two years after a public listing, firms reduce their local county employment by about 22 percentage points relative to non-local employment (Column 1) and reduce the number of local establishments by about 20 percentage points relative to non-local establishments (Column 2). On average, before a firm goes public, between 40 and 50 percent of

[27] Census disclosure requirements require us to round the reported number of observations to the nearest hundred.
[28] We correct for this effect to the extent that we can by matching more than one Census entity identifier to a sample firm, but it is not always possible to track firm links across these changes in identifiers.
[29] The specification we use here matches an earlier version of the paper. We currently do not have access to the Census Data Center to update these results, but will be able to do so when revising the paper for a journal.

employees and establishments of the firm are located in the firm's home county, so these shifts represent a nearly 50% decline in local production over the two years following the IPO. On an annual basis, this implies that the average firm (which has 3,000 employees pre-IPO) reduces its local employment by around 330 employees per year after going public (3,000*0.22/2).

Interpreted in the context of the framework established in Section I, these establishment-level results suggest that, on average, large IPO firms take advantage of reductions in information asymmetry that reduce the relative cost of non-local inputs. Importantly, our framework also suggests that a) for a given level of production shifting, there should be larger effects on local economic growth when the IPO firm is an important part of the local agglomeration (i.e., larger firm located in a less economically vibrant county) which we provide evidence of in Table 5, but also b) that the propensity to shift production should be related to the extent to which the IPO firm would benefit from a reduction in the cost of non-local inputs. Firms that stand to benefit more should shift production to a greater extent.

In general, it's hard to quantify the benefits from a reduction in the cost of non-local inputs ex-ante. However, we hypothesize that firms located in areas with a scarcity of productive labor are more likely to benefit from a reduction in the relative cost of non-local labor, and are more likely to hire non-locally when relative costs fall. Consequently, we use county-level wages as a proxy for the productivity of local labor markets and examine whether firms located in areas with less productive workers (i.e., low wage counties) display an especially strong shift to non-local operations after their IPO.

We provide preliminary evidence of this in Columns 3 and 4 of Table 7. In these specifications, the main effect of IPO completion on local firm production is still negative and significant, but the interaction between *Instrumented IPO Completion* and *Ln(Wages)* is positive (though it is only significant for establishment growth). These findings suggest that firms in poor counties – with arguably a less attractive labor pool – are more likely to shift their establishments and employees to other counties following an IPO.

We further examine this idea using geographic dispersion measures from data collected by Garcia and Norli (2012), which record the number of state-name mentions in firms' 10K reports. Their sample runs from 1995 to 2008 and, after requiring two consecutive data points of 10K state-name mentions in the first two years after going public, we match 1,390 issuers in our

sample to these data (approximately 60% of our completed IPOs over this period). We define Δ *Non-local Operations* as the one-year percentage point change in the ratio of total state mentions that are not the firm's headquarter state to total state mentions. In Appendix Table B3, we show that firms that go public in poor counties have a larger increase their non-local operations as measured by state name mentions in their 10-K reports.

V.B Post-IPO Filing Acquisition Activity

One benefit of going public is developing closer relationships with one or more investment banks. This relationship likely reduces the cost of finding acquisition targets and negotiating acquisition outcomes, particularly for non-local acquisitions where investment bank advice is most valuable. Motivated by this idea, we examine whether IPOs lead firms to purchase geographically distant targets. This analysis builds on the evidence presented in Bernstein (2015) that shows that completing an IPO increases firms' acquisition activity. We expand on this result by examining whether IPO firms shift the geographic focus of their acquisition activity after they complete an IPO.

In Table 8, we use the same 2SLS specification used in Table 4 to estimate the change in local vs. non-local acquisition activity after a firm goes public. The second stage dependent variable is filer-level acquisition activity in the two years after a firm files for an IPO, grouped by whether the target is in the same state as the IPO filer. In particular, we a) match all completed acquisitions conducted by both completed and withdrawn IPO filers over the two-years after their IPO filing, b) classify whether the target firms acquired were located within or outside the IPO filer's headquarter state, and c) sum (and log) the total deal value completed by each issuer over this period for deals in each group (i.e., inside or outside the state).

In Column 1, we corroborate the result in Bernstein (2015)—firms increase the total value of acquisition activity after going public. In Columns 2-4, we expand on this result by showing that after completing an IPO, firms conduct acquisitions of much larger value outside their home state than if they had stayed private, the proportion of total deal value that is non-local is significantly higher, and the distance of non-local mergers from firm headquarters increases. The estimates in Column 2 suggest that on average, IPO firms spend almost 3 times more purchasing non-local peers after going public compared to similar firms that do not go public. Additionally, the proportion of dollar deal volume spent purchasing non-local peers is

about 24% higher for completed vs. withdrawn IPOs (Column 3). Finally, in Column 4 we present suggestive evidence that the average target's distance from firm headquarters is approximately 834 miles further for non-local acquisitions that occur after an IPO. These results suggest that firms not only increase acquisition activity in aggregate as a result of going public but do so in a way that directs more investment to non-local markets. This evidence is consistent with the idea that an IPO reduces the relative cost of non-local production.

Taken together, the evidence in this section is consistent with the conceptual framework presented in Section I and suggests two things: first, one mechanism through which IPOs lead to lower local economic growth is that IPOs provide an avenue for firms to shift business activity outside their local agglomeration. Second, this shift in business activity is at least partially driven by a reduction in the cost of non-local inputs. Consistent with that, firms located in poorer counties with less access to productive labor pools display a particularly pronounced shift to non-local production. There are multiple channels through which a firm might geographically expand; as a result, none of the approaches that we use in this section capture total expansion. Moreover, understanding exactly why going public leads firms to geographically expand is beyond the scope of this paper, as it would require exogenous variation in IPO characteristics. However, such expansion is consistent with either (1) a friction whereby non-local inputs are relatively more expensive for private firms due to lower visibility, or (2) a capital raising friction that leads private firms to take marginal local investments as opposed to larger, disproportionately non-local, investments.

VI. Conclusion

This paper examines the causal effect of going public on local economic growth. We use market fluctuations during the book building phase as an instrument for IPO completion. This approach allows us to compare future economic growth in counties where firms go public to otherwise similar counties where firms file to go public but remain private. While the average IPO has no effect on local economic growth, we find robust evidence that large IPOs reduce economic growth in the county where the IPO firm originates, especially when the IPO firm originates from a poorer area. Reduced employment growth initiates in the tradable sector, and is followed by declines in the non-tradable sector over a longer horizon as the economy stabilizes. We also find a decline in establishment, population and income growth, while unemployment

rates remain stable. Additional tests indicate that one channel through which IPOs bring about reductions in local growth is that IPOs lead firms to expand outside their local economies. When the IPO firm is an important part of the local economy, this geographic expansion weakens the local agglomeration and generates spillover effects that stunt local growth.

Importantly, our findings pertain to growth and economies at the county level. They cannot be interpreted as evidence that IPOs undermine macroeconomic growth. Our findings do suggest a tradeoff though: to the extent that stock market listings facilitate macroeconomic growth, at least some of that growth is offset by a disruption in local agglomeration economies where public firms originate.

Appendix A – Data Descriptions

Variable Name	Variable Definition (source in parentheses)
Independent Variables	
IPO and Market Characteristics	
IPO Completion	Indicator variable taking a value one if an issuer that files for an IPO ultimately completes the IPO, and zero if an issuer that files for an IPO ultimately withdraws the IPO (SDC).
Market Ret.	Cumulative daily CRSP value-weighted Market Index return over the forty trading days beginning the day of an IPO filing (SDC).
IPO Size	Amount of proceeds filed for in the original IPO filing of a prospective IPO issuer, inflation adjusted to 2011 dollars (SDC).
Number of Lead Managers	Number of unique underwriters serving in the role of Lead Manager, as of the initial IPO filing (SDC).
Private Equity	Indicator variable taking a value of one if the IPO firm received pre-IPO private equity or venture capital funding, computed by combining SDC's private equity indicator with a search of all firms receiving private equity and venture capital funding in Thomson One's Venture Xpert database between the years 1975 and 2010.
Underwriter Reputation	Modified Carter-Manaster rankings of the top lead manager of the IPO, as computed in Ritter and Loughran (2004), with updated rankings made available on Jay Ritter's webpage (https://site.warrington.ufl.edu/ritter/ipo-data/).
County Characteristics	
Employee Growth	One year growth rate in the number of full-time and part-time jobs in the county of an IPO, covering wage and salary jobs and self-employment, from the pre-filing year to the filing year. Counts are reported as annual averages of monthly estimates (BEA). More information can be found in the BEA's regional account methodology: https://www.bea.gov/sites/default/files/methodologies/lapi2016.pdf
Establishment Growth	One year growth rate in the number of establishments in the county of an IPO, measured as of March 12 of the pre-filing year to March 12 of the filing year (County Business Patterns). Number of establishments comes from the Business Register, accounting for all single and multi-establishment companies, and is available beginning in 1986 (County Business Patterns, CBP). More information can be found in the County Business Patterns' Data User Guide: https://www2.census.gov/programs-surveys/cbp/resources/2015_CBP_DataUserGuide.pdf
Population Growth	One year growth rate in the Census Bureau's annual population estimates in the county of an IPO filing, from the pre-filing year to the filing year (BEA).
Unemployment Growth	One year growth rate in the unemployment rate in the county of an IPO filing, from the pre-filing year to the filing year. Unemployment rate is computed as the number of unemployed over the sum of the number of employed and unemployed in a county month, produced by the Local Area Unemployment Statistics (LAUS) program managed by the Bureau of Labor Statistics (BLS) of the US Department of Labor beginning in the year 1990 (BLS). More information can be found at website of the BLS: https://www.bls.gov/lau/.
Income Growth	One year growth rate from the pre-filing year to the filing-year in a county's per capita personal income, defined as the personal income of the residents in the county of an IPO filing, divided by the resident population of that county (BEA). Personal income is defined as income received by, or on behalf of, all persons resident in a county from all sources, calculated as the sum of wages and salaries, supplements to wages and salaries, proprietors' income, rental income, personal dividend income, personal interest income, and personal current transfer receipts, less contributions for government social insurance plus an adjustment for place-of-residence.
Income	Per-capita personal income, defined as the personal income of the residents in the county of an IPO filing, divided by the resident population of that county (BEA). Personal income is defined as income received by, or on behalf of, all persons resident in a county from all

	sources, calculated as the sum of wages and salaries, supplements to wages and salaries, proprietors' income, rental income, personal dividend income, personal interest income, and personal current transfer receipts, less contributions for government social insurance plus an adjustment for place-of-residence.
Wages	Log one of plus the total annual wages and salary (per-worker) by place-of-work in the county and filing year of an IPO. Total wages and salary are calculated as the monetary remuneration of employees, including the compensation of corporate officers; commissions, tips, and bonuses; voluntary employee contributions to certain deferred compensation plans, such as 401(k) plans; and receipts in kind that represent income, adjusted to 2011 dollars (BEA).
Relative Wages	Log one of plus the total annual wages and salary (per-worker) by place-of-work in a county, scaled by the average total annual wages and salary for all counties during the same year, in the county and filing year of an IPO. Total wages and salary are calculated as the monetary remuneration of employees, including the compensation of corporate officers; commissions, tips, and bonuses; voluntary employee contributions to certain deferred compensation plans, such as 401(k) plans; and receipts in kind that represent income, adjusted to 2011 dollars (BEA).

Dependent Variables

County Characteristics

Annualized 5-year Employment Growth	Five year percent change (converted to an annual geometric average) in the number of total waged, salaried, and proprietorship employment in the county of an IPO filing over the five years following the IPO filing, beginning in the IPO filing year. Each annual reported employment count is an average of monthly estimates (BEA).
Annualized 5-year Establishment Growth	Five year percent change (converted to an annual geometric average) in the number of establishments in the county of an IPO over the five years following the IPO filing, beginning as of March 12 of the IPO filing year (CBP).
Annualized 5-year Population Growth	Five year percent change (converted to an annual geometric average) in annual population estimates for the county of an IPO filing over the five years following the IPO filing, beginning in the IPO filing year (BEA).
Annualized 5-year Unemployment Growth	Five year percent change (converted to an annual geometric average) in the unemployment rate for the county of an IPO filing over the five years following the IPO filing, beginning in the filing year of the IPO.
Annualized 5-year Income Growth	Five year percent change (converted to an annual geometric average) in per capita personal income in the county of an IPO over the five years following the IPO filing, where per capita income is defined as the personal income of the residents in the county of an IPO filing, divided by the resident population of that county (BEA).

Appendix B: Additional Results

In this appendix, we present additional results including a variety of tests examining the plausibility of our identifying assumptions (in addition to those presented in the paper).

In Figure B1, we show the geographical distribution of our sample of large IPO-filings. This map is very similar to the map in Figure 2, showing that there are no marked differences in the distribution of the location of large and small IPOs over the course of our sample period. Importantly, our sample includes IPOs located across the entire United States. However, there is a significant concentration of IPOs located in California.

The geographic concentration of IPOs is one of the primary reasons that we choose to use CRSP market returns, rather than NASDAQ returns as in Bernstein (2015), as an instrument for IPO completion. Silicon Valley not only has a large concentration of IPOs, but also a particularly high concentration of NASDAQ-listed public firms. As a result, NASDAQ returns might be correlated with the local economic conditions in Silicon Valley, which would violate our exclusion restriction and call into question our analysis.

To alleviate this concern, we use CRSP market returns. The broad set of public firms underlying the CRSP market returns is much less geographically concentrated; as a result, the aggregate CRSP returns are less likely to disproportionately reflect local economic conditions. For completeness, though, in Table B1 we confirm that our main results are robust to following Bernstein (2015) and using NASDAQ returns as our instrument. Table B1 shows that our results are robust to this alternative specification: whether we use NASDAQ or CRSP returns as the instrument, we find that large IPOs lead to a slowdown in local growth relative to staying private.

This result suggests that the concentration of IPOs in Silicon Valley is not a problem in our setting, but we investigate this further by repeating our main analyses on the subsample that excludes all California IPOs. In Panel B of Table B2 and Figure B2, we show that both our main results and placebo tests are quantitatively and qualitatively similar after excluding California IPOs. This evidence makes it clear that the Silicon Valley IPOs do not drive the effect that we document in this paper.

In the paper, we present a set of placebo analyses that supports our exclusion restriction. Here, we conduct several additional tests to further support the plausibility of our identifying assumption, which assumes that two-month post-filing market returns represent a shock to IPO completion but are otherwise unrelated to future local economic growth.

Interpretation of our 2SLS results assumes that the relation between market fluctuations and long-run county-level growth is due solely to the effect that post-filing market fluctuations have on IPO completion rates. Aiding the intuition behind this assumption is our inclusion of year fixed effects, which forces our models to identify only off deviations in market returns from broader swings in market conditions. One potential vulnerability of this assumption is that, despite the inclusion of year fixed effects, the economic growth of counties with IPO filings could be more sensitive to market returns for reasons unrelated to the completion of an IPO. To the extent that our identifying assumption is violated in this way, we would expect the significant relation between market returns and future economic growth to persist even if we measure market returns over alternative two-month windows, not just two-month windows immediately following IPO filings.

Panels A-D of Figure 3 in the paper cast doubt on the possibility that market returns are directly correlated with economic growth of counties with IPO filings, since we show that market returns only predict future economic growth when they are measured during the book building phase of the IPO (after accounting for the number of placebo coefficients that would load by chance). In Figure B2 we examine the robustness of these results using alternative samples. In particular, in Panel A of Figure B2, we confirm that when we include the IPO bubble period (i.e., years 1998-1999) in our sample, we continue to conclude that market returns immediately after IPO filings negatively affect future local employment growth, while market returns measured over other two-month windows on either side of IPO filings have no effect on future local employment growth. We reach similar conclusions in Panel B–which removes California-headquartered firms from our sample–and Panel C–which removes spinoffs, limited partnerships, and unit offerings from our sample.

Panels A-D of Figure B3 extend this analysis. These figures plot the relation between *Post-filing 2-month Market Returns* and county-level economic growth in the years surrounding the IPO filing year, in addition to plotting the relation between county-level economic growth and two placebo periods of 2-month market returns measured 12 months before and 12 months after IPO filings. The figures show two things. First, *Post-filing 2-month Market Returns* are negatively related to local employee, establishment, population, and income growth in the five years after an IPO filing, but are unrelated to these measures of economic growth in the years prior to an IPO filing in that county. Second, neither of the two placebo periods of market returns (i.e., beginning one year before or one year after an IPO filing) are significantly related to a county employment, establishment, population, or income growth, either in the three years before or five years after an IPO filing.

In unreported tests, we conduct a second type of placebo analysis in which we examine the relation between *Instrumented IPO Completion* and economic growth in counties that are observably similar to counties that experience an IPO filing, but that did not experience an IPO filing that year. Specifically, we match a single non-filing county-year to each IPO filing county-year using propensity scores based on one-year lags in employment, population, and per-capita income, in addition to one-year lagged growth rates in these measures.[30] Consistent with our identifying assumption, we find no relation between IPO completion and future economic growth in otherwise similar counties that did not experience an IPO filing in the matched year.

In Table B2, we confirm that the reduced form evidence presented in Figure B2 holds in the full 2SLS specification. In particular, we re-estimate our main 2SLS specification for the alternative subsamples described above and show that our results are robust to each of these alternative subsamples. In Panel A, we find that the negative effect of IPO completion on employment persists when we include the IPO bubble period. In Panels B and C, we show that the effects are robust to removing all IPO filings for firms headquartered in California and to removing spinoffs, limited partnerships, and unit offerings.

The framework presented in the paper suggests that one channel through which IPOs can negatively affect local economic growth is if IPOs lead firms to geographically diversity their operations. We expect that firms located in areas with fewer productive resources will be more likely to move operations to other states. Table B3 provides evidence that is consistent with this idea. The analysis in Table B3 is limited to firms that completed an IPO. For this sample of

[30] We require the matched county-years to be the same calendar year as the IPO filing county-years.

firms, we examine the first two 10-K reports that the firm files after going public. Using data from Garcia and Norli (2012), we calculate the fraction of state name mentions that are non-local (i.e., states that are not the firm's headquarter state). After going public, we find that firms located in poorer counties have a larger increase the fraction of non-local states mentioned in their 10-K reports.

An alternative channel through which IPOs might affect the local economy is through the housing market. The wealth shock created by an IPO could lead to an increase in local home prices; such an effect might result in a change in the employee base and income levels in the local region. In Figure B4, we directly test this hypothesis. In Panels A and B, we find no evidence that an index of local home prices increases after exogenously completed IPOs. And in Panel C, we find no evidence of positive relation between 2-month market returns and local building permits in the areas where IPO firms are located.

To summarize, in assessing the validity of the exclusion restriction we find that a) market returns in the two months after an IPO filing uniquely predict subsequent county-level growth relative to surrounding periods of market returns; b) this period of two-month post-filing market returns is unrelated to past county-level growth; c) this period of two-month market returns does not positively predict local home price growth; d) these market returns (in an IV analysis) do not predict subsequent growth in observably similar counties without an IPO filing; and e) none of these conclusions are affected by whether we include the bubble period; exclude spinoffs, LPs, or unit offerings; or exclude California-based firms from the analysis. For these reasons, we believe our results are not the result of a spurious relation between *Instrumented IPO Completion* and county-level economic growth.

Figure B1: Geographical dispersion of Large IPO sample

This figure plots the geographical distribution of the 3,016 large IPO filings in our main sample, across U.S. counties. The sample runs from 1986 through 2010, and is restricted to IPOs in the top half of IPO size (i.e., the real value of filing proceeds). The color shading corresponds to the total number of IPO filings within each county throughout the sample period, with unmapped counties corresponding to zero IPOs filed.

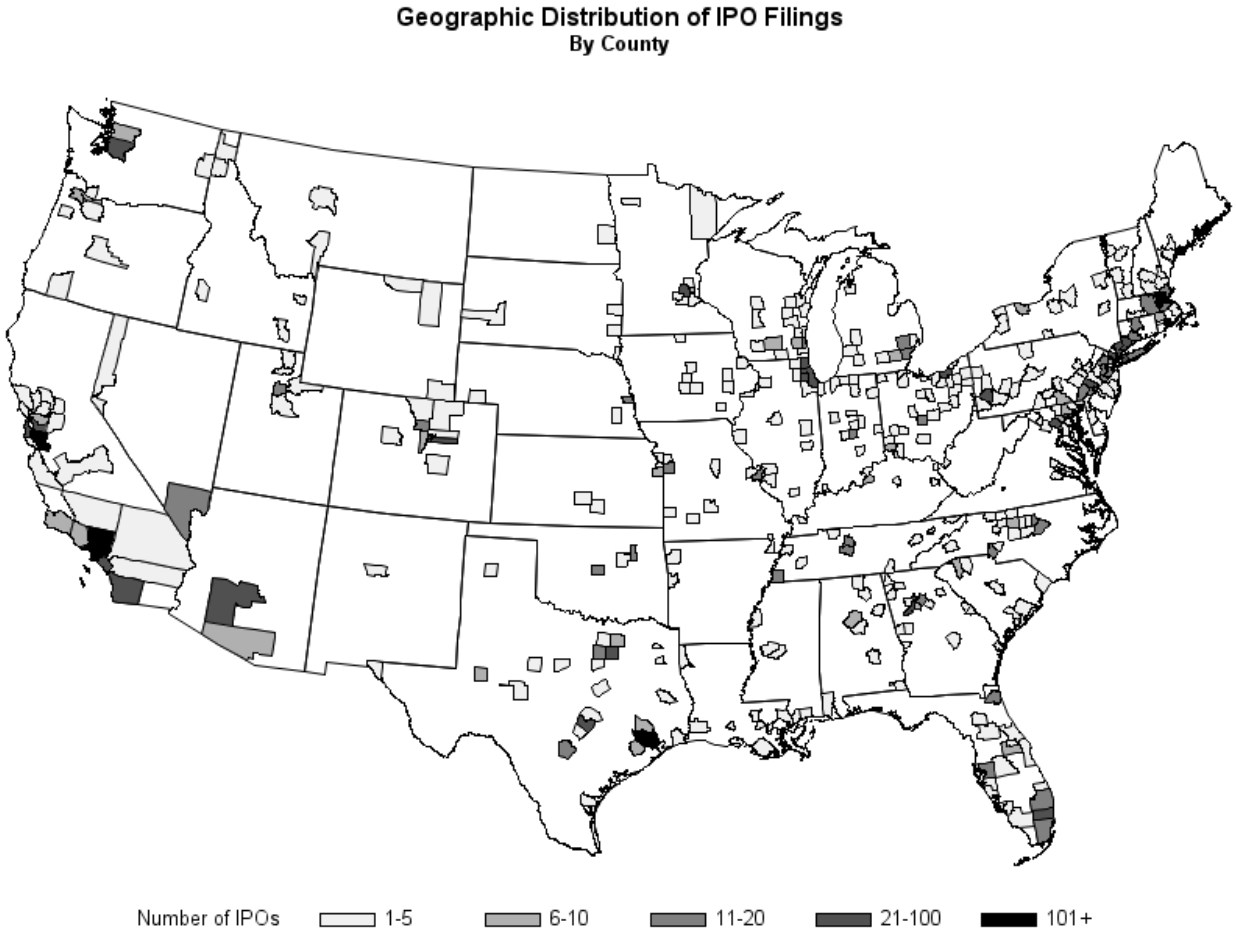
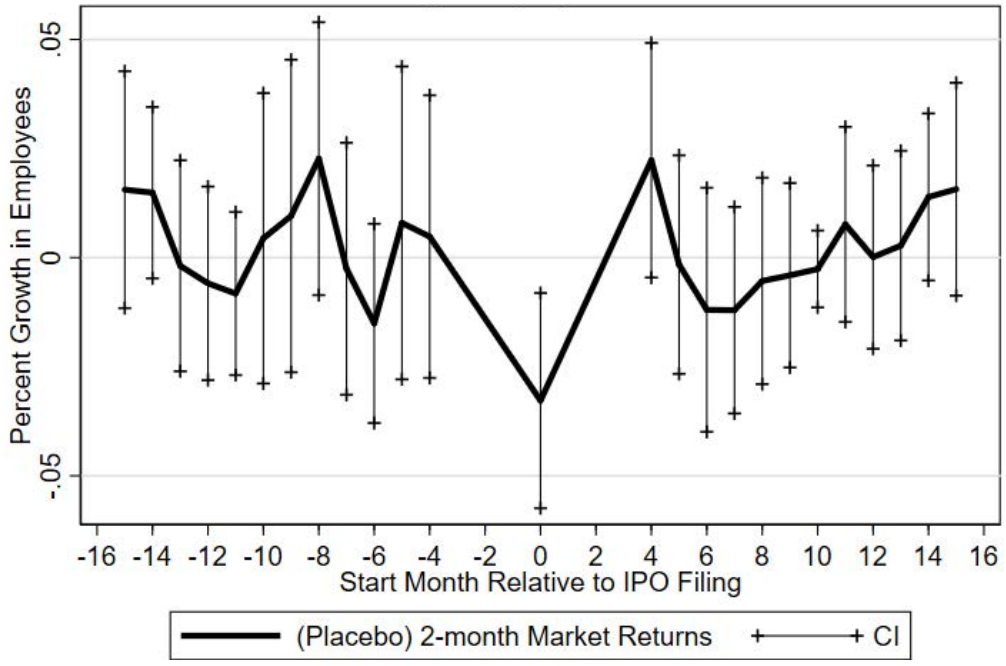


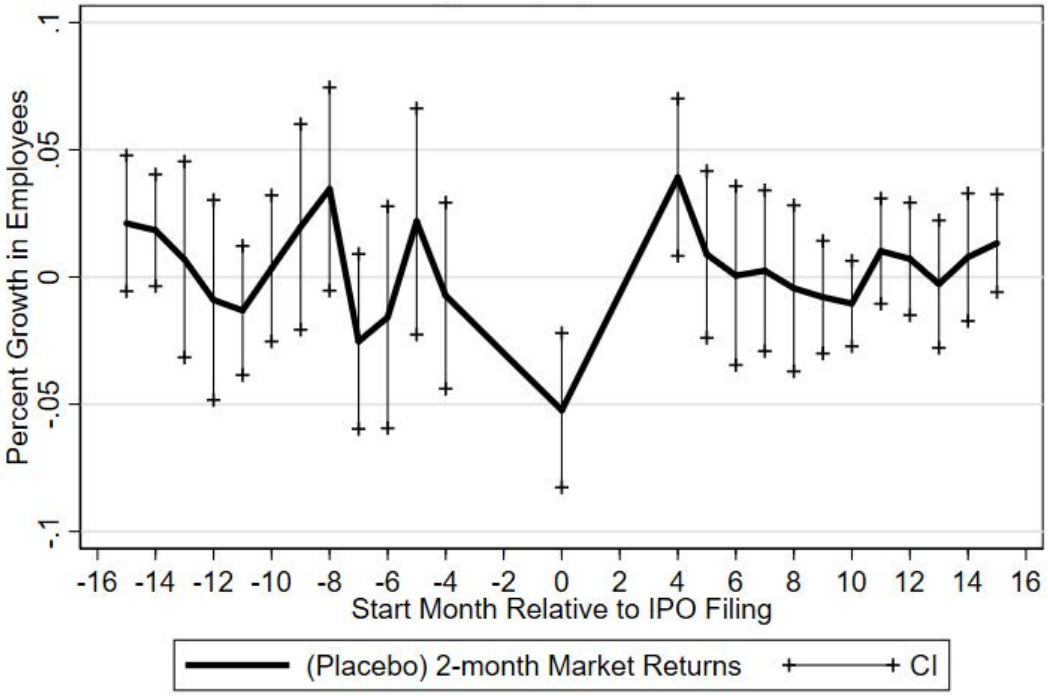
Figure B2: Placebo Market Returns – Plot of 25 different windows, Robust Samples

Panels A-C each plot coefficients from twenty-five different reduced form IV regressions, with five-year cumulative growth in county-level employees as the dependent variable in each regression. In each regression, employee growth is estimated as a function of two-month (CRSP value-weighted) market returns, in addition to the same county and IPO control variables used in Column 3 of Table 4. Each regression uses a different window of two-month market returns, varying the number of months before or after the filing date of each IPO that the market return window begins. The start date of the market return window is marked on the x-axis. For instance, the point on the figure corresponding to the zero tick on the x-axis represents a regression of five-year county employee growth as function of two-month market returns beginning the date of each IPO filing (along with controls and fixed effects), while the point at the +4 tick represents the same regression, but swapping market returns beginning four months *after* each IPO filing for market returns beginning at the filing date. We omit returns in the 6-month window surrounding the filing date because these returns possibly impact the book building phase of the IPO, which limits their usefulness as placebo tests. Vertical lines at each point represent 95% confidence intervals for the coefficient on the variable representing two-month market returns. The sample for each regression is restricted to large IPOs between 1986 and 2010, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars). In Panel A, we augment our full sample (used in Figure 3) by including IPOs filed during the IPO bubble period (1998-1999); in Panel B we exclude firms headquartered in California; and in Panel C we exclude spinoffs, limited partnerships, and unit offerings.

Panel A: Including years 1998-1999



Panel B: Excluding California Firms



Panel C: Excluding Spinoffs, LPs, and Unit Offerings

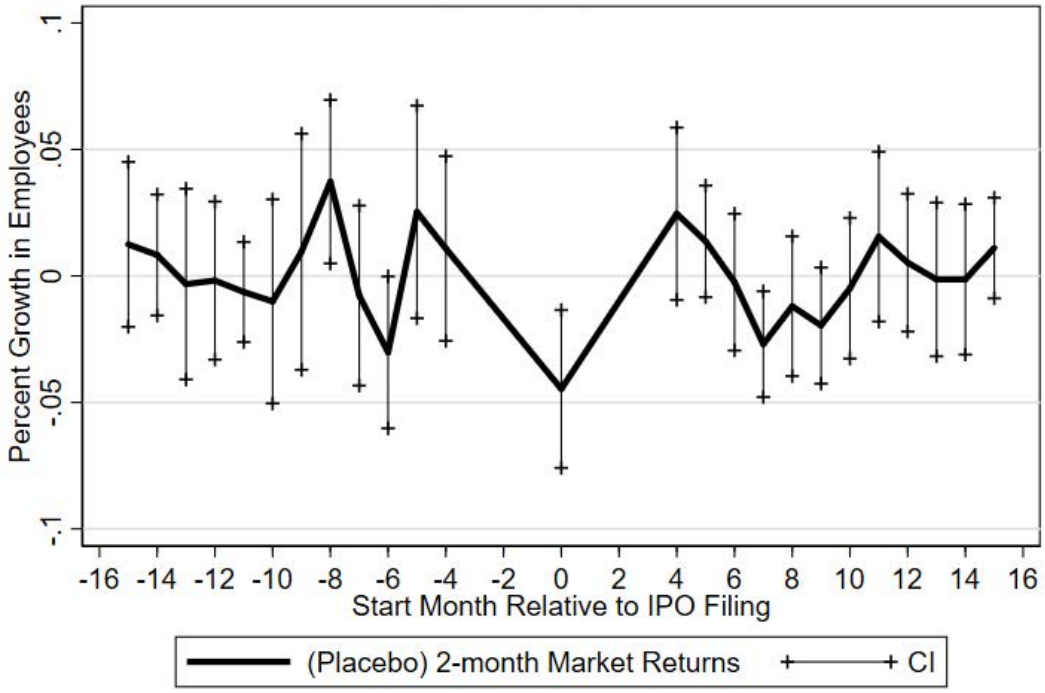
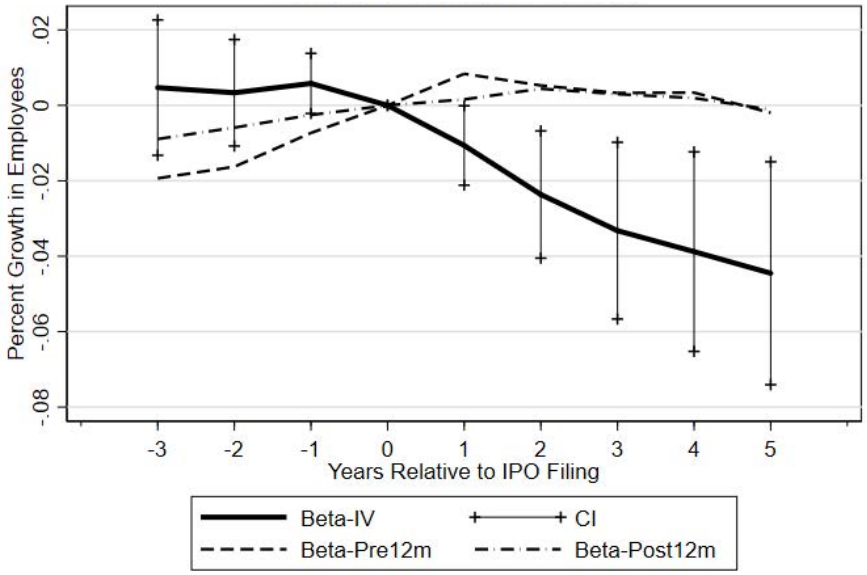


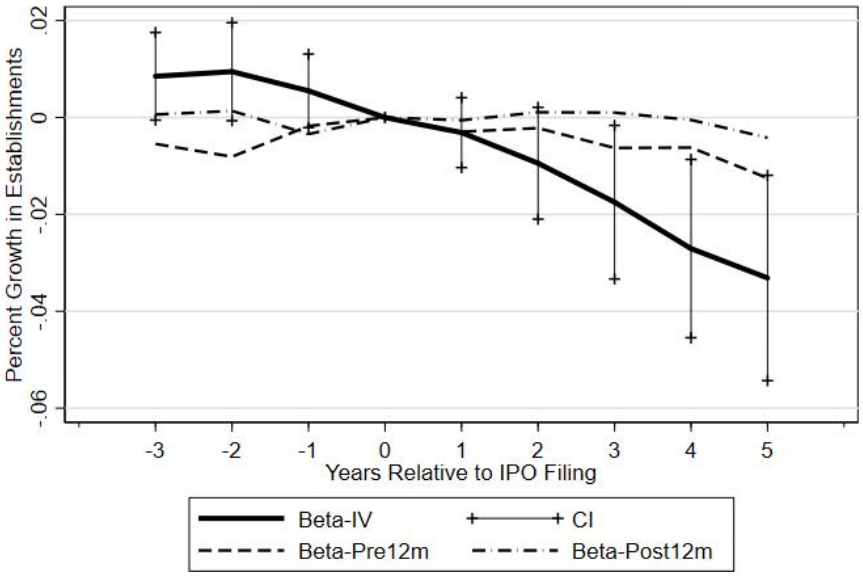
Figure B3: County Economic Growth Surrounding IPO Filings

These figures plot the evolution of the number of employees, establishments, population, and per-capita personal income in counties with IPO filings–beginning three years prior to the IPO filing and ending five years following the filing–as a function of (CRSP value-weighted) market returns surrounding the IPO filing date. The solid line plots coefficients from the reduced form 2SLS regression that corresponds to Eq. 2 (i.e., post-filing 2-month market returns replacing the instrumented IPO completion variable). The county-level control variables in each regression are the same as in Column 3 of Table 4, except regressions with dependent variables measuring growth prior to the IPO filing year include lagged growth rates as of year -3. The dashed line plots the same regressions, but with the main explanatory variable being a placebo two-month market return beginning 12 months prior to the IPO filing. The dash-dotted line plots regression coefficients for a similar set of regressions, where the main explanatory variable is a placebo two-month market return beginning 12 months after the IPO filing. Dependent variables in these regressions measure cumulative growth from the IPO filing year to the year marked on the x-axis, for each respective economic measure in each panel (e.g., number of employees in Panel A). Vertical lines at each point represent 95% confidence intervals for the coefficient on the reduced form IV, two-month market returns beginning at the IPO filing.

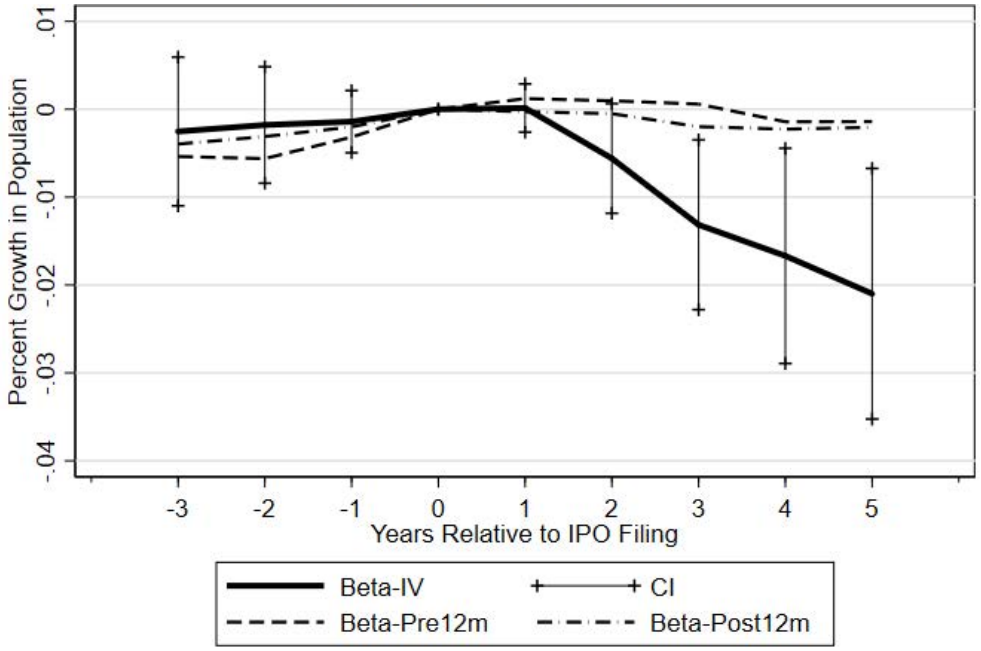
Panel A: Employees



Panel B: Establishments



Panel C: Population



Panel D: Personal Income

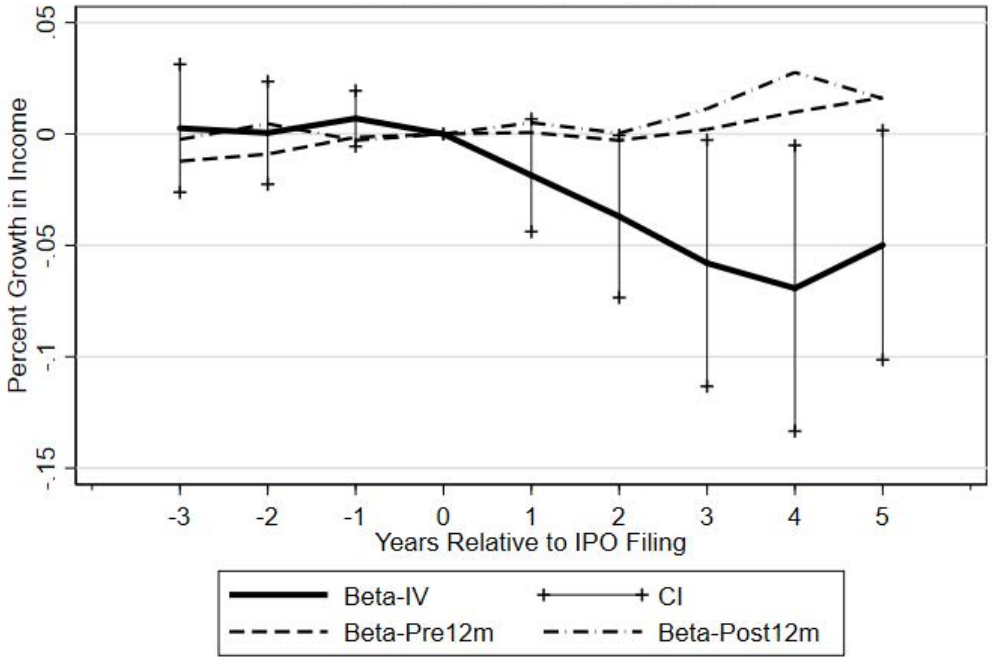
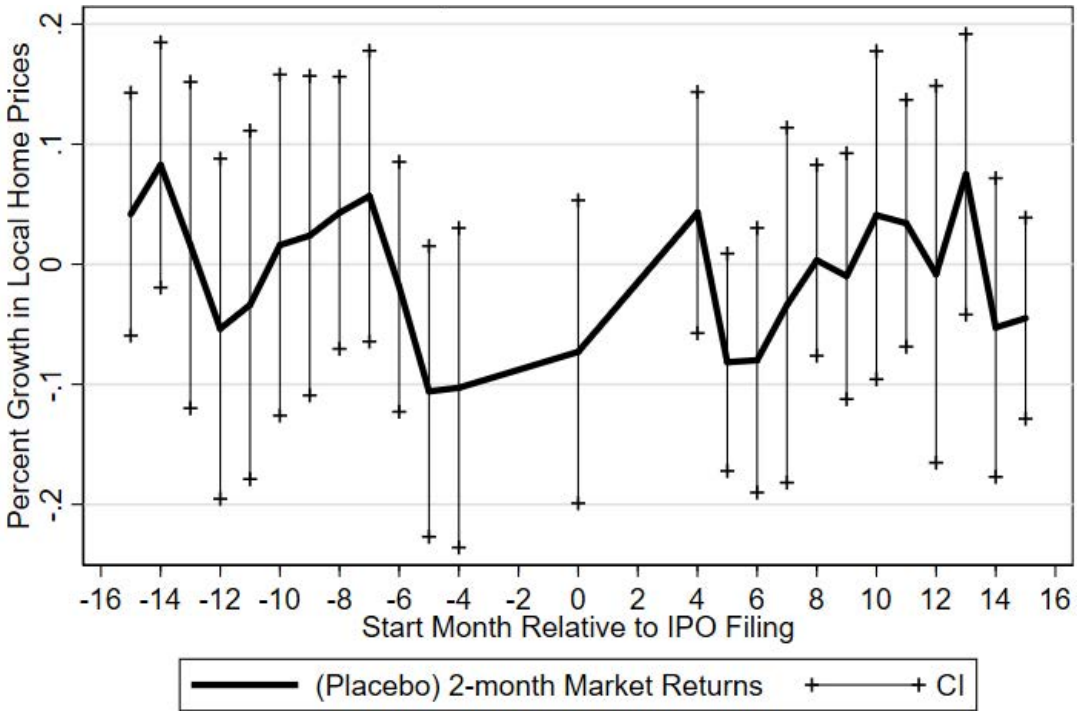


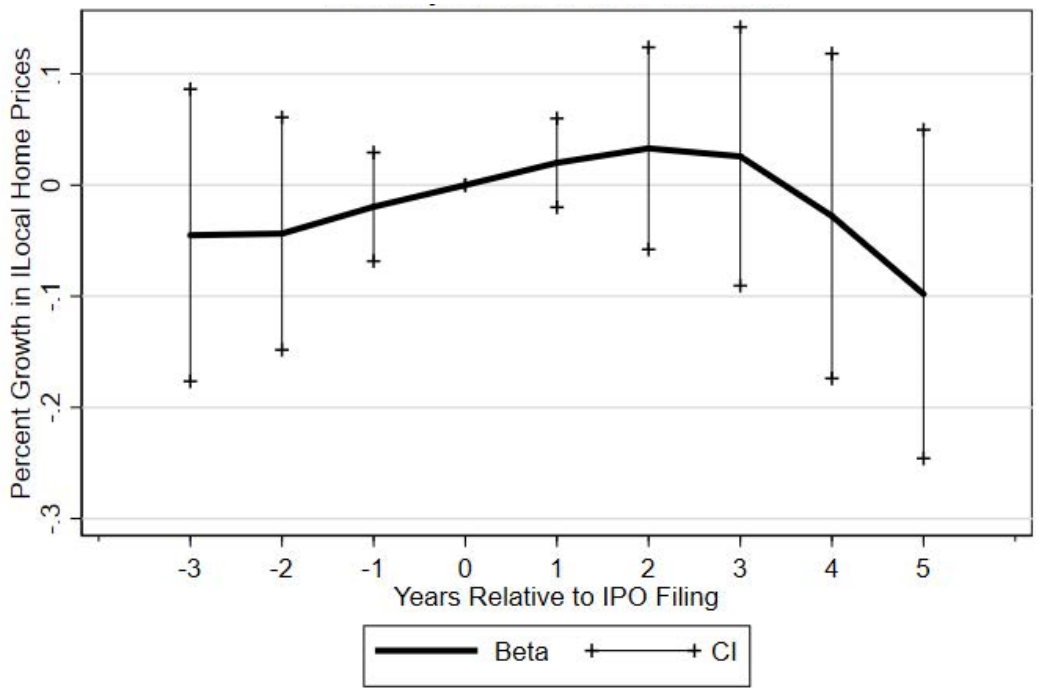
Figure B4: Growth in Local Home Prices and Building Permits

The figure examines the relation between 2-month (CRSP value-weighted) market returns and home price/building activity. Panels A and C plot coefficients from thirteen different reduced form IV regressions, with five-year cumulative growth in county-level home prices as the dependent variable in Panel A, and five-year cumulative growth in county-level building permits as the dependent variable in Panel C. In each regression, growth in home prices (or building permits) is estimated as a function of two-month market returns, in addition to the same county and IPO control variables used in Column 3 of Table 4. Each regression uses a different window of two month market returns, varying the number of months before or after the filing date of each IPO in the sample that the return window begins. The start date of the market return window is marked on the x-axis. For instance, the point on the figure corresponding to the zero tick on the x-axis represents a regression of five-year county home price growth as function of two month market returns beginning the date of each IPO filing (along with controls and fixed effects), while the point at the +4 tick represents the same regression, but swapping market returns beginning four months *after* each IPO filing for market returns beginning at the filing date. Vertical lines at each point represent 95% confidence intervals for the coefficient on the variable representing two-month market returns. Panel B plots the coefficients from reduced form 2SLS regressions with growth in local home prices (over varying periods) as the dependent variable, and post-filing 2-month market returns as the main independent variable (controls identical those in Column 3 of Table 4). The dependent variables measure cumulative growth from the IPO filing year to the year marked on the x-axis. Vertical lines at each point represent 95% confidence intervals for the coefficient on two-month market returns beginning at the IPO filing.

Panel A: Placebo Market Returns predicting Growth in County Home Prices



Panel B: County Home Price Growth surrounding IPO Filings



Panel C: Placebo Market Returns predicting Growth in County Building Permits

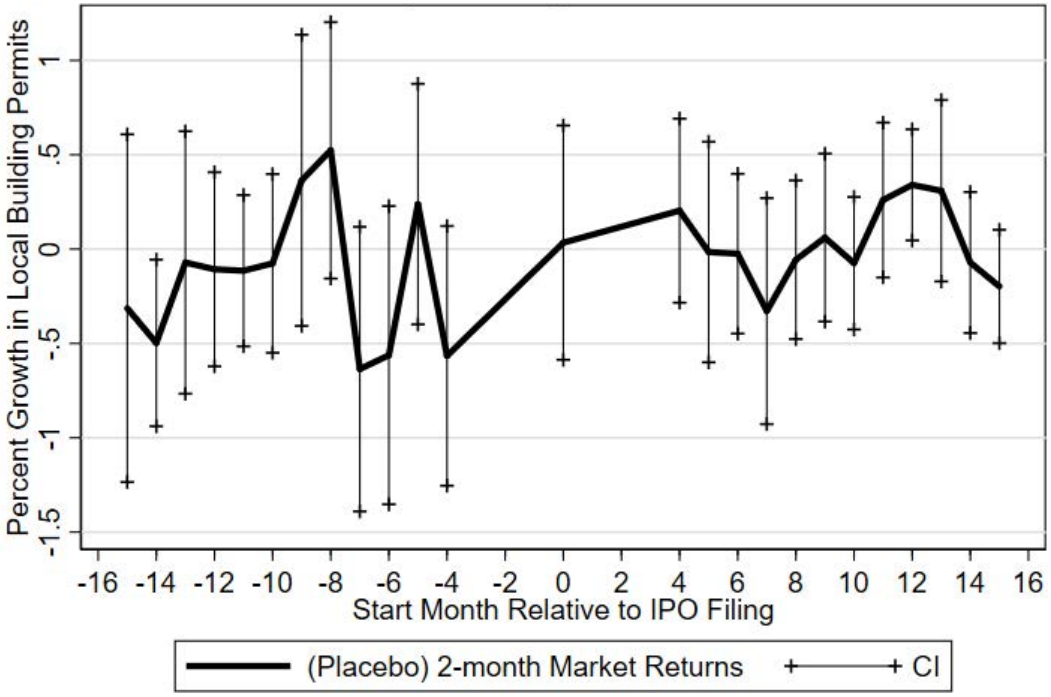


Table B1: IPOs and Local Economic Growth using NASDAQ as Instrument

This table repeats the main analysis from Table 4 in the paper using NASDAQ returns (rather than CRSP market returns) during the book-building phase as an instrument for the decision to complete vs withdraw the IPO. The dependent variable is the annual geometric average growth rate in a county's total number of employees over the five years after an IPO filing. We estimate the effect for the full sample of IPOs (column 1), the subsample of small IPOs, defined as those with below-median real filing proceeds (column 2), and the subsample of large IPOs, defined as those with above-median real filing proceeds (column 3). The sample period is between 1986 and 2010. We winsorize all dependent variables at the extreme 1%. All variables are defined in Appendix A. Standard errors are clustered at the county and year levels (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Employees	(2) Employees (Small IPOs)	(3) Employees (Large IPOs)
Instrumented IPO Completion	-0.0018	0.0037	-0.0060**
	(-0.90)	(0.89)	(-2.28)
Population Growth	0.2228***	0.1913*	0.2700**
	(2.87)	(1.88)	(2.73)
Employee Growth	0.0377	-0.0040	0.0356
	(1.23)	(-0.13)	(0.94)
Income Growth	-0.0603	-0.0489	-0.0561
	(-1.55)	(-1.71)	(-1.67)
IPO Size	0.0008	-0.0044	0.0012
	(0.56)	(-0.19)	(0.87)
Ln(Number of IPOs)	-0.0005	0.0021	-0.0023
	(-0.39)	(1.38)	(-1.49)
Number Lead Managers	0.0002	0.0008	0.0001
	(0.59)	(0.86)	(0.47)
PE/VC Funding	-0.0001	-0.0003	0.0004
	(-0.30)	(-0.54)	(0.79)
Underwriter Reputation	0.0000	-0.0000	0.0001
	(0.57)	(-0.46)	(0.98)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Adj. R-squared	0.037	0.013	–
Observations	6,205	3,029	3,016
First Stage F-statistic	80.4	14.0	72.7

Table B2: IPOs and Local Employee Growth, Robust Samples

This table presents second-stage 2SLS estimates–identical to those in Table 4–where the explanatory variable of interest is IPO completion instrumented with the return of the CRSP value-weighted market index in the two-months following an IPO filing, however, in each panel, we augment our main sample in a distinct way. In Panel A, we include IPOs filed during the IPO bubble period (1998-1999); in Panel B, we exclude IPO firms headquartered in California; and in Panel C, we exclude spinoffs, limited partnerships, and unit offerings. The dependent variable is the annual geometric average growth rate in a county's total number of employees over the five years after an IPO filing. We estimate the effect for the full sample of IPOs (column 1), the subsample of small IPOs, defined as those with below-median real filing proceeds (column 2), and the subsample of large IPOs, defined as those with above-median real filing proceeds (column 3). The sample period is between 1986 and 2010. We winsorize all dependent variables at the extreme 1%. All variables are defined in Appendix A. Standard errors are clustered at the county and year levels (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Employment – Including years 1998-1999

	(1) Employees	(2) Employees (Small IPOs)	(3) Employees (Large IPOs)
Instrumented IPO Completion	-0.0026	0.0057	-0.0071**
	(-1.43)	(1.60)	(-2.55)
Population Growth	0.2231***	0.1830*	0.2933***
	(2.90)	(1.88)	(3.08)
Employee Growth	0.0625*	0.0115	0.0595
	(1.73)	(0.36)	(1.30)
Income Growth	-0.0744**	-0.0714**	-0.0544**
	(-2.45)	(-2.50)	(-2.59)
IPO Size	0.0005	-0.0178	0.0014
	(0.45)	(-0.85)	(0.93)
Ln(Number of IPOs)	-0.0022	0.0012	-0.0042**
	(-1.41)	(0.84)	(-2.24)
Number Lead Managers	0.0003	0.0018	0.0001
	(0.99)	(1.43)	(0.40)
PE/VC Funding	-0.0000	-0.0004	0.0002
	(-0.08)	(-0.78)	(0.57)
Underwriter Reputation	0.0000	-0.0001	0.0001
	(0.68)	(-0.56)	(0.41)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Adj. R-squared	0.056	–	–
Observations	7,110	3,467	3,474
First Stage F-statistic	43.1	24.9	29.1

Panel B: Excluding California IPOs

	(1) Employees	(2) Employees (Small IPOs)	(3) Employees (Large IPOs)
Instrumented IPO Completion	-0.0060	0.0048	-0.0161**
	(-1.12)	(0.51)	(-2.56)
Population Growth	0.1500*	0.1641	0.1412*
	(1.90)	(1.41)	(1.98)
Employee Growth	0.0724*	0.0274	0.0734*
	(1.90)	(0.76)	(1.75)
Income Growth	-0.0236	-0.0402	-0.0192
	(-0.91)	(-1.26)	(-1.01)
IPO Size	0.0004	0.0245	0.0008
	(0.20)	(0.94)	(0.34)
Ln(Number of IPOs)	0.0003	0.0022	-0.0009
	(0.27)	(1.64)	(-0.70)
Number Lead Managers	0.0004	0.0010	0.0009
	(0.73)	(0.72)	(1.31)
PE/VC Funding	0.0003	-0.0001	0.0015*
	(0.43)	(-0.20)	(1.76)
Underwriter Reputation	-0.0000	-0.0002	0.0001
	(-0.26)	(-1.44)	(0.60)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Adj. R-squared	–	–	–
Observations	4,692	2,271	2,266
First Stage F-statistic	26.6	4.8	19.8

Panel C: Excluding Spin-Offs, LPs, and Unit Offerings

	(1) Employees	(2) Employees (Small IPOs)	(3) Employees (Large IPOs)
Instrumented IPO Completion	-0.0024	0.0070	-0.0097**
	(-0.82)	(1.51)	(-2.34)
Population Growth	0.2208**	0.1848*	0.2738**
	(2.81)	(1.84)	(2.37)
Employment Growth	0.0436	0.0037	0.0372
	(1.45)	(0.12)	(0.88)
Income Growth	-0.0651	-0.0455	-0.0630*
	(-1.62)	(-1.67)	(-1.89)
IPO Size	0.0005	0.0045	0.0007
	(0.31)	(0.16)	(0.36)
Ln(Number of IPOs)	-0.0006	0.0020	-0.0022
	(-0.40)	(1.29)	(-1.32)
Number Lead Managers	0.0005	0.0006	0.0004
	(1.16)	(0.51)	(1.22)
PE/VC Funding	0.0001	-0.0004	0.0010
	(0.23)	(-0.74)	(1.64)
Underwriter Reputation	0.0000	-0.0001	0.0003*
	(0.64)	(-0.70)	(2.00)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Adj. R-squared	0.033	–	–
Observations	5,361	2,604	2,629

Table B3: 10-K Geographic Dispersion for IPO firms

This table reports estimates from OLS regressions exploring determinants of geographic dispersion of state operations at the firm level. The dependent variable is Δ *Non-Local Operations,* defined as the one-year percentage point change in the ratio of number of state mentions in firms' 10K reports that are not the firms' headquarter state to the total number of states mentions (calculated using the data collected annually by Garcia and Norli (2012) between the years 1995 and 2008). An increase in this measure indicates that a firm is talking more about non-local states. Using our set of 6,205 IPOs, we match 1,390 IPOs that have two consecutive data points of 10K state name counts in the first two years following the IPO. The main explanatory variables are measures of county-level income in the year prior to the IPO filing: per-capita income (column 1), average wages (column 2), the log of average wages in the county benchmarked to average wages across all counties in that year (column 3), and job earnings (column 4). All variables are defined in Appendix A. Standard errors are clustered at the county and two-digit SIC levels (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Δ Non-Local Operations	(2) Δ Non-Local Operations	(3) Δ Non-Local Operations	(4) Δ Non-Local Operations
Income	-0.057*			
	(-1.90)			
Wages		-0.101***		
		(-2.84)		
Relative Wages			-0.031***	
			(-2.89)	
Job Earnings				-0.075***
				(-3.64)
Employee Growth	0.502	0.460	0.464	0.453
	(0.91)	(0.84)	(0.84)	(0.81)
Population Growth	-0.401	-0.454	-0.453	-0.453
	(-0.53)	(-0.58)	(-0.58)	(-0.58)
Income Growth	0.368***	0.387***	0.387***	0.370**
	(2.94)	(3.11)	(3.03)	(2.81)
IPO Size	-0.099*	-0.099*	-0.099*	-0.098*
	(-2.01)	(-2.05)	(-2.04)	(-2.01)
Ln(Number of	-0.007	-0.003	-0.003	-0.004
IPOs)	(-1.53)	(-0.65)	(-0.64)	(-0.83)
Number Lead	0.012	0.012	0.012	0.012
Managers	(1.30)	(1.25)	(1.26)	(1.25)
PE/VC Funding	-0.003	-0.003	-0.003	-0.004
	(-0.35)	(-0.33)	(-0.32)	(-0.38)
Underwriter	0.003	0.003	0.003	0.003
Reputation	(1.22)	(1.34)	(1.33)	(1.31)
County FE	No	No	No	No
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.004	0.005	0.005	0.004
Observations	1,390	1,390	1,390	1,390

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Figure 2: Geographical dispersion of IPO sample

This figure plots the geographical distribution of the 6,205 IPO filings in our main sample across U.S. counties. The sample runs from 1986 through 2010. The color shading corresponds to the total number of IPO filings within each county through the sample period, with unmapped counties corresponding to zero IPOs filed.

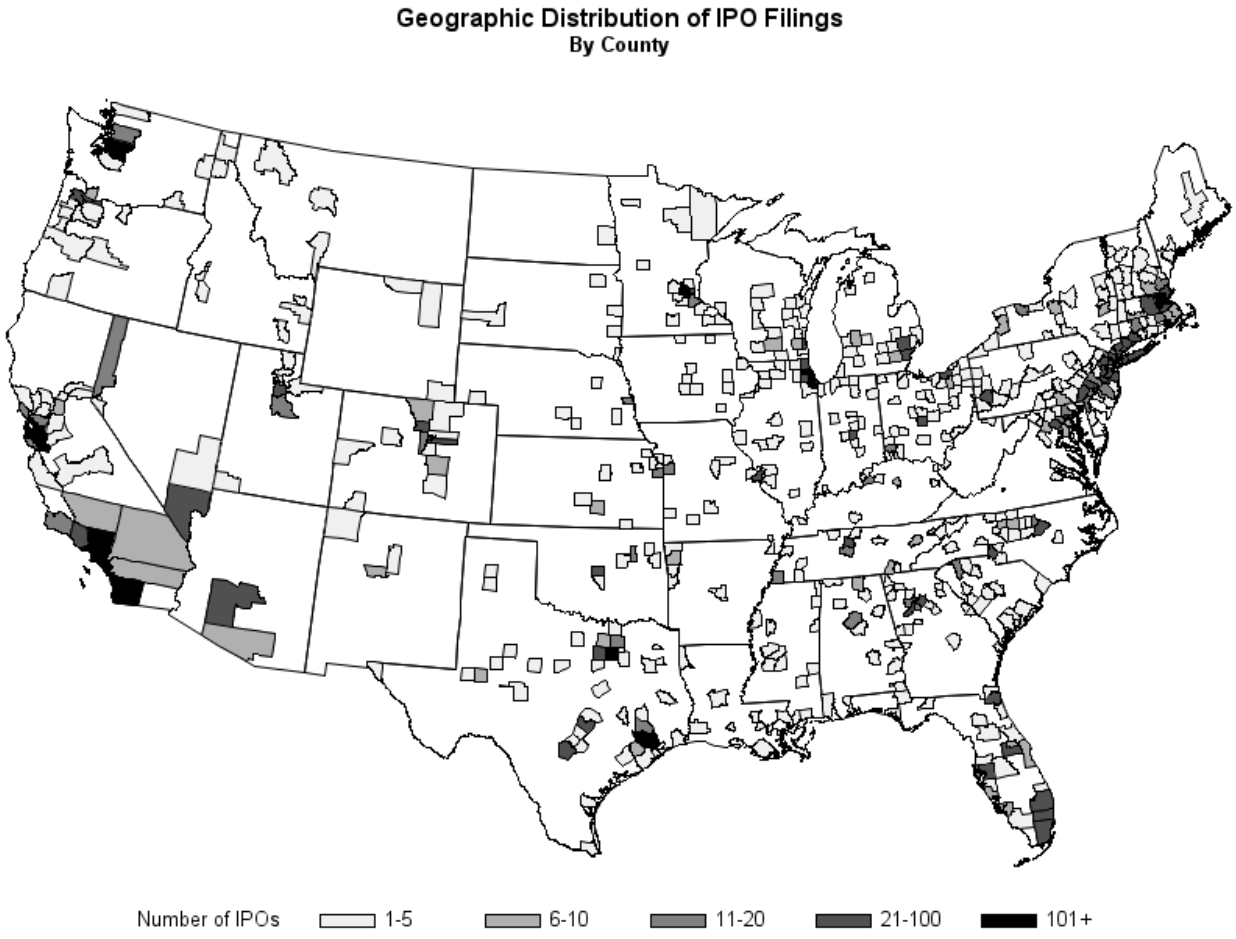
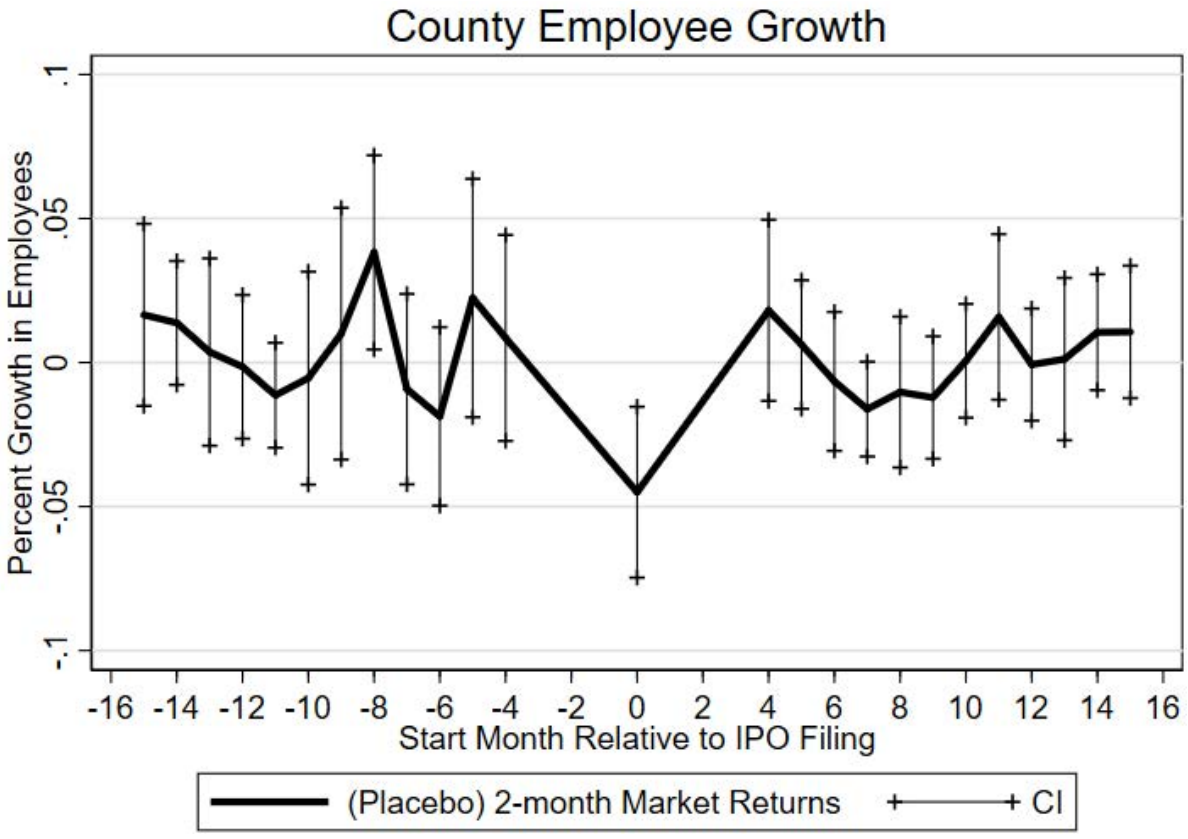


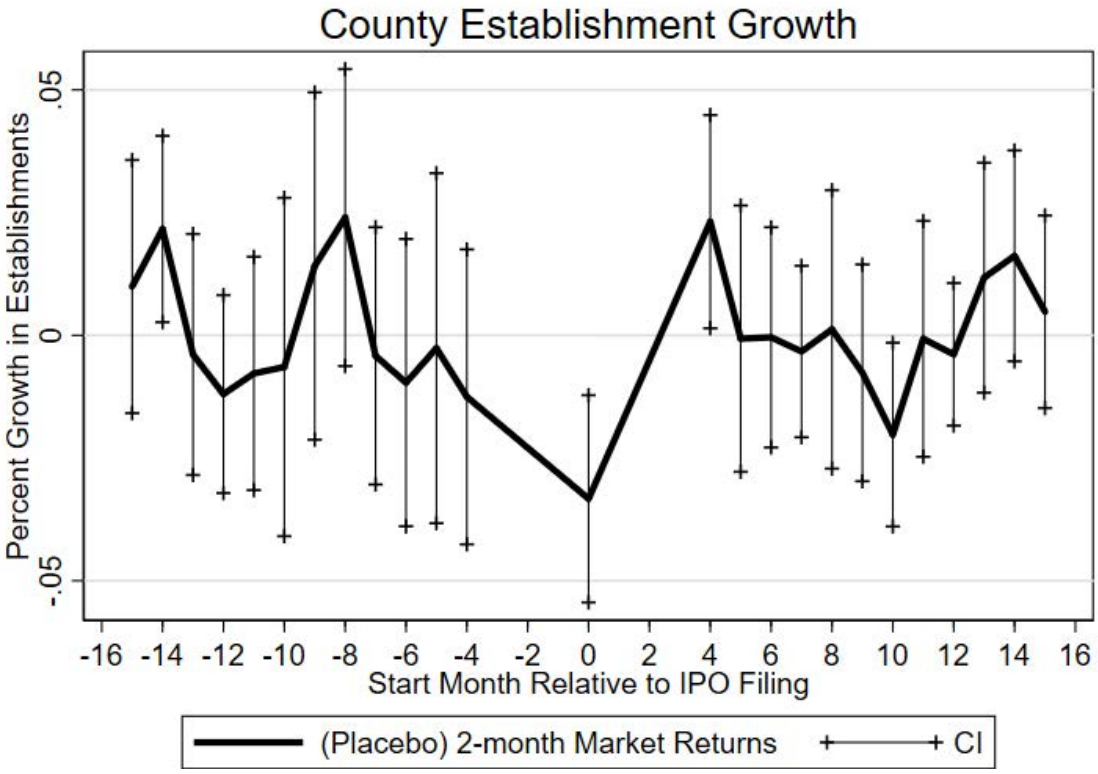
Figure 3: Placebo Robustness Test of Market Returns – Plot of 25 different windows

Panels A-D each plot beta coefficients from twenty-five different regressions, with a different measure of five-year county-level economic growth serving as the dependent variable in each. The dependent variable in Panel A is the cumulative five-year growth in county-level employees; in Panel B, cumulative five-year growth in county-level establishments; in Panel C, cumulative five-year growth in county-level population; and in Panel D, cumulative five-year growth in county-level per-capita personal income. In each regression, the respective measure of five-year economic growth is estimated as a function of two-month (CRSP value-weighted) market returns, in addition to the same county and IPO control variables used in Column 3 of Table 4. Each of the twenty-five regressions (for each economic measure) uses a different window of two-month market returns, varying the number of months before or after the filing date of each IPO that the market return window begins. The start date of the market return window is marked on the x-axis. For instance, the point on the figure corresponding to the zero tick on the x-axis represents a regression of five-year county employee growth as function of two month market returns beginning the date of each IPO filing (along with controls and fixed effects), while the point at the +4 tick represents the same regression, but swapping market returns beginning four months *after* each IPO filing for market returns beginning at the filing date. We omit returns in the 6-month window surrounding the filing date because these returns possibly impact the book building phase of the IPO, which limits their usefulness as placebo tests. Vertical lines at each point represent 95% confidence intervals for the coefficient on the variable representing two-month market returns. The sample for each regression is restricted to large IPOs between 1986 and 2010, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars).

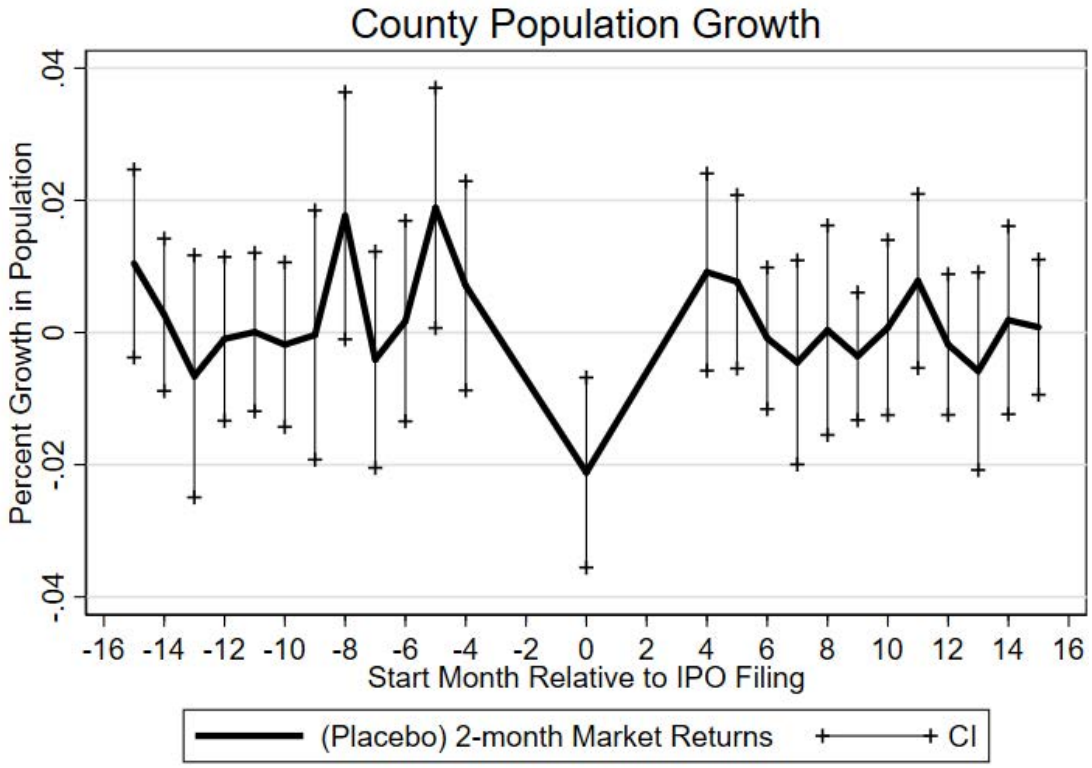
Panel A: Two-month Market Returns and Future Employment Growth



Panel B: Two-month Market Returns and Future Establishment Growth



Panel C: Two-month Market Returns and Future Population Growth



Panel D: Two-month Market Returns and Future Per Capita Income Growth

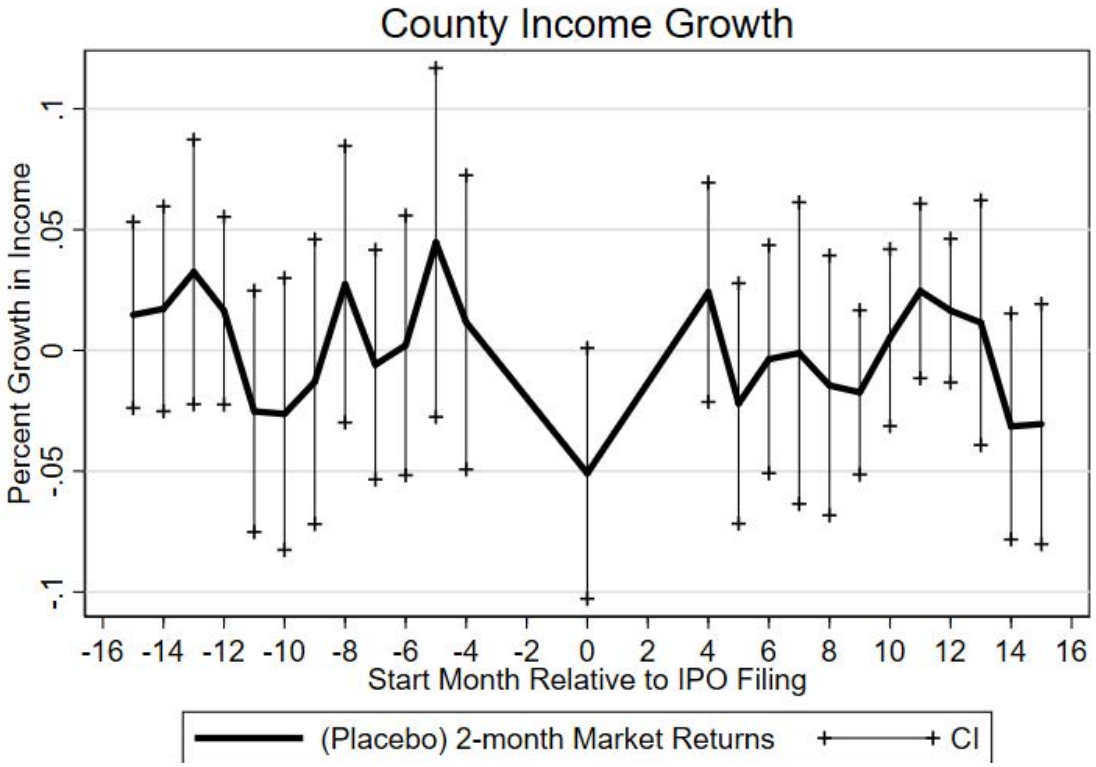


Figure 4: Evolution of County Employment Growth surrounding IPO Filings

This figure plots the evolution of the employment growth in counties following an exogenously completed IPO compared to counties in which an IPO was exogenously withdrawn. Each point on the line represents a coefficient from our second-stage 2SLS regression on the instrumented IPO completion variable (e.g., Column 3 of Table 4), where the dependent variable measures cumulative employment growth from the IPO filing year to the year marked on the x-axis. The county-level control variables in each regression are the same as in Column 3 of Table 4, except regressions with dependent variables measuring growth prior to the IPO filing year include lagged growth rates as of year -3. Vertical lines at each point represent 95% confidence intervals for the instrumented IPO completion coefficient. The sample for each regression is restricted to large IPOs between 1986 and 2010, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars).

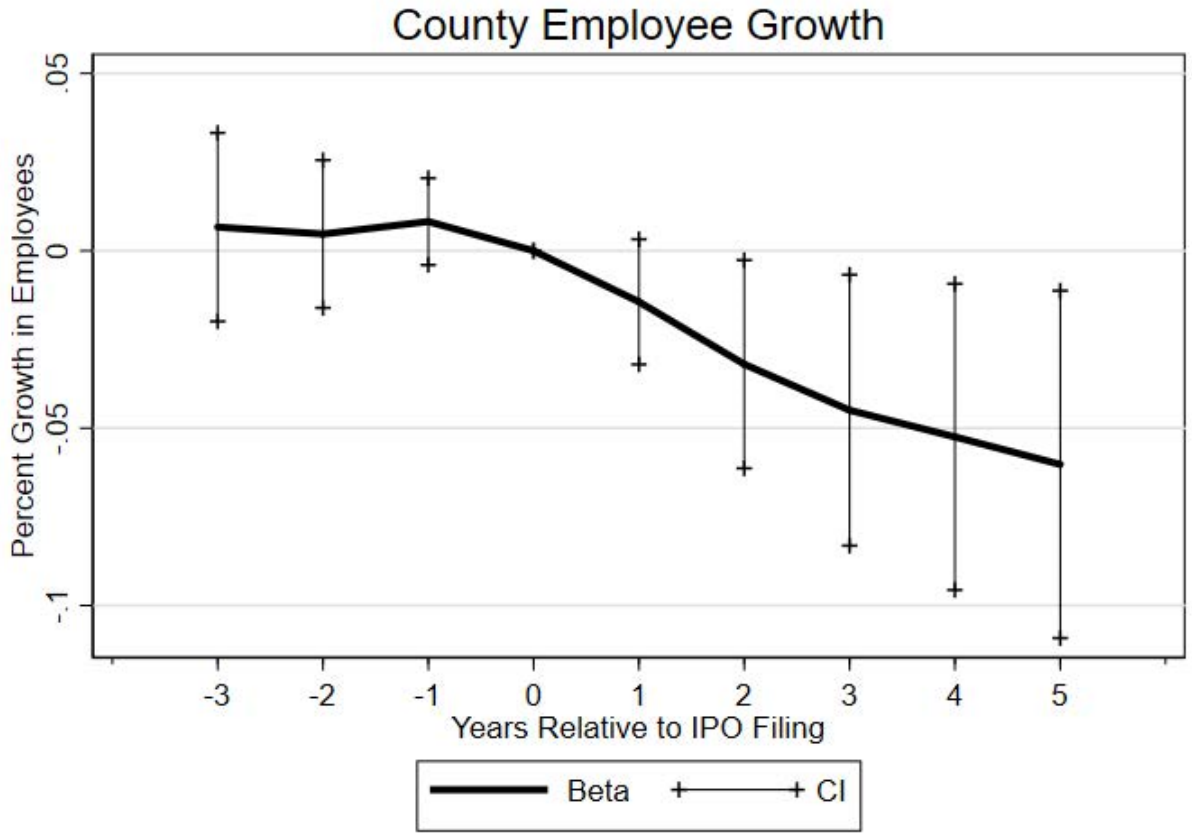


Figure 5: Industry Decomposition of County Employment Growth surrounding IPO Filings

This figure plots the industry decomposition of the evolution of employment in counties with completed IPOs, compared to counties with withdrawn IPOs. The figure covers the eight years after an IPO filing. Each point on each of the two lines represents a coefficient on the instrumented IPO completion variable from our second-stage 2SLS regression (e.g., Column 3 of Table 4), where the dependent variable measures cumulative employment growth from the IPO filing year to the year marked on the x-axis for the respective industry group. The tradable sector represents businesses in the construction and manufacturing sub-sectors (NAICS 23 & 31-33). The non-tradable sector represents businesses in all remaining sub-sectors (minus agriculture, mining, and public administration). Control variables in each regression are the same as in Column 3 of Table 4. The sample for each regression is restricted to large IPOs between 1986 and 2010, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars).

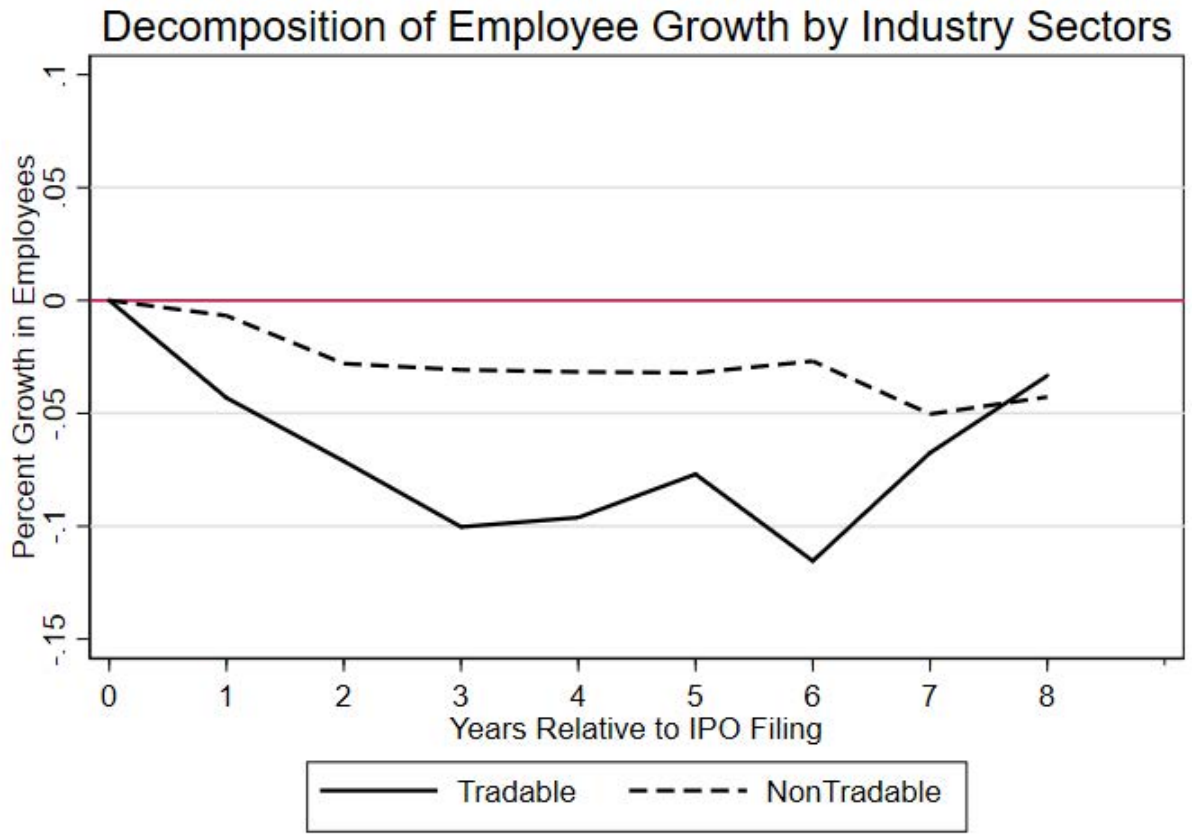
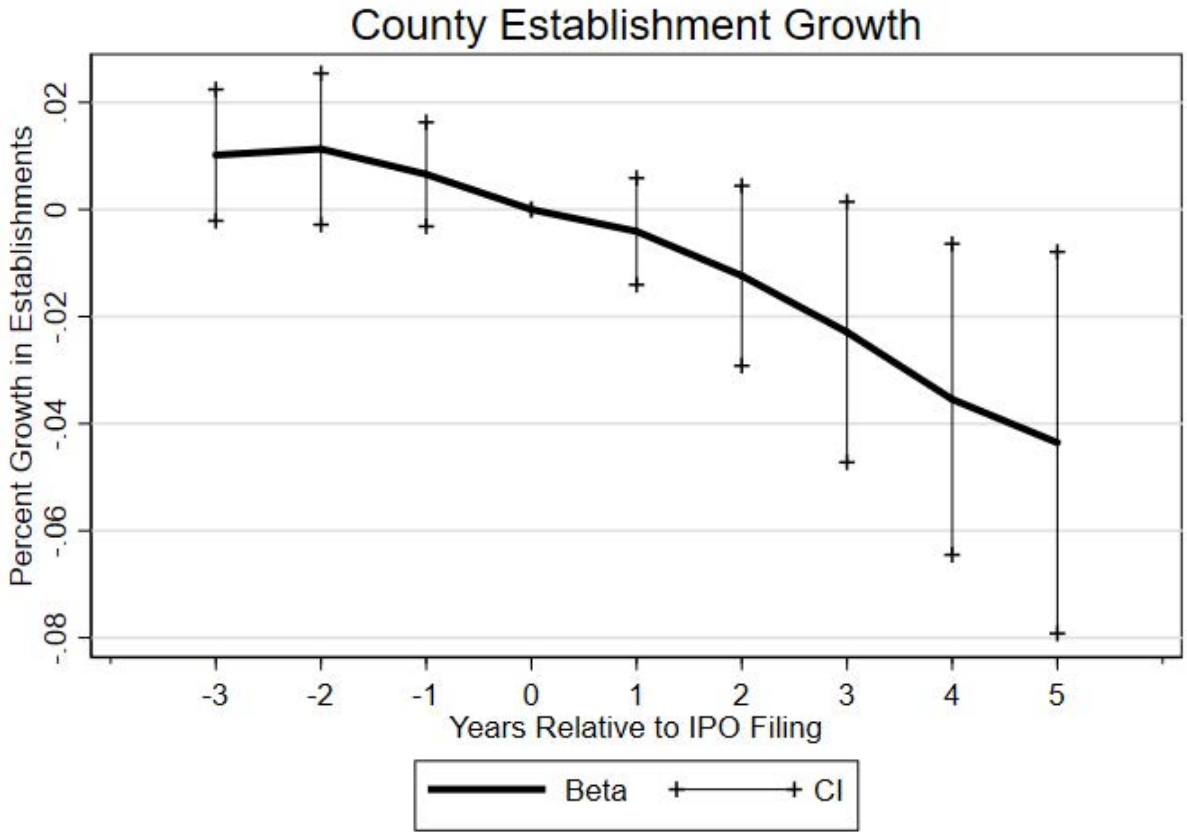


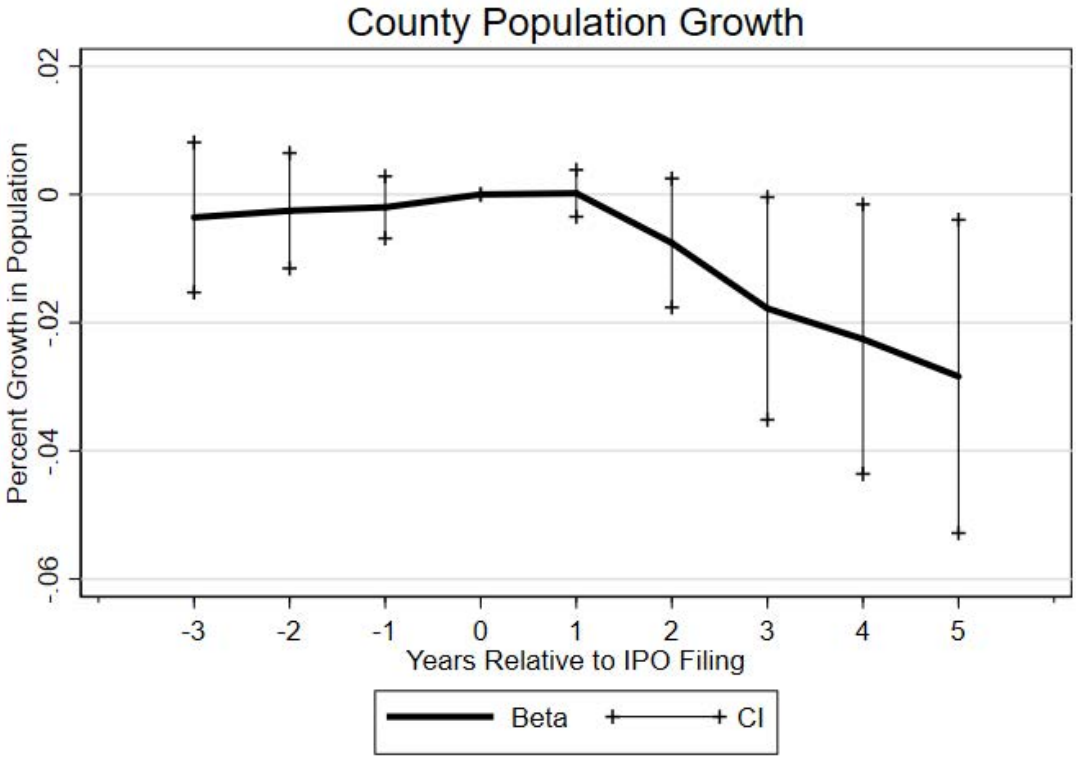
Figure 6: County Economic Growth surrounding IPO Filings

This figure plots the evolution of the establishment (Panel A), population (Panel B) and per capita income (Panel C) in counties following an exogenously completed IPO compared to counties in which an IPO was exogenously withdrawn. Each point on the line represents a coefficient from our second-stage 2SLS regression on the instrumented IPO completion variable (e.g., Column 3 of Table 4), where the dependent variable measures cumulative establishment (Panel A), population (Panel B) or per capita income (Panel C) growth from the IPO filing year to the year marked on the x-axis. The county-level control variables in each regression are the same as in Column 3 of Table 4, except regressions with dependent variables measuring growth prior to the IPO filing year include lagged levels and growth rates as of year -3. Vertical lines at each point represent 95% confidence intervals for the instrumented IPO completion coefficient. The sample for each regression is restricted to large IPOs between 1986 and 2010, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars).

Panel B: Establishments



Panel B: Population



Panel C: Income Growth

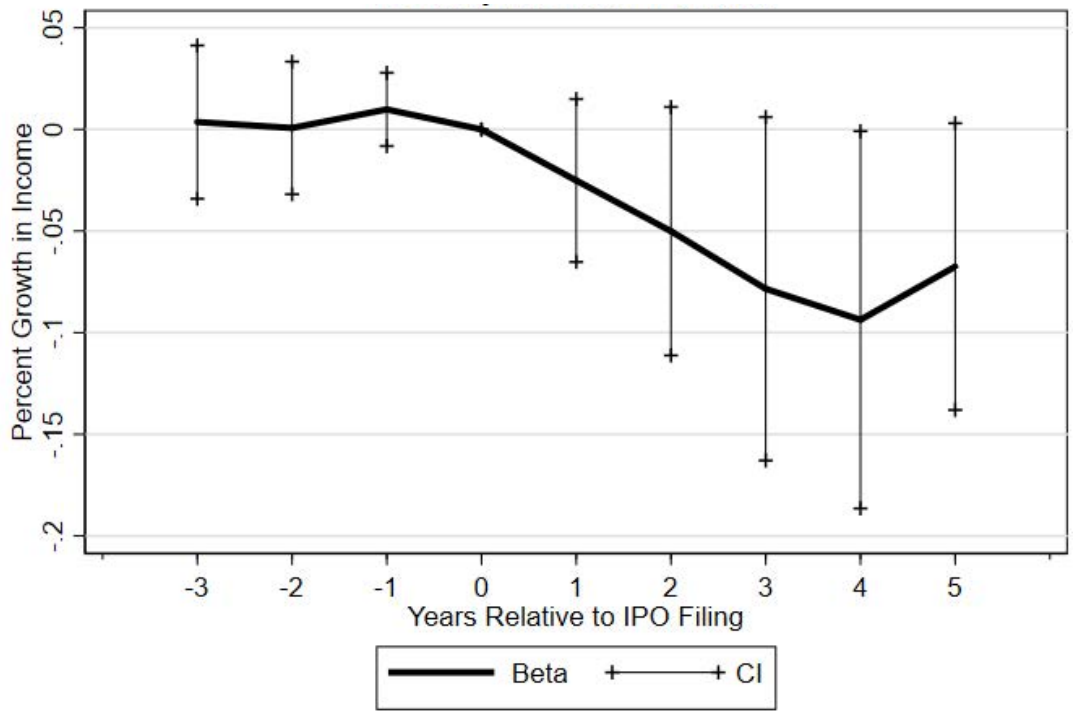


Table 1: Descriptive Statistics

This table presents means for the IPO characteristics (Panel A), and levels and growth rate of population, employees, and income per capita for county-years during our sample period (Panels B & C). Our sample includes IPO filings between 1986 and 2010. Large IPOs are defined as above median IPO filings, using the (inflation-adjusted) value of shares filed. Panel A presents averages of the IPO characteristics used throughout the analysis, partitioned by whether the IPO was completed or withdrawn. Panel B presents means and difference-of-means statistics, partitioned on whether an IPO was filed in that county, during that year (e.g., a county with an IPO in previous or future years, but not the current year, is classified as "No IPO Filings"). Panel C, presents similar statistics as Panel B, but the sample is restricted to county-years with IPO filings, and the sample is partitioned on whether the county-year experiences either only completed IPOs, or only withdrawn IPOs during that year (county-years with both completed and withdrawn IPO filings are excluded). The rightmost Column in Panels B and C presents the difference between the means across the partitions (Column 1 minus Column 3 in Panel B, and Columns 1 minus Column 2 in Panel C). *, **, and *** represent differences in means that are significant at the 10%, 5%, and 1% levels, respectively.

Panel A: IPO Characteristics

		All IPOs		Large IPOs	
IPO Characteristics	Full Sample	Completed	Withdrawn	Completed	Withdrawn
Proceeds Filed	86.60	81.34	106.14	149.17	154.09
PE/VC Funding	0.39	0.39	0.38	0.53	0.50
Underwriter Reputation	6.68	6.66	6.77	8.39	8.18
Number Lead Managers	1.21	1.18	1.30	1.38	1.45
N	6,205	4,887	1,318	2,203	813

Panel B: IPO filing county-years versus other county-years

	IPO Filings		No IPO Filings		
Economic Variables	Mean	Median	Mean	Median	Difference
Population	896,706	624,648	63,200	23,046	833,506***
Employees	570,237	393,908	33,708	10,541	336,529***
Real Income per Capita	40,830	37,908	28,479	27,431	12,352***
Lagged Population Growth	1.29%	1.04%	0.57%	0.46%	0.72%***
Lagged Employee Growth	1.83%	1.78%	1.06%	1.05%	0.77%***
Lagged Real Income Growth	4.43%	4.62%	4.41%	4.26%	0.02%
Population Growth 5yr	6.62%	5.24%	3.08%	1.97%	3.54%***
Employee Growth 5yr	9.53%	8.11%	5.39%	4.05%	4.14%***
Real Income Growth 5yr	23.33%	23.85%	23.17%	22.14%	0.16%
N	2,265	2,265	75,469	75,469	

Panel C: Completed IPO county-years versus withdrawn IPO county-years

Economic Variables	Completed IPOs	Withdrawn IPOs	Difference
Population	678,439	735,552	-57,112
Employees	415,271	466,988	-51,717*
Real Income per Capita	38,846	42,162	-3,316***
Lagged Population Growth	1.36%	1.20%	0.16%*
Lagged Employee Growth	2.03%	1.40%	0.62%***
Lagged Real Income Growth	4.50%	3.70%	0.80%***
Population Growth 5yr	7.08%	5.76%	1.32%***
Employee Growth 5yr	10.69%	6.75%	3.94%***
Real Income Growth 5yr	24.27%	18.71%	5.55%***
N	1,419	325	

Table 2: OLS Evidence

This table presents OLS estimates for regressions predicting future county-level economic growth, where the explanatory variable of interest in each column is an indicator for IPO completion. The dependent variable is the annual geometric average growth rate in a county's total number of employees over the five years following an IPO filing. We estimate the effect for the full sample of IPOs (column 1), the subsample of small IPOs, defined as those with below-median real filing proceeds (column 2), and the subsample of large IPOs, defined as those with above-median real filing proceeds (column 3). The sample period is between 1986 and 2010. We winsorize all dependent variables at the extreme 1%. All variables are defined in Appendix A. Standard errors are clustered at the county and year levels (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Employees	(2) Employees (Small IPOs)	(3) Employees (Large IPOs)
Instrumented IPO Completion	0.0007*	0.0010	0.0005
	(1.81)	(1.68)	(1.13)
Population Growth	0.2278***	0.1870*	0.2880***
	(2.93)	(1.83)	(2.99)
Employee Growth	0.0366	-0.0021	0.0344
	(1.20)	(-0.07)	(0.89)
Income Growth	-0.0607	-0.0491	-0.0570
	(-1.55)	(-1.71)	(-1.62)
IPO Size	0.0009	-0.0076	0.0011
	(0.66)	(-0.35)	(0.97)
Ln(Number of IPOs)	-0.0005	0.0020	-0.0023
	(-0.34)	(1.31)	(-1.41)
Number Lead Managers	0.0000	0.0009	-0.0002
	(0.11)	(0.92)	(-0.89)
PE/VC Funding	-0.0003	-0.0002	-0.0003
	(-0.82)	(-0.44)	(-0.84)
Underwriter Reputation	0.0000	-0.0000	0.0000
	(0.37)	(-0.17)	(0.26)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Adj. R-squared	0.724	0.765	0.715
Observations	6,205	3,029	3,016

Table 3: First Stage Estimation of IPO Completion

This table presents estimates for our first stage regression predicting IPO completion. We estimate the effect for the full sample of IPOs (columns 1 and 2), the subsample of small IPOs, defined as those with below-median real filing proceeds (column 3), and the subsample of large IPOs, defined as those with above-median real filing proceeds (column 4). Post-Filing 2-month Market Returns is the return of the CRSP value-weighted market index in the two-months following an IPO filing. The sample is composed of IPOs between 1986 and 2010. We winsorize all dependent variables at the extreme 1%. All variables are defined in Appendix A. Standard errors are clustered at the county and year levels (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Completed	(2) Completed	(3) Completed (Small IPOs)	(4) Completed (Large IPOs)
Post-Filing 2-month Market Returns	0.6586***	0.6796***	0.5936***	0.7493***
	(6.37)	(6.78)	(3.35)	(4.92)
Population Growth		-2.0059**	-1.4639	-2.9526*
		(-2.45)	(-1.14)	(-1.89)
Employee Growth		0.4757	0.6837	0.2872
		(1.18)	(1.21)	(0.43)
Income Growth		0.1894	0.0031	0.1384
		(0.68)	(0.01)	(0.34)
IPO Size		-0.0293	-1.0600*	0.0330
		(-0.26)	(-1.82)	(0.28)
Ln(Number of IPOs)		-0.0222	-0.0325	-0.0099
		(-1.27)	(-1.66)	(-0.45)
Number Lead Managers		0.0601***	0.0133	0.0521***
		(3.80)	(0.24)	(3.29)
PE/VC Funding		0.0631***	0.0218	0.0969***
		(5.01)	(1.49)	(5.42)
Underwriter Reputation		0.0054	0.0110**	0.0177*
		(1.58)	(2.50)	(1.97)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.130	0.142	0.078	0.174
Observations	6,293	6,205	3,029	3,016

Table 4: IPOs and Local Economic Growth

This table presents second-stage 2SLS estimates where the explanatory variable of interest is the fitted value of IPO completion, instrumented with the return of the CRSP value-weighted market index in the two-months following an IPO filing (see Table 3 for first stage estimates). The dependent variable is the annual geometric average growth rate in a county's total number of employees over the five years after an IPO filing. We estimate the effect for the full sample of IPOs (column 1), the subsample of small IPOs, defined as those with below-median real filing proceeds (column 2), and the subsample of large IPOs, defined as those with above-median real filing proceeds (column 3). The sample period is between 1986 and 2010. We winsorize all dependent variables at the extreme 1%. All variables are defined in Appendix A. Standard errors are clustered at the county and year levels (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Employees	(2) Employees (Small IPOs)	(3) Employees (Large IPOs)
Instrumented IPO Completion	-0.0039	0.0045	-0.0115**
	(-1.21)	(0.83)	(-2.42)
Population Growth	0.2186**	0.1925*	0.2550**
	(2.77)	(1.85)	(2.45)
Employee Growth	0.0386	-0.0045	0.0366
	(1.25)	(-0.14)	(0.97)
Income Growth	-0.0599	-0.0489	-0.0553
	(-1.55)	(-1.71)	(-1.71)
IPO Size	0.0007	-0.0035	0.0013
	(0.46)	(-0.15)	(0.71)
Ln(Number of IPOs)	-0.0006	0.0021	-0.0024
	(-0.42)	(1.44)	(-1.55)
Number Lead Managers	0.0003	0.0008	0.0004
	(0.90)	(0.84)	(1.16)
PE/VC Funding	0.0000	-0.0003	0.0009
	(0.04)	(-0.55)	(1.33)
Underwriter Reputation	0.0000	-0.0001	0.0002
	(0.72)	(-0.53)	(1.31)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Adj. R-squared	0.010	0.004	–
Observations	6,205	3,029	3,016

Table 5: Heterogeneous Effect of IPOs on Local Economic Growth

This table presents second-stage 2SLS estimates where the explanatory variables of interest are the fitted value of IPO completion and its interaction with IPO and county characteristics. The interaction with an instrumented variable implies that there are two first stages where we use CRSP value-weighted market returns in the two-months following an IPO filing and the interaction of this return with the corresponding IPO or county characteristic as instruments. We report the first stage F-statistic for these combined instruments in the bottom row of the table. The dependent variable is the annual geometric average growth rate in total employees in counties with an IPO filing over the five years following an IPO filing. In column 1, we interact instrumented IPO completion with IPO Size, defined as the real value of IPO filing proceeds (in billions). In column 2, we interact instrumented IPO completion with county-level per-capita personal income (in millions) measured in the year prior to the IPO filing. In column 3, we interact instrumented IPO completion with the ratio of IPO Size to per-capita county income (in millions). We winsorize all dependent variables at the extreme 1%. The sample includes IPO filings between 1986 and 2010. Each column is estimated using the control variables from Column 3 of Table 4 (suppressed to save space), and all variables are defined in Appendix A. Standard errors are clustered at the county and year levels (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Employees	(2) Employees	(3) Employees
Instrumented IPO Completion	0.0032	-0.0306***	0.0055
	(0.90)	(-4.40)	(1.25)
Instrumented IPO Completion*IPO Size	-0.0539**		
	(-2.36)		
Instrumented IPO Completion*Income		0.6126***	
		(3.70)	
Instrumented IPO Completion*IPO Size/Income			-3.1276**
			(-2.41)
IPO Size	0.0405**	0.0002	0.0055
	(2.37)	(0.13)	(0.33)
Income		-0.2904	
		(-0.71)	
Size/Income			2.1037*
			(1.81)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Observations	6,205	6,205	6,205
First Stage F-statistic	24.0	23.9	24.6

Table 6: IPOs and Local Establishment, Population, Unemployment, and Income Growth

This table presents second-stage 2SLS estimates where the explanatory variable of interest is the fitted value of IPO completion, instrumented with the CRSP value-weighted market return in the two-months following an IPO filing. The dependent variables are measured as the annual geometric average growth rate over the five years following an IPO filing. In Panel A we examine growth in the number of county-level establishments; in Panel B the growth in county population; in Panel C the change in the unemployment rate (i.e., unemployed divided by employed plus unemployed); and in Panel D the growth in per-capita personal income. Across each panel, we estimate the effect for the full sample of IPOs (column 1), the subsample of small IPOs, defined as those with below-median real filing proceeds (column 2), and the subsample of large IPOs, defined as those with above-median real filing proceeds (column 3). We winsorize all dependent variables at the extreme 1%. The sample includes IPO filings between 1986 and 2010. Each column is estimated using the control variables from Column 3 of Table 4, and all control variables are defined in Appendix A. Standard errors are clustered at the county and year levels (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: IPOs and Local Establishments Growth

	(1) Establishments	(2) Establishments (Small IPOs)	(3) Establishments (Large IPOs)
Instrumented IPO Completion	-0.0030	0.0047	-0.0083**
	(-1.19)	(0.97)	(-2.43)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Adj. R-squared	0.007	–	–
Observations	5,737	2,706	2,888
First Stage F-statistic	37.3	8.0	23.5

Panel B: IPOs and Local Population Growth

	(1) Population	(2) Population (Small IPOs)	(3) Population (Large IPOs)
Instrumented IPO Completion	-0.0019	0.0036	-0.0054**
	(-1.18)	(1.41)	(-2.28)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Adj. R-squared	0.133	0.081	–
Observations	6,205	3,029	3,016
First Stage F-statistic	46.0	11.3	24.2

Panel C: IPOs and Local Unemployment Rate Growth

	(1) Unemployment Rate	(2) Unemployment Rate (Small IPOs)	(3) Unemployment Rate (Large IPOs)
Instrumented IPO Completion	0.0079	-0.0184	0.0058
	(0.42)	(-0.55)	(0.29)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Adj. R-squared	0.062	0.036	0.078
Observations	5,208	2,341	2,739
First Stage F-statistic	28.0	3.6	27.0

Panel D: IPOs and Local Income Growth

	(1) Income	(2) Income (Small IPOs)	(3) Income (Large IPOs)
Instrumented IPO Completion	-0.0084*	-0.0007	-0.0113*
	(-1.86)	(-0.11)	(-1.83)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Adj. R-squared	–	0.034	–
Observations	6,205	3,029	3,016
First Stage F-statistic	46.0	11.3	24.2

Table 7: Establishment-Level Dispersion for IPO firms

This table presents second-stage 2SLS estimates where the explanatory variable of interest is the fitted value of IPO completion, and its interaction with the average wage in the county with an IPO filing, in the year preceding the filing. The dependent variable in Columns 1 and 3 is the cumulative percent growth in the number of IPO-firm employees that reside in the firm's home county less the percent growth in the number of IPO-firm employees outside of the home county in the two years after an IPO filing, while the dependent variable in Columns 2 and 4 is defined analogously for the number of IPO-firm establishments. We winsorize all dependent variables at the extreme 1%. The sample is restricted to large IPOs between 1986 and 2010, where large IPOs are defined as those in the top tercile in terms of the value of shares filed relative to the number of pre-filing employees in the county. All variables are defined in Appendix A. Standard errors are clustered at the county and year levels (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Employee Growth	(2) Establishment Growth	(3) Employee Growth	(4) Establishment Growth
Instrumented IPO Completion	-0.216**	-0.199**	-0.996*	-1.488***
	(-2.389)	(-2.112)	(-1.683)	(-3.116)
Instrumented IPO Completion × Ln(Wages)			0.882	1.451***
			(1.366)	(2.794)
IPO & County Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Observations	1,800	1,800	1,800	1,800

Table 8: Geographic Distribution of Acquisition Activity after IPO Filing

This table presents second-stage 2SLS estimates of post-IPO filing acquisition deal value, where the explanatory variable of interest is the fitted value of IPO completion, instrumented with the CRSP value-weighted market return in the two-months following an IPO filing. The dependent variable in Column 1 is the natural log of total deal value of all acquisitions completed by an IPO filing firm in the two years after the IPO filing; the dependent variable in Column 2 is the natural log of total deal value of out-of-state acquisitions by an IPO filing firm over the two years after the IPO filing; the dependent variable in Columns 3 is the percentage of total acquisition value by an IPO filing firm over the two years following an IPO filing that involves targets located outside the IPO firm's headquarter state; and the dependent variable in Column 4 is the average distance between the state of the IPO filing firm and the state of acquired firms in deals completed over the two years following and IPO filing. All variables are defined in Appendix A. Standard errors are clustered at the industry and year levels (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Total Acquisition Value	(2) Non-Local Acquisition Value	(3) % Non-Local Acquisition Value	(4) Distance of Non-Local Mergers
Instrumented IPO Completion	2.375***	1.056**	0.235*	834.085**
	(3.64)	(2.19)	(1.97)	(2.27)
Population Growth	14.435***	9.953**	2.257**	3550.826
	(3.12)	(2.72)	(2.52)	(1.34)
Employment Growth	-3.951**	-3.712**	-0.926***	-1208.198
	(-2.47)	(-2.36)	(-3.37)	(-1.51)
Income Growth	-0.088	-0.760	-0.105	-1440.499**
	(-0.06)	(-0.55)	(-0.33)	(-2.61)
IPO Size	2.355***	1.436***	0.054	345.171*
	(9.09)	(7.13)	(1.11)	(1.95)
Ln(Number of IPOs)	0.047	0.040	0.013	65.705*
	(0.92)	(0.75)	(0.83)	(1.97)
Number Lead Managers	-0.120	-0.046	-0.007	-64.421
	(-1.04)	(-0.57)	(-0.58)	(-1.60)
PE/VC Funding	-0.099	-0.064	-0.016	-51.436
	(-1.52)	(-1.00)	(-1.05)	(-1.45)
Underwriter Reputation	0.091***	0.074***	0.014***	24.957***
	(5.56)	(5.31)	(4.61)	(6.63)
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Adj. R-squared	0.000	0.053	0.041	0.009
Observations	6,205	6,205	6,205	6,205