Initial Public Offerings and the Local Economy

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Abstract

We provide evidence that a firm's transition from private to public ownership stunts local economic growth, but only for large IPOs located in relatively small local economies. After accounting for endogeneity in the ownership decision, areas hosting large companies that go public experience muted growth in employment, establishments, population, and wages, relative to areas where firms remain private. 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.

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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). This 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. This is supported empirically 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 it disproportionately lowers the cost of non-local inputs and leads firms to expand outside their home county. This 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 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 in which a firm files for an IPO. 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.[1] 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. 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) over 50 placebo tests indicate that there is no significant relation between future local economic growth and market returns occurring during any two-month period in the two years surrounding IPO filings, except for the two-month period immediately following an IPO filing, and b) there is no relation between market returns during

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

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.[2]

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 that the average IPO has no effect on local economic growth; however, we find that large IPOs reduce local county-level employment growth relative to what would have occurred had the firm remained private. During the five years after a large firm completes its IPO, we estimate that employment growth declines by approximately 50 basis points per year relative to areas with withdrawn IPOs. We also find that establishment growth declines by a similar magnitude. Consistent with our identifying assumptions, counties hosting exogenously completed and withdrawn IPOs have parallel trends in economic activity prior to IPO filings.

The fact that the effects of an IPO on local growth are limited to large IPOs is intuitive, since large firms are more likely to play an important role in the local agglomeration. We further examine this idea by estimating the effect of an IPO on local economic growth separately for dense and sparse business agglomerations. Because a large firm located in a sparse business agglomeration (as opposed to, for instance, a metropolitan area) is likely to be an especially crucial part of the local economy, we expect to find especially large effects when such a firm goes public. Consistent with this prediction, the negative effects of going public are limited to large IPOs going public in counties with below median employee or establishment density.

Averaging across several specifications, we estimate that a large IPO slows job growth by approximately 1,900 fewer jobs per year in the county where the firm is located relative to areas with withdrawn IPOs. We expect the IPO to directly affect employment growth at the IPO-firm, 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 that about 20% of these job losses come from the IPO-firm shifting employment outside of its headquarter county in the years after going public. Considering only

[2] 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 related to the firm's probability of completing their IPO.

the job losses at the IPO-firm and applying a spillover multiplier of 2, which is in the middle of the range estimated in the existing literature (Moretti, 2010), accounts for approximately 60% of the annual job reduction. Taking into account the direct effect of the IPO on other local firms, as well as allowing for the possibility of larger multipliers, can entirely explain the magnitudes we estimate.

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., 8 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 an entire decade to reach a new equilibrium after large firms go 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. We also find that completed IPOs significantly reduce county-level per-capita personal income and wage growth. Further tests provide ambiguous results as to whether these effects are driven by a changing composition of workers or within-worker changes in pay.

In our final set of tests, we explore the channel through which IPOs reduce local economic growth. In particular, we examine whether post-IPO changes in local economic growth are at least partially driven by firms geographically expanding after they go public, which would be consistent with a relative reduction in the cost of non-local inputs. To investigate this question, we introduce establishment-level data obtained from the U.S. Census Bureau's Longitudinal Business Database (LBD). These data allow us to explore within-firm geographic diversification of operations before and after an IPO filing. Not only do we find evidence that going public causes firms to more aggressively grow their labor force and establishments outside of their local economy, but this expansion is most pronounced in poorer local economies. We corroborate this

result using publicly available data and show that firms going public from 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 from wealthier areas. To further support the idea of geographic expansion, we extend Bernstein (2015)'s result that IPOs cause issuers to increase acquisition activity. We show that not only do IPOs lead to more acquisitions, but they also cause the mix of acquired targets to shift toward more geographically distant firms.

Taken together, these findings suggest that one likely mechanism through which going public stunts local economic growth is that IPOs enable firms to more easily grow outside of their local economies. 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.

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 have the opposite effect, adversely affecting 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 headquartered 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.[3] This literature has advanced substantially in recent years due to the empirical framework we employ, which was pioneered by Bernstein (2015). Bernstein (2015) shows that the increased

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

agency costs from going public reduce 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 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.[4] Thus, Butler et al. (2019) cannot identify county-level effects, and we 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 have negative aggregate effects on employment and establishments at the county level is consistent with both our findings and those in Butler et al (2019).[5]

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

[4] 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.
[5] 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.

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. And just as importantly, we do not conclude that all IPOs, or even the average IPO, disrupt local agglomeration economies. Rather, for a set of large firms that make up a significant part of their local agglomeration, going public results in slower growth for the local economy than it would have had if the firm had stayed private.

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."[6]

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.[7] And, the Farmington, Utah based Pluralsight, which went public in May 2018, 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).[8]

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 the 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, which represent oft-cited reasons managers decide to go public (see e.g., Roell, 1995; Brau and Fawcett, 2006).[9]

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,

[6] See "Under Armour gets serious" published in Fortune, available at http://fortune.com/2011/10/26/under-armour-gets-serious/.
[7] See https://www.wpri.com/business-news/biotech-firm-rubius-buys-former-alexion-factory-in-smithfield/1322970442
[8] See https://www.deseretnews.com/article/900018299/upcoming-pluralsight-ipo-aims-to-harvest-dollar228m-grow-valuation-to-dollar15b.html
[9] 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.

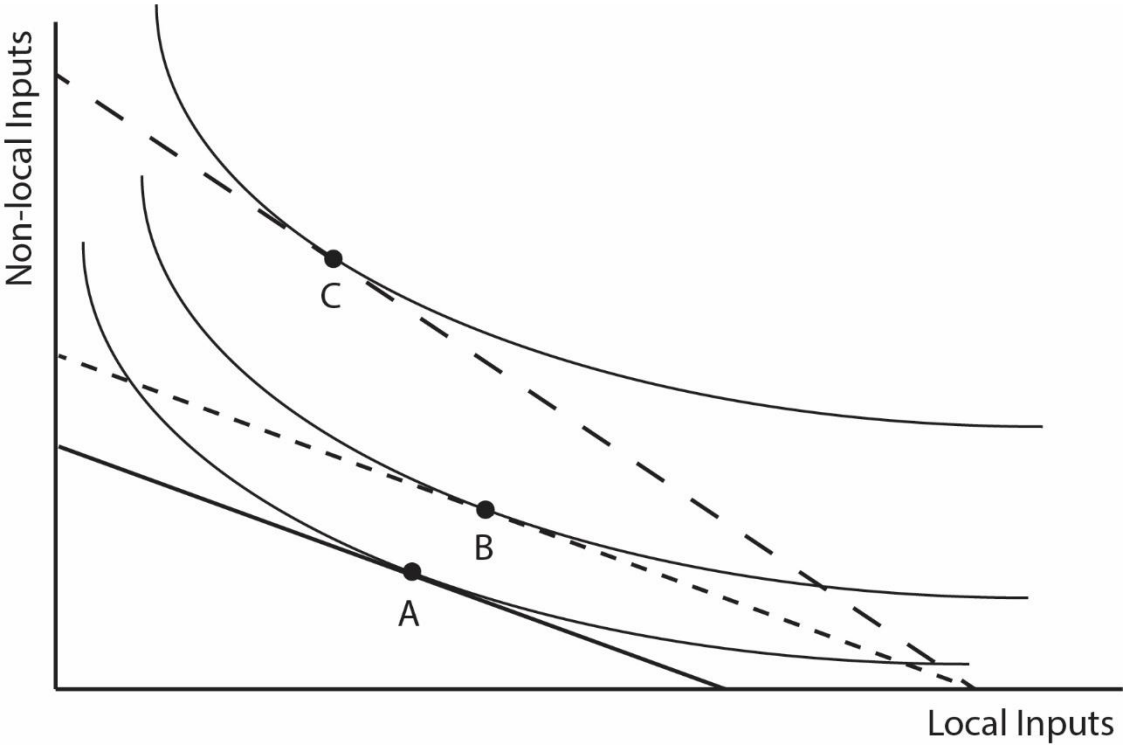
which improves firm visibility in the marketplace. Improved visibility reduces asymmetric information between the firm and suppliers of goods, labor, and funding, which in expectation allows the firm to negotiate better deals (Roell, 1995). Consistent with visibility improving bargaining power, Turban 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 total number of employees after going public (see e.g., Kenney, Patton, and Ritter, 2012, and Borisov, Ellul, and Sevilir, 2017).[10]

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).

[10] 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.

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 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 ultimately an empirical question. The effect will depend on the firm's specific 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 2011 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.[11] We obtain county-level establishments data from the 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.

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.[12]

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

Our analysis requires us to be able to separate small firms from large firms, since investment decisions of small firms are less likely to affect the local economy. We classify firm size based on IPO filing proceeds, primarily because other measures of firm size (e.g., employees, sales, etc.) are not available for the firms in our sample that withdraw their IPO and remain private. Consequently, we exclude IPOs without reported filing proceeds and define large IPOs as firms with filing proceeds (in real terms) greater than the sample median. Admittedly, this definition introduces some noise into our estimation, since it is possible for a small firm to raise a large amount of capital in an IPO. This problem is likely most acute during the 1998-1999 tech bubble, when a large number of extremely young firms went public at very high valuations. While our definition would classify these firms as large, most of these firms had few employees or physical assets and so we would not expect them to be large enough to affect the local economy in the near term. Consequently, to limit noise in our sample, we remove all IPOs during the bubble period.[13] We show in the appendix that our main results are robust to including the bubble period in our sample.

After imposing these restrictions, our sample includes 6,451 IPO filings in 578 counties across 24 years.[14] 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 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

[13] 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.
[14] The sample is slightly smaller for some outcomes due to data limitations.

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 $83 million in proceeds (in 2011 dollars), compared to $106 million for ultimately withdrawn offerings. Within the subset of large IPOs (of which approximately 27% are withdrawn), however, the filing proceeds are quite similar ($152 vs. $158 million). The industry distribution of completed and withdrawn IPOs is also similar with the three most frequent SIC 2-digit groups being business services (SIC 73), chemicals and allied products (SIC 28), and electronic equipment (SIC 36). In both subsamples, each of these three industries comprises between 7% and 20% of the sample.

Figure 2 illustrates the geographical dispersion of the large IPOs in our sample. Over the sample period, few counties host more than five IPOs, and the IPOs in our sample are spread across much of the United States.[15] 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 nine times larger than other county-years in terms of population, and ten times larger in terms of total employees. Inflation adjusted per capita income is 35% higher in IPO county-years. More importantly, IPO county-years exhibit significantly higher past and future growth in employment

[15] The geographic pattern of IPOs in the full sample looks very similar, and is reported in Figure B1 in the Appendix.

and population compared to 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, population, and income. 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 future county growth. The regressions in all four columns are estimated at the IPO level. In Column 1, the dependent variable is the annual geometric average of five-year county employment growth following an IPO filing, while in Column 2 the dependent variable is the annualized five-year county establishment growth rate. The positive coefficient on IPO completion in both specifications suggests that counties with completed IPOs experience higher future employment and establishment growth than counties with withdrawn IPOs, after controlling for lagged economic conditions.

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, in Columns 3 and 4 when we interact the completion decision with an indicator for large IPOs, we find that the relation between local economic growth and IPO completion is smaller for large IPOs. This could be consistent with omitted variables driving the observed relation between IPO

completion and future economic growth if, for example, smaller firms' IPO completion decisions were more predicated on expected growth in the local economy. Together, Table 1 and Table 2 indicate that a different empirical methodology is necessary to identify the causal relationship between IPOs and local economic growth.

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 to those same outcomes in similar counties with 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. In addition, the geographic concentration of firms driving broad market returns 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 other firms that drive fluctuations in NASDAQ returns. Nonetheless, our findings are similar when we use two-month post-filing NASDAQ returns as the IV (see Appendix B for these additional 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:

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

, 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. 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. 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., proceeds filed), the number of lead managers, private equity or venture capital backing, and underwriter reputation.[16] Finally, we include county or state (δ_k), 2-digit NAICS industry(λ_j), and filing year (γ_t) 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:

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

, where Δ *Econ. Outcome* represents the annual (geometric average) percent change in economic activity in county *i* over the five years beginning at time *t*, i.e., the year of the IPO filing. Our primary measures of economic activity are county-level employment and establishment growth, though we also examine changes in population, unemployment, and per-capita 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

[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.

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. Columns 1 and 3 include state fixed effects, while Columns 2 and 4 include county-level fixed effects; all columns include year and industry fixed effects. We estimate the first stage for both the entire sample of IPOs (Columns 1 and 2) and for the subset of large IPOs (Columns 3 and 4). Consistent with prior 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). Notably, including county-level fixed effects has little effect on the predictive power of post-filing market returns, suggesting that post-filing market returns are unrelated to county characteristics. In sum, 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-statistics range between 53 and 24, far exceeding a 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 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, it is plausible after including year fixed effects (and other controls for current economic conditions) in the regression. A violation of this assumption would require abnormal market returns during a seemingly arbitrary two-month period to predict 5-year economic growth after controlling for current economic conditions via time fixed effects and controls for

[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 14% (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%.

contemporaneous county-level economic conditions. 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:

$$\begin{aligned}\Delta\ Econ.\ Outcome_{it,t+5} = {} & \delta_1 Two\text{-}Month\ Market\ Ret._{it} + \delta_2\ Emp.\ Growth_{it-1} + \delta_3\ Pop.\ Growth_{it-1} + \\ & \delta_4\ Income\ Growth_{it-1} + \delta_5\ Ln(IPOs)_{it} + \delta_6\ Ln(IPO\ Size)_{it} + \delta_7\ Leads_{it-1} + \\ & \delta_8\ PE\ \ or\ VC_{it} + \delta_9\ Underwriter\ Rep._{it} + \delta_k + \lambda_j + \gamma_t + \varepsilon_{it} \end{aligned} \quad (3)$$

, 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, except when *Two-Month Market Ret.*$_{it}$ is measured immediately following a local firm filing to go public.

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 thirteen event-time windows over which *Two-Month Market Ret.*$_{it}$ is computed – other than the window immediately following a local IPO filing – are significantly related to future employment growth. And, as would be expected by random chance, six of the thirteen estimates are positive and seven 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 52 total regressions, market returns are only negatively correlated with future economic growth in the 4 regressions in which 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 measures of local economic activity are the number of employees and the number of establishments 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 in Panel A. 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 insignificant. In Column 2, we limit the sample to the subset of large IPO filings (above median proceeds). These large firms are more likely to be an important part of local business agglomerations; as a result, we expect the effect of going public on the local economy to be larger in magnitude. The significantly negative coefficient on *Instrumented IPO Completion* in Column 2 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. In Columns 3 and 4, we re-estimate this effect with a slightly different specification. We include the entire sample of IPO filings and estimate not only the effect of completing an IPO, but also the additional effect of being a large IPO filing that is completed (i.e., Instrumented IPO Completion × Large IPO).[20] In Column 3 we include state-level fixed effects; in Column 4 we include county-level fixed effects. Across both columns, the direct effect of IPO completion for smaller IPOs is small, negative, and insignificant. In contrast, the marginal effect of large IPOs is larger, negative, and highly

[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.
[20] Note that because we are interacting the instrumented variable with an indicator for large IPOs, there are technically 2 separate first stage regressions. We table the full first stage regressions for these columns in the Appendix.

significant. The magnitude of the estimated effect ranges from 38 to 44 basis points annually, or about a 0.16-0.19 standard deviation decrease in the annual employment growth rate.[21]

To get a sense of whether business displacement is responsible, at least in part, for the decline in employment growth, we examine how IPOs affect local establishment growth in Panel B of Table 4. We find very similar results. While there is no effect of small IPOs, an arguably random completion of a large IPO reduces local establishment growth by about 42 basis points per year (see Column 4).

Large IPO firms are likely to be particularly important to the local economy in areas where there is less business development. In Table 5, we measure the extent of local business development using the number of employees per square mile in the county (employee density) and the number of establishments per square mile in the county (establishment density). Both densities are measured as of the year prior to the IPO filing. We estimate the regressions reported in Columns 3 and 4 of Table 4 separately for counties that are below and above the sample median in employee and establishment density and report the results in Table 5.

Panels A and B examine local employee growth split on pre-IPO employee and establishment density, respectively. Panels C and D examine local establishment growth based on the same splits. Across all four panels, we find a negative and statistically significant effect of IPOs on local economic growth—but only for large IPOs located in below median density counties. The magnitude of the effect is similar to our earlier estimates; there is a roughly 50 basis point decline in the annual growth rate of employment, and a slightly lower annual decline in the growth rate of establishments. Importantly, though, the negative effect of going public is limited to relatively large IPOs located in relatively underdeveloped economies.

With the caveat that that 2SLS estimates represent local average treatment effects and that the estimates in Table 5 make strong functional form assumptions, we can use the results in Table 5 to generate a back of the envelope estimate of the number of jobs lost due to a large IPO. Benchmarked to the median number of employees located in counties with below median density, the estimates in Panel A and B imply that a large firm going public leads to between 1,300 to 2,800 fewer employees per year, with an average across all four specifications of 1,900

[21] Results are qualitatively similar including year-quarter fixed effects (in addition to industry) or including industry cross year fixed effects.

fewer employees per year. 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 is 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 385 employees per year out of its headquarters county after going public. The direct effect of the IPO also includes jobs at local non-IPO firms that are lost due to IPO firm shifting business away from the area. This effect may be quite large to the extent that the local economy is predicated on growth in the IPO firm and not its static size. For instance, if the IPO firm chooses not to reduce its local presence but does shift future growth out of the area, then many contractors and construction workers will be forced to leave the area. Thus, it is safe to assume that the direct effect of the IPO is somewhat larger than 385 employees per year. Moretti (2010) estimates local employment multipliers to be between 1.5 and 2.5; applying a multiplier in this range to just the 385 IPO firm employees per year implies that the total annual job losses of the IPO are between 963 and 1,347. Assuming that the IPO directly affects other local businesses would result in even larger estimates. This bottom-up estimate indicates that, though large, the total number of lost jobs is quite plausible given the direct shifts in employment at the IPO-firm combined with a reasonable local employment multiplier. 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.

Next, we explore the evolution of the effect that a large IPO has on local economic growth. In Figure 4, we examine the post-IPO employment (Panel A) and establishment (Panel B) decline in event time surrounding the IPO filing year. Specifically, 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 2 of Table 4.[22] These figures show that both employment

[22] Lagged local economic conditions are measured as of year -4 when measuring pre-IPO filing economic growth.

and establishment growth begin to decline in the year following the IPO filing, and continue to decline at a steady rate over the five-year post-filing window, with the effect becoming significant at the 5% level four years after the IPO filing. Unreported results indicate that the employment growth reduction persists through the tenth 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 or establishment growth, indicating similar pre-trends in both employment and establishment growth between counties hosting exogenously completed and withdrawn IPOs. This pattern mitigates the possibility that *Instrumented IPO Completion* is spuriously correlated with local economic conditions. 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., $\pm$ 5%). While the "positive" shock group has similar lagged growth as the "negative" shock group, the positive shock group has significantly lower long-run future county employment growth.

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

In Table 6, we further examine how IPOs affect local economic growth, shedding 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 decline in employment. The estimates in Column 1 indicate that a county's growth in population declines by approximately 25 basis points each year during the five years following a completed IPO; we find no evidence of a significant change in unemployment rates. Panel A of Figure 6 indicates that, like the reduction in post-completion employment growth, the population growth rate declines steadily for five years. We continue to find no significant relation between *Instrumented IPO Completion* and pre-filing county-level growth.

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 Columns 3 and 4 of Table 6, we use annual average changes in per-capita personal income and wages per employee 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 significant decline in the growth of both measures. Five years after IPO completion, the average county resident has personal income that is approximately 10% less than if the issuer had withdrawn its IPO. The effect of local IPOs on wages in Column 4 is qualitatively similar and significant but only about 40% of the magnitude. This differential magnitude could be due to either IPOs having a larger effect on business owners relative to wage earners or a changing composition of residents.

In unreported tests, we examine whether IPOs affect economic outcomes using more geographically broad definitions of the local economy. To do so, we use the same first-stage regression and explanatory variables, but compute second-stage dependent variables as the average growth rate in the five nearest counties to an IPO filer's headquarter county. We find that the estimated effect of IPO completion is in the same direction for employment, population, and income growth, but only between 16% and 28% of the size of the effect that we observe in the IPO filer's headquarter county. In all three cases, the estimated effect is insignificant, with t-

statistics ranging from -0.28 to -1.37. We also find insignificant effects defining dependent variables as the five-year growth rate in the single nearest neighboring county. Thus, the effect of IPOs on local economic growth appears to be concentrated within the IPO filer's home county, and there is no offsetting effect immediately outside the county's borders.

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 in relatively less dense local economies disrupt economic growth at the county level. 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.

A reduction in local investment by IPO firms after going public represents a natural starting point for this examination.[23] However, the magnitude of the effect we document relative to the average IPO-firm's size makes it unlikely that IPO-firm downsizing alone is responsible for our results.[24] Thus, it is likely that any changes in local investment by IPO firms after going public have substantial spillover effects that proliferate through the local economies.

[23] Existing literature offers a variety of reasons why firm-level local investment might decline following an IPO, such as reduced R&D spending and exodus of top talent (Asker, Farre-Mensa, and Ljungqvist, 2015; Bernstein, 2015; Babina, Ouimet, and Zarutskie, 2017).

[24] An alternative channel through which IPOs might disrupt local economic growth is the real-estate market: IPO insiders boost local housing prices, middle income residents are priced out, and economic activity declines. Using county-level housing price indices from the Federal Housing Finance Agency, we do not find evidence to support this channel. In fact, we find some evidence that local home prices decline following completed IPOs.

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 of the 3,202 large IPO 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.

[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.
[27] Census disclosure requirements require us to round the reported number of observations to the nearest hundred.
[28] We correct for this 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.

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 2 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 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.

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 small economy) 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.

[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.

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. If labor markets are efficient, wages should reflect the marginal productivity of labor. 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 Columns 3 and 4 of Table 7, 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). This finding suggests 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.

VI.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 and negotiating acquisitions, 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 two-stage least squares 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.[30] In particular, we a) match all completed acquisitions conducted by both completed and withdrawn

[30] Target firm zip codes are less reliably reported.

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 27% 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 nearly 700 miles further for 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.

VI.C Additional Evidence from Post-IPO Filings

Table 7 suggests that firms located in counties with a less productive marginal worker are more likely to expand non-locally following a public listing. We further examine this behavior using geographic dispersion measures from data collected by Garcia and Norli (2012), which record the number of state-name counts in firms' 10K reports. Their sample runs from 1995 to 2008 and, after requiring two consecutive data points of 10K state-name counts in the first two years after going public, we match 1,443 issuers in our sample to these data (58% of our completed IPOs over this period). We create two measures of post-IPO geographic expansion. Δ *Non-local Operations* is defined as the one-year percentage point change in the ratio of total counts of the states that are not the firm's headquarter state to total counts of all states. *Post-IPO Geographic Dispersion Growth* is defined as the one-year percentage change in the number of state-name mentions, using the earliest two consecutive post-IPO 10K filings.

The average one-year change in non-local operations after going public is an increase of 1.5 percentage points, while the average one-year change in geographic dispersion after going public is an increase of 15.2 percentage points. In Table 9, we examine whether this post-IPO growth in geographical dispersion (for completed IPO firms) is decreasing in the income of the issuer's county. We regress *Post-IPO Geographic Dispersion Growth* on the natural log of per capita income (Column 1 and 3) and relative average wages (Column 2 and 4). We find a significant negative relation between county income and post-IPO growth in non-local operations.[31]

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. While this makes it difficult to quantify the total effect, all three approaches – establishment level geographic distribution of production, acquisition activity, and geographic distribution of operations from disclosures – consistently show that firms expand outward after going public, especially in areas with fewer resources.

VII. 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 located in smaller local economies reduce employment and establishment growth in the county where the IPO originates. 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

[31] The effect is similar in magnitude whether or not we include controls.

that growth rates in population and wages decline, while unemployment rates remain stable. Additional tests indicate that one mechanism behind these lower growth rates 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 causing 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 2012 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 2011.
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	
Employees	Log of one plus the count of full-time and part-time jobs in the county and filing year of an IPO, covering wage and salary jobs and self-employment. 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
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).
Establishments	Log of one plus the number of establishments (measured as of March 12) in the county and filing year of an IPO filing. 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
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).
Population	Log of one plus the Census Bureau's annual population estimates, measured July1 of each year, in the county and filing year of an IPO filing (BEA).
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 Rate	Log of one plus the annual monthly average unemployment rate in the county and filing year of an IPO. 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/.
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.

Income	Log of one plus the personal income of the residents in the county of an IPO filing, divided by the resident population of that county, measured as of the year of the filing in 2012 dollars. 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. (BEA).
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).
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 2012 dollars (BEA).
Wages Growth	One year growth rate in the total annual wages and salary (per-worker) by place-of-work from the pre-filing year to the filing year. 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 (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 2012 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).
Annualized 5-year Wages Growth	Five year percent change (converted to an annual geometric average) in total annual wages and salary per worker by place of work. 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 (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 previously presented).

In Figure B1, we show the geographical distribution of our full sample of IPO-filings. This map is very similar to the map in Figure 3, 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, although there is a significant concentration of IPOs located in California, our sample includes IPOs located across the entire United States. We show later in this appendix that our results are robust to excluding California IPOs from our analysis.

Throughout much of the paper, our primary specification of interest includes both the decision to complete an IPO (instrumented with market returns) and the interaction of this variable with an indicator for large IPOs. This modelling choice results in two endogenous variables (Completed IPO and Completed IPO × Large IPO) and two instruments (Post-Filing 2-month Market Returns and Post-Filing 2-month Market Returns × Large IPO). Using two endogenous variables implies that there are two first stage regressions rather than the single first stage regression that we report in the paper. In Table B1, we show the full first stage specifications. Columns 1 and 2 report the two first stage regressions using state fixed effects (corresponding to Column 3 of Table 4), while Columns 3 and 4 include county fixed effects (corresponding to Column 4 of Table 4). The results reported in Table B4 corroborate our earlier analysis—market returns during the book-building phase of the IPO are a strong positive predictor of completing the IPO, both overall and separately for large IPOs. Consequently, market returns in the two months after an IPO filing are a relevant instrument for IPO completion.

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 bookbuilding phase of the IPO. Panels A-D of Figure B1 extend this analysis. This figure plots

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 economic growth in the five years after an IPO filing, but are unrelated to county-level 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.[32] 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 Figure B2 we examine the robustness of our main results, and placebo 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 the same conclusion from results displayed in Panel B – which removes spinoffs, limited partnerships, and unit offerings from our sample – and Panel C – which removes California-headquartered firms from our sample.

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 specifications. In Panels A and B we find that the negative effect of IPO completion on employment and establishment growth persists when we include the IPO bubble period. In Panels C and D, we show that the effects are robust to removing spinoffs, limited partnerships, and unit offerings, and in Panels E and F we show that the effects are robust to removing all IPO filings for firms headquartered in California.

As we explain the paper, we differ from Bernstein (2015) by using CRSP market returns, rather than NASDAQ returns, as an instrument for IPO completion. Our primary motivation for this decision is that Figure B1 reveals that many IPOs are located in Silicon Valley and the Seattle area. These areas have a particularly high concentration of NASDAQ-listed public firms. As a result, NASDAQ returns might be correlated with the local economic conditions of private firms located in Silicon Valley or Seattle, which would violate our exclusion restriction and call into question our analysis. (Note, though, the fact that our results are unchanged when excluding California suggests that this is not likely to be a problem in our setting.)

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

To alleviate this concern, we use CRSP market returns. This broad set of public firms is not geographically concentrated; as a result, the aggregate CRSP returns are unlikely to disproportionately reflect local economic conditions. Despite this, in Table B3 we confirm that our main results are robust to following Bernstein (2015) and using NASDAQ returns as our instrument. The takeaway is the same: whether we use NASDAQ or CRSP returns as the instrument, we find that large IPOs lead to a slowdown in local employment and establishment growth relative to staying private.

Finally, in Figure B4, we directly test the claim that our effects are rooted in housing market dynamics. Specifically, we test whether there is evidence of an increase in local home prices that could be responsible for a change in the employee base and income levels in the local region. 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 irregularities in the relation between 2-month market returns and local building permits in the areas where IPO firms are located.

To summarize, in our analysis assessing the validity of the exclusion restriction we find that a) market returns in the two months after an IPO filings 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 IPO sample

This figure plots the geographical distribution of the 6,451 IPO filings in our main sample across U.S. counties. The sample runs from 1986 through 2011. 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.

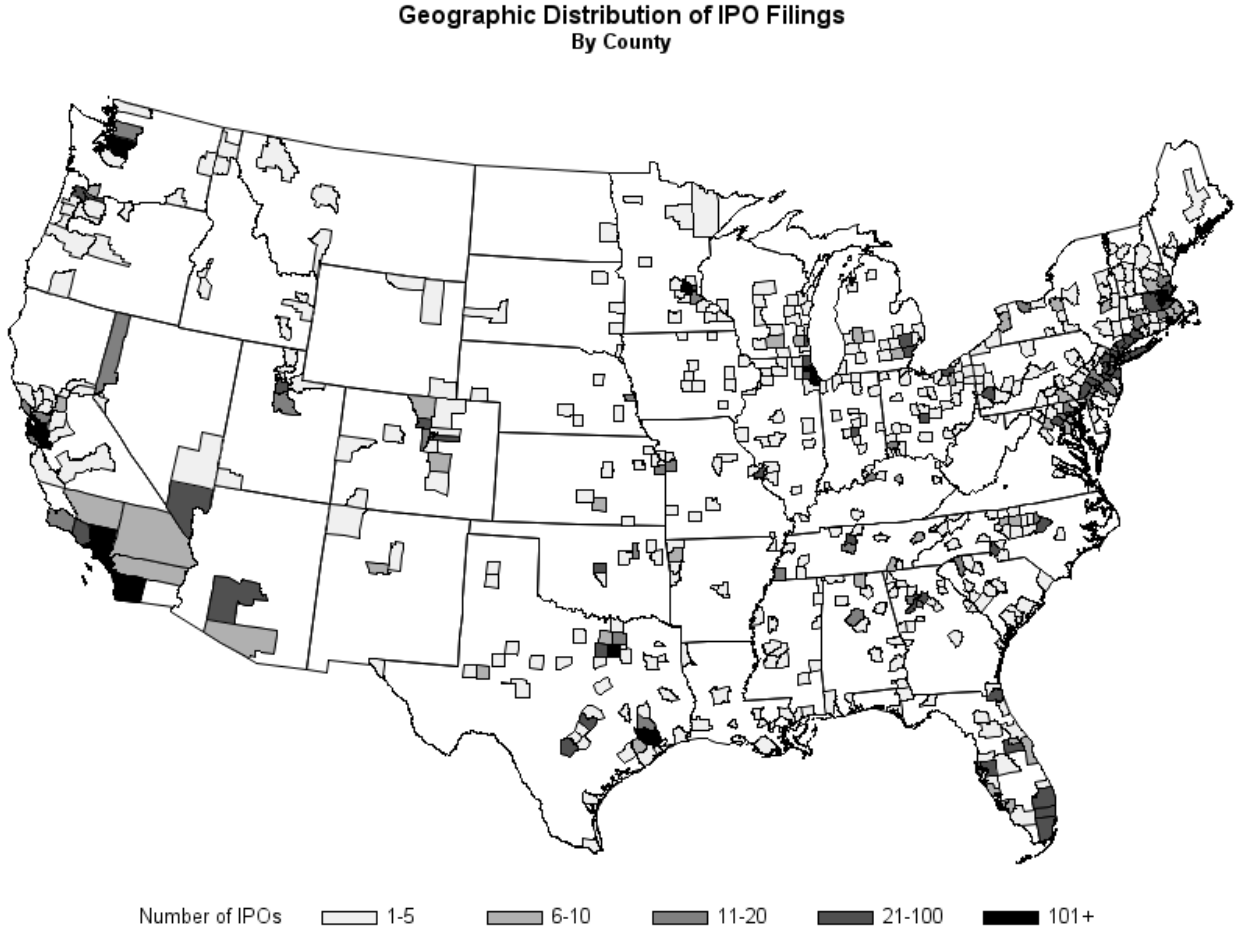
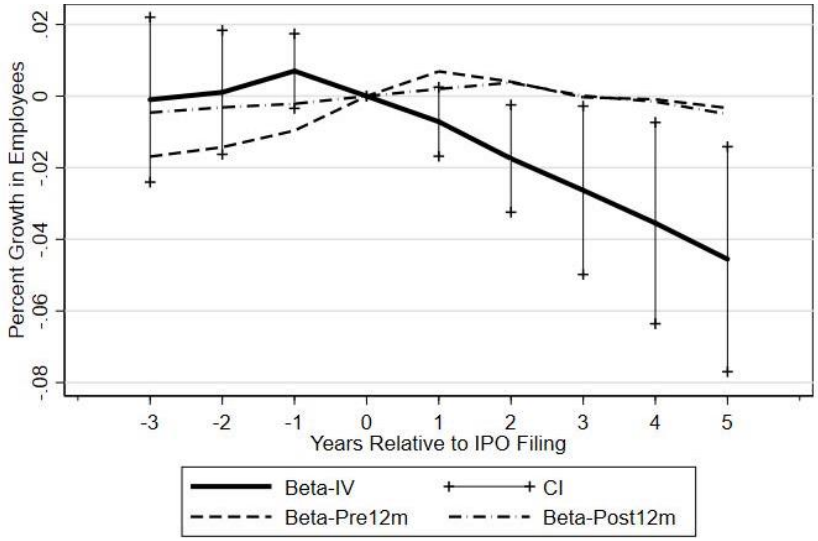


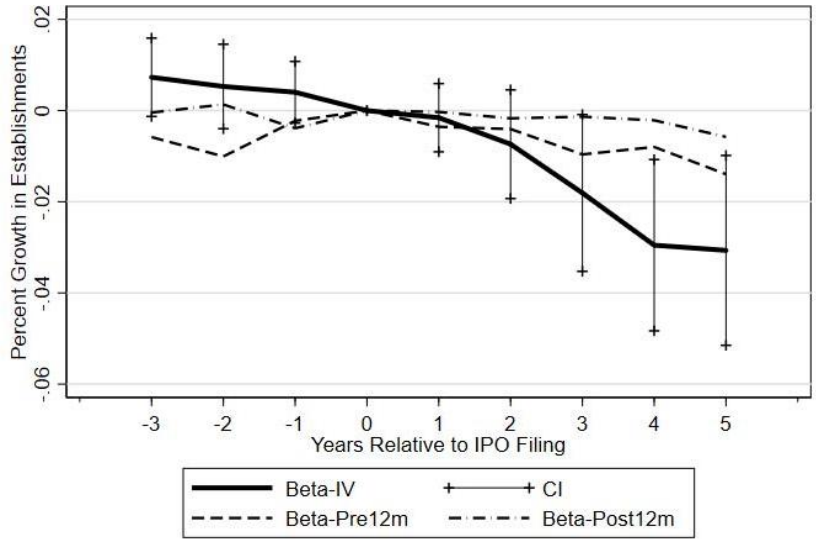
Figure B2: Placebo Test of Market Returns – County Growth relationship

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 IV 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 2 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.

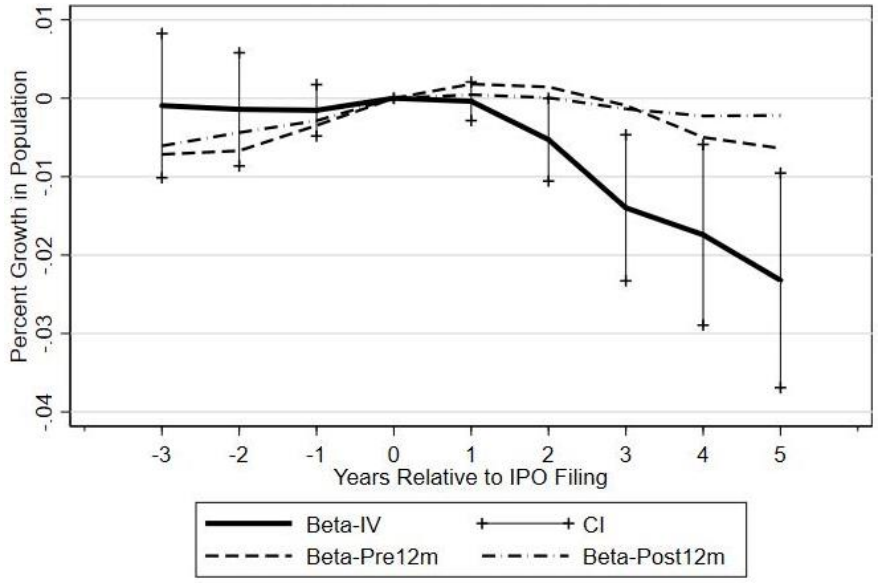
Panel A: Employees



Panel B: Establishments



Panel C: Population



Panel D: Personal Income

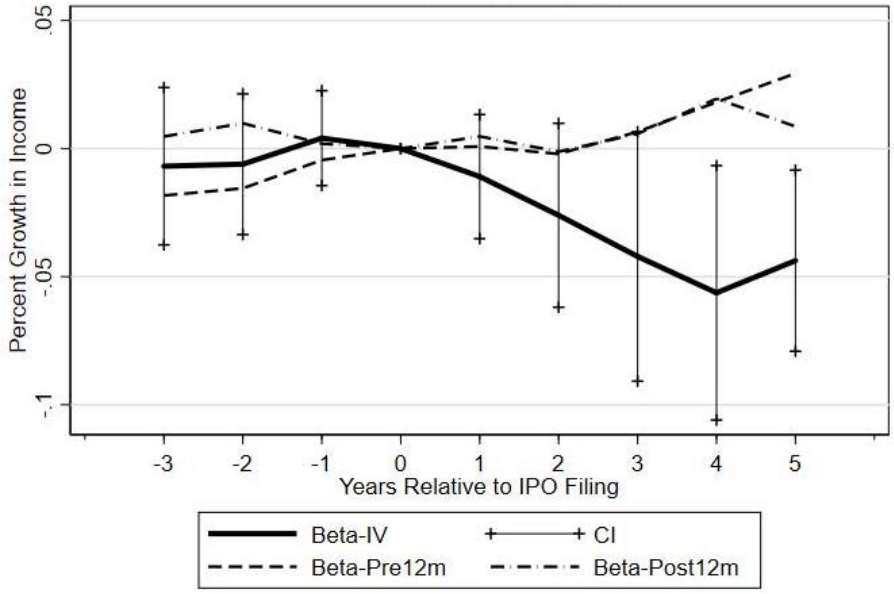
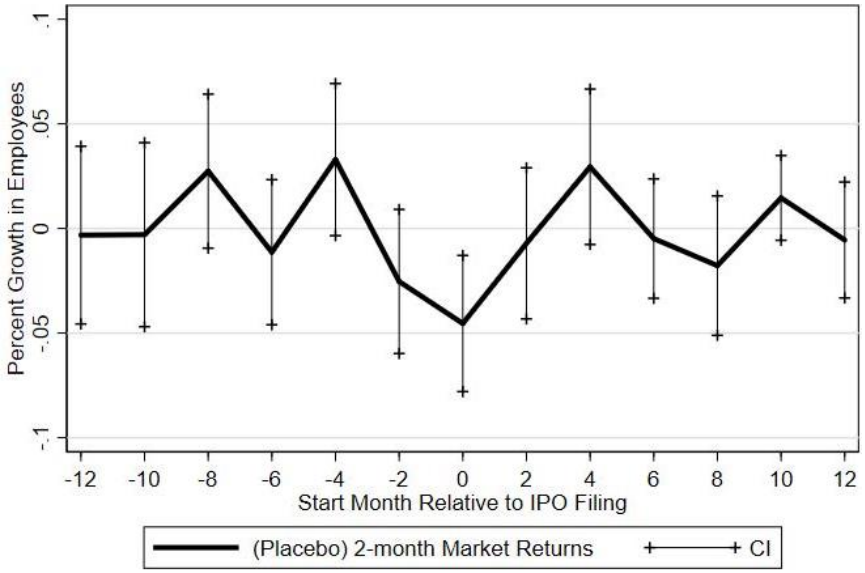


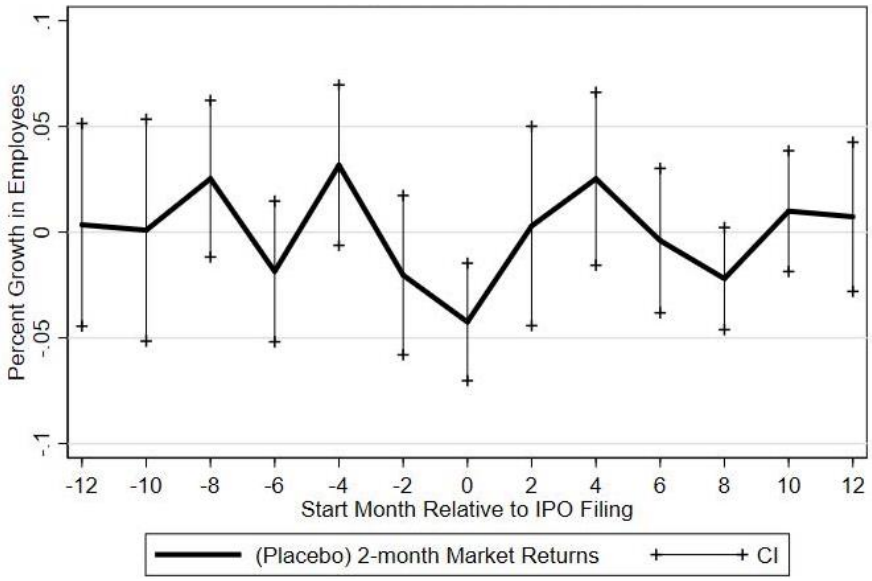
Figure B3: Placebo Market Returns – Plot of 13 different windows, Robust Samples

Panels A-C each plot coefficients from thirteen 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 2 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 +2 tick represents the same regression, but swapping market returns beginning two 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. 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 spinoffs, limited partnerships, and unit offerings; and in Panel C we exclude firms headquartered in California.

Panel A: Including years 1998-1999



Panel B: Excluding Spinoffs, LPs, and Unit Offerings



Panel C: Excluding California Firms

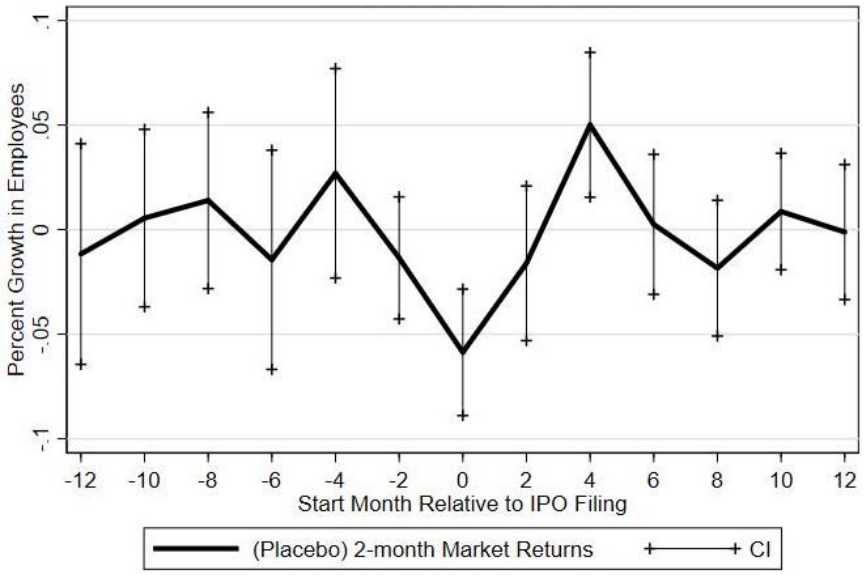
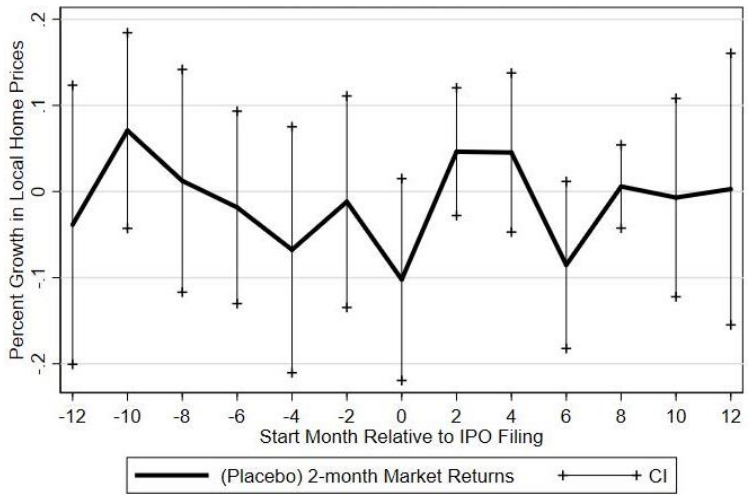


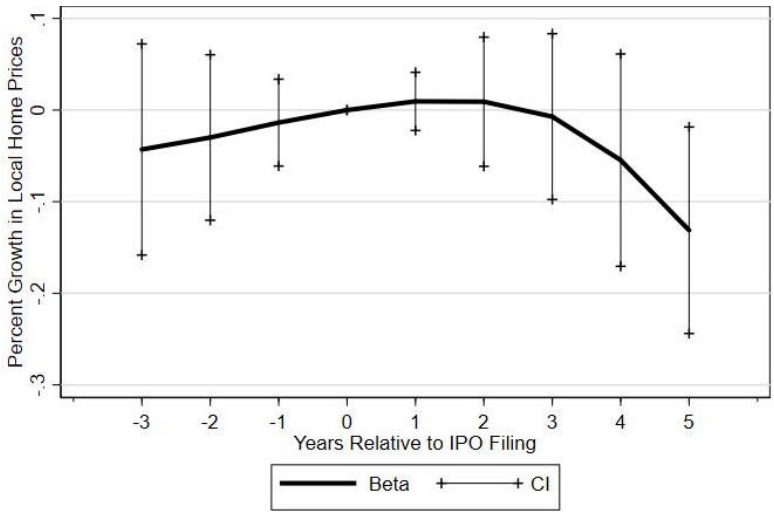
Figure B4: Growth in Local Home Prices and Building Permits

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 2 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 +2 tick represents the same regression, but swapping market returns beginning two 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 OLS 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 2 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 **Local Growth in Home Prices** – Plot of 13 different windows



Panel B: County Home Price Growth surrounding IPO Filings



Panel C: Placebo Market Returns predicting **Local Growth in Building Permits** – Plot of 13 different windows

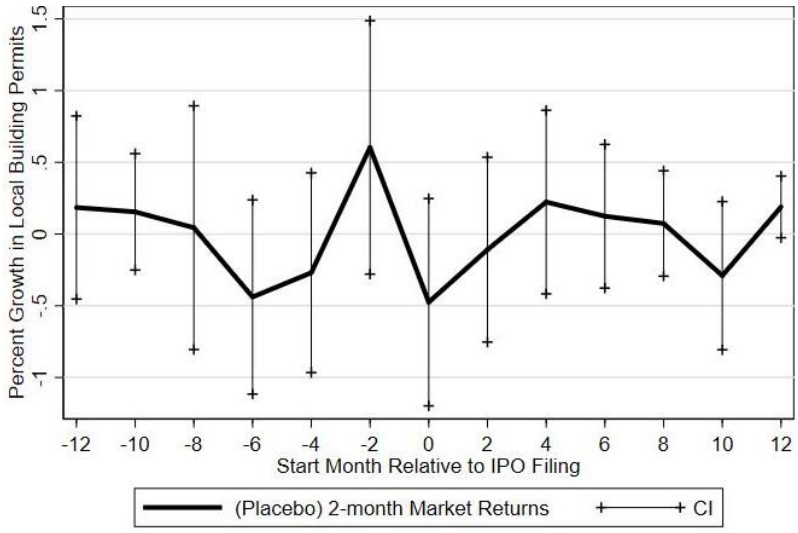


Table B1: First Stage Estimation of IPO Completion, Interaction Model

This table presents estimates for the two first stage regressions that correspond to the second stage regressions presented in Columns 3 and 4 of Table 4. Post-filing 2-month Market Returns is defined as the return on the CRSP value-weighted market index in the two-months following an IPO filing, and Large IPO is defined as IPOs with above-median filing proceeds (in 2011 dollars). All other variables are defined in Appendix A. Standard errors are clustered at the county and year levels. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Completed	(2) Completed × Large IPO	(3) Completed	(4) Completed × Large IPO
Post-Filing 2-month Market Returns	0.6880***	-0.9037*	0.6697***	-1.0062**
	(4.70)	(-2.06)	(4.64)	(-2.32)
Post-Filing 2-month Market Returns × Large IPO	-0.0045	2.7711***	0.0196	2.8663***
	(-0.02)	(3.57)	(0.11)	(3.80)
Pop Growth	-0.6801	-0.7661	-1.9026**	-1.7032**
	(-0.99)	(-1.24)	(-2.33)	(-2.22)
Employee Growth	0.2503	-0.3911	0.4220	-0.3225
	(0.66)	(-1.08)	(1.03)	(-0.64)
Income Growth	0.3013	0.3447	0.2457	0.4270
	(1.49)	(0.85)	(0.95)	(0.98)
Ln(Number of IPOs)	-0.0064	-0.0018	-0.0233	0.0022
	(-0.76)	(-0.23)	(-1.33)	(0.16)
Ln(IPO Size)	-0.0087	0.2169***	-0.0081	0.2178***
	(-0.62)	(8.31)	(-0.57)	(9.03)
Number Lead Managers	0.0592***	-0.0335*	0.0617***	-0.0258
	(4.10)	(-2.07)	(4.22)	(-1.64)
PE/VC Funding	0.0685***	0.0440**	0.0641***	0.0415**
	(6.01)	(2.33)	(5.17)	(2.32)
Underwriter Reputation	0.0071	0.0125*	0.0071	0.0129*
	(1.39)	(1.86)	(1.38)	(2.05)
County FE	No	No	Yes	Yes
State FE	Yes	Yes	No	No
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.142	0.412	0.141	0.411
Observations	6,451	6,451	6,201	6,201

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 the fitted value of IPO completion estimated from Table 3, 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 spinoffs, limited partnerships, and unit offerings; and in Panel C, we exclude IPO firms headquartered in California. The dependent variable in Panels A, C, and E is the annual geometric average growth rate in a county's total number of employees over the five years after an IPO filing; the dependent variable in Panels B, D, and F is the annual geometric average growth rate in the county's total number local establishments over this same period. Large IPO is defined as IPO filings with above-median filing proceeds (in 2011 dollars). 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 (Large IPOs)	(3) Employees	(4) Employees
Instrumented IPO	**-0.0044**	**-0.0112****	-0.0009	-0.0015
Completion	**(-1.40)**	**(-2.29)**	(-0.27)	(-0.52)
Instrumented IPO			**-0.0044*****	**-0.0036*****
Completion*Large IPO			**(-2.81)**	**(-4.46)**
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	No
County Fixed Effects	No	No	No	Yes
Adj. R-squared	0.616	0.551	0.619	0.720
Observations	6,451	3,202	6,451	6,451
Large IPO Level Effect	-2,933	-7,841	-3,482	-3,376

Panel B: Establishments – Including years 1998-1999

	(1) Establishments	(2) Establishments (Large IPOs)	(3) Establishments	(4) Establishments
Instrumented IPO	**-0.0032**	**-0.0075****	-0.0001	-0.0003
Completion	**(-1.29)**	**(-2.29)**	(-0.02)	(-0.11)
Instrumented IPO			**-0.0037****	**-0.0038*****
Completion*Large IPO			**(-2.17)**	**(-3.19)**
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	No
County Fixed Effects	No	No	No	Yes
Adj. R-squared	0.581	0.552	0.583	0.734
Observations	5,983	3,074	5,983	5,983
Large IPO Level Effect	-95	-226	-112	-123

Panel C: Employment – Without Spinoffs, LPs, and Unit offerings

	(1) Employees	(2) Employees (Large IPOs)	(3) Employees	(4) Employees
Instrumented IPO	**-0.0023**	**-0.0092****	0.0012	0.0005
Completion	**(-0.65)**	**(-2.46)**	(0.28)	(0.17)
Instrumented IPO			**-0.0044****	**-0.0042*****
Completion*Large IPO			**(-2.44)**	**(-3.83)**
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	No
County Fixed Effects	No	No	No	Yes
Adj. R-squared	0.626	0.569	0.626	0.725
Observations	5,600	2,703	5,600	5,600
Large IPO Level Effect	-1,516	-6,412	-2,183	-2,485

Panel D: Establishments – Without Spinoffs, LPs, and Unit offerings

	(1) Establishments	(2) Establishments (Large IPOs)	(3) Establishments	(4) Establishments
Instrumented IPO	**-0.0024**	**-0.0071****	0.0013	0.0002
Completion	**(-0.83)**	**(-2.51)**	(0.31)	(0.07)
Instrumented IPO			**-0.0044****	**-0.0039****
Completion*Large IPO			**(-2.01)**	**(-2.41)**
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	No
County Fixed Effects	No	No	No	Yes
Adj. R-squared	0.587	0.553	0.587	0.740
Observations	5,197	2,611	5,197	5,197
Large IPO Level Effect	-73	-217	-93	-110

Panel E: Employment – Without California Firms

	(1) Employees	(2) Employees (Large IPOs)	(3) Employees	(4) Employees
Instrumented IPO	**-0.0082***	**-0.0174*****	-0.0053	-0.0041
Completion	**(-1.69)**	**(-2.88)**	(-0.99)	(-0.76)
Instrumented IPO			**-0.0038****	**-0.0033****
Completion*Large IPO			**(-1.99)**	**(-2.54)**
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	No
County Fixed Effects	No	No	No	Yes
Adj. R-squared	0.596	0.419	0.600	0.720
Observations	4,935	2,449	4,935	4,935
Large IPO Level Effect	-4,352	-9,945	-4,851	-3,933

Panel F: Establishments – Without California Firms

	(1) Establishments	(2) Establishments (Large IPOs)	(3) Establishments	(4) Establishments
Instrumented IPO	**-0.0072**	**-0.0115*****	-0.0047	-0.0058
Completion	**(-1.63)**	**(-2.85)**	(-0.87)	(-1.18)
Instrumented IPO			**-0.0031**	**-0.0030****
Completion*Large IPO			**(-1.54)**	**(-2.11)**
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	No
County Fixed Effects	No	No	No	Yes
Adj. R-squared	0.615	0.546	0.621	0.762
Observations	4,558	2,349	4,558	4,558
Large IPO Level Effect	-176	-287	-191	-213

Table B3: 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 panels show second-stage 2SLS estimates. In Panel A, 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. In Panel B, the dependent variable is the annual geometric average growth rate in a county's total number of establishments over the same time period. In Column 2, the sample is restricted to large IPOs, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars). The sample period is between 1986 and 2011. 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: Employee Growth

	(1) Employees	(2) Employees (Large IPOs)	(3) Employees	(4) Employees
Instrumented IPO Completion	-0.0018	-0.0063**	0.0021	0.0008
	(-0.78)	(-2.15)	(0.73)	(0.39)
Instrumented IPO Completion × Large IPO			-0.0050**	-0.0037**
			(-2.33)	(-2.79)
Pop Growth	0.5385***	0.4973***	0.5382***	0.2188**
	(6.87)	(5.38)	(6.88)	(2.81)
Employee Growth	0.1384***	0.1350***	0.1349***	0.0492*
	(5.59)	(4.18)	(5.68)	(1.94)
Income Growth	-0.0727	-0.0794	-0.0719	-0.0767*
	(-1.58)	(-1.70)	(-1.60)	(-1.80)
Ln(Number of IPOs)	-0.0011	-0.0021**	-0.0011	-0.0003
	(-1.26)	(-2.59)	(-1.29)	(-0.21)
Ln(IPO Size)	-0.0001	0.0003	0.0011*	0.0010**
	(-0.46)	(0.71)	(2.01)	(2.67)
Number Lead Managers	0.0003	0.0003	-0.0001	-0.0001
	(0.72)	(0.87)	(-0.32)	(-0.23)
PE/VC Funding	0.0009*	0.0011	0.0008*	-0.0002
	(2.02)	(1.67)	(1.99)	(-0.44)
Underwriter Reputation	0.0000	-0.0000	0.0001	0.0001
	(0.26)	(-0.10)	(0.85)	(0.66)
County FE	No	No	No	Yes
State FE	Yes	Yes	Yes	No
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.297	0.222	0.293	0.055
Observations	6,451	3,202	6,451	6,201

Panel B: Establishment Growth

	(1) Establishments	(2) Establishments (Large IPOs)	(3) Establishments	(4) Establishments
Instrumented IPO	-0.0023	-0.0050**	0.0016	0.0017
Completion	(-1.00)	(-2.56)	(0.42)	(0.65)
Instrumented IPO			-0.0047*	-0.0042**
Completion × Large IPO			(-1.96)	(-2.56)
Establishment Growth	0.0978***	0.1224***	0.1001***	0.0260
	(3.64)	(4.08)	(3.76)	(0.88)
Pop Growth	0.4782***	0.4482***	0.4751***	0.1238
	(5.63)	(6.23)	(5.51)	(1.46)
Employee Growth	0.0873***	0.0790**	0.0845***	0.0582**
	(3.45)	(2.67)	(3.53)	(2.77)
Income Growth	-0.0290	-0.0413	-0.0283	-0.0237
	(-0.98)	(-1.32)	(-0.99)	(-0.88)
Ln(Number of IPOs)	-0.0004	-0.0011**	-0.0004	-0.0003
	(-0.70)	(-2.25)	(-0.72)	(-0.29)
Ln(IPO Size)	-0.0000	0.0007**	0.0011*	0.0012**
	(-0.11)	(2.35)	(1.93)	(2.61)
Number Lead Managers	0.0004	0.0001	-0.0001	-0.0001
	(0.91)	(0.47)	(-0.11)	(-0.35)
PE/VC Funding	0.0007**	0.0012**	0.0007**	0.0000
	(2.33)	(2.25)	(2.20)	(0.16)
Underwriter Reputation	-0.0000	-0.0000	0.0000	0.0000
	(-0.48)	(-0.27)	(0.35)	(0.39)
County FE	No	No	No	Yes
State FE	Yes	Yes	Yes	No
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.338	0.302	0.334	0.023
Observations	5,983	3,074	5,983	5,735

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Figure 2: Geographical dispersion of IPO sample

This figure plots the geographical distribution of the 3,202 large IPO filings in our main sample, across U.S. counties. The sample runs from 1986 through 2011, 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.

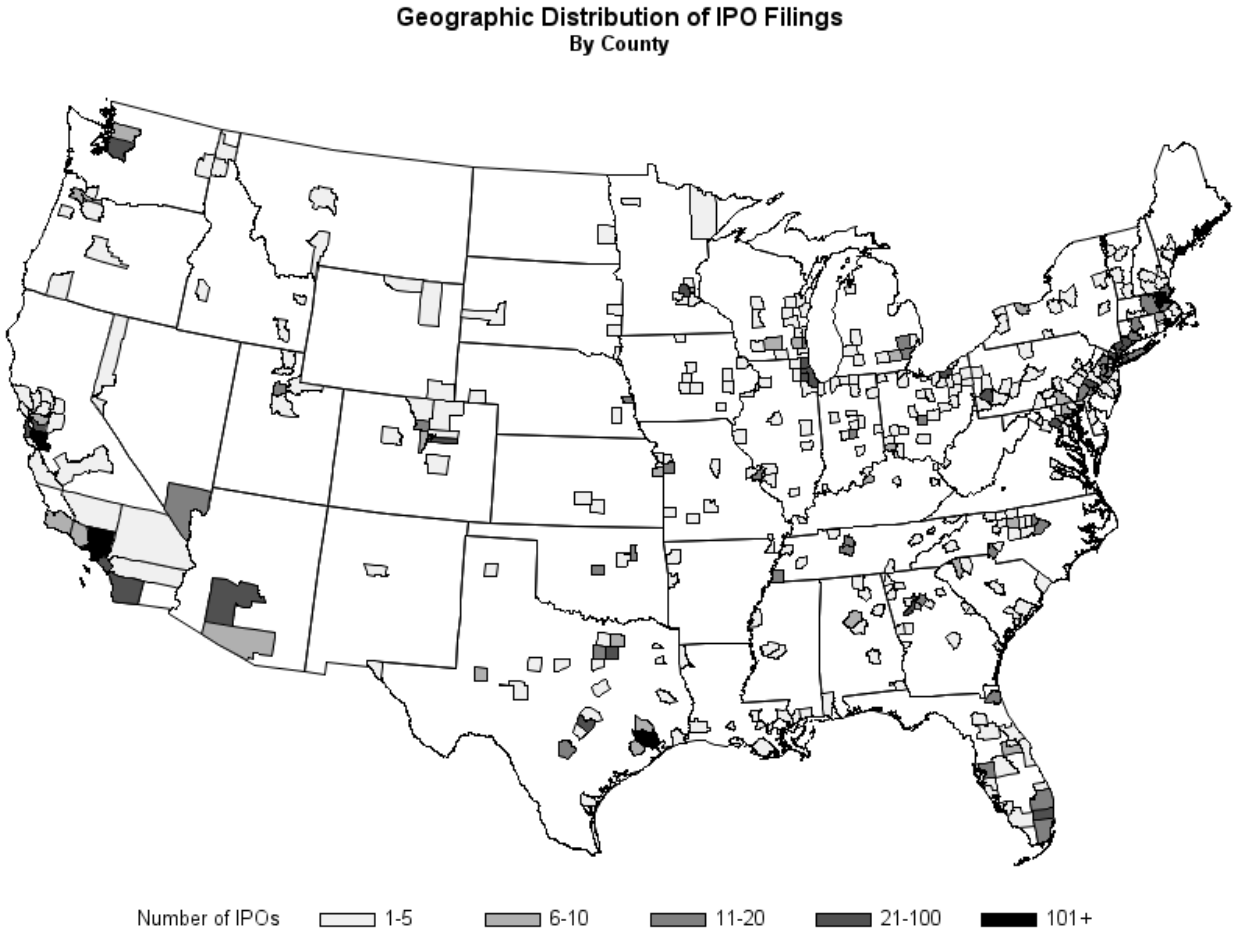
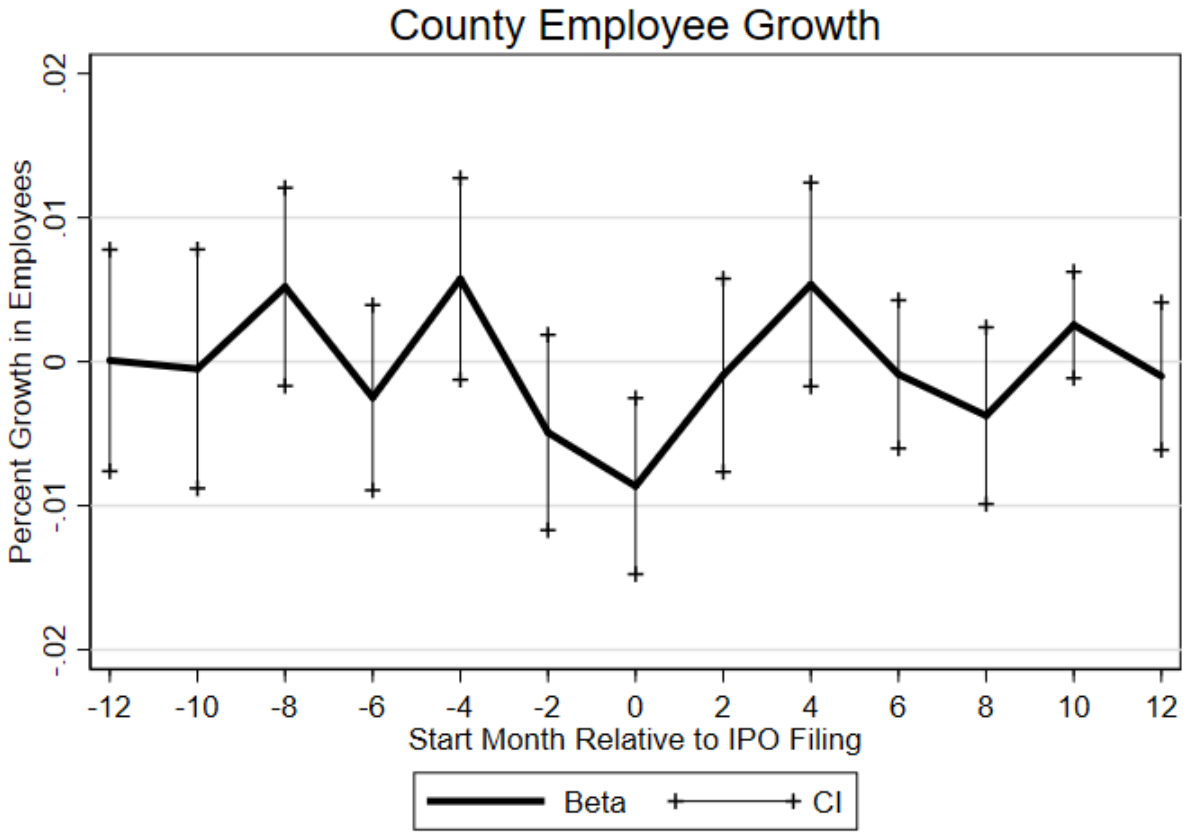


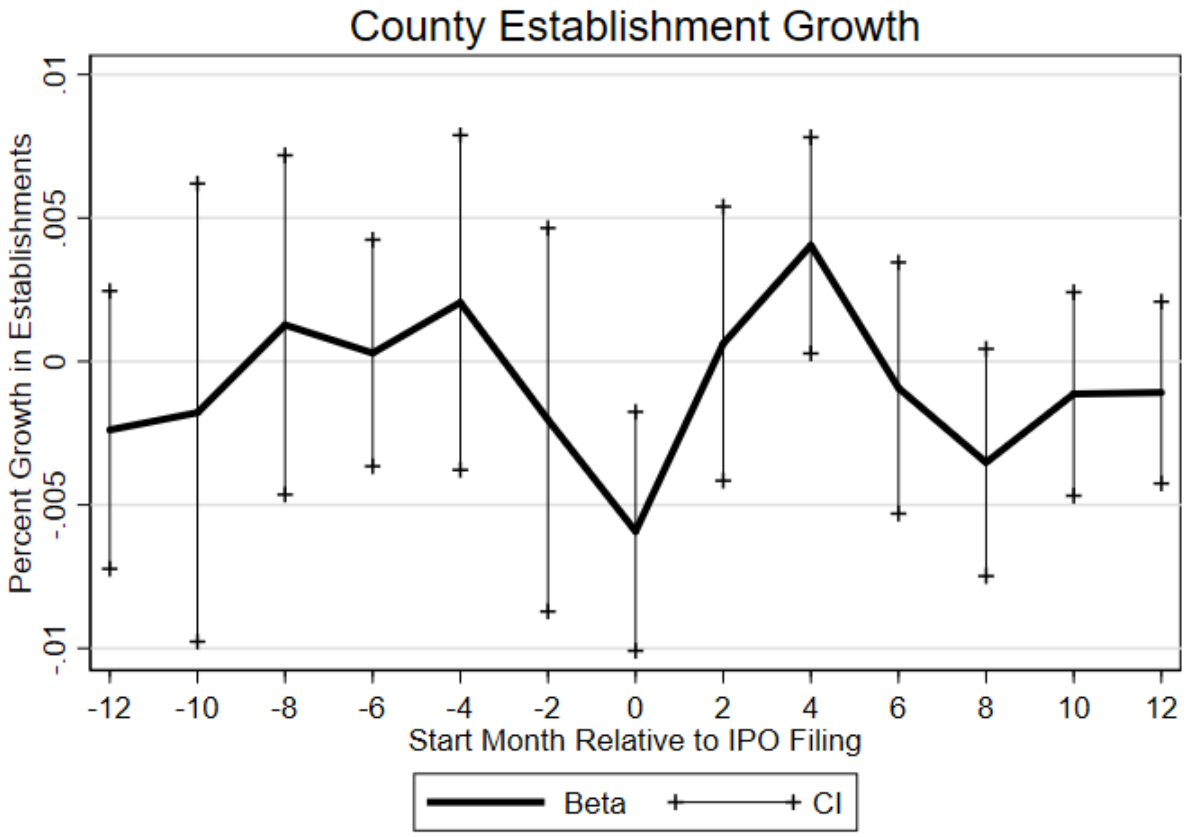
Figure 3: Placebo Robustness Test of Market Returns – Plot of 13 different windows

Panels A-D each plot beta coefficients from thirteen 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 1 of Table 4. Each of the thirteen 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 +2 tick represents the same regression, but swapping market returns beginning two 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. The sample for each regression is restricted to large IPOs between 1986 and 2011, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars).

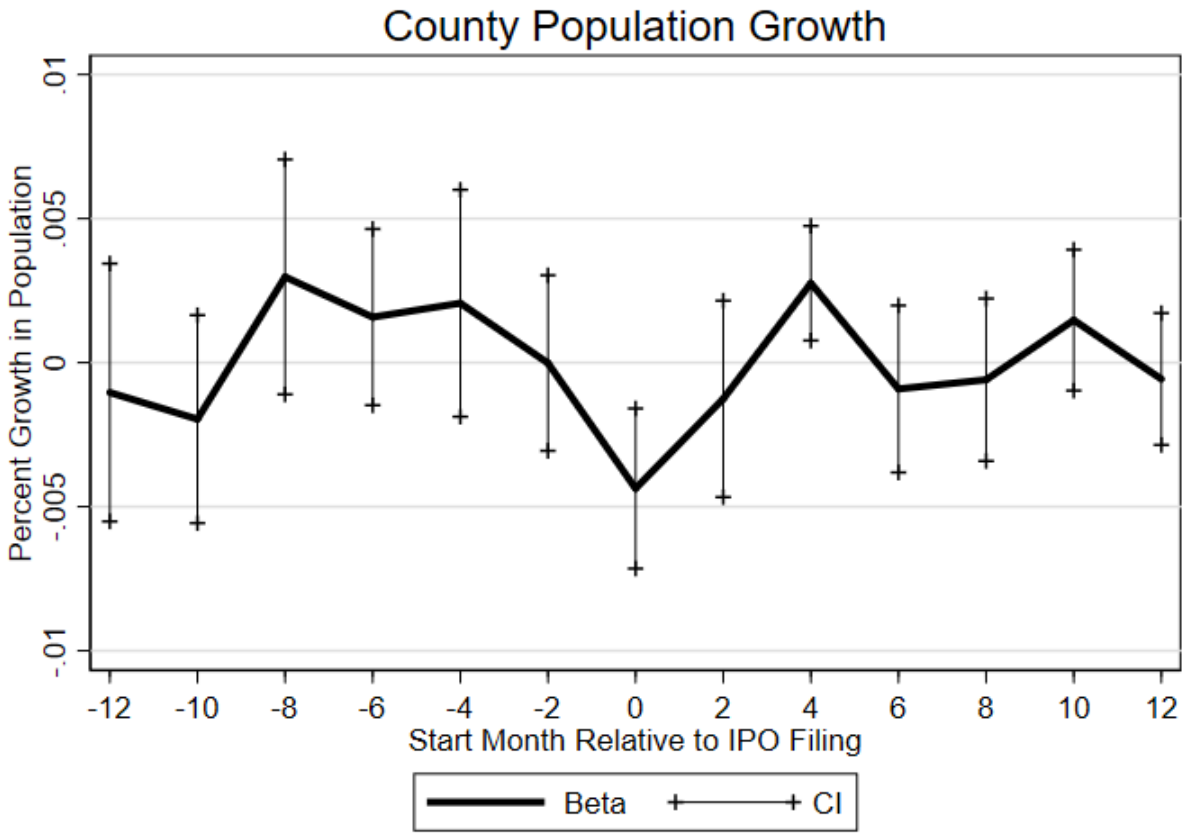
Panel A: Two-month Market Returns and Future Employment Growth



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

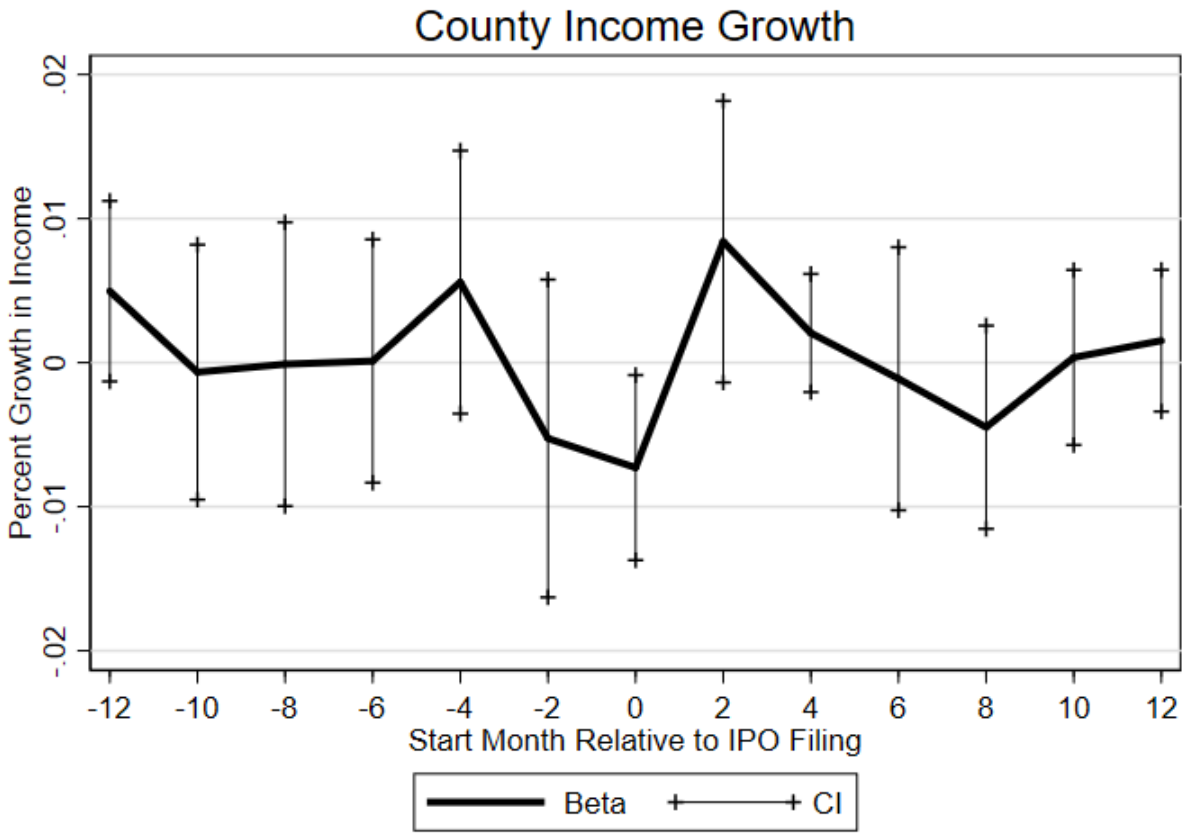
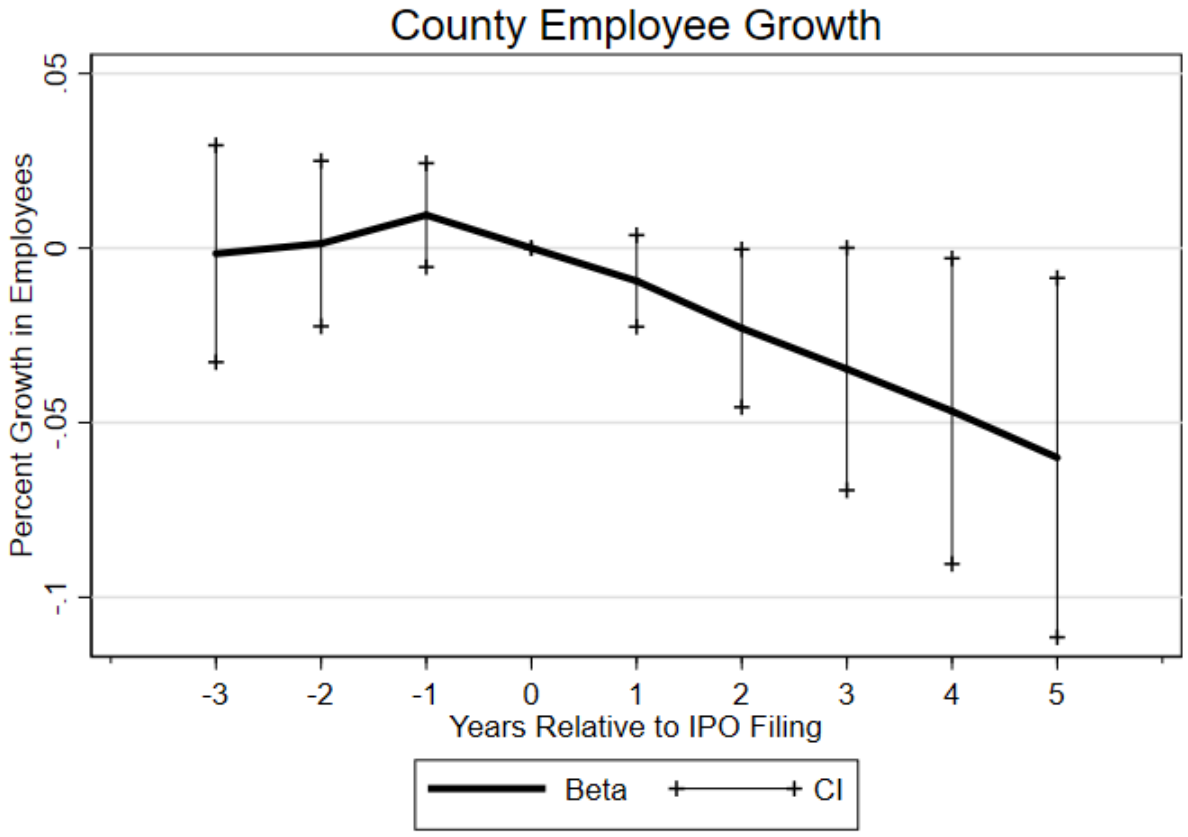


Figure 4: County Employment & Establishment Growth surrounding IPO Filings

This figure plots the evolution of the number of employees 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 4 of Table 4), where the dependent variable measures cumulative employment growth (in Panel A) or cumulative establishment growth (in Panel B) 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 4 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 2011, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars).

Panel A: Employment



Panel B: Establishments

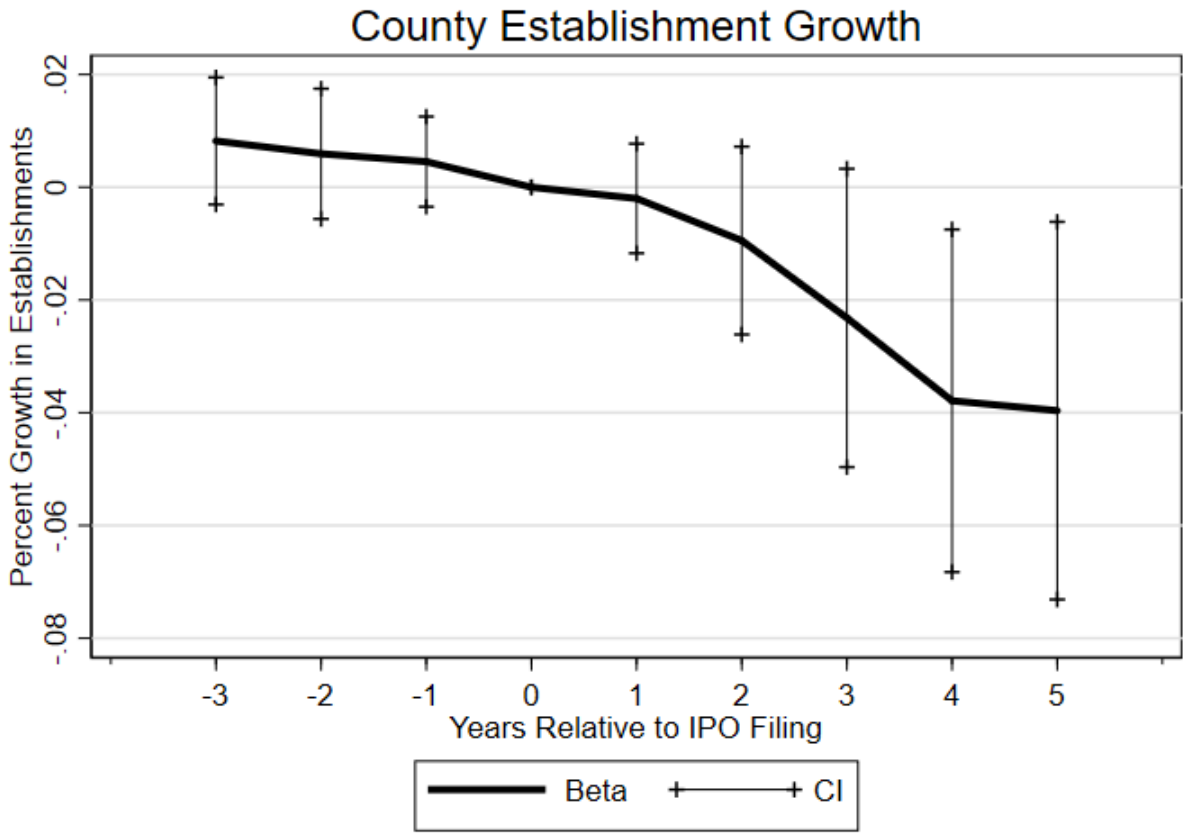


Figure 5: Industrial Decomposition of County Employment Growth surrounding IPO Filings

This figure plots the industrial decomposition of the evolution of employment in counties with completed IPOs, compared to counties with withdrawn IPOs. The figure covers three years before IPO filings until eight years after. 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 4 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 industrial 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 4 of Table 4. Vertical lines at each point represent 95% confidence intervals for the instrumented IPO completion coefficient in regressions for employment in the tradable sector. The vertical line at 0 represents the IPO filing year.

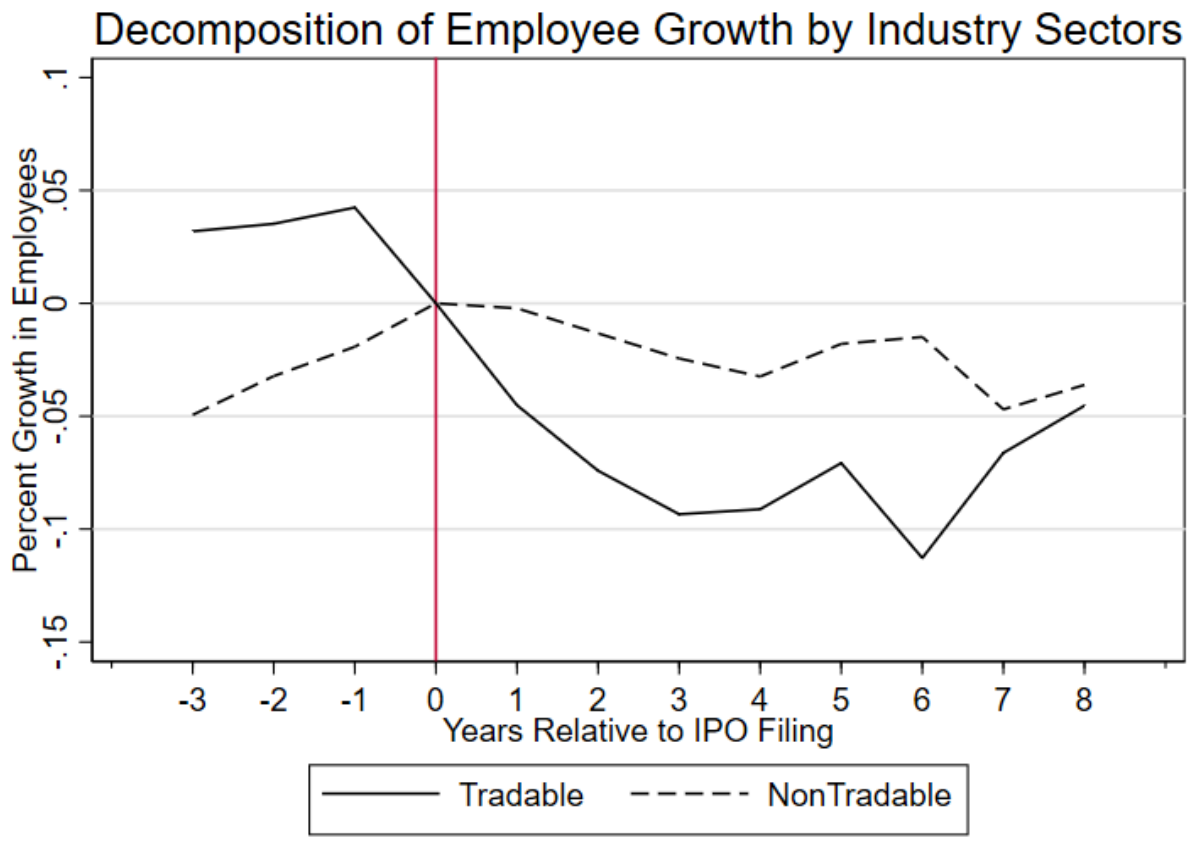
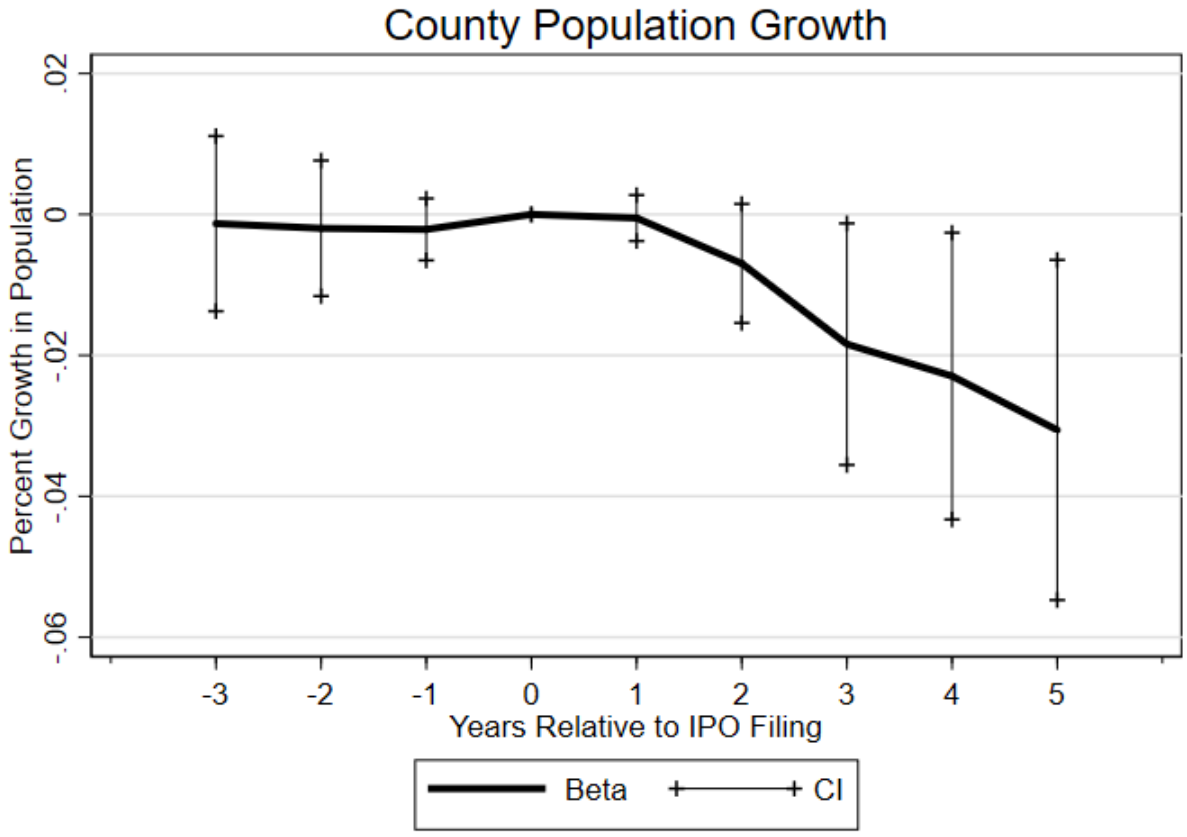


Figure 6: County Economic Growth surrounding IPO Filings

This figure plots the evolution of the population (Panel A) and per capita income (Panel B) 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 4 of Table 4), where the dependent variable measures cumulative population (panel A) or per capita income (Panel B) 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 4 of Table 3, 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 2011, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars).

Panel A: Population



Panel B: Personal Income

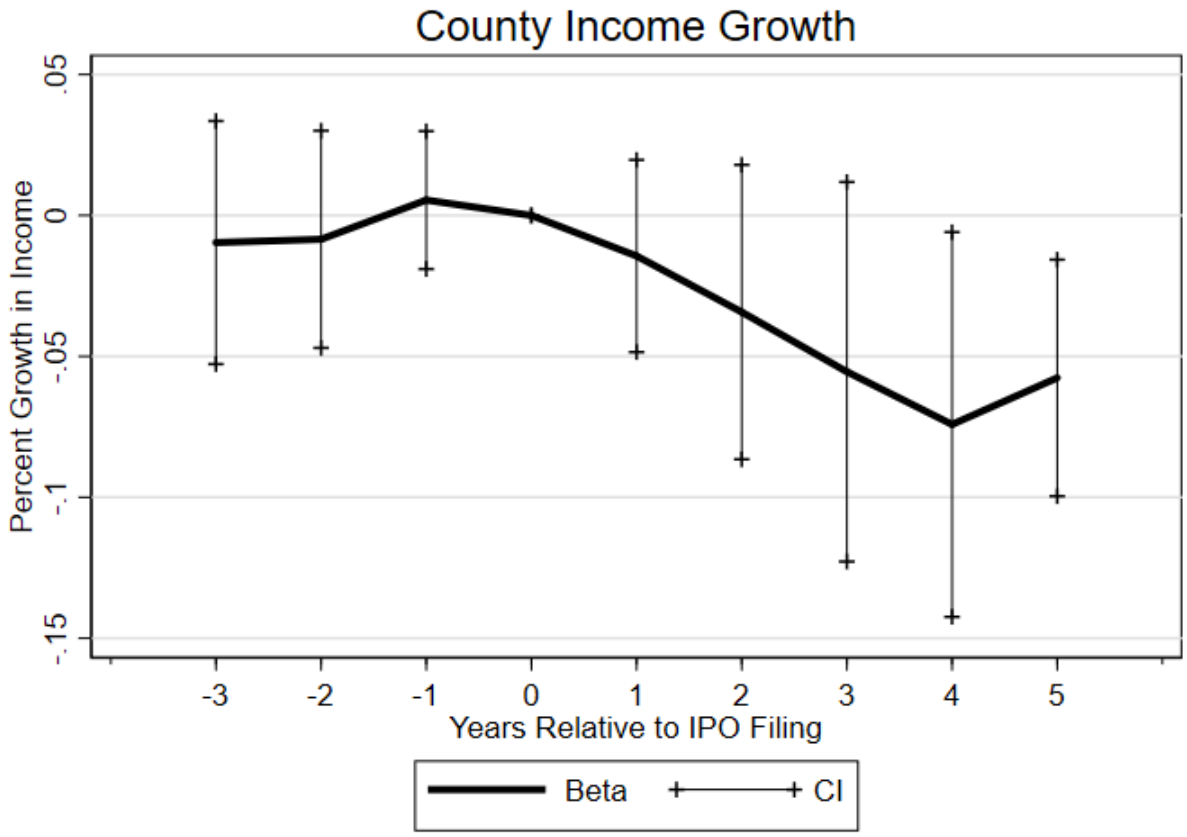


Table 1: Descriptive Statistics

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 2011. 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 2). *, **, 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	87.70	82.69	106.37	152.33	157.66
PE/VC Funding	0.38	0.39	0.38	0.52	0.49
Underwriter Reputation	6.66	6.64	6.74	8.38	8.18
Number Lead Managers	1.20	1.18	1.30	1.37	1.46
N	6,451	5,086	1,365	2,353	849

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

Economic Variables	IPO Filings	No IPO Filings	Difference
Population	577,565	62,448	515,117***
Employees	369,041	33,809	335,232***
Real Income per Capita	40,193	29,831	10,362***
Lagged Population Growth	1.27%	0.56%	0.72%***
Lagged Employee Growth	1.87%	1.06%	0.82%***
Lagged Real Income Growth	4.44%	4.49%	-0.05%
Population Growth 5yr	6.44%	2.91%	3.53%***
Employee Growth 5yr	9.54%	5.18%	4.36%***
Real Income Growth 5yr	23.43%	22.59%	0.84%***
N	2,511	78,344	

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

Economic Variables	Completed IPOs	Withdrawn IPOs	Difference
Population	504,963	518,843	-13,886
Employees	316,874	334,545	-17,671
Real Income per Capita	38,543	40,822	-2,279***
Lagged Population Growth	1.33%	1.21%	0.11%
Lagged Employee Growth	2.07%	1.46%	0.61%***
Lagged Real Income Growth	4.51%	3.76%	0.75%***
Population Growth 5yr	6.80%	5.72%	1.08%***
Employee Growth 5yr	10.61%	6.97%	3.64%***
Real Income Growth 5yr	24.34%	19.10%	5.24%***
N	1,618	372	

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 in Columns 1 and 3 is the annual geometric average growth rate in a county's total number of employees over the five years following an IPO filing. The dependent variable in Columns 2 and 4 is the annual geometric average growth rate in a county's establishments over the five years following an IPO filing. The *Large IPO* interaction variable in Columns 3 and 4 is an indicator variable for IPOs with above-median filing proceeds (in 2011 dollars). 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) Establishments	(3) Employees	(4) Establishments
IPO Completion	0.0010**	0.0007**	0.0016***	0.0014***
	(2.50)	(2.20)	(4.17)	(3.85)
IPO Completion × Large IPO			-0.0011*	-0.0012***
			(-2.01)	(-3.80)
Pop Growth	0.5403***	0.5195***	0.5401***	0.5193***
	(6.89)	(5.99)	(6.89)	(6.00)
Employee Growth	0.1378***	0.1292***	0.1370***	0.1284***
	(5.56)	(5.19)	(5.51)	(5.18)
Income Growth	-0.0735	-0.0262	-0.0732	-0.0260
	(-1.59)	(-0.87)	(-1.60)	(-0.87)
Ln(Number of IPOs)	-0.0011	-0.0008	-0.0011	-0.0008
	(-1.25)	(-1.24)	(-1.25)	(-1.25)
Ln(IPO Size)	-0.0001	-0.0001	0.0002	0.0002
	(-0.32)	(-0.66)	(0.96)	(0.94)
Number Lead Managers	0.0001	0.0002	0.0000	0.0002
	(0.33)	(0.83)	(0.10)	(0.55)
PE/VC Funding	0.0007	0.0006*	0.0007	0.0006*
	(1.52)	(1.76)	(1.56)	(1.81)
Underwriter Reputation	0.0000	-0.0000	0.0000	-0.0000
	(0.04)	(-0.28)	(0.17)	(-0.11)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.630	0.573	0.630	0.573
Observations	6,451	6,430	6,451	6,430

Table 3: First Stage Estimation of IPO Completion

This table presents estimates for our first stage regression predicting IPO completion. Columns 1 and 2 present estimates from regressions using our full sample of IPO filings, while Columns 3 and 4 present estimates from regressions using only large IPOs, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars). 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 2011. 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 (Large IPOs)	(4) Completed (Large IPOs)
Post-Filing 2-month Market Returns	0.6857***	0.6800***	0.7664***	0.7533***
	(7.27)	(6.82)	(4.87)	(5.01)
Pop Growth	-0.6804	-1.9004**	-0.1924	-2.6921*
	(-0.98)	(-2.33)	(-0.18)	(-1.72)
Employee Growth	0.2505	0.4213	-0.1360	0.1963
	(0.65)	(1.03)	(-0.33)	(0.30)
Income Growth	0.3013	0.2459	0.2478	0.2165
	(1.48)	(0.95)	(0.91)	(0.63)
Ln(Number of IPOs)	-0.0064	-0.0233	0.0125	-0.0127
	(-0.76)	(-1.33)	(0.92)	(-0.57)
Ln(IPO Size)	-0.0087	-0.0080	0.0093	0.0179
	(-0.63)	(-0.56)	(0.34)	(0.63)
Number Lead Managers	0.0592***	0.0617***	0.0456**	0.0507***
	(4.10)	(4.21)	(2.50)	(3.05)
PE/VC Funding	0.0684***	0.0642***	0.1059***	0.0979***
	(6.01)	(5.19)	(6.27)	(5.43)
Underwriter Reputation	0.0071	0.0072	0.0174*	0.0164*
	(1.39)	(1.38)	(1.83)	(1.72)
County FE	No	Yes	No	Yes
State FE	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.142	0.141	0.166	0.174
Observations	6,451	6,201	3,202	3,014

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. In Panel A, 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. In Panel B, the dependent variable is the annual geometric average growth rate in a county's total number of establishments over the same time period. In Column 2 the sample is restricted to large IPOs, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars). The sample period is between 1986 and 2011. 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: Employee Growth

	(1) Employees	(2) Employees (Large IPOs)	(3) Employees	(4) Employees
Instrumented IPO Completion	-0.0045	-0.0113**	-0.0011	-0.0015
	(-1.36)	(-2.22)	(-0.31)	(-0.50)
Instrumented IPO Completion × Large IPO			-0.0044**	-0.0038***
			(-2.73)	(-4.28)
Pop Growth	0.5368***	0.4972***	0.5364***	0.2143**
	(6.81)	(5.30)	(6.82)	(2.71)
Employee Growth	0.1390***	0.1338***	0.1359***	0.0500*
	(5.59)	(4.15)	(5.65)	(1.96)
Income Growth	-0.0719	-0.0781	-0.0712	-0.0761*
	(-1.58)	(-1.70)	(-1.60)	(-1.80)
Ln(Number of IPOs)	-0.0011	-0.0020**	-0.0011	-0.0003
	(-1.27)	(-2.50)	(-1.29)	(-0.26)
Ln(IPO Size)	-0.0001	0.0003	0.0009**	0.0010***
	(-0.60)	(0.66)	(2.11)	(3.12)
Number Lead Managers	0.0004	0.0005	0.0001	0.0000
	(1.04)	(1.28)	(0.15)	(0.11)
PE/VC Funding	0.0011**	0.0016*	0.0010**	-0.0000
	(2.26)	(1.87)	(2.28)	(-0.03)
Underwriter Reputation	0.0000	0.0001	0.0001	0.0001
	(0.45)	(0.33)	(0.99)	(0.89)
County FE	No	No	No	Yes
State FE	Yes	Yes	Yes	No
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.276	0.109	0.279	0.030
Observations	6,451	3,202	6,451	6,201

Panel B: Establishment Growth

	(1) Establishments	(2) Establishments (Large IPOs)	(3) Establishments	(4) Establishments
Instrumented IPO	-0.0032	-0.0076**	0.0002	-0.0001
Completion	(-1.25)	(-2.22)	(0.06)	(-0.05)
Instrumented IPO			-0.0042**	-0.0042***
Completion × Large IPO			(-2.45)	(-3.25)
Establishment Growth	0.0972***	0.1217***	0.0994***	0.0249
	(3.62)	(4.05)	(3.71)	(0.85)
Pop Growth	0.4782***	0.4493***	0.4754***	0.1217
	(5.62)	(6.17)	(5.54)	(1.42)
Employee Growth	0.0874***	0.0783**	0.0850***	0.0587**
	(3.44)	(2.66)	(3.47)	(2.70)
Income Growth	-0.0288	-0.0407	-0.0281	-0.0233
	(-0.98)	(-1.33)	(-0.99)	(-0.87)
Ln(Number of IPOs)	-0.0004	-0.0011**	-0.0004	-0.0003
	(-0.70)	(-2.15)	(-0.72)	(-0.33)
Ln(IPO Size)	-0.0000	0.0007*	0.0010**	0.0011***
	(-0.17)	(2.05)	(2.26)	(3.12)
Number Lead	0.0004	0.0003	0.0000	-0.0000
Managers	(1.03)	(0.78)	(0.11)	(-0.02)
PE/VC Funding	0.0008**	0.0014**	0.0007**	0.0002
	(2.60)	(2.27)	(2.53)	(0.49)
Underwriter	-0.0000	-0.0000	0.0000	0.0000
Reputation	(-0.40)	(-0.01)	(0.37)	(0.53)
County FE	No	No	No	Yes
State FE	Yes	Yes	Yes	No
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.330	0.248	0.331	0.006
Observations	5,983	3,074	5,983	5,735

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, instrumented with the CRSP value-weighted market return in the two-months following an IPO filing, and its interaction with an indicator for large IPO, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars). The sample in Columns 1 and 2 of each panel includes only IPOs with below median employee density (Panels A and C) or establishment density (Panels B and D), and the sample in Columns 3 and 4 of each panel includes only IPOs with above median employee density (Panels A and C) or establishment density (Panels B and D). Employee Density is measured as the lagged number employees in the county, scaled by the square mileage of the county; Establishment Density is measured as the lagged number of establishments in the county, scaled by the square mileage of the county. The dependent variable in Panels A and B is the annual geometric average growth rate in total employees in counties with an IPO filing over the five years following an IPO filing. The dependent variable in Panels C and D is the annual geometric average growth rate in total establishments in counties with IPO over the five years following an IPO filing. We winsorize all dependent variables at the extreme 1%. The sample includes IPO filings between 1986 and 2011. 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: Local Employee Growth split by Pre-IPO County-level Employee Density

	(1) Below Median Employee Density	(2) Below Median Employee Density	(3) Above Median Employee Density	(4) Above Median Employee Density
Instrumented IPO Completion	-0.0012	0.0007	-0.0047	-0.0084
	(-0.26)	(0.17)	(-0.90)	(-1.33)
Instrumented IPO Completion × Large IPO	-0.0063**	-0.0039**	-0.0032	-0.0028
	(-2.72)	(-2.36)	(-1.21)	(-1.10)
County FE	No	Yes	No	Yes
State FE	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.206	-0.003	0.076	-0.281
Observations	3,226	2,977	3,220	3,217

Panel B: Local Employee Growth split by Pre-IPO County-level Establishment Density

	(1) Below Median Establishment Density	(2) Below Median Establishment Density	(3) Above Median Establishment Density	(4) Above Median Establishment Density
Instrumented IPO Completion	0.0005	0.0011	-0.0082	-0.0112
	(0.13)	(0.28)	(-1.48)	(-1.47)
Instrumented IPO Completion × Large IPO	-0.0059**	-0.0042**	-0.0014	-0.0002
	(-2.26)	(-2.09)	(-0.62)	(-0.07)
County FE	No	Yes	No	Yes
State FE	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.255	0.044	-0.036	-0.359
Observations	3,307	3,058	3,139	3,135

Panel C: Local Establishment Growth split by Pre-IPO County-level Employee Density

	(1) Below Median Employee Density	(2) Below Median Employee Density	(3) Above Median Employee Density	(4) Above Median Employee Density
Instrumented IPO	0.0019	0.0036	-0.0048	-0.0072
Completion	(0.44)	(0.81)	(-0.95)	(-1.58)
Instrumented IPO	-0.0039*	-0.0045**	-0.0037	-0.0028
Completion × Large IPO	(-1.88)	(-2.54)	(-1.35)	(-1.45)
County FE	No	Yes	No	Yes
State FE	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.340	0.025	-0.132	-0.456
Observations	2,995	2,747	2,983	2,982

Panel D: Local Establishment Growth split by Pre-IPO County-level Establishment Density

	(1) Below Median Establishment Density	(2) Below Median Establishment Density	(3) Above Median Establishment Density	(4) Above Median Establishment Density
Instrumented IPO	0.0040	0.0040	-0.0053	-0.0082
Completion	(1.10)	(0.99)	(-1.11)	(-1.34)
Instrumented IPO	-0.0041*	-0.0047**	-0.0019	-0.0010
Completion × Large IPO	(-1.96)	(-2.51)	(-0.89)	(-0.50)
County FE	No	Yes	No	Yes
State FE	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.334	0.012	-0.086	-0.445
Observations	3,023	2,773	2,957	2,954

Table 6: IPOs and Local 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, and its interaction with an indicator for large IPO, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars). The dependent variable in Column 1 is the annual geometric average growth rate in a county's population over the five years following an IPO filing relative to the IPO filing year; the dependent variable in Column 2 is the annual geometric average growth rate in the unemployment rate (i.e., unemployed divided by employed plus unemployed) over this period; and the dependent variable in Column 3 is the annual geometric average growth rate in per-capita personal income over this period; and the dependent variable in Column 4 is the annual geometric average growth rate in wages and salary per worker over this period. We winsorize all dependent variables at the extreme 1%. The sample includes IPO filings between 1986 and 2011. 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.

	(1) Population	(2) Unemployment Rate	(3) Income	(4) Wages
Instrumented IPO Completion	-0.0008	0.0072	-0.0050	-0.0003
	(-0.53)	(0.35)	(-1.07)	(-0.07)
Instrumented IPO Completion	-0.0017**	0.0015	-0.0050***	-0.0044**
× Large IPO	(-2.77)	(0.23)	(-3.04)	(-2.33)
Pop Growth	0.2481***	0.3988	-0.0132	0.0613
	(4.20)	(0.89)	(-0.19)	(0.77)
Employee Growth	0.0294	0.4204*	0.0708	0.0980
	(1.44)	(2.06)	(1.34)	(1.54)
Income Growth	-0.0137	0.2751***	-0.1229*	-0.1158
	(-1.09)	(4.20)	(-2.00)	(-1.64)
Ln(Number of IPOs)	-0.0010	-0.0084**	0.0024	0.0035
	(-1.56)	(-2.83)	(1.27)	(1.65)
Ln(IPO Size)	0.0005**	-0.0010	0.0009*	0.0006
	(2.15)	(-0.60)	(2.01)	(1.48)
Number Lead Managers	0.0003	-0.0019	0.0002	-0.0000
	(1.08)	(-1.15)	(0.37)	(-0.10)
PE/VC Funding	0.0001	-0.0007	0.0010*	0.0007
	(0.54)	(-0.34)	(2.02)	(1.46)
Underwriter Reputation	0.0000	0.0001	0.0002	0.0002
	(0.28)	(0.34)	(1.62)	(1.31)
County FE	Yes	Yes	Yes	Yes
State FE	No	No	No	No
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.140	0.061	-0.021	0.046
Observations	6,201	5,206	6,201	6,201

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 2011, 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 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 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	2.258***	1.034**	0.265**	671.629*
Completion	(3.38)	(2.15)	(2.05)	(1.66)
Pop Growth	5.864**	5.296**	1.531***	1892.729*
	(2.11)	(2.40)	(3.34)	(1.68)
Employee Growth	-4.232***	-3.520***	-0.688***	-421.158
	(-3.32)	(-3.55)	(-4.63)	(-0.60)
Income Growth	-0.138	-0.630	-0.099	-1160.652***
	(-0.12)	(-0.66)	(-0.46)	(-2.60)
Ln(Number of IPOs)	0.013	0.003	0.001	12.002
	(0.47)	(0.12)	(0.23)	(1.09)
Ln(IPO Size)	0.354***	0.229***	0.022***	36.050*
	(11.99)	(8.07)	(3.21)	(1.89)
Number Lead	-0.060	-0.016	-0.012	-40.360
Managers	(-0.49)	(-0.19)	(-0.89)	(-1.02)
PE/VC Funding	-0.134*	-0.095	-0.024	-62.307
	(-1.91)	(-1.40)	(-1.52)	(-1.53)
Underwriter	0.024	0.029**	0.008**	19.432***
Reputation	(1.48)	(2.42)	(2.39)	(4.06)
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Adj. R-squared	0.103	0.124	0.101	0.070
Observations	6,451	6,451	6,451	6,451

Table 9: Geographic Dispersion for IPO firms

This table reports estimates from OLS regressions exploring determinants of geographic dispersion of state operations at the firm level. Geographic dispersion is the number of unique state-name counts in firms' 10K reports (specifically, in the four sections Business, Properties, Consolidated Financial Data, and Management's Discussion and Analysis), using the data collected annually by Garcia and Norli (2012) between the years 1995 and 2008. Using our set of 6,451 IPOs, we match 1,443 IPOs that have two consecutive data points of 10K state name counts in the first two years following the IPO. The dependent variable in Columns 1 and 2, Δ *Non-Local Operations*, is the one-year percentage point change in the ratio of states mentioned that are not the firm's headquarter state to the total number of states mentioned. The dependent variable in Columns 3 and 4, *Dispersion*, is the post-IPO one-year growth in unique 10K state-name counts. Dependent variables are winsorized at the 1% and 99% levels. The main explanatory variable in Columns 1 and 3 is the log of per-capita personal income of the county of an IPO in the year prior to the IPO filing; the main explanatory variable in Columns 2 and 4 is the log of relative average wages in the county, benchmarked to average wages across all counties in that year. 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) Dispersion	(4) Dispersion
Ln(Income)	**-0.036*** **		**-0.092** **	
	(-3.01)		**(-2.35)**	
Ln(Relative Wage)		**-0.094*** **		**-0.170**
		(-4.99)		**(-1.21)**
Employee Growth	0.352	0.302	0.131	0.089
	(0.74)	(0.63)	(0.12)	(0.08)
Pop Growth	-0.174	-0.225	-1.672	-1.749
	(-0.27)	(-0.35)	(-0.98)	(-1.03)
Income Growth	0.392***	0.408***	0.539	0.530
	(3.82)	(3.95)	(1.21)	(1.35)
Ln(Number of IPOs)	-0.007**	-0.003	-0.011	-0.006
	(-2.09)	(-0.78)	(-0.71)	(-0.24)
Ln(IPO Size)	-0.013	-0.014	-0.011	-0.012
	(-1.60)	(-1.63)	(-0.44)	(-0.46)
Number Lead Managers	0.006	0.006	0.019	0.019
	(0.87)	(0.85)	(0.62)	(0.64)
PE/VC Funding	-0.004	-0.005	-0.018	-0.020
	(-0.48)	(-0.49)	(-0.45)	(-0.49)
Underwriter Reputation	0.004*	0.005**	0.005	0.005
	(1.88)	(2.06)	(0.44)	(0.50)
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	0.009	0.010	Yes	Yes
Adj. R-squared	0.352	0.302	0.003	0.003
Observations	1,435	1,435	1,443	1,443