# Public Ownership 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, especially in less populated and poorer areas. After accounting for endogeneity in the ownership decision, areas hosting companies that go public experience muted growth in employment, establishments, population, and wages, relative to areas where firms file to go public and remain private. Establishment-level analyses 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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## I. Introduction

Local economies form as the result of industrial co-location. Ellison and Glaeser (1997) and others note that spatial concentration of economic activity arises primarily due to natural advantages and agglomeration economies (i.e., synergistic benefits to co-location).[1] Agglomeration economies include deeper consumer and labor markets, knowledge and technological spillovers, and reduced transportation costs.[2] 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) further show that agglomeration economies impact firm policies, as firm investment is highly sensitive to the investment of firms in other industries headquartered nearby.

In this paper, we study the effect of firm ownership structure on local agglomeration economies. Specifically, we examine how a firm's transition from private to public ownership affects economic growth in the firm's local economy. Firms undergo several substantial changes when transitioning to public ownership, two of the largest being increased access to capital and increased visibility, with visibility derived from media attention, public disclosures, due diligence, and underwriter certification.[3] These changes increase firms' bargaining power for capital, labor, and other inputs to production (see e.g., Pagano, Panetta, and Zingales, 1998; Turban and Cable, 2003). If these changes spur local production then the local economy will benefit, both directly via increased demand for local inputs and indirectly via enhanced

[1] Ellison and Glaeser (1999) conclude that natural advantages can explain, at most, half of geographic concentration.
[2] See Glaeser and Gottlieb (2009) and Moretti (2011a) for surveys of the agglomeration literature.
[3] See, e.g., Roell (1995), Brau and Fawcett (2006).

agglomeration economies, as the area becomes more attractive for other businesses looking to shares goods, people, or ideas (Ellison, Glaeser, and Kerr, 2010).

Alternatively, the enhanced visibility gained by going public may stunt local production, and consequently local economic growth, if greater bargaining power 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).

Our empirical strategy involves first identifying the causal effect of going public on local economic growth, and then providing 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 (i.e., conducts an initial public offering, IPO). To address this selection problem, we first restrict the sample to county-years in which a firm that is large relative to the county's economy files for an IPO. [4] 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 firms that are a significant part of the local economy randomly complete their IPOs to growth in areas where similar firms randomly withdraw their IPO filings.

[4] Our main specification retains IPOs in the top tercile of IPO proceeds relative to county employees, but results are similar retaining IPOs that are in the top quartile or above the median of this measure.

As in Bernstein (2015), we use two-month NASDAQ returns following an IPO filing to instrument for IPO completion in a two-stage least squares (2SLS) framework. We first corroborate that NASDAQ returns in the two months after IPO filings are a significant predictor of IPO completion, with first stage t-statistics of 4.73 and 6.02 in our two main specifications. Our exclusion restriction assumes that NASDAQ 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) placebo tests indicate that there is no significant relation between future local economic growth and NASDAQ returns occurring during any two-month period in the three years surrounding IPO filings, except for the two-month period immediately following an IPO filing, and b) there is no relation between these NASDAQ returns 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 NASDAQ returns during a random two-month period and future local economic outcomes.[5]

We begin the empirical analysis by comparing employment growth rates in counties with quasi-randomly completed IPOs to counties with quasi-randomly withdrawn IPOs. In contrast to OLS evidence, 2SLS estimates indicate that going public reduces local employment growth relative to remaining private. During the five years after a firm completes its IPO, we estimate that employment growth declines by between 1.2 and 1.7 percentage points per year relative to areas with withdrawn IPOs. We also find that establishment growth declines by about 0.7 percentage points per year in these same areas relative to areas with withdrawn IPOs. Consistent

[5] For instance, these placebo tests make it unlikely that the effect of two-month post-filing NASDAQ 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 NASDAQ 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.

with our identifying assumptions, we show within the two-stage framework that counties hosting exogenously completed and withdrawn IPOs have parallel trends in economic activity prior to IPO filings.

Since a firm is more likely to play an important role in the local agglomeration when it is large or the county is small, we expect the reduction in employment growth following IPOs to be decreasing in the size of the county and increasing in the size of the IPO. Consistent with this, the negative employment effect of going public is concentrated among IPOs in the top tercile of IPO size (relative to county employment). Additionally, interacting IPO completion with county size, county average income, and IPO size, we show that the reduction in employment growth attributable to firms going public is decreasing in county size and average income, and increasing in IPO size. These results suggest that the level effect of IPOs on local employment is fairly constant; going public results in around 2,300 fewer jobs per year in the county where the firm is located relative to areas with withdrawn IPOs.

This effect is large relative to the size of the average IPO firm in our sample, which employs approximately 3,000 employees.[6] Thus, a substantial fraction of the effects we observe are likely due to a combination of direct effects—including an impact on firms with business ties to the firm going public—and indirect effects through negative agglomeration spillovers. Assuming a multiplier of 2, our estimates imply that going public directly leads to roughly 780 fewer local jobs per year.[7] Our later estimates show that approximately 40% of this direct loss is explained by the IPO firm shifting employees away from its headquarter county.

[6] Using pre-IPO firm employee totals from Computstat for those with non-missing information.
[7] Moretti (2010) estimates that one job loss in the tradable sector can result in 1.5 to 2.5 additional job losses in the local economy, with larger multipliers for the loss of skilled workers. We argue that these estimates are reasonable in our setting because employees working in the IPO firm's headquarter location are likely to be skilled, even when the IPO firm is not in the tradable sector.

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 six years. Consistent with 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., 10 years). This chain of events suggests that IPOs first lower agglomeration benefits among goods-producing firms; this then spills over into non-tradable industries as there are less workers to service. It also suggests that it can take local economies an entire decade to reach a new equilibrium after large firms go public.

To study whether the reduction in local employment is driven by changes in population or unemployment, we estimate our main 2SLS employment model with five-year changes in population and unemployment rates. We find that completed IPOs reduce county population growth by 3.4 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 post-IPO geographic expansion, 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 both inside and outside of a firm's headquarter county 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.

Taken together, these findings suggest that one likely mechanism through which going public stunts local economic growth is that the IPO enables firms to more effectively grow outside their local economy. Although we argue above that the most likely explanation for this geographical expansion is that IPOs enhance visibility, a thorough treatment of exactly what it is about IPOs that leads firms to expand beyond their local economy is beyond the scope of this paper, requiring exogenous variation in IPO characteristics. For instance, going public may also lead IPO firms to geographically expand if it provides sufficient capital to make large lumpy non-local investments instead of more marginal local investments.

Our results contribute to several strands of literature. First, we contribute to the agglomeration economics literature by showing that a shock to a large firm's ownership type, which increases visibility and bargaining power, adversely affects local agglomeration economies. IPO firms shift to non-local production, which reduces the incentives of goods-producing firms to invest locally, and this spills over to non-tradable industries, further eroding economic growth.

Our paper also relates to the literature on IPOs and the consequences to going public.[8] Bernstein (2015) shows that the increased agency costs from going public reduce firms' internal innovation, Borisov, Ellul, and Sevilir (2017) find that going public increases IPO firm-level employment, and Babina, Ouimet, and Zarutskie (2017) show that going public provides an avenue for employees to leave and start their own businesses. Our paper extends this literature by showing that not only is there a geographic element to how issuers shift their business operations after going public, but such changes create negative externalities for business activity in issuers' local economies.

Finally, we contribute to the literature on stock market development and macroeconomic growth (e.g., King and Levine, 1993; Levine and Zervos, 1998; and Wurgler, 2000). A unique feature of our work is that it examines the effects of stock market development at the local level. Our results do not refute evidence that stock market development stimulates macroeconomic development, but they do suggest that these gains may come at the expense of agglomeration economies in the areas where firms originate.

## II. Conceptual Framework

To understand how public ownership affects agglomeration economies, it is necessary to first understand how the change from private to public ownership affects an IPO issuer's incentive to invest locally. We center the following discussion on the visibility benefits to going

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

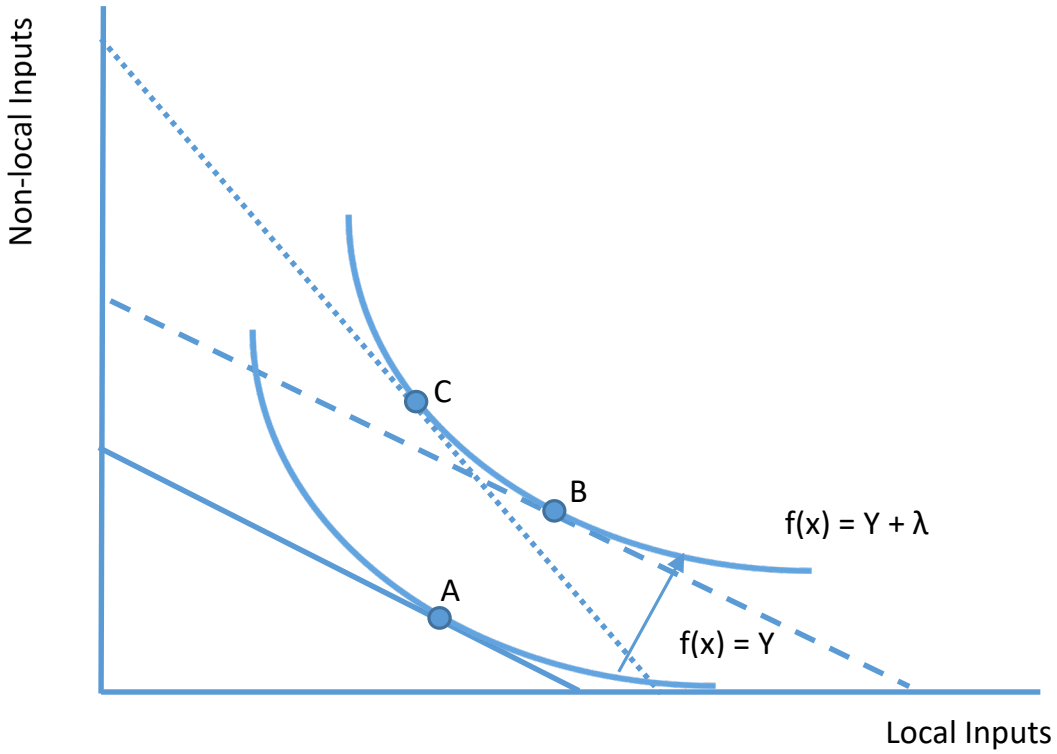
public, which represent an oft-cited reason managers decide to go public (see e.g., Roell, 1995; Brau and Fawcett, 2006).[9]

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, 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.

To formalize the possible effects that improved visibility from going public 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 output Y using the input bundle A. After the IPO, optimal production shifts outward from Y to $Y + \lambda$ because of reduced information asymmetry, which lowers input costs (i.e., lower costs of capital, better terms on trade credit, more productive employees, etc.).

[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.

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




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

While this shift in production after the IPO clearly increases the firm's demand for aggregate inputs, the change in demand for local vs. non-local inputs, and thus the firm's concentration of growth locally vs non-locally, is ambiguous. The change in relative local vs. non-local demand depends on a) the specific production function, and b) the relative decrease in local and non-local input costs. If the ability to negotiate better deals following an IPO affects the costs of acquiring local and non-local inputs equally, there is a parallel shift outward to the long-dashed isocost line. In this case, the firm shifts production to input bundle B and demands more of both inputs. This will 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).

In contrast, the reduction in information asymmetry could primarily affect non-local inputs. Previous work argues that information asymmetry increases with distance. For example, Costello (2013) uses geographic distance as a measure of information asymmetry between customers and suppliers and finds that distance is related to shorter and more covenant-laden contracts. Knyazeva and Knyazeva (2012) and Hollander and Verriest (2016) document similar evidence of distanced-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). Thus, the visibility and certification associated with going public could especially reduce the cost of non-local inputs, shifting the isocost curve to the short-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 could then reduce potential agglomeration economies for other businesses, and generate negative spillovers in business activity throughout the IPO firm's local economy.

In sum, whether a firm transitioning to public ownership positively or negatively affects the local economy is ultimately an empirical question. The main contribution of this paper is to identify the aggregate effect of going public on the local economy. We then provide descriptive evidence on the underlying mechanism.

## III. Sample Description

Our sample begins with all U.S. IPOs filed between 1986 and 2011 from Thomson One's New Equity Issues database, excluding blank check offerings. 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. This is also where we collect population and income per capita data.[10] 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.[11]

We apply several filters to the IPO sample to focus on IPOs that are most likely to affect the local economy. First, we exclude filings without reported filing proceeds or lagged county employment figures. Second, we exclude issues during the 1998-1999 tech bubble, during which a large number of extremely young firms went public, which we cannot otherwise filter on due to data limitations for withdrawn IPO filers.[12] Finally, we restrict our main sample to the largest third of the remaining IPO filings in terms of the amount of real proceeds filed for, scaled by the

[10] BEA uses the Census Bureau's annual midyear (July 1) for population estimates.
[11] Defining the calendar year so that the end of the NASDAQ 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.
[12] 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.

lagged number of employees in the filer's headquarter county.[13] After imposing these restrictions, our sample includes 2,862 IPO filings and 2,038 county-years.[14] Seventy-eight percent of these IPOs are completed and twenty-two 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 withdrawn IPOs in our sample are similar in terms of proceeds filed, venture capital backing, and the number of lead underwriters employed. Specifically, the issuers that complete their offerings file for approximately $186 million in proceeds (in 2011 dollars), compared to $207 million for ultimately withdrawn offerings. Both groups are venture capital- or private equity-backed approximately 39% of the time. The industry distribution of the two groups is also similar with the three most frequent SIC 2-digit groups being business services (SIC 73), finance holdings and investment offices (SIC 67), and chemicals and allied products (SIC 28). In both subsamples, each of these three industries comprises between 8% and 13% of the sample.

[13] See Section V.B for a discussion of how these and additional sample restrictions impact our findings.
[14] The sample is slightly smaller for some outcomes due to data limitations.

The descriptive statistics for our sample of IPOs highlight several notable differences between our sample and more comprehensive samples of IPO activity used in the literature. For example, according to data on Jay Ritter's website, the average proceeds raised for IPOs between 1980 and 2017 is approximately $100 million (in 2003 dollars) or two-thirds of the amount raised by the average firm in our sample after adjusting for the base year.[15] In addition, our sample underrepresents technology IPOs and IPOs in populated areas. Figure 2 illustrates the geographical dispersion of our sample. Over our 25-year sample period, few counties host more than five IPOs, and the IPOs in our sample are spread across much of the United States. Our analysis should be interpreted as an investigation of how the local economy is affected when firms that are a significant part of the local area go public.

## IV. Identification Strategy

### IV.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 an IPO filer likely differ from counties without an IPO filer in a number of ways.

Panel B of Table 1 illustrates the empirical 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 over seven 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 over 40% higher in IPO county-

[15] See https://site.warrington.ufl.edu/ritter/files/2018/01/IPOs2017Statistics_January17_2018.pdf.

years. More importantly, IPO county-years exhibit significantly higher past and future growth in employment 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 when to complete their IPO based in part on the future prospects of the local economy. Panel C of Table 1 provides some evidence that this possible selection translates into better future prospects for completed IPOs. Conditional on hosting an IPO filing, county-years with exclusively completed IPOs are similar to counties with exclusively withdrawn IPOs in terms of employment and population levels. However, counties with completed IPOs have 6.4% lower per-capita income, and have significantly higher lagged and future growth rates in employees, population, and income.

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 six columns are estimated at the IPO level, resulting in repeated observations for county-years with multiple IPOs. Regressions in Columns 4-6 are weighted by one over the number of IPOs filed in the county-year.

In Columns 1 and 4, the dependent variable is the annual geometric average of five-year cumulative growth in county employment, relative to the IPO filing year. The positive and significant coefficient on IPO completion suggests that counties with completed IPOs experience higher future growth in employment than counties with withdrawn IPOs, even after controlling for lagged economic conditions. The remaining columns provide similar evidence with respect to

post-filing population and real income growth, although the estimated effects are smaller and statistically insignificant.

The positive association between completed IPOs and future employment growth is consistent with evidence in Butler, Fauver, and Spyridopoulos (2016). However, the statistical methods used thus far are not sufficient to claim that the relation is causal. In particular, it is possible that, as mentioned above, an IPO filer's expectation of future local economic conditions affects the IPO completion decision.

### IV.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. We then compare county-years with completed IPOs to those with withdrawn IPOs. This approach holds constant the presence of a large and growing private firm at the IPO filing stage. However, counties hosting completed and withdrawn IPOs are still different from each other, as shown in Table 1. The fact that IPO completion is non-random makes it challenging to draw causal inference by simply comparing across these groups.

To address the endogeneity of the IPO completion decision, and identify the causal effect of IPO completion on the local economy, we use an instrumental variable approach. As in Bernstein (2015), we use the two-month NASDAQ returns following an IPO filing to instrument

for IPO completion.[16] Our first stage model regresses an indicator for IPO completion on two-month post-filing NASDAQ returns, in addition to controls for economic conditions:

$$\begin{aligned} IPO\ Completion_{it} = {} & \alpha_1 NASDAQ\ Ret._{it} + \alpha_2\ Ln(Emp)_{it-1} + \alpha_3\ Ln(Pop)_{it-1} + \\ & \alpha_4\ Ln(Income)_{it-1} + \alpha_5\ Emp.\ Growth_{it-1} + \alpha_6\ Pop.\ Growth_{it-1} + \\ & \alpha_7\ Income\ Growth_{it-1} + \alpha_8\ Ln(IPOs)_{it} + \alpha_9\ Ln(IPO\ Size)_{it} + \\ & \alpha_{10}\ Leads_{it-1} + \alpha_{11}\ PE\ \ or\ VC_{it} + \alpha_{12}\ Underwriter\ Rep._{it} + \lambda_j + \gamma_t + \varepsilon_{it} \end{aligned} \quad (1)$$

, where *IPO Completion* equals one for a completed IPO and zero for a withdrawn IPO. *NASDAQ Ret.* is the two-month NASDAQ return following the IPO filing. We control for nationwide economic conditions with year fixed effects. We further control for local economic conditions with the natural log of the lagged number of employees, population, and income per capita in a county, as well as the most recent annual percentage growth in these variables, although we also estimate regressions excluding these lagged economic controls to simplify interpretation and reduce mean-reversion concerns. We also 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.[17] Finally, $\lambda_j$ represents SIC 2-digit industry fixed effects. See Appendix A for variable definitions and data sources.

[16] Our findings are similar using two-month post filing CRSP value-weighted returns as the instrumental variable.
[17] 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.

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:

$$\begin{aligned}\Delta\ Econ.\ Outcome_{it,t+5} = {} & \beta_1 Instrumented\ IPO\ Completion_{it} + \beta_2\ Ln(Emp)_{it-1} + \\ & \beta_3\ Ln(Pop)_{it-1} + \beta_4\ Ln(Income)_{it-1} + \beta_5\ Emp.\ Growth_{it-1} + \beta_6\ Pop.\ Growth_{it-1} + \\ & \beta_7\ Income\ Growth_{it-1} + \beta_8\ Ln(IPOs)_{it} + \beta_9\ Ln(IPO\ Size)_{it} + \beta_{10}\ Leads_{it-1} + \\ & \beta_{11}\ PE\ \ or\ VC_{it} + \beta_{12}\ Underwriter\ Rep._{it} + \lambda_j + \gamma_t + \varepsilon_{it} \end{aligned} \quad (2)$$

, where $\Delta$ *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 measure of economic activity is county-level employment; we also examine county-level establishments, population, 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.

### IV.C Identifying Assumptions

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

In Table 3, we estimate the first stage regression (i.e., Equation 1) to examine the relevance condition, which requires that our instrument, $NASDAQ\ Ret._{it}$, is a significant predictor of IPO completion. Column 1 includes only year and industry fixed effects and IPO-level

controls, along with our instrument. Consistent with prior evidence linking market fluctuations during the book-building period to IPO completion, the coefficient estimate of 0.560 suggests that a 10% increase in NASDAQ returns in the two months after an IPO filing predicts a 5.60 percentage point increase in the probability of IPO completion.[18] Additionally, the number of lead managers, private equity or venture capital backing, and underwriter quality all positively predict IPO completion. In Column 2, we find similar results weighting each observation by one over the number of IPOs in a county-year.

In Columns 3 and 4, we add controls for county-level economic conditions in the year prior to the IPO filing date. Only lagged employment is predictive of IPO completion.[19] Notably, these controls have little effect on the predictive power of post-filing NASDAQ returns, suggesting that post-filing NASDAQ returns are unrelated to county characteristics. In sum, across all four columns of Table 3, we see that post-filing NASDAQ fluctuations are a strong predictor of IPO completion, alleviating concerns of a weak instrument. Our first stage F-statistics in Columns 3 and 4 are 36 and 22, 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%.

The second half of our identifying assumption, the exclusion restriction, requires that two-month post-IPO NASDAQ 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 much more likely to be satisfied after including year fixed effects (and other

[18] 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).
[19] In unreported tests, we include longer-term lagged controls for employment, population, and income. Several of these controls are at least moderately predictive of IPO completion, which suggests IPO completion is non-random with respect to economic conditions

controls for current economic conditions) in the regression. A violation of this assumption would require NASDAQ 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 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 placebo tests to examine the likelihood that such a violation of the exclusion restriction exists.

These placebo tests take the following form:

$$\begin{aligned}\Delta\ Econ.\ Outcome_{it,t+5} = {} & \delta_1 Two\text{-}Month\ NASDAQ\ Ret._{it} + \delta_2\ Ln(Emp)_{it-1} + \\ & \delta_3\ Ln(Pop)_{it-1} + \delta_4\ Ln(Income)_{it-1} + \delta_5\ Emp.\ Growth_{it-1} + \delta_6\ Pop.\ Growth_{it-1} + \\ & \delta_7\ Income\ Growth_{it-1} + \delta_8\ Ln(IPOs)_{it} + \delta_9\ Ln(IPO\ Size)_{it} + \delta_{10}\ Leads_{it-1} + \\ & \delta_{11}\ PE\ \ or\ VC_{it} + \delta_{12}\ Underwriter\ Rep._{it} + \lambda_j + \gamma_t + \varepsilon_{it} \end{aligned} \quad (3)$$

, where *Two-Month NASDAQ 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 NASDAQ Ret.*$_{it}$ and future economic outcomes, except when *Two-Month NASDAQ Ret.*$_{it}$ is measured immediately following a local firm filing to go public.

Panel A of Figure 3 presents estimates of $\delta_1$ 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 NASDAQ 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 $\delta_1$. Consistent with our exclusion restriction, none of the eighteen event-time windows over which *Two-Month NASDAQ Ret.*$_{it}$ is computed – other than the window immediately following a local IPO filing – are significantly related to future employment growth. Panels B and C present

similar results estimating Equation 3 with future population and per capita income growth as dependent variables.

Bernstein (2015) – the first paper to employ this 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. The author shows that firms exposed to high and low NASDAQ fluctuations following their IPO filing are similar along observable dimensions and confirms that placebo periods of two-month NASDAQ returns measured before the IPO filing or one year after the filing are not related to long-run firm outcomes. Given this existing evidence, we defer additional placebo tests, and other robustness analyses, to Sections V and VI.

## V. 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 IV. Our main measure of local economic activity is the number of employees working in an IPO filer's headquarter county.

Columns 1-4 of Table 4 (Panel A) present second-stage 2SLS estimates using a county's average annual employment growth rate over the five years following an IPO filing as the dependent variable. The explanatory variable of interest, *Instrumented IPO Completion*, is the fitted value from the corresponding column in Table 3. The coefficient can be interpreted as the effect of an IPO being randomly completed (i.e., nudged to completion due to two-month post-filing market returns that were just favorable enough) in the county in year zero relative to what would have happened had the IPO been randomly withdrawn.[20]

[20] 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 will complete the IPO.

The significantly negative coefficients on *Instrumented IPO Completion* in Panel A of Table 4 indicate that going public reduces county-level employment growth over the succeeding five years, compared to what would have happened had the firm remained private. The magnitude of the estimated effect ranges from 1.2 to 1.7 percentage points annually, or about a 0.65-0.85 standard deviation decrease in annual employment growth.[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 that IPOs negatively affect establishment growth as well; an arguably random completion of an IPO reduces local establishment growth by about 0.7 percentage points per year.

Before translating the percentage decline in employment growth into a level effect, it is necessary to consider how the effect varies cross-sectionally with the size of the county and the size of the IPO. In particular, we expect the change in employment growth to be larger when the local economy is small, and when the IPO firm is large. In Table 5, we interact our instrumented IPO completion variable with various measures of the size of the headquarter county as well as the size of the IPO itself. We find that the percentage decline in employment growth is smaller for larger counties, counties with higher per capita income, and following smaller IPO filings. In unreported tests, we re-estimate the interaction with IPO size in Column 4 of Table 5 after relaxing our sample restriction that an IPO be in the top tercile of IPO size (relative to county employment). The results are consistent with small IPOs having no significant effect on future local employment growth, and the effect growing in magnitude as the size of the IPO increases relative to the size of the county's economy. We revisit this idea in Section VII.B when we examine the scope of the effect of IPOs on the local economy.

[21] Results are similar including year-quarter fixed effects, instead of year fixed effects.

Using results from Table 5, we can estimate the number of jobs lost due to an IPO.[22] From Column 1 of Table 5, a 10% increase in pre-filing county employees reduces the magnitude of the effect by 10 basis points annually. Plugging in for the 25th and 75th percentile of logged pre-filing county employees and multiplying by nominal pre-filing employment suggests that going public leads to a relatively constant effect of approximately 2,300 fewer employees per year.[23] This effect is large relative to the size of the average IPO firm in our sample (which has around 3,000 employees, based on pre-IPO Compustat estimates), suggesting substantial spillover effects from the IPO firm to other businesses in the local economy.

To check whether this effect is plausible, we first estimate the direct effect necessary to generate a total loss of 2,300 jobs. Moretti (2010) estimates local employment multipliers to be between 1.5 and 2.5; applying a multiplier in this range to our setting implies that an IPO directly leads to around 780 fewer local employees per year. In section VI.A, we use U.S. Census Bureau establishment-level data to show that about 40% of this is explained by the IPO firm shifting employment away from their headquarter county after going public. The remaining 450 annual job losses are a spillover effect from the IPO, resulting from activity such as the use of fewer local suppliers and less local financing. This is a large spillover effect, but not implausibly so.

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 decline in event time surrounding the IPO filing year. Specifically, we plot the *Instrumented IPO Completion* coefficient from a series of

[22] Importantly, precisely identifying the magnitude is complicated by the fact that 2SLS estimates represent local average treatment effects and depend on the functional form of the estimated model.
[23] The 25th percentile of logged and unlogged lagged employment is 11.887 and 145,295, and at the 75th percentile is 13.317 and 607,191.

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 4 of Table 4.[24] This figure shows that employment begins to decline in the year following the IPO filing, and continues to decline at a steady rate over the five-year post-filing window, with the effect becoming significant at the 5% level two years after the IPO filing. Unreported results indicate that the employment growth reduction persists through the 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 growth, indicating similar pre-trends in employment 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 NASDAQ 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 higher long-run county employment growth.

In Table 6, 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.[25] The literature on local multipliers

[24] Lagged local economic conditions are measured as of year -4 when measuring pre-IPO filing economic growth.
[25] Tradable is composed of all goods-producing industries, minus agriculture and mining, as defined by the North American Industry Classification System (NAICS). Non-tradable includes all remaining industries, excluding public administration.

(Moretti, 2010) typically characterizes agglomeration dynamics as initiating from an increase or decrease in tradable sector jobs, followed by a change in 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 spillover into non-tradable industries. The results in Table 6 show that by the fifth year following the IPO, employment growth within the tradable sector declines by 3 percentage points per year, while employment growth in non-tradable industries experiences an insignificant 0.5 percentage point per year decline. By the tenth year following the IPO, the decline in tradable sector employment growth levels out, while the decline in non-tradable sector employment growth increases to about 1.7 percentage points annually. This evolution in employment growth among sectors is illustrated in Figure 5, which displays the quicker response of tradable sector employment. The figure suggests that it takes approximately a decade for a local economy to rebalance after a large firm goes public, but once it does, it has a similar proportion of employees in tradable and non-tradable sectors as it did prior to the IPO.

In Table 7, 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 7 is most consistent with a population decline driving the decline in employment. The estimates in Columns 1 and 2 indicate that a county's growth in population declines by approximately 0.6 to 0.8 percentage points each year during the five years following a completed IPO; we find no evidence of a 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 Table 8, we use annual average changes in per-capita personal income and earnings 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 and salaries, which we present in Columns 3 and 4 of Table 8, are qualitatively similar and significant but only 33% to 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.

Our results thus far consistently suggest that local agglomerations are disrupted when an incumbent firm goes public: aggregate growth in employees, establishments, population, and wages are weakened in the aftermath of a local firm transitioning from private to public, 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.

## VI. Discussion and Descriptive Evidence on Mechanism

The analysis thus far exploits exogenous variation in IPO completions to provide evidence that IPOs 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

behind the negative consequences requires additional exogenous variation, which we do not have. Nevertheless, in this section we descriptively examine the economic drivers behind the effect of IPOs on local economic growth.

A reduction in local investment by IPO firms after going public represents a natural starting point for thinking about drivers of reduced local economic growth.[26] 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.[27] Thus, it is likely any changes in local investment by IPO firms after going public have substantial spillover effects through reduced local agglomeration economies.

In this section we present descriptive evidence examining one particular mechanism that, when combined with local agglomeration economies, is a potential driver of the post-IPO reduction in local economic growth. Specifically, we examine the prediction discussed in Section II that increased visibility associated with going public better enables firms to expand outside their local economies to access better resources. This then reduces co-location benefits for other businesses in the area.

This idea is broadly consistent with the results in Column 3 of Table 5, where we find larger employment effects of completed IPOs in counties with lower income. This result is consistent with IPOs allowing firms to substitute non-local inputs for local inputs when the benefits to doing so are larger. We confirm a similar conclusion when examining the population

[26] Existing literature provides 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).

[27] 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.

and income effects of completed IPOs; population and income growth are more severely affected in counties with lower income. This is consistent with the adverse local economic effects of IPOs being concentrated in the areas that offer the most upside for gaining access to factors of production in non-local markets.

### VI.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 diversification is related to the average income of the IPO firm's headquarter county. We begin by matching our sample 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).[28]

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.[29] 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 2,862 firms in our sample.[30] The primary reason that we are unable to match all firms is that the

[28] The BR was formerly called the Standard Statistical Establishment List (SSEL).
[29] We use the generalized Levenshtein edit distance to compare names and match observations with scores less than 200.
[30] Census disclosure requirements require us to round the reported number of observations to the nearest hundred.

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.[31] This, along with the fact that some firms exit the sample via merger, leads our sample size to shrink, especially when examining employee and establishment growth over long horizons.

Given these constraints, we focus on the two-year window after the filing of an IPO. The results are broadly similar for longer windows, though smaller sample sizes reduce the power of the tests. The LBD allows us to construct two separate measures of the geographic dispersion of firm production: the number of establishments and the number of employees. We define *Employee Growth* as the percentage change in the number of employees in the firm's home county less the percentage change in the number of employees outside of the firm's home county, measured from the year prior to the IPO filing 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 the same two-stage specification found in Table 4 to estimate the effect of going public on the geographic dispersion of firm activity, and report the results in Table 9. The

[31] 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.

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. 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.

Next, we repeat the analysis found in Column 3 of Table 5 and examine whether an IPO's effect on geographic dispersion is concentrated in poorer counties. Columns 3 and 4 of Table 9 present the results. The main effect of IPO completion on local firm production is still negative and significant, but similar to the county-level results discussed earlier, the interaction between *Instrumented IPO Completion* and *Ln(Wages)* is positive (though it is only significant for establishment growth). This suggests that firms in poor counties are most likely to shift their establishments and employees to other counties following an IPO.

### VI.B Additional Evidence from Post-IPO Filings

As additional evidence that firms in poor counties are more likely to expand non-locally following a public listing, we obtain geographic dispersion measures from data collected by Garcia and Norli (2012), which record the number of unique 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 within the first three years, we are able to match 778 of the 2,862 issuers in our sample to these data (85% of our completed IPOs over this period). We measure *Post-IPO Geographic Dispersion Growth* as a one-year percentage change in the number of state-names using the earliest two consecutive post-IPO 10K filings.

The average one-year increase in geographic dispersion after going public is 19%. In Table 10, 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 relative average wages (Column 2). We find a significant negative relation between county income and post-IPO growth in geographic diversification of operations.[32] The only other significant county-level predictor of post-IPO geographical dispersion growth is population growth, the coefficient on which suggests that newly public firms are more likely to disperse operations when the local economy is contracting (consistent with Column 2 of Table 5).

Taken together, the evidence in this section is consistent with the conceptual framework presented in Section II and suggests two things: first, IPOs provide an avenue for firms to shift business activity outside their agglomeration, particularly in areas deemed insufficient to foster future firm growth. And second, this shift in business activity is particularly detrimental for poorer counties with possibly less dense business networks, where aggregate economic growth severely declines following a local IPO.

## VII. Additional Analyses

In this section, we conduct a variety of tests examining the plausibility of our identifying assumptions (in addition to those presented previously), and the scope of the estimated effect of IPOs on the local economy.

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

VII.A. Validation Tests

In Section IV.C, we presented a set of placebo analyses that supports our exclusion restriction (see Figure 3). Here, we conduct several additional tests to further establish the plausibility of our identifying assumption, which assumes that two-month post-filing NASDAQ returns represent a shock to IPO completion but are otherwise unrelated to future local economic growth.

Panel A of Table 11 presents regression results for some of the analysis illustrated in Figure 5, intended to further demonstrate the economic significance of the relation between NASDAQ returns measured over various periods and subsequent employment growth. In Column 1, we examine the direct association between our IV, two-month post-filing NASDAQ returns, and subsequent county-level employment growth. As expected, higher post-filing NASDAQ returns result in less future employment growth. Given that the standard deviation of 2-month NASDAQ returns is 7.9%, the coefficient of -0.0092 in Column 1 suggests that a one standard deviation increase in post-filing NASDAQ returns predicts a 0.4 percentage point decrease in county-level employment over the next five years.

Interpretation of our 2SLS results assumes that this relation between NASDAQ fluctuations and long-run county-level growth is due solely to the effect that post-filing NASDAQ 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 NASDAQ returns from broader swings in market conditions. One potential vulnerability though is that, despite the inclusion of year fixed effects, the economic growth of counties with IPO filings could be more sensitive to NASDAQ 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 NASDAQ returns and future economic growth to persist even if we measure NASDAQ returns over alternative two-month windows, not just two-month windows immediately following IPO filings.

Columns 2 and 3 of Table 11 Panel A conduct placebo tests regressing future county-level employment growth on NASDAQ returns in alternative two-month periods. Specifically, we examine the relation between two-month NASDAQ returns beginning one year prior to an IPO filing (Column 2) and one year after an IPO filing (Column 3), which is analogous to these two points in Figure 3. Given that 90% of completed IPOs are conducted within six months of their filing dates, we do not expect NASDAQ returns in either of these windows to be significantly related to IPO completion. If our identifying assumption holds, then NASDAQ returns during these placebo periods should be unrelated to future county growth. Consistent with this assumption, we find no significant relation between these placebo NASDAQ returns and future employment growth.

Panels B and C replicate this placebo analysis with our other two measures of county-level economic activity – population growth and personal income growth. In both cases, post-filing NASDAQ returns (i.e., our IV) are significantly related to future economic growth, but NASDAQ returns a year before or after the filing date are not. Unreported tests show similar results using NASDAQ returns measured six months before and six months after the filing date.

The solid lines in Figure 7 extend the analyses in Column 1 of Table 11. This figure plots the relation between *Post-filing 2-month NASDAQ* and county-level economic growth in the years surrounding the IPO filing year. Panels A through C indicate that *Post-filing 2-month NASDAQ* 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. The dashed lines in Figure 7 plot the same estimated relations for our two placebo windows of NASDAQ returns (i.e., beginning one year before or one year after an IPO filing). We find no evidence of a significant relation between NASDAQ returns during these placebo windows and a county's employment, 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 and five-year lagged growth rates in these measures.[33] 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.

To summarize our analysis assessing the validity of the exclusion restriction, we find that a) NASDAQ returns in the two months after an IPO filing uniquely predict subsequent county-level growth relative to surrounding periods of NASDAQ returns, b) this period of two-month post-filing NASDAQ returns is unrelated to past county-level growth, and c) these NASDAQ returns (in an IV analysis) do not predict subsequent growth in observably similar counties without an IPO filing. For these reasons, we believe our results are not the result of a spurious relation between *Instrumented IPO Completion* and county-level economic growth.

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

## VII.B. Scope of IPO's Effect on the Local Economic Growth

In this section, we conduct two sets of tests to examine the scope of the effect of IPOs on the local economy.

### VII.B.1 Are Neighboring Counties Affected?

We first examine whether completed IPOs predict economic growth in neighboring counties. This analysis could help reconcile our evidence with contrasting evidence presented in Butler, Fauver, and Spyridopoulos (2016) showing that completed IPOs are accompanied by higher income growth in the metropolitan area. We are inclined to attribute these differences to differences in empirical methods; our method does not rely on matching on observable dimensions to plausibly identify the causal effect of IPOs on the local economy. But it is also possible that differences in geographic focus between the two studies, with metropolitan areas typically being about five times larger than counties, drive our differential results. To ensure that our focus on a smaller geographical area does not undermine an offsetting effect over a larger region, we examine the net effect of an IPO on growth in surrounding counties.

Panel A of Table 12 presents second-stage 2SLS estimates using the same design as in Column 4 of Table 4. We use the same first-stage regression and explanatory variables, but compute the second-stage dependent variable as the average growth rate in the five nearest counties to an IPO filer's headquarter county. Across all three columns, the estimated effect of IPO completion is in the same direction, 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. In unreported analyses, 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.

### VII.B.2 Do Smaller IPOs Matter?

Thus far we have focused on the effect that large IPOs have on the local economy. We define large IPOs as those in the top tercile of the value of proceeds filed divided by total employees in the county. The median IPO issuer from this sample raises $343 per employee in the county, and employs approximately 3,000 people (for the sample with non-missing employees in Compustat). In Panel B of Table 12, we replicate our main tests using a sample of medium sized IPOs, which raise on average approximately 8% of what large IPOs raise (adjusted for county employees). As shown in Panel B of Table 12, medium sized IPOs do not significantly affect their local economy. All of the estimated effects are insignificant, while two of the three estimates are in the opposite direction of the negative effects we estimate for the sample of large IPOs.

## VIII. Conclusion

This paper examines the causal effect of going public on local economic growth. We use NASDAQ fluctuations in 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 and remain private. We find robust evidence that IPOs reduce employment growth in areas where firms originate. 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 that growth rates in establishments, population, and wages decline, while unemployment rates remain stable.

We find supporting evidence that IPOs increase the incentive for firms to expand outside their local economy to access new markets and factors of production. Consistent with poor counties providing the lowest reward to continued local production, we find that issuers from these counties more aggressively geographically diversify their labor force and business locations after going public.

Importantly, our findings pertain to growth and economies at the agglomeration 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). |
| Nasdaq Ret. | Cumulative daily Nasdaq 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). |

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Figure 2: Geographical dispersion of IPO sample

This figure plots the geographical distribution of the 2,862 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 tercile of IPO size (i.e., the value of shares filed relative to the total number of (pre-filing) employees in the county). The color shading corresponds to the total number of IPOs filing within each county through the sample period, with white corresponding to zero IPOs filed.

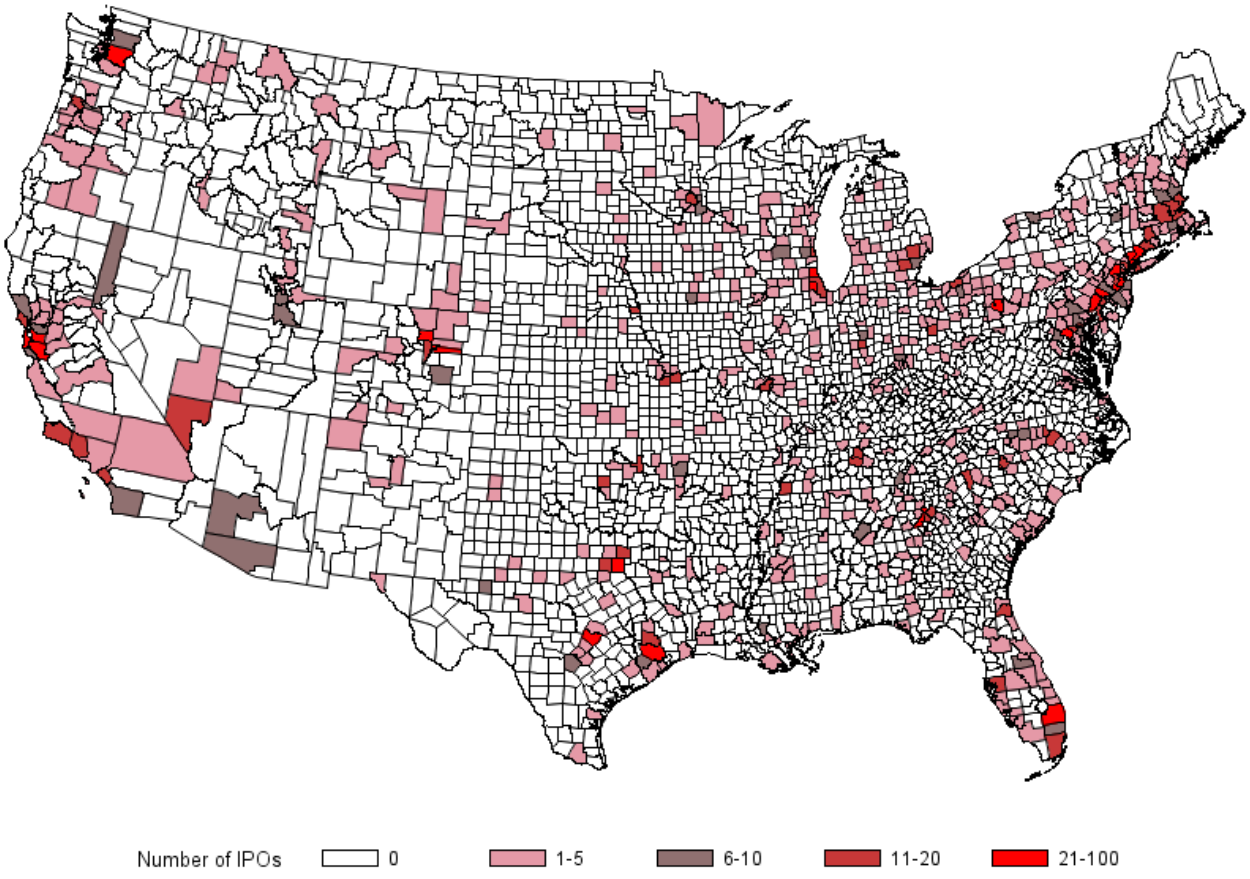
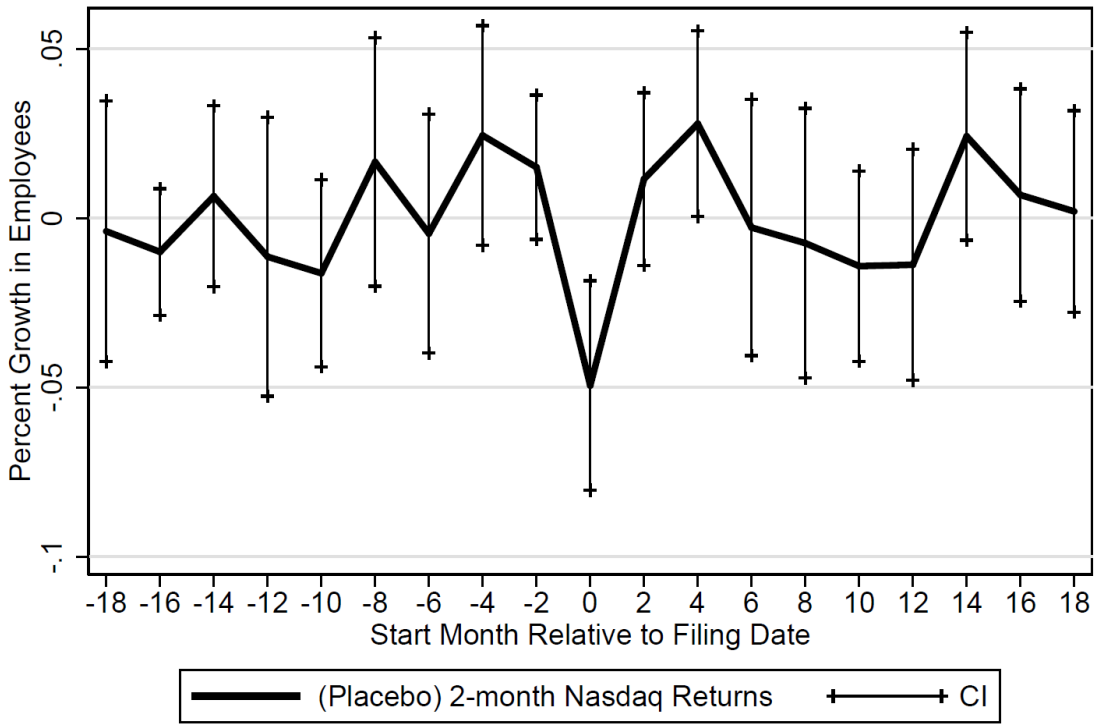


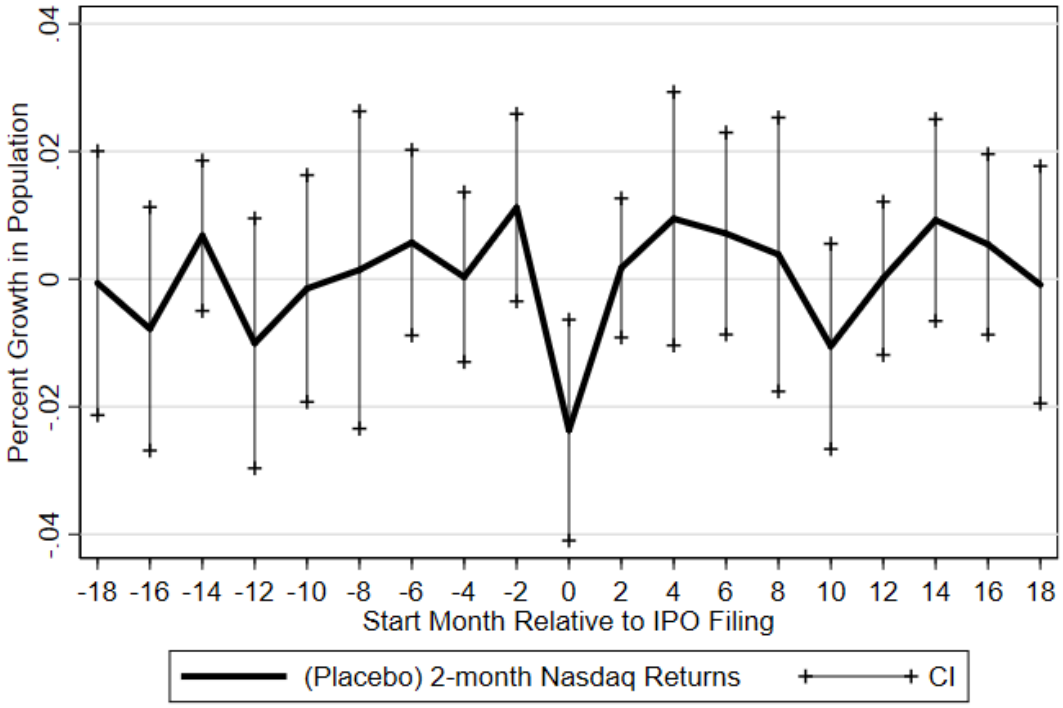
Figure 3: Placebo Robustness Test of NASDAQ Returns – Plot of 18 different windows

Panel A plots beta coefficients from nineteen different regressions, with five-year cumulative growth in county-level employees as the dependent variable in each. In each regression, employee growth is estimated as a function of two-month NASDAQ returns, in addition to the same county and IPO control variables used in Column 1-2 of Table 4. Each regression uses a different window of two month NASDAQ returns, varying the number of months before or after the filing date of each IPO in the sample that the NASDAQ return window begins. The start date of the NASDAQ 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 NASDAQ 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 NASDAQ returns beginning two months *after* each IPO filing for NASDAQ returns beginning at the filing date. Vertical lines at each point represent 95% confidence intervals for the coefficient on the variable representing two-month NASDAQ returns. Panels B and C are identical to Panel A, except that the dependent variables are five-year cumulative growth in county-level population and per capita income, respectively.

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



Panel B: Two-month NASDAQ Returns and Future Population Growth




Panel C: Two-month NASDAQ Returns and Future Per Capita Income Growth

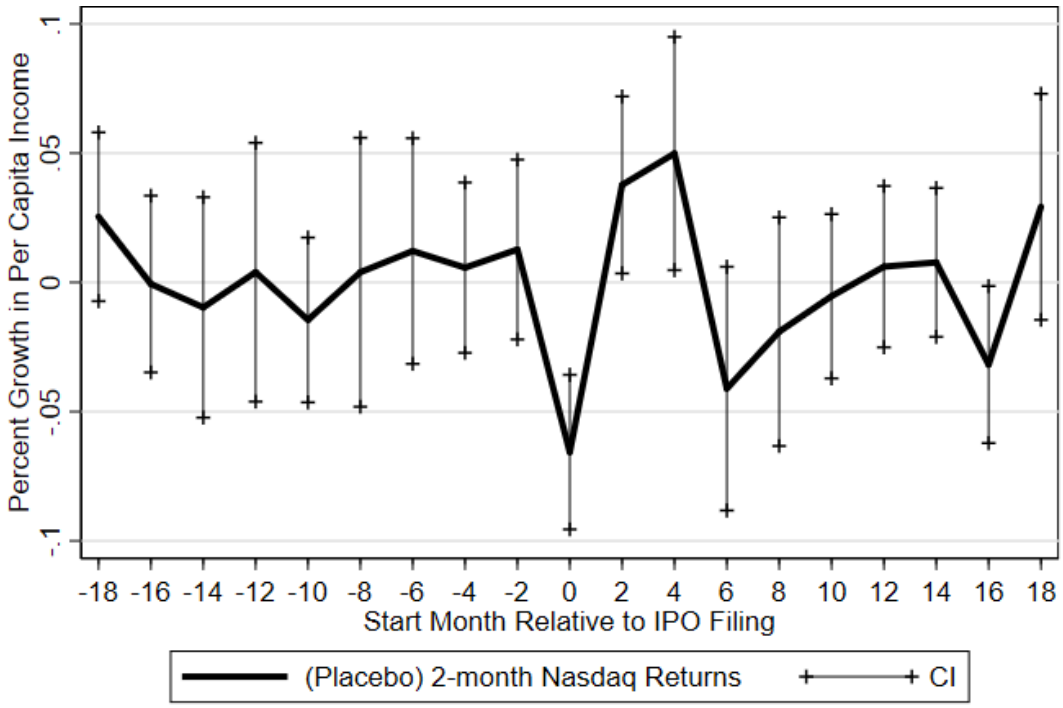


Figure 4: County Employment Growth surrounding IPO Filings

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 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.

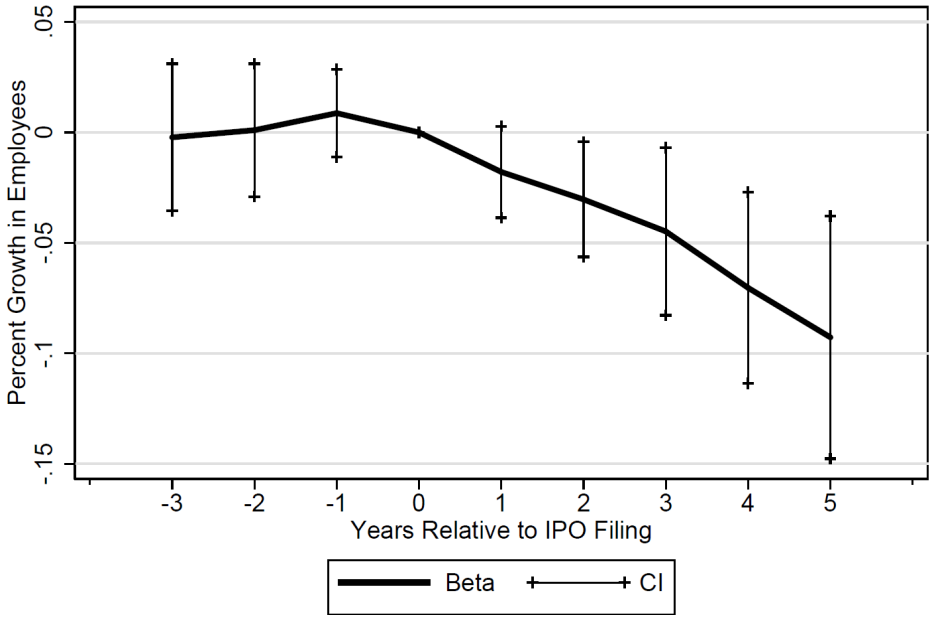
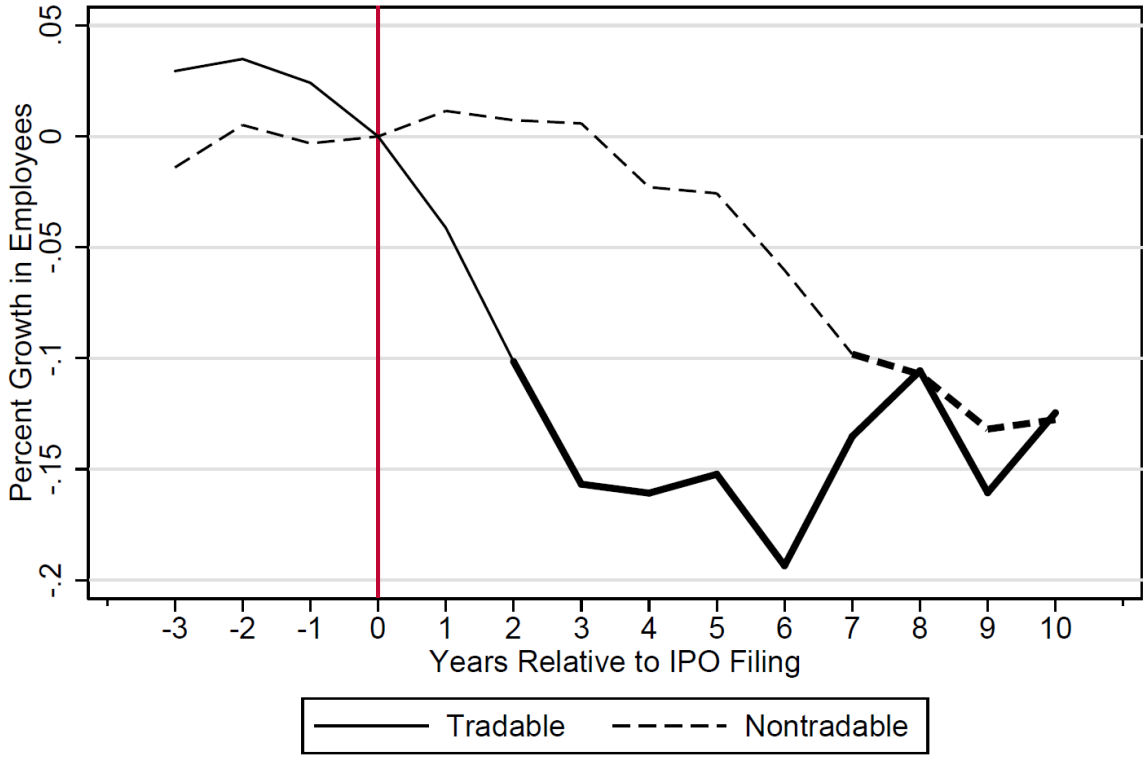


Figure 5: Industrial Decomposition of County Employment Growth surrounding IPO Filings

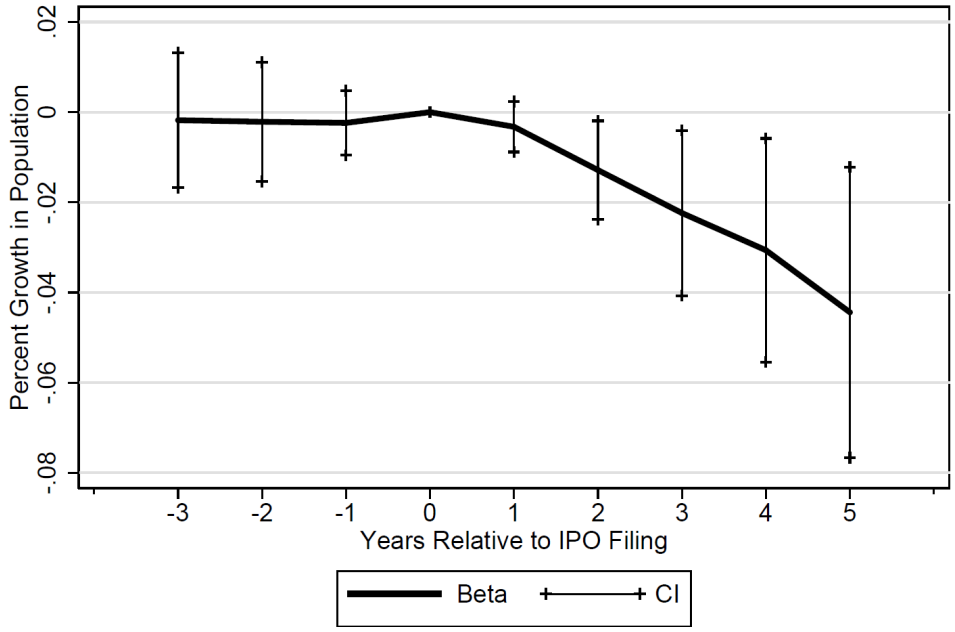
This figure plots the industrial decomposition of the evolution of employment in counties with completed IPOs, compared to counties with withdrawn IPOs. The figure covers three years before IPO filings until ten years after. Each point on each of the two lines represents a coefficient on the instrumented 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-tradeable 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. The respective lines plotting the regression coefficients for each industry group are bold when the coefficient on the IV is significant at the 5% level. The vertical line at 0 represents the IPO filing year.



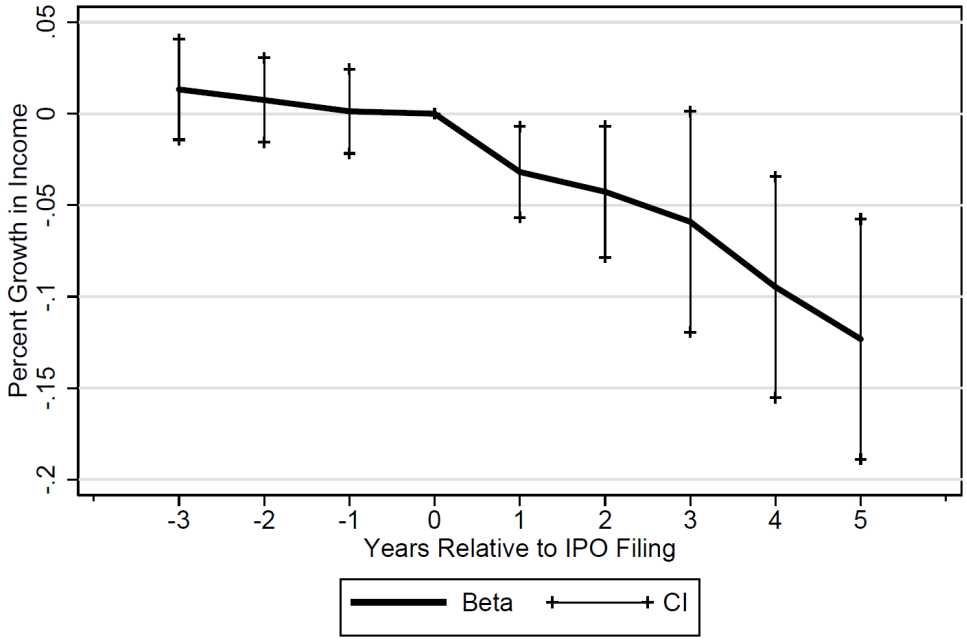
## Figure 6: County Employment Growth surrounding IPO Filings

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.

Panel A: Population



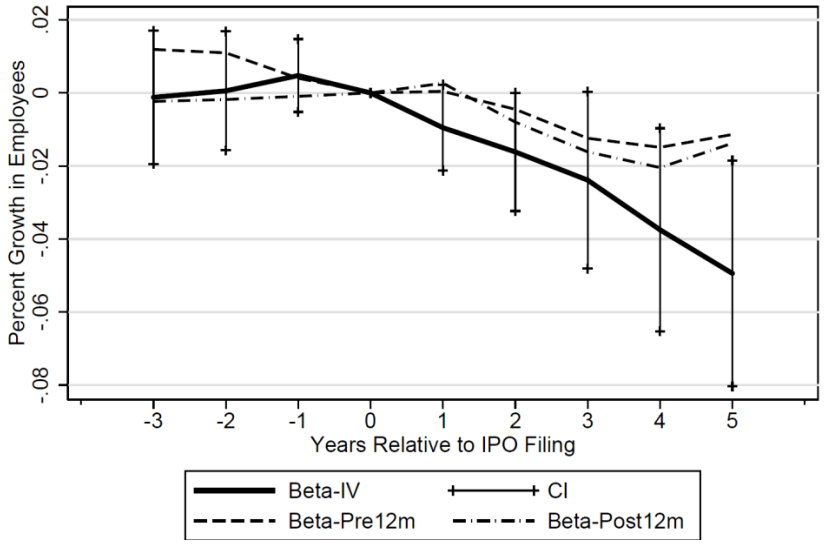

Panel B: Personal Income



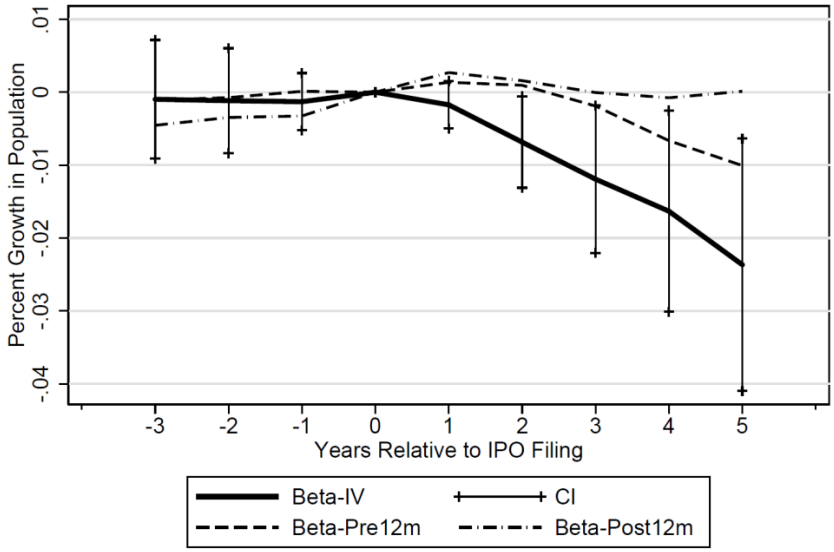
## Figure 7: Placebo Test of NASDAQ Returns – County Growth relationship

These figures plot the evolution of the number of employees, 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 NASDAQ returns surrounding the IPO filing date. The solid line plots coefficients from regressions that are almost identical to the second stage regressions in Tables 4, 7, & 8, but with post-filing 2-month NASDAQ returns replacing the instrumented IPO completion variable (i.e., reduced-form IV regression). 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. The dashed line plots the same regressions, but with the main explanatory variable being a placebo two-month compounded NASDAQ return beginning 12 months prior to the IPO filing. And the dash-dotted line plots regression coefficients for a similar set of regressions, where the main explanatory variable is a placebo two-month compounded NASDAQ 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 main IV, two-month NASDAQ returns beginning at the IPO filing.

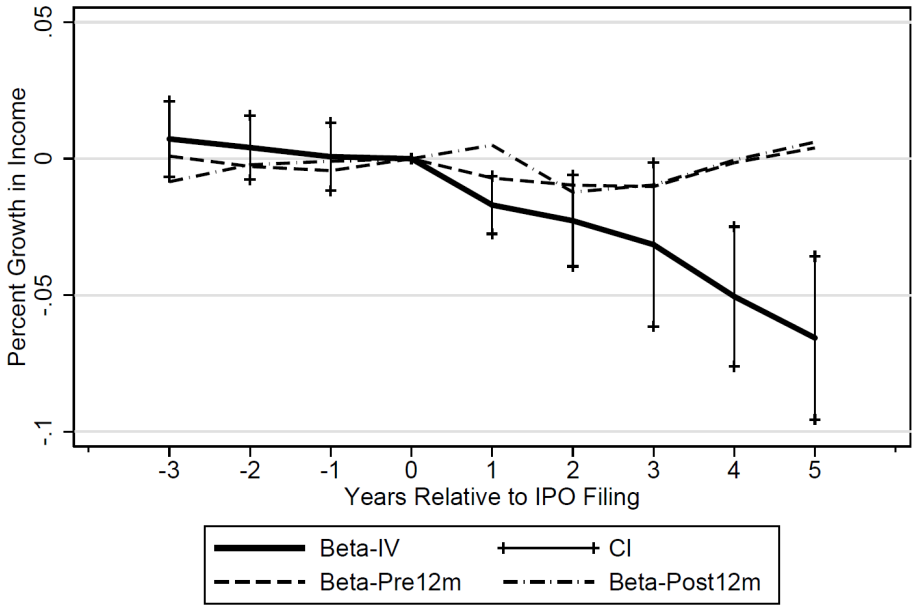
Panel A: Employees




Panel B: Population



Panel C: 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, and is restricted to the top tercile of IPO filings (i.e., top third using the value of shares filed relative to the total number of pre-filing employees in the county). 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 county-year (e.g., a county with an IPO in previous or future years, but not the current, 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 (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

| IPO Characteristics | Full Sample | Completed IPOs | Withdrawn IPOs |
|---|---|---|---|
| Proceeds Filed | 190.85 | 186.27 | 207.04 |
| PE/VC Funding | 0.39 | 0.39 | 0.39 |
| Underwriter Reputation | 7.99 | 7.97 | 8.05 |
| Number Lead Managers | 1.44 | 1.42 | 1.56 |
| N | 2862 | 2231 | 631 |

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

| Economic Variables | IPO Filings | No IPO Filings | Difference |
|---|---|---|---|
| Population | 632,172 | 74,792 | 557,380*** |
| Employees | 420,669 | 40,642 | 380,027*** |
| Real Income per Capita | 42,253 | 29,983 | 12,271*** |
| | | | |
| Lagged Population Growth | 1.32% | 0.56% | 0.76%*** |
| Lagged Employee Growth | 1.96% | 1.07% | 0.89%*** |
| Lagged Real Income Growth | 5.89% | 5.08% | 0.81%*** |
| | | | |
| Population Growth 5yr | 6.54% | 2.98% | 3.56%*** |
| Employee Growth 5yr | 9.86% | 5.35% | 4.51%*** |
| Real Income Growth 5yr | 22.85% | 22.76% | 0.01% |
| N | 2,038 | 78,801 | |

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

| Economic Variables | Completed IPOs | Withdrawn IPOs | Difference |
|---|---|---|---|
| Population | 592,592 | 614,171 | -21,578 |
| Employees | 388,723 | 399,141 | -10,417 |
| Real Income per Capita | 40,704 | 43,500 | -2,795*** |
| | | | |
| Lagged Population Growth | 1.37% | 1.19% | 0.18%** |
| Lagged Employee Growth | 2.09% | 1.59% | 0.50%*** |
| Lagged Real Income Growth | 5.95% | 5.32% | 0.63%*** |
| | | | |
| Population Growth 5yr | 6.89% | 5.65% | 1.24%*** |
| Employee Growth 5yr | 10.67% | 7.31% | 3.35%*** |
| Real Income Growth 5yr | 23.94% | 19.01% | 4.93%*** |
| N | 1,492 | 344 | |

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 4 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 5 is the annual geometric average growth rate in a county's population over the five years following an IPO filing, while the dependent variable in Columns 3 and 6 is the annual geometric average growth rate in a county's per-capita personal income, over this same period. Columns 4-6 weight regressions by the number of IPOs filed within a county-year. 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) Employees | (2) Population | (3) Personal Income | (4) Employees (Weighted) | (5) Population (Weighted) | (6) Personal Income (Weighted) |
|---|---|---|---|---|---|---|
| **IPO Completion** | **0.0007*** | **0.0002** | **0.0003** | **0.0013***** | **0.0004** | **0.0007** |
| | **(1.82)** | **(0.65)** | **(0.60)** | **(3.10)** | **(1.14)** | **(1.35)** |
| Ln(Employees) | -0.0096*** | -0.0021** | 0.0021 | -0.0104*** | -0.0022* | 0.0021 |
| | (-5.13) | (-2.17) | (0.97) | (-5.75) | (-1.81) | (1.32) |
| Ln(Population) | 0.0096*** | 0.0024** | -0.0029 | 0.0103*** | 0.0026** | -0.0028* |
| | (5.00) | (2.47) | (-1.40) | (5.67) | (2.02) | (-1.74) |
| Ln(Income) | 0.0075*** | 0.0013 | 0.0099*** | 0.0071*** | 0.0009 | 0.0080*** |
| | (3.33) | (1.12) | (3.20) | (3.15) | (0.71) | (2.75) |
| Pop Growth | 0.6177*** | 0.7211*** | -0.0198 | 0.5868*** | 0.7080*** | -0.0512 |
| | (8.70) | (18.04) | (-0.41) | (9.12) | (19.02) | (-1.34) |
| Employee Growth | 0.1603*** | 0.0397** | 0.0105 | 0.1644*** | 0.0441*** | 0.0338 |
| | (4.97) | (2.41) | (0.35) | (5.34) | (2.69) | (1.23) |
| Income Growth | -0.0390 | 0.0038 | -0.0431 | -0.0101 | 0.0128 | -0.0322 |
| | (-1.23) | (0.27) | (-1.51) | (-0.47) | (1.07) | (-1.62) |
| Ln(Number of IPOs) | 0.0000 | -0.0005 | 0.0037 | 0.0018 | 0.0000 | 0.0042*** |
| | (0.01) | (-0.52) | (1.64) | (1.13) | (0.02) | (2.70) |
| Ln(IPO Size) | -0.0002 | -0.0004* | 0.0006 | -0.0002 | -0.0002 | 0.0004 |
| | (-0.73) | (-1.89) | (1.56) | (-0.63) | (-1.12) | (1.11) |
| Number Lead | 0.0004* | 0.0003 | -0.0001 | 0.0002 | 0.0003 | 0.0002 |
| Managers | (1.74) | (1.33) | (-0.33) | (1.45) | (1.13) | (0.39) |
| PE/VC Funding | 0.0004 | 0.0001 | 0.0006 | 0.0008* | 0.0001 | 0.0008 |
| | (1.00) | (0.19) | (0.95) | (1.86) | (0.42) | (1.14) |
| Underwriter | -0.0001 | 0.0000 | -0.0002 | -0.0000 | 0.0000 | -0.0002 |
| Reputation | (-0.59) | (0.40) | (-1.33) | (-0.15) | (0.06) | (-0.86) |
| Year Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-squared | 0.634 | 0.743 | 0.499 | 0.615 | 0.738 | 0.484 |
| Observations | 2,862 | 2,862 | 2,862 | 2,862 | 2,862 | 2,862 |

Table 3: First Stage Estimation of IPO Completion

This table presents estimates for our first stage regression predicting IPO completion. Columns 1 and 3 present estimates from unweighted OLS regressions, while Columns 2 and 4 weight regressions by the number of IPOs filed in the county-year. NASDAQ Ret. is the returns of the NASDAQ index in the two-months following an IPO filing. 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) Completed | (2) Completed (weighted) | (3) Completed | (4) Completed (weighted) |
|---|---|---|---|---|
| **NASDAQ Ret.** | **0.5600*** ** | **0.5328*** ** | **0.5615*** ** | **0.5330*** ** |
| | **(6.04)** | **(4.73)** | **(6.02)** | **(4.73)** |
| Ln(Employees) | | | 0.0691* | 0.0716 |
| | | | (1.72) | (1.39) |
| Ln(Population) | | | -0.0690 | -0.0709 |
| | | | (-1.62) | (-1.33) |
| Ln(Income) | | | -0.0083 | -0.0027 |
| | | | (-0.21) | (-0.06) |
| Pop Growth | | | 0.6842 | 0.5374 |
| | | | (0.88) | (0.73) |
| Employee Growth | | | 0.2534 | 0.2724 |
| | | | (0.48) | (0.48) |
| Income Growth | | | 0.1670 | 0.0335 |
| | | | (0.47) | (0.10) |
| Ln(Number of IPOs) | -0.0132 | -0.0324* | -0.0205 | -0.0409* |
| | (-0.73) | (-1.75) | (-0.98) | (-1.85) |
| Ln(IPO Size) | -0.0090 | -0.0021 | -0.0105 | -0.0043 |
| | (-0.59) | (-0.15) | (-0.62) | (-0.28) |
| Number Lead Managers | 0.0560*** | 0.0531*** | 0.0551*** | 0.0528*** |
| | (4.62) | (3.76) | (4.63) | (3.80) |
| PE/VC Funding | 0.0954*** | 0.1100*** | 0.0940*** | 0.1090*** |
| | (5.05) | (4.65) | (4.96) | (4.64) |
| Underwriter Reputation | -0.0023 | -0.0057 | -0.0028 | -0.0064 |
| | (-0.32) | (-0.77) | (-0.40) | (-0.89) |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes |
| Adj. R-squared | 0.126 | 0.139 | 0.126 | 0.139 |
| Observations | 2,862 | 2,862 | 2,862 | 2,862 |

Table 4: IPOs and Local Employee and Establishment Growth

This table presents second-stage 2SLS estimates where the explanatory variable of interest is the fitted value of IPO completion, as estimated from the matching column from Table 3. The dependent variable in Panel A is the annual geometric average growth rate in a county's total number of employees over the five years after an IPO filing, while dependent variable in Panel B is the annual geometric average growth rate in a county's number of establishments over this period. We winsorize all dependent variables at the extreme 1%. Regressions in Columns 2 and 4 of each Panel are weighted by the number of IPOs filed in the county-year. 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.

Panel A: Employee Growth

| | (1) Employees | (2) Employees (Weighted) | (3) Employees | (4) Employees (Weighted) |
|---|---|---|---|---|
| **Instrumented IPO** | **-0.0146**** | **-0.0181***** | **-0.0119**** | **-0.0172***** |
| **Completion** | **(-2.19)** | **(-2.78)** | **(-2.28)** | **(-3.36)** |
| Ln(Employees) | | | -0.0088*** | -0.0090*** |
| | | | (-4.79) | (-4.20) |
| Ln(Population) | | | 0.0088*** | 0.0090*** |
| | | | (4.59) | (4.04) |
| Ln(Income) | | | 0.0074*** | 0.0070*** |
| | | | (3.02) | (2.64) |
| Pop Growth | | | 0.6268*** | 0.5985*** |
| | | | (9.27) | (9.56) |
| Employee Growth | | | 0.1628*** | 0.1680*** |
| | | | (5.23) | (5.65) |
| Income Growth | | | -0.0368 | -0.0092 |
| | | | (-1.18) | (-0.42) |
| Ln(Number of IPOs) | -0.0017 | -0.0004 | -0.0002 | 0.0011 |
| | (-0.58) | (-0.21) | (-0.10) | (0.65) |
| Ln(IPO Size) | -0.0026*** | -0.0022*** | -0.0004 | -0.0003 |
| | (-3.57) | (-2.86) | (-1.05) | (-0.68) |
| Number Lead Managers | 0.0017*** | 0.0017*** | 0.0011*** | 0.0012*** |
| | (3.08) | (2.73) | (2.80) | (2.59) |
| PE/VC Funding | 0.0026*** | 0.0038*** | 0.0016** | 0.0028*** |
| | (2.66) | (3.67) | (2.00) | (3.06) |
| Underwriter Reputation | -0.0000 | -0.0000 | -0.0001 | -0.0002 |
| | (-0.02) | (-0.02) | (-0.87) | (-0.84) |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes |
| Observations | 2,862 | 2,862 | 2,862 | 2,862 |

Panel B: Establishment Growth

| | (1) Establishments | (2) Establishments (Weighted) | (3) Establishments | (4) Establishments (Weighted) |
|---|---|---|---|---|
| **Instrumented IPO** | **-0.0100*** | **-0.0086** | **-0.0067*** | **-0.0070*** |
| **Completion** | **(-1.87)** | **(-1.60)** | **(-1.69)** | **(-1.96)** |
| Ln(Employees) | | | -0.0021 | -0.0017 |
| | | | (-1.13) | (-0.77) |
| Ln(Population) | | | 0.0106*** | 0.0118*** |
| | | | (3.21) | (3.71) |
| Ln(Income) | | | 0.0075** | 0.0083** |
| | | | (2.03) | (2.37) |
| Pop Growth | | | 0.6509*** | 0.6037*** |
| | | | (11.64) | (12.74) |
| Employee Growth | | | 0.0917*** | 0.1074*** |
| | | | (3.78) | (5.24) |
| Income Growth | | | -0.0051 | 0.0092 |
| | | | (-0.22) | (0.55) |
| Ln(Establishments) | | | -0.0093** | -0.0110*** |
| | | | (-2.27) | (-2.97) |
| Establishment Growth | | | 0.0691*** | 0.0696** |
| | | | (2.76) | (2.57) |
| Ln(Number of IPOs) | -0.0013 | -0.0006 | 0.0009 | 0.0016 |
| | (-0.54) | (-0.34) | (0.60) | (1.37) |
| Ln(IPO Size) | -0.0028*** | -0.0022*** | -0.0003 | -0.0001 |
| | (-5.03) | (-4.59) | (-0.74) | (-0.14) |
| Number Lead Managers | 0.0018*** | 0.0015*** | 0.0011*** | 0.0010*** |
| | (3.84) | (3.23) | (3.30) | (2.79) |
| PE/VC Funding | 0.0018* | 0.0021** | 0.0009 | 0.0013* |
| | (1.79) | (2.12) | (1.29) | (1.68) |
| Underwriter Reputation | 0.0001 | 0.0001 | 0.0000 | -0.0000 |
| | (0.52) | (0.32) | (0.21) | (-0.07) |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes |
| Observations | 2,862 | 2,862 | 2,862 | 2,862 |

Table 5: Heterogeneous effect of IPOs on Local Employment Growth

This table presents second-stage 2SLS estimates where the explanatory variables of interest are the fitted value of IPO completion, as estimated from Column 3 of Table 3, and its interaction with measures of county size, county income, and the size of the IPO. County Size is measured either using the log of lagged employment or population levels (Columns 1 and 2); county income is measured using the lagged level of nationally-adjusted real wages and salary (Column 3); and IPO size is computed as total IPO proceeds filed for scaled by lagged county employment (Column 4). The dependent variable in each column is the annual geometric average growth rate in total employment in counties with an IPO filing over the five years following an IPO filing. 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. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

| | (1) Employee Growth (weighted) | (2) Employee Growth (weighted) | (3) Employee Growth (weighted) | (4) Employee Growth (weighted) |
|---|---|---|---|---|
| Instrumented IPO Completion | **-0.1549**** | **-0.1548***** | **-0.1432***** | **-0.0070** |
| | **(-2.39)** | **(-2.72)** | **(-3.05)** | **(-1.40)** |
| Instrumented IPO Completion* Ln(Employees) | **0.0114**** | | | |
| | **(2.18)** | | | |
| Instrumented IPO Completion* Ln(Population) | | **0.0110**** | | |
| | | **(2.47))** | | |
| Instrumented IPO Completion* Ln(Relative Wage) | | | **0.1451***** | |
| | | | **(2.74)** | |
| Instrumented IPO Completion* IPO Size | | | | **-0.0229*** |
| | | | | **(-1.81)** |
| Controls | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes |
| Adj. R-squared | 0.371 | 0.393 | 0.312 | 0.493 |
| Observations | 2,862 | 2,862 | 2,862 | 2,862 |

Table 6: Decomposition of County Level Employee Growth

This table presents second-stage 2SLS estimates where the explanatory variable of interest is the fitted value of IPO completion, as estimated from Column 4 of Table 3. The dependent variables in Columns 1 and 2 are the annual geometric average growth rate in a county's tradable sector employees over the five and ten years after an IPO filing, respectively. The dependent variables in Columns 3 and 4 are the annual geometric average growth rate in a county's non-tradable employees over these same two periods. Tradable sector is defined as manufacturing and construction industries (NAICS sectors 23 and 31-33), and non-tradable sector is defined as all other industries, minus agriculture, mining, and public administration. 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 ofpre-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) Employees (Tradable) 5Yr | (2) Employees (Tradable) 10Yr | (3) Employees (Non-Tradable) 5Yr | (4) Employees (Non-Tradable) 10Yr |
|---|---|---|---|---|
| **Instrumented PO** | **-0.0316*** ** | **-0.0247** | **-0.0046** | **-0.0171*** |
| **Completion** | **(-3.17)** | **(-1.51)** | **(-0.62)** | **(-1.88)** |
| Ln(Employees) | -0.0112** | -0.0333*** | -0.0071*** | -0.0252*** |
| | (-2.55) | (-4.45) | (-2.98) | (-3.95) |
| Ln(Population) | 0.0099** | 0.0308*** | 0.0054** | 0.0183*** |
| | (2.04) | (3.58) | (2.22) | (3.05) |
| Ln(Income) | 0.0044 | 0.0248*** | 0.0081*** | 0.0175*** |
| | (0.73) | (3.63) | (2.75) | (2.73) |
| Pop Growth | 0.4464*** | 0.8919*** | 0.6819*** | 1.2875*** |
| | (3.72) | (5.11) | (9.34) | (9.67) |
| Employee Growth | 0.3085*** | 0.4421*** | 0.1439*** | 0.1365* |
| | (4.90) | (6.33) | (2.89) | (1.69) |
| Income Growth | -0.0268 | -0.0629 | 0.0158 | 0.0654 |
| | (-0.68) | (-1.16) | (0.60) | (1.48) |
| Ln(Number of IPOs) | 0.0030 | -0.0031 | 0.0003 | 0.0009 |
| | (1.03) | (-0.60) | (0.15) | (0.21) |
| Ln(IPO Size) | -0.0003 | -0.0016 | 0.0005 | -0.0003 |
| | (-0.23) | (-1.23) | (1.11) | (-0.27) |
| Number Lead Managers | 0.0020*** | 0.0018 | 0.0003 | 0.0012 |
| | (2.62) | (0.77) | (0.44) | (0.99) |
| PE/VC Funding | 0.0070*** | 0.0047 | 0.0005 | 0.0016 |
| | (3.42) | (1.44) | (0.41) | (1.04) |
| Underwriter Reputation | -0.0012*** | -0.0017** | 0.0001 | 0.0006 |
| | (-2.60) | (-2.05) | (0.48) | (1.00) |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes |
| Adj. R-squared | 0.032 | 0.262 | 0.401 | 0.394 |
| Observations | 2,507 | 2,082 | 2,508 | 2,083 |

Table 7: IPOs and Local Population and Unemployment Growth

This table presents second-stage 2SLS estimates where the explanatory variable of interest is the fitted value of IPO completion, as estimated from Column 4 of Table 3. The dependent variable in Columns 1 and 2 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, while dependent variable in Columns 3 and 4 is the annual geometric average growth rate in the unemployment rate (i.e., unemployed divided by employed plus unemployed) over this period. We winsorize all dependent variables at the extreme 1%. We winsorize all dependent variables at the extreme 1%. Regressions in Columns 2 and 4 are weighted by the number of IPOs filed in the county-year. 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 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) Population (Weighted) | (3) Unemployment Rate | (4) Unemployment Rate (Weighted) |
|---|---|---|---|---|
| **Instrumented IPO Completion** | **-0.0060**** | **-0.0080***** | **0.0046** | **0.0113** |
| | **(-2.08)** | **(-2.60)** | **(0.24)** | **(0.49)** |
| Ln(Employees) | -0.0016* | -0.0016 | 0.0101 | 0.0097 |
| | (-1.77) | (-1.23) | (1.09) | (1.04) |
| Ln(Population) | 0.0020** | 0.0020 | -0.0086 | -0.0082 |
| | (2.06) | (1.44) | (-0.89) | (-0.83) |
| Ln(Income) | 0.0012 | 0.0008 | -0.0254*** | -0.0286*** |
| | (1.02) | (0.63) | (-2.88) | (-3.23) |
| Pop Growth | 0.7256*** | 0.7133*** | -0.1110 | -0.0289 |
| | (18.44) | (19.48) | (-0.83) | (-0.22) |
| Employee Growth | 0.0410*** | 0.0457*** | 0.1576 | 0.1572* |
| | (2.58) | (2.96) | (1.50) | (1.91) |
| Income Growth | 0.0048 | 0.0132 | 0.1508*** | 0.0816* |
| | (0.34) | (1.03) | (2.71) | (1.84) |
| Ln(Number of IPOs) | -0.0006 | -0.0003 | -0.0055 | -0.0027 |
| | (-0.66) | (-0.36) | (-1.27) | (-0.72) |
| Ln(IPO Size) | -0.0005** | -0.0003 | -0.0015 | -0.0025** |
| | (-2.22) | (-1.24) | (-1.32) | (-2.09) |
| Number Lead Managers | 0.0006** | 0.0007** | -0.0002 | -0.0005 |
| | (2.40) | (2.17) | (-0.15) | (-0.30) |
| PE/VC Funding | 0.0006 | 0.0011* | -0.0021 | -0.0029 |
| | (1.12) | (1.79) | (-0.79) | (-0.82) |
| Underwriter Reputation | 0.0000 | -0.0001 | -0.0006 | -0.0002 |
| | (0.18) | (-0.47) | (-0.91) | (-0.31) |
| Unemployment Growth | | | -0.0086*** | -0.0080*** |
| | | | (-2.79) | (-2.90) |
| Unemployment Rate | | | -0.0446*** | -0.0404*** |
| | | | (-3.02) | (-2.76) |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes |
| Observations | 2,862 | 2,862 | 2,472 | 2,472 |

Table 8: IPOs and Local Income and Earnings Growth

This table presents second-stage 2SLS estimates where the explanatory variable of interest is the fitted value of IPO completion, as estimated from Column 4 of Table 3. The dependent variable in Columns 1 and 2 is the annual geometric average growth rate in a county's personal income over the five years after an IPO filing relative to the IPO filing year, and the dependent variable in Columns 3 and 4 is the annual geometric average growth rate in a county's average wages and salary per job over this period. We winsorize all dependent variables at the extreme 1%. Regressions in Columns 2 and 4 are weighted by the number of IPOs filed in the county-year. 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. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

| | (1) Income | (2) Income (Weighted) | (3) Wages | (4) Wages (Weighted) |
|---|---|---|---|---|
| **Instrumented IPO Completion** | **-0.0160*** ** | **-0.0214*** ** | **-0.0056*** | **-0.0089** ** |
| | **(-3.15)** | **(-3.67)** | **(-1.94)** | **(-2.45)** |
| Ln(Employees) | 0.0032 | 0.0037* | 0.0055*** | 0.0057*** |
| | (1.52) | (1.67) | (3.24) | (5.47) |
| Ln(Population) | -0.0040* | -0.0044* | -0.0051*** | -0.0051*** |
| | (-1.93) | (-1.91) | (-3.23) | (-4.45) |
| Ln(Income) | 0.0098*** | 0.0079*** | 0.0089*** | 0.0104*** |
| | (3.25) | (2.63) | (3.44) | (4.08) |
| Pop Growth | -0.0080 | -0.0373 | 0.1129*** | 0.0699** |
| | (-0.17) | (-0.95) | (3.26) | (2.18) |
| Employee Growth | 0.0137 | 0.0382 | 0.0383* | 0.0542*** |
| | (0.45) | (1.29) | (1.71) | (2.60) |
| Income Growth | -0.0404 | -0.0310 | 0.0287* | 0.0116 |
| | (-1.44) | (-1.46) | (1.70) | (0.84) |
| Ln(Number of IPOs) | 0.0034 | 0.0033** | 0.0043*** | 0.0030* |
| | (1.50) | (1.97) | (2.95) | (1.96) |
| Ln(IPO Size) | 0.0003 | 0.0003 | -0.0001 | -0.0000 |
| | (0.74) | (0.54) | (-0.36) | (-0.03) |
| Number Lead Managers | 0.0008* | 0.0014** | -0.0005 | 0.0000 |
| | (1.65) | (2.36) | (-1.34) | (0.02) |
| PE/VC Funding | 0.0021** | 0.0032** | 0.0012*** | 0.0013** |
| | (2.09) | (2.43) | (2.71) | (2.43) |
| Underwriter Reputation | -0.0003 | -0.0003 | 0.0001 | -0.0000 |
| | (-1.56) | (-1.38) | (0.47) | (-0.32) |
| Ln(Wages) | | | -0.0005 | -0.0052 |
| | | | (-0.08) | (-1.04) |
| Wage Growth | | | -0.1090 | 0.0039 |
| | | | (-1.56) | (0.07) |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes |
| Observations | 2,862 | 2,862 | 2,862 | 2,862 |

Table 9: 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, as estimated from Column 4 of Table 3, 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 x Ln(Wages)** | | | **0.882** | **1.451***** |
| | | | **(1.366)** | **(2.794)** |
| Ln(Employees) | 0.003 | -0.025 | -0.000 | -0.032* |
| | (0.152) | (-1.479) | (-0.006) | (-1.661) |
| Ln(Population) | 0.003 | 0.031* | 0.005 | 0.036** |
| | (0.127) | (1.864) | (0.290) | (2.462) |
| Ln(Income) | 0.024 | 0.020 | -0.005 | -0.032 |
| | (1.383) | (0.754) | (-0.192) | (-0.792) |
| Pop Growth | -0.076 | -0.170 | -0.160 | -0.259 |
| | (-0.202) | (-0.478) | (-0.432) | (-0.666) |
| Employee Growth | -0.130 | -0.201 | 0.020 | 0.043 |
| | (-1.036) | (-0.810) | (0.108) | (0.135) |
| Income Growth | -0.099 | -0.130 | -0.096 | -0.100 |
| | (-0.463) | (-0.634) | (-0.406) | (-0.382) |
| Ln(Number of IPOs) | 0.002 | -0.005 | -0.000 | -0.010 |
| | (0.104) | (-0.303) | (-0.015) | (-0.641) |
| Ln(IPO Size) | 0.012** | 0.017** | 0.007 | 0.009 |
| | (2.298) | (2.475) | (0.891) | (1.088) |
| Number of Lead Managers | 0.019*** | 0.006 | 0.021** | 0.008 |
| | (2.866) | (0.931) | (2.519) | (1.237) |
| PE/VC Funding | 0.033*** | 0.034*** | 0.035*** | 0.037*** |
| | (4.117) | (5.800) | (3.721) | (4.615) |
| Underwriter Reputation | -0.010*** | -0.004 | -0.011*** | -0.005 |
| | (-2.747) | (-1.337) | (-2.584) | (-1.515) |
| Observations | 1,800 | 1,800 | 1,800 | 1,800 |

Table 10: 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 measured as 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 2,862 IPOs, we match 778 IPOs that have two consecutive data points of 10K state name counts at any point between the year of the issue date and the third year following the IPO's issue date. We then use the earliest two consecutive name counts to compute a one-year growth measure that is winsorized at the 1% and 99% levels. The dependent variable is thus the (earliest) post-IPO one-year growth in 10K state-name counts. The main explanatory variable in Column 1 is the log of per-capita personal income of the home county of an IPO in the year prior to the IPO filing, while the main explanatory variable in Column 2 is the log of relative average wages in the county, benchmarked to average wages across all counties in that year. Each regression is weighted by the number of IPOs filed in the county-year. All variables are defined in Appendix A. Standard errors are clustered at the county and two-digit SIC levels. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.

| | (1) Geographic Dispersion | (2) Geographic Dispersion |
|---|---|---|
| **Ln(Income)** | **-0.201**** | |
| | **(-2.50)** | |
| **Ln(Relative Wage)** | | **-0.345**** |
| | | **(-2.11)** |
| Ln(Employees) | 0.072 | 0.059 |
| | (0.60) | (0.56) |
| Ln(Population) | -0.051 | -0.036 |
| | (-0.41) | (-0.33) |
| Pop Growth | -4.712** | -4.770*** |
| | (-2.47) | (-2.59) |
| Employee Growth | 1.509 | 1.156 |
| | (1.11) | (0.84) |
| Income Growth | -0.778 | -0.744 |
| | (-1.58) | (-1.46) |
| Ln(Number of IPOs) | 0.017 | 0.013 |
| | (0.28) | (0.23) |
| Ln(IPO Size) | -0.010 | -0.012 |
| | (-0.16) | (-0.19) |
| Number Lead Managers | 0.013 | 0.014 |
| | (0.34) | (0.38) |
| PE/VC Funding | 0.002 | -0.006 |
| | (0.04) | (-0.11) |
| Underwriter Reputation | -0.013 | -0.012 |
| | (-0.57) | (-0.52) |
| Year Fixed Effects | Yes | Yes |
| Industry Fixed Effects | Yes | Yes |
| Observations | 778 | 778 |

Table 11: Reduced Form and Placebo Analyses

This table presents OLS estimates from regressions relating future county-level economic growth to 2-month NASDAQ returns. In Panel A, the dependent variable is the annual geometric average growth rate in a county's number of employees over the five years after an IPO filing relative to the IPO filing year. In Panel B, the dependent variable is the annual geometric average growth rate in a county's population over the five years after an IPO filing relative to the IPO filing year. And in Panel C, the dependent variable is the annual geometric average growth rate in a county's per-capita personal income over the five years after an IPO filing relative to the IPO filing year. In each panel, the main explanatory variable in Column 1 is the compounded 2-month NASDAQ return following an IPO filing (i.e., our IV), while the main explanatory variable in Columns 2 and 3 are the compounded 2-month NASDAQ returns beginning 12 months prior to an IPO filing, and 12 months following an IPO filing (i.e., placebo IVs). Control variables are the same as those used in Column 4 of Table 4. We winsorize all dependent variables at the extreme 1%. All regressions are weighted by the number of IPOs filed in the county-year. 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. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Employees Placebos

| | (1) Employees | (2) Employees | (3) Employees |
|---|---|---|---|
| **Post-Filing 2-month Nasdaq** | **-0.0092*** ** | | |
| | **(-3.23)** | | |
| Placebo 2-month NASDAQ (-1Yr) | | -0.0018 | |
| | | (-0.48) | |
| Placebo 2-month NASDAQ (+1Yr) | | | -0.0024 |
| | | | (-0.72) |
| Control Variables | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes |
| Adj. R-squared | 0.616 | 0.615 | 0.615 |
| Observations | 2,862 | 2,862 | 2,862 |

Panel B: Population Placebos

| | (1) Population | (2) Population | (3) Population |
|---|---|---|---|
| **Post-Filing 2-month Nasdaq** | **-0.004** ** | | |
| | **(-2.56)** | | |
| Placebo 2-month NASDAQ (-1Yr) | | -0.002 | |
| | | (-0.86) | |
| Placebo 2-month NASDAQ (+1Yr) | | | -0.000 |
| | | | (-0.01) |
| Control Variables | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes |
| Adj. R-squared | 0.738 | 0.737 | 0.737 |
| Observations | 2,862 | 2,862 | 2,862 |

Panel C: Income Placebos

| | (1)<br>P-Income | (2)<br>P-Income | (3)<br>P-Income |
|---|---|---|---|
| **Post-Filing 2-month Nasdaq** | **-0.0114*****<br>**(-4.29)** | | |
| Placebo 2-month NASDAQ (-1Yr) | | 0.0006<br>(0.14) | |
| Placebo 2-month NASDAQ (+1Yr) | | | 0.0013<br>(0.48) |
| Control Variables | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes |
| Adj. R-squared | 0.486 | 0.483 | 0.483 |
| Observations | 2,862 | 2,862 | 2,862 |

## Table 12: Scope of the Effect of IPOs on the Local Economy

This table presents second-stage 2SLS estimates where the explanatory variable of interest is the fitted value of IPO completion, as estimated from Column 4 of Table 3. In Panel A the dependent variables in Columns 1, 2, and 3, respectively, are the annual geometric average growth rates in employment, population, and personal income in counties with an IPO filing over the five years following an IPO filing, averaged across the five nearest counties to the county with an IPO filing. 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. Panel B measures these same economic outcomes, i.e., annual geometric average growth in employment, population, and personal income in the IPO filers' county, but instead of using a sample of large IPOs, it estimates a 2SLS model with medium sized IPOs, defined as those in the middle tercile in terms of the value of shares filed relative to the number of pre-filing employees in the county. All regressions are weighted by the number of IPOs filed in the county-year. We winsorize all dependent variables at the extreme 1%. Control variables for regressions in both panels are identical those reported in Column 4 of Table 4. All 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.

Panel A: Neighboring Counties

| | (1) Employee Growth | (2) Population Growth | (3) Income Growth |
|---|---|---|---|
| Instrumented IPO Completion | -0.0050 | -0.0012 | -0.0053 |
| | (-0.73) | (-0.28) | (-1.37) |
| Control Variables | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| Observations | 2,689 | 2,689 | 2,689 |

Panel B: Medium-Sized IPOs

| | (1) Employee Growth | (2) Population Growth | (3) Income Growth |
|---|---|---|---|
| Instrumented IPO Completion | 0.0039 | -0.0012 | 0.0076 |
| | (0.51) | (-0.33) | (0.87) |
| Control Variables | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes |
| Observations | 2,861 | 2,861 | 2,861 |