

Internet Appendix for Initial Public Offerings and the Local Economy

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Internet Appendix B: Additional Results

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

In Figure B1, we present the distribution of time from filing date to IPO completion (or withdrawal) for the full sample of IPOs (Panel A) and the subsample of large IPOs (Panel B). Very few IPOs are completed within one month of filing, but around 40% of IPOs are completed within 2 months and over 70% of IPOs are completed within 3 months of filing. This motivates our decision to use two months of market returns as an instrument for the IPO filing decision. Longer windows are unlikely to be a valid instrument, since these windows will incorporate returns after the most IPO completion decisions have been made.

In Figure B2, we show the geographical distribution of our sample of large IPO-filings. This map is very similar to the map in Figure 2, showing that there are no marked differences in the distribution of the location of large and small IPOs over the course of our sample period. Importantly, our sample includes IPOs located across the entire United States. However, there is a significant concentration of IPOs located in California.

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

To alleviate this concern, we use CRSP market returns. The broad set of public firms underlying the CRSP market returns is much less geographically concentrated; as a result, the aggregate CRSP returns are less likely to disproportionately reflect local economic conditions. For completeness, though, in Table B2 we confirm that our main results are robust to following Bernstein (2015) and using NASDAQ returns as our instrument. Table B2 shows that our results are robust to this alternative specification: whether we use NASDAQ or CRSP returns as the instrument, we find that large IPOs lead to a slowdown in local growth relative to staying private.

This result suggests that the concentration of IPOs in Silicon Valley is not a problem in our setting, but we investigate this further by repeating our main analyses on the subsample that excludes all California IPOs. In Panel B of Table B3 and Figure B3, we show that both our main results and placebo tests are quantitatively and qualitatively similar after excluding California IPOs. This evidence makes it clear that Silicon Valley IPOs do not drive the effects that we document.

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

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

Panels A-D of Figure 9 in the paper cast doubt on the possibility that market returns are directly correlated with economic growth of counties with IPO filings, since we show that market returns only predict future economic growth when they are measured during the book building phase of the IPO (after accounting for the number of placebo coefficients that would load by chance). In Figure B3 we examine the robustness of these results using alternative samples. In particular, in Panel A of Figure B3, we confirm that when we include the IPO bubble period (i.e., years 1998-1999) in our sample, we continue to conclude that market returns immediately after IPO filings negatively affect future local employment growth, while market returns measured over other two-month windows on either side of IPO filings have no effect on future local employment growth. We reach similar conclusions in Panel B – which removes California-headquartered firms from our sample.

Panels A-D of Figure B4 extend this analysis. These figures plot the relation between *Post-filing 2-month Market Returns* and county-level economic growth in the years surrounding the IPO filing year, in addition to plotting the relation between county-level economic growth and two placebo periods of 2-month market returns measured 12 months before and 12 months after IPO filings. The figures show two things. First, *Post-filing 2-month Market Returns* are negatively related to local employee, establishment, population, and income growth in the five years after an IPO filing, but are unrelated to these measures of economic growth in the years prior to an IPO filing in that county. Second, neither of the two placebo periods of market returns (i.e., beginning one year before or one year after an IPO filing) are significantly related to a county employment, establishment, population, or income growth, either in the three years before or five years after an IPO filing.

In unreported tests, we conduct a second type of placebo analysis in which we examine the relation between *Instrumented IPO Completion* and economic growth in counties that are observably similar to counties that experience an IPO filing, but that did not experience an IPO filing that year. Specifically, we match a single non-filing county-year to each IPO filing county-year using propensity scores based on one-year lags in employment, population, and per-capita

income, in addition to one-year lagged growth rates in these measures.¹ Consistent with our identifying assumption, we find no relation between IPO completion and future economic growth in otherwise similar counties that did not experience an IPO filing in the matched year.

In Table B3, we confirm that the reduced form evidence presented in Figure B2 holds in the full 2SLS specification. In particular, we re-estimate our main 2SLS specification for the alternative subsamples described above and show that our results are robust to each of these alternative subsamples. In Panel A, we find that the negative effect of IPO completion on employment persists when we include the IPO bubble period. In Panel B, we show that the effects are robust to removing all IPO filings for firms headquartered in California. In Panel C, we show that the results are similar after excluding the years 2000, 2002, and 2008, which are the years in our sample with the lowest average market returns during the book building period following the IPO filing. In Panels D and E, we show that the main results are qualitatively similar if we use industry by year or industry by year-quarter FE. This stringent fixed effects setting alleviates concerns that macroeconomic trends confound our results.

An alternative channel through which IPOs might affect the local economy is through the housing market. The wealth shock created by an IPO could lead to an increase in local home prices; such an effect might result in a change in the employee base and income levels in the local region. In Figure B5, we directly test this hypothesis. In Panels A and B, we find no evidence that an index of local home prices increases after exogenously completed IPOs. And in Panel C, we find no evidence of positive relation between 2-month market returns and local building permits in the areas where IPO firms are located.

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

For our main analysis, we define the local economy as the county of IPO firms' headquarters. In Table B4, we extend the geography over which we measure local economic effects. For this table, we limit the analysis to large IPOs. In Column 1, we examine the average five-year growth rate of employment in IPO firms' commuting zones, while in Column 2 we examine the average growth rate of employment across all neighboring counties (i.e., counties that share a border with firms' headquarters counties). We find negative, but statistically insignificant effects of an IPO on employment growth in these more distant areas. The size of these effects is roughly 10-40% of the size of the effect we observe at the county level. Together, Table B4 suggests that the negative effects of IPOs on their local economies are limited to areas relatively close to IPO-firms' headquarters.

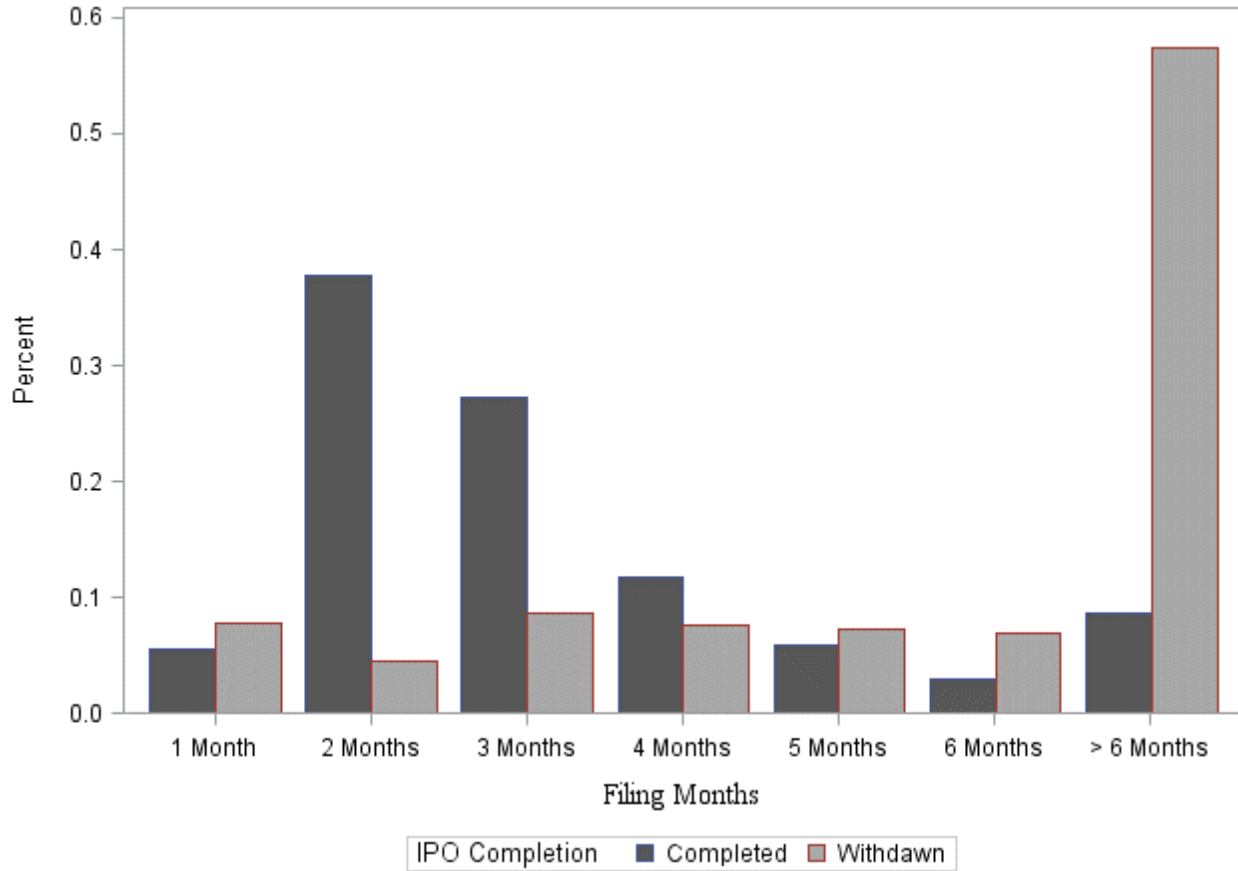
¹ We require the matched county-years to be the same calendar year as the IPO filing county-years.

In Table B5, we use Census microdata to examine the extent to which firms grow their employment faster outside of their headquarters county following a completed IPO. In Columns 1 and 2 of Table B5, we examine the effect of going public on relative local employment growth in the two- and three-year periods after the IPO filings. We define relative local employment growth as the percent change in the number of firm employees located in the HQ county minus the percent change in the number of firm employees located outside the HQ county (but within the U.S.). Using a two-stage specification similar to the analysis in Table 3, we find a significant reduction in the growth of IPO-firm employees working in the HQ county relative to the growth of firm employees working in other counties. In the two years after a public listing, firms reduce the growth rate of local employment by around 20 percentage points relative to the growth rate of non-local employment.

Figure B1: Time spent during the book-building period for completed and withdrawn deals

This figure plots the mean number of calendar days between the official IPO filing date and the date of either completion (dark bars) and withdrawal (light bars) of the IPO deal. The sample includes all deals in the regression sample with filing years between 1988 and March 2011; nearly all deals that are filed and withdrawn in 1986 and 1987 have misreported withdrawn dates in SDC. Panel A reports the data for the full sample of IPOs, while Panel B shows the subsample of large IPOs, defined as IPOs with above-median real filing proceeds.

Panel A: Full Sample of IPOs



Panel B: Large-IPO Sample

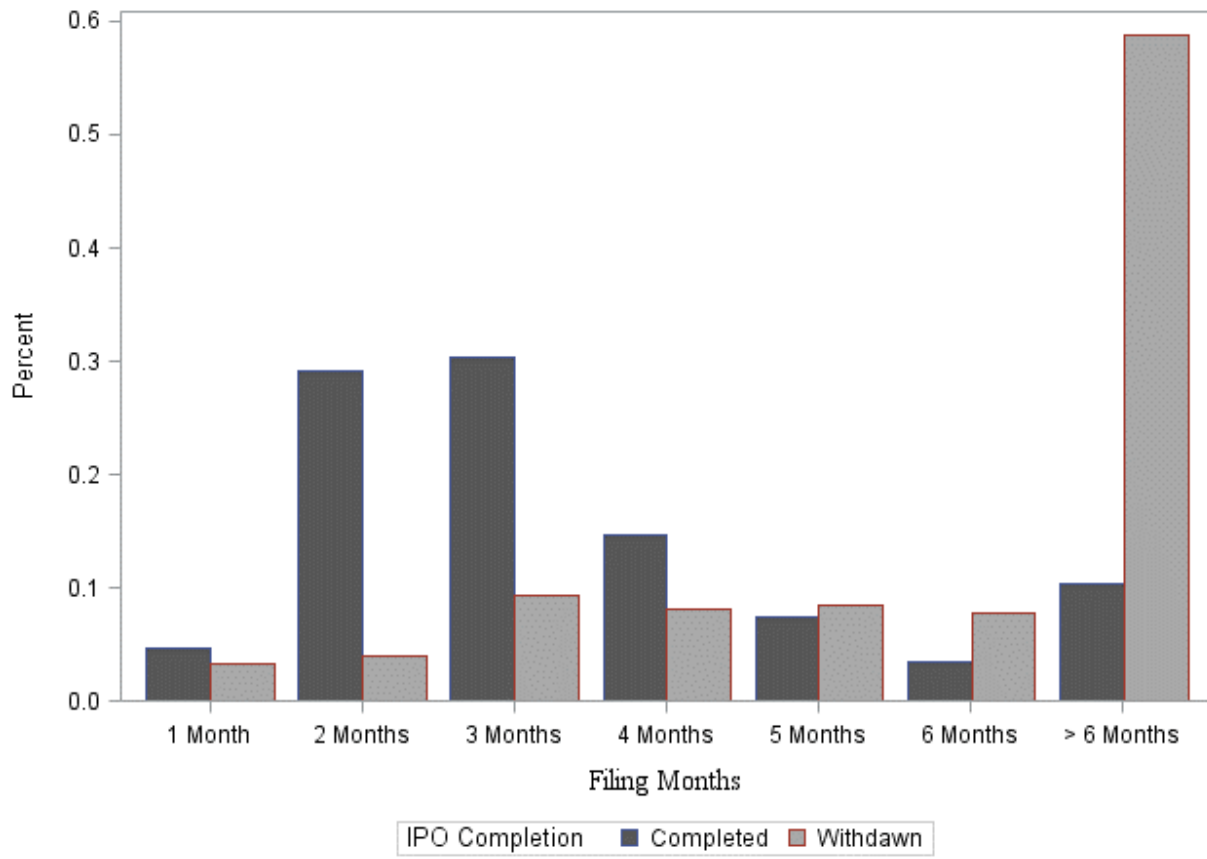


Figure B2: Geographical dispersion of Large IPO sample

This figure plots the geographical distribution of the 3,016 large IPO filings in our main sample, across U.S. counties. The sample runs from 1986 through March 2011, and is restricted to IPOs in the top half of IPO size (i.e., the real value of filing proceeds). The color shading corresponds to the total number of IPO filings within each county throughout the sample period, with unmapped counties corresponding to zero IPOs filed.

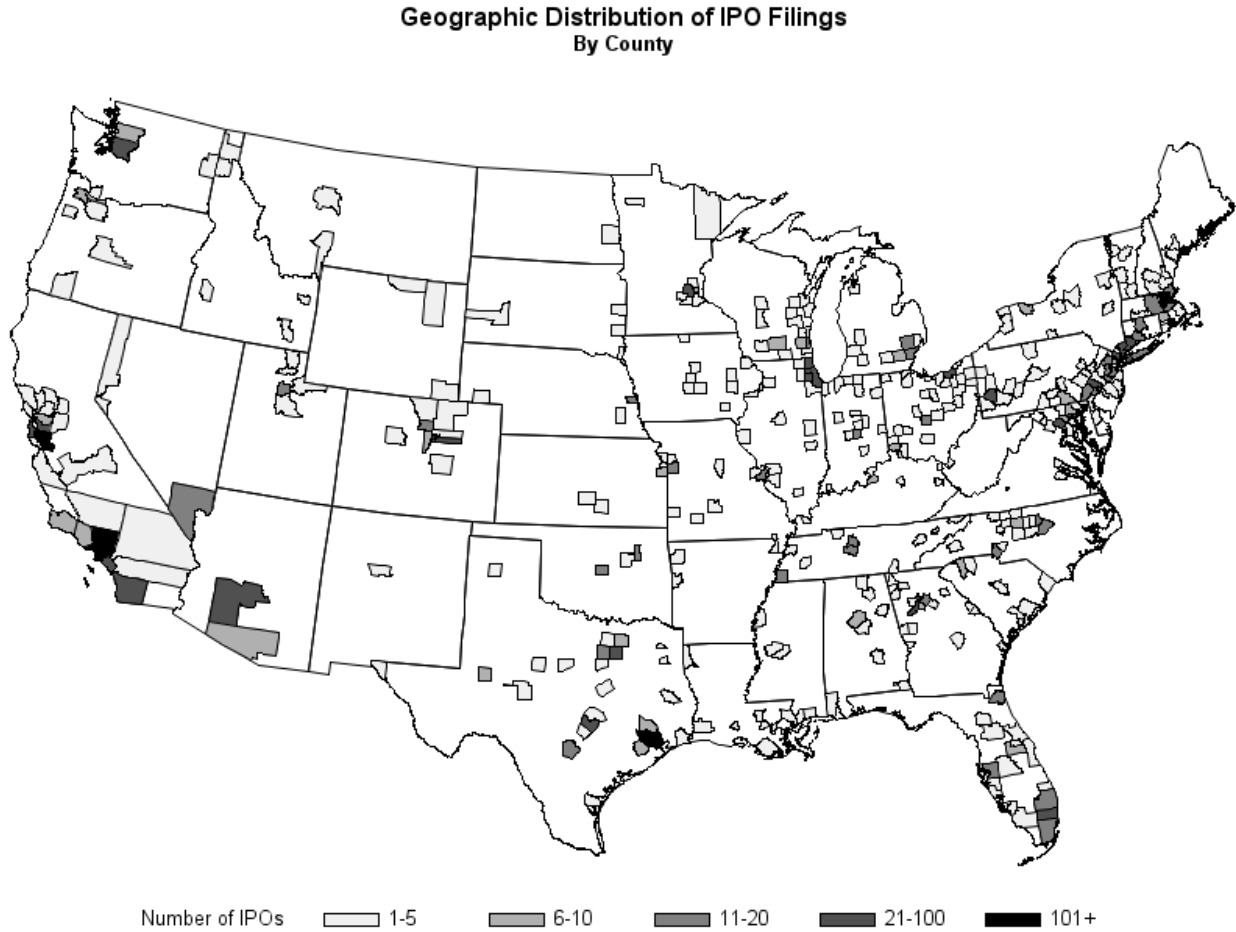
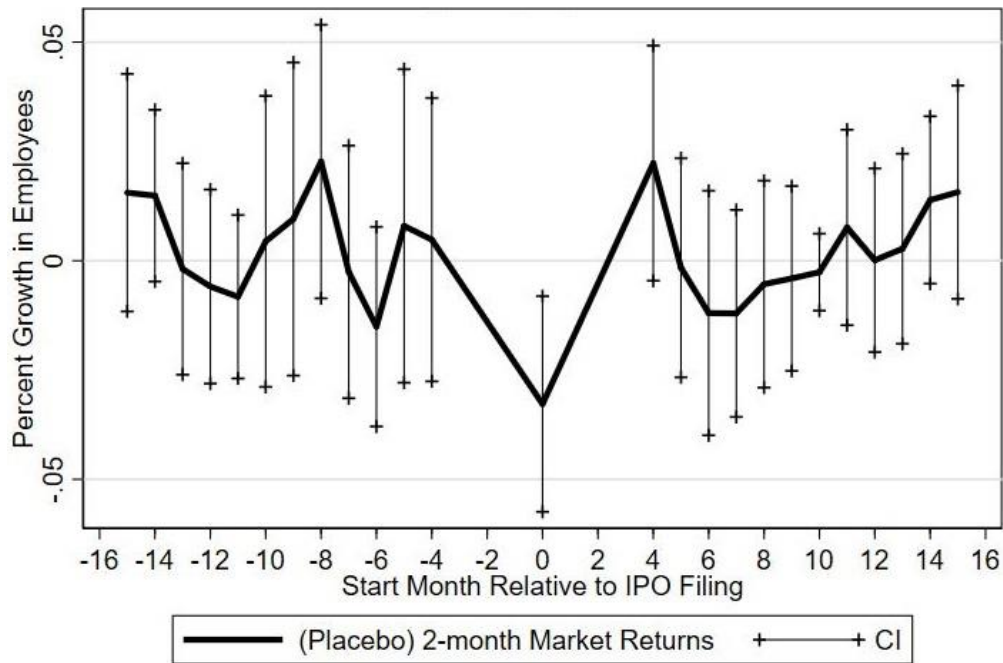


Figure B3: Placebo Market Returns – Plot of 25 different windows, Robust Samples

Panels A-C each plot coefficients from twenty-five different reduced form 2SLS regressions, with five-year cumulative growth in county-level employees as the dependent variable in each regression. In each regression, employee growth is estimated as a function of two-month (CRSP value-weighted) market returns, in addition to the same county and IPO control variables used in Table 3. Each regression uses a different window of two-month market returns, varying the number of months before or after the filing date of each IPO that the market return window begins. The start date of the market return window is marked on the x-axis. For instance, the point on the figure corresponding to the zero tick on the x-axis represents a regression of five-year county employee growth as function of two-month market returns beginning the date of each IPO filing (along with controls and fixed effects), while the point at the +4 tick represents the same regression, but swapping market returns beginning four months *after* each IPO filing for market returns beginning at the filing date. We omit returns in the 6-month window surrounding the filing date because these returns possibly impact the book building phase of the IPO, which limits their usefulness as placebo tests. Vertical lines at each point represent 95% confidence intervals for the coefficient on the variable representing two-month market returns. The sample for each regression is restricted to large IPOs between 1986 and March 2011, where large IPOs are defined as those with above-median filing proceeds (in 2011 dollars). In Panel A, we augment our full sample (used in Figure 9) by including IPOs filed during the IPO bubble period (1998-1999); in Panel B we exclude firms headquartered in California.

Panel A: Including years 1998-1999



Panel B: Excluding California Firms

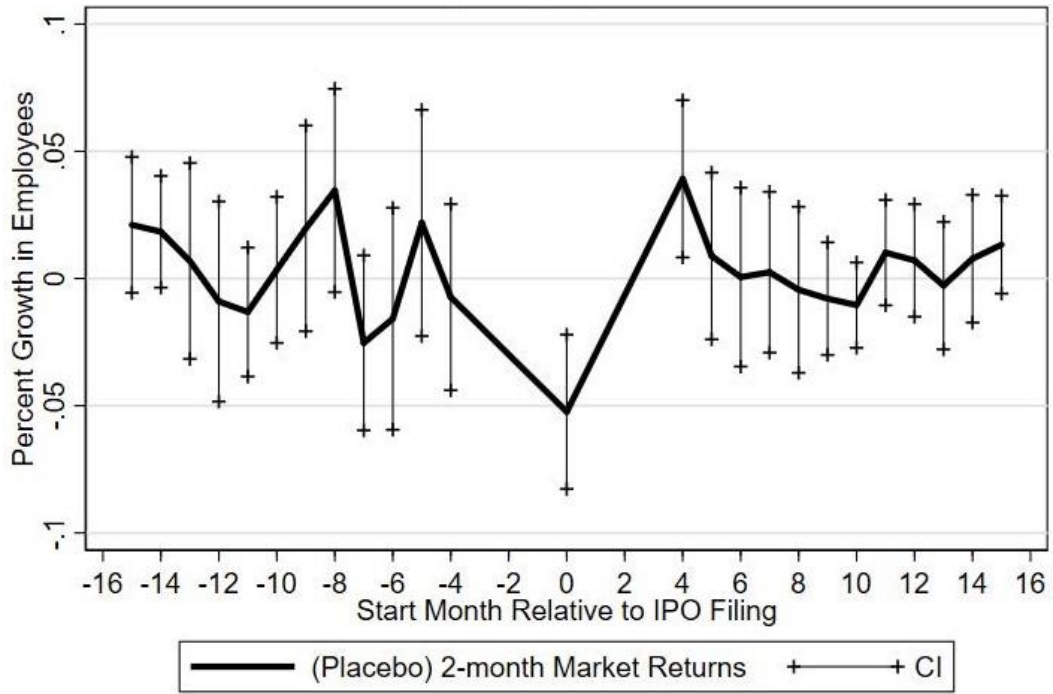
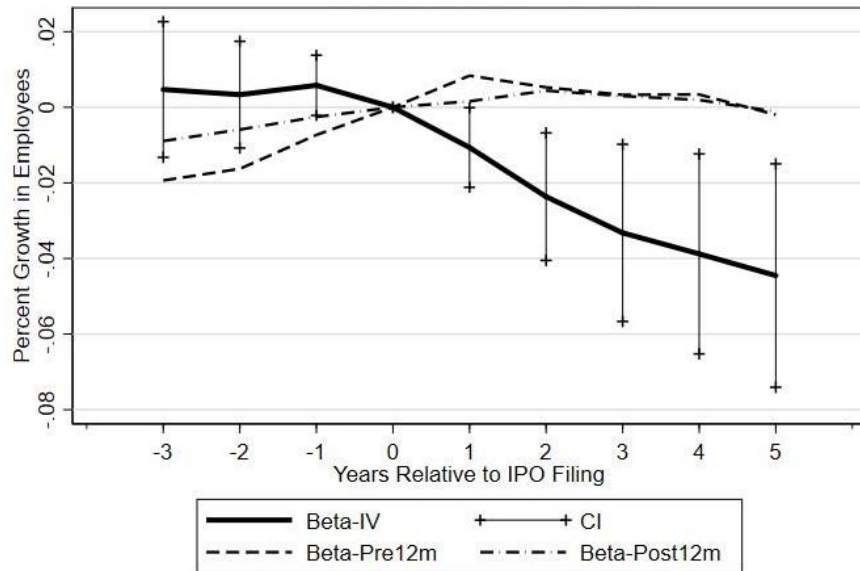


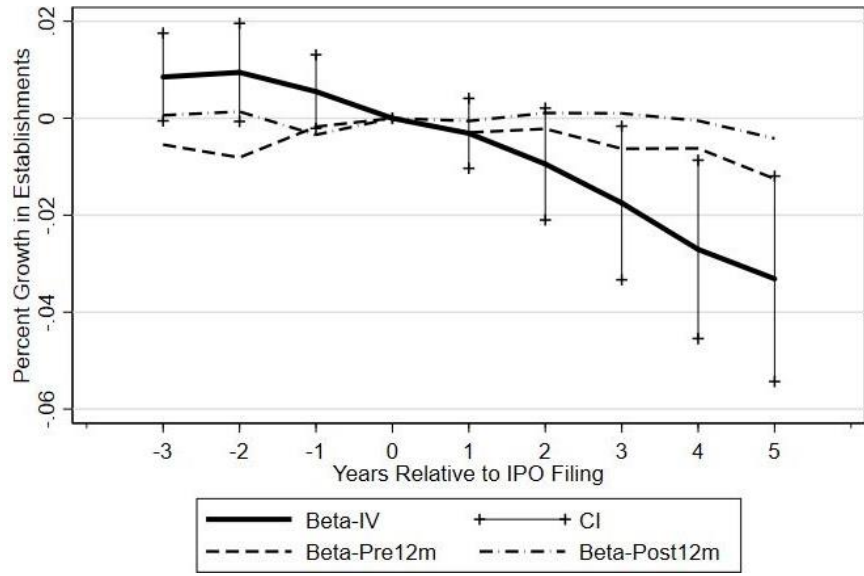
Figure B4: County Economic Growth Surrounding IPO Filings

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

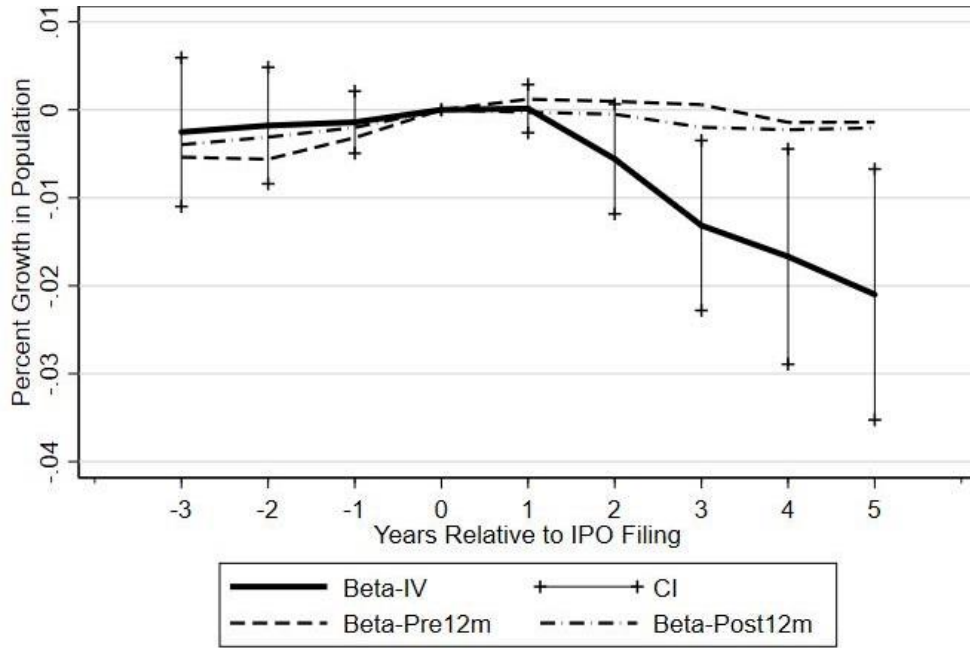
Panel A: Employees



Panel B: Establishments



Panel C: Population



Panel D: Personal Income

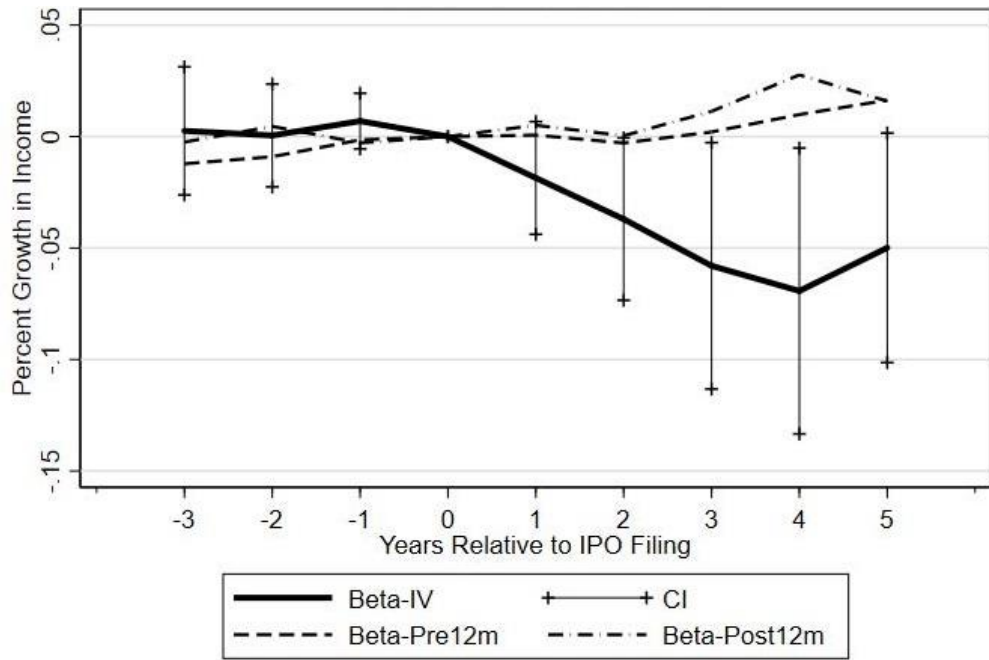
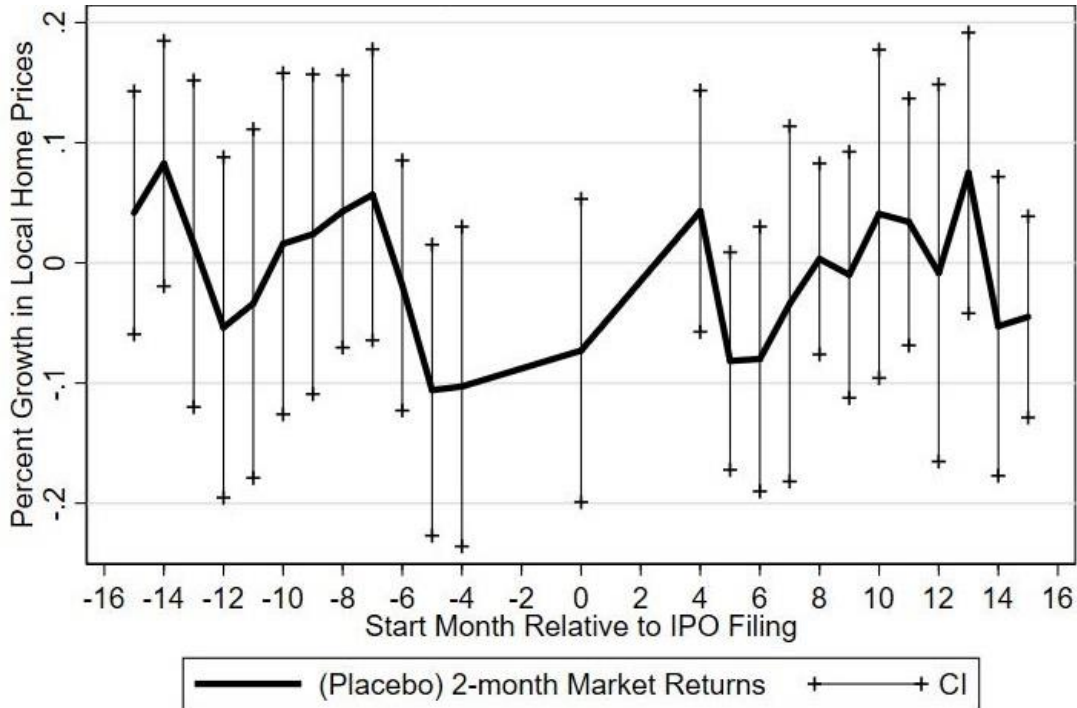


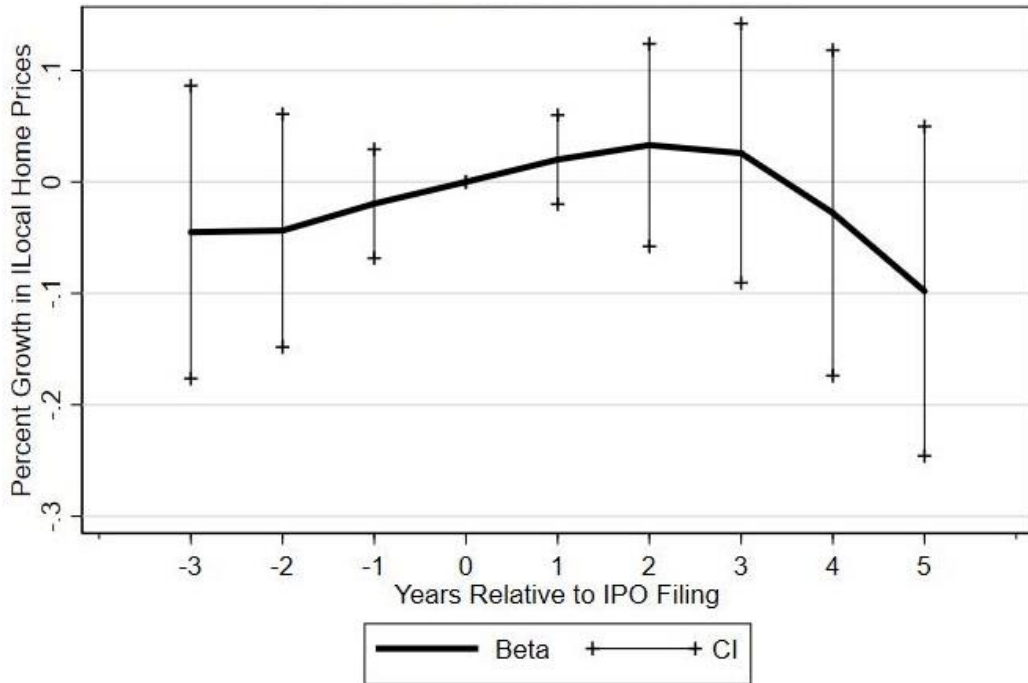
Figure B5: Growth in Local Home Prices and Building Permits

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

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



Panel B: County Home Price Growth surrounding IPO Filings



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

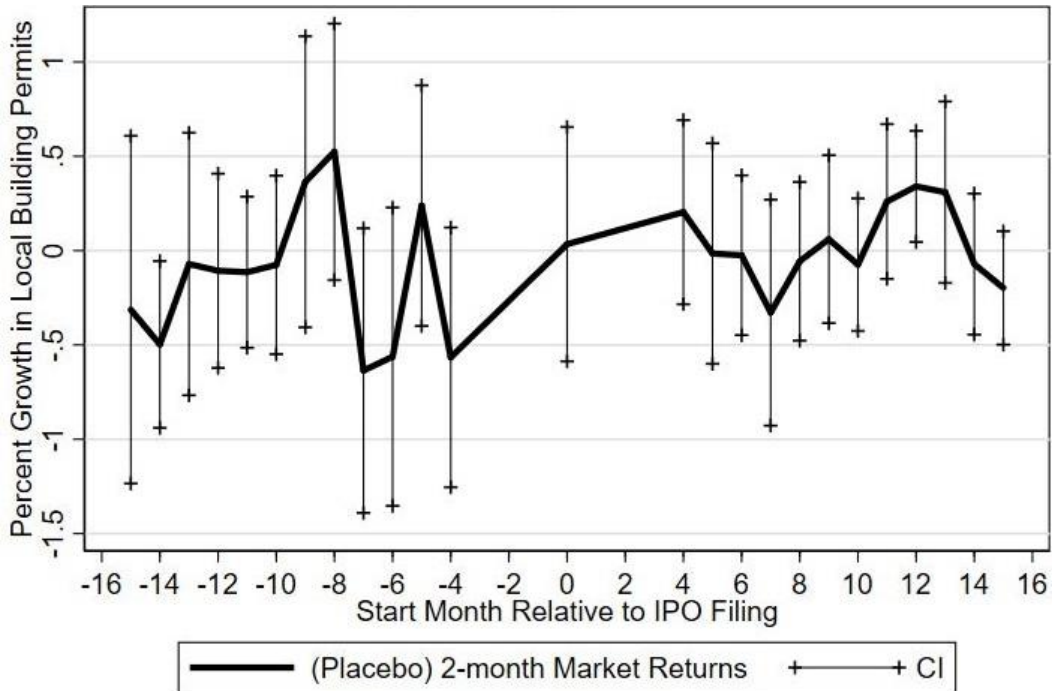


Table B1: OLS Analysis of IPOs and Local Economic Growth

This table presents OLS regression estimates where the explanatory variable is an indicator for whether an IPO filing resulted IPO completion, as opposed to withdrawal. The dependent variable in Columns 1-4 is the annual geometric average growth rate in a county's total number of employees over the five years after an IPO filing. We estimate the effect of IPO completion using the full sample of IPOs in Columns 1 and 2, the subsample of small IPOs – defined as those with below-median real filing proceeds – in Column 3, and the subsample of large IPOs – defined as those with above-median real filing proceeds – in Column 4. The sample period includes IPOs filed between our sample years of 1986 and March 2011. We winsorize all dependent variables at the extreme 1%. All variables are defined in Appendix A in the paper. Standard errors are clustered at the county and year levels (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Employees	(2) Employees	(3) Employees (Large IPOs)	(4) Employees (Large IPOs)
IPO Completion	0.0006 (1.22)	0.0007* (1.76)	0.0003 (0.52)	0.0005 (1.10)
Population Growth		0.2279*** (2.93)		0.2881*** (2.99)
Employee Growth		0.0366 (1.20)		0.0344 (0.89)
Income Growth		-0.0607 (-1.55)		-0.0570 (-1.62)
IPO Size		0.0009 (0.65)		0.0011 (0.95)
Ln(Number of IPOs)		-0.0005 (-0.35)		-0.0023 (-1.41)
Number Lead Managers		0.0000 (0.11)		-0.0002 (-0.89)
PE/VC Funding		-0.0003 (-0.82)		-0.0003 (-0.83)
Underwriter Reputation		0.0000 (0.36)		0.0000 (0.27)
Constant	0.0166*** (115.99)	0.0169*** (5.02)	0.0135*** (76.68)	0.0166*** (3.96)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.709	0.724	0.692	0.715
Observations	6,208	6,205	3,017	3,016

Table B2: IPOs and Local Economic Growth using NASDAQ as Instrument

This table repeats the main analysis from Table 3 in the paper using NASDAQ returns (rather than CRSP market returns) during the book-building phase as an instrument for the decision to complete vs withdraw the IPO. The dependent variable is the annual geometric average growth rate in a county's total number of employees over the five years after an IPO filing. We estimate the effect for the full sample of IPOs (Columns 1 and 2), the subsample of small IPOs, defined as those with below-median real filing proceeds (Column 3), and the subsample of large IPOs, defined as those with above-median real filing proceeds (Column 4). The sample period is between 1986 and March 2011. We winsorize all dependent variables at the extreme 1%. All variables are defined in Appendix A. Standard errors are clustered at the county and year levels (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Employees	(2) Employees	(3) Employees (Small IPOs)	(4) Employees (Large IPOs)
Instrumented IPO Completion	-0.0018 (-0.90)	0.0039 (1.31)	0.0037 (0.89)	-0.0060** (-2.28)
Instrumented IPO Completion*IPO Size		-0.0460** (-2.10)		
Population Growth	0.2228*** (2.87)	0.2272*** (2.88)	0.1913* (1.88)	0.2700** (2.73)
Employee Growth	0.0377 (1.23)	0.0376 (1.21)	-0.0040 (-0.13)	0.0356 (0.94)
Income Growth	-0.0603 (-1.55)	-0.0592 (-1.54)	-0.0489 (-1.71)	-0.0561 (-1.67)
IPO Size	0.0008 (0.56)	0.0347** (2.10)	-0.0044 (-0.19)	0.0012 (0.87)
Ln(Number of IPOs)	-0.0005 (-0.39)	-0.0006 (-0.42)	0.0021 (1.38)	-0.0023 (-1.49)
Number Lead Managers	0.0002 (0.59)	0.0001 (0.39)	0.0008 (0.86)	0.0001 (0.47)
PE/VC Funding	-0.0001 (-0.30)	-0.0000 (-0.07)	-0.0003 (-0.54)	0.0004 (0.79)
Underwriter Reputation	0.0000 (0.57)	0.0000 (0.70)	-0.0000 (-0.46)	0.0001 (0.98)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	6,205	6,205	3,029	3,016
First Stage F-statistic	80.4	40.9	14.0	72.7

Table B3: IPOs and Local Employee Growth, Robust Samples

This table presents second-stage 2SLS estimates—identical to those in Table 3—where the explanatory variable of interest is IPO completion instrumented with the return of the CRSP value-weighted market index in the two-months following an IPO filing, however, in each panel, we augment our main sample in a distinct way. In Panel A, we include IPOs filed during the IPO bubble period (1998-1999); in Panel B, we exclude IPO firms headquartered in California; in Panel C, we exclude years with stock market crashes (2000, 2002, and 2008); in Panel D, we include year by industry fixed effects; and in Panel E, we include year-quarter by industry fixed effects. The dependent variable is the annual geometric average growth rate in a county’s total number of employees over the five years after an IPO filing. We estimate the effect for the full sample of IPOs (Columns 1 and 2), the subsample of small IPOs, defined as those with below-median real filing proceeds (Column 3), and the subsample of large IPOs, defined as those with above-median real filing proceeds (Column 4). The sample period is between 1986 and March 2011. We winsorize all dependent variables at the extreme 1%. All variables are defined in Appendix A. Standard errors are clustered at the county and year levels, except for Panel C, which clusters by county and year-quarter levels due to the limited number of years in the regression sample (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Employment – Including years 1998-1999

	(1) Employees	(2) Employees	(3) Employees (Small IPOs)	(4) Employees (Large IPOs)
Instrumented IPO Completion	-0.0026 (-1.43)	0.0033 (1.40)	0.0057 (1.60)	-0.0071** (-2.55)
Instrumented IPO Completion*IPO Size		-0.0529** (-2.58)		
Population Growth	0.2231*** (2.90)	0.2268*** (2.90)	0.1830* (1.88)	0.2933*** (3.08)
Employee Growth	0.0625* (1.73)	0.0627 (1.71)	0.0115 (0.36)	0.0595 (1.30)
Income Growth	-0.0744** (-2.45)	-0.0738** (-2.46)	-0.0714** (-2.50)	-0.0544** (-2.59)
IPO Size	0.0005 (0.45)	0.0405** (2.56)	-0.0178 (-0.85)	0.0014 (0.93)
Ln(Number of IPOs)	-0.0022 (-1.41)	-0.0023 (-1.44)	0.0012 (0.84)	-0.0042** (-2.24)
Number Lead Managers	0.0003 (0.99)	0.0002 (0.73)	0.0018 (1.43)	0.0001 (0.40)
PE/VC Funding	-0.0000 (-0.08)	0.0001 (0.46)	-0.0004 (-0.78)	0.0002 (0.57)
Underwriter Reputation	0.0000 (0.68)	0.0000 (0.65)	-0.0001 (-0.56)	0.0001 (0.41)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	7,110	7,110	3,467	3,474
First Stage F-statistic	43.1	24.0	24.9	29.1

Panel B: Excluding California IPOs

	(1) Employees	(2) Employees	(3) Employees (Small IPOs)	(4) Employees (Large IPOs)
Instrumented IPO Completion	-0.0062 (-1.14)	0.0014 (0.28)	0.0043 (0.46)	-0.0160** (-2.54)
Instrumented IPO Completion*IPO Size		-0.0603* (-1.92)		
Population Growth	0.1468* (1.93)	0.1561* (1.99)	0.1579 (1.42)	0.1391* (1.96)
Employee Growth	0.0740* (1.90)	0.0722* (1.79)	0.0287 (0.79)	0.0757* (1.78)
Income Growth	-0.0279 (-1.08)	-0.0298 (-1.19)	-0.0460 (-1.51)	-0.0224 (-1.20)
IPO Size	0.0005 (0.24)	0.0447* (1.94)	0.0227 (0.90)	0.0009 (0.36)
Ln(Number of IPOs)	0.0004 (0.32)	0.0003 (0.28)	0.0022 (1.62)	-0.0009 (-0.68)
Number Lead Managers	0.0004 (0.75)	0.0005 (0.74)	0.0009 (0.66)	0.0009 (1.35)
PE/VC Funding	0.0003 (0.42)	0.0003 (0.59)	-0.0001 (-0.16)	0.0015* (1.72)
Underwriter Reputation	-0.0000 (-0.32)	-0.0000 (-0.05)	-0.0002 (-1.43)	0.0001 (0.59)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	4,692	4,692	2,271	2,266
First Stage F-statistic	26.5	17.0	4.8	19.7

Panel C: Excluding Years with Stock Market Crashes (2000, 2002, 2008)

	(1)	(2)	(3)	(4)
	Employees	Employees	Employees (Small IPOs)	Employees (Large IPOs)
Instrumented IPO Completion	-0.002 (-1.17)	0.007 (1.35)	0.003 (1.22)	-0.010** (-2.54)
Instrumented IPO Completion*IPO Size		-0.097* (-1.87)		
Population Growth	0.179*** (2.69)	0.174** (2.59)	0.183** (2.13)	0.161** (2.28)
Employee Growth	0.031 (0.97)	0.035 (1.01)	-0.007 (-0.27)	0.053 (1.30)
Income Growth	-0.007 (-0.32)	-0.008 (-0.34)	-0.042* (-1.89)	0.010 (0.42)
IPO Size	0.000 (0.28)	0.076* (1.87)	-0.002 (-0.09)	0.000 (0.14)
Ln(Number of IPOs)	0.001 (0.83)	0.001 (0.61)	0.002* (1.83)	-0.000 (-0.40)
Number Lead Managers	0.000 (0.84)	0.000 (0.05)	0.000 (0.30)	0.000 (0.91)
PE/VC Funding	0.000 (0.09)	0.000 (0.55)	-0.000 (-0.60)	0.001 (1.41)
Underwriter Reputation	-0.000 (-0.08)	-0.000 (-0.07)	-0.000 (-0.66)	0.000 (1.49)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	5,700	5,700	2,997	2,547
First Stage F-statistic	30.1	2.5	16.9	18.1

Panel D: Including Industry-Year Fixed Effects

	(1) Employees	(2) Employees	(3) Employees (Small IPOs)	(4) Employees (Large IPOs)
Instrumented IPO Completion	-0.0031 (-1.04)	0.0023 (0.74)	0.0035 (0.87)	-0.0077* (-1.97)
Instrumented IPO Completion*IPO Size		-0.0448** (-2.41)		
Population Growth	0.2421*** (3.13)	0.2458*** (3.17)	0.2236** (2.16)	0.3040*** (2.90)
Employee Growth	0.0299 (1.07)	0.0282 (1.03)	-0.0217 (-0.73)	0.0292 (0.85)
Income Growth	-0.0611 (-1.45)	-0.0617 (-1.49)	-0.0543* (-1.86)	-0.0609 (-1.55)
IPO Size	0.0011 (0.77)	0.0352** (2.52)	-0.0028 (-0.13)	0.0016 (0.79)
Ln(Number of IPOs)	-0.0006 (-0.40)	-0.0005 (-0.37)	0.0022 (1.46)	-0.0022 (-1.56)
Number Lead Managers	0.0003 (0.90)	0.0003 (0.98)	0.0005 (0.42)	0.0003 (0.92)
PE/VC Funding	-0.0000 (-0.07)	0.0001 (0.19)	-0.0001 (-0.27)	0.0005 (0.97)
Underwriter Reputation	0.0001 (1.06)	0.0001 (1.10)	-0.0001 (-0.56)	0.0002 (1.20)
County FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Observations	6,065	6,065	2,925	2,873
First Stage F-statistic	77.3	31.9	16.9	72.0

Panel E: Including Industry-Year-Quarter Fixed Effects

	(1)	(2)	(3)	(4)
	Employees	Employees	Employees (Small IPOs)	Employees (Large IPOs)
Instrumented IPO Completion	-0.0026 (-0.27)	0.0003 (0.03)	0.0036 (0.59)	-0.0202** (-2.68)
Instrumented IPO Completion*IPO Size		-0.0275 (-1.59)		
Population Growth	0.2571*** (3.32)	0.2570*** (3.31)	0.2153* (1.88)	0.2769** (2.63)
Employee Growth	0.0119 (0.44)	0.0121 (0.45)	-0.0315 (-0.94)	0.0191 (0.45)
Income Growth	-0.0600 (-1.34)	-0.0603 (-1.36)	-0.0447 (-1.37)	-0.0705* (-1.83)
IPO Size	-0.0003 (-0.20)	0.0209 (1.56)	-0.0088 (-0.35)	0.0004 (0.11)
Ln(Number of IPOs)	-0.0005 (-0.31)	-0.0005 (-0.32)	0.0023 (1.36)	-0.0022 (-1.42)
Number Lead Managers	0.0003 (0.43)	0.0003 (0.45)	0.0005 (0.28)	0.0009 (1.62)
PE/VC Funding	-0.0001 (-0.09)	0.0000 (0.03)	-0.0001 (-0.37)	0.0016 (1.62)
Underwriter Reputation	0.0001 (0.66)	0.0001 (0.69)	-0.0000 (-0.09)	0.0002 (0.77)
County FE	Yes	Yes	Yes	Yes
Industry-Year-Quarter FE	Yes	Yes	Yes	Yes
Observations	5,761	5,761	2,701	2,544
First Stage F-statistic	6.4	3.1	3.1	6.3

Table B4: Large IPOs and Regional Employment Growth

This table presents second-stage 2SLS estimates where the explanatory variable of interest is the fitted value of IPO completion, instrumented with the return of the CRSP value-weighted market index in the two-months following an IPO filing. The sample is restricted to large IPOs, defined as above-median real filing proceeds. The dependent variable is the annual geometric average growth rate in a commuting zone's (Column 1) or a neighboring county's (Column 2) total number of employees over the five years after an IPO filing. The sample period is between 1986 and March 2011. We winsorize all dependent variables at the extreme 1%. All variables are defined in Appendix A. Standard errors are clustered at the county and year levels (with *t*-statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Commuting Zone	(2) Neighboring Counties
Instrumented IPO Completion	-0.0042 (-1.19)	-0.0012 (-0.49)
Commuting Zone Population Growth	0.0477 (0.64)	-0.0096 (-0.11)
Commuting Zone Employment Growth	0.1232** (2.44)	0.1643*** (2.93)
Commuting Zone Income Growth	-0.0714 (-1.31)	-0.0697 (-1.67)
IPO Size	0.0013 (1.06)	0.0007 (0.55)
Ln(Number of IPOs)	-0.0020 (-1.41)	-0.0019 (-1.63)
Number Lead Managers	0.0002 (0.66)	-0.0004 (-0.93)
PE/VC Funding	0.0005 (1.04)	0.0003 (1.19)
Underwriter Reputation	0.0001 (0.68)	0.0001 (0.37)
County FE	Yes	Yes
Year FE	Yes	Yes
Industry FE	Yes	Yes
Observations	3,067	3,065
First Stage F-statistic	25.1	25.1

Table B5: US Census Employment-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. The dependent variable in Column 1 (2) are 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 (three) years after an IPO filing. We winsorize all dependent variables at the 1% level. The sample is restricted to large IPOs between 1986 and March 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) 2-year change Employment Growth	(2) 3-year change Employment Growth
Instrumented IPO Completion	-0.216** (-2.389)	-0.140** (-1.965)
Ln(Employees)	0.003 (0.152)	-0.021 (-0.733)
Ln(Population)	0.003 (0.127)	0.026 (0.886)
Ln(Income)	0.024 (1.383)	0.006 (0.283)
Pop Growth	-0.076 (-0.202)	0.013 (0.037)
Employee Growth	-0.130 (-1.036)	-0.427* (-1.762)
Income Growth	-0.099 (-0.463)	-0.273 (-1.227)
Ln(Number of IPOs)	0.002 (0.104)	0.020 (1.175)
Ln(IPO Size)	0.012** (2.298)	0.022*** (4.525)
Number of Lead Managers	0.019*** (2.866)	0.013 (1.598)
PE/VC Funding	0.033*** (4.117)	0.023*** (3.294)
Underwriter Reputation	-0.010*** (-2.747)	-0.012*** (-3.621)
First-stage F-statistic	14.70	14.34
Observations	1,800	1,700

Internet Appendix C: Reconciling our Results with Butler et al. (2019)

Butler et al. (2019) match zip code-years that host an IPO to nearby zip code-years that do not host an IPO, but that do host an IPO at some other point in their sample. This creates a set of treatment and control zip codes that are similar on observable dimensions (employment, establishment, etc.). Then authors find that large IPOs increase employment growth in the firms' headquarters zip codes by about 78 basis points per year. Their inclusion of county-year fixed effects makes this effect relative to other zip codes in the same county-year. Thus, this result is difficult to relate to our main county-level analyses, but appears to contradict our zip code-level evidence that IPOs negatively affect employment growth in their headquarters zip code.

In Column 1 of Table C1, we report the main result from their paper for reference. In Column 2, we replicate their result. Although we cannot perfectly recreate their sample, we match their point estimates quite closely. Importantly, the positive effect of IPOs on the local economy is limited to the zip code of the firm's HQ; there are no effects on employment in nearby zip codes. While we do not replicate their establishment results, in their paper Butler et al. (2019) find no effect on establishments in the HQ zip code, but a positive effect on establishment growth in nearby zip codes (even though there is no employment effect in these areas). It is difficult to understand the set of economic spillovers that would justify this set of effects, and the authors do not offer any explanation. This leads us to examine their specification in more depth.

We first explore whether sample selection choices can reconcile our two sets of results. Butler et al. use IRS data to control for zip code-level income. This data is not available for three years of their sample (1999, 2000, and 2003); as result, they throw out a subset of IPOs. In Column 3 of Table C1, we add back in these years. Adding in these missing years reduces the magnitude of the positive employment growth in the headquarters zip code. It also leads to a significant negative effect of IPOs on employment growth in zip codes located between 5 and 10 miles from IPO firms' headquarters (and negative point estimates for more nearby zip codes). In unreported results, we find that excluding the income control but maintaining the original sample (i.e., same years as Column 1) does not change the results. This suggests that it is the additional years of data, not the omission of a relevant control, that drive the differences between Columns 2 and 3.

In Column 4, we re-estimate Butler et al.'s result including the missing years and excluding all other control variables. In a true random experiment, it is not necessary to include control variables to estimate a treatment effect. This should be especially true after the inclusion of county-year fixed effects. The effects of a reasonably exogenous treatment (which Butler et al. (2019) argue they have after matching) should not hinge on the zip code level controls included in their main analysis. Indeed, in Table 3 of the paper, we show that our main 2SLS results are not sensitive to whether we include control variables, which gives us additional confidence in the methods we use.

In contrast to our main results which depend little on the precise controls selected, the evidence in Butler et al. (2019) fundamentally changes without the inclusion of controls. The positive effect of IPOs on employment growth in the headquarters zip code is no longer

significant and there is a negative and significant effect of IPOs on employment in the surrounding zip codes. These negative effects represent large reductions in annual employment growth of between 54 and 65 basis points. Because there are many zip codes included in these mileage bands, the cumulative reduction in job growth represented in the estimates in either Column 3 or 4 would likely easily subsume any positive effect of the IPO in the single headquarters zip code when aggregated across the entire county. Thus, with slight perturbations to the sample and/or control variables, the methods in Butler et al. (2019) deliver results that are consistent with our main finding that large IPOs reduce county-level employment growth.

Whether there is a limited positive employment effect of IPOs in the zip code of firms' headquarters is less clear. Our 2SLS analysis in Table 5 of the paper shows a negative relation, while the main specification in Butler et al. (2019) indicates a positive relation. In either case, the data support the conclusion that IPOs result in a reduction in economic growth at the county level.

Table C1: Butler et al. (2019) Replication and Extension

This table shows results from tests of the effect of an IPO on employment growth in the zip code of the IPO firm's headquarters. These regressions examine the robustness of the results in Butler et al. (2019) to alternative specifications. Column 1 prints the results from Column 2 of Table 5 in Butler et al. (2019). Columns 2, 3, and 4 are our replications of the same test under alternative specifications. Column 2 is our attempt at an exact replication. Both Columns 1 and 2 include data from years 1998 to 2015, but exclude IPOs filed in 1999, 2000, and 2003 due to missing data IRS data on wage income. Column 3 adds back in these excluded years by excluding the control variable $\ln(\text{Wage Income})$. Column 4 excludes all control variables and includes all data from 1998 to 2015. Standard errors are two-way clustered at zip code and county-year levels (with t -statistics reported in parentheses). *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Butler et al. (2019) Table 5, Column 2	(2) Replication	(3) All years >=1998	(4) All year >= 1998 Without Controls
Large IPO HQ Zip Code	0.784* (0.437)	0.773** (0.366)	0.622** (0.314)	0.498 (0.322)
<0-2 Miles from Large IPO HQ	0.598 (0.701)	-0.190 (0.580)	-0.247 (0.457)	-0.562 (0.454)
2-5 Miles from Large IPO HQ	0.184 (0.358)	-0.151 (0.306)	-0.289 (0.276)	-0.542** (0.272)
5-10 Miles from Large IPO HQ	0.279 (0.261)	-0.308 (0.284)	-0.497* (0.266)	-0.651** (0.262)
Number of SEOs	0.130 (0.241)	0.069 (0.058)	0.099* (0.055)	
Ln(Population)	0.053 (0.168)	-0.069 (0.129)	0.091 (0.118)	
Ln(Establishments)	-1.171*** (0.180)	-0.723*** (0.181)	-0.843*** (0.171)	
Lagged Dependent Variable	-0.918 (2.559)	-1.677 (2.353)	0.250 (1.518)	
Ln(Population Density)	-0.061*** (0.020)	-0.041*** (0.012)	-0.055*** (0.011)	
Ln(Wage Income)	1.084*** (0.234)	1.079*** (0.226)		
County-Year FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.161	0.160	0.164	0.153
Observations	9,284	8,960	12,218	12,218