# The Effects of Going Public on Firm Performance and Commercialization Strategy: Evidence from International IPOs

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**Abstract**

We study the effects of going public using a unique panel of firms in 16 European countries for which we observe financial data before and after firms' initial-public-offering (IPO) attempts. We compare firms that complete their IPO with firms that withdraw their IPO. We instrument for the going public decision using prior market returns. We find that firm profitability goes up after going public - contrary to previous results in the literature. The increase in profitability is stronger for firms in countries with higher disclosure requirements and investor protection. We also find a post-IPO expansion in sales per employee, the number of subsidiaries and countries in which IPO firms operate. Overall, our results are consistent with going public inducing a shift in strategy towards commercialization to increase profitability.

**Keywords**: IPOs, Profitability, Commercialization, Selection versus Treatment
**JEL Codes: G32**

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# 1 Introduction

Going public is a key decision for many firms. Explanations for why firms go public include raising capital for expansion, as well as diversification and liquidity for previous owners. Raising capital benefits the firm by allowing it to undertake new investment opportunities. However, ownership dispersion can lead to diverging incentives of managers and investors (Jensen and Meckling, 1976), and managers faced with stock market pressure can become myopic (Stein, 1989). Overall, it is unclear whether going public is beneficial or detrimental for firm performance.

The stylized fact in the literature so far is that, on average, profitability falls after IPOs, which seems to speak against the benefits of public status (Degeorge and Zeckhauser 1993, Jain and Kini 1994, Mikkelson, Partch, and Shah 1997). However, in order to provide a definitive answer, it is crucial to distinguish the selection and causal effects of going public. Selection means that firms time their IPO decisions according to their life-cycle, profitability shocks, or industry shocks (Pástor, Taylor, and Veronesi 2009, Spiegel and Tookes 2020, Maksimovic, Phillips, and Yang 2020). The dynamics of these other variables can explain many post-IPO outcomes, and not going public in itself. The true causal effect of going public, instead, refers to changes that happen to firms because they go public, and not simply changes that are correlated with the going public decision. Bernstein (2015) shows casual evidence that IPO firms have fewer citations to their patents after going public. These results could be interpreted as evidence of short-termism as firms focus on less influential patents or agency costs of being public (see Kahle and Stulz 2017, and Sheen 2020). However, going public might alternatively be associated with an optimal change in strategy: raising funds to increase commercialization activities while focusing less on new product innovation. The focus on commercialization may be an optimal strategy aligned with public investors. Our paper studies whether going public leads to this shift in strategy.

Our empirical design sheds light on the consequences of going public in two ways. First, we collect novel data on firms' financials and commercialization activities for firms going public as well as on firms that withdraw their IPOs. Withdrawn IPO firms represent a reasonable counterfactual for firms that go public and allow us to control for a host of self-selection issues. Our sample consists of close to 3,500 IPO-attempt firms spread across 16 European countries between 1997 and 2017. A key advantage of the European data is that, due to reporting requirements, we have post-IPO-attempt profitability data even for firms that withdraw their IPOs. This type of data has not been available in the previous literature. The panel structure of the data allows us to include firm fixed effects and, therefore, to control for all time-invariant characteristics at the firm level.

We use withdrawn IPOs as counterfactuals for completed IPOs, but the decision to withdraw is still endogenous. Therefore, a second crucial element of the identification strategy is to instrument for the IPO completion. In the same spirit as Bernstein (2015), we use market returns over the previous 30 days to instrument for the IPO decision. These 30 days coincide with the marketing and book-building phase of the IPO. Firms that pull their IPOs usually blame poor market conditions for the withdrawal. In line with previous evidence, we find that positive market returns in the previous 30 days increase the likelihood of IPO completion by 6.9% (from an unconditional probability of 87%). The pre-choice return is basically uncorrelated with firm characteristics of candidate IPOs, so it behaves as a well-balanced instrument. Market returns in this narrow window are unlikely to directly affect firm outcomes several years after the attempt, which is what the exclusion restriction requires. In short, our identification strategy is based on the idea that good returns are a nudge for some firms that are at the margin between listing or not, but have no effect on the firm except for their impact on the going public decision.

As in the previous literature, we find that the profitability of IPO firms goes down after going public. However, withdrawn IPOs exhibit a similar pattern, so the effect is basically zero in OLS regressions that compare completed and withdrawn IPOs. This suggests that the previously documented post-IPO drop in profitability is related to selection issues in IPO attempts rather than a causal effect of going public.

In our instrumental variables (IV) estimation, we find a significant increase in profitability for firms that go public. Hence, once we are able to isolate the exogenous transitions to listed status, we find a positive effect instead of a negative effect in profitability. The profitability of completed IPOs increases by close to one standard deviation in this sample. The effect is large, but plausible. We check that our setup is not affected by a weak-instrument problem, which could artificially increase the magnitude of the coefficients (Jiang, 2017). We also show that our inference is robust to the exclusion of clusters of observations as recommended by Young (2022). In terms of theory, we can expect the IV coefficient to be larger than the OLS coefficient because selection effects induce a downward bias. In equilibrium, a large increase in profitability is likely to be needed to compensate for the equally large costs of going public (close to 5% of firm value according to Gahng, Ritter, and Zhang 2021).

In order to identify mechanisms, we study variation of the IV estimate across countries and firms. We find that the increase in profitability is stronger in countries with high disclosure requirements for IPOs (La Porta, López-de-Silanes, and Shleifer, 2006) and high investor protection (Djankov, La Porta, López-de-Silanes, and Shleifer, 2008). This suggests that providing information and containing agency problems are important to realize the full benefits of listed status. We find that the post-IPO increase in profitability is weaker in small firms. Small firms increase assets and sales aggressively after going public. However, the increase in assets is stronger than the increase in sales, which dampens profitability relative to large firms. Beyond profitability we find that all firms experience a significant increase in sales per employee, the number of subsidiaries, and presence in international markets. Together with the reduction in patents, in line with the decline in innovation found by Bernstein (2015), our results suggest that going public causes a shift in strategy from exploration and innovation towards commercialization, where growth is focused on segments with strong margins.[1]

[1]Although not in our sample, the recent experience of Uber can illustrate this new focus on profitability after going public: "*Mr. Khosrowshahi (CEO) has moved to restructure Uber to deliver on a promise to make the company profitable, scaling back many of its expensive side businesses (...) The company has promised to be profitable on an adjusted basis before interest, taxes, depreciation and amortization by the end of next year*

Finally, we highlight two important results regarding the exclusion restriction of the IV strategy. First, we conduct placebo tests with market returns that accrue right after the IPO attempt. If returns provide a signal of future profitability, then their predictive power should not vanish at the IPO. However, we find that placebo returns have no impact on the likelihood of IPO completion (1st stage), and no predictive power for future profitability (reduced-form regressions). Hence, consistent with the exclusion restriction, if there is no impact on the ownership choice (1st stage), then there is no effect on firm outcomes (2nd stage). One would expect this under the exclusion restriction because the channel of influence between returns and long-run outcomes is severed once the listed status is fixed.

Second, we note that the self-selection effects that follow models of asymmetric information with unobservable quality do not invalidate our analysis. In asymmetric information models, strong firms are more likely to stay out of the market because raising equity is too costly for them (Myers and Majluf, 1984). If this is the case, then OLS underestimates the true effect of going public on profitability when comparing firms that enter the market (completed IPOs) with firms that stay outside (withdrawn IPOs). The IV estimation is already designed to overcome this obstacle. A more relevant concern is that our instrument can be correlated with self-selection. The costs of asymmetric information decrease with high market returns, and hence the average quality of firms going public improves in hot markets (Lucas and McDonald, 1990). However, the relevant comparison in the IV estimation is not between the average issuer in hot and cold markets, but among firms that respond to short-term returns by completing or withdrawing (i.e., compliers in IV parlance). That is, the IV coefficient relies on the comparison between the marginal issuer after high returns and the marginal withdrawal after low returns. According to asymmetric information models, these two types of firms are of similar quality. So, even if there is a change in the average quality of issuers following 30-day returns, which is already debatable, this does not affect the exclusion restriction. The main reason is that quality is comparable across firms that

*(2021).*" (Uber Sells Self-Driving-Car Unit to Autonomous-Driving Startup, *Wall Street Journal*, December 7, 2020).

are nudged by returns to complete or withdraw their IPOs.

Our paper contributes, first and foremost, to the literature on the consequences of going public (Pagano, Panetta, and Zingales 1998, Kim and Weisbach 2008). The going public decision is an endogenous outcome. For instance, more productive firms, firms with better governance, or firms with more investment opportunities, can self-select into public markets. Our focus is on isolating the treatment effect of going public from selection effects. Estimating the causal effect is a first-order question in corporate finance, which speaks directly of the advantages (or disadvantages) of accessing public markets.

Other recent work also deals with the treatment effect of going public, but focusing on outcomes such as innovation, institutional ownership, employment, and local spillovers (Bernstein 2015; Borisnov, Ellul, and Sevilir 2019; Butler, Fauver, and Sypiridopoulos 2019; Babina, Ouimet, and Zarutskie 2020; Cornaggia, Gustafson, Kotter, and Pisciotta 2020; Dambra, Gustafson, and Pisciotta 2021). Compared to previous work, we have access to panel data with financial variables for both completed and withdrawn IPOs, which allows us to study the long-standing puzzle of the post-IPO drop in profitability. We find that profitability goes up after going public, contrary to the stylized fact in the literature (Degeorge and Zeckhauser 1993, Jain and Kini 1994, Mikkelson, Partch, and Shah 1997). Our findings using variation in disclosure and investor protection across countries are particularly important since they arguably provide the first causal estimate of the effects of going public under different institutional settings.

Our results are also related to the literature that compares the performance of private and public firms. Asker, Farre-Mensa, and Ljungqvist (2015) and Sheen (2020) argue that private firms react more to industry shocks than similar public firms. Gilje and Taillard (2016) and Phillips and Sertsios (2017) find the opposite using particular events in the natural gas and medical devices industries respectively. The comparisons between private and public companies can be blurred by selection issues. In this respect, our results contribute by estimating the pure treatment effect of going public, which can help bridge the gap between apparently contradictory results.

The rest of the paper is organized as follows. Section 2 describes the data. Section 3 explains the identification strategy behind our IV estimation. Section 4 reports the main IV results, placebo tests, and auxiliary results. Section 6 concludes.

# 2 Data

## 2.1 Sample Selection and Panel Structure

Our data consist of 3,467 IPO attempts, out of which 3,037 are completed and 430 are withdrawn. Hence, the unconditional likelihood of IPO completion is 87%. We arrive at this sample through the following procedure. From *Dealogic*, *SDC*, and *Zephyr* we obtain the dates of all IPO attempts between 1997 and 2017 in 16 European countries. These data vendors compile dates for IPO completions and withdrawals from regulators, stock exchanges, the financial press, and other outlets. We merge the list of IPO attempts with financial information for each firm from *Amadeus*, which reports data for public and, crucially, private firms. We get year-end financial information from two years before the IPO attempt to two years after the IPO attempt. This gives us a 5-year event window from *t-2* to *t+2*, where year $t$ is the year of the IPO attempt for each firm.[2] We keep the IPO attempt only if we have financial data, albeit incomplete, for years before and after the attempt. We drop observations if the 5-year window overlaps with other IPO attempts, successful or not, for the same firm.[3]

Figure 1 shows the distribution of IPO attempts by country and calendar year. As can be expected, bigger markets, such as the UK, Germany, and France, have more IPO filings. There are peaks of IPO filings in 2000 and 2006-7, which coincide with years of high stock

[2] We use a relatively short window to minimize attrition in the sample. In Section 4.6 we show that our results are robust to extending the horizon.

[3] We drop 52 withdrawn attempts that are followed by a completed IPO within the 5-year period. We also exclude 15 withdrawn attempts that are followed by another withdrawn attempt within the 5-year period. The appendix provides more details on our sample selection and details on withdrawn attempts.

market valuations. The percentage of withdrawn IPOs also moves with the stock market cycle, with relatively more withdrawals in years of poor returns such as 2001, 2008, and 2010. In terms of country coverage and distribution across years, our sample is comparable to the sample in Helbing, Lucey, and Vigne (2019) who also study IPOs in Europe. It is important to note that, given the relative decline of the U.S. IPO market (Doidge, Karolyi, and Stulz, 2013), the European market was probably the largest market for new issues in this period.

Insert Figure 1 here

Figure 2 shows the distribution of the 14,110 firm-year observations by event year. Observations for completed and withdrawn IPOs are shown separately. The panel is not perfectly balanced as there are close to 15% fewer observations in the extreme years of the event window (years *t+2* and *t-2*) than in the years adjacent to the IPO attempt (years *t* and *t-1*). However, the panel for withdrawn IPOs is not more unbalanced than the panel for completed IPOs. There is attrition in the extreme years because firms do not have operating history so much in advance of their IPO attempts, or because firms disappear or get acquired later on.[4]

Insert Figure 2 here

Table 1 shows summary statistics for the main variables in our analysis. The first three variables in the table make reference to the structure of the data. The average of the Completed IPO dummy implies that firms that eventually go public represent 87% of the firm-year observations. The Post dummy captures the post-attempt observations for completed and withdrawn firms. The average of 60% shows that typically there are 3 post-attempt years (*t*, *t+1*, and *t+2*) and 2 pre-attempt years (*t-2* and *t-1*). The IPO dummy is the Completed IPO dummy times the Post dummy, and it captures the post-IPO for the firms that go public. The average of the IPO dummy implies that 52% of the observations correspond to firms while they are publicly traded, and 48% correspond to firms while they are private.

[4]Most European markets require at least three annual reports before listing. However, the AIM in the UK does not require a minimum of operating history (Helbing, Lucey, and Vigne, 2019).

Insert Table 1 here

## 2.2 Firm-level Variables

The advantage of the European setting is that all private firms have to file financial statements with regulators. This gives us access to financial data for a wide range of private firms, which is hard to get in the U.S. (with the exception of some regulated industries). Operating return on assets (OROA=earnings before interest and taxes/book assets) is the main measure of profitability. In Table 1 we show that mean (median) profitability is -2%(3%), but with a large standard deviation of 24%. There are slightly fewer observations for profitability than other variables because its computation requires information from both the income statement and the balance sheet of the firm. The coverage for income statements is not as good as for balance sheets due to variation in reporting standards across Europe (see Bernard, Burgstahler, and Kaya 2018). For instance, the obligation to file often refers only to abbreviated financial statements.

Average assets are 172 million Euros, but the size distribution is highly skewed to the right as implied by a much lower median assets of 10.6 million Euros. Something similar happens with sales. Both sales over employees and sales over assets are skewed to the right. From the ownership data provided by *Amadeus*, we can measure firm scope using the information on the subsidiaries operated by each firm. On average, 35% of firm-year observations in our sample have subsidiaries, 19% have subsidiaries in multiple industries, and 9% have subsidiaries in countries different from the firm's headquarters.

Besides financials, we consider additional outcomes for each firm. Patents correspond to the number of patent applications that are eventually granted. These data comes from *Zephyr*, which matches firms and patent information from the European Patent Office. Acquisitions, also from *Zephyr*, correspond to the number of firms acquired.[5] Table A.1 provides

[5] As Erel, Jang, and Weisbach (2015), we use *Zephyr* for acquisitions rather than the *SDC* because *Zephyr* shares with *Amadeus* the firm identifiers from Bureau Van Dyck. The coverage of the acquisitions of private firms is also better with *Zephyr*.

summary statistics for additional variables in our analysis.

# 3 Empirical Design

## 3.1 OLS and Instrumental Variables (IV)

We first estimate the effect on firm profitability ($Y_{it}$) around the IPO decision with the following differences-in-differences regression:

$$Y_{it} = \beta IPO_{it} + \alpha_i + \alpha_\tau + \alpha_{m\tau} + \alpha_{jt} + \varepsilon_{it} \tag{1}$$

The dependent variable is measured for firm $i$ at the end of calendar year $t$. The main variable of interest is $IPO_{it}$, which takes a value of 1 if firm $i$ has gone public in year $t$ or earlier, and 0 if the firm is still private. $IPO_{it}$ captures the before-and-after for completed IPOs relative to withdrawn IPOs. Since withdrawn attempts represent the counterfactual, our comparisons are all within IPO attempts. Hence, our setup already controls for a host of selection effects that explain why firms attempt to go public in the first place (e.g., Degeorge and Zeckhauser 1993, Pástor, Taylor, and Veronesi 2009, Spiegel and Tookes 2020).

One of the advantages of the panel structure is that we can include firm fixed effects ($\alpha_i$), and focus on within-firm variation. With the firm fixed effects we also avoid the need to control for firm conditions that are fixed over time such as initial conditions.[6] We do not include time-varying firm-level controls because they are endogenous to the IPO decision, and hence defeat the purpose of the IV strategy. The unbalanced nature of the panel suggests that it is necessary to control for event-time fixed effects ($\alpha_\tau$). These fixed effects absorb the $Post_{it}$ dummy while also allowing us to control for life-cycle dynamics that are common

[6] Alternatively, one could collapse the panel into a pre-vs-post setting. In that case, and in order to level the field for cross-sectional comparisons, it is common to include in the regression firm-level initial characteristics (e.g., assets in *t-2*). However, this implies having a full set of control variables in the pre-attempt period. In our context, this would mean losing 15% of the sample, and thus losing substantial power in the IV estimates we discuss later on. See more on the relationship between the panel and cross-sectional estimations in Section 4.6.

to all IPO attempts (see, for example, Degeorge and Zeckhauser 1993). We also include an IPO-month effect for the post-IPO-decision period ($\alpha_{m\tau}$), which controls for the potential differeential effect of firms go public in January vs. firms that go public in other months near the end of the year, Firms that go public at the end of the year will have a short post-IPO first year vs. firms that go public in January will have a longer post-IPO firms yeat. Finally, the industry-by-calendar-year fixed effects ($\alpha_{jt}$) capture annual swings at the one-digit SIC level.[7] For example, Spiegel and Tookes (2020) argue that more than 50% of IPO decisions are related to industry trends.

Even within a sample of IPO attempts there can be selection effects for completing the IPO. For example, industry trends can induce many firms in an industry to attempt to go public, but on top of those trends there can be firm-specific signals that induce some firms to complete their attempt instead of withdrawing. This is problematic for a causal interpretation of the OLS coefficient on $IPO_{it}$. As in Pástor, Taylor, and Veronesi (2009), a positive shock to current firm profitability ($\varepsilon_{it}$) triggers the decision to complete the IPO. To the extent that there is mean reversion in profitability, future residuals in equation (1) will look unusually low (i.e., $\varepsilon_{it+1}$ and others will be low). Therefore, as the initial shock fades away, completed IPOs will show a larger drop in future profitability than withdrawn IPOs. This negative correlation between $IPO_{it}$ and residuals introduces a downward bias in the OLS estimate of $\beta$. In this case, OLS does not capture the true causal effect of going public on profitability.[8]

An indication of endogeneity is presented in Table 2. Panel A shows that, on average, firms that complete their IPOs are significantly less profitable and smaller (both in terms of assets and sales), and have lower leverage than withdrawn IPOs. This evidence is consistent with Busaba, Benveniste, and Guo (2001) who show that highly levered firms and firms

[7] Our results are robust to including industry-by-year fixed effects when industries are defined at the two-digit SIC level. See section 4.6 on robustness.

[8] Another possibility is that firms practice earnings management and inflate profitability before the IPO (Teoh, Welch, and Wong, 1998). In this case, the post-IPO period simply reveals the true nature of the firm, while the pre-IPO period is manipulated.

with more sales are more likely to withdraw their IPOs in the U.S. These differences suggest that, even this late in the IPO process, firms self-select into listed status. For instance, although all firms in this sample have announced their intention to list, larger firms can be less financially constrained (Hadlock and Pierce 2010), and hence be more likely to withdraw their IPO.

Insert Table 2 here

In order to interpret the effect of going public in a causal way we need exogenous variation in the decision to complete the IPO. Exogenous does not mean totally random, as could be the hypothetical case of a stock market regulator who runs a lottery for firms that have been short-listed for an IPO, analogous to lotteries for foreigners applying for visas. Exogenous does not mean either that the firm is unaware of the consequences of going public, or that the firm is subsequently surprised by what going public entails. Similarly, visa applicants are not unaware of what a visa implies. We need to clear a lower bar than that; namely, that the source of variation in IPO completion is uncorrelated with future firm outcomes except through the decision to complete the IPO.

Bernstein (2015) proposes as an instrument for IPO completion the market returns in the pre-IPO-decision period. He uses returns on the two months that follow the IPO filing date with the SEC. There is no uniform rule in Europe for IPO filings, nor a unique form such as the SEC's Form S-1, thus we need to count backwards from the actual date in which the decision to complete or withdraw the IPO is made.

The typical IPO process starts approximately six months before the planned date, in most cases by contacting an investment bank. A preliminary prospectus is submitted to the stock market regulator about one or two months before the IPO. However, most of the IPO-related activities, such as presentation to analysts, investor education, roadshow, and book-building, are reserved for the last month before the planned date. We focus on the market returns over these 30 days that precede the IPO completion or withdrawal date.[9]

[9]For example, if the IPO is on August 15, we compute the returns between July 15 and August 14.

Since we have IPOs from several countries we use the returns for the main stock index in each country.

It is common for firms to blame "poor market conditions" for their decision to withdraw the IPO. There are rational and behavioral explanations for this behavior, although we do not take a stance as to which explanation is more appropriate. According to Edelen and Kadlec (2005), if owners are focused on reaching a certain level of proceeds, then strong prior returns increase the likelihood that they will accept the underpricing that affects listings. Loughran and Ritter (2002) give a related explanation based on the prospect-theory preferences of owners. Finally, Derrien (2005) and Cornelli, Goldreich, and Ljungqvist (2006) argue that prior returns are a proxy for the mood or sentiment of IPO investors. Strong returns are a sign of overvaluation in this case. Irrespective of the particular explanation, strong market returns are likely to increase the willingness of owners to go through with the listing. Naturally, this relationship does not need to be deterministic, as in no IPO being completed when returns are low. Rather, high and low returns need only affect the likelihood of completion for those firms at the margin between listing or not.

The first stage of the IV estimation is then:

$$IPO_{it} = \gamma PreReturn_c \times Post_{it} + \alpha_i + \alpha_\tau + \alpha_{m\tau} + \alpha_{jt} + \zeta_{it} \tag{2}$$

In order to account for the time dimension in the panel we need to interact the market return in each country in the pre-decision period ($PreReturn_c$) with the indicator variable for the post-decision period ($Post_{it}$). This is necessary because the returns in the pre-decision period are only relevant to explain post-decision outcomes, and not whatever happened in, say, event-year *t-2* or *t-1*.

## 3.2 IV Assumptions

### 3.2.1 Balance and Instrument Relevance

In terms of instrument balance, Panel B in Table 2 shows that IPO attempts are not significantly different when comparing periods of high and low 30-day returns. This goes against the idea that high market returns coincide with a stronger cohort of firms attempting to list in the stock market. Perhaps high long-run returns (e.g., annual returns) coincide with a strong cohort of firms, but short-run returns do not seem to make a difference. Potential differences in the average quality of annual cohorts are captured by calendar time fixed effects in our regression. The absence of differences in pre-IPO variables across samples of short-run returns is consistent with as-random assignment of the instrument (Atanasov and Black 2016; Bennedsen, Nielsen, Perez-Gonzalez, and Wolfenzon 2007). It also means that excluding these pre-IPO characteristics, which are also absorbed by the firm fixed effects, does not bias the first-stage regression.

As seen in Panel B in Table 2, firms are 7.3% more likely to complete their IPO when market returns in the previous 30 days are higher than average. That is, when returns are on average 5% instead of -3.3%. This difference in completion rates suggests that the instrument is relevant for the IPO decision.

### 3.2.2 Exclusion Restriction

The exclusion restriction implies that 30-day market returns have no effect on future firm outcomes, except through their impact on the IPO decision. All firms in our sample are at the margin of listing. Some firms receive a “nudge” in the form of high (low) market returns and end up listing (withdrawing). Crucially, this nudge needs to have no influence on future outcomes except through its impact on the going public decision.

One challenge to the exclusion restriction is that the instrument may induce the self-selection of firms into listed status according to some unobservable measure of quality. Models of asymmetric information, such as Myers and Majluf (1984), predict that strong firms stay out of the market because raising equity is too costly for them. Therefore, withdrawn attempts should represent, on average, better firms than completed IPOs, which would bias the comparison against (instead of in favor) completed IPOs. Firm fixed effects capture unobserved, time-invariant firm quality, but there may still be a residual, time-varying element of quality. Hence, a potential concern is whether 30-day returns induce a sorting of firms based on an unobserved and time-varying element of quality.

It is important to note that standard asymmetric information models do not imply a sorting that invalidates our IV setup. When there is a time-varying asymmetric information (Lucas and McDonald, 1990), more firms decide to list after high market returns and the quality of the average issuer goes up. This happens because the asymmetric information discount is lower in hot markets. However, the relevant comparison in the IV setup is not between the average issuer after high and low returns, but between members of the subpopulation of *compliers*, i.e., agents that respond to the instrument as expected. In our case, compliers are firms that complete their IPOs after high returns, and firms that withdraw their IPOs after low returns. In asymmetric information models, the marginal issuer after high returns and the marginal withdrawal after low returns are both firms of similar quality. The IV only requires that all compliers are of comparable quality. A violation of the exclusion restriction would be that the completed IPOs after high returns are in someway better (or worse) than the withdrawn IPOs after low returns, but this is not what the asymmetric information models predict.

Overall, even in the presence of asymmetric information, there should be no bias when making comparisons across compliers. The very high quality firms that never issue (the "never-takers"), and the very low quality firms that always issue (the "always-takers"), are not part of these comparisons. Figure 3 summarizes the theoretical implications of asymmetric information models that are relevant for our setup.

Insert Figure 3 here

Another potential violation of the exclusion restriction would be that 30-day returns provide a valuable signal of higher expected profitability according to the market. Some firms pick up this signal and complete their IPOs to raise capital. Perhaps the signal is only relevant for the firms that decide to complete their IPO. Under this scenario, future profitability is not caused by listed status, but it is only correlated with it.

One immediate doubt about this alternative hypothesis is whether 30-day returns have predictive power for profitability in two or even three more years. As far as we know, this predictive power has not been documented in the literature. Another way to tackle this alternative hypothesis is to take advantage of the binary nature of the IPO decision. Unless the informational content of returns discontinuously drops at the IPO date, which is unlikely, one can expect that returns on the 30 days *after* the IPO decision also provide a useful signal for long-run profitability. Hence, a placebo test for our identification strategy is to use as instrument the returns immediately after the IPO decision, which cannot influence the IPO decision since it has already been taken. Given that the first stage of the IV setup is severed by construction in this case, then the second stage, which relates future outcomes to the IPO decision, should also be severed. We examine this possibility using reduced-form regressions that explore the predictive power of returns at different horizons.

# 4 The Effect of Going Public on Firm Profitability

## 4.1 OLS Results

Before showing results for regression (1), we report a close analog in Figure 4. We run regressions with separate event-year fixed effects for completed and withdrawn IPOs (firm fixed effects are also included, but industry-year fixed effects are not). In Figure 4 we report these event-year fixed effects ($\alpha_\tau$) for OROA. The effects on year *t-1* are normalized to zero. In Figure 4 we see a fall in the OROA of completed IPOs starting with the year of the IPO (*t=0*), and also going forward. This post-IPO drop in profitability of about 4 percentage points fits with the previous literature on U.S. IPOs (Degeorge and Zeckhauser 1993; Jain and Kini 1994; Mikkelson, Partch, and Shah 1997). Therefore, European and U.S. IPOs share similar profitability dynamics around the going public decisions.

A new finding is that withdrawn IPOs see a similar decline in profitability after their IPO attempt. In fact, for *t=0* and *t=1* the decline in profitability of completed and withdrawn IPOs is statistically indistinguishable. This suggests that the post-IPO drop in profitability is related to selection issues affecting both completed and withdrawn IPOs rather than a causal effect of going public. This is also in line with the conclusion of Spiegel and Tookes (2020) who argue that most IPOs anticipate (not cause) broad negative trends that affect all participants in an industry.

Insert Figure 4 here

Column 1 in Table 3 shows results for the OLS regression in equation (1). Firm OROA is not significantly different post IPO, consistent with what we see in Figure 4. Both completed and withdrawn IPOs face a similar decline in profitability after the attempt, so the difference-in-differences $\beta$ coefficient becomes negligible. As we noted earlier, this result does not take into account the endogeneity of the IPO decision. We now turn to the IV estimation that directly addresses endogeneity.

Insert Table 3 here

## 4.2 First-stage IV Results

Column 2 in Table 3 shows that 30-day returns sharply separate IPO filers into those firms that complete their IPOs and those that withdraw. If returns over the previous 30 days are positive, then the likelihood of listing increases by 6.9%. The first stage works well with the continuous measure of returns instead of the dummy for positive returns, but the interpretation is easier with the dummy variable so we use this specification throughout the paper. The first-stage F-statistic is also stronger with the dummy variable, which gives us more precision in the second stage.

The dummy for positive returns passes the standard threshold of an F-statistic of 10 using the Kleinbergen-Paap test for weak instruments, which is robust to non i.i.d. errors.[10] The F-statistic for the main instrument is 27.84 (see column 2), so the instrument is far from weak. In Table A.2 in the appendix we explore the power of returns at other horizons before the attempt to explain listing decisions (e.g., returns between days -60 and -30, or between -90 and -60). Although statistically significant, these other returns have less power than the returns over the previous 30 days, and present lower F-tests.

Column 3 in Table 3 explores the predictive power of market returns over the 30 days that follow the IPO decision. The coefficient is much smaller and statistically insignificant showing that post-IPO returns are not correlated with the decision to complete the IPO. Pre- and post-IPO returns are likely to capture similar market signals since they are adjacent in time. However, given that post-IPO returns are basically uncorrelated with the IPO decision, they represent a good placebo instrument that we can use to examine the validity of the exclusion restriction.

## 4.3 Second-stage IV results

Column 4 in Table 3 shows the second-stage results. We see that the IPO dummy is positively and significantly related to firm profitability. The IV result is in sharp contrast with the OLS result in column 1, where the coefficient on the IPO dummy was insignificant and close to zero. The IV result suggests that the post-IPO drop in profitability documented in the previous literature is a consequence of endogenous forces rather than a causal effect of going public. With a proper counterfactual (withdrawn IPOs) and as-random variation (given by the instrument), firms that go public appear to increase their profitability rather than

[10] An alternative F-test for weak instruments is proposed by Montiel-Olea and Pflueger (2013). Their test is not yet available for panel IV estimation. To implement their test, we estimate our main regressions with a LSDV model instead of a panel fixed-effects model. We find that the Olea-Montiel F-test is nearly identical to the Kleinbergen-Paap test that we report.

decrease it. Isolating the treatment effect of going public provides important new evidence, although it does not imply that selection effects are small or irrelevant. In fact, Maksimovic, Phillips, and Yang (2020) argue that selection effects are at least as large as the treatment effect of going public. We simply argue that the exogenous piece of the variation in listing decisions (i.e., the variation predicted by recent market returns) has a positive impact on profitability.

Our interpretation is that completed IPOs develop a focus on profitability that is not present among still-private firms. For example, Gear4music, a UK-based online retailer of musical instruments that went public in 2015, announced in 2017 that the company had recently open new distribution centers in Sweden and Germany. The CEO was quoted as saying: "This has been a transformational year for the business, with further expansion of the Gear4music brand driving record sales and profits." It is likely that, in exchange for capital, public investors put pressure on firms to achieve profitability. This pressure seems to promote profitability more than the potential agency costs, at least for the first few years after going public.

Column 5 in Table 3 shows results for the regression of OROA directly on the dummy for positive 30-day returns interacted with Post. The coefficient represents the reduced-form or intent-to-treat effect. The result is consistent with the exclusion restriction holding, as the coefficient from the reduced-form regression (1.6%) is approximately the coefficient from the first-stage regression (6.9%) times the coefficient from the second-stage regression (23.3%). If there was an additional direct effect from short-term returns on long-term profitability, the reduced-form coefficient could have been significantly different (e.g., in the hypothetical case that after strong returns firms receive a long-term benefit such as perpetually lower cost of capital). Although consistent with as-random assignment, the fact that the coefficient on the reduced-form regression is well-behaved is merely suggestive (Atanasov and Black, 2016).

More compelling evidence on the validity of the exclusion restriction is presented in column 6 of Table 3. The results show that the placebo instrument (*Positive 30 days forward*

*X Post*) has no bearing on post-IPO outcomes in a reduced-form regression. If market returns were acting as market signals that correlate with future profitability, we should find a direct effect on profitability, regardless of whether returns are from right before or right after the IPO attempt. However, if the effect on profitability is coming from the IPO decision itself, then only when returns alter the IPO decision (i.e., when there is a first stage effect) there should be an effect on profitability. Overall, the results speak in favor of the exclusion restriction since, when market returns are uncorrelated to the IPO decision (column 3 in Table 3), there is no effect on profitability either (column 6 in Table 3).

## 4.4 Magnitude of the IV Coefficients

The IV coefficient in column 4 of Table 3 is large, but plausible considering our sample of small, high-growth firms. Firms that go public increase OROA by 23 percentage points, or close to one standard deviation of profitability in this sample (which includes within- and across-firm variation). For instance, a firm in the 20th percentile of the distribution of profitability would jump to close to the 80th percentile of the distribution in response to going public. The within-firm standard deviation of OROA in our sample is 19%, so the going public effect entails a similar increase of 1.2 standard deviations if we consider only within-firm changes.

Large IV coefficients relative to OLS coefficients could indicate an weak instrument problem, whereby a weak instrument amplifies a potentially small violation of the exclusion restriction. We address this issue from both the econometrics and theory angles following the recommendations of Jiang (2017).

First, from the econometric perspective, we already showed that our instrument passes standard weak-instruments tests, including those robust to non-i.i.d. errors (see the K-P F-statistic in column 2 of Table 3). Similarly, our inference is robust to the adjustment of standard errors as a function of first-stage F-statistics proposed by Lee, McCrary, Moreira, and Porter (2021).[11] We study further empirical robustness in Section 4.6, and all auxiliary tests imply that our inference is on firm ground.

Second, from the perspective of economic theory, we argue that the magnitude of the IV coefficient is plausible. The first thing to note is the direction of the endogeneity bias in OLS. Several theories suggest that this bias should be negative. First, asymmetric information models imply that firms that complete IPOs are on average worse, i.e., less profitable, than firms that withdraw their attempts. Second, Pástor, Taylor, and Veronesi (2009) argue that mean-reversion in profitability should lead to a negative bias in estimating post-IPO profitability. Hence, from an ex-ante perspective we should not be surprised that IV coefficients are larger than OLS coefficients.

Also from the theory perspective, it is worth noting that a change in strategy associated with going public can result in a one-time large change in performance like the one we document. A jump in profitability is likely to be needed in equilibrium to compensate for the large costs involved in going public. For example, Gahng, Ritter, and Zhang (2021) show that the costs of the median IPO are close to 5% of market capitalization (e.g., underwriter commissions, underpricing, etc.). Save for exceptions, going public happens only once in a firm's life cycle, so it is not unreasonable to think that it can change the course of a firm's history in a dramatic way.

## 4.5 External Validity and Compliers

The IV approach identifies the effects of going public by focusing on *compliers*, i.e., the sub-population of firms whose IPO decision is affected by prior market returns. This local average treatment effect (LATE) may not coincide with the average treatment effect (ATE) in the population. In order to assess the external validity of our results it is important to identify and characterize the complier population.

[11] Standard errors in column 4 of Table 3 have to be multiplied by a factor of 1.008 as implied by the Cragg-Donald Wald $F$-statistic of 97.06 in our first stage (column 2 in Table 3). See Table 3.a in Lee, McCrary, Moreira, and Porter (2021).

Compliers can be understood as firms who were at the "margin" of completing their IPO before they were nudged by market returns to complete or withdraw. Compliers cannot be individually identified, because we observe each firm's decision once and not under different return scenarios. However, the first stage coefficient gives us the fraction of compliers in the full sample. As implied by column 2 in Table 3, compliers represent 6.90% of the sample. This size of the complier population is comparable to other IV studies (see, for example, Table 4.4.2 in Angrist and Pischke 2009), and hence it is enough for identification purposes.

We can further decompose the complier population into the fraction of compliers among the completed (treated) and the withdrawn (untreated) IPOs. The fraction of compliers among completed IPOs is given by:

$$P(\text{Complier}|\text{Completed IPO}) = \frac{\text{1st Stage Coefficient} \times P(\text{Positive 30 day returns})}{P(\text{Completed IPO})} \quad (3)$$

As we show in Panel A of Table 4, the fraction of compliers among completed IPOs is 4.87%. Analogously, the fraction of compliers among withdrawn IPOs is 34.38%, which implies that market returns have a stronger incidence in the decision to withdraw than in the decision to complete. A larger fraction of withdrawn IPOs would have been completed if returns were positive, than the fraction of listed IPOs that would have been withdrawn if returns were negative. This fits with the fact that firms more often cite poor market conditions as a reason to withdraw than good returns as a reason to complete.

Insert Table 4 here

In Panel B of Table 4 we study the characteristics of compliers. The relative likelihood that compliers have a certain characteristic is given by the ratio of the first stage coefficient in the sub-sample with that characteristic over the first stage coefficient for the full sample. A ratio of one means that compliers are as likely as the rest of the sample of having a given characteristic. For example, the likelihood that compliers are large firms (i.e., with assets above the sample median) is just 1.04 times the likelihood in the full sample. Compliers are marginally more likely to have high pre-IPO OROA (ratio 1.12), to be older (ratio 1.17), and to come from countries with high disclosure (ratio 1.48) and high anti-self-dealing index (ratio 1.27).[12] These characteristics are typically associated with strong firm quality. Hence, in line with Figure 3, compliers are unlikely to come from the bottom of the quality distribution. Overall, compliers are comparable to the average firm in the sample, which suggests that the results carry over to the population of IPO attempts.

### 4.6 Robustness

IV estimations have been subject to recent scrutiny (see, among others, Atanasov and Black 2016, Lee, McCrary, Moreira, and Porter 2021, Young 2022). For, instance, Young (2022) shows that many IV setups are sensitive to minimal changes in the sample definition. He finds that results are often not robust to deleting just one cluster of observations. In our case, clusters are defined at the firm level, so we check whether results are robust to excluding firms. As seen in Figure 5, the coefficient and p-values for the main profitability regression are tightly estimated in the samples that result from excluding one firm at a time. The largest p-value that we find is 4%, while the smallest is 2.6%. Hence, our results are not levering up on just a few extreme observations.

Insert Figure 5 here

Another indication of potential problems with the IV estimation is that confidence bands often contain the OLS point estimate. In our case, the IV confidence bands (from column 4 in Table 3) reject the OLS estimate (column 1 in Table 3). Overall, our statistical inference is on firm ground.

In Table 5 we show that the first and second stages are robust to changes in sample definition and empirical specification. For example, our main results do not vary in significance

[12]The disclosure index of IPO prospectuses is taken from La Porta, López-de-Silanes, and Shleifer (2006). The anti-self-dealing index, which measures investor protection, is taken from Djankov, La Porta, López-de-Silanes, and Shleifer (2008).

if we include events dropped in our cleaning procedure, nor if we exclude particular events such as cross-listings. The IV coefficient for profitability varies between 21% and 25.8% in the different robustness exercises in columns 1 through 5.

Include Table 5 here

The event window that we study goes up to the end of the second calendar year after the IPO attempt (e.g., for an IPO attempt in the year 2010 we take data up to 2012). In columns 6-8 of Table 5 we gradually extend the post-attempt window up to the end of the fifth year without changing the sample of firms. The statistical significance and magnitude of the coefficients are very stable. If anything, the effect on profitability increases with the horizon. Extending the pre-event window is problematic because of confounding events and sample attrition. Fewer firms have observations on very early years. There is also sample attrition if we keep extending the post-attempt window.

Finally, in Table A.3 we connect the panel estimation with a cross-sectional alternative where the dependent variable is simply the change in profitability between the post- and pre-attempt periods. The panel regression has advantages like the possibility to add firm and event time fixed effects. Still, the cross-sectional regression can showcase the variation in the data in an interesting way. The IV coefficient in the profitability regression increases from 23.3% in the baseline panel estimation to 51.3% in the cross-sectional regression (significant at the 5% level). The cross-sectional regression boosts the coefficient mainly through the inclusion of country and filing year fixed effects, which in the panel regression can be replicated by adding country times Post and filing year times Post fixed effects. In the next section, we explore the heterogeneity to country and other variables that this suggests.

# 5 Mechanisms

## 5.1 Cross-sectional Variation in the Effect of Going Public On Profitability

In Table 6 we examine the heterogeneity of our results to country and firm-level variation. Adding an interaction term between the endogenous IPO decision and a time-invariant characteristic requires estimating two first stages: One for the IPO dummy and one for the interaction term. The standard approach is to use as additional instrument the interaction of the cross-sectional characteristic with the time-varying instrument. We present the extended first-stages in Table A.4.

Include Table 6 here

The first characteristic that we study is related to informational frictions. We capture cross-country variation in information with an index for disclosure requirements in the IPO prospectus (La Porta, López-de-Silanes, and Shleifer, 2006). The variable *High Disclosure* takes a value of one when the disclosure index is above the sample mean. In column 1 of Table 6 we find an incremental positive effect of high disclosure on OROA of 2.4 percentage points. If more and better information is disclosed, then IPO capital is more likely to be used efficiently.

A second characteristic is related to agency problems. One potential disadvantage of going public is the dispersion of ownership, which results in weaker incentives for owner-managers (Jensen and Meckling, 1976). If agency is a serious concern, then the benefits of going public should be stronger when minority investors are better protected by laws and regulations. We capture cross-country variation in investor protection using the anti-self-dealing index of Djankov, La Porta, López-de-Silanes, and Shleifer (2008), which reflects how difficult it is for corporate insiders to get away with the diversion of resources and opportunities. In column 2 of Table 6 we find an additional positive impact of investor protection on OROA. For example, a firm that goes public in the UK (high investor protection) would achieve a 2.5% higher profitability than a similar firm going public in Germany (low investor protection).

In practice, investor protection and disclosure are positively correlated. Hence, whether the incremental effect in profitability can be attributed to better investor protection or to better disclosure is not entirely clear. The key takeaway from studying country-level variation is that the positive effects of the IPO are stronger when there is a more investor-friendly environment.

We now study variation according to firm characteristics. One of the advantages of going public is to improve a firm's access to capital. For example, when studying private and public companies in the U.K., Brav (2009) concludes that private equity is more costly than public equity. If this is the case, then the benefits of going public should be more prevalent among firms that face higher costs of external funds. Traditional indicators of financial constraints include firm size and age (Hadlock and Pierce, 2010). We use dummy variables for small and young firms compared to the sample mean of assets and age before the IPO attempt. Column 3 in Table 6 shows that the impact of going public on profitability is 3.4 percentage points lower for small firms. The differential effect on younger firms in column 4 is economically small and statistically insignificant. At least preliminarily, the evidence is not consistent with standard measures of financial constraints. The results are consistent with small and large firms being at different stages of their life cycle. Large firms show a more immediate return to becoming public firms since they have grown their capital base earlier. We explore this hypothesis in more detail next by studying changes in assets and sales as firms go public.

## 5.2 Commercialization

Higher profitability can be achieved in several ways. One obvious way is by increasing sales, which we explore in Table 7. The OLS coefficient on sales is large and positive (column 1), but the IV coefficient, although larger than the OLS coefficient, is not statistically significant (column 2).

Include Table 7 here

When we add interactions we find interesting sources of heterogeneity. IPO firms in high disclosure countries add fewer sales than the average IPO (column 3). However, these firms also add fewer assets (column 6), and therefore end up with a higher ratio of sales over assets (column 9). This can explain the positive differential effect of disclosure on profitability that we find in Table 6.

Small and young firms add significantly more sales after going public, as implied by the positive interactions of *IPO* with the dummy variables for small and young (column 3). Small and young firms are simultaneously adding more assets (column 6). The pace at which small firms add assets is stronger than the pace at which they add sales, hence the negative coefficient of *IPO x Small Firm* on the regression for sales over assets (column 9). This can explain the negative differential effect of going public on the profitability of small firms (column 3 of Table 6). The most natural explanation is that small firms need to invest more heavily in the early part of their growth path. Going public pushes firms along their life cycle with heterogeneous effects on growth and profitability since the starting point is different for different firms.

The last three columns in Table 7 show the results for sales per employee, which serve as a proxy for labor productivity. We find a strong average effect of going public on productivity (column 11). None of the interactions is significant (column 12), which implies that the effects on sales per employee do not depend on country or firm characteristics. Irrespective of the heterogeneous paths to profitability suggested by Table 6, the unconditional effect on sales per employee is positive and strong, which points to a shift in strategy towards commercialization and efficiency.

## 5.3 Firm Scope

In Table 8 we provide evidence of changes in firm scope as firms go public. We find that completed IPOs expand significantly towards multiple industries, new subsidiaries, and international markets. For example, Vexim, a French manufacturer of medical equipment that went public in 2012, announced in 2013 it was opening offices in Spain and the UK to serve those new markets. The effects are statistically significant for both OLS and IV, but the magnitudes are much bigger for the IV estimation.

Insert Table 8 here

As indicated by the negative coefficient on *IPO x Small Firm*, small firms tend to grow more organically. Small firms aggressively increase sales and assets, as we saw before, but diversifying less into more industries (column 3), and without adding as many subsidiaries (column 6), nor stepping into more countries (column 9). Larger firms, instead, step outside their current market segments in order to achieve higher margins and profitability.

We also examine whether the IPO firm engages in acquisitions or becomes the target of an acquisition after the IPO. In Table 9 we find a significant increase in acquisition activity with OLS (column 1), in line with Arikan and Stulz (2016) who find that recent IPO firms make many acquisitions (although relative to older *public* firms, not relative to withdrawn IPOs like we do). The effect is very small and not significant once we instrument for the IPO decision (column 2). This also shows that the IV estimates are not always bigger than the OLS estimates, as would be implied by a weak-instruments problem. We still find that young firms make significantly more acquisitions after listing (column 3). In columns 4 to 6 we show results where the dependent variable is a with a dummy equal to one if the firm is the target of an acquisition in the 5 years after follow the IPO attempt. We find that IPO firms are not more likely to become targets of acquisitions than withdrawn IPOs.[13]

[13] Gao, Ritter, and Zhu (2013) argue that, instead of going public, many small firms prefer to be acquired by a large organization to bring their product to market faster and more efficiently than what they can do by growing on a stand-alone basis. This suggests a higher likelihood of being the target of an acquisition among withdrawn firms, which we do not find in Table 9.

Insert Table 9 here

### 5.4 Patents

In Table 10 we explore the impact of going public on patenting activity. We focus on the number of eventually granted patent applications, since we do not have data on patent citations like Bernstein (2015) for U.S. firms. We examine the impact of the IPO decision by itself and also interacted with country and firm characteristics.

Insert Table 10 here

Using the IV setup we find a negative average impact of going public on patent applications (column 2 in Table 10), which is consistent with firms becoming less innovative as they go public (Bernstein, 2015). However, this average effect is not statistically significant. We find a stronger and significant negative effect on patents for IPOs in countries with high disclosure (column 3) and high investor protection (column 4). Hence, in markets that resemble the U.S. (Bernstein's sample) in terms of investor friendliness, we find that the number of patents declines after going public.[14] This does not prevent young firms from a boost of innovation once listed, as indicated by the last column in Table 10.

The negative impact on innovation, combined with the positive impact on profitability and sales per employee, again suggests that there is a shift in strategy from exploration and searching for new ideas to commercialization. Private firms provide strong incentives to discover patentable ideas. When firms enter public markets, they are move towards commercialization and a focus on higher margins.

[14] The negative coefficient on the interaction of IPO and high disclosure is also consistent with the results in Dambra and Gustafson (2021) who study the innovation of newly listed firms using variation in disclosure rules across the U.S.

## 5.5 Going Public: Capital Raising and Beyond

Going public is, in essence, a capital infusion, but it may be special compared to other capital infusions. In this final section, we explore three related issues that can help us understand the uniqueness of capital raising at IPOs. First, we study whether withdrawn-IPO firms substitute their lack of public equity with other sources of funds. Second, we study whether IPOs lead to changes in firms' management that could help explain the shift in strategy towards commercialization. Third, we explore whether IPOs differ from other instances of equity raising in firms that are already public (i.e., SEOs).

We address the first issue with our sample of completed and withdrawn IPOs. From the balance sheet data, we identify years in which debt or shareholders' net funds (i.e., paid capital) increase by more than 100%. This captures instances of capital raising that are comparable to IPOs. For example, on average, shareholders' funds increase by a factor of 4 in the IPO year.

In Table 11 we present results for our main IV specification using as dependent variables dummies for the years of large increases in shareholders' funds and debt. When using the dummy for large increases in shareholders' funds we do not consider equity increases during the IPO year in the case of completed IPOs, otherwise we would mechanically get a positive coefficient. In column 1, we find a small and insignificant coefficient on the IPO dummy, meaning that, outside the IPO itself, there is no relevant difference in capital increases between completed and withdrawn IPOs. This is consistent with the idea that withdrawn firms do not easily tap private equity markets once they stay away from public equity (Brav, 2009).

Insert Table 11 here

We find a negative and significant coefficient for the IPO dummy in the case of large increases in debt growth (column 2). This suggests that withdrawn IPOs raise more debt to compensate for the lack of equity financing. Hence, there is at least partial substitution of public equity with other sources of funds. However, it is only partial since IPO firms tend to increase the size of their balance sheets compared to withdrawn IPO firms (i.e., assets grow more in completed IPOs as seen in Table 7). The key takeaway is that the large capital raising is hard to replicate outside IPOs.[15]

Second, we study management turnover for completed and withdrawn IPO firms. In particular, whether the pressure from stock market investors to achieve higher profitability translates into executive turnover. In columns 3 and 4 of Table 11, we study the frequency of changes in CEO and CFO. We find that it is 14.9% (5.6%) more likely to see a change in CEO (CFO) in completed than in withdrawn IPOs. Hence, going public has an impact on corporate governance. Arm's-length capital is unlikely to have these effects.

Finally, we study profitability around other events of capital raising in public firms, in particular, SEOs. Butler, Fauver, and Sypiridopoulos (2019) consider SEOs as a sort of placebo for IPOs in order to disentangle capital raising from other channels. The study of SEOs is also motivated by Loughran and Ritter (1997) who show that there is a drop in profitability after SEOs similar to the drop after IPOs. In Figure 6 we show the OROA of 500 European SEO firms and matched firms in this sample period. The match is based on country, year, size, and profitability in the year before the SEO. We find a slight decline in profitability of around one percentage point two years after the SEO. There is a similar decline in the firms' profitability in the matched sample. Hence, the drop in profitability is not comparable to the one seen for IPOs. Profitability dynamics seem more stable in public firms raising capital through an SEO. This is consistent with the idea that IPO firms are subject to a much larger shock since they receive a larger (relative-to-size) capital infusion and go through a change in strategy and corporate governance as they become listed.

Insert Figure 6 here

Overall, the impact of going public seems hard to replicate through other capital raising

[15] From *Zephyr* we also collect data on venture capital deals and syndicated loans. We find that there is no significant difference in the use of these funding sources when comparing completed and withdrawn IPOs. The data coverage for private firms is, however, spotty.

activities due to its magnitude and because it goes above and beyond simply raising capital. By going public, firms are pushed to change along several directions, including their profitability, scope, patenting activity, and corporate governance, which is not the case with other instances of capital raising.

## 6 Conclusions

This paper sheds light on the consequences of going public using a large sample of close to 3,400 firms that file for an initial public offering in 16 European countries over 1997-2017. Our data contains pre- and post-filing financial information irrespective of whether firms complete their IPO or not (i.e., for completed and withdrawn IPOs). The panel structure of our data allows us to control for a host of self-selection issues and life-cycle patterns. We directly address the endogeneity of IPO completion following the strategy in Bernstein (2015). We instrument for IPO completion using short-term market returns before the IPO decision. Market returns in this short window can affect the decision to complete the IPO, but are unlikely to directly affect long-run outcomes.

Consistent with prior finding in the literature, we show that firms profitability goes down after the IPO. However, we also show that the profitability of withdrawn-IPO firms also goes down after the IPO, so the OLS differences-in-differences effect on firm performance is essentially zero. After correcting for endogeneity, we find a strong a positive effect on performance. Using a proper counterfactual and tackling the endogeneity problem, we find increased profitability in firms that go public - a result that is the opposite of the prior literature.

We also show that firms increase sales per employee, expand their operations to more countries and operate more subsidiaries post-IPO - consistent with increased commercialization. Exploiting cross-sectional variation across countries we find that the effects on profitability are stronger in investor-friendly countries. However, patenting activity goes down in investor-friendly environments. Both of these findings are consistent with a switch in firm strategy from exploration and innovation towards commercialization.

Our findings support the proposition that firms benefit by going public despite potentially higher agency problems after the IPO. Becoming publicly traded provides financial capital to firms, which allows them commercialize their products. Our results are consistent with IPO firms increasing profitability in the years subsequent to the IPO by changing their strategy from innovation to commercialization.

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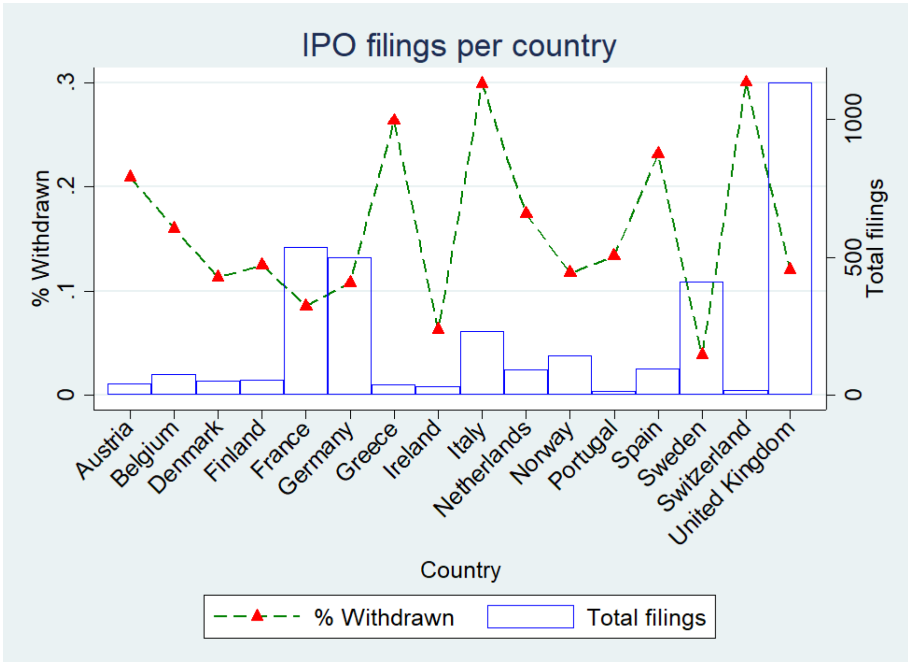
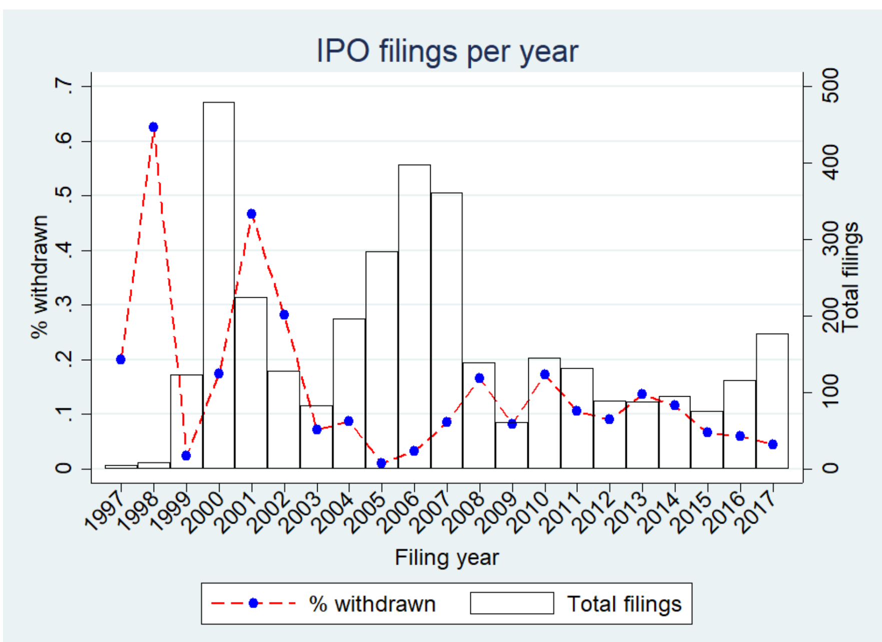
Figure 1: IPOs over Countries and Years



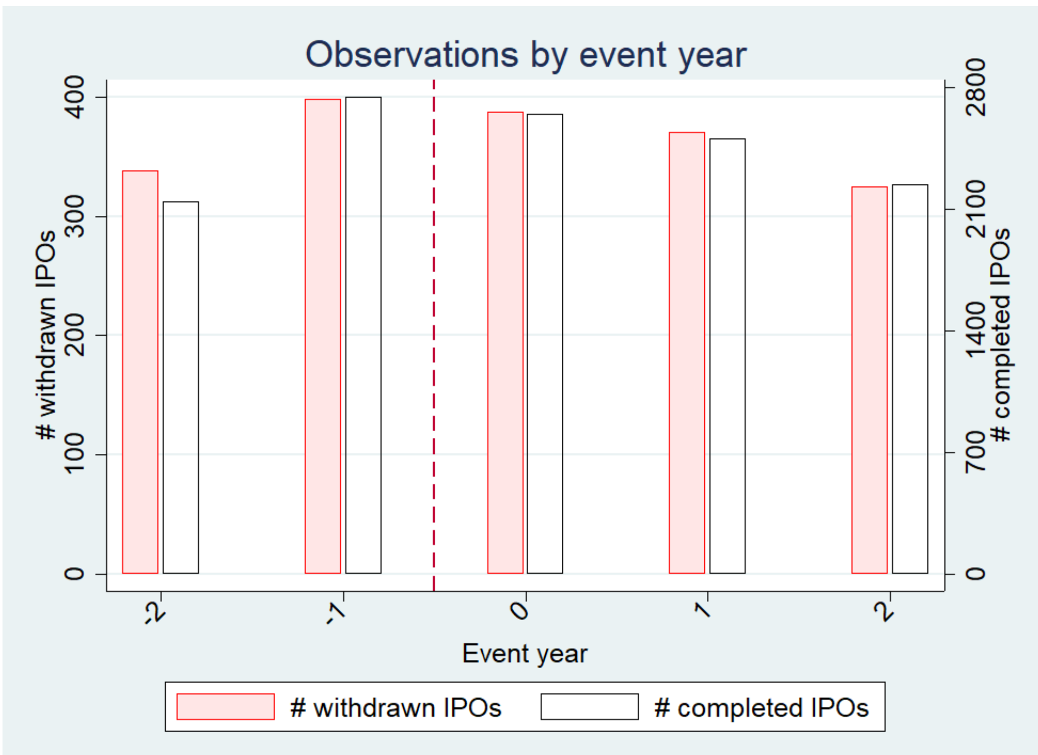

(a)




(b)

**Notes**: This figure shows the total number of IPO filings and the fraction of withdrawn IPOs by country of listing (panel a) and year (panel b).

Figure 2: Observations by Event Year

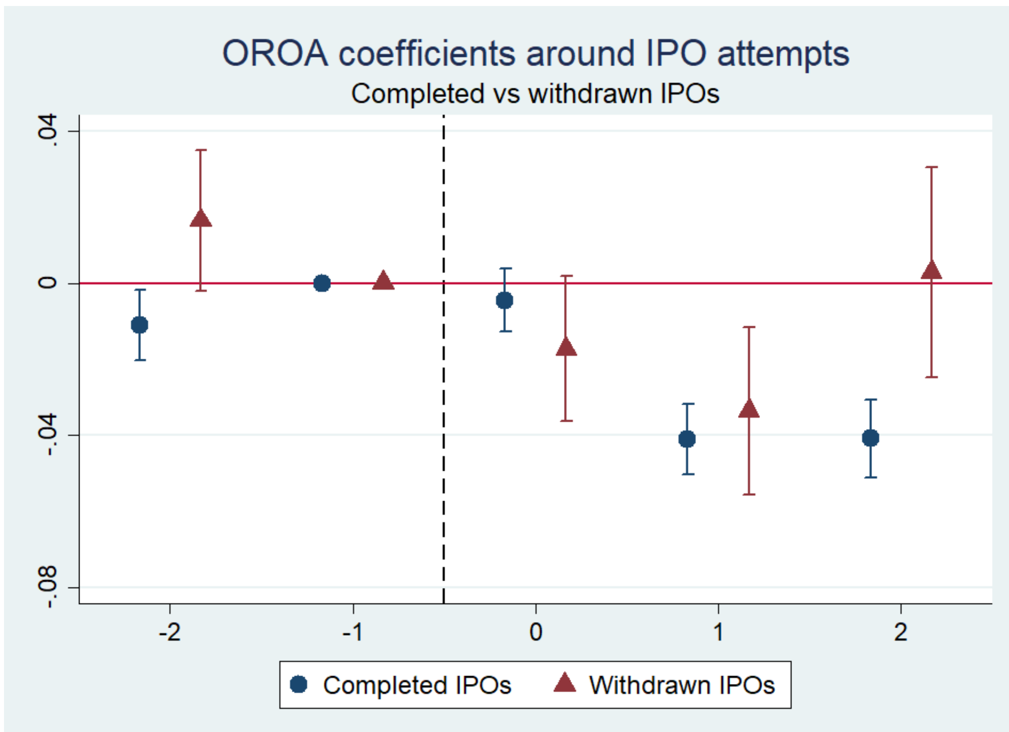



**Notes**: The figure shows the number of observations per event-year for withdrawn IPOs (dark bar, left axis) and completed IPOs (light bar, right axis). Event years are measured at the end-of-the-year around the IPO-attempt year (t=0).

Figure 3: Asymmetric Information and Market Returns

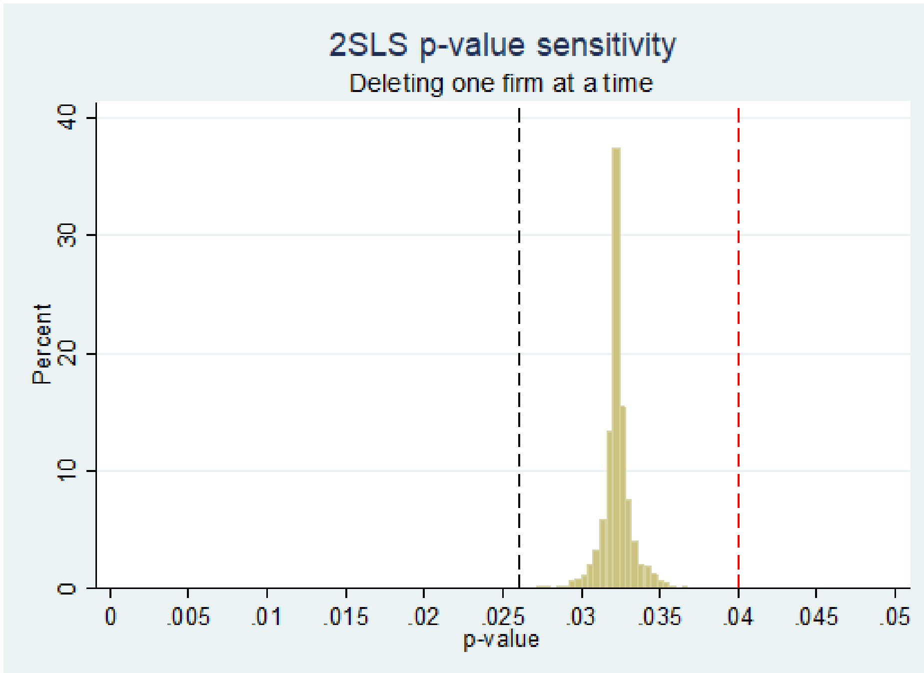

**Notes**: The figure shows the theoretical predictions of asymmetric information models regarding the decision to complete or withdraw the IPO according to (unobservable) firm quality and past market returns. More firms complete their IPOs after positive returns, and the average quality of issuers goes up. In the language of treatment effects, firms that complete or withdraw their IPOs in response to past returns are "compliers." Compliers are of intermediate quality. Firms that complete (withdraw) their IPOs regardless of past returns are "always takers" ("never takers").

Figure 4: Profitability by Event Year for Completed and Withdrawn IPOs

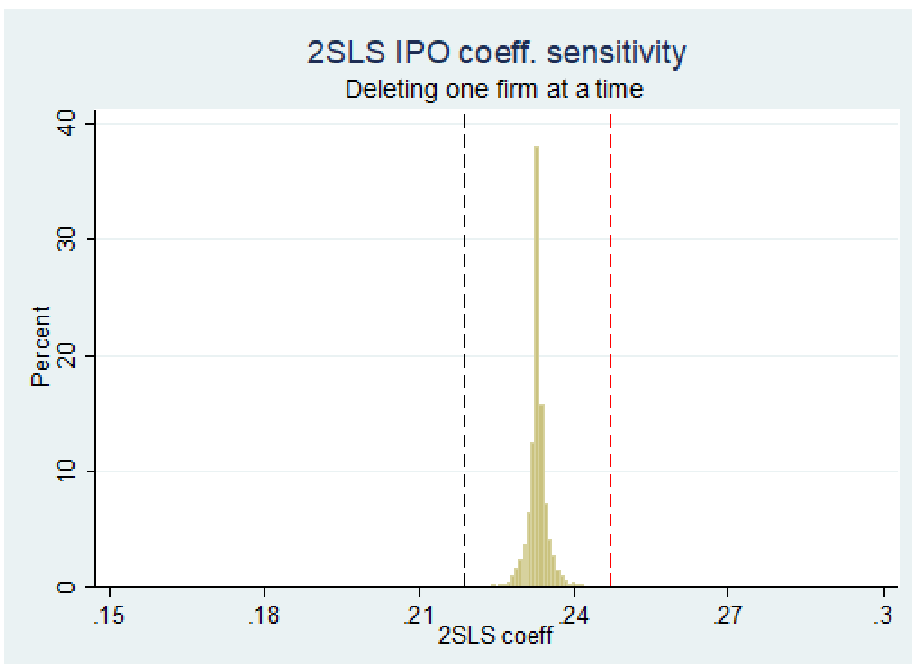



**Notes**: The figures displays coefficient estimates of event-time fixed effects. The dependent variable is run against event-time fixed effects for completed and withdrawn IPOs, setting t=-1 as the default category. The regressions include firm fixed effects to account for within-firm dynamics.

Figure 5: Statistical Inference when Excluding Clusters



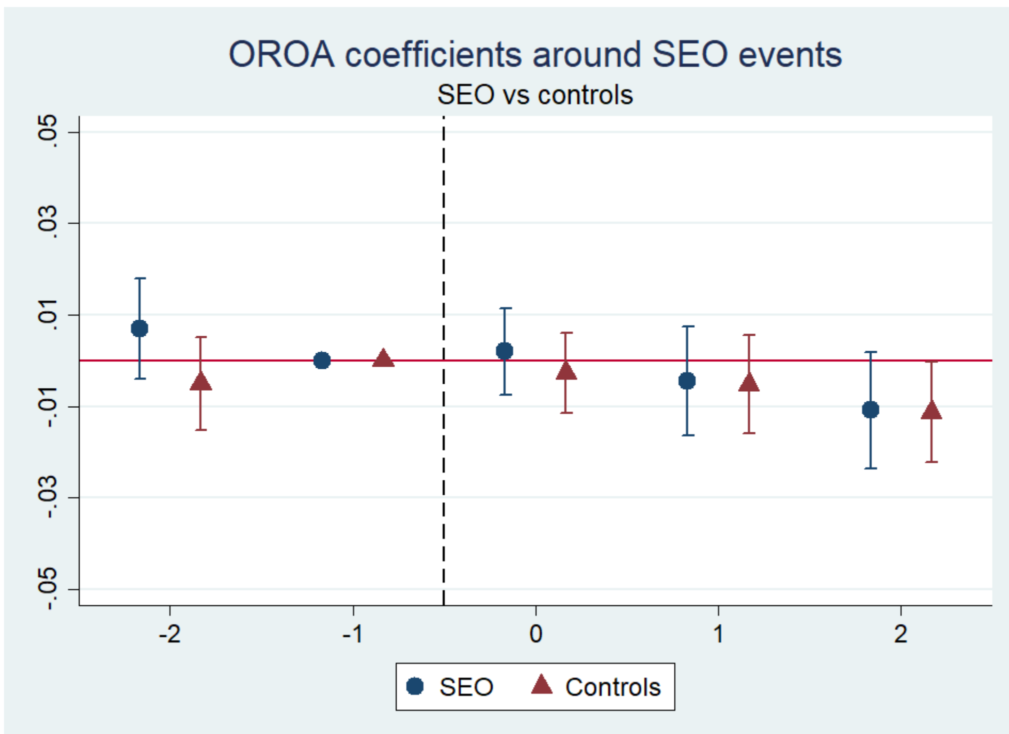

(a)




(b)

**Notes**: The figure shows the distribution of $p$-values (panel a) and $\beta$ coefficients (panel b) from the second stage OROA regression of the multiple samples that result from excluding one firm (cluster) at a time. Dashed vertical lines show the maximum and minimum values obtained.

Figure 6: SEOs




**Notes**: The figures displays coefficient estimates of event-time fixed effects for OROA in a sample of SEO firms and matched control firms based on country, year, size, and profitability the year before the SEO. ROA is run against event-time fixed effects setting t=-1 as the default category. The regressions include firm fixed effects to account for within-firm dynamics.

## Table 1: Summary Statistics

Completed IPO is an indicator variable that takes a value of 1 if a firm completed an IPO and 0 otherwise. Post is an indicator variable that takes a value of 1 for the IPO-attempt year and after; 0 otherwise. IPO is the Completed IPO dummy times the Post dummy. OROA (operating return on assets) is EBIT over book assets. Assets (Sales) is book assets (yearly sales) in 2019 million Euros. Sales are also reported over the number of employees (Sales(MM)/Empl.) and over book assets (Sales/Assets). Age is calendar year minus incorporation year. Subsidiary is a dummy for when the firm has a subsidiary. Multiple industries is a dummy for when subsidiaries are in different 3-digit SIC industries. International is a dummy for when at least one subsidiary is in a different country from the firm's headquarters. Number of countries counts the countries where the subsidiaries (# Subs.) are located. Patents is the number of patent applications eventually granted. Acquisitions is the number of acquisitions undertaken by a firm in a year. Returns 30 days is the market return (country-index) where the firm is listed, for the month preceding the IPO attempt. Positive 30-day ret is a dummy for when 30-day returns are positive. Accounting variables are winsorized at the 1% level.

| | Mean | P10 | P25 | P50 | P75 | P90 | SD | Total |
|---|---|---|---|---|---|---|---|---|
| Completed IPO | 0.87 | 0 | 1 | 1 | 1 | 1 | 0.34 | 14,110 |
| Post | 0.6 | 0 | 0 | 1 | 1 | 1 | 0.49 | 14,110 |
| IPO | 0.52 | 0 | 0 | 1 | 1 | 1 | 0.5 | 14,110 |
| OROA | -0.02 | -0.35 | -0.09 | 0.03 | 0.1 | 0.19 | 0.24 | 12,321 |
| Assets (MM) | 172.77 | 0.48 | 2.34 | 10.6 | 55.21 | 333.48 | 598.43 | 13,696 |
| Sales(MM) | 318.67 | 0.45 | 3.73 | 20.27 | 113.76 | 640.92 | 1,085.94 | 12,332 |
| Sales(MM)/Empl. | 0.65 | 0.04 | 0.09 | 0.18 | 0.4 | 1.13 | 2 | 9,801 |
| Sales/Assets | 2.69 | 0.11 | 0.66 | 1.96 | 3.65 | 6.01 | 2.9 | 12,024 |
| Age | 11.31 | 1 | 3 | 7 | 13 | 24 | 15.22 | 12,883 |
| Subsidiary | 0.35 | 0 | 0 | 0 | 1 | 1 | 0.48 | 14,110 |
| Multiple Industries | 0.19 | 0 | 0 | 0 | 0 | 1 | 0.4 | 14,110 |
| International | 0.09 | 0 | 0 | 0 | 0 | 0 | 0.28 | 14,110 |
| # Countries | 1.22 | 1 | 1 | 1 | 1 | 1 | 0.93 | 14,110 |
| # Subs. | 3.24 | 0 | 0 | 0 | 2 | 7 | 11.3 | 14,110 |
| Patents | 0.49 | 0 | 0 | 0 | 0 | 0 | 7.8 | 14,110 |
| Acquisitions | 0.23 | 0 | 0 | 0 | 0 | 1 | 0.71 | 14,110 |
| Returns 30 days | 0.01 | -0.06 | -0.02 | 0.01 | 0.04 | 0.07 | 0.06 | 14,110 |
| Positive 30-day ret | 0.62 | 0 | 0 | 1 | 1 | 1 | 0.49 | 14,110 |

Table 2: Average Firm Characteristics by IPO status and Market Returns

This table shows averages of the main variables for sample splits before the IPO attempt. Panel A presents the means and differences according to IPO status (treatment): withdrawn vs. completed IPOs. Panel B shows the means and differences according to pre-attempt market returns above or below the sample mean (exposure to the instrument). Significant at: *10%, **5% and ***1%.

Panel A: Split by Endogenous Treatment

| Variable (pre IPO attempt) | Completed IPO | Withdrawn | Diff. |
|---|---|---|---|
| OROA | -0.015 | 0.013 | -0.028** |
| Assets (MM) | 135.69 | 251.926 | -116.236*** |
| Sales (MM) | 255.154 | 540.566 | -285.412*** |
| Sales(MM)/Employees | 0.579 | 0.843 | -0.264** |
| Sales/Assets | 2.87 | 3.078 | -0.207 |
| Multiple Industries | 0.08 | 0.1 | -0.020* |
| Subsidiary | 0.153 | 0.166 | -0.014 |
| International | 0.034 | 0.048 | -0.014* |
| Leverage | 0.541 | 0.571 | -0.030* |
| Acquisitions | 0.075 | 0.122 | -0.047** |
| Patents | 0.413 | 1.022 | -0.61 |
| Returns 30 days | 0.011 | -0.009 | 0.020*** |
| Positive 30-day ret | 0.645 | 0.428 | 0.217*** |
| # of firms | 3,037 | 430 | |

Panel B: Split by the Instrument

| Variable (pre IPO attempt) | High Returns | Low Returns | Diff. |
|---|---|---|---|
| OROA | -0.018 | -0.005 | -0.012 |
| Assets (MM) | 146.306 | 152.721 | -6.415 |
| Sales (MM) | 284.995 | 296.418 | -11.423 |
| Sales(MM)/Employees | 0.551 | 0.671 | -0.12 |
| Sales/Assets | 2.838 | 2.953 | -0.115 |
| Multiple Industries | 0.082 | 0.082 | 0 |
| Subsidiary | 0.154 | 0.155 | -0.001 |
| International | 0.039 | 0.033 | 0.006 |
| Leverage | 0.542 | 0.547 | -0.005 |
| Acquisitions | 0.087 | 0.075 | 0.012 |
| Patents | 0.494 | 0.483 | 0.011 |
| Returns 30 days | 0.05 | -0.033 | 0.083*** |
| Positive 30-day ret | 1 | 0.237 | 0.763*** |
| Completed IPO | 0.912 | 0.84 | 0.073*** |
| # of firms | 1,733 | 1,734 | |

Table 3: The Effects of Going Public on Firm Profitability: Main Results

Column 1 presents results of the OLS regression of OROA using equation 1. Column 2 is the first stage of the IV setup where the instrument is a dummy for positive returns over the 30 days before the IPO is completed or withdrawn. Column 3 is a placebo first stage using a dummy for positive market returns in the 30 days after the IPO is completed or withdrawn. Column 4 is the second stage of the IV setup that follows the first stage in Column 2. Column 5 is the reduced form regression of OROA directly on the dummy for positive returns. Column 6 is a placebo reduced form regression of OROA on the dummy for positive returns on the following 30 days. We report the Kleibergen-Paap F-test for weak instruments that is robust to non-i.i.d. errors. Standard errors (in parentheses) are adjusted for heteroskedasticity and clustered at the firm level. Significant at: *10%, **5% and ***1%.

| VARIABLES | (1)<br>OROA | (2)<br>IPO | (3)<br>IPO | (4)<br>OROA | (5)<br>OROA | (6)<br>OROA |
|---|---|---|---|---|---|---|
| IPO | -0.002 | | | 0.233** | | |
| | (0.010) | | | (0.109) | | |
| Positive 30-day ret x Post | | 0.069*** | | | 0.016** | |
| | | (0.013) | | | (0.007) | |
| Positive 30 days forward x Post | | | 0.01 | | | 0.001 |
| | | | (0.013) | | | (0.007) |
| | | | | | | |
| Observations | 12,189 | 12,189 | 12,189 | 12,189 | 12,189 | 12,189 |
| R-squared | 0.062 | 0.894 | 0.893 | | 0.063 | 0.062 |
| Number of firms | 3,195 | 3,195 | 3,195 | 3,195 | 3,195 | 3,195 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Event year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| SIC x calendar year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| IPO month x Post FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Regression | OLS | First stage | Placebo 1st | Second stage | Red. form | Placebo r.f. |
| K-P F-test | | 27.84 | 0.69 | | | |
| Instrument | | | | Positive 30d ret. | | |

Table 4: Frequency and Characteristics of Compliers

Panel A computes the fraction of compliers among the completed IPOs (withdrawn IPOs) as the first stage coefficient from the full sample (column 2 in Table 3) times the likelihood of experiencing positive returns divided by the likelihood of completing (withdrawing) the IPO. Panel B shows the first stage coefficient in different subsamples of firms with a given characteristic above the full-sample median. The relative complier likelihood of having a given characteristic is the first stage coefficient in each subsample divided by the full-sample first stage coefficient.

Panel A: Frequency of Compliers

| P(Completed IPO) | P(Positive 30d ret.) | First Stage | % of Compliers in Completed IPOs | % of Compliers in Withdrawn IPOs |
|---|---|---|---|---|
| 87.59% | 61.84% | 6.90% | 4.87% | 34.38% |

Panel B: Characteristics of Compliers

| Variable (pre IPO attempt) | First Stage | Relative complier likelihood |
|---|---|---|
| High OROA | 7.74% | 1.12 |
| High Assets | 7.18% | 1.04 |
| High Sales | 6.97% | 1.01 |
| High Sales/Employees | 7.17% | 1.04 |
| High Sales/Assets | 4.59% | 0.66 |
| High Age | 8.05% | 1.17 |
| High Leverage | 4.10% | 0.59 |
| High Disclosure Index | 10.18% | 1.48 |
| High Anti-self-dealing Index | 8.76% | 1.27 |

Table 5: Robustness to Sample Selection and Regression Specification

This table shows the second stage results for OROA across different samples and specifications. Columns 1-4 deal with particular events identified in the appendix on sample selection: adding excluded IPOs (column 1), dropping non-overlapping attempts (column 2), dropping firms with previous withdrawn attempts (column 3), dropping cross-listings (column 4). In column 5 we add industry-times-year fixed effects when industries are defined at the 2-digit SIC level. In columns 6-8 we extend the post-IPO-attempt horizon keeping the same sample of firms. The first year in each sample is always year *t-2*, and the last year goes from *t+3* in column 6 to *t+5* in column 8. Standard errors (in parentheses) are adjusted for heteroskedasticity and clustered at the firm level. Significant at: *10%, **5% and ***1%.

| Sample/ Specification | (1) plus excl. IPO | (2) drop non-overlap | (3) drop prior withdr. | (4) drop cross-list | (5) 2-dig-SIC x Year FE | (6) Window [-2, 3] | (7) Window [-2, 4] | (8) Window [-2, 5] |
|---|---|---|---|---|---|---|---|---|
| VARIABLES | OROA | OROA | OROA | OROA | OROA | OROA | OROA | OROA |
| IPO | 0.210** | 0.237** | 0.250** | 0.258** | 0.253** | 0.245** | 0.268** | 0.272*** |
| | (0.093) | (0.108) | (0.116) | (0.114) | (0.117) | (0.107) | (0.107) | (0.105) |
| Observations | 12,450 | 12,076 | 12,143 | 11,889 | 12,189 | 14,162 | 15,932 | 17,584 |
| Number of firms | 3,259 | 3,165 | 3,183 | 3,121 | 3,195 | 3,195 | 3,195 | 3,195 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| SIC x calendar year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| IPO month x Post FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Table 6: Cross-sectional Variation in the Effect of Going Public on Profitability

This table shows the second stage regressions including interactions of the IPO variable with indicators for countries with high disclosure, high the anti-self-dealing index, small firms, and young firms. These variables are defined with respect to the sample average of each characteristic pre-IPO-attempt. To obtain the instrumented interactions, we extend the first-stage regression to include as additional instruments the interaction of our main instrument times the dummy indicators for high disclosure, high anti-self-dealing, small and young firms. Standard errors (in parentheses) are adjusted for heteroskedasticity and clustered at the firm level. Significant at: *10%, **5% and ***1%.

| VARIABLES | (1)<br>OROA | (2)<br>OROA | (3)<br>OROA | (4)<br>OROA | (5)<br>OROA |
|---|---|---|---|---|---|
| IPO | 0.203* | 0.222** | 0.242** | 0.216* | 0.187 |
| | (0.112) | (0.108) | (0.110) | (0.119) | (0.121) |
| IPO x High Disclosure | 0.024** | | | | 0.026** |
| | (0.011) | | | | (0.011) |
| IPO x High Anti-self-dealing | | 0.025** | | | 0.015 |
| | | (0.011) | | | (0.012) |
| IPO x Small Firm | | | -0.034*** | | -0.032*** |
| | | | (0.012) | | (0.012) |
| IPO x Young Firm | | | | -0.007 | -0.007 |
| | | | | (0.011) | (0.011) |
| Observations | 12,189 | 12,189 | 12,055 | 10,884 | 10,789 |
| Number of firms | 3,195 | 3,195 | 3,142 | 2,835 | 2,798 |
| Firm FE | Yes | Yes | Yes | Yes | Yes |
| Event year FE | Yes | Yes | Yes | Yes | Yes |
| SIC x calendar year FE | Yes | Yes | Yes | Yes | Yes |
| IPO month x Post FE | Yes | Yes | Yes | Yes | Yes |
| Regression | Second stage | Second stage | Second stage | Second stage | Second stage |
| Instrument #1 | Positive 30d ret | Positive 30d ret | Positive 30d ret | Positive 30d ret | Positive 30d ret |
| Instrument #2 | Positive 30d ret<br>x High Discl. | Positive 30d ret<br>x High anti-sd | Positive 30d ret<br>x Small | Positive 30d ret<br>x Young | All interactions |

Table 7: The Effects of Going Public on Sales and Assets

This table shows regression results for log sales (columns 1-3), log assets (columns 4-6), the ratio of sales over assets (columns 7-9), and sales per employee (columns 10-12). For each dependent variable we show the OLS regression, the basic IV regression following Table 3, and the IV regression with interactions following Table 6. All log variables correspond to the log of one plus the variable of interest. Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the firm level. Significant at: *10%, **5% and ***1%.

| VARIABLES | (1) Sales | (2) Sales | (3) Sales | (4) Assets | (5) Assets | (6) Assets | (7) S/A | (8) S/A | (9) S/A | (10) S/Empl. | (11) S/Empl. | (12) S/Empl. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| IPO | 0.626*** | 0.74 | 0.633 | 0.659*** | 0.529 | -0.181 | -0.266** | -0.426 | -0.926 | 0.113 | 2.983** | 4.524** |
| | (0.128) | (1.392) | (1.790) | (0.092) | (0.834) | (0.900) | (0.109) | (1.205) | (1.356) | (0.124) | (1.408) | (2.177) |
| IPO x High Disclosure | | | -0.456** | | | -0.231*** | | | 0.309*** | | | -0.156 |
| | | | (0.182) | | | (0.086) | | | (0.118) | | | (0.155) |
| IPO x High Anti-self-dealing | | | 0.223 | | | 0.237** | | | 0.122 | | | 0.157 |
| | | | (0.189) | | | (0.101) | | | (0.132) | | | (0.166) |
| IPO x Small Firm | | | 0.844*** | | | 1.127*** | | | -0.310** | | | -0.196 |
| | | | (0.161) | | | (0.085) | | | (0.125) | | | (0.171) |
| IPO x Young Firm | | | 1.151*** | | | 0.621*** | | | 0.191 | | | -0.194 |
| | | | (0.146) | | | (0.085) | | | (0.118) | | | (0.124) |
| Observations | 12,208 | 12,208 | 10,554 | 13,670 | 13,670 | 12,158 | 11,884 | 11,884 | 10,463 | 9,462 | 9,462 | 8,130 |
| R-squared | 0.13 | | | 0.27 | | | 0.06 | | | 0.04 | | |
| Number of firms | 3,141 | 3,141 | 2,697 | 2,660 | 2,660 | 2,274 | 3,091 | 3,091 | 2,694 | 2,660 | 2,660 | 2,274 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| SIC x calendar year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| IPO month x Post FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Regression | OLS | 2nd stage | 2nd stage | OLS | 2nd stage | 2nd stage | OLS | 2nd stage | 2nd stage | OLS | 2nd stage | 2nd stage |

Table 8: The Effects of Going Public on Firm Scope

This table shows regression results using as dependent variables dummies of firms in multiple industries (columns 1-3), with subsidiaries (columns 4-6), and international presence (columns 7-9). For each dependent variable we show the OLS regression, the basic IV regression following Table 3, and the IV regression with interactions following Table 6. Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the firm level. Significant at: *10%, **5% and ***1%.

| VARIABLES | (1)<br>Mult. Ind. | (2)<br>Mult. Ind. | (3)<br>Mult. Ind. | (4)<br>Sub. | (5)<br>Sub. | (6)<br>Sub. | (7)<br>Int'l | (8)<br>Int'l | (9)<br>Int'l |
|---|---|---|---|---|---|---|---|---|---|
| IPO | 0.084*** | 0.361** | 0.431** | 0.103*** | 0.481*** | 0.481** | 0.035*** | 0.215** | 0.375*** |
| | (0.016) | (0.141) | (0.181) | (0.019) | (0.160) | (0.200) | (0.012) | (0.106) | (0.141) |
| IPO x High Disclosure | | | 0.026 | | | 0.031 | | | -0.025 |
| | | | (0.019) | | | (0.021) | | | (0.015) |
| IPO x High Anti-self-dealing | | | 0.026 | | | 0.012 | | | -0.036** |
| | | | (0.020) | | | (0.023) | | | (0.016) |
| IPO x Small Firm | | | -0.163*** | | | -0.126*** | | | -0.113*** |
| | | | (0.021) | | | (0.022) | | | (0.016) |
| IPO x Young Firm | | | 0.040** | | | 0.051*** | | | 0.000 |
| | | | (0.017) | | | (0.020) | | | (0.013) |
| | | | | | | | | | |
| Observations | 14,110 | 14,110 | 12,299 | 14,110 | 14,110 | 12,299 | 14,110 | 14,110 | 12,299 |
| R-squared | 0.26 | | | 0.39 | | | 0.13 | | |
| Number of firms | 3,467 | 3,467 | 2,992 | 3,467 | 3,467 | 2,992 | 3,467 | 3,467 | 2,992 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| SIC x calendar year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| IPO month x Post FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Regression | OLS | 2nd stage | 2nd stage | OLS | 2nd stage | 2nd stage | OLS | 2nd stage | 2nd stage |

Table 9: The Effects of Going Public on M&A Activity

This table shows regressions using the log of the number of acquisitions (columns 1-3) and a dummy for being the target of an acquisition (columns 4-6) as dependent variables. For each dependent variable we show the OLS regression, the basic IV regression following Table 3, and the IV regression with interactions following Table 6. All log variables correspond to the log of one plus the variable of interest. Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the firm level. Significant at: *10%, **5% and ***1%.

| VARIABLES | (1) Acquisitions | (2) Acquisitions | (3) Acquisitions | (4) Target | (5) Target | (6) Target |
|---|---|---|---|---|---|---|
| IPO | 0.128*** | 0.012 | -0.036 | 0.001 | 0.063 | 0.056 |
| | (0.014) | (0.130) | (0.166) | (0.012) | (0.094) | (0.120) |
| IPO x High Disclosure | | | -0.032* | | | -0.003 |
| | | | (0.018) | | | (0.013) |
| IPO x High Anti-self-dealing | | | 0.000 | | | -0.020 |
| | | | (0.019) | | | (0.014) |
| IPO x Small Firm | | | -0.032 | | | -0.029** |
| | | | (0.019) | | | (0.014) |
| IPO x Young Firm | | | 0.047*** | | | 0.000 |
| | | | (0.016) | | | (0.012) |
| | | | | | | |
| Observations | 14,110 | 14,110 | 12,299 | 14,110 | 14,110 | 12,299 |
| R-squared | 0.11 | | | 0.09 | | |
| Number of firms | 3,467 | 3,467 | 2,992 | 3,467 | 3,467 | 2,992 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Event year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| SIC x calendar year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| IPO month x Post FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Regression | OLS | 2nd stage | 2nd stage | OLS | 2nd stage | 2nd stage |

Table 10: The Effects of Going Public on Patents

This table shows regressions using the log of the number of patents eventually granted. We show the OLS regression (column 1), the second stage from the basic IV regression following Table 3 (column 2), and the IV regressions with interactions following Table 6. All log variables correspond to the log of one plus the variable of interest. Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the firm level. Significant at: *10%, **5% and ***1%.

| VARIABLES | (1) Patents | (2) Patents | (3) Patents | (4) Patents | (5) Patents | (6) Patents | (7) Patents |
|---|---|---|---|---|---|---|---|
| IPO | 0.007 | -0.011 | 0.013 | -0.002 | -0.002 | -0.035 | -0.002 |
| | (0.009) | (0.068) | (0.077) | (0.069) | (0.073) | (0.067) | (0.078) |
| IPO x High Disclosure | | | -0.020** | | | | -0.011 |
| | | | (0.010) | | | | (0.008) |
| IPO x High Anti-self-dealing | | | | -0.016** | | | -0.005 |
| | | | | (0.008) | | | (0.006) |
| IPO x Small Firm | | | | | -0.004 | | 0.001 |
| | | | | | (0.010) | | (0.008) |
| IPO x Young Firm | | | | | | 0.013* | 0.013* |
| | | | | | | (0.008) | (0.007) |
| Observations | 14,110 | 14,110 | 14,110 | 14,110 | 13,674 | 12,612 | 12,299 |
| R-squared | 0.02 | | | | | | |
| Number of firms | 3,467 | 3,467 | 3,467 | 3,467 | 3,351 | 3,072 | 2,992 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| SIC x calendar year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| IPO month x Post FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Regression | OLS | 2nd stage | 2nd stage | 2nd stage | 2nd stage | 2nd stage | 2nd stage |

Table 11: Other Capital Raising and Corporate Governance Outcomes

This table shows second stage results when dependent variables are the following: a dummy for years where shareholders' net funds growth is higher than 100% not considering the year of the IPO in the case of completed IPOs (column 1), a dummy for years where debt growth is higher than 100% (column 2), and a dummy for years with a change in CEO or CFO (columns 3 and 4). Standard errors (in parentheses) are adjusted for heteroskedasticity and clustered at the firm level. Significant at: *10%, **5% and ***1%.

| VARIABLES | (1) Shareholder's net funds growth>100% | (2) Debt growth>100% | (3) CEO change | (4) CFO change |
|---|---|---|---|---|
| IPO | 0.008 | -0.575** | 0.149* | 0.056** |
| | (0.219) | (0.273) | (0.090) | (0.028) |
| Observations | 8,941 | 9,182 | 14,110 | 14,110 |
| Number of firms | 2,751 | 2,813 | 3,467 | 3,467 |
| Firm FE | Yes | Yes | Yes | Yes |
| Event year FE | Yes | Yes | Yes | Yes |
| SIC x calendar year FE | Yes | Yes | Yes | Yes |
| IPO month x Post FE | Yes | Yes | Yes | Yes |
| Regression | 2nd stage | 2nd stage | 2nd stage | 2nd stage |

# Appendix

## Sample Selection

### From raw data to final sample

- IPO attempts with valid IPO-attempt dates and data (not necessarily for all variables in our analysis) before and after the attempt: 3,534.
- Define a 5-year event window around the event: from -2 to +2.
- Exclude firms with overlapping withdrawn IPO and a completed IPO events: 52 events.
- Exclude the first withdrawn event that is followed by another withdrawn event within 5 years: 15 events.
  - 12 were followed by another withdrawn event included in the sample.
  - 2 were followed by non-overlapping withdrawn-and-completed IPO events included the sample.
  - 1 was followed by overlapping withdrawn-and-completed IPO events not included in the sample.
- Final sample: 3,467 IPO attempts (=3,534-52-15)
  - 430 withdrawn and 3,037 completed.
  - Final sample includes:
    * 12 withdrawn attempts that represent the follow-up withdrawn attempt outside the 5-year interval.
    * 19 firms (38 events) that have a withdrawn attempt followed by a completed IPO more than 5 years apart (non-overlapping events).

**Firms that withdraw**

- Combining the included and excluded events from the sample there are 456 firms that withdraw their IPOs.
- 408 firms that withdraw their IPO do not file again.
- 48 attempt to go public again.
- 36 eventually go public later on.
- Mean (median) time between a withdrawn IPO attempt and the next attempt is 2.4 (0.5) years.
- Mean (median) time between a withdrawn IPO attempt and the next withdrawn attempt is 2.2 years (0.16 year = 2 months).
- Mean (median) time between a withdrawn IPO attempt and a completed IPO is 2.5 years (1.2 years).

Table A.1: Additional Summary Statistics

This table summary statistics for additional variables. Anti-self-dealing is a measure of legal protection of minority shareholders against expropriation by corporate insiders at the country level following Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2008). Disclosure is an index of disclosure requirements for initial public offerings at the country level following La Porta, Lopez-de-Silanes, and Shleifer (2006). Target is an indicator variable that takes a value of 1 if the firm is the target of an acquisition during the first 5 years after the IPO attempt. Shareholders' net funds growth higher than 100% is a dummy for firm-years that meet this condition (without considering the year of the IPO for completed IPOs). Debt growth higher than 100% is a dummy for firm-years that meet this condition. The last two variables are dummies for years with a change in CEO or CFO.

| | Mean | P10 | P25 | P50 | P75 | P90 | SD | Total |
|---|---|---|---|---|---|---|---|---|
| Anti-self-dealing | 0.562 | 0.28 | 0.33 | 0.42 | 0.95 | 0.95 | 0.291 | 14,110 |
| Disclosure | 0.661 | 0.42 | 0.5 | 0.67 | 0.83 | 0.83 | 0.164 | 14,110 |
| Target | 0.03 | 0 | 0 | 0 | 0 | 0 | 0.171 | 14,110 |
| Shareholder's NF growth>100% | 0.097 | 0 | 0 | 0 | 0 | 0 | 0.295 | 9,450 |
| Debt growth>100% | 0.187 | 0 | 0 | 0 | 0 | 1 | 0.39 | 9,559 |
| CEO change | 0.04 | 0 | 0 | 0 | 0 | 0 | 0.197 | 14,110 |
| CFO change | 0.005 | 0 | 0 | 0 | 0 | 0 | 0.072 | 14,110 |

Table A.2: First and Second Stage Results with Alternative Return Instruments

This table shows the first stage (columns 1-4) and second stage (columns 5-6) results for OROA when using dummy indicators for positive market returns at different horizons (30, 60 or 90 days before the IPO attempt). Standard errors (in parentheses) are adjusted for heteroskedasticity and clustered at the firm level. Significant at: *10%, **5% and ***1%.

| | (1)<br>IPO | (2)<br>IPO | (3)<br>IPO | (4)<br>IPO | (5)<br>OROA | (6)<br>OROA |
|---|---|---|---|---|---|---|
| IPO | | | | | 0.183** | 0.162** |
| | | | | | (0.091) | (0.082) |
| Positive 30-day ret x Post | | | 0.071*** | 0.070*** | | |
| | | | (0.013) | (0.013) | | |
| Positive (60-30)-day ret x Post | 0.042*** | | 0.045*** | 0.049*** | | |
| | (0.013) | | (0.013) | (0.013) | | |
| Positive (90-60)-day ret x Post | | 0.038*** | | 0.039*** | | |
| | | (0.013) | | (0.013) | | |
| Observations | 12,189 | 12,175 | 12,189 | 12,175 | 12,189 | 12,175 |
| R-squared | 0.893 | 0.894 | 0.895 | 0.895 | | |
| Number of firms | 3,195 | 3,192 | 3,195 | 3,192 | 3,195 | 3,192 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Event year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| SIC x calendar year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| IPO month x Post FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Stage | First | First | First | First | Second | Second |
| 1st Stage K-P F-stat | 9.93 | 8.04 | 19.53 | 15.02 | | |
| Instruments | | | | | Col. (3) | Col. (4) |

Table A.3: From Panel to Cross-Sectional Regressions

This table shows the connection between our baseline panel regression for OROA (column 1) and cross-sectional regressions. In column (2) the sample is restricted to firms with OROA observations before and after the IPO attempt. Column (3) adds country and filing-year times Post fixed effects to level the field with subsequent cross-sectional regressions that include country and filing-year fixed effects (columns 4-5). Column 4 uses a cross-sectional sample where for each firm we consider the change in OROA. The change in OROA is the difference between the after-IPO-attempt average OROA and the before-IPO-attempt average OROA. Column 5 adds pre-attempt control variables (log of the average number of subsidiaries, log of the average number of countries, log of assets, and leverage). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the firm level. Significant at: *10%, **5% and ***1%.

| VARIABLES | (1)<br>OROA | (2)<br>OROA | (3)<br>OROA | (4)<br>OROA Change | (5)<br>OROA Change |
|---|---|---|---|---|---|
| IPO | 0.233** | 0.231** | 0.485** | 0.453* | 0.513** |
| | (0.109) | (0.110) | (0.248) | (0.239) | (0.250) |
| Observations | 12,189 | 11,531 | 11,531 | 2,934 | 2,771 |
| Number of firms | 3,195 | 2,934 | 2,934 | 2,934 | 2,771 |
| Data structure | Panel | Panel | Panel | Cross-section | Cross-section |
| Estimation | IV | IV | IV | IV | IV |
| Firm FE | Yes | Yes | Yes | No | No |
| Event year FE | Yes | Yes | Yes | No | No |
| SIC x calendar year FE | Yes | Yes | Yes | No | No |
| IPO month x Post FE | Yes | Yes | Yes | No | No |
| Country FE x Post | No | No | Yes | No | No |
| Filing year x Post FE | No | No | Yes | No | No |
| SIC FE | No | No | No | Yes | Yes |
| Country FE | No | No | No | Yes | Yes |
| Filing year FE | No | No | No | Yes | Yes |
| Month FE | No | No | No | Yes | Yes |
| Sample | All | OROA pre & post | Col. (2) | Col. (2) | Col. (2) + pre controls |

## Table A.4: First Stage for Interactions

This table shows the first-stage regressions that include interaction terms with High Disclosure, High Anti-self-dealing, and dummies for small and young firms in comparison to the sample mean. These first stage regressions correspond to the results presented in Table 6. For example, columns 1 and 2 in this table are used to generate the second stage results presented in column 1 of Table 6. The sample is restricted to observations where OROA is available. Standard errors (in parentheses) are adjusted for heteroskedasticity and clustered at the firm level. Significant at: *10%, **5% and ***1%.

| VARIABLES | (1) IPO | (2) IPO x High Discl. | (3) IPO | (4) IPO x High Anti-sd | (5) IPO | (6) IPO x Small | (7) IPO | (8) IPO x Young |
|---|---|---|---|---|---|---|---|---|
| Positive 30-day ret x Post | 0.055*** | -0.363*** | 0.081*** | -0.391*** | 0.041*** | -0.388*** | 0.055*** | -0.400*** |
| | (0.015) | (0.015) | (0.014) | (0.015) | (0.015) | (0.015) | (0.016) | (0.016) |
| Positive 30-day ret x Post x High Disclosure | 0.028** | 0.902*** | | | | | | |
| | (0.014) | (0.010) | | | | | | |
| Positive 30-day ret x Post x High Anti-sd | | | -0.025* | 0.874*** | | | | |
| | | | (0.014) | (0.012) | | | | |
| Positive 30-day ret x Post x Small | | | | | 0.065*** | 0.931*** | | |
| | | | | | (0.013) | (0.009) | | |
| Positive 30-day ret x Post x Young | | | | | | | 0.024* | 0.915*** |
| | | | | | | | (0.015) | (0.010) |
| Observations | 12,189 | 12,189 | 12,189 | 12,189 | 12,055 | 12,055 | 10,884 | 10,884 |
| R-squared | 0.89 | 0.76 | 0.89 | 0.74 | 0.90 | 0.76 | 0.90 | 0.75 |
| Number of firms | 3,195 | 3,195 | 3,195 | 3,195 | 3,142 | 3,142 | 2,835 | 2,835 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| SIC x calendar year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| IPO month x Post FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |