The Effects of Going Public on Firm Profitability and Strategy

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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 initial-public-offering (IPO) attempts. We compare completed and withdrawn IPO attempts. We instrument for the decision to complete the IPO using prior market returns. After instrumenting, we find a positive effect of going public on profitability, contrary to previous results. We also find a post-IPO expansion in sales per employee, subsidiaries, and countries in which firms operate. Overall, our results are consistent with going public inducing a shift in strategy towards increased commercialization.

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1 Introduction

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

The stylized fact in the literature is that, on average, profitability falls after the IPO (Degeorge and Zeckhauser 1993, Jain and Kini 1994, Mikkelson, Partch, and Shah 1997). This result has often been interpreted as evidence that the costs of going public can exceed the benefits. The literature has also considered selection effects to explain this post-IPO drop in profitability. For instance, firms may time IPO decisions according to their life-cycle, their peers, profitability shocks, or industry shocks (Pástor, Taylor, and Veronesi 2009, Chemmanur, He, and Nandy 2010, Spiegel and Tookes 2020, Aghamolla and Thakor 2022). Under these explanations, the dynamics of exogenous variables explain which firms self-select into public markets and their post-IPO outcomes, and not going public in itself. In order to provide more accurate evidence regarding the costs and benefits of going public, it is crucial to distinguish the causal effect of going public from potential selection effects.

Our paper revisits the initial finding that profitability drops post-IPO taking into account the endogeneity of the going public decision. Our main contribution is to isolate the causal effect of going public on profitability. This causal effect refers to changes in profitability that happen *because* firms go public, and not simply changes that are correlated with the going public decision. We show that this casual effect is related to relaxing financial constraints so that the firm is better able to move away from exploration and towards commercialization.

Our empirical design rests on two building blocks. First, we focus on the profitability of firms that complete their IPOs relative to firms that withdraw their IPOs. This implies that we identify the effect of going public within the population of IPO-attempts. Withdrawn attempts represent a reasonable counterfactual for firms that go public and allow us to control for a host of self-selection issues. We collect data on 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 also 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 building block of the identification strategy is to instrument for IPO completion. In the same spirit as Bernstein (2015), we use market returns over the previous 30 days as instrument. 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%). We show that the pre-choice return is uncorrelated with firm characteristics of candidate IPOs, thus it behaves as a well-balanced instrument. Market returns in this narrow window are unlikely to directly impact firm outcomes several years after the attempt, which is what the exclusion restriction requires. In short, the identification strategy is based on the idea that good/bad returns are a "nudge" for some firms that are at the margin between listing or not but have no effect on firms except for their impact on the going public decision. These firms are called compliers - firms that change their decision based on short-term returns.

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

IPO attempts rather than to a causal effect of going public.

In our instrumental variables (IV) estimation, we attempt to isolate the causal impact of going public. We find a significant increase in profitability for firms that go public relative to firms that withdraw. 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 relative to withdrawn firms increases by close to one standard deviation in this sample. The effect is large, but plausible. One possible explanation is that, in equilibrium, a large increase in profitability is needed to compensate for the equally large costs of going public (close to 5% of firm value according to Gahng, Ritter, and Zhang 2021).[1]

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 type of self-selection that follows models of asymmetric information with unobservable quality does 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). To the extent that better firms are more likely to opt out of the market, 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.

[1] We also check that our results are not affected by a weak-instruments problem, which could artificially increase the magnitude of IV coefficients (Jiang, 2017). In addition, we show that our inference is robust to the exclusion of clusters of observations, which Young (2022) recently recommends as a robustness exercise for IV estimations.

A related concern is that our instrument may induce 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 our IV estimation is not between the average issuer in hot and cold markets, but among marginal firms that respond to short-term returns by completing or withdrawing (i.e., complier firms). That is, our IV estimate 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. Thus, even if there is a change in the average quality of issuers following 30-day returns, it does not affect the exclusion restriction. The main reason is that quality is comparable across firms that are nudged by returns to complete or withdraw their IPOs.

What explains the positive impact of going public on profitability? There are several, non-mutually exclusive explanations. A first possibility is that, despite their lower ownership stake, controlling shareholders consume fewer private benefits in listed companies than in private companies (Pagano and Roell, 1998). Private benefits -from pure diversion to pet projects- are harder to extract in public companies that face tighter regulations, disclosure requirements, and monitoring. We find that the increase in profitability is stronger in countries with better protection of minority investors, consistent also with stock markets being generally more developed in countries that are better able to control agency problems (Djankov, La Porta, López-de-Silanes, and Shleifer, 2008).[2]

Another agency problem is managerial short-termism, or the idea that the boost in profitability comes at the expense of long-run opportunities (Stein, 1989). This hypothesis is less consistent with our findings for two reasons. First, although our main results refer to profitability in a two-year horizon after the IPO attempt, we do not see any reversal

[2]Although not in our sample, the recent experience of Uber can potentially illustrate the reduction of pet projects and the 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 (2021).*" (Uber Sells Self-Driving-Car Unit to Autonomous-Driving Startup, *Wall Street Journal*, December 7, 2020).

on profitability when we extend the analysis to 4 years after the attempt. Extending the analysis beyond 4 years lacks power. Second, we find that investment is more sensitive to opportunities (proxied by the industry's Tobin's q) after firms go public, which is consistent with higher investment efficiency instead of myopic behavior. These results hold in OLS and IV estimations, as well as when using higher-order cumulant regressions to correct for possible error-in-variables in Tobin's q (Erickson, Jiang, and Whited, 2014). While we cannot rule out that the increase in profitability comes at the expense of other, potentially unobservable long-run opportunities, our results are not consistent with such an interpretation.

A third possibility is that going public alleviates financial constraints. In theory, the impact of lifting financial constraints on profitability is ambiguous. With decreasing returns to scale, one could expect profitability to fall with the larger scale allowed by lifting financial constraints. At the same time, if lifting financial constraints allows the firm to respond to opportunities that are otherwise outside its reach (e.g., new markets), then profitability can increase. We find strong support for the financial constraints interpretation in several outcomes related to scale and scope. Specifically, we find that sales and assets increase more strongly post-IPO in small and young firms, which are typically considered to be more constrained firms (Hadlock and Pierce, 2010). The average IPO firm increases investment by opening new subsidiaries and expanding its presence to more industries and international markets. These discrete changes in scale and scope can rationalize the positive effect of lifting financial constraints on profitability.

We hypothesize that, in addition to the capital injection, public status pushes the firm to focus on commercialization. For example, stock market investors can exert pressure on new firms to attain strong margins by commercializing already-tested products and services. The shift in strategy is consistent with requiring additional funds, and thus with our results on the importance of alleviating financial constraints, but it points to a specific mechanism for why the additional funds are needed. Several empirical results are consistent with such a focus on commercialization. First, firms experience a significant increase in sales per employee. Second, we confirm in our sample of European firms the decline in patenting found by Bernstein (2015) for U.S. firms, but we find that this post-IPO fall in innovation is concentrated on exploratory patents (Custodio, Ferreira, and Matos, 2019). Exploitative patents (i.e., those based on previous knowledge) increase at the margin. Third, we find more CEO and CFO turnover as firms go public. A shift in strategy after the IPO can rationalize these results. Increasing sales per employee through the expansion to new segments and markets; the need for new management with a different set of skills; and concentrating on innovation that takes advantage of already acquired knowledge, are all facts consistent with an emphasis on commercialization activity following the going public decision.

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 causal effect of going public from selection effects. Estimating the causal effect is a first-order question in corporate finance, which speaks directly of the benefits (or costs) of accessing public markets. Our main advantage, compared to this literature, is that we have access to panel data with financial variables for both completed and withdrawn IPOs. These data allow us to study the long-standing puzzle of the post-IPO drop in profitability (Degeorge and Zeckhauser 1993, Jain and Kini 1994, Mikkelson, Partch, and Shah 1997, Pástor, Taylor, and Veronesi 2009). The focus on profitability, or the ability of the firm to generate cash-flows out of assets-in-place, is justifiable in the IPO context since firm value is not observable for any firm before the IPO, nor for withdrawn firms after the IPO attempt. One caveat, though, is that we obtain the causal *differential* effect on profitability between completed and withdrawn IPOs, but we cannot identify the causal *level* effect of going public on profitability.

Our interpretation is that public capital induces a shift in firm strategy towards commercialization. We explore this change in strategy along multiple dimensions like firm scope, internationalization, patenting activity, and executive turnover. Other recent work deals with the causal effect of going public on outcomes such as the structure of a firm's labor force, the geographical diversification of acquisitions, and local spillovers (Babina, Ouimet, and Zarutskie 2020; Borisov, Ellul, and Sevilir 2021; Butler, Fauver, and Sypiridopoulos 2019; Cornaggia, Gustafson, Kotter, and Pisciotta 2020, 2021; Dambra, Gustafson, and Pisciotta 2021). These findings are consistent with a change in strategy, as we propose, but do not consider the debate on post-IPO profitability.

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), Phillips and Sertsios (2017) and Maksimovic, Phillips, and Yang (2020) find the opposite using demand-increasing exogenous shocks. However, even in these cases, the comparisons between private and public companies may contain both selection and treatment effects. In this respect, our results contribute by estimating the pure causal effect of going public, which can help bridge the gap between apparently contradictory results. Still, it is important to note that our results apply specifically to private firms that are attempting to go public, and not necessarily to the overall population of private firms.

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.[3] 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.[4]

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 in 2000 and 2006-7, which coincide with years of high stock 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.

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.[5]

[3]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.

[4]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.

[5]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 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 period 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.

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, in smaller firms the obligation to file refers only to abbreviated financial statements. For a similar reason we do not have access to good measures of capital expenditures or R&D since these are often not reported.

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 correspond to firms with subsidiaries, and 9% to firms with subsidiaries in countries other than the firm's headquarters. The average number of 3-digit SIC industries covered by each firm is 1.58.

Patent data come from *Zephyr*, which matches firms and patent information from the European Patent Office. The number of patents corresponds to patent applications for each firm and year that are eventually granted. Average patents are low in our sample, as a few firms have most of the patents. Patent is a dummy for firm-years with at least one patent application that is eventually granted. We follow Custodio, Ferreira, and Matos (2019) in classifying patents as exploratory or exploitative. An exploratory (exploitative) patent is one where 60% or more of its citations are based on new (old) knowledge. New knowledge refers to citations outside of the firm's previous portfolio of patents. Acquisitions, also from *Zephyr*, correspond to the number of firms acquired.[6] Table A.1 provides 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:

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

[6] 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*.

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 among 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 (α_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.[7] 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 (α_τ). These fixed effects absorb the $Post_{it}$ dummy while also allowing us to control for life-cycle dynamics that are common 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 differential effect of firms going public in the early part of the year compared to firms going public near the end of the year. Firms that go public in December have a short post-IPO first year, while firms that go public in January have a long post-IPO first year. Finally, the industry-by-calendar-year fixed effects (α_{jt}) capture annual swings at the one-digit SIC level.[8] 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

[7]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.

[8]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.

IPO. For example, industry trends can induce many firms in an industry to attempt to go public, but on top of those there can be firm-specific signals that induce some firms to complete their attempt. 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 (ε_{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., ε_{it+1} and subsequent residuals 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 β.[9] If there is momentum in profitability shocks instead of mean reversion, then there is an upward bias in the OLS estimate. In any case, OLS is unlikely to capture the true causal effect of going public on profitability.

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 with higher 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

[9] 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.

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 (see, for example, Table 8 in Ljungqvist and Wilhelm (2002) for the typical timeline of European IPOs). We focus on the market returns over the 30 days that precede the IPO completion or withdrawal date.[10] 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

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

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:

$$IPO_{it} = \gamma 30dayRet_c \times Post_{it} + \alpha_i + \alpha_\tau + \alpha_{m\tau} + \alpha_{jt} + \zeta_{it} \tag{2}$$

In order to account for the time dimension in the panel we need to interact the market return in each country in the previous 30 days ($30dayRet_c$) 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 the transition to public status, 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 of 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 average returns are 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). These firms that change their decision based on short-term returns are called *compliers*. Crucially, these short-term returns need to have no influence on future outcomes except through their 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 unobservable and time-varying 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 the marginal issuer and withdrawal responding to market returns. In the language of IV, these firms constitute the sub-population of compliers. The IV coefficient is the local average treatment effect (LATE) for compliers, and not necessarily for the overall population of firms. 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 completed IPOs after high returns are in some way better (or worse) than withdrawn IPOs after low returns, but this is not what asymmetric information models predict.

Figure 3 summarizes the theoretical implications of standard asymmetric information models that are relevant for our setup. More firms complete their IPOs after positive returns, and the average quality of issuers goes up. Compliers, i.e., firms that change their decision based on returns, are firms of intermediate quality in this model. What is crucial for our identification strategy is that compliers are of homogeneous quality so, even in the presence of asymmetric information, there should be no bias when making comparisons across them. The very high-quality firms that withdraw regardless of returns (the "never-takers"), and the very low-quality firms that list regardless of returns (the "always-takers"), are not part of the IV comparisons.[11] Naturally, other models besides asymmetric information may provide different predictions regarding the quality of firms that list or withdraw. The necessary condition for our identification strategy to be valid is that compliers are homogeneous or that we can make fair comparisons between them. Whether the estimates from this sub-population are representative of the entire population is an external validity issue, but it

[11] "Never-takers" does not refer to firms that literally never had the intention to list. It is only the case that their withdrawal decision is not affected by returns.

does not invalidate the IV estimation.

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 (α_τ) 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 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 decision.

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 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 β 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 nudge firms into completing their IPOs. 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 we use the dummy variable throughout the paper because it makes the interpretation easier (e.g., the analysis of compliers). 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.[12] The F-statistic for the main instrument is 27.84 (see column 2), 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 *t-60* and *t-30*, or between *t-90* and *t-60*). Although statistically significant, they have less power than those 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 as-random variation (given by the instrument), firms that go public appear to increase their profitability rather than decrease it. It is important to remember that

[12] 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.

this effect is relative to the counterfactual (withdrawn IPOs) and not an absolute effect on profitability. In a similar vein, although isolating the treatment effect of going public provides important new evidence, it does not imply that selection effects are small or irrelevant. In fact, Maksimovic, Phillips, and Yang (2020) argue that selection effects can be 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 switch their focus to profitability, which does not happen 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 expect firms to achieve profitability. This expectation can help promote profitability more than the potential decrease in profitability that may arise from agency costs resulting from the separation of ownership and control.

Column 5 in Table 3 shows results for the regression of OROA directly on the instrument (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 all 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 computed 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 a weak instruments 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).[13] 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 is 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, it is possible to find IV coefficients that are larger than OLS coefficients.[14]

Also from a theoretical perspective, 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.

[13] 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).

[14] Other theories may suggest a positive bias in OLS. For instance, suppose stronger firms are more likely to go public and increase their profitability. Then, the OLS coefficient would be higher than the IV coefficient. However, such theories are not entirely consistent with some of our findings. Table 2 Panel A shows that larger and more profitable firms are less likely to complete their IPOs. Alternatively, suppose that only the decision of weak firms is affected by returns (i.e., weak firms are compliers). Then, the IV would identify the causal effect of going public among weak firms, potentially identifying a smaller effect than that of the OLS estimation. Nonetheless, in the next section we show that, on average, compliers are not weak firms.

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.

Compliers can be understood as firms that are at the "margin" of completing their IPO before they are nudged by market returns to complete or withdraw. Compliers cannot be identified individually, because we observe each firm's decision only 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 6.9% of the sample. This size of the complier population is not small in comparison to other IV studies, and hence it can be enough for identification purposes (see, for example, Table 4.4.2 in Angrist and Pischke 2009).

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 21.22%, which implies that market returns have a stronger incidence in the decision to withdraw than in the decision to complete. This fits well 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 given characteristic is equal to the ratio of the first stage coefficient in the sub-sample with that characteristic over the first stage coefficient in 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 1.11), to be older (ratio 1.13), and to come from countries with a high anti-self-dealing index (ratio 1.27).[15] Overall, compliers do not seem to be weak firms timing the market, but instead they are relatively average firms, as in asymmetric information models. Since compliers' characteristics are similar to those of the overall population, our results are likely to carry over to most IPO attempts.

4.6 Robustness

IV estimations have been subject to recent scrutiny (see, among others, Atanasov and Black 2016, Jiang 2017, Lee, McCrary, Moreira, and Porter 2021, Young 2022). For instance, Young (2022) shows that many IV setups are sensitive to minimal changes in 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 dependent 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

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

in Table 3) reject the OLS estimate (column 1 in Table 3). Overall, our statistical inference is on solid ground.

In Table 5, we show that the effect on profitability is robust to changes in sample definition and empirical specification. Our main results do not vary in significance if we drop or include events identified by our cleaning procedure in the appendix (columns 1-3), nor if we exclude particular events such as cross-listings (column 4), or if we add industry-times-year fixed effects at the 2-digit SIC level (column 5). The IV coefficient for profitability varies between 21% and 25.8% in the different robustness exercises in columns 1 through 5.[16]

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). As a robustness check, we gradually extend the post-attempt window up to the end of the fourth year. We do this without changing the sample of firms, hence some firms have a 3- or 4-year post-event window while others remain with the 2-year window because they disappear or are acquired after the second year. Extending the pre-event sample beyond two years is not feasible due to limited data on the very early years. As seen in columns 6-7 of Table 5, the statistical significance and magnitude of the coefficients are very stable in the estimations with longer post-event windows. In fact, the effect on profitability increases with the horizon instead of reverting to zero, implying that the effect that we identify is not a transitory boost in profitability. Figure 6 shows the dynamics of our effects by event year. We show the reduced-form estimates where each event-year dummy is multiplied by the indicator for positive 30-day returns, and the IV estimates where each event-year dummy times the indicator for completed IPOs is instrumented with the event-year dummy

[16]We also explore the robustness of our results to alternative performance metrics. For instance, we show in Table A.3 that after going public it is more likely that firms cross certain thresholds for EBIT (e.g., 25 or 100 million Euros). We also find that the return on sales (ROS=EBIT/sales) increases after going public, while the ratio of expenses over sales decreases. Statistical significance is diminished in these cases because both ratios are more volatile than OROA (e.g., large expenses or strongly negative EBIT are often accompanied with very small sales).

multiplied by positive returns. As seen in the figure, the effects become stronger in years *t+3* and *t+4*.

Insert Figure 6 here

Finally, in Table A.4 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.[17] In the next section, we explore the heterogeneity to country and other variables that this suggests.

5 Mechanisms

Our empirical findings show that profitability increases in firms that go public relative to firms that withdraw the IPO attempt. We now describe four non-mutually exclusive explanations for the increase in profitability. Two of them are based on agency problems: One is related to private benefits of control and the other to managerial short-termism. As a third possibility, we discuss financial constraints. Finally, we present an explanation based on the shift of firm strategy towards commercialization.

After briefly discussing the hypotheses in section 5.1, we present empirical tests in section 5.2. We study several other dependent variables beyond profitability to give a more comprehensive account of changes at the firm when it goes public. We also explore cross-sectional

[17] Adding more fixed effects has the risk of over-fitting, which could also explain the large coefficients in Table A.4.

heterogeneity of the effects. The results from these tests do not provide irrefutable evidence of a single mechanism. Nevertheless, in section 5.3 we take stock regarding the most likely channel driving the increase in profitability.

5.1 Hypotheses

5.1.1 Private benefits of control

Controlling shareholders (or managers) with less than full ownership face a classical agency problem. They are tempted to extract private benefits at the expense of firm profitability, and hence at the expense of minority investors, because they do not face the full cost of their actions (Jensen and Meckling, 1976). Private benefits can take many forms, from pure perks to the diversion of investment opportunities (Bertrand, Mehta, and Mullainathan, 2002). Crucially, the extraction of private benefits depends on the laws and regulations protecting minority shareholders, and the disclosure requirements in different markets. Going into public markets implies a reduced ability to extract private benefits, since public markets imply tighter regulation and monitoring from investors and analysts (Pagano and Roell, 1998). According to this hypothesis, the post-IPO increase in profitability reflects the reduction in private benefits in public markets. This hypothesis has cross-sectional implications: if we compare across public markets with different levels of protection to minority investors (Djankov, La Porta, López-de-Silanes, and Shleifer, 2008), we should find that the increase in profitability is stronger as the level of protection increases.

5.1.2 Short-termism

Another agency problem arises when the compensation of managers is linked to the firm's performance. Stein (1989) shows that, when faced with imperfectly informed investors, the manager boosts short-term earnings to increase her own compensation. Short-termism could explain the increase in profitability for firms completing their IPOs. This refers to earnings manipulation after the firm goes public, and not before the IPO like in Teoh, Welch, and

Wong (1998). For instance, a manager may be tempted to increase profitability to maximize the value of the company at the moment of her exit in a few years. Importantly, under short-termism, the increase in profitability is short lived and eventually reverses. Also, short-termism implies that firm investment is less aligned with investment opportunities, as managers put more weight on boosting short-term profits, rather than on long-run firm value.

5.1.3 Financial constraints

One of the advantages of going public is to improve a firm's access to capital. For example, Brav (2009) concludes that public equity is cheaper than private equity. Reducing the cost of capital allows the firm to invest more and grow. The implications of relaxing financial constraints for firm profitability are not straightforward, though. If there are decreasing returns to scale, then average profitability falls as less profitable projects are undertaken at the margin. However, if relaxing financial constraints allows firms to respond to investment opportunities outside their previous reach, then profitability can increase by going public. Therefore, it is more likely that lifting financial constraints contributes to an increase in profitability if it goes hand in hand with discrete changes in firm scale, such as expanding to new industries, international markets, or opening subsidiaries.

5.1.4 Commercialization

This hypothesis argues that there is a change in firm strategy after the IPO: Firms shift their focus from exploration to commercialization of already-tested products and services. By construction, this hypothesis predicts higher profitability and can thus help explain our main finding. There are two ingredients to the commercialization hypothesis. First, public market investors exert pressure on firms to deliver strong profitability. Second, commercialization is a capital-intensive activity, and therefore it is possible when firms have access to abundant capital. Hence, lifting financial constraints is a prerequisite of the commercialization hypothesis, or we can think of commercialization as a variant of the financial constraints hypothesis.

The commercialization hypothesis predicts a deliberate change in firm strategy beyond just increasing firm scale. There are a few factors that lifting financial constraints alone cannot explain, but the focus on commercialization can explain. First, firms are more likely to improve their sales efficiency. Second, firms should shift their patent strategy from exploratory to exploitative. Third, changes in firm strategy usually go in hand with management changes. Hence, we should observe that firms are more likely to replace key executives after going public.

5.2 Empirical results

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

In Table 6 we examine the heterogeneity of the profitability 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.5.

Include Table 6 here

First, we study cross-country variation in agency problems through 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 corporate resources and opportunities.[18] The variable *High Anti-self-dealing* takes a value of one when the index is above the sample mean, which implies a high level of investor protection. In column 1 of Table 6 we find an additional positive impact of investor protection on OROA. For example, a

[18]We find similar results if we use the index of IPO disclosure requirements from La Porta, López-de-Silanes, and Shleifer (2006), which is strongly correlated with the anti-self-dealing index.

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). Hence, the positive effect of the IPO on profitability is stronger when there is a more investor-friendly environment. This is consistent with public status reducing the extraction of private benefits. However, the extra profitability that is achieved by going public in a high-protection country is relatively small when compared to the baseline effect on profitability in a low-protection country (22.2%).

Next we study variation according to firm characteristics, in particular those related to financial constraints. 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 2 in Table 6 shows that the impact of going public on profitability is 3.4 percentage points lower for small firms when compared to large firms. However, this negative interaction term is much smaller than the positive main effect as indicated by the IPO dummy, meaning that the overall effect on profitability is still strongly positive for small firms (24.2% - 3.4% = 20.8%). These differences between large and small IPO firms do not contradict the financial constraints' hypothesis since the effect of lifting constraints on profitability depends on the heterogeneous growth paths of firms. In the next section we study such heterogeneity.

5.2.2 Scale and Productivity

In Table 7 we explore the consequences of going public for sales, assets, and productivity. Changes in sales and assets are our proxies for changes in scale since variables such as CAPEX and R&D are not available in our data. The OLS coefficient on sales is large and positive (0.626 in column 1), but the IV coefficient, although larger than the OLS coefficient, is not statistically significant (0.740 in column 2). When we add interactions we find interesting heterogeneity. 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). Thus, small and young firms appear to be particularly focused on their sales efforts after going public.

Include Table 7 here

Columns 4 to 6 present the results with assets, which mirror the behavior of sales. In particular, small and young firms are also 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). These results can also explain the negative differential effect of going public on the profitability of small firms documented before (column 2 of Table 6).

The last three columns in Table 7 show the results for sales per employee, which is a proxy for labor productivity. We find a strong average effect of going public on productivity (columns 11 and 12). 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. The unconditional effect on sales per employee is positive and strong, which is consistent with a shift in efficiency for all IPOs.

5.2.3 Firm Scope

In Table 8 we provide evidence of changes in firm scope as firms go public. We find that completed IPOs expand significantly with new subsidiaries (columns 1-3), and towards international markets (columns 4-6). The effects are statistically significant for both OLS and IV, but the magnitudes are bigger in the IV estimation. Our results coincide with the increase in geographical diversification of IPO firms documented by Cornaggia, Gustafson, Kotter, and Pisciotta (2021), and with anecdotal evidence. 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.

Insert Table 8 here

We note that, as indicated by the negative coefficient on *IPO x Small Firm* in columns 3 and 6 of Table 8, small firms grow less in terms of scope. They aggressively increase sales and assets, as we find in Table 7, but without adding as many subsidiaries (column 3), nor expanding as strongly into more countries (column 6). Small firms still grow in scope. For instance, the overall effect on subsidiaries is positive for small IPO firms (0.389=0.511-0.122, from column 3), but they expand less strongly than large firms. These results, combined with the results on sales and assets, are consistent with small firms needing the new capital partly to exploit opportunities within their current scope. Larger firms, instead, step outside their current market segments, which allows them to avoid decreasing returns to scale to a larger extent than small firms. This can explain why large firms achieve higher post-IPO profitability than small firms.

We also examine whether IPO firms engage in acquisitions or become targets of acquisitions after IPOs (columns 7 through 12). We find a significant increase in acquisition activity with OLS (column 7), 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 8). This also shows that the IV estimates are not always bigger than OLS, as would be implied by a weak-instruments problem. We do find that young firms make significantly more acquisitions post-IPO than small firms (column 9) even after instrumenting for the IPO decision.

In columns 10 to 12 we show results where the dependent variable is a dummy equal to one if the firm is the target of an acquisition in the 5 years after the IPO attempt. We find that the average IPO firm is not less likely to become the target of an acquisition than withdrawn IPOs.[19] However, small IPO firms are less likely than the average firm to become targets as implied by the negative and significant coefficient on the interaction *IPO x Small*

[19] 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 result suggests a higher likelihood of being the target of an acquisition among withdrawn IPO firms, which we do not find in Table 8.

Firm (column 12).

5.2.4 Investment efficiency

In Table 9 we study the response of firms to investment opportunities as proxied by Tobin's q. We add to our main specification the interaction of the IPO indicator with Tobin's q at the 3-digit SIC industry level (Tobin's q alone is also added to the regression, but it is not reported). The interaction captures the excess sensitivity to Tobin's q of firms that go public in comparison to firms that stay private. For each dependent variable we show the OLS and IV estimations. Furthermore, we show the minimum distance estimator based on higher-order cumulants that follows from Erickson, Jiang, and Whited (2014). This last estimation takes into account the measurement error in Tobin's q. Since it does not allow for fixed effects, we de-mean the data before the estimation.

Insert Table 9 here

In columns 1 through 3 we report the results for assets as dependent variable. We find that assets respond more to Tobin's q after firms go public across all three estimation methods. This result is in line with the comparisons between public and private firms in Gilje and Taillard (2016), Phillips and Sertsios (2017) and Maksimovic, Phillips, and Yang (2020).

IPO firms tap into more industries, again showing an expansion of scope, as seen in the positive coefficient for the IPO indicator in columns 4 and 5. However, this expansion is dampened by a high Tobin's q in the main industry of the IPO firm, which follows from the negative interaction between the IPO indicator and Tobin's q across the three estimation methods. This result is in line with careful diversification on the part of public firms since firms are more likely to stay within their main industry when there are better opportunities in it.

5.2.5 Innovation

In Table 10 we explore the impact of going public on patenting activity. First, we study whether IPO firms apply more or less frequently for patents (that are eventually granted). We focus on the extensive margin and use a dummy for patent applications as the dependent variable.[20]

Insert Table 10 here

There is a positive, but insignificant OLS effect of going public on patenting (column 1 in Table 10). Using the IV setup we find a negative average impact of going public on patent applications (column 2), which is consistent with firms becoming less innovative as they go public (Bernstein, 2015).

In columns 4-9 of Table 10 we study the impact of going public on patent applications that are exploratory or exploitative following the classification of Custodio, Ferreira, and Matos (2019). We find a negative IV coefficient on exploratory patents (-0.088 in column 5), which is statistically significant at the 5% level. We also find a positive, although smaller and marginally significant, IV coefficient on exploitative patents (0.040 in column 8). These results are consistent with the firm's marginal efforts shifting away from exploration.

5.2.6 Corporate Governance

We end our examination of changes related to going public by studying management turnover. In Table 11, we study the frequency of changes in the CEO and CFO. With the IV we find that it is 14.9% (5.6%) more likely to see a change in CEO (CFO) in completed than in withdrawn IPOs (columns 2 and 5). The results are consistent with going public having an impact on corporate governance. Interestingly, the effect is heterogeneous for young and old firms, consistent with the different stages of their life cycle. CEOs are less likely to change in young firms when compared to large firms (-2.4% in column 3). CFOs, on the other hand,

[20] Another reason to use a dummy variable for patents is the difficulty with using count data in financial applications (Cohn, Liu, and Wardlaw, 2022).

are more likely to change in young firms (0.8% in column 6). These results are consistent with the idea that young firms still need the leadership of their founder (most likely the CEO), but at the same time, they need new financial expertise.

Insert Table 11 here

5.3 Discussion

Our empirical evidence is most consistent with the IPO capital causing a relaxation of financial constraints, and more specifically enabling a shift in strategy towards commercialization.

In favor of financial constraints we find a stronger increase in assets and sales for small and young firms, which are typically considered to be more constrained. All firms that go public broaden their scope through more subsidiaries and increasing presence in other industries and international markets, which are capital-intensive activities. Interestingly, large firms tend to grow more in scope, i.e., through more new subsidiaries and towards more new countries, than small firms, which can account for their stronger increase in post-IPO profitability. Finally, the increased response of IPO firms to investment opportunities captured by Tobin's q also speaks in favor of lifting financial constraints.

A shift towards commercialization is consistent with the previous facts, for instance, the push to international markets. Importantly, additional results point more exclusively to commercialization. First and foremost, the increase in sales per employee reflecting an increase in sales efficiency. Second, the strong decrease in exploratory patents, while exploitative patents increase at the margin. Finally, the increased turnover in management is consistent with the need for skills that differ from those employed in the pre-IPO exploratory phase.

Regarding agency problems, we find evidence of the importance of private benefits, but not of short-termism. We find that the increase in profitability is larger in countries that better curb the extraction of private benefits. However, the differential effect of better investor protection is small in comparison to the baseline effect of going public. Against short-termism, we do not find a reversal in profitability when we extend the horizon as much as the data allow. Additionally, the higher sensitivity of investment to Tobin's q, which captures long-run opportunities, is contrary to myopic behavior.

6 Conclusions

This paper sheds light on the consequences of going public using a large sample of close to 3,500 firms that file for an initial public offering in 16 European countries between 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 findings in the literature, we show that firms' profitability goes down after the IPO. However, we show that the profitability of withdrawn-IPO firms also goes down, so the OLS differences-in-differences effect on firm profitability is essentially zero. Moreover, once we directly tackle the endogeneity problem using the instrumental variables approach, we find increased profitability in firms that go public - a result that is the opposite of prior literature. One important caveat is that we obtain the causal *differential* effect between completed and withdrawn IPOs, but we cannot identify the causal *level* effect of going public on profitability.

We attribute the increase in post-IPO profitability to a commercialization strategy that is enabled by lifting financial constraints. Small firms, in particular, strongly increase their sales and assets. The average IPO firm expands operations to more countries, industries, and subsidiaries, and significantly increases sales per employee. Also, while overall patenting activity goes down, there is a shift from exploratory to exploitative patents.

Overall, we show that going public is unlikely to be like other capital infusions as it goes above and beyond simply raising capital. The positive effect on profitability, sales per employee, new subsidiaries and countries, combined with a shift away from exploratory patents, are consistent with a change from the search of new ideas to the commercialization of tested products and services.

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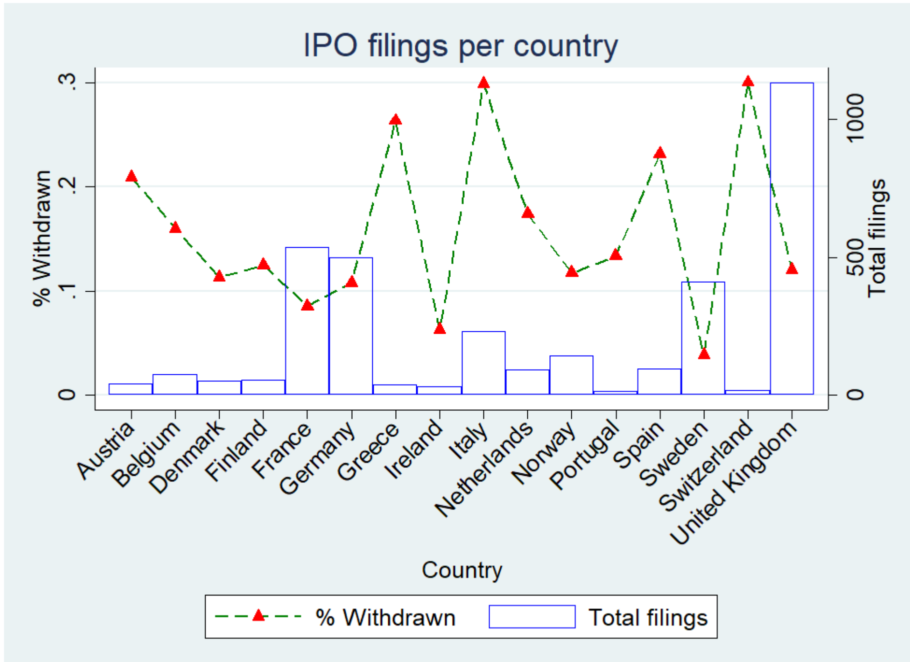
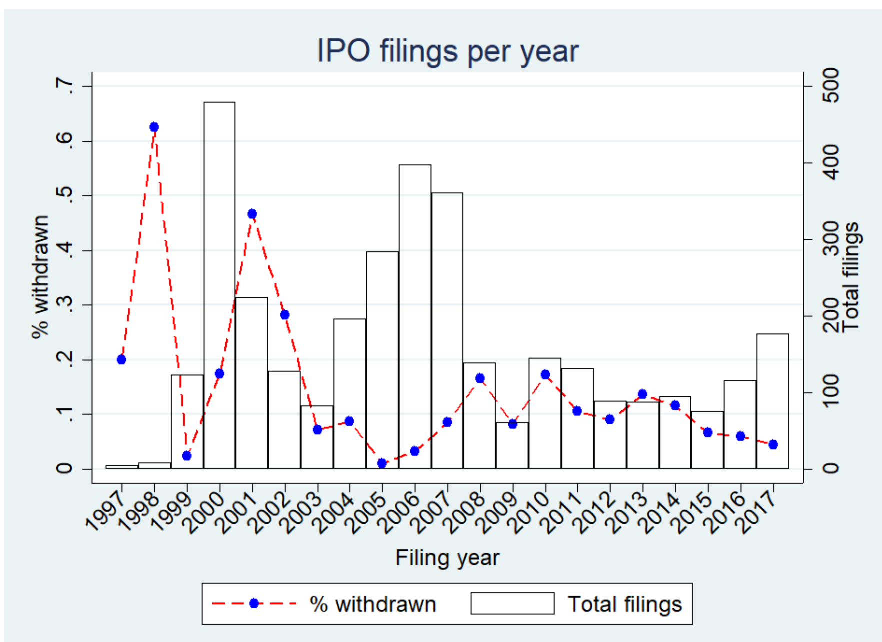
Figure 1: IPOs over Countries and Years



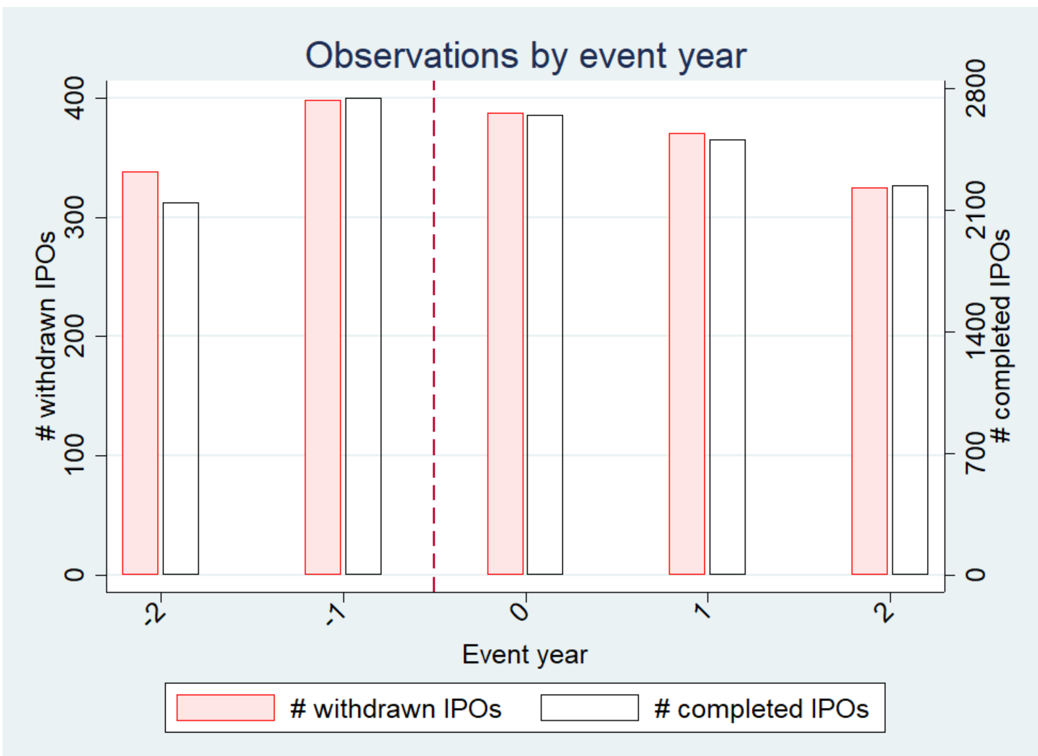
(a)



(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

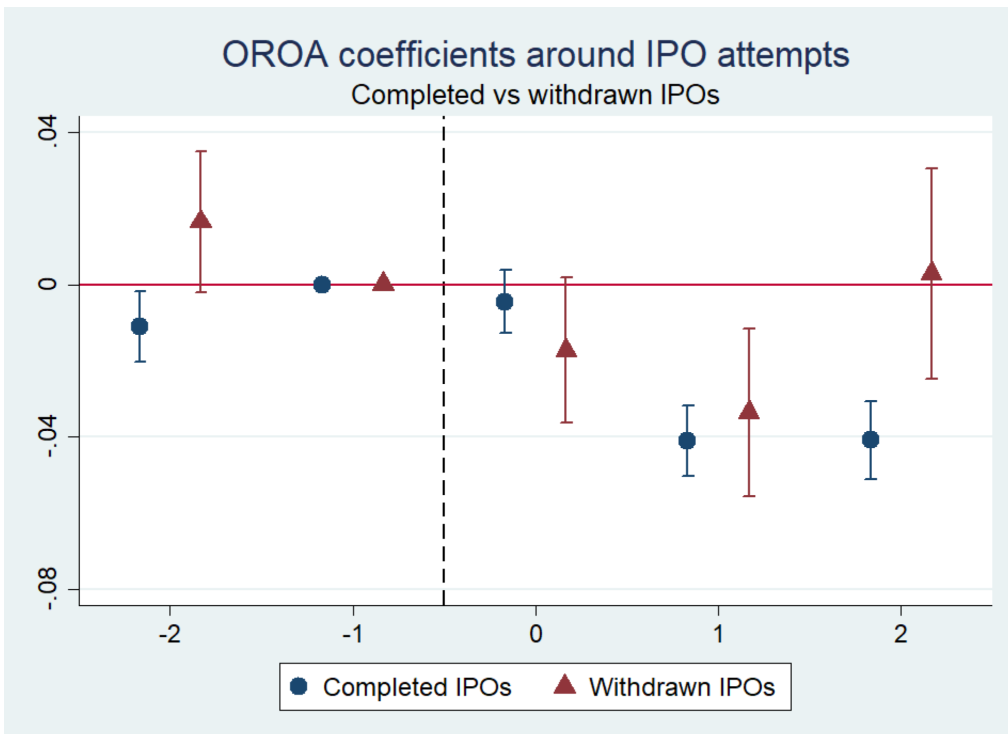


Notes: The figure shows the number of observations per event-year for withdrawn IPOs (dark bar, left axis) and completed IPOs (light bar, right axis). Event years are measured at the end-of-the-year around the IPO-attempt year (t=0).

Figure 3: Asymmetric Information and Market Returns

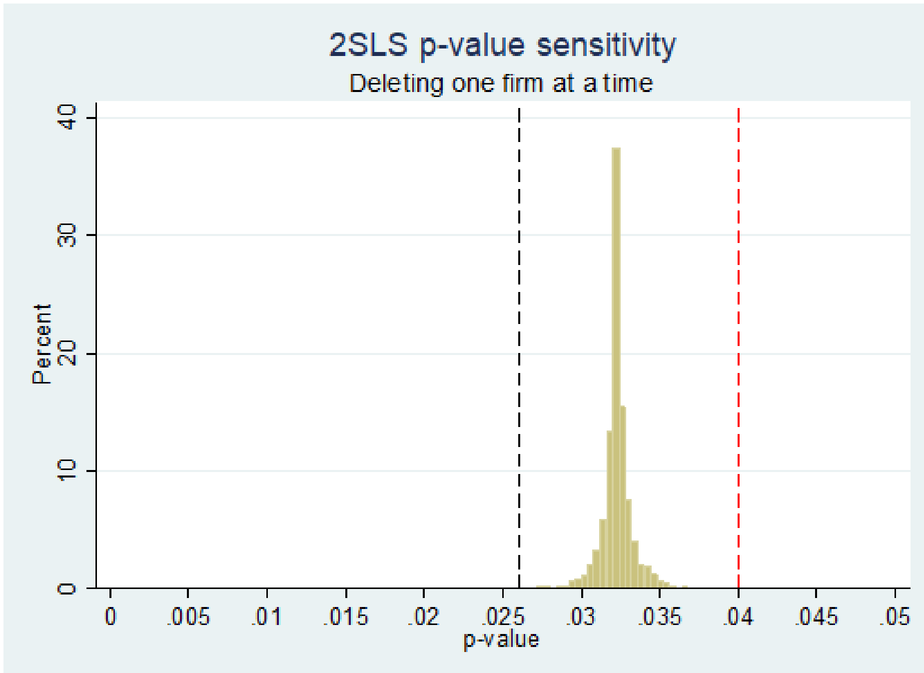
Notes: The figure shows the theoretical predictions of asymmetric information models regarding the decision to complete or withdraw the IPO according to (unobservable) firm quality and past market returns. More firms complete their IPOs after positive returns, and the average quality of issuers goes 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

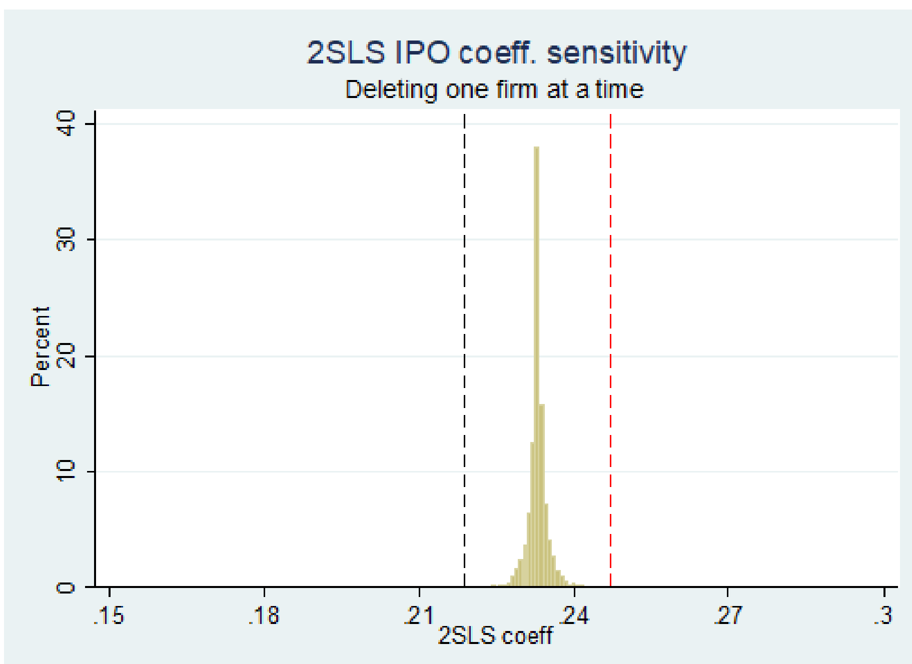


Notes: The figures displays coefficient estimates and 95% confidence intervals of event-time fixed effects. The dependent variable is run against event-time fixed effects for completed and withdrawn IPOs, setting t=-1 as the default category. The regressions include firm fixed effects to account for within-firm dynamics.

Figure 5: Statistical Inference when Excluding Clusters



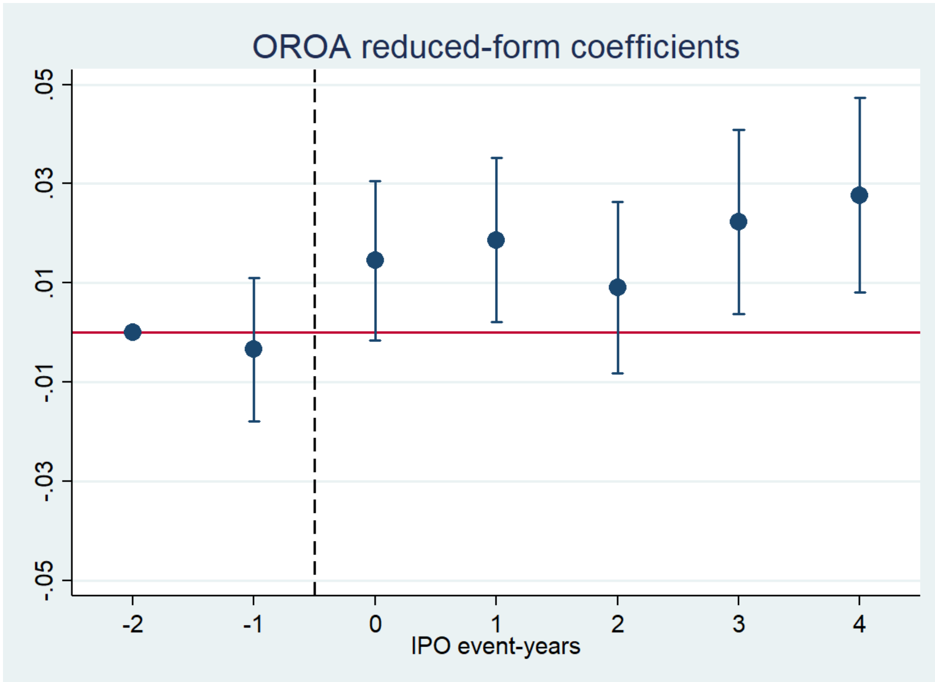
(a)



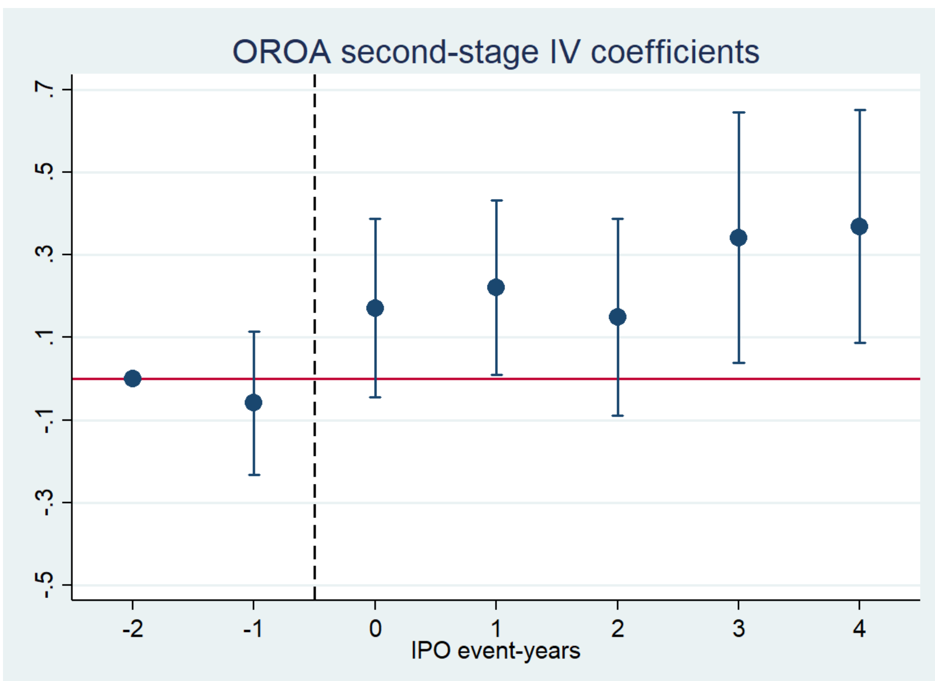
(b)

Notes: The figure shows the distribution of p-values (panel a) and β 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: Dynamics of the Effects by Event-Year



(a)



(b)

Notes: The figures displays the reduced-form coefficient estimates with 95% confidence intervals of event-year fixed effects times the indicator for positive returns (panel a), and the IV coefficients with 95% confidence intervals for event-year fixed effects times the completed IPO indicator (panel b). We set *t=-2* as the default category. In the IV estimation the instruments are the event-year fixed effects times the indicator for positive returns.

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. International is a dummy for when at least one subsidiary is in a different country from the firm's headquarters. The number of industries in which each firm is present is measured at the 3-digit SIC code. The number of patents corresponds to patent applications that are eventually granted. Patent (exploratory/exploitative) is a dummy for years with (exploratory/exploitative) patent applications that are eventually granted. Acquisitions corresponds to the number of firms acquired. 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 1%-winsorized.

	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
International	0.09	0	0	0	0	0	0.28	14,110
# Industries	1.58	1	1	1	1	3	1.65	14,110
# Patents	0.49	0	0	0	0	0	7.8	14,110
Patent	0.02	0	0	0	0	0	0.15	14,110
Exploitative	0.01	0	0	0	0	0	0.11	14,110
Exploratory	0.02	0	0	0	0	0	0.14	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
# Industries	1.239	1.366	-0.127**
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
# Industries	1.262	1.248	0.014
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) OROA	(2) IPO	(3) IPO	(4) OROA	(5) OROA	(6) 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			0.010			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 (non-positive) returns divided by the likelihood of completing (withdrawing) the IPO in the cross section. 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%	21.22%

Panel B: Characteristics of Compliers

Variable (pre IPO attempt)	First Stage	Relative complier likelihood
High OROA	7.65%	1.11
High Assets	7.16%	1.04
High Sales	6.11%	0.89
High Age	7.82%	1.13
High Leverage	4.04%	0.59
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 1-3 deal with particular events identified in the appendix on sample selection. In column 1, we add to the baseline sample excluded IPOs due to overlapping events. In column 2, we drop from the baseline sample firms with non-overlapping attempts. In column 3, we exclude the 12 withdrawn attempts that represent follow-up withdrawn attempts outside the 5-year interval window. In column 4, we drop cross-listings. Cross-country listing is when the main country of listing is different from the country where the firm is headquartered. In column 5, we add industry-times-year fixed effects when industries are defined at the 2-digit SIC level. In columns 6-7 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+4* in column 7. 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]
VARIABLES	OROA	OROA	OROA	OROA	OROA	OROA	OROA
IPO	0.210**	0.237**	0.250**	0.258**	0.253**	0.245**	0.268**
	(0.093)	(0.108)	(0.116)	(0.114)	(0.117)	(0.107)	(0.107)
Observations	12,450	12,076	12,143	11,889	12,189	14,162	15,932
Number of firms	3,259	3,165	3,183	3,121	3,195	3,195	3,195
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

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 a high anti-self-dealing index, for small firms, and for 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 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) OROA	(2) OROA	(3) OROA	(4) OROA
IPO	0.222**	0.242**	0.216*	0.216*
	(0.108)	(0.110)	(0.119)	(0.119)
IPO x High Anti-self-dealing	0.025**			0.023*
	(0.011)			(0.012)
IPO x Small Firm		-0.034***		-0.029**
		(0.012)		(0.012)
IPO x Young Firm			-0.007	-0.007
			(0.011)	(0.011)
Observations	12,189	12,055	10,884	10,789
Number of firms	3,195	3,142	2,835	2,798
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	Second stage	Second stage	Second stage	Second stage
Instrument #1	Positive 30d ret	Positive 30d ret	Positive 30d ret	Positive 30d ret
Instrument #2	Positive 30d ret x High anti-sd	Positive 30d ret x Small	Positive 30d ret x Young	All interactions

Table 7: The Effects of Going Public on Firm Scale and Productivity

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	0.093	0.659***	0.529	-0.406	-0.266**	-0.426	-0.565	0.113	2.983**	4.276**
	(0.128)	(1.392)	(1.684)	(0.092)	(0.834)	(0.877)	(0.109)	(1.205)	(1.314)	(0.124)	(1.408)	(2.003)
IPO x High Anti-self-dealing			0.089			0.153			0.21			0.103
			(0.166)			(0.100)			(0.130)			(0.145)
IPO x Small Firm			0.794***			1.100***			-0.278**			-0.206
			(0.165)			(0.086)			(0.126)			(0.172)
IPO x Young Firm			1.143***			0.621***			0.196*			-0.199
			(0.146)			(0.085)			(0.118)			(0.122)
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	3,401	3,401	2,986	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 for firm-years with subsidiaries (columns 1-3), dummies for firm-years with international presence (columns 4-6), the log of the number of acquisitions (columns 7-9), and a dummy for being the target of an acquisition (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) Sub.	(2) Sub.	(3) Sub.	(4) Int'l	(5) Int'l	(6) Int'l	(7) Acquis.	(8) Acquis.	(9) Acquis.	(10) Target	(11) Target	(12) Target
IPO	0.103***	0.481***	0.511***	0.035***	0.215**	0.351***	0.128***	0.012	-0.067	0.001	0.063	0.053
	(0.019)	(0.160)	(0.196)	(0.012)	(0.106)	(0.135)	(0.014)	(0.130)	(0.161)	(0.012)	(0.094)	(0.117)
IPO x High Anti-self-dealing			0.024			-0.045***			-0.012			-0.021*
			(0.021)			(0.014)			(0.018)			(0.013)
IPO x Small Firm			-0.122***			-0.115***			-0.036*			-0.029**
			(0.022)			(0.016)			(0.020)			(0.014)
IPO x Young Firm			0.051***			0.000			0.047***			0.000
			(0.020)			(0.013)			(0.016)			(0.012)
Observations	14,110	14,110	12,299	14,110	14,110	12,299	14,110	14,110	12,299	14,110	14,110	12,299
R-squared	0.39			0.13			0.11			0.09		
Number of firms	3,467	3,467	2,992	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	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 9: The Effects of Going Public on the Sensitivity to Tobin's q

This table shows regression using the log of assets (columns 1-3), and the log of the number of industries where each firm is present (columns 4-6) as dependent variables. Besides the IPO dummy, we add the interaction of IPO with the log of each industry's median Tobin's q. The median is computed in the sample of all publicly traded companies within a 3-digit SIC code industry in the 16 European countries for June of each year. All regressions also include the median Tobin's q as control. Columns 3 and 6 correct for measurement error in Tobin's q and its interaction with the IPO dummy using the minimum distance estimator based on higher-order cumulants, following Erickson, Jiang, and Whited (2014). We use the `xtewreg` Stata command developed by Erickson, Parham, and Whited (2014) with 5-order cumulants. As this command does not allow for fixed effects, we de-mean all the variables using the full set of fixed effects we include in the OLS and IV regressions. 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) Assets	(2) Assets	(3) Assets	(4) Industries	(5) Industries	(6) Industries
IPO	0.402**	0.044	-0.041	0.094***	0.289**	-0.011
	(0.163)	(0.856)	(0.118)	(0.030)	(0.138)	(0.056)
IPO x Tobin's q	0.288**	0.539**	0.335**	-0.049**	-0.076**	-0.187***
	(0.140)	(0.213)	(0.142)	(0.024)	(0.035)	(0.058)
Observations	13,670	13,670	13,670	14,110	14,110	14,110
FE	Yes	Yes	No	Yes	Yes	No
Regression	OLS	2nd stage	EJW	OLS	2nd stage	EJW
De-mean data	No	No	Yes	No	No	Yes

Table 10: The Effects of Going Public on Innovation

This table shows regressions using the dummies for firm-year observations with patent applications that are eventually granted (*Patent*), and for the presence of exploratory (*Exploratory*) or exploitative (*Exploitative*) patents. The classification of patents into exploratory and exploitative follows Custodio, Ferreira, and Matos (2019). We show the OLS regression, the second stage from the basic IV regression following Table 3, and the IV regressions 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) Patent	(2) Patent	(3) Patent	(4) Exploratory	(5) Exploratory	(6) Exploratory	(7) Exploitative	(8) Exploitative	(9) Exploitative
IPO	0.003	-0.079*	-0.042	0.004	-0.088**	-0.056	0.001	0.040*	0.021
	(0.004)	(0.041)	(0.046)	(0.004)	(0.042)	(0.046)	(0.003)	(0.023)	(0.027)
IPO x High Anti-self-dealing			-0.004			-0.002			-0.006**
			(0.004)			(0.004)			(0.003)
IPO x Small Firm			-0.002			0.002			-0.001
			(0.006)			(0.006)			(0.003)
IPO x Young Firm			0.006			0.008*			-0.002
			(0.004)			(0.004)			(0.002)
Observations	14,110	14,110	12,299	14,110	14,110	12,299	14,110	14,110	12,299
R-squared	0.03			0.03			0.02		
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 11: The Effects of Going Public on Management Turnover

This table shows regressions where dependent variables are dummies for firm-years with a change in CEO or CFO. We show the OLS regression, the second stage from the basic IV regression following Table 3, and the IV regressions 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) CEO Change	(2) CEO Change	(3) CEO Change	(4) CFO Change	(5) CFO Change	(6) CFO Change
IPO	0.014	0.149*	0.179	0.001	0.056**	0.043
	(0.009)	(0.090)	(0.114)	(0.004)	(0.028)	(0.034)
IPO x High Anti-self-dealing			0.001			0.008
			(0.012)			(0.005)
IPO x Small Firm			-0.016			-0.003
			(0.013)			(0.005)
IPO x Young Firm			-0.024**			0.008**
			(0.011)			(0.004)
Observations	14,110	14,110	12,299	14,110	14,110	12,299
R-squared	0.02			0.03		
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

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). 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. Patents exploratory (exploitative) is the number of exploratory (exploitative) patent applications that are eventually granted. An exploratory (exploitative) patent is one where 60% or more of its citations are based on new (current) knowledge (Custodio, Ferreira, and Matos, 2019). The next two variables are dummies for years with a change in CEO or CFO. Industry Tobin's q is the median q considering all publicly traded companies within a 3-digit SIC code industry in the 16 European countries in the sample in June of each year. If there is no data on Tobin's q for a 3-digit SIC-year, we consider the median q at a higher level of aggregation (2- or 1-digit levels).

	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
Target	0.03	0	0	0	0	0	0.171	14,110
# Exploitative	0.16	0	0	0	0	0	2.92	14,110
# Exploratory	0.29	0	0	0	0	0	4.67	14,110
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
Industry Tobin's q	1.58	0.95	1.08	1.4	1.86	2.31	0.71	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) IPO	(2) IPO	(3) IPO	(4) IPO	(5) OROA	(6) 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: The Effect of Going Public on EBIT

This table shows regressions where dependent variables are EBIT in millions of Euros (column 1), and dummies for firm-years where EBIT crosses a certain threshold defined in millions of Euros (columns 2-5). We show the second stage from the basic IV regression following Table 3. Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the firm level. Significant at: *10%, **5% and ***1%.

VARIABLES	(1) EBIT	(2) EBIT>25	(3) EBIT>50	(4) EBIT>75	(5) EBIT>100
IPO	29.519	0.237**	-0.005	0.129	0.135*
	(22.466)	(0.115)	(0.095)	(0.086)	(0.073)
Observations	12,189	12,189	12,189	12,189	12,189
Number of id	3,195	3,195	3,195	3,195	3,195
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	2nd stage	2nd stage	2nd stage	2nd stage	2nd stage

Table A.4: 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) OROA	(2) OROA	(3) OROA	(4) OROA Change	(5) 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.5: First Stage for Interactions

This table shows the first-stage regressions that include interaction terms with 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 Anti-sd	(3) IPO	(4) IPO x Small	(5) IPO	(6) IPO x Young
Positive 30-day ret x Post	0.081***	-0.391***	0.041***	-0.388***	0.055***	-0.400***
	(0.014)	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)
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	12,055	12,055	10,884	10,884
R-squared	0.89	0.74	0.90	0.76	0.90	0.75
Number of firms	3,195	3,195	3,142	3,142	2,835	2,835
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