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Ph.D Thesis

Conflicts of interest in initial public offerings

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To my family

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Summary introduction

In my thesis I investigate the conflicts of interests between underwriters, issuers, and investors in initial public offerings (IPOs). I provide empirical evidence that conflicts of interest exist, affect the IPO process, and have real monetary costs for IPO issuers. The novel data, methodologies, and identification strategies used in this thesis allow answering questions so far unexplored by the existing literature. The thesis consists of two chapters: Chapter 1 "Nepotism in IPOs: consequences for issuers and investors" and Chapter 2 "Do institutional investors play hide-and-sell in the IPO aftermarket?".

Nepotism in IPOs: consequences for issuers and investors (with François Degeorge)

In this chapter, we investigate the conflicts of interest that may arise when IPO underwriters allocate IPO shares to their affiliated funds. We hypothesize that nepotism incentives may affect IPO pricing: the underwriter may intentionally underprice the IPO, with a view to allocating the underpriced shares to its affiliated funds and make them gain at the expenses of the issuer. Using a novel hand-collected dataset of U.S. IPO allocations, we find support for this hypothesis in a regression discontinuity design (RDD) setting: a one percentage point increase in IPO allocations to affiliated funds leads to an increase in underpricing of 5.4 percentage points.

To construct our dataset we rely on section 10(f)-3 of the Investment Company Act, which requires investment companies to report their affiliated transactions to the U.S. Securities and Exchange Commission (SEC). Using reports from the SEC EDGAR database, we compile data on all IPO allocations to underwriter-affiliated funds between 2001 and 2013. Our novel hand-collected dataset provides a unique opportunity to investigate the role of underwriter-affiliated allocations within the IPO process, as prior studies could only rely on imprecise proxies for initial IPO allocations.

To identify the causal effect of affiliated IPO allocations on IPO underpricing we implement a fuzzy RDD, exploiting the institutional setting provided by rule 10(f)-3. This rule sets a threshold, requiring issuers to be at least three years old before the underwriter is allowed to allocate shares to its affiliated funds. Therefore, the size (and the probability) of underwriter-affiliated allocations jumps discontinuously when the age of the issuing firm is equal to or above the three year cutoff date. A fuzzy RDD exploits this discrete jump at the cutoff point, allowing us to estimate the effect of the treatment (affiliated allocations) on the outcome (underpricing), while eliminating any observed or unobserved confounding factors. Intuitively, firms that go public at slightly older than three years are arguably similar, on average, to firms that go public at slightly younger than three years. Hence, they have similar characteristics and expected underpricing. Because of the 10(f)-3 rule, however, they differ in their underwriter-affiliated allocations. By exploiting the three year cutoff in a fuzzy RDD setting, we estimate the causal effect of affiliated allocations on underpricing. Our evidence is consistent with underwriters intentionally underpricing IPOs to benefit their affiliated funds.

Our hand-collected dataset of affiliated IPO allocations also allows us to revisit a milder version of nepotism analyzed in prior studies, and we find much clearer support for it than prior work: we find a strong positive association between IPO underpricing and affiliated allocations, which strengthens when nepotism incentives are stronger.

Overall, our evidence suggests that the conflicts of interest generated by nepotism incentives are pervasive in the IPO allocation market and have real monetary costs for IPO issuers. Our results contribute to the existing literature by shedding light on the types of conflicts of interest that affect the IPO process and their consequences for both issuing firms and fund shareholders.

Do institutional investors play hide-and-sell in the IPO aftermarket? (with Tamara Nefedova)

In this chapter, we investigate a moral hazard problem faced by IPO investors. We hypothesize that investors have an incentive to hide their sell trades from the lead underwriters in the IPO aftermarket for two main reasons. First, one common view about book-built IPOs, which investment bankers tend to emphasize, is that IPO allocations are directed toward long-term investors, rather than to investors that readily sell their allocations in the IPO aftermarket (commonly referred to as "flippers"). Hence, flippers might try to hide their allocations sales in order to preserve their business with the lead underwriters in the IPO allocations market.

The second reason for hiding sell trades from the lead underwriters is related to a practice known as "laddering", which involves a quid-pro-quo arrangement between underwriters and their clients: investors receive IPO allocations in exchange for a committment to purchase additional shares in the aftermarket. Ladderers may have an incentive to break their quid-pro-quo arrangements if the shares that they committed to buy in the secondary market are in excess of their optimal holdings in the IPO firm. The potential costs for the investors that break the agreement, in terms of future business with the underwriters, may incentivize them to hide their sell trades. To the best of our knowledge, we are the first to hypothesize that the laddering mechanism may provide an incentive for investors to hide their sell trades.

We investigate a simple hiding strategy, which is to sell stocks through brokers other than the lead underwriters. Using detailed institutional trading data, we document that institutional investors are less likely to sell than buy through the lead underwriters in the aftermarket of IPOs issued between 1999 and 2010 in the United States. The probability of trading through a lead underwriter during the first month after the issue is about 6 percentage points less for sell trades than for buy trades. This result holds when controlling for important determinants of the choice to trade with a lead underwriter, such as the relationship between the institution and the lead underwriters, and is robust to institution, IPO, and institution-IPO fixed effects. Moreover, we find that the buy/sell asymmetry strengthens when hiding incentives are stronger.

Our data and methodology allow us to disentangle investors' allocation sales from their buying and selling activity in the secondary market. Hence, we can investigate the reasons behind institutions' hiding behavior, in order to understand whether it is driven by flipping or laddering motives. Contrary to the conventional view, we find that the intention to flip is not an important motive for hiding sell trades from the lead underwriters; institutions that sell shares through non-lead brokers tend to have bought them through the lead underwriters in the IPO aftermarket, consistent with institutions breaking their laddering agreements.

Our evidence sheds light on how hiding incentives affect institutions' choice of their broker in the IPO aftermarket and stimulates further research to investigate how the incentives of IPO investors may influence the IPO allocation process.

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This thesis is dedicated to my family, for their unconditional and constant love, support, patience, and care, that kept me going.

Nepotism in IPOs: consequences for issuers and investors*

François Degeorge[†] and Giuseppe Pratobevera[‡]

Abstract

Potential conflicts of interest arise when IPO underwriters allocate IPO shares to their affiliated funds. We hypothesize that nepotism incentives may affect IPO pricing. Using a novel hand-collected dataset, we find support for this hypothesis in a regression discontinuity design (RDD): a one percentage point increase in affiliated allocations increases underpricing by 5.4 percentage points. Our evidence suggests that nepotism has real monetary costs for IPO issuers. We also use our dataset to revisit a milder version of nepotism analyzed in prior studies, and we find much clearer support for it than prior work: we find a strong positive association between IPO underpricing and affiliated allocations, which strengthens when nepotism incentives are stronger.

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1 Introduction and motivation

The bookbuilding mechanism is the dominant method of bringing companies public in the United States. A distinctive feature of this mechanism is that the investment bank that underwrites the IPO has discretion over the choice of the offer price and the investors who receive allocations. Therefore, when an IPO underwriter is affiliated with a fund manager, three potential conflicts of interest arise:

1. The underwriter may allocate shares in overpriced ("cold") IPOs to its affiliated funds in order to ensure the completion of the issue. Ritter and Zhang (2007) refer to this conflict of interest as the "dumping ground" hypothesis.

2. The underwriter may allocate shares in underpriced ("hot") IPOs to its affiliated funds in order to boost the performance of those funds. Ritter and Zhang (2007) refer to this conflict of interest as the "nepotism" hypothesis.

3. The underwriter may intentionally underprice the IPO, with a view to allocating the underpriced shares to its affiliated funds. To our knowledge this potential conflict has not been investigated before. We label it the "supernepotism" hypothesis.

Using a hand-collected dataset of U.S. IPO allocations, we find support for the supernepotism hypothesis in a regression discontinuity design (RDD) setting: a one percentage point increase in IPO allocations to affiliated funds leads to an increase in underpricing of 5.4 percentage points. Our evidence suggests that the conflict of interest inherent in the underwriter-fund manager association has real monetary costs for IPO issuers, in addition to the distortions affecting investors that are documented in the existing literature (Ritter and Zhang (2007)).

To construct our dataset we rely on section 10(f)-3 of the Investment Company Act, which requires investment companies to report their affiliated transactions to the U.S. Securities and Exchange Commission (SEC). Using reports from the SEC EDGAR database, we compile data on all IPO allocations to underwriter-affiliated funds between 2001 and 2013. Our final dataset includes 1,294 IPOs underwritten by 64 underwriters involved in transactions with their affiliated funds.

Identifying the causal effect of affiliated IPO allocations on IPO underpricing is challenging because IPO allocations and IPO offer prices are jointly endogenously determined. As the outcome of profit-maximizing decisions of investment banks, both allocations and offer prices are most likely affected by and correlated with firm characteristics and other unobserved confounding factors. We argue that the 10(f)-3 rule provides the institutional setting needed to single out the causal effect we are interested in identifying. This rule sets a threshold, requiring issuers to be at least three years old before the underwriter is allowed to allocate shares to its affiliated funds. Therefore, the size (and the probability) of underwriter-affiliated allocations jumps discontinuously when the age of the issuing firm is equal to or above the three year cutoff date. A fuzzy regression discontinuity design (RDD) exploits this discrete jump at the cutoff point, allowing us to estimate the effect of the treatment (affiliated allocations) on the outcome (underpricing), while eliminating any observed or unobserved confounding factors. Intuitively, firms that go public at slightly older than three years are arguably similar, on average, to firms that go public at slightly younger than three years. Hence, they have similar characteristics and expected underpricing. Because of the 10(f)-3 rule, however, they differ in their underwriter-affiliated allocations. By exploiting the three year cutoff in a fuzzy RDD setting, we can estimate the causal effect of affiliated allocations on underpricing.

Our hand-collected dataset of affiliated IPO allocations also allows us to revisit the dumping-ground and nepotism hypotheses analyzed in the prior literature, especially by Ritter and Zhang (2007). Several prior studies use fund holdings to proxy for initial IPO allocations (Ritter and Zhang (2007), Reuter (2006), Hao and Yan (2012), and Mooney (2015)). These proxies may be imprecise, as the first few days following an IPO typically exhibit strong trading volumes (Ellis et al. (2000)). Moreover, underwriters trying to dump cold shares on an affiliated fund are more likely to do so in aftermarket trading than during an initial IPO allocation, when they would run afoul of the spirit of rule 10(f)-3, which is to protect "fund shareholders by preventing an affiliated underwriter from placing or 'dumping' unmarketable securities with the fund."[1] Hence, the use of secondary-market data (rather than initial allocations) is likely to overstress the relative importance of dumping-ground incentives compared to nepotism incentives. In our dataset of initial IPO allocations, we find strong evidence that nepotism is pervasive in IPO allocations and dominates any dumping-ground incentives. Affiliated funds receive more allocations when IPOs are more severely underpriced, suggesting that the funds are favored by their affiliated investment banks.

We consider three elements that might determine the relative importance to investment banks of the nepotism and dumping-ground conflicts. First, dumping-ground incentives should be stronger when the underwriter is completing an abnormally low number of IPOs (Ritter and Zhang (2007)). In such times, the marginal benefit of completing an additional IPO is higher for the investment bank, which not only receives revenues from the underwriting discount but may also be protecting its reputation. Second, underwriters receive commissions kickbacks when they allocate underpriced shares to independent, meaning unaffiliated, funds (Reuter (2006), Nimalendran et al. (2007), and Goldstein et al. (2011)); this source of revenue dampens their incentive to favor their affiliated funds (Ritter and Zhang (2007)). Accordingly, the nepotism incentive should be weaker when the underwriter receives an abnormally high stream of brokerage commissions from institutional investors. Third, we argue that the relative benefits and costs of affiliated allocations depend on the level of asymmetry in information concerning the issuer's value. When information asymmetry is high, the contribution of affiliated funds to price discovery may be lower than that of independent funds, as the affiliated funds might have access to signals that are highly correlated with those of the underwriters. Nepotism incentives might be relatively low and dumping-ground behavior might rise as a consequence of favoring independent funds to gain increased access to information. Therefore, we postulate that the nepotism conflict weakens as information asymmetry increases.

Overall, we find evidence consistent with these hypotheses. This suggests that while the nepotism and dumping-ground conflicts are likely both at play in the IPO allocation process, the nepotism conflict dominates the other.

[1] See for example https://www.sec.gov/rules/final/ic-25888.htm, section A.3.

2 Literature review and hypothesis development

An increasing body of literature investigates the role played by conflicts of interest within the IPO bookbuilding process, providing extensive evidence that underwriters allocate shares in ways that could be detrimental to issuers. Several researchers examine the hypothesis that underwriters preferentially allocate IPO shares to institutional investors that give back part of the underpricing gains in the form of brokerage commissions (the "commission-kickbacks conflict" hypothesis). Using an event-study methodology, Goldstein et al. (2011) find that underwriters' brokerage commission revenues are abnormally high in the period preceeding hot IPOs. Consistent with Nimalendran et al. (2007), they find that one of the strategies used to increase commissions is churning shares through round-trip trades in liquid stocks. Moreover, Reuter (2006) and Jenkinson et al. (2017) find a direct positive correlation between the dollar amount of commissions paid by a fund family to an investment bank and the family's allocations of underpriced IPOs underwritten by the same bank. Griffin et al. (2007) find evidence of the practice known as "laddering," which involves a quid-pro-quo arrangement between underwriters and their clients: investors receive IPO allocations in exchange far a promise to buy additional shares in the aftermarket. Liu and Ritter (2010) focus on "spinning," the practice of allocating hot shares to corporate executives to influence their decisions to hire the investment bank for future services; they find that these executives are less likely to switch investment bankers in follow-on offers. Ritter and Zhang (2007) and Mooney (2015) analyze the conflicts of interest involved in the allocation of IPOs to underwriter-affiliated funds, in the U.S. market and worldwide, respectively. Their evidence is mixed. Ritter and Zhang (2007) find some evidence of nepotism (underwriters favor their affiliated funds in the allocation of hot IPOs, mainly during the internet bubble period). Mooney (2015) finds large cross-country differences in the types of conflicts of interest that affect the allocation of IPO shares to affiliated funds.

Another line of research focuses on conflicts of interest between investment banks and their affiliated investment management arms. Consistent with the existence of costly agency problems, Berzins et al. (2013) find that bank-affiliated funds significantly underperform independent funds. Hao and Yan (2012) find one reason behind this underperformance to be that affiliated funds tend to hold a disproportionately large amount of cold equity issues underwritten by their affiliated banks, consistent with dumping-ground behavior.

Our study joins these two lines of research, as we examine the conflicts of interest between issuers, investment banks, and their affiliated investment management companies in the context of IPO allocations to underwriter-affiliated funds. Like Ritter and Zhang (2007), we investigate the conflicts of interest involved in the allocation of IPO shares to underwriter-affiliated funds, and we frame our discussion in terms of the nepotism and dumping-ground conflicts. However, we approach these questions using different hypotheses, methodology, data sources, and the time period covered by our sample.

Our study makes four novel contributions. First, we construct a direct measure of IPO allocations to affiliated funds using hand-collected data, instead of relying on proxies based on fund holdings. Second, we argue that conflicts of interest incentives may affect IPO pricing, not just IPO allocations to affiliated funds, and we find support for this new hypothesis using a RDD methodology (see subsection 2.1, Hypothesis 1). Our empirical analysis allows us to assess the monetary costs of conflicts of interest for issuers. Third, we exploit our data to test some hypotheses that have been developed by prior studies, but have not been directly tested yet; for example, we use trading commission data to directly test that nepotism incentives are weaker when the underwriter receives a high stream of brokerage commissions in the secondary market (see subsection 2.3, Hypothesis 4). Fourth, we develop and test a new hypothesis about the cross-sectional variation of conflicts of interest incentives; that is, nepotism incentives are weaker when the information asymmetry about the issuer's value is higher (see subsection 2.3, Hypothesis 5).

2.1 The effect of the underwriter/affiliated fund conflict of interest on IPO pricing

The nepotism hypothesis is generally framed within the allocation choice of the underwriter, which gives preferential treatment to its affiliated funds. The existing literature emphasizes the role played by the discretion of the underwriter in the choice of allocation. However, the underwriter has discretion over both the allocation decision and the pricing decision. When a bookbuilding method is used, an investment bank can jointly set the offer price and the amount allocated to its affiliated investors in a way that will maximize its own profits. We postulate that if an underwriter is subject to nepotism, then there is an incentive to abnormally underprice IPOs to benefit the affiliated funds. Hence, we formulate the following "strong-form" of the nepotism conflict, which we label the supernepotism hypothesis:

Hypothesis 1. *If underwriters face supernepotism incentives, then underpricing is an increasing function of the percentage of shares allocated to affiliated funds.*

2.2 Nepotism vs. dumping-ground

Our hand-collected dataset of affiliated IPO allocations also enables us to revisit the dumping-ground and nepotism hypotheses analyzed in the prior literature. On the one hand, underwriters might give preferential treatment to their affiliated funds, giving them hot IPOs to enhance their performance (nepotism hypothesis). Such behavior might be costly for issuers, as their shares would not be allocated according to their best interests. On the other hand, underwriters might dump cold IPOs on their affiliated funds, so that more deals could be completed at the expense of funds' shareholders (dumping-ground hypothesis). These potential conflicts of interest generate two opposite testable predictions. If the nepotism conflict dominates the IPO allocation market, then allocations to underwriter-affiliated funds and underpricing should be positively related. If the dumping-ground conflict dominates the IPO allocation market, then allocations to underwriter-affiliated funds and underpricing should be negatively related. Based on this discussion, we formulate the following hypothesis:

Hypothesis 2. *(2a) If nepotism incentives dominate dumping-ground incentives, then the correlation between underpricing and the percentage of shares allocated to* *affiliated funds is positive.* ***(2b)*** *If dumping-ground incentives dominate nepotism incentives, then the correlation between underpricing and the percentage of shares allocated to affiliated funds is negative.*

2.3 Variation in conflict of interest incentives

Ritter and Zhang (2007) argue that the relative weight of these two incentives in the investment bank's profit function depends on the market conditions the underwriter faces. When the underwriter faces a cold IPO market, dumping-ground incentives gain importance, as the marginal benefit of completing an IPO is higher. We build on this intuition to argue that this incentive is underwriter-specific. When the underwriter is completing a low number of IPOs, relative to its normal business, then the pressure to complete IPOs gain importance and the dumping-ground conflict emerges. When the underwriter is completing a high number of IPOs, relative to its normal business, then the benefit of completing an additional IPO is low. The revenues from the management and performance fees of affiliated funds gain weight in the investment bank's profit function and the nepotism conflict stands out. Hence, we formulate the following hypothesis:

Hypothesis 3. *The correlation between underpricing and the percentage of shares allocated to affiliated funds is lower when the underwriter expects to complete a small number of IPOs relative to its normal business.*

Ritter and Zhang (2007) argue that IPO allocations depend on the relative ability of affiliated and independent funds to generate revenues for the investment bank. As the commission-kickbacks conflict gains importance in the underwriter's profit function, the incentive to allocate underpriced shares to affiliated funds is reduced. If the underwriter enters a quid-pro-quo agreement with unaffiliated, independent funds, it might tend to give them preferential treatment in exchange for higher brokerage commission revenues (Reuter (2006), Nimalendran et al. (2007), Goldstein et al. (2011), and Jenkinson et al. (2017)), thus putting nepotism incentives aside. Our access to trading commissions data enables us to test the following hypothesis:

Hypothesis 4. ***(4a)*** *The correlation between underpricing and the percentage of shares allocated to affiliated funds is higher when the underwriter receives a low stream of brokerage commissions in the secondary market.* ***(4b)*** *The correlation between underpricing and the percentage of shares allocated to unaffiliated funds is higher when the underwriter receives a high stream of brokerage commissions in the secondary market.*

In standard information-based bookbuilding theories (such as Benveniste and Spindt (1989)), underpricing is the compensation for the information-revealing indications of interest by institutional investors. We argue that the level of information asymmetry influences conflict of interest incentives because of the roles played by different classes of investors in providing information. In firms with high information asymmetry, the contribution of affiliated funds to price discovery may be lower than that of independent funds. The affiliated funds might have access to signals that are highly correlated with those of their affiliated underwriters, thus making their contribution to price discovery of little value. Nepotism incentives still exist, but they might be relatively low, as the underwriter needs to reward the unaffiliated funds for providing information. Therefore, underwriters might give preferential treatment to independent funds that reveal their signals when information asymmetry is high, thus penalizing the affiliated funds. Some dumping-ground behavior might also arise as a consequence of favoring independent funds. In firms with low information asymmetry, instead, price discovery matters less, giving the underwriter more scope to allocate hot shares to its affiliated funds. Hence, the nepotism incentive might gain importance in the profit function of the investment bank. Based on this argument, we posit that the correlation between underpricing and affiliated allocations should be higher in low information asymmetric firms, while the correlation between underpricing and non-affiliated allocations should be greater in high information asymmetric firms. We formulate the following hypothesis:

Hypothesis 5. ***(5a)*** *The correlation between underpricing and the percentage of shares allocated to affiliated funds is higher when information asymmetry is low.* ***(5b)*** *The correlation between underpricing and the percentage of shares allocated to unaffiliated funds is higher when information asymmetry is high.*

3 Data and summary statistics

Section 10(f) of the investment company act of 1940 prohibits underwriters from selling any shares of a security offering to funds that are in any way affiliated with any member of the syndicate. This regulation was amended in 1958 and in subsequent years to exempt certain transactions. As of today, rule 10(f)-3 permits funds to buy securities underwritten by their affiliated underwriters if certain conditions are satisfied. For the purposes of this research, four of these conditions are of particular importance:

- the issuer must have been in continuous operation for at least three years prior to the offering, including the operations of any predecessors;
- the securities are offered under a firm-commitment contract;[2]
- the affiliated transaction has to be executed by a syndicate member other than the affiliated underwriter;[3]
- the existence of any transaction pursuant to the 10f-3 rule has to be reported on the form N-SAR of the investment company, attaching a written record of the details of each transaction.

The first three items allow us to identify IPOs that are eligible for 10(f)-3 transactions, that is, IPOs whose shares can be allocated to underwriter-affiliated funds. The last item allows us to hand collect a novel dataset containing data about IPO allocations received by funds affiliated to the underwriters.

[2]In a firm-commitment contract, the underwriter guarantees to purchase all the securities offered by the issuer, regardless of whether or not they can sell them to investors.

[3]For example, take issuer X, underwritten by banks A and B. Rule 10(f)-3 says that funds affiliated to bank A can receive allocations only from bank B, and, viceversa, funds affiliated to bank B can receive allocations only from bank A.

In the following subsections, we describe our sample selection criteria, define the main variables used in our analyses, and provide summary statistics.

3.1 IPO data

We use the Thomson Financial Security Data Company (SDC) database to identify IPOs made in the United States from 2001 to 2013.[4] We exclude all American Depository Receipts (ADRs), Real Estate Investment Trusts (REITs), unit and rights offerings, closed-end funds, IPOs with SIC codes between 6000 and 6199 and IPOs with offer price smaller than $5. Moreover, we require IPOs to have a match with the Center for Research in Security Prices database (CRSP) within seven calendar days from the issue. These filters leave us with 1,294 IPOs.

From SDC and CRSP we get the name of the issuer and its SIC code, the nation where the issuer is located, the CUSIP and PERMNO numbers of the security issued, the issue date and filing date, the offer price and the original midpoint of the filing price range, the first day closing price, the number of shares issued and whether they are primary or secondary shares, the total assets of the issuer before the IPO,[5] the primary exchange where the shares are listed, the identity and number of lead managers and other syndicate members, the underwriting gross spread and the type of underwriting contract under which the securities are issued, and a flag identifying venture backed IPOs. We match our sample with data available on the IPO data website managed by Jay R. Ritter at the University of Florida to find the issuers' founding years and the underwriters' reputation rankings.[6] When the founding year is not available on the Ritter website, we complement it with the founding date available on SDC. Underwriters' reputations are coded using numbers ranging from 1 (lowest ranking) to 9 (highest ranking). These rankings are described in Loughran and Ritter (2004) and are an adjustment to the Carter and Manaster (1990) rankings. Table 1 describes the IPO variables we compute by matching the SDC, CRSP, and Ritter data.

[Table 1 about here.]

We define an IPO to be eligible for affiliated transactions pursuant to rule 10(f)-3 if each of the following four conditions is met:

- $Age \geq 3$
- $FirmCommitment = 1$
- $NumberSyndicateMembers > 1$
- at least one lead underwriter has been involved in a 10(f)-3 transaction in our sample.

[4]We clean the database from known mistakes by manually applying the corrections listed, as of April 2014, on the IPO database managed by Jay R. Ritter at the University of Florida: https://site.warrington.ufl.edu/ritter/ipo-data/.

[5]When the total assets pre-IPO are missing in SDC, we proxy them by subtracting the total proceeds of the IPO from the total assets after the IPO, taking the latter from COMPUSTAT.

[6]The link is: https://site.warrington.ufl.edu/ritter/ipo-data/

The first three conditions are a direct consequence of the 10(f)-3 rule's requirements. The rationale behind our fourth condition is that underwriters that have never been involved in 10(f)-3 transactions might not have affiliated funds.[7] From our original sample of 1,294 IPOs, we count 1,086 IPOs that are eligible for affiliated transactions; 208 IPOs do not satisfy at least one of the four requirements. Figure 1 plots the number of IPOs by year, distinguishing between in eligible and non-eligible IPOs.

[Figure 1 about here.]

The total number of IPOs per year varies considerably, ranging from 21 in 2008 to 169 in 2004. The percentage of eligible IPOs, at about 84% on average, appears to be stable in the period 2001-2013.

Table 2 provides summary statistics on our sample of IPOs, breaking them down into eligible IPOs (Panel A) and non-eligible IPOs (Panel B). All non-dummy variables except *Age* are winsorized at the 95% level.[8] Table 2 shows that non-eligible IPOs differ from eligible IPOs in that they are smaller and younger, have lower underpricing, and are less likely to be underwritten by a top-ranked underwriter.

[Table 2 about here.]

3.2 Allocations data

Investment companies report their affiliated transactions to the Securities and Exchange Commission (SEC) through the N-SAR filings. We download from the SEC EDGAR database all the N-SAR forms filed from January 2001 to December 2014 and collect data on affiliated IPO allocations in the period 2001-2013. (Appendix A explains the downloading, parsing, and matching procedures.) Using this data, we build our Affiliated Allocations dataset, which contains: IPO identifiers (issuer name, CUSIP, and issue date); the name of the affiliated fund and/or the sub-portfolio of the fund and/or the investment company that receive an allocation; the number of shares received by the affiliated fund and/or by the sub-portfolio of the fund and/or by the investment company the fund is managed or advised by; the name(s) of the affiliated underwriter(s); and the name(s) of the underwriter(s) from whom the shares were purchased, often referred to as the "broker" in the N-SAR filings. Hence, we observe the number of shares allocated at the IPO-investor-broker level.

For the purposes of this paper, in our main analyses we aggregate affiliated allocations at the IPO level, letting A_i be the total number of shares allocated to affiliated funds in IPO i. Then we build the two main variables of our analysis: $AffiliatedAllocPerc$ and $AffiliatedAllocDummy$. The variable $AffiliatedAllocPerc$ is the percentage of the issue allocated to affiliated funds. If N_i is the number of shares issued in IPO i, then:

$$AffiliatedAllocPerc_i = 100\frac{A_i}{N_i}$$

[7]Another possibility is that they do have affiliated funds, but consider the costs of allocating shares to them to be too high (such as the costs of compliance with the 10(f)-3 rule).

[8]We do not winsorize *Age* because it is the forcing variable in the RDD of section 4.

For robustness, we also use the variable $AffiliatedAllocDummy$, which is a dummy variable equal to one if at least one share is allocated to an affiliated fund:

$$AffiliatedAllocDummy_i = \mathbb{1}(A_i > 0)$$

The N-SAR filings provide information about affiliated allocations only. We also build a proxy for the percentage of the issue allocated to independent funds, that is, to funds not affiliated with the underwriters of a given IPO. First, we match the SDC sample to the Thomson Financial CDA/Spectrum 1&2 database (s12) using CUSIP numbers. Then we compute the total holdings held by mutual funds at the first reporting date after each IPO, excluding non-U.S. mutual funds and mutual funds with investment codes of 5, 6, or 8, letting H_i be the total number of shares held by mutual funds in company i at the first reporting date after the IPO of company i. Then we build a proxy for the percentage of the issue allocated to independent funds as:[9]

$$IndependentAllocPerc_i = 100\frac{H_i - A_i}{N_i}$$

In order to reduce the impact of potential data errors and outliers, we winsorize the allocation variables $AffiliatedAllocPerc$ and $IndependentAllocPerc$ at the 95% level.

Table 3 summarizes the allocation data at the issuer level for the 1,086 eligible IPOs (Panel A) and the 208 non-eligible IPOs (Panel B). Panel (A) reports that 611 IPOs, about 56% of the eligible IPOs, involve at least one affiliated transaction and, on average, 1.44% of the issue is allocated to funds affiliated with the underwriters. This implies that, conditional on involving at least one 10(f)-3 transaction, the average percentage allocated to affiliated funds is 2.57% (1.44 divided by 0.56). The median affiliated allocation is lower than the mean, indicating a positive skewness. The average percentage of the issue allocated to independent funds is 18.3%.

Panel (B) reports the same statistics for non-eligible IPOs. Interestingly, underwriters allocate shares of non-eligible IPOs to their affiliated funds in 17 IPOs, about 8% of such IPOs. Eight of these IPOs do not satisfy the age requirement, being less than three years old. There are several reasons why underwriters might have allocated shares to their affiliated funds in these cases. First, these IPOs may be misclassified as "non-eligible". Errors in the issuers' founding dates or the existence of unknown predecessors could have led us to miscalculate the issuers' age. A second possibility is that the age is correct, but no enforcement action was recommended by the SEC. In a private conversation, an SEC expert pointed out that the Securities and Exchange Commission takes into account the general principles behind the 10(f)-3 rule when interpreting and applying it. Consequently, certain transactions that seem to formally violate the rule could, in fact, be allowed.[10] A

[9]This proxy is noisy for two reasons. First, it is affected by aftermarket trading of both affiliated and unaffiliated funds. Second, it is affected by the different coverage of funds in our Affiliated Allocations dataset and in the s12 database.

[10]One popular example dates back to 2008, when the Goldman Sachs Trust requested assurance that the SEC would not have recommended any enforcement action related to some affiliated allocations of fixed-income securities issued by companies that were less than three years old. These securities were co-issued with and 100% guaranteed by another company that was more

third possibility is that underwriters might have broken the 10(f)-3 rule in these cases, allocating shares of non-eligible issuers to their affiliated funds. A search on Google provides information consistent with the founding dates contained in our dataset, and we decide to flag these eight IPOs as non-eligible.

One of the 17 non-eligible IPOs does not satisfy the firm commitment requirement, while the remaining eight non-eligible IPOs do not satisfy the lead underwriter requirement, meaning that none of their lead underwriters has ever been involved in a 10(f)-3 transaction in our sample. In these eight IPOs, affiliated transactions involve other syndicate members only.[11]

[Table 3 about here.]

Figure 2 shows the average allocations to affiliated and independent funds over the period 2001-2013 for the 1,086 eligible IPOs. Panel (A) shows that the percentage of IPOs with affiliated allocations ranges from a minimum of 41% in 2008 to a peak of 77% in 2009, with no apparent trend in the period 2001-2013. The average percentage allocation to affiliated funds ranges from a minimum of 0.87% in 2005 to a peak of 2.72% in 2009 and behaves similarly to the average percentage of the issue allocated to affiliated funds conditional on IPOs involving at least one affiliated transaction. This means that in periods when underwriters are more likely to allocate some shares to their affiliated funds, the size of the affiliated allocations tend, on average, to be larger.

We notice no apparent increase in affiliated allocations after 2003, when the SEC amended the 10(f)-3 rule, loosening some of its constraints. In particular, after 2003 the maximum amount of shares that an underwriter can allocate to its affiliated funds (the "percentage limit," or 25% of the issue) applies to the principal underwriter only. This constraint is not binding in the IPO allocations market, as affiliated allocations are far below the percentage limit imposed by the 10(f)-3 rule.

While affiliated allocations do not show a clear trend over the time period of our sample, we do notice that the percentage of the issue allocated to independent funds has sharply increased in recent years, from about 15% before 2010 to almost 25% afterward.

[Figure 2 about here.]

To assess the contribution of our novel dataset, it is worth comparing these summary statistics with those of Ritter and Zhang (2007), as they used the Spectrum 1&2 holdings to proxy for affiliated allocations. The only overlapping year between our research and theirs is 2001. Ritter and Zhang (2007) find that affiliated funds report positive holdings for approximately 26% of the IPOs in 2001, while the true percentage of IPOs involving affiliated allocations, based on N-SAR filings, is about

than three years old and, thus, was compliant with the 10(f)-3 rule. The SEC concluded that the characteristics of the co-issue and the 100% guarantee were consistent with the aim of the rule, which is to avoid unmarketable securities being dumped to affiliated funds. Hence, it assured Goldman Sachs that it would not have recommended any enforcement action. See the SEC's interpretative letter for more details:
https://www.sec.gov/divisions/investment/noaction/2008/goldmansachstrust081908.htm

[11]Including these 17 IPOs in the eligible sample does not sensibly change the magnitude and the statistical significance of the regressions estimates in section 5.

71%. Moreover, they find that the average allocation - conditional on the allocation being greater than zero - is 0.7%, while according to the N-SAR filings it is 2.93%. These numbers suggest that using the Spectrum 1&2 holdings to proxy for affiliated allocations might considerably understate their prevalence and size.

In our dataset, we preserve the names of the underwriters affiliated with the funds that receive allocations. We count 64 underwriters involved in at least one 10(f)-3 transaction in our sample. In the average IPO, there are 5.3 syndicate members - 2.2 of whom are lead managers - who could be involved in an affiliated transaction. On average, 1.2 of them allocate some shares to their affiliated funds. Table 4 lists the names of the 14 underwriters that are most active in the affiliated allocations market.[12] The table reports the number of eligible IPOs underwritten by each underwriter and the number and percentage of IPOs in which each underwriter allocates some shares to its affiliated funds. JP Morgan stands out, with 230 IPOs allocated to its affiliated funds, about 60% of the eligible IPOs that it underwrites. Morgan Stanley and Merrill Lynch follow, with about half the number of IPOs allocated to their affiliated funds. Some banks, however, do not often allocate IPO shares to their affiliated funds. For example, Credit Suisse allocated only 32 IPOs out of 352 to its affiliated funds.

[Table 4 about here.]

3.3 Do IPOs allocated to affiliated funds differ from other IPOs?

Table 5 reports difference-of-means (Panel A) and difference-of-proportions (Panel B) tests to assess whether IPOs with a positive allocation to affiliated funds differ from those with no allocations to affiliated funds. The table shows that the two groups of IPOs do differ significantly, both economically and statistically. Noticeably, affiliated funds are more likely to receive allocations when the issue is more underpriced: the first day return is about 11.8 percentage points higher when funds affiliated with the underwriters receive some allocation, consistent with nepotism behavior. This pattern is confirmed by the main predictor of underpricing, which is the percentage adjustment from the midpoint of the filing range to the offer price (Hanley (1993)). On average, IPOs allocated to affiliated funds are priced 3.1 percentage points above the midpoint of the filing range, while IPOs with no allocations to affiliated funds are priced about 7.6 percentage points below the midpoint of the filing range. The two groups differ by approximately 10.7 percentage points.

Though suggestive, this univariate evidence is not enough to conclude that the nepotism/supernepotism hypotheses hold or that the dumping-ground hypothesis does not hold, as IPOs with affiliated allocations also differ from those with no affiliated allocations in several other ways. As concerns the characteristics of the issuer, affiliated funds are more likely to receive shares of older and larger firms: IPOs with affiliated allocations are approximately seven years older than, and almost two times as large as, other IPOs. Hence, affiliated funds are more likely to receive shares when the information asymmetry of the issuer is lower. This finding is broadly consistent with bookbuilding theories, as underwriters might allocate more shares

[12] These 14 most active affiliated underwriters are involved in 10(f)-3 transactions in at least 25 IPOs.

to independent funds that reveal their signals when information asymmetry is high, thus penalizing their affiliated funds.[13] As concerns the characteristics of the issue itself, affiliated funds are more likely to receive shares when the size of the issue is larger, when the number of syndicate members and lead managers is greater, and when at least one underwriter's reputation is ranked highly. On the other hand, the gross spread, the percentage of IPOs listed on NASDAQ, the percentage of IPOs issuing only primary shares, and the percentage of issuers backed by venture capitalists are all significantly lower for IPOs allocated to affiliated funds. The positive relation between affiliated allocations and the number of lead managers and syndicate members is not surprising. The larger the syndicate, the more likely it is that more than one member has affiliated funds to which to allocate shares. It is also more likely that the shares can be allocated pursuant to rule 10(f)-3, as they must be allocated through an underwriter other than the affiliated one. Finally, the percentage of shares received by independent funds is greater by about 2.5 percentage points when the issue is allocated to affiliated funds. Since all these characteristics might be significant determinants of underpricing, it is important to control for them in the regressions of section 5.

[Table 5 about here.]

In subsection 3.2 we find that the percentage of IPOs allocated to affiliated funds varies by year. Moreover, we find that the affiliated allocation business is dominated by certain underwriters. It is interesting to investigate whether the practice of favoring affiliated funds with the allocation of underpriced shares, observed for the whole sample, is driven by some subperiods or by a few underwriters. Table 6 shows that this is not the case: the tendency to allocate more underpriced shares to affiliated funds holds in every sub-period (Panel A), and for every underwriter (Panel B), though with some variation in the magnitude and statistical significance of the difference. In Panel (A), we see that affiliated funds were favored the most in 2007: the IPOs in which they received allocations were more underpriced than other IPOs by almost 20 percentage points. The smallest difference in underpricing between IPOs with affiliated allocations ("Allocated" column) and those without affiliated allocations ("Not allocated" column) occurred in 2001, when it was about 6 percentage points and statistically insignificant. For comparison with Ritter and Zhang (2007), it is worth noting that they find the opposite result for 2001: in their sample, underpricing of IPOs allocated to affiliated funds is smaller than it is for other IPOs. This suggests that using the Spectrum 1&2 to proxy for affiliated allocations might not only influence their average size, as pointed out in the previous subsection, but also their variation and correlation with other variables. Panel (B) shows that each of the 14 main underwriters is prone to favoritism. The underwriter that seems to favor its affiliated funds the most is Merrill Lynch: when it allocates shares to its affiliated funds, underpricing is 18 percentage points higher. For Citigroup, by comparison, the difference between IPOs allocated to affiliated investors and other IPOs is only 1 percentage point and statistically insignificant.

[13]Broadly consistent with this argument, we notice that the correlation between the fraction of shares received by independent funds, *IndependentAllocPerc*, and the size of the firm before the issue is -0.1 (untabulated). The correlation between *IndependentAllocPerc* and the age of the firm is -0.06 (untabulated).

[Table 6 about here.]

4 The effect of affiliated allocations on underpricing

In section 2, we posit that underwriters might underprice IPOs in order to increase their affiliated funds profits (Hypothesis 1). In order to test this supernepotism hypothesis and identify a causal link between affiliated allocations and underpricing, we need to find a source of exogenous variation in affiliated allocations.

Rule 10(f)-3 provides the institutional setting we need to the design a quasi-experiment. The rule requires issuers to be at least three years old for the underwriter to be permitted to allocate shares to its affiliated funds. Hence, the probability of allocating some shares to affiliated funds might discontinuously increase at the cutoff point, thus allowing us to implement a fuzzy regression discontinuity design (RDD).[14]

In order to introduce the RDD terminology, we use the following terms interchangeably: $Underpricing$ is the "outcome" variable; our affiliated allocations measures – $AffiliatedAllocPerc$ and $AffiliatedAllocDummy$ – are the "treatment" variables; and Age is the "forcing" (or "running") variable that determines the assignment-to-treatment status through the three year cutoff. We are interested in the causal effect of the treatment on the outcome variable. The fuzzy RDD exploits the discontinuous variation in the treatment status provided by the forcing variable at the three-year cutoff point in order to identify that causal effect.

The RD framework allows us to approximate an ideal experimental setup, where the possibility of allocating shares to underwriter-affiliated funds is randomly assigned, thus helping us overcome the joint endogeneity of affiliated allocations and underpricing. Consider an underwriter who is hired by firms of random ages in order to perform their IPOs. Firms that choose to go public at two years old probably differ, in several dimensions, from those that go public when they are in their twenties. These IPO-specific differences may influence both the allocation and the pricing decisions of the underwriter, thus making it difficult to identify causal effects. If we consider an arbitrarily small neighborhood around the three year cutoff point, however, we can compare firms that differ discontinuously in their treatment status (that is, firms just above and just below the cutoff point), but do not differ discontinuously along other dimensions.

The identification assumption is that only the treatment (the affiliated allocations) changes discontinuously at the cutoff point, while the conditional expectation function of other unobservable and observable factors is continuous. If there is some randomness in the age of the IPO firm around the cutoff, that is, if the underwriter has only imprecise control over the age of the firm at the offer date, then the conditional expectation function of other factors is indeed continuous in the forcing variable (Lee and Lemieux (2010)). We discuss the validity of this identification assumption in section 4.1.

Our identification strategy is illustrated in Figure 3. Consider an underwriter

[14] As observed in section 3, the three year cutoff does not perfectly determine the affiliated allocation decision, neither below nor above the threshold. Hence, a sharp RDD does not fit our setting.

that faces nepotism incentives and which has a profit function such that:[15] *i)* its optimal choice of the offer price, P, as a function of the affiliated allocation, A, is given by the line $P^*(A)$; *ii)* its optimal choice of A, as a function P, is given by the line $A^*(P)$. If the underwriter complies with the 10(f)-3 rule, its affiliated allocations are constrained to zero when the age of the IPO falls just below the cutoff. In this case, the affiliated allocation and the optimal price are given by the pair $(0, P_0)$. When the age of the IPO is just above the cutoff, instead, the underwriter can optimally choose P and A to maximize its profits, that is, it chooses the pair (A_1, P_1). Hence, the cutoff identifies movements along the $P^*(A)$ function, thus allowing us to estimate its slope, that is, to estimate the change in the optimal offer price caused by a change in the allocation to affiliated investors. Since we implement a fuzzy RDD, we estimate a Local Average Treatment Effect (LATE), that is, the effect of affiliated allocations on underpricing for units that comply to the 10(f)-3 rule.

[Figure 3 about here.]

For the purposes of this section, we restrict the sample to eligible IPOs (1,086 observations) and IPOs that are not eligible because they do not meet the age requirement (65 observations), that is, syndicated IPOs issued under a firm-commitment contract whose lead underwriters have been involved in at least one 10(f)-3 transaction in our sample. In this way, we focus the RDD analysis on observations for which the three year cutoff is binding.

The remaining 143 IPOs are not eligible regardless of their age, as they do not meet at least one of the other 10(f)-3 requirements. The cutoff is not binding for them and they are useful for placebo tests only.

4.1 Relevance and exogeneity: graphical analysis and discussion

We follow the RDD literature (Imbens and Lemieux (2008) and Lee and Lemieux (2010)), providing graphical evidence that supports the relevance and exogeneity of the three year threshold.

For the cutoff to be a valid instrument in a fuzzy RDD, it must discontinuously affect the treatment variable. Figure 4 plots the average value of the variables $AffiliatedAllocDummy$ and $AffiliatedAllocPerc$ by one year age groups (bins). Panel (A) shows that the probability of receiving the treatment jumps at the cutoff. The probability that an IPO involves a 10(f)-3 transaction is less than 20% for IPOs below the threshold, but jumps to more than 50% just above the threshold. A similar pattern holds for the average percentage of the issue allocated to affiliated funds (Panel (B)): it is smaller than 0.5% below the cutoff, but jumps to much more than 1% above the cutoff.

[Figure 4 about here.]

If the cutoff affects underpricing through a discontinuous change in affiliated allocations, then we should observe a jump in the outcome variable at the cutoff point

[15] For the sake of simplicity, we rule out dumping-ground incentives for the purposes of this illustration.

(this is known as the intent-to-treat effect). Figure 5 plots the average underpricing by age bins. Underpricing shows a large, clear jump at the cutoff, from about 5% to more than 15%.

[Figure 5 about here.]

The exogeneity of the cutoff is not testable. However, we can check to see if the implications of exogeneity hold in our setting.

In principle, the three year cutoff could be endogenous. Underwriters do have some control over the length of the IPO process, and they might time their IPOs so as to make them eligible for 10(f)-3 transactions. Although appealing, this argument is not supported by empirical evidence. If underwriters were manipulating the length of the IPO process, then we would see a jump or spike in the variable $LengthIPOprocess$ at the cutoff point: three-year-old firms would experience longer IPO processes because of their underwriters' timing strategy. Figure 6, Panel (B), shows this not to be the case. There is no evidence of a jump or spike at the cutoff point.

Digging into the issue of manipulation more deeply, we can see that if manipulation were a concern, then a particular group of IPOs might be subject to it: firms that start their going-public process before they are three years old, but perform the IPO when they become three years old. In our sample, we find only four IPOs for which the length of the process might have been manipulated in order to meet the 10(f)-3 requirement: Vanda Pharmaceuticals Inc., Encore Acquisition Company, Orbitz Inc., and Talecris Biotherapeutics Holdings. Each filed its IPO when it was two years old and completed it when the firm was three years old or older. We notice that:

- the percentage of IPOs that start the process at two years old and complete it at three years old or older is 17% (4 out of 23). For comparison, the percentage of IPOs that start the process when they are already three years old (that is, when they do not have any incentive to manipulate the timing) and complete it when they are more than three years old is 37%.[16] Hence, there is no evidence that underwriters systematically time IPOs.

- two of the four IPOs we identified were clearly not timed for reasons related to nepotism or dumping-ground incentives. Vanda Pharmaceuticals has no affiliated allocations. Encore Acquisition has a relatively small percentage of its issue allocated to underwriter-affiliated funds (0.35%) and its underpricing is positive and low (3.9%).

- two of the four IPOs we identified do raise suspicions that they might have been timed: Orbitz and Talecris Biotherapeutics. A relatively large percentage of their shares has been allocated to affiliated funds (3% and 6.5%, respectively), and the underpricing of each IPO raises concerns that dumping-ground and nepotism incentives might be involved (-3.9% and 11.3%, respectively). However, their IPO processes took 19 and 26 months, respectively. Most likely

[16] One might be concerned about age misclassification. However, we find similar percentages for higher values of age.

they went public when their ages were already several months above the three year cutoff, suggesting that their IPOs were not timed to meet the threshold.

Overall, we conclude that the underwriters' manipulation of the length of the IPO process, if any, is unlikely to be a concern in our setting.

[Figure 6 about here.]

Another possibility, however, is that the underwriter might manipulate the age of the issuer by postponing the filing date and the beginning of the IPO process. This would leave the length of the IPO process unchanged for three-year-old firms, thus preventing us from detecting their manipulation in Figure 6, Panel (B) and invalidating our design. We find this argument not convincing for three reasons. First, underpricing the IPO is not the underwriter's sole objective. Accomplishing the IPO and not missing a window of opportunity most likely dominates underpricing as an objective. This would push the underwriter to not delay the start of the IPO process, as the issuer might turn to a competing underwriter in order to complete its IPO. Thus, competition among underwriters to get deals reduces the scope for manipulation. Second, the RDD setting is invalid only if underwriters can precisely manipulate the assignment variable (Lee and Lemieux (2010)). It is unlikely that an underwriter could do so before starting the IPO process, as the length of the process is a random variable over which the underwriter does not have full control.[17] Third, if underwriters were systematically manipulating the IPO age, then we would observe a jump in the density of the variable *Age* at the cutoff point. Figure 6, Panel (A), shows that this is not the case: there seems to be no jump in the density of *Age* at the three year threshold, suggesting that *Age* manipulation by underwriters is unlikely to be systematic. Figure 7 plots by age bin the number of IPOs underwritten by the most important underwriters: there seems to be no general jump in the number of IPOs underwritten by each underwriter at the cutoff point; only Wells Fargo shows a spike there. Overall, the non-manipulation evidence seems to hold also at the underwriter level.

[Figure 7 about here.]

The identification assumption of the RD design is that the conditional expectation functions of observable and unobservable factors related to the outcome (other than the treatment) are continuous at the cutoff point. We cannot test whether this assumption holds for unobservable factors, but in Figure 8 we plot the average value of the observable covariates by age bins. The figure shows no clear jump in the conditional expectation function of any of the covariates. Interestingly, the main predictor of underpricing – the variable $Adjustment$ – is continuous at the cutoff point. Some variables ($NumberLeadManagers$ and $NumberSyndicateMembers$) show a spike at the three year threshold, but this spike does not seem to be a jump in the conditional expectation function, which might plausibly be continuous. Overall, the expectation functions of the covariates conditional on age do not seem to be discontinuous at the cutoff point.

[17] The random component in the length of the IPO process includes factors that make it not fully predictable, such as the processing capacity of the SEC, indications of interest collected during the bookbuilding process, last minute news, pressures from the firm to complete the IPO, etc.

[Figure 8 about here.]

Another identification concern that we need to address is the following. The goal of the 10(f)-3 rule is to prevent underwriters from dumping unmarketable securities on their affiliated funds. Hence, the regulators might have chosen the three year threshold exactly because IPOs in their early stages of life are more likely to be unmarketable, thus resulting in lower average underpricing. This argument, though plausible, does not in itself affect the RD design, which focuses on the discontinuities at the cutoff point. It suggests, however, that it might be important to control for the underlying relation between underpricing and age in our regressions.

4.2 Local linear IV results

In this subsection, we estimate the effect of underwriter-affiliated allocations on underpricing in a fuzzy RD design.

Let x_i be the age of firm i at the IPO date minus the cutoff level, $x_i = Age_i - 3$, and let z_i be a dummy variable identifying firms that are at least three years old, $z_i = \mathbb{1}(x_i \geq 0)$. We then estimate several specifications of the following local linear IV model, where $Alloc_i$ is one of our two measures of affiliated allocations, $AffiliatedAllocPerc_i$ or $AffiliatedAllocDummy_i$, and $Underpricing_i$ is the first day return:

$$\begin{cases} Underpricing_i = \beta_0 \ + \ \beta_1 Alloc_i \ + \ \beta_2 x_i \ + \ \beta_3 z_i x_i \ + \ e_i & with\ x_i \in [-h, h-1] \\ Alloc_i = \gamma_0 + \ \gamma_1 z_i \ + \ \gamma_2 x_i \ + \ \gamma_3 z_i x_i \ + \ v_i & with\ x_i \in [-h, h-1] \end{cases}$$

Based on the discussion and the graphical evidence presented in our previous subsection, we assume that $\mathbb{E}(e_i|x_i)$ is continuous at the cutoff point. Following Imbens and Lemieux (2008), we estimate the model via 2SLS, using z_i as the instrumental variable for $Alloc_i$, in a neighborhood of the cutoff.

Our setting faces three distinct challenges. First, the forcing variable Age is discrete: we observe it only at the year level. Second, Age is measured with noise: given its definition (see Table 1), some truly n-year old firms might fall into the $n+1$ age bin. This might generate some misclassification around the cutoff. Third, the number of values that the forcing variable can take around the threshold is low: it can only take three distinct values below the cutoff. These three issues affect our choice of the bandwidth and standard errors to use.

Concerning the bandwidth size, h, we face a trade-off that goes beyond the usual one related to the sample size, between bias and variance. If we choose $h = 1$, then we use observations relatively close to the cutoff point, which are more likely to meet the random assignment condition. However, given the discrete nature of our forcing variable, we cannot control for the underlying relation between $Underpricing$ and x. If we choose $h > 1$, for example $h = 3$, then we can control for a local linear relation between the outcome variable and the discrete forcing variable. However, we do so at the cost of using observations relatively far from the cutoff point, which are less likely to meet the random assignment condition.

Concerning standard errors, clustering by the forcing variable is popular in the literature on RDD with discrete running variables (Lee and Card (2008)). However, in a recent paper Kolesàr and Rothe (2017) warn that clustering by the forcing variable can lead to serious over-rejection problems when the number of clusters is low. In particular, they show that clustered standard errors perform worse than robust standard errors. We run simulations (unreported here) and confirm that Kolesàr and Rothe's concerns persist in our particular setting, with its low number of clusters and its misclassification around the cutoff. We find that clustered standard errors face a major over-rejection problem, while robust standard errors seem to be fairly conservative in our setting. However, the power of our test is very low when we choose $h = 2$ or $h = 3$ and control for the underlying relation between underpricing and age.[18]

Based on this discussion, we use robust standard errors and we perform our analysis using three symmetric bandwidth levels ($h = 1$, $h = 2$, and $h = 3$), in order to check the robustness of the results in regards to the particular problems we face. Table 7 reports the results of the local 2SLS estimation for different values of the bandwidth.

[Table 7 about here.]

Consistent with the supernepotism hypothesis, Hypothesis 1, the coefficients of our affiliated allocation variables are positive in all specifications; they are statistically significant at conventional levels in all specifications but one, probably due to a lack of power. Focusing on model (6) of Panel (A), which controls for changes in the underlying relation between the outcome and the forcing variable, we find that a one percentage point increase in the fraction of the issue allocated to affiliated funds increases underpricing by about 5.4 percentage points. Table 7 also reports the first-stage F statistic, which is always bigger than 10, suggesting that the instrument z is not weak.

As a benchmark for judging the size of the LATE effect, we estimate the control complier mean (CCM) (Katz et al. (2001)): the average underpricing of IPOs below the cutoff whose underwriters would have allocated shares to affiliated funds if they had been eligible for 10(f)-3 transactions. First, we use the estimates from the first-stage regression of Table 7, Panel (B), using the $h = 3$ bandwidth:

$$AffiliatedAllocDummy = \hat{\gamma}_0 + \ \hat{\gamma}_1 z_i \ + \ \hat{\gamma}_2 x_i \ + \ \hat{\gamma}_3 z_i x_i \ + \ \hat{v}_i$$

Second, we limit the sample to IPOs that are not allocated to affiliated funds. On the right hand side of the cutoff, we have IPOs that are eligible for 10(f)-3 transactions, but nevertheless are not allocated to affiliated funds (never-takers). On the left hand side of the threshold, we have IPOs that are not eligible for 10(f)-3 transactions and are not allocated to affiliated funds (a mixture of compliers and never-takers). We estimate the reduced-form regression on this subsample, using a bandwidth level of $h = 3$:

$$Underpricing_i = \theta_0 \ + \ \theta_1 z_i \ + \ \theta_2 x_i \ + \ \theta_3 z_i x_i \ + \epsilon_i$$

Letting $\hat{\kappa} = (1 - \hat{\gamma}_0 - \hat{\gamma}_1)/(1 - \hat{\gamma}_0)$ be the percentage of never-takers among IPOs that are not eligible for 10(f)-3 transactions and are not allocated to affiliated

[18]Our simulations show that the power of a two-sided 5% test can be as low as 15%, depending on parameter values.

funds, we estimate the CCM as:

$$CCM = \frac{\hat{\theta}_0 - \hat{\theta}_1 \hat{\kappa}}{1 - \hat{\kappa}}$$

and find $CCM = -6.8$. This result suggests that IPOs whose shares are allocated to affiliated funds because of the 10(f)-3 rule would be on average overpriced by 6.8 percentage points if they were not eligible. By adding the LATE evaluated at the mean value of $AffiliatedAllocPerc$ for complier IPOs, which is equivalent to the coefficient of $AffiliatedAllocDummy$, we find the treated complier mean (TCM): $TCM = CCM + 24.8 = 18$. The 10(f)-3 rule moves the average underpricing of compliers from -6.8% to 18%.

Dong (2015) shows that the conventional fuzzy RDD estimator may be biased when the running variable is discrete and rounded down. However, the bias is equal to zero when the slopes (and higher derivatives) of the outcome and the treatment, as functions of the forcing variable, do not change around the cutoff. We notice that the coefficient for the forcing variable x is weakly significant in only one specification, while the interaction term z_x is not statistically different from zero. Hence, we do not expect this bias to significantly affect our results. (We analyze this issue more in detail in our robustness subsection.)

4.3 Placebo IPOs

If the three year threshold affects underpricing only through affiliated allocations, then we should observe no jumps in the outcome variable when the cutoff is not binding.

Underwriters of non-eligible IPOs (such as non-syndicated IPOs) cannot allocate shares to their affiliated funds, regardless of the age of the issuer. Hence, there should be no jump in underpricing at the cutoff for these non-eligible IPOs. Figure 9 plots the average underpricing by age bins for non-eligible IPOs: we see no evidence of discontinuities at the three year threshold.

[Figure 9 about here.]

The three year threshold is set by the 10(f)-3 rule and is specific to U.S. regulations. Therefore, we should observe no jump in underpricing at the three year cutoff for non-U.S. IPOs. We verify this fact using a SDC sample of 488 European IPOs issued in the period 2001-2013.[19] In Figure 10 we plot their average underpricing by age bins and we find no evidence of discontinuities at the three year threshold.

[Figure 10 about here.]

Following the RDD literature (Imbens and Lemieux (2008)), we check that there are no jumps at non-discontinuity points, that is, where the effect on underpricing should be zero. We define three arbitrary thresholds: the median value of age conditional on $Age > 3$, which is 11 years; the 25th percentile of age conditional on

[19] In addition to the usual filters, we require the founding date to be non-missing in the SDC database. We compute underpricing using the closing prices available in SDC.

$Age > 3$, which is 7 years; and the 75th percentile of age conditional on $Age > 3$, which is 25 years. Figure 11 plots the average underpricing by age bins around these arbitrary thresholds and we see no evidence of discontinuities.

[Figure 11 about here.]

4.4 Robustness checks

Two additional pieces of evidence suggest that the discretization of the forcing variable does not affect our conclusions.

Dong (2015) derives a formula to correct for the bias that arise when the running variable is discrete. Under standard assumptions, the fuzzy RDD local average treatment effect can be expressed as the ratio between the intent-to-treat effect (θ_1) and the coefficient of the first-stage regression of the treatment variable on the assignment-to-treatment variable (γ_1): $\hat{\beta}_{FRD} = \frac{\hat{\theta_1}}{\hat{\gamma_1}}$. Dong shows that this ratio is biased when the forcing variable is discrete and rounded. The direction of the bias depends on the change in the slope (and higher derivatives) of the outcome and the treatment, as functions of the forcing variable, around the cutoff. In order to implement Dong's correction, we need to assume a polynomial relation between underpricing and age. Given the structure of our data, we consider the case of a linear relation only. We estimate via OLS the intent-to-treat equation and the first-stage equation as:

$$Underpricing_i = \theta_0 \ + \ \theta_1 z_i \ + \ \theta_2 x_i \ + \ \theta_3 z_i x_i \ + \epsilon_i$$

$$Alloc_i = \gamma_0 + \ \gamma_1 z_i \ + \ \gamma_2 x_i + \gamma_3 z_i x_i \ + \ v_i$$

Dong's bias-corrected version of $\hat{\beta}_{FRD}$ can be computed as:

$$\hat{\beta}_{FRD} = \frac{\hat{\theta}_1 \ - \ \frac{1}{2}\hat{\theta}_3}{\hat{\gamma}_1 \ - \ \frac{1}{2}\hat{\gamma}_3}$$

Focusing on the $h = 3$ case, we find that the linear correction changes the estimated FRD coefficient of $AffiliatedAllocDummy$ from 24.8 to 27.35. The coefficient of $AffiliatedAllocPerc$ changes from 5.43 to 6.1. The bias, if any, seems to work against finding results, thus suggesting that our results in section 4.2 are conservative.

For a small subsample of 280 IPOs, we know the exact founding date at the *mm/dd/yyyy* level and can compute the precise age of the firm at the issue date; 33 of these IPOs fall within the one-year bandwidth around the cutoff point. Table 8 replicates the fuzzy RDD analysis of section 4.2 for these 33 IPOs.[20] Given their precise age, we can, in principle, control for the underlying relation between underpricing and age within the one-year bandwidth. However, the small sample size might affect the statistical significance of the estimates and the validity of the instrument. Hence, these results should be interpreted very cautiously.

[20]The bandwidth selector proposed by Calonico et al. (2014) would include 29 IPOs with age between 2.1 and 3.9 years. This is very close to the one-year bandwidth that we use for consistency with our baseline analysis.

[Table 8 about here.]

The coefficients of $AffiliatedAllocPerc$ and $AffiliatedAllocDummy$ are always positive in all specifications. We notice that the estimates of model (1) are very similar in magnitude to the results reported in Table 7. The statistical significance is weaker because of the smaller sample size. The results of model (2) and model (3) are qualitatively consistent with section 4.2, but their estimates are statistically insignificant. Moreover, the magnitudes are implausible in some specifications. We acknowledge that the instrument z becomes weak in models (2) and (3), when we introduce x and z_x as control variables in the first-stage regression. Model (3), in particular, suffers from multicollinearity. Nevertheless, Table 8 suggests that the positive effect documented in section 4.2 is unlikely to be driven entirely by the discrete nature of our forcing variable.

Our main treatment variables ($AffiliatedAllocPerc$ and $AffiliatedAllocDummy$) measure allocations to underwriter-affiliated funds without distinguishing the role played by the affiliated underwriter in the syndicate. Hence, Table 7 implicitly assumes that the lead managers set the IPO offer price while acting in the interests of the underwriting syndicate as a whole. If the lead managers act in their own interests, however, they may choose the IPO price to maximize their own profit as a function of the allocations received by their own affiliated funds. For robustness, Table 9 replicates the fuzzy RDD analysis of section 4.2, using as the treatment variable the allocation received by funds affiliated with the lead underwriters only. If anything, our second stage results are stronger. However, we acknowledge that the instrument becomes weak in some specifications of Panel (A), according to the first stage F statistic. The reason is that the percentage of the issue allocated to funds affiliated to the lead underwriters is about as half as the percentage of the issue allocated to affiliated funds as a whole, thus reducing the jump of $AffiliatedAllocPerc$ around the cutoff.

[Table 9 about here.]

5 Nepotism and dumping-ground incentives

We now revisit two hypotheses analyzed in prior work: a milder version of the nepotism hypothesis, and the dumping ground hypothesis. According to the former, underwriters will tend to allocate underpriced shares preferentially to their affiliated funds to boost their performance. According to the latter, underwriters will tend to allocate overpriced shares to their affiliated funds to ensure the success of the IPO. Both these hypotheses have affiliated allocations as the outcome variable. A natural specification would then have a measure of affiliated allocations as the dependent variable, and underpricing as one of the explanatory variables. However, Ritter and Zhang (2007) argue that such a specification could be misleading, as the coefficient of underpricing would capture also the relation between initial IPO returns and allocations to institutional investors as a whole. Building on the empirical model of Aggarwal et al. (2002), they propose to circumvent this issue by regressing underpricing on affiliated allocations, and controlling for independent allocations to capture any private information institutional investors may have. We follow their approach in our analyses.

We first assess which of the two conflicts of interest dominates the IPO market (subsection 5.1). Then we analyze how variation in conflict of interest incentives affects IPO allocations to affiliated funds (subsections 5.2 and following).

5.1 Nepotism or dumping-ground?

In order to assess which type of conflict of interest, nepotism or dumping-ground, is more pervasive in the IPO market, we follow Ritter and Zhang (2007) and estimate several specifications of the following reduced-form model at the IPO level:

$$\begin{aligned} Underpricing = \beta_0 \ + \ \beta_1(Alloc) \ + \ \beta_2(IndependentAllocPerc) \\ + \ \beta_3(Controls) \ + \ \beta_4(indFE) \ + \ \beta_5(yearFE) \ + \ \beta_6(uwFE) \ + \ u \quad (1) \end{aligned}$$

where $Underpricing$ is the first day return and $Alloc$ is either one of our two measures of affiliated allocations: the percentage of the issue allocated to affiliated funds, $AffiliatedAllocPerc$, or a dummy variable identifying IPOs with affiliated allocations, $AffiliatedAllocDummy$. Under the null hypothesis of no conflict of interest, there should be no relation between underpricing and allocations to affiliated funds at the IPO level: $\beta_1 = 0$. The nepotism hypothesis predicts a positive relation between underpricing and affiliated allocations (Hypothesis 2a), $\beta_1 > 0$, while the dumping-ground hypothesis predicts a negative relation between underpricing and affiliated allocations (Hypothesis 2b), $\beta_1 < 0$. Control variables and fixed-effects dummies are described below. We estimate the model via OLS. Since we reject the null hypothesis of homoskedasticity of the error term u, we use robust standard errors for inference.[21] Results are reported in Table 10.

[Table 10 about here.]

Our affiliated allocation measures, $AffiliatedAllocDummy$ and $AffiliatedAllocPerc$, have a positive coefficient in all specifications, providing evidence that the nepotism conflict dominates the dumping-ground conflict. The coefficient estimates are statistically significant either at the 1% or the 5% level. They are also economically significant. If we consider the most conservative estimates, underpricing is 6.28 percentage points higher when underwriter-affiliated funds receive shares in an IPO. Moreover, a one percentage point increase in the fraction of the issue allocated to affiliated funds is associated with a 0.62 percentage point increase in underpricing, meaning that affiliated allocations account for 6.3% of average underpricing.[22]

We control for several factors that might jointly determine underpricing and affiliated allocations. Control variables enter the regression equation with the sign that we expect, often consistent with the existing literature.

Following Ritter and Zhang (2007), we include in all specifications the percentage allocation received by non-affiliated funds, $IndependentAllocPerc$, in order to control for the effect of private information possessed by financial institutions. Consistent with Aggarwal et al. (2002), we find that $IndependentAllocPerc$ is positively

[21] In unreported tables, we also use industry-year clustered standard errors and bootstrapped standard errors, with similar findings.

[22] This number is computed as: β_1*average($AffiliatedAllocPerc$)/average($Underpricing$).

related to underpricing in all regressions and the coefficient estimates are statistically significant at the 1% level. This result is in line with the partial adjustment literature (Hanley (1993)): financial institutions seem to have private information which is not fully incorporated into the offer price during the bookbuilding process. It is also consistent with the conflicts of interest literature, as the positive coefficient might be driven by underwriters favoring some clients with the allocation of underpriced shares (Reuter (2006), Goldstein et al. (2011)). We shed more light on these two potential interpretations in the next subsections.

As expected, $ln(Age+1)$ and $ln(Assets)$ are negatively correlated with underpricing. Consistent with the standard "winner's curse" (Rock (1986)) and bookbuilding (Benveniste and Spindt (1989)) arguments, underpricing is higher when the information asymmetry about the issuer is more pronounced, that is, when the issuer is younger and smaller. However, statistical significance varies across the model specifications: $ln(Age+1)$ is always statistically significant at conventional levels; $ln(Assets)$ becomes insignificant only when other variables highly correlated with it and potentially prone to endogeneity problems, such as the issue size and the syndicate size, are added to the specification. Consistent with the partial adjustment literature, the coefficient for $Adjustment$ is positive and statistically significant at the 1% level in all specifications. The coefficients for $OnlyPrimaryShares$, $Nasdaq$ and $Foreign$, however, are not significantly different from zero.

Columns (3) and (8) introduce additional control variables that might affect both underpricing and allocations. We introduce them in a separate specification because of endogeneity concerns.

Lowry et al. (2017) notice that, since the 1990s, the largest IPOs are frequently the most underpriced. Indeed, we find a positive correlation between $ln(Proceeds)$ and the first day return in most specifications. However, the coefficient is not statistically significant.

Consistent with Lee and Wahal (2004), we find that venture capital backed IPOs have significantly higher underpricing. This positive relation is consistent with the "grandstanding effect" described by Gompers (1996), but it might also be due to endogeneity, as the $VentureCapitalBack$ dummy might capture the effect of firm characteristics positively related to the first day return (Lowry et al. (2017)).

Aggarwal et al. (2002) argue that the IPO process might take longer in times of high issuance volume and high underpricing, thus generating a positive correlation between the first day return and the amount of time spent in registration. In contrast, we find that the length of the IPO process is significantly negatively related to underpricing: an additional month spent in the registration process is associated with a decrease of about 0.3 percentage points in the first day return. This finding is broadly consistent with the intuitive idea that underwriters can price the issue more accurately when they have more time to do it.

The variable $HighRankDummy$, which is a dummy equal to one if at least one underwriter has a Carter-Manaster rank of 9, is positively related to underpricing. Although the statistical significance is weak, this positive relation is broadly consistent with the existing literature (Beatty and Welch (1996), Loughran and Ritter (2004), and Ritter and Zhang (2007)) and might be driven by endogeneity, as riskier and more difficult-to-price issuers, with higher expected underpricing, are more likely to choose highly ranked underwriters to perform their IPOs (Habib and

Ljungqvist (2001) and Lowry et al. (2017)).

The syndicate size and the number of lead underwriters are not significantly related to underpricing.[23] Hence, we do not find evidence in favor of larger syndicates being able to produce more information during the bookbuilding process. However, endogeneity problems work against finding such a result, as issuers whose value is more uncertain and whose expected underpricing is higher are more likely to hire larger syndicates (Lowry et al. (2017)).

Finally, we find a positive relation between the gross underwriting spread and the first day return. This is consistent with the existing literature and with the idea that underpricing and gross spreads are complements: underwriters that can charge high spreads to their customers are also able to leave more money on the table (Kim et al. (2010) and Ritter (2011)).

Columns (4) and (9) introduce year and industry dummy variables, to control for year-specific and industry-specific effects on underpricing and affiliated allocations. In order to define industry dummies, we use SIC codes and we implement the Fama-French 12-industries classification (Fama and French (1997)), as available on Kenneth French's website.[24] In columns (5) and (10), we introduce variables that control for lead underwriters' specific effects on underpricing and affiliated allocations. Controlling for underwriter-specific effects might be important for at least two reasons. First, Hoberg (2007) finds underpricing to have a persistent underwriter-specific component. Second, in section 3 we find that the affiliated allocation business is dominated by a few underwriters and that there is some variation in their propensity to allocate underpriced shares to affiliated funds. Therefore, for each underwriter j, we define the variable $uwFE_{i,j}$ to be equal to 1 if the underwriter is a lead manager of IPO i.[25] It is important to note that these underwriter-specific control variables are not mutually exclusive, as an IPO can have more than one lead manager in its syndicate. Year and industry fixed effects and underwriter-specific controls do not seem to have a major impact on the correlation between underpricing and our affiliated allocation measures.

Overall, we find a positive and statistically significant relation between underpricing and allocations to underwriter-affiliated funds. This evidence is consistent with the nepotism hypothesis: underwriters seem to favor their affiliated funds with the allocation of underpriced shares. This positive correlation persists after controlling for issuer and issue characteristics, year and industry fixed effects, and underwriter-specific control variables. Hence, we find that fund managers' incentives, in the context of IPO allocations, seem to be more in line with those of the fund's shareholders than with those of their affiliated investment bankers. Conversely, the investment bankers' incentives seem to be more in line with those of their affiliated funds than with those of the issuer. Our evidence, based on the actual affiliated allocations reported by investment companies to the SEC, is much

[23] Regression diagnostics raise a weak concern of multicollinearity by introducing $NumberLeadManagers$ as a regressor. For robustness, we run the same regressions excluding it from the independent variables and our results do not sensibly change (not reported).

[24] The link is: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/index.html

[25] In order to define the underwriter-specific variable $uwFE_{i,j}$, we require the underwriter j to be a lead manager at least four times in our dataset. Moreover, we require the underwriter j to be involved in at least one 10(f)-3 transaction in our dataset. These filters allow us to define the variable $uwFE_{i,j}$ for 33 distinct lead underwriters.

clearer than that available in the existing literature.

We stress that the evidence provided in this subsection does not necessarily mean that dumping-ground incentives do not exist or that they are irrelevant. It could be that dumping-ground incentives are simply weaker than nepotism incentives. There are several reasons why the nepotism conflict of interest might stand out. First, it might inherently have a greater weight in the profit function of investment banks, given the structure of the IPO market. Second, the 10(f)-3 rule might be effective in preventing dumping-ground behavior, thus leaving space mainly for nepotism conflicts. Third, the affiliated funds might circumvent the 10(f)-3 rule by buying cold securities in the IPO aftermarket, supporting their price. This would transfer the dumping-ground conflict of interest to the secondary market, allowing us to observe mainly the nepotism conflict in the primary market. In any case, we should observe the dumping-ground conflict in the IPO allocations market whenever the benefits of dumping cold shares to affiliated funds are high enough. We explore this possibility in the next subsections, analyzing how variation in conflict of interest incentives affects the correlation between IPO allocations to affiliated funds and underpricing.

5.2 Conflict of interest incentives and the number of IPOs

Hypothesis 3 states that dumping-ground incentives are stronger when the underwriter is completing a relatively low number of deals. To test this idea, we measure the abnormal number of deals completed by each underwriter at the time the IPO in question and check whether the correlation between underpricing and affiliated allocations varies consistently with conflict of interest incentives.

For each IPO, we measure the abnormal number of IPOs completed by its underwriters as follows. Take IPO i performed in quarter q by underwriter j. We require that each underwriter j has been involved in at least one 10(f)-3 transaction in our sample. First, we define $F_{i,j,q-t}$ to be the number of IPOs filed by the underwriter j of IPO i in the quarter $q-t$. We compute $F_{i,j,q-1}$ and use it as a proxy for the number of deals that underwriter j expects to complete in quarter q. Then, we compute a benchmark measure as the average number of IPOs filed by underwriter j from quarter $q-6$ to quarter $q-3$ before the IPO i as:[26]

$$\overline{F}_{i,j} = \frac{1}{4}\sum_{t=3}^{6} F_{i,j,q-t}$$

Using this benchmark, we measure the abnormal number of IPOs that underwriter j expects to complete in quarter q as:

$$AF_{i,j} = F_{i,j,q-1} - \overline{F}_{i,j}$$

Finally, as IPO i may have more than one underwriter, we compute an aggregate measure of abnormal number of IPOs underwritten by the underwriters of IPO i as:

$$\overline{AF}_i = \frac{1}{J_i}\sum_{j=1}^{J_i} AF_{i,j}$$

[26] To compute the benchmark measure for IPOs performed in 2001 and 2002, we download additional IPO data for the period 1999-2000 from the SDC database.

where J_i is the number of underwriters of IPO i that satisfy the filter of being involved in at least one 10(f)-3 transaction in our sample.

We split the sample into terciles based on $\overline{AF}_i$. The top (bottom) tercile contains IPOs whose underwriters expect to complete a high (low) abnormal number of deals in the quarter of the IPO in question. Hypothesis 3 states that nepotism incentives dominate dumping-ground incentives in the highest tercile, while dumping-ground incentives gain importance relative to nepotism incentives in the lowest tercile. We estimate model 1 in the subsample of IPOs in the highest and lowest terciles of the variable $\overline{AF}_i$ and report the OLS regression results in Table 11. Under Hypothesis 3, we expect the coefficient β_1 to be higher in the top tercile.

[Table 11 about here.]

Consistent with Hypothesis 3, we find that the coefficient of $AffiliatedAllocPerc$ is positive and statistically significant in the highest tercile. In the lowest tercile, instead, the coefficient is much smaller in magnitude (and even negative in one specification) and is not statistically significant.

We notice that a similar qualitative pattern holds for independent funds, suggesting that unaffiliated funds are favored the most when the underwriter's need to complete deals is weakest. Changes in the magnitude and statistical significance of the coefficient of *IndependentAllocPerc*, however, are not as pronounced as they are for affiliated funds.

Even though the difference between the coefficients in the bottom and top terciles is not significant at conventional levels, we nevertheless notice that the nepotism conflict observed for the whole sample is enhanced by the highest tercile, while it is weakened by the lowest tercile. Overall, this evidence is consistent with conflict of interest incentives. When the underwriter expects to complete an abnormally low number of deals, the benefits of completing an additional IPO gain importance. This increases the incentive for dumping cold IPOs to affiliated funds, thus lowering the correlation between underpricing and affiliated allocations.

5.3 Conflict of interest incentives and commission kickbacks

Hypothesis 4a states that the correlation between underpricing and affiliated allocations should be weaker when the underwriter receives a high stream of commissions from institutional investors. Hypothesis 4b states that the correlation between underpricing and allocations to independent funds should be stronger when the underwriter receives a high stream of commissions from institutional investors.

We follow Goldstein et al. (2011) in measuring the abnormal commissions received by the brokerage arm of the lead underwriters around the IPOs' issue dates. We use the Abel Noser Solutions database to gather trade-level brokerage commission data for the period October 2000 to March 2011. We match Abel Noser's brokers to SDC's underwriters by name and require IPOs to have at least one lead underwriter matched to the Abel Noser Solutions database. Hence, for the purposes of this subsection, we drop from our sample IPOs performed in the period 2011-2013, as well as non-matched IPOs. These filters leave us with 735 IPOs in the period 2001-2010. For each IPO, we collect all trades in non-IPO stocks executed by its lead underwriters in a time window of [-60,+60] trading days around the IPO issue date and aggregate commission revenues at the daily level. We let $C_{i,j,t}$ be the dollar amount of brokerage commissions received by the lead underwriter j of IPO i in the trading day t relative to the offer date. First, we compute a benchmark level of brokerage commissions received by the lead underwriter j of IPO i as the average daily commission revenues in the non-event period [-60,-21] and [+21,+60], using this equation:

$$\overline{C}_{i,j} = \frac{1}{80}(\sum_{t=-60}^{-21} C_{i,j,t} + \sum_{t=21}^{60} C_{i,j,t})$$

Then we compute the average abnormal commission revenue in the event period [-10,-1] as:[27]

$$AC_{i,j} = \frac{1}{10}(\sum_{t=-10}^{-1} C_{i,j,t} - \overline{C}_{i,j})$$

Finally, as IPO i may have more than one lead manager, we compute an aggregate measure of abnormal brokerage commissions received by its underwriters as:

$$\overline{AC}_i = \sum_{j=1}^{J_i} AC_{i,j}$$

where J_i is the number of lead underwriters of IPO i matched to Abel Noser Solutions' brokers.

We split the sample into terciles based on $\overline{AC}_i$. The top (bottom) tercile contains IPOs whose underwriters received a high (low) abnormal stream of brokerage commissions from institutional trading in non-IPO stocks in the 10-day window before the IPO in question. We estimate model 1 in these two subsamples of IPOs and report our OLS regression results in Table 12. Under Hypotheses 3a and 3b, we expect the coefficient β_1 to be higher in the bottom tercile and the coefficient β_2 to be higher in the top tercile.

[Table 12 about here.]

Consistent with Hypothesis 4a, we observe that the coefficient of $AffiliatedAllocPerc$ is lower in magnitude when the lead underwriters receive an abnormally high stream of brokerage commissions from institutional investors. Statistical significance is also weaker in the highest tercile of $\overline{AC}_i$. Consistent with Hypothesis 4b, the coefficient of $IndependentAllocPerc$ is higher when quid-pro-quo incentives are likely at play. Moreover, the coefficient is not statistically different from zero when institutional investors do not pay high brokerage commissions to the lead underwriters. This finding provides additional evidence of the importance of commission paybacks in the IPO allocation process, supporting Reuter (2006), Nimalendran et al. (2007), Goldstein et al. (2011), and Jenkinson et al. (2017).

Even though the differences between the coefficients in the bottom and top terciles are not significant at conventional levels, we nevertheless notice that the nepotism conflict (the commission-kickbacks conflict) observed for the whole sample is enhanced (weakened) by the lowest tercile. Overall, this evidence is consistent

[27] The abnormal commission revenue in the event period is positive on average and statistically different from zero (result not reported).

with underwriters' conflict of interest incentives. When brokerage commissions gain weight in the profit function of the investment bank, the revenues from allocating underpriced shares to the affiliated investment management arm become less important and the underwriter tends to favor non-affiliated institutions that have entered into a quid-pro-quo agreement.

5.4 Conflict of interest incentives and information asymmetry

Hypothesis 5a states that the correlation between underpricing and affiliated allocations should be stronger for firms with low information asymmetry. Hypothesis 5b states that the correlation between underpricing and unaffiliated, independent allocations should be stronger for firms with high information asymmetry.

As our proxy for information asymmetry we use the size of the firm, $ln(Assets)$, and split the sample into terciles based on firm size. We estimate model 1 in the highest and lowest terciles and report our OLS regression results in Table 13. Under Hypotheses 5a and 5b, we expect the coefficient β_1 to be higher in the top tercile and the coefficient β_2 to be higher in the bottom tercile.

[Table 13 about here.]

Consistent with Hypothesis 5a, we observe that the coefficient of $AffiliatedAllocPerc$ is positive and statistically significant in the highest tercile, while it is statistically not different from zero in the lowest tercile. Moreover, in two specifications, the sign of the coefficient becomes negative. There is some evidence in favor of Hypothesis 5b as well, though it is weaker: the magnitude and statistical significance of the coefficient of $IndependentAllocPerc$ are higher in the lowest tercile of $ln(Assets)$.

Even though the difference between the coefficients in the bottom and top terciles is not significant at conventional levels, we nevertheless notice that the nepotism conflict observed for the whole sample is driven by the highest tercile, while it is weakened by the lowest tercile. Overall, this evidence is consistent with underwriters' conflict of interest incentives and with standard information production theories of bookbuilding. When information asymmetry is high, the underwriter tends to favor those investors whose indications of interest in the bookbuilding process are more valuable. When information asymmetry is low, price discovery is less important and the nepotism conflict emerges.

6 Conclusion

We argue that nepotism incentives might not only influence the allocation decision, as prior research has suggested, but also the pricing decision, which has not before been suspected. We hypothesize that underwriters might abnormally underprice IPOs to benefit their affiliated funds (our "supernepotism" hypothesis).

We exploit the 10(f)-3 rule of the Investment Company Act, and we construct a novel hand-collect dataset containing IPO allocations received by funds affiliated to the underwriters. To test our idea, and to assess the causal effect of affiliated allocations on the IPO offer price, we implement a fuzzy regression discontinuity design. We exploit a regulatory threshold, set by section 10(f)-3 of the Investment

Company Act, that provides exogenous variation in the allocation decision. We find that a one percentage point increase in the allocations to affiliated funds causes underpricing to be nearly 5.4 percentage points higher. Our evidence suggests that the supernepotism conflict of interest might have large consequences and costs for the issuing firm.

Our hand-collected dataset of affiliated IPO allocations also enables us to revisit the dumping-ground and nepotism hypotheses analyzed in the prior literature. We find that, controlling for other joint determinants, there is a strong and statistically significant positive correlation between underpricing and affiliated allocations: a one percentage point increase in the allocation to affiliated funds is associated with a 0.62 percentage point increase in underpricing. This evidence suggests that the nepotism conflict is more pervasive than the dumping-ground one. Our evidence supporting the nepotism hypothesis is much clearer than that reported in previous papers.

We also investigate whether the correlation between affiliated allocations and underpricing varies consistently with the nepotism and dumping-ground incentives. We find that the positive correlation between affiliated allocations and underpricing is weaker in periods when the underwriter performs an abnormally low number of IPOs. This result is consistent with the idea that, in such periods, dumping-ground incentives gain importance relative to those of nepotism, as the marginal benefit of completing an IPO is higher for the underwriter. Moreover, we find that the positive correlation between affiliated allocations and underpricing is weaker when the investment bank underwriting the IPO receives an abnormally high stream of brokerage commissions from other non-affiliated funds. In this scenario, underwriters tend to favor the clients that give them commission kickbacks, and nepotism incentives become less important. Finally, we find some evidence consistent with both information-based bookbuilding theories and conflict of interest incentives. The positive correlation between affiliated allocations and underpricing is stronger when the information asymmetry about the issuer is lower. In these IPOs, the information providing role of the bookbuilding method is not as important as it is for IPOs whose value is more uncertain. Hence, underwriters do not need to reward independent funds for their information-revealing indications of interest and the nepotism conflict emerges.

One interesting question that remains unanswered is why the nepotism conflict dominates the dumping-ground one in the context of IPO allocations. We argue that there are several reasons why the nepotism conflict might stand out. First, it might inherently have a greater weight in the profit function of investment banks, given the structure of the IPO market. Second, the 10(f)-3 rule might be an effective tool preventing dumping-ground behavior, thus leaving space mainly for nepotism conflicts. Third, affiliated funds might circumvent the 10(f)-3 rule by buying cold securities in the IPO aftermarket, supporting their price. This would transfer the dumping-ground conflict to the secondary market, allowing us to observe mainly the nepotism conflict in the primary market. We plan to explore this third possibility in an extension of the present paper.

Our findings shed light on a previously unexplored tradeoff facing IPO issuers. For them, the benefits of going public must be compared with the potential foregone IPO proceeds stemming from supernepotism behavior on the part of the IPO underwriter. Our conversations with asset managers suggest to us that the supernepotism behavior we document is known to at least some participants in the IPO market. It is not clear to us whether this behavior is widely known to potential IPO issuers. Conceivably, an IPO issuer concerned about supernepotism could turn to an underwriter less active in the fund management business, but we have no indication, even anecdotal, that this is the case. An intriguing possibility is that issuers may view the underwriter's dumping ground incentives as an offsetting virtue to nepotism: an issuer might accept the risk of foregone proceeds due to supernepotism, if that risk comes bundled with the guarantee that the underwriter will use his own funds to place the issuer's shares and guarantee a successful offering when market conditions deteriorate.

Overall, using a novel dataset that allows us to directly measure affiliated allocations, we find that underwriters favor their affiliated funds when allocating underpriced IPOs. Our results contribute to the existing literature by shedding light on the types of conflicts of interest that affect the IPO process and their consequences for both issuing firms and fund shareholders.

A Appendix: downloading and parsing N-SAR filings

The 77o item of the N-SAR filing asks the filer whether it was involved in affiliated transactions pursuant to the 10(f)-3 rule. If the answer is yes, then the filer has to provide additional information about the affiliated transaction in an attachment. We download from the SEC EDGAR database the 104,207 N-SAR forms filed in the period January 2001 to December 2014. This time span covers the affiliated transactions executed in the period 2001-2013, because an N-SAR form filed in year X can contain information about year X-1. Since 2001, institutions are instructed to name their attachment type: "EX-99.77O 10f-3 RULE." However, a non-negligible number of attachments is filed with a wrong or incomplete name. Hence, we do not rely only on that tag to find the attachments we are interested in. We focus on the N-SAR filings that satisfy at least one of the following (case insensitive) criteria:

- contain in the main form or in any attachment the string "077 O000000 Y";
- contain in the main form or in any attachment the string "10f";
- contain in the main form or in any attachment the string "77o."

Using these criteria, we keep many false positives that do not contain a 10(f)-3 attachment. Our objective is to minimize false negatives, so as to lose the smallest possible amount of information.[28] These criteria leave us with 10,622 N-SAR filings. We parse them manually because the reporting format differs considerably, both between and within investment companies. Figure 12 provides an example of a 10(f)-3 attachment to the N-SAR filings.

[Figure 12 about here.]

[28]Under these criteria, false negatives are N-SAR filings that contain a 10f-3 attachment, but: i) mistakenly answer "NO" to the 77o item, and ii) do not contain the terms "10f" or "77o" in the entire N-SAR document and its attachments.

10(f)-3 attachments report information about both equity and bond issues. We hand-collect information about equity issues only. Sometimes the filings explicitly distinguish the two categories; most of the time, however, we have to infer the kind of security issued. For bond issues, filings often report the maturity date or the yield to maturity; the name of the fund receiving an allocation often reveals whether it is a bond/municipal fund or an equity fund; the reported offer price is typically close to 100 for bond issues; etc. When no such information is provided and we are unable to distinguish equity from bond issues, we store the observation in our dataset in order to minimize false negatives.[29] In this way, we collect 18,872 observations at the issue-"investor"-broker level, meaning that we observe the number of shares allocated to investor f in IPO i by broker b. The "investor" can be a fund, a sub-portfolio of a fund, or an investment management company.

We match 10(f)-3 issuers to SDC issuers mainly by using issuer names and issue dates. We complement the matching with other pieces of information (such as the offer price and the number of shares issued) to increase the accuracy of the match. Moreover, we match 10(f)-3 underwriters to SDC underwriters by name, taking into account name changes and M&A activities. The matching with SDC allows us to disentangle IPOs and SEOs and to focus on IPOs that satisfy the usual filters applied in the literature. This leaves us with 8,828 IPO-investor-broker observations.

We identify and exclude duplicates. Duplicates arise when distinct N-SAR forms report the same information about fund f receiving n shares in the IPO i from broker b. This happens, for example, when an investment company reports the same information both in the annual and semi-annual N-SAR filings (both NSAR-B and NSAR-A).

Some 10(f)-3 attachments contain missing values. For example the amount of shares allocated to affiliated funds is missing for about 5% of the observations, before any cleaning. We use information from other filings to fill in some of these missing values. For example, if the individual number of shares n of IPO i allocated to the fund f affiliated to underwriter j is missing in a filing, but we observe the total number of shares W allocated to the adviser of fund f, then, if other filings report the individual number of shares m received by other funds with the same adviser, we can find out n as: $n = W - m$. In this way, we reduce the percentage of observations with missing allocations to about 1.5%. This implies that we slightly underestimate the total percentage of shares allocated to affiliated funds at the IPO level ($AffiliatedAllocPerc$). The allocation dummy ($AffiliatedAllocDummy$), however, is not affected by this problem.

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Table 1. List of all variables

Variable	Description
IPO VARIABLES	
Underpricing	(first day closing price - offer price)*100/offer price
Age	age of the issuer in years computed as: issue year - founding year
Proceeds	total proceeds from the issue in millions of dollars
Assets	total assets before the IPO in millions of dollars
Adjustment	(offer price - midpoint)*100/midpoint, where "midpoint" is the original midpoint of the filing range
OnlyPrimaryShares	dummy variable equal to one if all the shares issued are primary shares
Nasdaq	dummy variable equal to one if the IPO is listed on the NASDAQ
Foreign	dummy variable equal to one if the issuer is located outside the United States
VentureCapitalBack	dummy variable equal to one if the IPO is backed by a venture capitalist
LengthIPOprocess	length of the IPO process in months computed as: (issue date - filing date)/30.4375
HighRankDummy	dummy variable equal to one if at least one underwriter has a Ritter ranking equal to 9
NumberLeadManagers	number of bookrunners and lead managers in the syndicate
NumberSyndicateMembers	total number of syndicate members
GrossSpread	gross underwriters' spread
FirmCommitment	dummy variable equal to one if the securities are issued under a firm-commitment contract
ALLOCATION VARIABLES	
AffiliatedAllocPerc	percentage of the issue allocated to affiliated funds
AffiliatedAllocDummy	dummy variable equal to one if affiliated funds receive shares in the IPO
IndependentAllocPerc	percentage of the issue held by mutual funds at the first reporting date after the IPO minus AffiliatedAllocPerc
OTHER VARIABLES	
$\overline{AF}$	Abnormal number of deals that the lead managers expect to complete in the quarter of the IPO
$\overline{AC}$	Abnormal stream of brokerage commissions to the lead underwriters in a 10-day window before the IPO

Table 2. This table provides summary statistics at the issuer level for 1,086 eligible IPOs (Panel A) and 208 non-eligible IPOs (Panel B). We define an IPO as "eligible" if it satisfies these conditions: the issuer is at least three years old; the securities are issued under a firm-commitment contract; there is more than one underwriter in the syndicate; at least one lead underwriter has been involved in a 10(f)-3 transaction in our sample. IPO variables are defined in Table 1. For each variable, the table reports its average (mean), its median (p50), and its standard deviation (sd).

(A) Eligible IPOs

	mean	p50	sd
Underpricing	14.2	9.09	19.4
Age	22.9	11	27.7
Proceeds	219.1	117.3	266.2
Assets	1351.2	217.6	2372.7
Adjustment	-1.59	0	13.3
GrossSpread	6.63	7	0.73
NumberLeadManagers	2.38	2	1.47
NumberSyndicateMembers	7.51	6	4.59
LengthIPOprocess	4.41	3.37	3.57
OnlyPrimaryShares	0.52	1	0.50
Nasdaq	0.61	1	0.49
Foreign	0.097	0	0.30
VentureCapitalBack	0.45	0	0.50
HighRankDummy	0.78	1	0.41

(B) Non-eligible IPOs

	mean	p50	sd
Underpricing	5.13	1.16	13.9
Age	11.1	5	22.5
Proceeds	86.7	48.2	112.3
Assets	1122.7	51.3	2455.2
Adjustment	-4.49	0	11.2
GrossSpread	6.93	7	0.66
NumberLeadManagers	1.69	1	1.13
NumberSyndicateMembers	4.80	4	3.34
LengthIPOprocess	4.39	3.60	3.39
OnlyPrimaryShares	0.79	1	0.41
Nasdaq	0.75	1	0.43
Foreign	0.21	0	0.41
VentureCapitalBack	0.31	0	0.46
HighRankDummy	0.25	0	0.44

Table 3. This table summarizes the allocation data at the issuer level for 1,086 eligible IPOs (Panel A) and 208 non-eligible IPOs (Panel B). *AffiliatedAllocPerc* is the percentage allocated to funds affiliated with the underwriters; *AffiliatedAllocDummy* is a dummy variable identifying IPOs with at least one share allocated to affiliated funds; and *IndependentAllocPerc* is the percentage allocated to funds that are not affiliated with the underwriters.

(A) Eligible IPOs

	mean	p50	sd
AffiliatedAllocPerc	1.44	0.12	2.36
AffiliatedAllocDummy	0.56	1	0.50
IndependentAllocPerc	18.3	16.1	13.3

(B) Non-eligible IPOs

	mean	p50	sd
AffiliatedAllocPerc	0.077	0	0.68
AffiliatedAllocDummy	0.082	0	0.27
IndependentAllocPerc	10.1	5.73	12.0

Table 4. List of the underwriters that are more active in the affiliated allocations market. The table reports the number of eligible IPOs underwritten by each underwriter and the number and percentage of IPOs in which each underwriter has allocated some shares to its affiliated funds.

Underwriter	IPOs underwritten	IPOs allocated	%
JP Morgan (JPM)	390	230	59.0%
Morgan Stanley & Co	307	116	37.8%
Merrill Lynch & Co Inc	234	112	47.9%
Goldman Sachs & Co	321	81	25.2%
Banc of America Securities LLC	196	78	39.8%
Wells Fargo	118	69	58.5%
Deutsche Bank Securities Corp	276	69	25.0%
Jefferies & Co Inc	182	60	33.0%
UBS Investment Bank	262	53	20.2%
Raymond James & Associates Inc	149	50	33.6%
Citigroup	226	43	19.0%
Needham & Co Inc	98	38	38.8%
Credit Suisse First Boston	352	32	9.1%
Wachovia Securities Inc	118	25	21.2%
Other 50 underwriters (average)	50.1	4.6	9.1%

Table 5. Difference-of-means tests with unequal variances (Panel A) and difference-of-proportions tests (Panel B) of IPO characteristics by affiliated allocation dummy. The sample includes 1,086 eligible IPOs, 611 of which have some allocation to affiliated funds (i.e., they are "Allocated"). IPO characteristics are defined in Table 1; *IndependentAllocPerc* is the percentage of the issue allocated to funds not affiliated with the underwriters. Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

(A) Difference-of-means tests

	Allocated	Not Allocated	diff.	t-stat
Underpricing	19.4	7.61	11.8***	10.9
Proceeds	302.0	112.5	189.5***	13.6
Assets	1661.0	952.6	708.4***	5.04
Adjustment	3.08	-7.59	10.7***	14.3
GrossSpread	6.49	6.82	-0.32***	-7.78
Age	26.0	18.9	7.17***	4.43
NumberLeadManagers	2.72	1.94	0.78***	9.48
NumberSyndicateMembers	8.80	5.86	2.94***	11.6
LengthIPOprocess	4.44	4.37	0.066	0.30
IndependentAllocPerc	19.4	16.9	2.49***	3.08

(B) Difference-of-proportions tests

	Allocated	Not allocated	diff.	z-stat
OnlyPrimaryShares	0.42	0.64	-0.22***	-7.44
Nasdaq	0.47	0.79	-0.33***	-11.9
Foreign	0.092	0.10	-0.012	-0.63
VentureCapitalBack	0.40	0.51	-0.11***	-3.47
HighRankDummy	0.92	0.60	0.33***	13.1

Table 6. This table reports the results of the difference-of-means tests with unequal variances of underpricing by the affiliated allocation dummy in different sub-periods (Panel A) and for different underwriters (Panel B). In panel (A), the sample includes 1,086 eligible IPOs, 611 of which have some allocation to affiliated funds. In Panel (B), the sample includes the IPOs underwritten by each of the 14 main underwriters (see Table 4), and the affiliated allocation dummy is defined at the IPO-underwriter level. Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

(A) Difference of mean underpricing by sub-periods

	Allocated	Not allocated	diff.	t-stat
2001	17.0	10.9	6.11	1.48
2002	12.0	5.25	6.71	1.48
2003	16.7	9.52	7.18^{*}	1.74
2004	20.5	5.73	14.8^{***}	5.51
2005	14.8	7.08	7.73^{***}	2.67
2006	19.6	5.97	13.6^{***}	4.29
2007	25.8	6.46	19.4^{***}	5.26
2008	16.4	2.64	13.8	1.27
2009	14.8	0.69	14.1^{***}	3.39
2010	13.6	4.24	9.36^{***}	2.74
2011	20.1	10.7	9.38^{*}	1.77
2012	20.8	13.5	7.22	1.34
2013	26.4	14.5	11.9^{***}	2.86

(B) Difference of mean underpricing by underwriters

	Allocated	Not allocated	diff.	t-stat
JP Morgan (JPM)	17.8	6.12	11.7^{***}	6.53
Morgan Stanley & Co	24.0	11.4	12.6^{***}	4.93
Merrill Lynch & Co Inc	24.2	6.23	18.0^{***}	7.68
Goldman Sachs & Co	25.2	13.8	11.3^{***}	3.93
Banc of America Securities LLC	19.6	7.02	12.6^{***}	4.97
Deutsche Bank Securities Corp	24.0	8.29	15.7^{***}	5.47
Wells Fargo	15.6	8.24	7.37^{**}	2.30
Jefferies & Co Inc	20.0	10.6	9.42^{***}	2.97
UBS Investment Bank	24.2	10.4	13.8^{***}	4.19
Raymond James & Associates Inc	20.2	10.8	9.43^{***}	2.79
Citigroup	13.0	11.9	1.02	0.38
Needham & Co Inc	27.1	11.1	16.0^{***}	3.50
Credit Suisse First Boston	17.8	12.4	5.37^{*}	1.71
Wachovia Securities Inc	24.0	11.6	12.4^{**}	2.33

Table 7. This table contains the second stage coefficients of a local 2SLS regression of *Underpricing* on two measures of affiliated allocations instrumented by z, for different values of the bandwidth h. The two measures are $AffiliatedAllocPerc$ (Panel A) and $AffiliatedAllocDummy$ (Panel B). z is a dummy variable equal to one if $Age \geq 3$ and zero otherwise, $x = Age - 3$, and $z\_x = z \cdot x$. Relevant statistics from the first stage regression (F, coefficient of z, t-stat of z, and R^2) are also reported. All percentages and returns are multiplied by 100. All non-dummy variables except Age are winsorized at the 95% level. Heteroschedasticity-robust t-statistics are in parentheses. Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

(A)

	(1) h=1	(2) h=2	(3) h=2	(4) h=3	(5) h=3	(6) h=3
AffiliatedAllocPerc	6.72**	8.76***	5.28	10.4***	6.55*	5.43*
	(2.22)	(3.12)	(1.29)	(3.59)	(1.74)	(1.90)
x			2.17		1.40	2.67*
			(0.79)		(1.02)	(1.67)
z_x						-2.16
						(-0.70)
Constant	4.47***	3.73*	7.15*	1.49	5.01	7.64***
	(2.67)	(1.90)	(1.76)	(0.58)	(1.48)	(2.67)
F (2nd stage)	4.93	9.76	6.47	12.9	9.76	7.23
F (1st stage)	10.0	24.6	12.2	23.0	12.8	14.4
Coefficient of z (1st stage)	1.53	1.28	1.79	1.13	1.59	1.64
t-stat of z (1st stage)	3.16	4.96	2.18	4.79	2.68	3.30
R^2 (1st stage)	0.14	0.097	0.10	0.064	0.067	0.067
Observations	57	130	130	217	217	217

(B)

	(1) h=1	(2) h=2	(3) h=2	(4) h=3	(5) h=3	(6) h=3
AffiliatedAllocDummy	24.6**	28.5***	21.1	27.4***	29.0**	24.8**
	(2.66)	(3.62)	(1.47)	(5.12)	(2.00)	(2.17)
x			1.42		-0.22	1.09
			(0.48)		(-0.12)	(0.68)
z_x						-1.83
						(-0.73)
Constant	1.72	0.91	3.88	0.51	-0.097	2.87
	(0.74)	(0.33)	(0.69)	(0.24)	(-0.02)	(0.69)
F (2nd stage)	7.05	13.1	7.82	26.3	12.7	9.11
F (1st stage)	13.1	28.0	13.9	55.6	28.2	18.9
Coefficient of z (1st stage)	0.42	0.39	0.45	0.43	0.36	0.36
t-stat of z (1st stage)	3.63	5.29	2.41	7.46	2.62	2.71
R^2 (1st stage)	0.19	0.15	0.15	0.16	0.16	0.16
Observations	57	130	130	217	217	217

Table 8. This table contains the second stage coefficients of a local 2SLS regression of *Underpricing* on two measures of affiliated allocations instrumented by z, for a bandwidth $h = 1$, in a subsample of 33 IPOs whose exact age is known. The two measures are *AffiliatedAllocPerc* (Panel A) and *AffiliatedAllocDummy* (Panel B). z is a dummy variable equal to one if $Age \geq 3$ and zero otherwise, $x = Age - 3$, and $z\_x = z \cdot x$. Relevant statistics from the first stage regression (F, coefficient of z, t-stat of z, and R^2) are also reported. All percentages and returns are multiplied by 100. All non-dummy variables except *Age* are winsorized at the 95% level. Heteroschedasticity-robust t-statistics are in parentheses. Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

(A)

	(1)	(2)	(3)
AffiliatedAllocPerc	5.44*	3.63	3.63
	(1.72)	(0.81)	(0.68)
x		3.87	3.85
		(0.47)	(0.21)
z_x			0.027
			(0.00)
Constant	7.65**	9.81*	9.80
	(2.05)	(1.78)	(0.77)
F (2nd stage)	2.96	1.66	1.46
F (1st stage)	10.8	6.46	4.97
Coefficient of z (1st stage)	2.08	4.03	2.98
t-stat of z (1st stage)	3.29	1.76	2.05
R^2 (1st stage)	0.15	0.20	0.21
Observations	33	33	33

(B)

	(1)	(2)	(3)
AffiliatedAllocDummy	24.9*	38.2	43.0
	(1.97)	(0.90)	(0.68)
x		-6.18	-10.4
		(-0.36)	(-0.29)
z_x			4.41
			(0.13)
Constant	5.70	1.20	-1.50
	(1.30)	(0.08)	(-0.05)
F (2nd stage)	3.88	1.48	1.35
F (1st stage)	10.3	5.18	7.21
Coefficient of z (1st stage)	0.45	0.38	0.25
t-stat of z (1st stage)	3.21	1.14	0.73
R^2 (1st stage)	0.19	0.19	0.20
Observations	33	33	33

Table 9. This table contains the second stage coefficients of a local 2SLS regression of *Underpricing* on two measures of lead managers' affiliated allocations instrumented by z, for different values of the bandwidth h. The two measures are $AffiliatedAllocPerc$ (Panel A) and $AffiliatedAllocDummy$ (Panel B). z is a dummy variable equal to one if $Age \geq 3$ and zero otherwise, $x = Age - 3$, and $z\_x = z \cdot x$. Relevant statistics from the first stage regression (F, coefficient of z, t-stat of z, and R^2) are also reported. All percentages and returns are multiplied by 100. All non-dummy variables except Age are winsorized at the 95% level. Heteroschedasticity-robust t-statistics are in parentheses. Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

(A)

	(1) h=1	(2) h=2	(3) h=2	(4) h=3	(5) h=3	(6) h=3
AffiliatedAllocPerc	10.9**	15.3***	8.11	20.1***	9.94*	8.53*
	(2.17)	(2.80)	(1.31)	(2.90)	(1.73)	(1.83)
x			2.56		1.91	2.88*
			(1.00)		(1.63)	(1.70)
z_x						-1.79
						(-0.57)
Constant	4.63***	3.74*	7.77**	0.85	5.96**	8.01***
	(2.67)	(1.87)	(2.09)	(0.25)	(2.06)	(2.72)
F (2nd stage)	4.69	7.84	6.28	8.41	9.17	6.81
F (1st stage)	7.18	14.9	7.42	11.6	7.14	8.29
Coefficient of z (1st stage)	0.95	0.74	1.17	0.58	1.05	1.04
t-stat of z (1st stage)	2.68	3.86	1.94	3.41	2.42	2.87
R^2 (1st stage)	0.11	0.061	0.066	0.034	0.040	0.040
Observations	57	130	130	217	217	217

(B)

	(1) h=1	(2) h=2	(3) h=2	(4) h=3	(5) h=3	(6) h=3
AffiliatedAllocDummy	28.9***	35.3***	23.7	37.3***	30.2**	27.3**
	(2.72)	(3.59)	(1.54)	(4.68)	(2.07)	(2.17)
x			1.80		0.72	1.49
			(0.69)		(0.51)	(0.95)
z_x						-1.21
						(-0.47)
Constant	2.15	1.56	5.11	0.44	2.44	4.15
	(0.92)	(0.59)	(1.10)	(0.19)	(0.59)	(1.09)
F (2nd stage)	7.39	12.9	8.25	21.9	12.7	8.90
F (1st stage)	10.2	21.6	10.7	34.3	17.1	11.4
Coefficient of z (1st stage)	0.36	0.32	0.40	0.32	0.34	0.33
t-stat of z (1st stage)	3.19	4.65	2.21	5.86	2.59	2.63
R^2 (1st stage)	0.15	0.11	0.11	0.097	0.097	0.098
Observations	57	130	130	217	217	217

Table 10. This table contains the coefficient estimates from several specifications of an OLS regression of *Underpricing* on two measures of affiliated allocations: a dummy variable that identifies IPOs with affiliated allocations (columns 1-5) and the percentage of the issue allocated to affiliated funds (columns 6-10). The sample includes 1086 eligible IPOs in the period 2001-2013. Columns 2, 3, 7 and 8 introduce IPO level control variables, as defined in section 3. Columns 4 and 9 introduce year and industry fixed effects. Columns 5 and 10 introduce lead underwriters' control variables. All percentages and returns are multiplied by 100. All non-dummy variables except *Age* are winsorized at the 95% level. Heteroschedasticity-robust t-statistics are in parentheses. Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
AffiliatedAllocDummy	11.0***	6.94***	6.54***	6.28***	6.50***					
	(10.30)	(6.11)	(5.45)	(5.15)	(5.15)					
AffiliatedAllocPerc						0.99***	0.81***	0.70***	0.62**	0.67**
						(3.48)	(3.31)	(2.80)	(2.44)	(2.52)
IndependentAllocPerc	0.30***	0.21***	0.19***	0.18***	0.17***	0.34***	0.23***	0.21***	0.19***	0.17***
	(6.50)	(5.18)	(4.71)	(4.44)	(3.93)	(7.21)	(5.55)	(4.98)	(4.59)	(4.03)
ln(Age+1)		-1.64***	-1.13*	-1.70***	-1.60**		-1.65***	-1.08*	-1.61**	-1.48**
		(-2.86)	(-1.91)	(-2.64)	(-2.44)		(-2.88)	(-1.83)	(-2.51)	(-2.25)
ln(Assets)		-1.55***	-0.68	-0.94	-0.90		-1.45***	-0.78	-1.06	-1.07
		(-3.91)	(-1.10)	(-1.43)	(-1.30)		(-3.54)	(-1.26)	(-1.60)	(-1.54)
Adjustment		0.63***	0.62***	0.57***	0.56***		0.70***	0.67***	0.63***	0.61***
		(15.91)	(14.12)	(12.70)	(11.92)		(18.46)	(15.95)	(14.38)	(13.60)
OnlyPrimaryShares		-0.91	-1.23	-0.32	-0.33		-1.59	-1.76*	-0.79	-0.80
		(-0.93)	(-1.26)	(-0.31)	(-0.31)		(-1.62)	(-1.80)	(-0.78)	(-0.75)
Nasdaq		1.43	1.17	1.85	2.05		0.38	0.43	1.21	1.39
		(1.09)	(0.89)	(1.42)	(1.51)		(0.30)	(0.33)	(0.94)	(1.04)
Foreign		0.88	0.17	-0.080	-0.034		1.07	0.29	-0.0047	0.11
		(0.54)	(0.11)	(-0.05)	(-0.02)		(0.64)	(0.17)	(-0.00)	(0.06)
ln(Proceeds)			-0.33	0.45	0.27			0.28	1.15	0.91
			(-0.23)	(0.31)	(0.17)			(0.20)	(0.79)	(0.58)
VentureCapitalBack			3.52**	4.98***	5.19***			3.49**	4.98***	5.20***
			(2.49)	(3.48)	(3.44)			(2.47)	(3.49)	(3.45)
LengthIPOprocess			-0.39***	-0.28**	-0.29**			-0.38***	-0.27**	-0.28**
			(-3.09)	(-2.19)	(-2.21)			(-2.96)	(-2.09)	(-2.10)
HighRankDummy			0.87	1.11	2.01			2.01	2.29*	2.89*
			(0.66)	(0.82)	(1.17)			(1.51)	(1.68)	(1.68)
NumberLeadManagers			0.40	-0.34	1.89			0.38	-0.33	1.48
			(1.02)	(-0.73)	(1.26)			(0.95)	(-0.71)	(0.98)
NumberSyndicateMembers			-0.028	0.12	0.10			0.0067	0.12	0.11
			(-0.22)	(0.77)	(0.63)			(0.05)	(0.75)	(0.66)
GrossSpread			1.65*	1.74*	1.61			2.17**	2.20**	2.08*
			(1.71)	(1.77)	(1.43)			(2.27)	(2.26)	(1.89)
Constant	2.63***	19.8***	3.97	8.67	9.33	6.66***	22.8***	0.057	5.26	6.49
	(2.81)	(6.36)	(0.38)	(0.78)	(0.73)	(6.67)	(7.27)	(0.01)	(0.48)	(0.52)
industry FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
year FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
underwriter FE	No	No	No	No	Yes	No	No	No	No	Yes
R^2	0.131	0.342	0.354	0.393	0.408	0.067	0.328	0.343	0.383	0.397
F	86.7	64.8	36.4	16.7	9.99	32.4	60.9	34.4	15.9	9.47
Observations	1086	1086	1086	1086	1086	1086	1086	1086	1086	1086

Table 11. This table contains the coefficient estimates from several specifications of an OLS regression of *Underpricing* on its determinants in two subsamples. For each IPO, we compute a measure of the abnormal number of IPOs completed by its underwriters. We split the sample into terciles based on this measure. Regression results are reported for the top tercile ("High") and the bottom tercile ("Low"). The sample includes IPOs performed in the period 2001-2013. All percentages and returns are multiplied by 100. All non-dummy variables except *Age* are winsorized at the 95% level. Heteroschedasticity-robust t-statistics are in parentheses. Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	Low number of IPOs			High number of IPOs		
	(1)	(2)	(3)	(4)	(5)	(6)
AffiliatedAllocPerc	0.32	0.15	-0.15	1.20**	0.87**	1.08**
	(0.69)	(0.33)	(-0.31)	(2.33)	(2.14)	(2.39)
IndependentAllocPerc	0.32***	0.19***	0.17**	0.42***	0.30***	0.23***
	(4.26)	(2.97)	(2.51)	(4.82)	(4.09)	(2.89)
IPO controls	No	Yes	Yes	No	Yes	Yes
industry FE	No	No	Yes	No	No	Yes
year FE	No	No	Yes	No	No	Yes
underwriter FE	No	No	Yes	No	No	Yes
R^2	0.054	0.357	0.456	0.087	0.381	0.469
F	9.46	13.6	5.90	13.7	13.3	5.91
Observations	362	362	362	362	362	362

Table 12. This table contains the coefficient estimates from several specifications of an OLS regression of *Underpricing* on its determinants in two subsamples. For each IPO, we compute a measure of abnormal brokerage commissions received by its underwriters from institutional investors in a 10-day window before the IPO. We split the sample into terciles based on this measure. Regression results are reported for the top tercile ("High") and the bottom tercile ("Low"). The sample includes IPOs performed in the sub-period 2001-2010. All percentages and returns are multiplied by 100. All non-dummy variables except *Age* are winsorized at the 95% level. Heteroschedasticity-robust t-statistics are in parentheses. Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	Low commissions from institutional investors			High commissions from institutional investors		
	(1)	(2)	(3)	(4)	(5)	(6)
AffiliatedAllocPerc	2.10***	1.45**	1.71**	0.90**	0.62*	0.79*
	(3.11)	(2.04)	(2.30)	(1.99)	(1.67)	(1.95)
IndependentAllocPerc	0.080	0.027	0.088	0.26**	0.25***	0.29***
	(0.84)	(0.35)	(1.05)	(2.56)	(2.82)	(2.95)
IPO controls	No	Yes	Yes	No	Yes	Yes
industry FE	No	No	Yes	No	No	Yes
year FE	No	No	Yes	No	No	Yes
underwriter FE	No	No	Yes	No	No	Yes
R^2	0.053	0.425	0.499	0.038	0.349	0.445
F	5.31	10.4	5.13	4.85	8.59	3.98
Observations	246	246	246	245	245	245

Table 13. This table contains the coefficient estimates from several specifications of an OLS regression of *Underpricing* on its determinants in two subsamples. We split the sample into terciles based on $ln(Assets)$. Regression results are reported for the top tercile ("Large") and the bottom tercile ("Small"). The sample includes IPOs performed in the period 2001-2013. All percentages and returns are multiplied by 100. All non-dummy variables except *Age* are winsorized at the 95% level. Heteroschedasticity-robust t-statistics are in parentheses. Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	Small firm size			Large firm size		
	(1)	(2)	(3)	(4)	(5)	(6)
AffiliatedAllocPerc	1.15	-0.21	-0.22	1.12***	0.86***	0.73**
	(1.59)	(-0.39)	(-0.36)	(3.35)	(2.86)	(2.27)
IndependentAllocPerc	0.35***	0.18***	0.17*	0.17**	0.13**	0.051
	(4.14)	(2.65)	(1.95)	(2.34)	(2.02)	(0.70)
IPO controls	No	Yes	Yes	No	Yes	Yes
industry FE	No	No	Yes	No	No	Yes
year FE	No	No	Yes	No	No	Yes
underwriter FE	No	No	Yes	No	No	Yes
R^2	0.058	0.389	0.486	0.056	0.336	0.403
F	10.7	15.5	7.70	8.97	11.9	4.48
Observations	362	362	362	362	362	362

Figure 1. This figure shows the number of eligible and non-eligible IPOs by year

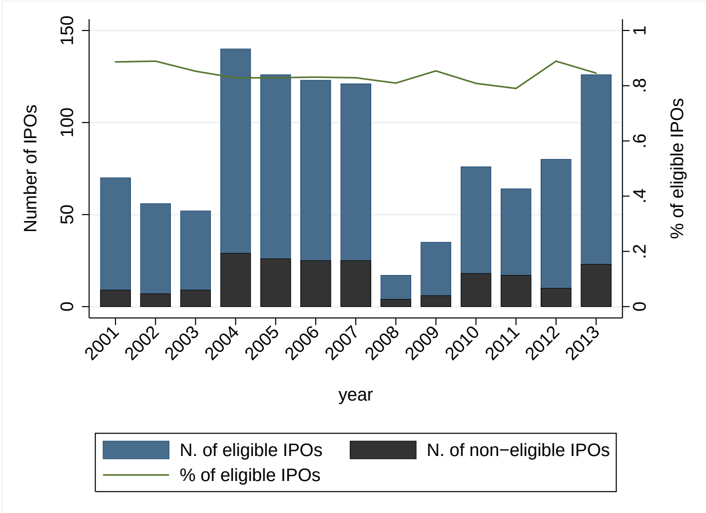
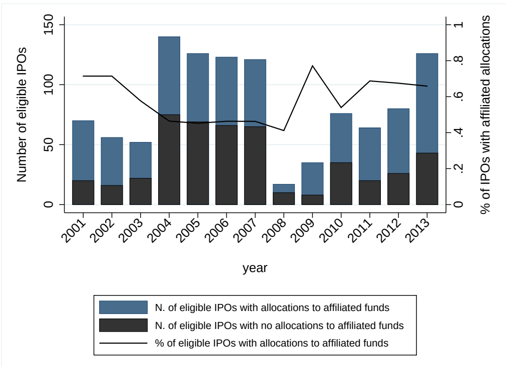
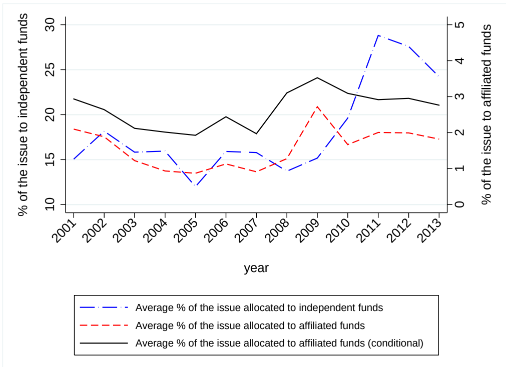


Figure 2. This figure shows the affiliated and independent allocations from 2001 to 2013 of 1,086 eligible IPOs. Panel (A) plots the number and the percentage of IPOs that involve at least one affiliated transaction, and the number of IPOs with no affiliated allocations. Panel (B) plots the average percentage of the issue allocated to affiliated funds, the average percentage of the issue allocated to independent funds, and the average percentage of the issue allocated to affiliated funds conditional on IPOs involving at least one affiliated transaction.



(A)



(B)

Figure 3. A visual and intuitive representation of our identification strategy.

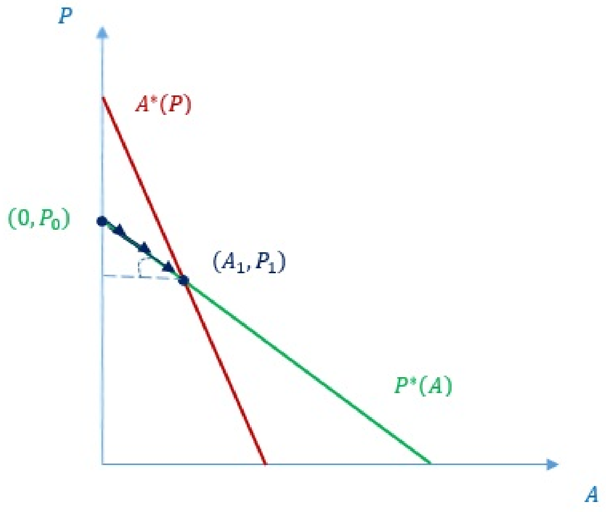
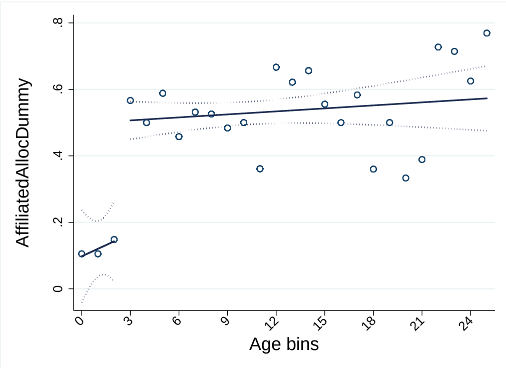
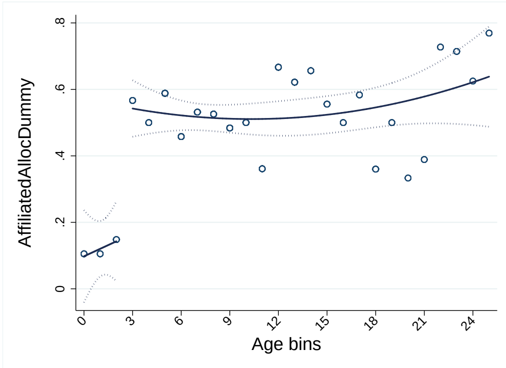


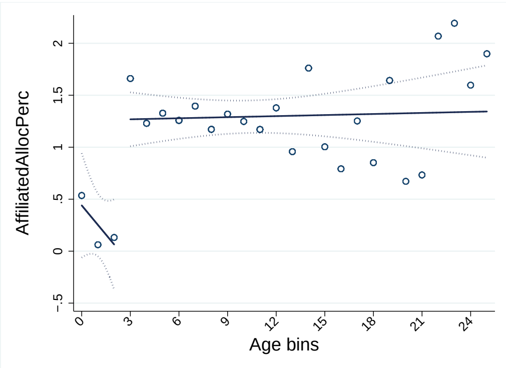
Figure 4. This figure plots average treatments by forcing variable. We compute the average $AffiliatedAllocDummy$ (Panel A and B) and $AffiliatedAllocPerc$ (Panel C and D) for each age group (bin) of one-year size. Fitted values come from a linear fit on both sides of the three-year cutoff in panels (A) and (C); they come from a linear fit for $Age < 3$ and a quadratic fit for $3 \leq Age \leq 25$ in panels (B) and (D). 95% confidence intervals are reported with dotted lines.



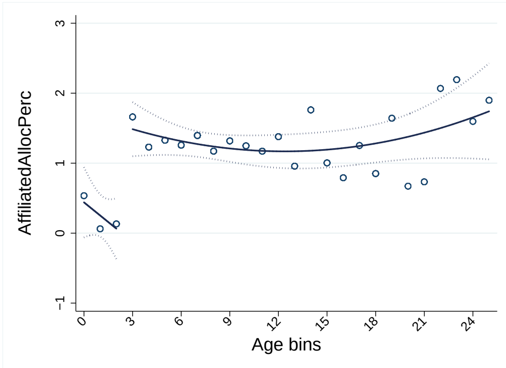
(A)



(B)

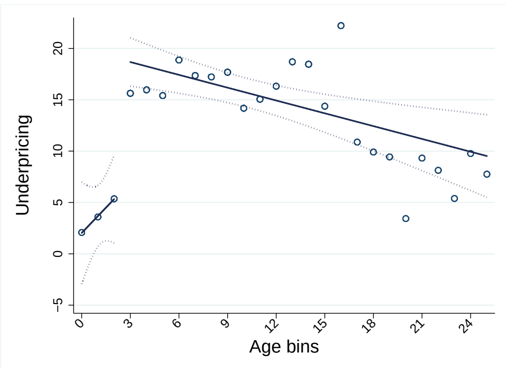


(C)

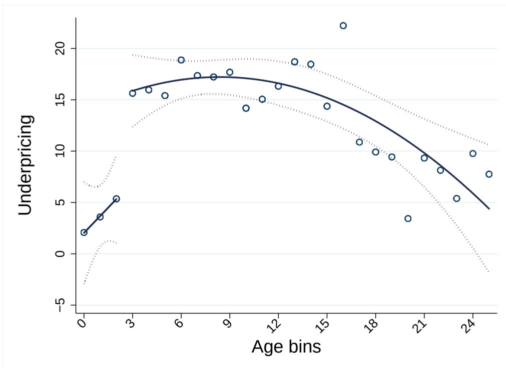


(D)

Figure 5. This figure plots the average outcome by forcing variable. We compute average $Underpricing$ for each age group (bin) of one-year size. Fitted values come from a linear fit on both sides of the three-year cutoff in panel (A); they come from a linear fit for $Age < 3$ and a quadratic fit for $3 \leq Age \leq 25$ in panel (B). 95% confidence intervals are reported with dotted lines.

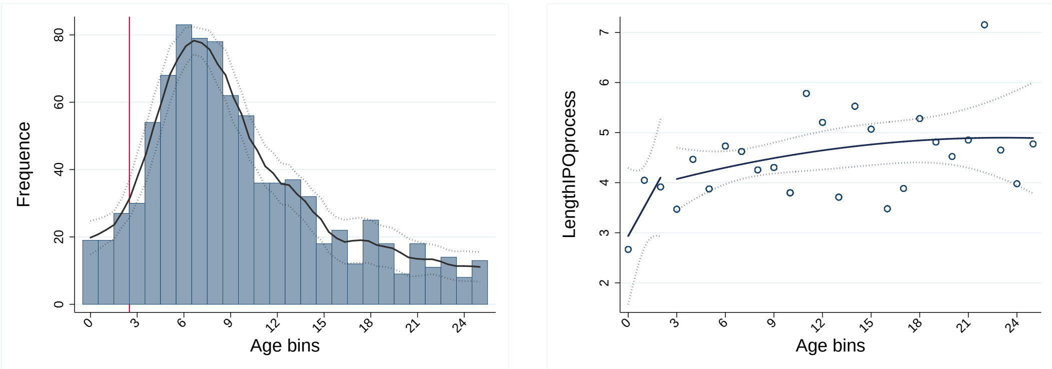


(A)



(B)

Figure 6. This figure plots the number of IPOs (Panel A) and the average length of the IPO process (Panel B) by forcing variable. Panel (A) reports the histogram and its smoothed values from a kernel-weighted polynomial regression with epanechnikov kernel. In Panel (B), we compute average $LengthIPOprocess$ for each age group (bin) of one-year size. Fitted values come from a linear fit for $Age < 3$ and a quadratic fit for $3 \leq Age \leq 25$. 95% confidence intervals are reported with dotted lines.



(A) (B)

Figure 7. This figure plots the number of IPOs underwritten by the most important underwriters by age groups (bins) of one-year size. All sub-figures report histograms and smoothed values from kernel-weighted polynomial regressions with epanechnikov kernel. 95% confidence intervals are reported with dotted lines.

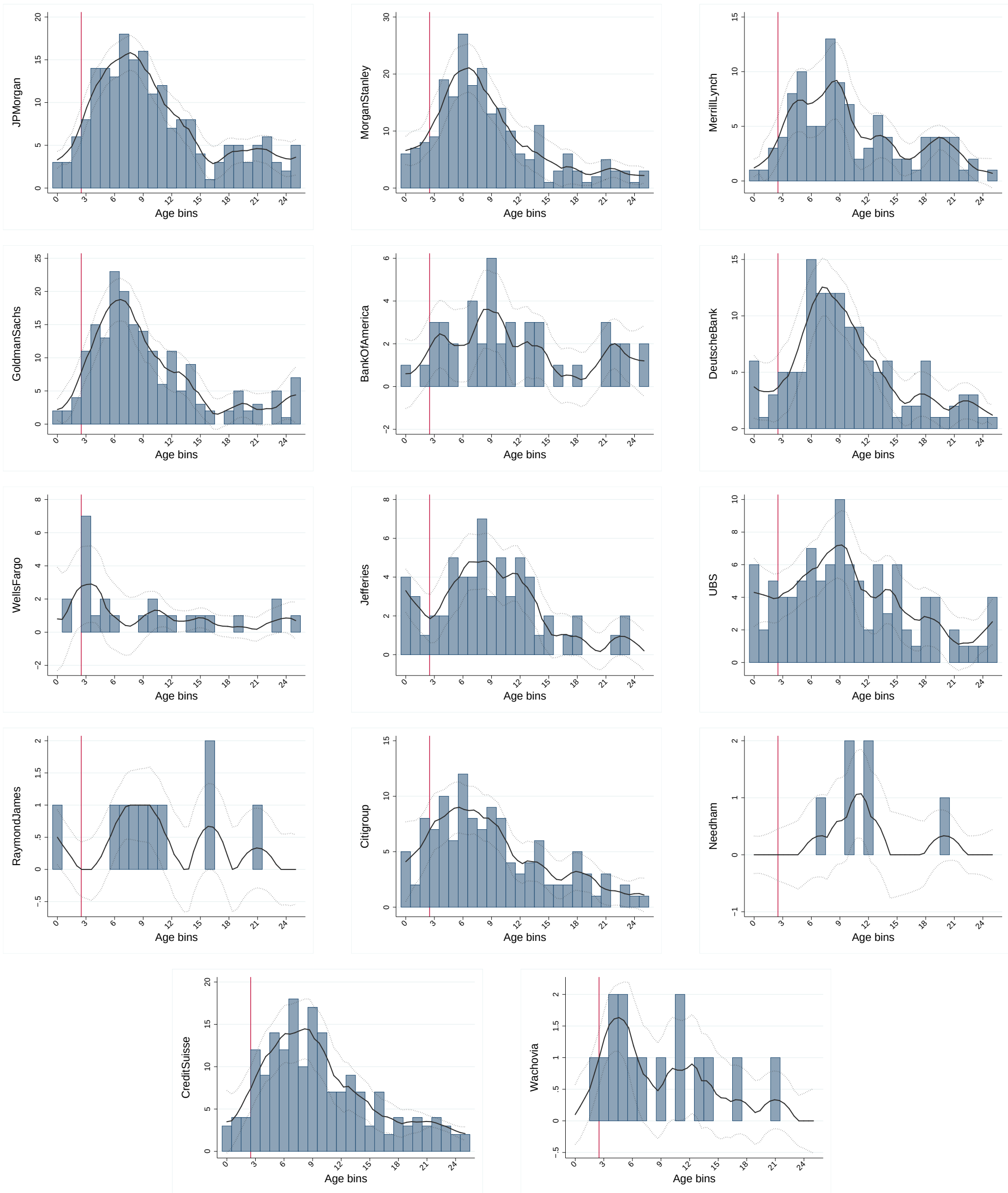


Figure 8. This figure plots average covariates by forcing variable. We compute the average value of each control variable by age groups (bins) of one-year size. Fitted values come from a linear fit for $Age < 3$ and a quadratic fit for $3 \leq Age \leq 25$. 95% confidence intervals are reported with dotted lines.

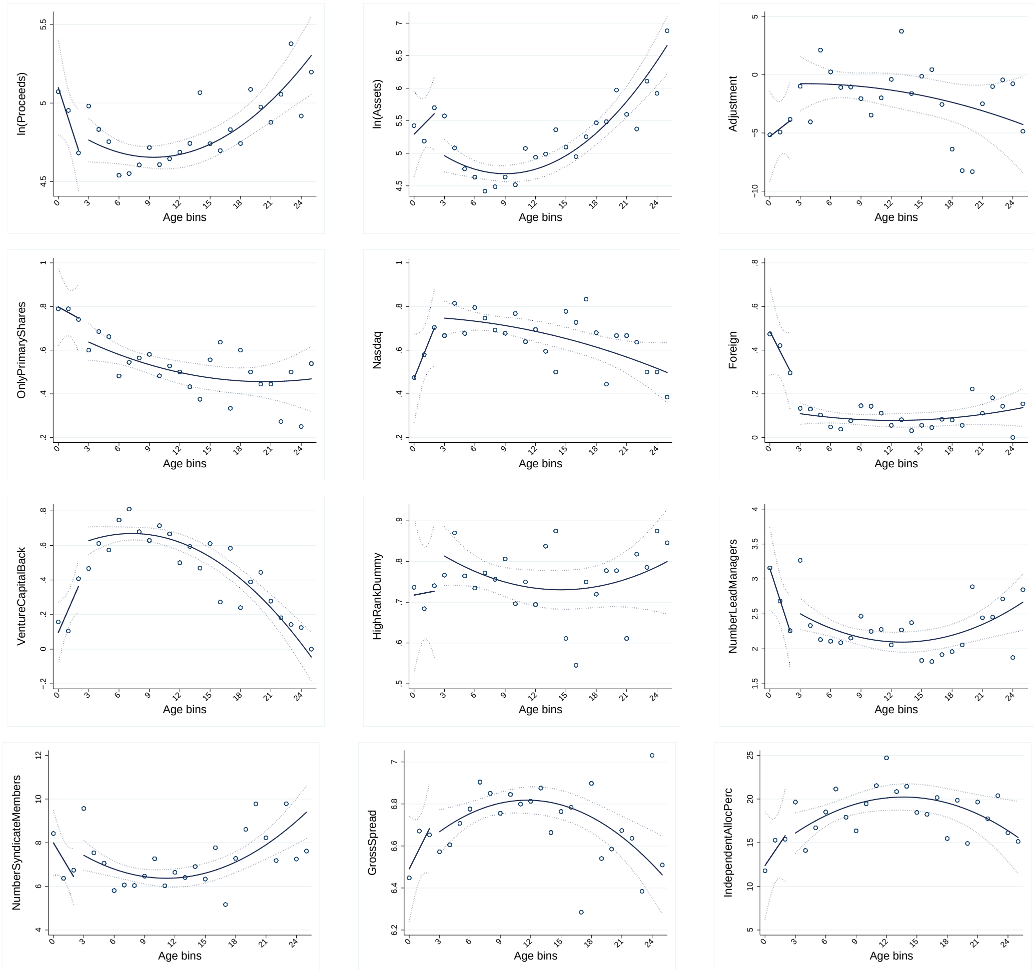
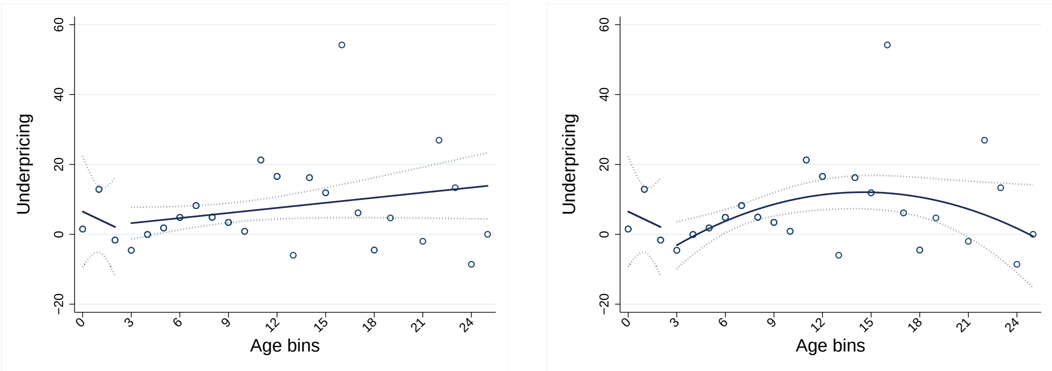
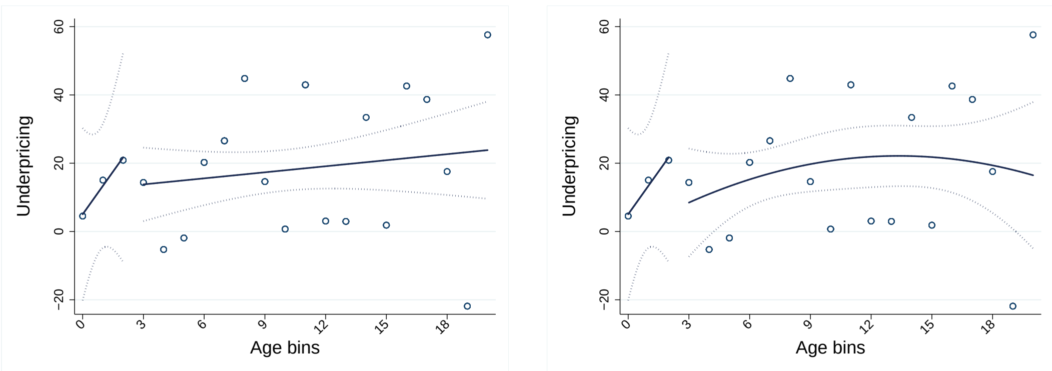


Figure 9. This figure plots the average outcome by forcing variable for non-eligible IPOs. We compute average *Underpricing* for each age group (bin) of one-year size. Fitted values come from a linear fit on both sides of the three-year cutoff in panel (A); they come from a linear fit for $Age < 3$ and a quadratic fit for $3 \leq Age \leq 25$ in panel (B). 95% confidence intervals are reported with dotted lines.



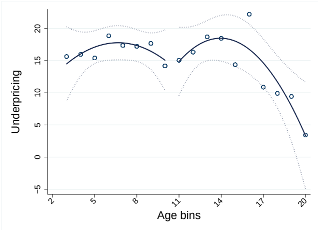
(A) (B)

Figure 10. This figure plots the average outcome by forcing variable for a sample of 488 European IPOs performed in the period 2001-2013. We compute average *Underpricing* for each age group (bin) of one-year size. Fitted values come from a linear fit on both sides of the three-year cutoff in panel (A); they come from a linear fit for $Age < 3$ and a quadratic fit for $3 \leq Age \leq 25$ in panel (B). 95% confidence intervals are reported with dotted lines.

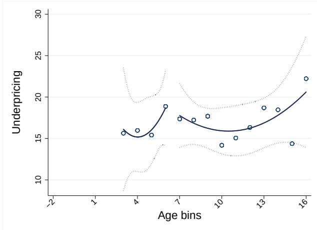


(A) (B)

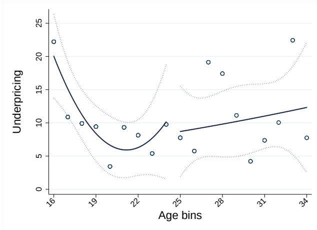
Figure 11. This figure plots the average outcome by forcing variable for arbitrary thresholds. In Panel (A), the arbitrary threshold is the median value of the forcing variable, conditional on the forcing variable being higher than the cutoff. In Panel (B), the arbitrary threshold is the 25th percentile of the forcing variable, conditional on the forcing variable being higher than the cutoff. In Panel (C), the arbitrary threshold is the 75th percentile of the forcing variable, conditional on the forcing variable being higher than the cutoff. Fitted values come from a quadratic fit on both sides of the arbitrary cutoff. 95% confidence intervals are reported with dotted lines.



(A)



(B)



(C)

Figure 12. An example of a 10(f)-3 attachment to the N-SAR form

FORM 10f-3
Registered Domestic Securities and Government Securities

FUND: The UBS Funds - UBS U.S. Small Cap Growth Fund
Name of Adviser or Sub-Adviser: UBS Global Asset Management (Americas) Inc.
1. Issuer: Green Dot Corp. - Class A
2. Date of Purchase: 7/21/2010 3. Date offering commenced: 7/21/2010
4. Underwriter(s) from whom purchased: JP Morgan Chase Fleming
5. "Affiliated Underwriter" managing or participating in syndicate:
UBS Investment Bank
6. Aggregate principal amount or number of shares purchased: 20,000 shares (firmwide)
7. Aggregate principal amount or total number of shares of offering: 4,560,000 shares
8. Purchase price per unit or share(net of fees and expenses): $36.00
9. Initial public offering price per unit or share: $36.00
10. Commission, spread or profit: ____________% $_1.512_________
11. Have the following conditions been satisfied?

FUND: THE UBS Funds - UBS High Yield Fund
Name of Adviser or Sub-Adviser: UBS Global Asset Management (Americas) Inc.
1. Issuer: Pride International Inc. 6 7/8% due 8/15/2020
2. Date of Purchase: 8/03/2010 3. Date offering commenced: 8/03/2010
4. Underwriter(s) from whom purchased: Goldman Sachs & Co.
5. "Affiliated Underwriter" managing or participating in syndicate:
UBS Investment Bank
6. Aggregate principal amount or number of shares purchased: $500, 000 firmwide
7. Aggregate principal amount or total number of shares of offering: $900,000,000
8. Purchase price (net of fees and expenses): $100.00
9. Initial public offering price: $100.00
10. Commission, spread or profit: .735% $____________
11. Have the following conditions been satisfied?

Do institutional investors play hide-and-sell in the IPO aftermarket?*

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Abstract

We document a robust buy/sell asymmetry in the choice of the broker in the IPO aftermarket: institutional investors are less likely to sell than buy through the lead underwriters in a sample of IPOs issued between 1999 and 2010 in the United States. Consistent with investors hiding their sell trades, the asymmetry is the strongest in cold IPOs and it is limited exclusively to the first month after the issue. The asymmetry survives when we control for any unobserved institution, IPO, and institution-IPO specific characteristics, including any relationship between institutional investors and underwriters. Contrary to the conventional view, the intention to flip IPO allocations is not an important motive for hiding sell trades from the lead underwriters; institutions that sell shares through non-lead brokers tend to have bought them through the lead underwriters in the IPO aftermarket, consistent with institutions breaking their laddering agreements.

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1 Introduction

Despite considerable research on the conflicts of interest in initial public offerings, there is little evidence describing moral hazard problems faced by IPO investors. This topic deserves attention because investors' behavior may ultimately affect the benefits and the costs of the bookbuilding method. In particular, we are interested if the IPO mechanism in place motivates the choice of the broker(s) to which investors direct their trades in the IPO aftermarket. We hypothesize that the IPO bookbuilding method provides incentives to investors to avoid lead underwriters for their sell trades in the IPO stocks in the early aftermarket.

Institutional investors may have an incentive to hide their sell trades from the lead underwriters in the IPO aftermarket (we call it "hide-and-sell" hypothesis) for two main reasons. First, investors might try to hide their allocations sales in order to preserve their business with the lead underwriters in the IPO allocations market. A key feature of book-built IPOs is that the investment banks that underwrite the issue have considerable discretion over who receives allocations. As explained by Jenkinson and Jones (2004), one of the popular justifications for such discretion, often emphasized by investment bankers, is that underwriters can allocate shares to long-term holders of the stock in the interests of the issuer. Investors that readily sell their allocations in the IPO aftermarket, commonly referred to as "flippers", tend to put a downward pressure on the trading price. While this might not be a relevant concern in hot IPOs, where flipping may serve to increase market liquidity, the selling pressure generated by flippers could lower the price below the offer price in cold offerings (Aggarwal (2003)). Underwriters may find it convenient to reward institutions that play a supportive role and do not flip their allocations, as they play a role as market makers in the secondary market (Ellis et al. (2000)), and they may face reputational losses in case of poor aftermarket performance and too much flipping activity (Aggarwal (2003)). Consistent with this view, Chemmanur et al. (2010) find that investors receive larger allocations when they hold their allocations for longer periods. This gives investors an incentive to hide their allocation sales from the lead underwriters. We label this incentive as the "flipping hiding motive". Some existing studies suggest that investors may try to hide their allocation sales in post-IPO trading (Griffin et al. (2007), Chemmanur et al. (2010)).

The second reason for hiding sell trades from the lead underwriters is related to a practice known as "laddering", which involves a quid-pro-quo arrangement between underwriters and their clients: investors receive IPO allocations in exchange for a committment to purchase additional shares in the aftermarket. The clients that enter in such an agreement are called "ladderers". As explained by Hao (2007) and Griffin et al. (2007), laddering could be beneficial for the lead underwriters as the buying pressure from ladderers could reduce the underwriters' price support costs in the IPO aftermarket, especially in cold IPOs. Moreover, the pre-arranged client demand in the aftermarket may increase underwriters' brokerage commission revenues. The Securities and Exchange Commission (SEC) considers laddering as a manipulative practice prohibited by Rule 101 of Regulation M under the Securities Exchange Act of 1934. However, the legal definition of laddering requires the aftermarket purchase to be a condition imposed by the underwriter, thus leaving some space for implicit quid-pro-quo arrangements in which investors volunteers to buy additional shares (Hao (2007)). Consistent with lead underwriters engaging in laddering agreements with their clients, Griffin et al. (2007) find that investors are net buyers through the lead underwriters in a sample of Nasdaq IPOs. We posit that ladderers may have an incentive to break their quid-pro-quo arrangements if the shares that they committed to buy in the secondary market are in excess of their optimal holdings in the IPO firm. The potential costs for the investors that break the agreement, in terms of future business with the underwriters, may incentivize them to hide their sell trades. We label the incentive to hide sell trades that break investors' laddering agreements with the lead underwriters as "laddering hiding motive". To the best of our knowledge, we are the first to document that laddering mechanism may provide an incentive for the investor to avoid the underwriting brokers when selling the IPO stock in the aftermarket.

The hiding strategy that we consider in this paper is to sell IPO shares through brokers other than the lead underwriters (henceforth, "non-lead brokers"). We motivate our focus on this hiding strategy because of its simplicity of execution, as institutional investors usually trade through more than one brokerage house (Goldstein et al. (2009)). If the hide-and-sell hypothesis holds and investors use this simple hiding strategy, then we should observe them to be less likely to trade through the lead underwriters when they sell than when they purchase shares in the IPO aftermarket. We directly test this prediction using detailed institutional trading data, which allow us to control for important variables that may affect both the selling decision and choice of the broker, such as the relationship between the institution and the lead underwriters or any other institution-IPO specific characteristic. To the best of our knowledge, we are the first to directly test this prediction. Our analyses document a robust buy/sell asymmetry in the choice of the broker in the IPO aftermarket: institutional investors are significantly less likely to sell than buy through the lead underwriters during the first month of trading after the IPO.

We consider two factors that may affect the hiding incentives of financial institutions. First, if the buy/sell asymmetry is driven by hiding incentives, then it should be the strongest in cold IPOs: both the "flipping hiding motive" and the "laddering hiding motive" predict the lead underwriters to be concerned the most about investors' selling activity in weak offerings. Second, if the buy/sell asymmetry is driven by hiding incentives, then we should not be able to detect it when there are no incentives to hide stock sales from the lead underwriters. We perform placebo tests to show that the buy/sell asymmetry disappears after few months from the issue date and in a matched sample of non-IPO stocks. Overall, our evidence is consistent with the predictions of the hide-and-sell hypothesis.

The buy/sell asymmetry may be driven by both the "flipping hiding motive" and the novel "laddering hiding motive". Our data and methodology allow us to disentangle allocation sales from investors' buying and selling activity in the secondary market. Hence, we can investigate the reasons behind institutions' hiding behavior, in order to understand whether it is driven by flipping or laddering motives.

We argue that the "flipping hiding motive" might be overall weak in the United States because underwriters receive reports documenting the allocation sales of their customers. Flipping of shares is tracked via the Depository Trust Company's (DTC) IPO Tracking System and the lead underwriters receive two types of reports (Aggarwal (2003)). The first report provides them with client-level information about the flipping activity of the investors to whom they allocated IPO shares. The second report provides them with information about the aggregate flipping activity for each syndicate member, but this does not include client-level details. Therefore, lead underwriters can detect which one of their clients sold its allocations, but do not have direct access to the identity of flippers that received their allocations from other syndicate members. Consequently, investors that received IPO shares from other syndicate members have some chances to hide their flipping activity from the lead underwriters by avoiding selling through them. Morevover, flipping reports are not flawless and there is anecdotal evidence of institutional investors circumventing the DTC IPO Tracking System.[1] Though imperfect, the DTC IPO Tracking System dampens the scope for hiding flipping trades. The risk of being caught by the lead underwriters might not be zero even for other syndicate members' clients, as lead underwriters could exploit their relationship with the other syndicate members or use allocations and aggregate flipping data to infer flippers' identities. Since a great portion of the IPO shares are underwritten by the lead managers (Corwin and Schultz (2005)), the incentive to hide allocations sales might be overall weak.

On the contrary, the hiding technology that we investigate in this paper, that is, selling IPO shares through non-lead brokers, might allow investors to break their laddering agreements without being caught by the lead underwriters. Ladderers may purchase the shares that they committed to buy through the lead underwriters and then sell the shares in excess of their optimal holdings through any other broker. Since these stock sales (henceforth, "other sales" or "other sell trades" or "secondary sales") do not involve allocation sales, they are not detected by the DTC IPO Tracking System and leave scope for hiding them.

We disentangle allocation sales from other sales and, consistent with the above arguments and contrary to the conventional view, we find that flipping is not a relevant hiding motive: the buy/sell asymmetry is mainly driven by sell trades other than allocation sales. Furthermore, we investigate other predictions of the novel laddering hiding motive. First, if investors break their laddering agreements, then it has to be the case that they sell the shares that they committed to buy through the lead underwriters. Second, if investors hide the breaking of the agreement and use the simple hiding technology considered in this paper, then they should tend to execute a higher proportion of their sell trades through non-lead brokers when they buy shares through the lead underwriters and when they sell secondary shares. Third, investors that buy through the lead underwriters in the aftermarket and want to hide their secondary sales, should avoid allocation sales. Because of flipping reports, allocation sales signal to the underwriters that an investor sold more shares than it bought in the aftermarket, meaning that it sold also secondary shares. These three arguments predict a positive correlation between the proportion of sell trades executed through non-lead brokers, the volume of shares bought through the lead underwriters, and the volume of other sales, and a negative correlation between the volume of shares bought through the lead underwriters and the volume of shares flipped. Overall, we find evidence consistent with these predictions and with financial institutions breaking their laddering agreements.

The idea that investors may hide their sell trades is not new, even though the

[1] Griffin et al. (2007) report that "in March 2005, the NASD fined Spear, Leeds and Kellogg $1 million for concealing IPO shares from the DTC system from August 1997 to January 2001".

literature has exclusively framed it within the flipping hiding motive. Some existing studies suggest that investors might try to hide their allocation sales from the lead underwriters in the IPO aftermarket. For example, Griffin et al. (2007) find that investors are overall net sellers through brokers that do not belong to the syndicate group and net buyers through the lead underwriters during the first month after the issue. Using institutional trading data, Chemmanur et al. (2010) finds that institutional investors abnormally split their orders in the IPO aftermarket and suggest that it might be an attempt to hide flipping trades. In both papers, the idea is that flippers would like to hide their allocations sales in order to preserve their business with the lead underwriters in subsequent IPOs.

Though suggestive and relevant, the existing evidence is far from being conclusive. Investors could split their orders or sell through non-lead brokers for reasons other than hiding. For example, they could split their trades in order to generate a stream of abnormal commissions to the lead underwriters as a reward for receiving IPO allocations (Reuter (2006), Nimalendran et al. (2007), Goldstein et al. (2011), and Jenkinson et al. (2017)). The difference in net buy between lead underwriters' clients and non-lead brokers' clients might be driven by the characteristics of the trading institutions, such as their relationship with the lead underwriters. Since institutional investors tend to keep stable relationships with their brokers (Goldstein et al. (2009)), institutions that are usual underwriters' clients are more likely to trade with them in the IPO aftermarket. In order to preserve this relationship, they may also be more likely to support IPO prices by buying or avoiding to sell in the secondary market. On the contrary, institutions that are not usual underwriters' clients are more likely to trade with their own usual brokers in the IPO aftermarket and may also be more likely to sell IPO stocks. Moreover, the existence of flipping reports dampens the scope for hiding allocations sales through any trading strategy in the aftermarket. Whether, to what extent, and why hiding behavior is at place are, therefore, open questions and the aim of this paper is to shed light on them.

Our findings contribute two streams of research. First, our paper is related to an extensive literature that investigates the benefits and costs of the bookbuilding method of bringing companies public. While underwriters' discretion may have the benefits of incentivizing investors' information production (Benveniste and Spindt (1989), Benveniste and Wilhelm (1990), Sherman (2000), Cornelli and Goldreich (2001), and Sherman and Titman (2002)) and of placing allocations in the hands of long-term investors (Aggarwal (2003), Jenkinson and Jones (2004), Jenkinson and Jones (2009), and Chemmanur et al. (2010)), an increasing body of research unravels the conflicts of interest inherent to the bookbuilding method (Loughran and Ritter (2004), Reuter (2006), Griffin et al. (2007), Hao (2007), Nimalendran et al. (2007), Ritter and Zhang (2007), Jenkinson and Jones (2009), Liu and Ritter (2010), Goldstein et al. (2011), Ritter (2011), and Jenkinson et al. (2017)).[2] As the existing literature mainly focuses on the conflicts of interest between underwriters and issuers, we enrich it by investigating a so far overlooked moral hazard problem faced by investors. Our findings suggest that investors' hiding behavior may affect the potential benefits and costs of underwriters' discretion and stimulate further research to study the incentives of IPO investors.

[2]See Lowry et al. (2017) for a recent comprehensive survey of the IPO literature.

Second, we shed light on the determinants of the choice of the broker by financial institutions. Our findings are consistent with models in which investors face a trade-off between preserving long-term relationships with brokers that give them access to premium services and the need to hide their trading strategies (Goldstein et al. (2009)). We find a clear persistence in the choice of the broker, which is not much affected by trading costs motives and depends strongly on the long-term relationship between institutions and their brokers. However, we show how hiding incentives affect the choice of the broker in the context of IPOs.

The rest of the paper is organized as follows. Section 2 describes our sample selection criteria, defines the main variables used in our analyses, and provides summary statistics. Section 3 presents our baseline results and documents that institutions are less likely to trade through the lead underwriters when they sell than when they buy shares in the IPO aftermarket, especially in cold IPOs, consistent with the hide-and-sell hypothesis. Morever, it performs placebo analyses to check that this behavior is not present when there are no hiding incentives. Section 4 rules out potential alternative explanations, addresses endogeneity problems, and performs several robustness checks. Section 5 investigates the motives and drivers of institutions' hiding behavior. Finally, Section 6 concludes.

2 Data and summary statistics

2.1 IPO data

We use the Thomson Financial Security Data Company (SDC) database to identify IPOs made in the United States from 1999 to 2010.[3] We exclude all American Depository Receipts (ADRs), Real Estate Investment Trusts (REITs), unit and rights offerings, closed-end funds, IPOs with SIC codes between 6000 and 6199 and IPOs with offer price smaller than $5. Moreover, we require IPOs to have a match with the Center for Research in Security Prices database (CRSP) within seven calendar days from the issue. These filters leave us with 1,439 IPOs. In addition, we require IPOs to have a CUSIP match with the ANcerno/Abel Noser Solutions database, which provides us with detailed institutional trading data. We describe ANcerno trading data in the next subsection. This criterion leads us to drop 51 IPOs. Moreover, we drop three IPO firms that show inconsistent data: these firms show trading activity in the ANcerno database before the IPO date. Finally, we require at least one lead underwriter of each IPO to be matched with a broker of the Abel Noser Solutions database. This filter leaves out 24 firms. Our final sample consists of 1,361 IPOs involving 89 different lead underwriters. The number of IPOs varies considerably by year, ranging from 14 in 2008 to 373 in 1999.

By matching SDC and CRSP, we get the percentage return from the IPO offer price to the first day closing price (*Underpricing*) and we winsorize it at the 95% level. The average underpricing in our sample is 37.6% and the median is 14.8%. Since the hide-and-sell hypothesis depends on underpricing, we split our sample in terciles based on this variable. We define an IPO as "hot" if it is in the highest

[3] We clean the database from known mistakes by manually applying the corrections listed, as of April 2014, on the IPO database managed by Jay R. Ritter at the University of Florida: https://site.warrington.ufl.edu/ritter/ipo-data/.

tercile (*Underpricing* $> 29.4\%$), "weak" if it is in the middle tercile ($5.1\% >$ *Underpricing* $\leq 29.4\%$), and "cold" if it is in the lowest tercile (*Underpricing* $\leq 5.1\%$).

2.2 Institutional trading data in the IPO aftermarket

We obtain institutional trading data for our sample of 1,361 IPOs from the ANcerno/Abel Noser Solutions database. The IPO trading data covers the period from January 1999 to March 2011. For each trade placed by an institution, we get the following information: the name and the identity code ("managercode") of the institution, the name and the identity code ("brokercode") of the broker executing the trade, the trading date, the CUSIP of the stock traded, the number of shares traded, a variable identifying the side of the trade (buy or sell), the execution price, and the commissions paid. The reader may refer to the data appendix for the detailed description of the database.

We require trades to have non-missing managercodes and brokercodes, and to be sent to ANcerno by pension plan sponsors or money managers.[4] We match the Abel Noser Solutions database to the Thomson Reuters Institutional 13F Holdings database by institution names. We require institutions to have a match with 13F. A description of the matching procedure across several databases is provided in Figure 5 of the data appendix.

Summary statistics for more than 1.2 million institutional trades during the first year after the issue date are presented in Table 1.[5] The trades in the sample are placed by 227 different institutions of Abel Noser Solutions and are executed by 700 different brokers. The average trade involves 6565 shares. 8.2 billion IPO shares are traded during the first year from the issue, for a total value of 251.9 billion dollars. Lead underwriters have a large weight in the brokerage market of IPO stocks: during the first month after the IPO date, 40.4% of the IPO shares are traded through the lead underwriters. The percentage decreases in subsequent months to about 15%. The brokerage market shares of brokers that did not participate in the underwriting syndicate (henceforth, "other brokers") shows the opposite pattern: it is 52.4% during the first month after the IPO date and it increases in subsequent months to about 70%.

[Table 1 about here.]

The hide-and-sell hypothesis predicts that institutions' decision to trade with the lead underwriters depend on the side of their trade. Figure 1 breaks down the brokerage market shares of the lead underwriters for buying trades (black lines) and selling trades (light grey lines). For each IPO, we compute the percentage volume of institutional buy and sell trades executed by the lead underwriters and other brokers in each month from the IPO date. Then we average these percentages across IPOs and compute 95% confidence intervals around the means. Panel (A) shows that the weight in the brokerage market of the lead underwriters during the first month after the IPO date differs significantly depending on the trade side: it is

[4] This means that we require trades to have client-type code equal to 1 or 2. We exclude the relatively small amount of trades sent to ANcerno by brokers.

[5] Results are very similar if we exclude IPOs issued after March 2010.

almost 40% for buy trades and it is below 30% for sell trades, consistent with hiding behavior.[6] The market share of buy and sell trades becomes undistinguishable after the first month, consistent with hiding incentives being at place only during the first month of trading. Panels (B)-(D) break down the brokerage market share by underpricing terciles. We notice that the difference between buy and sell trades is mainly driven by cold IPOs, consistent with hiding incentives being stronger in cold IPOs.

[Figure 1 about here.]

In the rest of the paper, we aggregate Abel Noser Solutions' trade volumes at the daily level. Thus, our trading dataset comprises observations at the IPO-institution-broker-day level. Henceforth, with the word "trade" we mean "daily trade". The daily level of aggregation allows us to neglet intra-day trading decisions, which might involve several factors unrelated to our subject of study, such as institutions' churning shares to generate commissions to the lead underwriters (Goldstein et al. (2011)). Morevover, it allows us to avoid complications related to the intra-day trading time reported by the Abel Noser Solutions database. Figure 2 focuses on the first 21 trading days after the IPO. For each IPO, we compute the total amount bought and sold in each day by institutions that trade through the lead underwriters, through other syndicate members, and through brokers that did not participate in the IPO syndicate (bars). We also compute the cumulative netbuy of lead managers' clients, syndicate members' clients, and other brokers' clients (lines). The volume traded is scaled by the number of shares issued and it is averaged across IPOs. Panel (A) plots buy, sell, and cumulative netbuy volumes for all sample IPOs. Broadly consistent with the existing literature (Griffin et al. (2007), we see that institutions are net buyers through lead managers and syndicate members and net sellers through other brokers in the first few trading days after the IPO. Moreover, the daily volume sold tends to be larger through other brokers than through the lead underwriters; on the contrary, the daily volume bought tends to be larger through the lead underwriters than through other brokers. Finally, the difference in net buy between lead underwriters' clients and other brokers' clients is greater in cold IPOs. This is broadly consistent with hiding behavior.

[Figure 2 about here.]

The aggregate graphical evidence presented in this section suggests that some hiding behavior might be at place, but it is far from being conclusive. For example, the difference between buy and sell trades might be driven by institutions characteristics affecting both the decision to sell and the decision to trade with the lead underwriters, without any hiding behavior being at place. Institutions that decide to buy IPO shares and support the price of cold IPOs might be usual lead underwriters' clients; therefore, they might also tend to trade more through lead underwriters in the IPO aftermarket. Institutions that decide to sell IPO shares migth not be usual lead underwriters' clients; therefore, they might also tend to

[6] These numbers are slightly different from those in Table 1 because Figure 1 computes the average broker market shares in IPOs, while Table 1 computes brokerage market shares in the IPO aftermarket for IPOs as a whole.

trade more through their usual brokers in the IPO aftermarket. Our institution-level analysis of section 3 sheds light on these issues and directly tests the predictions of the hide-and-sell hypothesis.

2.3 Identifying institutional IPO allocations sales

We identify institutional IPO allocations sales following the algorithm proposed by Chemmanur et al. (2010), which is consistent with the Depository Trust Company's (DTC) IPO Tracking System. The objective is to disentangle an institution's allocations sales from its buying and selling activity in the IPO aftermarket. In order to do so, we classify as IPO allocation sales only those shares that are sold in excess of the shares bought until that point in time by an institution. For example, consider an institution that buys 500 shares in the secondary market during the first day after the issue date and then sells 300 shares on the second day and 300 shares on the third day. Then the IPO allocation sales of that institution are equal to zero on day 1 and 2 and are equal to 100 on day 3.

Figure 3 tracks the average cumulative percentage of IPO shares flipped, scaled by the number of shares offered, by month. 95% confidence intervals are reported with dotted lines. We report the average for the whole sample of IPOs (black line) and for the subsamples of hot, weak, and cold IPOs, defined by underpricing terciles (grey lines). Our sample institutions flip 3.2% of the shares offered within the first 21 trading days post-IPO and continue to sell their allocations in subsequent months. By the end of the first year, our sample institutions flip 8.5% of the shares issued on average. The amount of flipping is the highest for hot IPOs (almost 12% at the end of the first year) and the lowest for cold IPOs (less than 5% at the end of the first year).

[Figure 3 about here.]

2.4 Identifying institutional IPO allocations

We identify IPO allocations by combining institutional trading data with quarterly holdings data reported in 13F. The basic idea is to compute IPO allocations as the difference between the institution's holdings in the IPO firm at the first 13F filing date following the IPO and the net buying by the institution in the IPO firm between the IPO date and the 13F filing date. However, as pointed out by Chemmanur et al. (2010), it is unlikely to compute allocations precisely by matching 13F and the Abel Noser's Solution Database because of data differences in the two databases. For example, 13F might not contain all stock holdings, as institutions are required to disclose common stock positions greater than 10,000 shares or $200,000. This kind of matching problems might generate some inconsistencies when computing allocations as holdings minus net buying. For example, we might compute negative allocations and/or allocations smaller than the amount of shares flipped.

In order to rule out these inconsistencies, we complement our allocation proxy with flipping data. The idea is that an IPO allocation has to be at least equal to the amount of shares flipped by the institution. Formally, we proxy IPO allocations as follows. Let $H_{i,j}$ be the number of shares of IPO i held by institution j at the first filing date after the IPO. Let $\Delta_{i,j}$ be the total netbuy of IPO i shares by institution

j between the IPO date and the first filing date after the IPO. Let $F_{i,j}$ be the number of shares of IPO i flipped by institution j – as computed in section 2.3 – in the first three months after the IPO. We compute the percentage of shares of IPO i allocated to institution j, $AllocPerc_{i,j}$, as:

$$AllocPerc_{i,j} = \frac{\max(H_{i,j} - \Delta_{i,j},\ F_{i,j})}{SharesIssued_i}100$$

and we winsorize it at the 95% level. Table 2 reports IPO allocations summary statistics at the institution and issuer level. Conditional on receiving an allocation, the average institution gets 1.89% of the issue. In the average IPO, about 23 sample institutions receive an allocation and get 42.7% of the offer.

Allocations vary with underpricing. Institutions that receive cold IPO shares get a larger percentage of the issue than institutions that receive hot IPO shares (2.53% versus 1.53%). However, the number of institutions that receive allocations is much smaller in cold IPOs than in hot IPOs (13.6 versus 30.6). Thus, the total allocation to institutional investors is lower in cold IPOs than in hot IPOs (34.3% versus 47%).

[Table 2 about here.]

3 Do institutional investors hide their sell trades?

If investors systematically hide some of their sell trades from the lead underwriters (hide-and-sell hypothesis) by trading with other brokers, then we should observe the probability of trading through the lead underwriters to be lower for sell trades than for buy trades in the IPO aftermarket. In order to test this prediction, we run several specifications of the following linear probability model (LPM):

$$LeadDummy_{i,m,b,t} = \alpha + \beta Sell_{i,j,b,t} + X_{i,j,b,t}\Gamma + \delta_j + \theta_i + \lambda_{i,j} + u_{i,j,b,t}$$

where $Sell_{i,j,b,t}$ is a dummy variable equal to one if the istitution j is selling the IPO i through broker b on day t and zero if it is buying. The dependent variable, $LeadDummy_{i,j,b,t}$, is a dummy variable equal to one if the broker b executing the trade is any of the lead underwriters of IPO i and zero otherwise. $X_{i,j,b,t}$ is a vector of control variables, which are described below. δ_j, θ_i, and $\lambda_{i,j}$ are institution, IPO, and institution-IPO fixed effects. $u_{i,j,b,t}$ is the error term, which we allow to be correlated within institution. The hide-and-sell hypothesis predicts $\beta < 0$.

The vector of control variables includes the trading volume $RelVol$, which is the number of shares traded by the institution scaled by the number of shares issued and multiplied by 100. Moreover, we control for the relationship between institutional investors and lead underwriters. Lead underwriters' usual clients are more likely to choose a lead underwriter as a broker at any point in time, including the IPO aftermarket (Goldstein et al. (2009)). They might also be more likely to support the IPO price to preserve their relationship with the underwriters, thus being less likely to sell IPO shares than other investors. Conversely, institutions that are not usual underwriters' clients are less likely to trade with them and might be more likely to be IPO sellers. Therefore, a negative correlation between the decision to sell IPO shares and the decision to trade with a lead underwriter might be driven by the relationship between investors and underwriters. We control for it by means of the variable *NormalTradeLead*. For each institution-IPO pair, we compute the percentage volume traded in non-IPO stocks by the institution through the lead underwriters in a 6-month period prior to the issue.[7] We compute this variable separately for buy and sell trades, to capture any potential heterogeneity in the investor/lead underwriters relationship by trade side. We include in the specification the variable *Day*, which is the day in which the trade is executed relative to the issue date, in order to control for the likely decreasing trend in the probability of trading with a lead underwriter. One important determinant of the choice to trade with the lead underwriters might be their trading expenses. *ExcLeadComm* is the average percentage commission to the lead underwriters minus the average percentage commission to any other broker paid by sample institutions in the first 21 trading days after the issue date. With this variable we capture how expensive it is to trade with the lead underwriters relative to other brokers in the IPO aftermarket. We compute this variable separately for buy and sell trades to capture any potential heterogeneity in brokerage commissions by trade side. Finally, we control for the percentage IPO allocation received by an institution, *AllocPerc*. Institutions that receive IPO allocations might be more likely to trade with the underwriters for several reasons, including quid-pro-quo agreements to generate a stream brokerage commissions to the lead underwriters (Goldstein et al. (2011), Reuter (2006), and Nimalendran et al. (2007)) and "laddering" agreements to buy shares in the IPO aftermarket (Griffin et al. (2007)).

We choose a LPM because it allows us to control for fixed effects without incurring in the incidental parameter problem and it estimates marginal effects. The potential biasedness and inconsistency of OLS with binary outcome are unlikely to be a concern in our setting, as the average value of the dependent variable is not at the boundaries of the unit interval (it is 0.292). For monitoring purposes, we keep track of the proportion of predicted probabilities outside the [0, 1] interval in our regression tables.

Table 3 reports the OLS estimation results. We use standard errors clustered at the institution level for inference.[8] Panel (A) includes trades executed during the first 21 trading days after the issue date. We focus on this period because lead underwriters' practices suggest that investors' incentives to hide their sell trades should exist mainly during the first month of trading. For example, lead underwriters track IPO flipping through the Depository Trust Company's (DTC) IPO Tracking System and engage in market stabilization activities usually during the first 30 calendar days after the issue date (Aggarwal (2000)). In column (1) we regress *LeadDummy* on *Sell*; column (2) introduces control variables in the specification; columns (3), (4), and (5) control for institution, institution and firm, and institution-firm fixed effects.

[Table 3 about here.]

[7] The 6-month period includes trades in non-IPO stocks executed from the trading day -147 to the trading day -22 from the issue date.

[8] In unreported analyses, we allow the error term to be correlated within IPO, clustering standard errors at the firm level. Results get stronger.

The coefficient of the variable *Sell* is negative and statistically significant in all specifications. Considering the estimate in column (1), institutional investors are 6 percentage points less likely to trade through a lead underwriter when they sell IPO shares than when they buy, consistent with the hide-and-sell hypothesis. The coefficient is statistically significant at least at the 5% level. It is also economically significant: the probability of selling with a lead underwriter is almost 20% less than the probability of buying (0.06/0.32). The correlation survives when we control for institution, firm, and institution-firm fixed effects. Column (3) controls for institution fixed effects, such as their usual trading strategies in IPOs. Column (4) introduces IPO fixed effects, which capture any IPO-specific characteristics, including the identity of the lead underwriters. It might be argued that $NormalTradeLead$ controls only for the past relationship between institutions and lead underwriters in brokarage services, but not for their future expected relationship nor for their relationship in other services; in column (5) we control for any institution-IPO specific factor, exploiting within institution-IPO variation: an institution that is both buying and selling a given IPO is more likely to trade with the lead underwriters when it buys than when it sells.

The coefficient of $RelVol$ is positive and significant in all specifications: institutions that make larger trades are more likely to trade with the lead underwriters. A one percentage point increase in the trading volume is associated with about 13 percentage points increase in the probability of trading with a lead underwriter. As expected, there is a positive and statistically significant correlation between *LeadDummy* and $NormalTradeLead$. A one percentage point increase in the proportion of trades that the institution normally execute through the lead underwriters is associated with about 0.9 percentage points increase in the probability of trading with a lead underwriter in the IPO aftermarket. The coefficient becomes much smaller and statistically insignificant when we control for institution-firm fixed effects, suggesting that the relationship between investors and underwriters is homogeneuos across trade side and, thus, captured by these fixed effects. As expected, the coefficient of *Day* is negative and statistically significant. A one day increase in the trading time relative to the issue date is associated with about one percentage point decrease in the probability of trading with a lead underwriter. The coefficient of $ExcLeadComm$ is negative in all specifications. However, it is statistically significant only when we control for institution-firm fixed effect. Even though commissions does not seeem to be a main driver of the choice of the broker, differences in trading commissions across trade side help explain the within institution-IPO variation of *LeadDummy*. Finally, *AllocPerc* is only weakly significant in one specification (at the 10% level). Moreover, its sign flips across specifications. We cannot make definitive conclusions about its correlation with the choice of the broker in the IPO aftermarket.

In Panel (B), we replicate our analysis considering trades executed during the first 7 trading days after the issue date. The coefficient on *Sell* gets much stronger in all specifications, suggesting that most of the documented effect is concentrated in the first few trading days after the IPO.

Hiding incentives are stronger in cold IPOs. Underwriters are more likely concerned with sell trades when the aftermarket demand for the IPO stock is weak, as they put additional downward pressure on the price (Chemmanur et al. (2010)).

Hence, we hypothesize the buy/sell asymmetry in the choice of the broker documented in Table 3 to be stronger in cold IPOs. We define the variable $ColdIPO_i$ to be equal to one if the firm i is in the lowest tercile of the variable $Underpricing_i$ and zero otherwise. We introduce an interaction variable between $ColdIPO_i$ and $Sell_{i,j,b,t}$ in our regression specifications. Under the hide-and-sell hypothesis, we expect the coefficient on the interaction term to be negative. Table 4 reports the estimation results.

[Table 4 about here.]

Consistent with the hide-and-sell hypothesis, the negative correlation between *LeadDummy* and *Sell* is stronger when hiding incentives are more pronounced. The coefficient of the interaction term is negative and statistically significant at least at the 5% level. The economic magnitude is also significant: considering column (1), investors are about 11.7 percentage points less likely to trade with a lead underwriter when they sell cold IPOs' shares than when they buy cold IPOs' shares (0.051+0.066). This number is about as double as it is for other IPOs (5.1).

3.1 Placebo tests

If institutional investors are less likely to sell through the lead underwriters because they try to hide their sell trades, then we should not observe this behavior when there is no incentive to hide.

Lead underwriters' practices suggest that investors' incentives to hide their sell trades should exist mainly during the first month of trading. Hence, we should not detect systematic hiding behavior after the first month. Table 5 implements our regression analysis for institutional investors' trading activity during the third month after the IPO date. The coefficient of *Sell* is not statistically different from zero in all specifications. Moreover, its sign is positive in most specifications and its magnitude is often economically small.

[Table 5 about here.]

The hiding incentive is peculiar to IPOs: it should not exist for non-IPO stocks. Hence, we test the hide-and-sell hypothesis in a matched sample of trades in non-IPO stocks. We match trades as follows. First, we require candidate non-IPO stocks to be similar to the matched IPO. For each IPO, we select candidate non-IPO stocks that: (i) are in the same one-digit industry; (ii) are in the same quintile of market capitalization; (iii) are in the same tercile of Tobin's Q.[9] Then, we match each buy (sell) trade in IPO stocks with a buy (sell) trade made by the same institution in a candidate non-IPO stock within a 21 trading days window from the IPO date. The matched trade is the one with the closest dollar volume. We lose 1,909 trades in 55 IPOs because of missing data about market capitalization, industry, or Tobin's Q. Moreover, we lose 13,677 trades because of no match found. Our final sample consists of 28,990 trades in non-IPO stocks matched to 1,109 IPOs.[10] Table 6 implements our regression analysis for institutions' trading activity

[9] We get this data from CRSP and COMPUSTAT.

[10] The median volume difference between matched non-IPO trades and original IPO trades is 50 dollars. The correlation between dollar volumes of original and matched trades is 0.7.

in non-IPO stocks. The coefficient of *Sell* is not statistically different from zero in most specifications and it is weakly significant (10% level) only in two specifications. Moreover, its sign is positive in most specifications and its magnitude is economically small.

[Table 6 about here.]

Overall, our placebo tests confirm that the buy/sell asymmetry in the choice of the broker is peculiar to the IPO aftermarket, consistent with hiding incentives.

4 Alternative explanations and endogeneity issues

An alternative explanation to our findings is the following. Underwriters might try to disincentivize selling of IPO stocks by increasing brokerage commissions selectively on sell trades. If this is the case, some investors might choose to sell through brokers other than the lead underwriters in order to save on commissions, without any intention to hide their trade. This would generate the buy/sell asymmetry in the choice of the broker observed in our regressions even when the null hypothesis of no hiding behavior holds, thus invalidating our conclusions. Broadly consistent with this argument, Ellis (2006) finds evidence of bookrunners offering better terms on buy trades in a sample of Nasdaq IPOs.

We show that the commission story is unlikely to drive our results. First, notice that we control for the average commission required by lead underwriters in excess of the commission required by other brokers (*ExcLeadComm*) in our regressions. The variable *ExcLeadComm* is computed for buy trades and sell trades separately. Hence, it controls for the effect of the potential differential treatment that lead underwriters give to different trades on the investors' probability of choosing a lead underwriter as a broker. Second, we can dig into the commission story more deeply. If the commission story is a concern, then we should observe lead underwriters to require higher brokerage commissions for sell trades relative to at least one of these benchmarks: i) lead underwriters' commissions for buy trades in the IPO aftermarket; ii) lead underwriters' commissions for sell trades few months after the IPO; iii) commissions of brokers other than the lead underwriters for sell trades in the IPO aftermarket. Figure 4 and Table 7 show the none of the above statements holds. Figure 4 plots the average trading commission paid to the lead underwriters for buying trades (dark grey line) and sell trades (light grey line) by month from the issue date. Commissions are scaled by the dollar volume traded and 95% confidence intervals are reported with dotted lines. If anything, average brokerage commissions of lead underwriters are higher for buy trades than for sell trades during the IPO aftermarket. Moreover, average brokerage commissions for sell trades tend to be somewhat higher several months after the IPO than during the first month after the issue date. Table 7 reports difference of means tests for the percentage trading commission paid to lead underwriters and to any other broker during the first month after the IPO. The table shows that trading commissions do not significantly differ between broker type for sell trades. They do differ, however, for buy trades: lead underwriters require higher commissions for buy trades than other brokers do.

[Figure 4 about here.]

[Table 7 about here.]

Hence, empirical evidence does not support the commission story: lead underwriters do not increase commissions on sell trades to disincentivize selling of IPO stocks. In fact, there is some evidence that they might do the opposite: commissions on buy trades seem to be particularly high in the IPO aftermarket.[11] If anything, this could actually work against finding results in favor of the hide-and-sell hypothesis.

In Section 3.1, we find that institutional investors are about 6 percentage points less likely to sell with the lead underwriters relative to their buy trades. One could argue that our results are driven by investors buying through the lead underwriters and not by investors selling through other brokers. We do not claim that abnormal buying through lead underwriters does not play a role in this setting. In fact, in Section 5 we argue that it might be a relevant factor for understanding why investors hide their sell trades. However, we need to provide evidence that the buy/sell asymmetry in the choice of the broker documented in this section is driven also by investors selling less through the lead underwriters. Figure 2 shows that the asymmetry between buy and sell trades is driven both by investors buying more through lead underwriters and by investors selling more through other brokers during the first month after the issue. Though relevant, Figure 2 focuses on the first 21 trading days and, hence, it does not take into account that investors might trade more with other brokers at any point in time, not just in the IPO aftermarket. Only few brokers are lead underwriters of an IPO and, thus, it is not surprising that investors trade more shares with other brokers. To overcome this issue, in Table 8 we test whether investors sell more shares through other brokers than through lead underwriters during the first month of trading relative to the third month of trading. We perform a Difference-in-Differences (DiD) analysis and run the following regression:

$$SV_{i,m,l} = \beta_0 + \beta_1 LeadDummy_{i,l} + \beta_2 Month1_{i,m} + \beta_3 LeadDummy^{*}Month1_{i,m,l} + \epsilon_{i,m,l}$$

$SV_{i,m,l}$ is the total number of shares of IPO i, scaled by the number of shares issued, sold by financial institutions in month m through a broker of type l. We consider trades executed in month 1 and month 3 after the IPO ($m = 1, 3$); $Month1_{i,m}$ is a dummy variable equal to one if the shares are sold in month 1 and zero otherwise. We consider trades executed through two types of brokers: lead underwriters and brokers that do not belong to the underwriting syndicate ($l = Lead, Other$); $LeadDummy_{i,l}$ is a dummy variable equal to one 1 if the shares are sold through the lead underwriters and zero otherwise. $LeadDummy^{*}Month1_{i,m,l}$ is the interaction between the two variables. Table 8 reports the results of the DiD estimation. We cluster standard errors at the IPO level.

[11] Understanding why lead underwriters' commissions on buy trades are high in the IPO aftermarket goes beyond the scope of this paper. Though difficult to reconcile with Ellis (2006)'s result, we notice that our evidence is broadly consistent with the literature on quid-pro-quo agreements in IPOs, which suggest that investors might get preferential treatment in the allocation of IPOs in exchange of paying excessive brokerage commissions to the lead underwriters (e.g., Reuter (2006)). Our finding is also broadly consistent with Griffin et al. (2007), who finds that there is more net buying through the bookrunners in IPOs in which the bookrunner charges higher trading costs.

[Table 8 about here.]

Table 8 shows that investors sell a smaller amount of shares through the lead underwriters than through other brokers both in month 3 and in month 1, as expected. The magnitude of the difference, however, is more pronounced in month 1 ($\beta_3 = -0.64$), consistent with hiding incentives.[12] Hence, the buy/sell asymmetry in the choice of the broker, that we document in Section 3, is likely not only driven by investors buying relatively more through the lead underwriters, but also by investors selling relatively less through the lead underwriters.

The decision to sell is endogenous. Institutions that decide to sell an IPO stock might differ from institutions that buy the IPO under several dimensions that might be correlated with their choice of the broker. In an ideal experiment, we would like to observe how institution j would have traded IPO i if, for a given trade, it would have switched trade side. Since in one of our specifications we exploit within institution-IPO variation, we rule out sources of endogeneity that are constant within institution-IPO pairs (e.g., the relationship between an investor and the lead underwriters of an IPO): we observe the same institution buying and selling the same IPO stock through different brokers, often over the same trading day.[13] Even though this might seem reasonably close to the ideal experiment mentioned above, we cannot exclude that some trade-varying unobserved factors jointly drive investors' selling and broker choices within institution-IPO pairs. However, it is hard to find a trade-level factor that would make the buy/sell asymmetry in the choice of the broker vanish, given that we control for commissions, volume, and day. Another source of potential criticism is related to the fact that our estimation in column (5) of Table 3 exploits variation in the trading side within institution-IPO pairs. In our sample, more than 50% of the observations do not exhibit variation within institution-IPO; i.e., the investor is either buying or selling the IPO stock. Hence, in column (5) we use information of a specific subsample of observations. This is unlikely to be a relevant issue for our purposes, as the specification of column (5) still serves the goal of detecting hiding behavior. Moreover, the coefficient of $Sell$ in the regressions of Table 3 is fairly stable across different specifications, including column (5). Overall, even though we do not claim that we estimate a causal effect, endogeneity concerns are unlikely to qualitatively change our conclusions about the buy/sell asymmetry in the choice of the broker.

For robustness, we also seek for a source of exogenous variation in the selling decision of financial institutions. Funds in distress, which experience large outflows, tend to decrease their existing positions (Coval and Stafford (2007)), including their IPO holdings. Hence, institutions that manage funds in distress are more likely to sell IPO shares. This suggests a candidate instrument for financial institutions' selling decisions: the number of funds in distress managed by the institution. This instrument is plausibly exogenous in this setting, as funds' distress events are likely unrelated with the probability that the institution trades through the lead under-

[12]In an unreported analysis, we perform DiD comparing the first month with all the other months during the first year after the issue, excluding month 2 and dropping IPOs issued after March 2010 because of lack of trading data in later months. Results are robust.

[13]We observe an institution j trading the same stock i through several distinct brokers b during the same trading day t for 23% of the observations.

writers of a given IPO.[14] Moreover, underwriters usually allocate shares to fund families, which then decide how to distribute them within the family (Ritter and Zhang (2007)). This lowers the scope for direct links between distressed funds and the institution's choice to trade through the underwriters in the IPO aftermarket.

We use clientcode-clientmgrcode pairs in the Abel Noser Solutions' database to identify distinct funds managed by our sample institutions.[15] We define a fund to be in distress in a given month if two conditions are met: 1) more than 99% of its trading volume in non-IPO stocks is due to sell trades; 2) the monthly dollar volume traded by the fund in non-IPO stoks is above the 90th percentile. The idea is that funds with large selling volumes are likely experiencing a fire-sales event. Our institution-level distress variable, $LnDistressFunds_{i,j}$, is the natural logarithm of the number of funds in distress managed by institution j during the month in which the IPO i is made. We use it as instrumental variable for $Sell$. Table 9 reports the 2SLS results, which are qualitatively consistent with our baseline regressions.

[Table 9 about here.]

The results of Table 9 have to be taken cautiously. We acknowledge that they are sensitive to the choice of the dollar volume threshold: the instrument becomes weak when we set lower thresholds, such as the 50th or the 75th percentiles of the monthly volume traded. Even though it make sense that only large transaction volumes are related to fire-sales events that could be relevant in the first stage regression, we cannot justify the choice of a specific volume threshold to build our variable. Table 9 suggests that endogeneity concerns do not seem to qualitatively change our conclusions, but the potential weakness of the instrument does not allow us to make strong causal statements.

4.1 Other robustness checks

We use a linear probability model (LPM) in our baseline regressions and we estimate its coefficients via OLS. We justify the use of OLS because the unconditional probability of trading with the lead underwriters is not at the boundaries of the unit interval (it is 0.292). Moreover, a very small proportion of the predicted probabilities of trading with the lead underwriters fall outside the [0, 1] interval and only one specification out of five suffers of this problem (see Table 3). Horrace and Oaxaca (2006) show that OLS is unbiased and consistent if all the observations have true predicted probabilities within the unit interval. We cannot know the true predicted

[14] A theoretically possible channel that could invalidate the exogeneity assumption is that institutions with several funds in distress might be institutions with little or no connections with important brokers, which also underwrite IPOs. Under this "connection" argument, institutions with distressed funds would tend to trade more with non-lead brokers regardless of the side of the trade. We find no evidence in this direction: the number of distressed funds of an institution is not significantly correlated with its normal number of trades executed through the lead underwriters in non-IPO stocks ($NormalTradeLead$).

[15] From our talks with ANcerno it became clear that clientmgrcode identifies individual funds, fund managers, or separately managed accounts (see also Hu et al. (2017)). Clientmgrcode is provided by the client and may change over time, ANcerno however reassured us that clientmgrcode remains unchaged within each a batch of data provided by the client (identified by the lognumber). For this reason, we follow Eisele et al. (2017) and use a couple clientcode-clientmgrcode to separate among individual funds.

probabilities, but our predicted probabilites do not raise suspect that potential OLS biasedness and inconsistency are relevant concerns in our setting. Finally, a LPM is desirable in our situation because it allows us to control for fixed effects without incurring in the incidental parameter problem and it estimates marginal effects. For robustness, we also run logit regressions and get rid of the fixed effects by means of a conditional logit model. Table 10 reports the estimation results, which are overall consistent with our baseline regressions.[16]

[Table 10 about here.]

Almost 50% of the IPOs in our sample are issued during the internet bubble period. We replicate our regression analysis excluding IPOs issued in 1999 and 2000 and report our findings in Table 11. The results are similar to those of our baseline regressions.

[Table 11 about here.]

We use *LeadDummy* as dependent variable in our baseline regressions. This implies that we pool in the same group of brokers the other syndicate members and brokers that do not belong to the underwriting syndicate. For robustness, we replicate our regression analysis using *UWDummy* as dependent variable. *UWDummy* takes the value of 1 if the trade is executed through any of the underwriters of the IPO and zero otherwise. Table 12 shows that results are overall consistent with our baseline regressions. If anything, they are slightly weaker, consistent with hiding incentives being mainly related to lead underwriters.

[Table 12 about here.]

5 Why do institutional investors hide their trades?

In this section, we investigate the drivers and motives of institutional investors' hiding behavior. The existing literature suggests that investors might try to hide their allocations sales in order to preserve their business with the lead underwriters in the IPO allocations market (Griffin et al. (2007), Chemmanur et al. (2010)). Though relevant, consistent, and sound, the incentive to hide allocation sales might be overall weak because of the lead underwriters' ability to infer flippers' identities: though imperfect, the flipping reports produced via the DTC IPO Tracking System dampen unambiguously the investors' chances to hide their allocation sales. We find evidence consistent with this view by introducing in our baseline regression of Section 3 the dummy variable *Flip*, which takes the value of one when the sell trade contains an allocation sale and zero otherwise.[17] *Flip* trades are a subset

[16] We cannot estimate all the specifications because of computational problems with the conditional logit model. In unreported analyses, we also run the LPM while trimming observations with predicted probabilities outside the unit interval, as suggested by Horrace and Oaxaca (2006). If anything, our results get stronger.

[17] Since we observe allocation sales at the institution-IPO-day level (see section 2.3), this definition of *Flip* may be inaccurate if investor j executes both secondary sales and allocation sales of IPO i during the same trading day t through several distinct brokers b. In our sample, this problem can affect at most 645 observations out of 44,576. In Table 13, we assume that all of these 645 sell trades contain an allocation sale. In unreported analyses, we exclude these 645 observations from the sample and find similar results.

of *Sell* trades, thus every *Flip* is a *Sell*, but not vice versa. For the sake of the interpretation of the regression coefficients, *Flip* is essentially an interaction variable because it can take the value of one only when *Sell* is also one.[18] Table 13 reports the results.

[Table 13 about here.]

Table 13 shows that the buy/sell asymmetry is mainly driven by sell trades other than allocation sales. The coefficient of *Flip* is positive and significant, meaning that sell trades are significantly more likely to be executed through the lead underwriters when they contain allocation sales than when they do not contain allocation sales. Nevertheless, there is evidence of some hiding activity also for flipping trades: sell trades that contain allocations sales are somewhat less likely to be executed through the lead underwriters than buy trades, as the coefficient of *Sell* plus the coefficient of *Flip* is a negative number.

We suggest a novel reason for why investors might have an incentive to hide their sell trades. An investor that enters in a laddering agreement a là Hao (2007) receives an IPO allocation and agrees with the lead underwriters to generate additional demand in the IPO aftermarket by buying shares. As argued by Griffin et al. (2007), this form of laddering helps explaining why investors are overall net buyers through the lead underwriters in the IPO aftermarket. However, investors might have an incentive to break the laddering agreement if the shares bought in the secondary market are in excess of their optimal holding in the IPO firm. A way to do it without being caught by the lead underwriters is to sell the shares in excess through any other broker. If investors sistematically break their laddering agreements, then we should observe them simultaneously buying through the lead underwriters and selling through non-lead brokers. Column (5) of Table 3 is consistent with this view: since we control for institution-firm fixed effects, we do observe institutional investors that simultaneously buy and sell an IPO being more likely to buy than sell through the lead underwriters. Table 3 detects a behavior which is consistent with investors breaking the laddering agreement, but it is silent on how relevant this hiding motive is. If laddering is a relevant hiding motive, we should observe a clear positive correlation between hiding behavior (i.e., selling through non-lead brokers) and buying through the lead underwriters.

In order to dig into the drivers of hiding behavior, we decompose trading volume in four parts. Let $V_{i,j}^T$ be the total number of shares traded by institution j in IPO i during the first 21 trading days after the issue and let N_i be the number of shares issued in IPO i. The total volume traded can be written as:

$$\frac{V_{i,j}^T}{N_i} = \frac{B_{i,j}^L}{N_i} + \frac{F_{i,j}^T}{N_i} + \frac{S_{i,j}^T - F_{i,j}^T}{N_i} + \frac{B_{i,j}^{NL}}{N_i}$$

where $F_{i,j}^T$ is the total number of shares of IPO i flipped by institution j during the first 21 trading days, $B_{i,j}^L$ ($S_{i,j}^L$) is the number of shares of IPO i bought (sold)

[18]This may raise a concern of collinearity between *Flip* and *Sell*. Standard regression diagnostic suggests that this is not the case. For robustness, we also modify our regression specification by including the variable *OnlySecondary* := *Sell* − *Flip* instead of *Sell*. *OnlySecondary* takes the value of one when the sell trade does not contain any allocation sales and zero otherwise. Results (unreported) are consistent.

by institution j through the lead underwriters during the first 21 trading days, $B^{NL}_{i,j}$ ($S^{NL}_{i,j}$) is the number of shares of IPO i bought (sold) by institution j through brokers other than the lead underwriters during the first 21 trading days, and $B^T = B^L + B^{NL}$ ($S^T = S^L + S^{NL}$). The third component on the right hand side of the identity, $(S^T_{i,j} - F^T_{i,j})/N_i$, is the institution's total volume of "secondary" shares sold, meaning total sales excluding allocations sales, scaled by the number of shares issued. In order to capture the propensity to sell through brokers other than the lead underwriters, we compute the percentage of shares of IPO i sold by institution j through non-lead brokers, $S^{NL}_{i,j}/S^T_{i,j}$. Since we are interested in analyzing selling hiding motives, we constrain our dataset to institutions that have positive sales (i.e., $S^T_{i,j} > 0$). We count 9,018 institution-firm observations.

Under the laddering motive for hiding, institutions tend to sell shares through non-lead brokers, while having bought them in the IPO aftermarket through the lead underwriters. Hence, controlling for how the institution normally trades with the lead underwriters ($NormalTradeLead$), we should observe the percentage of shares sold through non-lead brokers, $S^{NL}_{i,j}/S^T_{i,j}$, to be positively correlated with the relative volume of shares bought through the lead underwriters, $B^L_{i,j}/N_i$, and the relative volume of "secondary" shares sold, $(S^T_{i,j} - F^T_{i,j})/N_i$. These predictions are conditional on the institution j having received some allocation in the IPO i, as institutions involved in laddering received some allocation in the IPO. Hence, under the laddering motive, these predictions should not hold for institutions with no allocations. Moreover, they should not hold after the first month of trading, when there are no hiding incentives.

In order to test these predictions, we perform a linear projection of the propensity to sell through non-lead brokers on the trading volume components, running several specifications of the following regression:

$$\frac{S^{NL}_{i,j}}{S^T_{i,j}} = \gamma_0 + \gamma_1 \frac{B^L_{i,j}}{N_i} + \gamma_2 \frac{S^T_{i,j} - F^T_{i,j}}{N_i} + \gamma_3 \frac{F^T_{i,j}}{N_i} + \gamma_4 \frac{B^{NL}_{i,j}}{N_i} + X_{i,j}\Gamma + \phi_i + \varphi_j + v_{i,j}$$

where $X_{i,j}$ is a vector of control variables (which includes $NormalTradeLead_{i,j}$ and $AllocPerc_{i,j}$), ϕ_i and φ_j are firm and institution fixed effects, and $v_{i,j}$ is the error term, which we allow to be correlated within institution. The laddering motive for hiding predicts $\gamma_1 > 0$ and $\gamma_2 > 0$ for institutions that received allocations and trade during the first month after the issue. Table 14 reports the OLS results. All ratios are multiplied by 100, thus being expressed as percentages. We use institution-clustered standard errors for inference.[19]

[Table 14 about here.]

In columns (1)–(4), we perform the regression on first-month trading data, including in the sample institutions that received some allocations (i.e., institutions with $AllocPerc_{i,j} > 0$). Overall results are consistent with the laddering motive for hiding. The coefficients γ_1 and γ_2 are positive in all specifications: institutions

[19] In unreported analyses, we allow the error term to be correlated within IPO, clustering standard errors at the firm level. Results are consistent.

tend to execute a higher proportion of their sell trades through non-lead brokers when they buy more shares through the lead underwriters and when they sell more "secondary" shares. Looking at column (4), a one unit increase in the volume of shares bought through the lead underwriters (volume of "secondary" shares sold) as a percentage of the amount of shares issued is associated with a 1.24 (2.02) percentage points increase in the proportion of sell trades executed through non-lead brokers. Results are also statistically significant at the 1% level in most specifications. The specification in column (2), which does not control for fixed effects, shows insignificant or weakly significant results. Firm fixed effects keep IPO characteristics constant, including the identity of the lead underwriters, which might be relevant factors affecting both the propensity to sale and the amount of shares bought in the aftermarket through lead underwriters. For example, some underwriters might have simultaneously a higher proportion of sell trades executed through them and a larger buying activity from investors than other underwriters, thus making it difficult to detect the laddering hiding motive in specifications (1) and (2).[20] Controlling for IPO fixed effects allows us to keep these factors constant, exploiting within IPO variation. Hence, specifications (3) and (4) are more suitable tests of the laddering motive for hiding.

Consistent with flipping not being a relevant hiding motive, we find that γ_3 is negative in most specifications and statistically significant at the 1% level when controlling for IPO and institution fixed effects: the proportion of shares sold through non-lead brokers is lower when institutions flip more of their IPO allocations.

In column (5) we perform a placebo analysis, including in the sample only institutions with no IPO allocations (i.e., institutions with $AllocPerc_{i,j} = 0$). Consistent with the laddering motive for hiding, γ_1 and γ_2 are not statistically different from zero for institutions with no allocations; in addition, γ_1 enters the regression with a negative sign. In column (6) we perform another placebo analysis, running the regression on volumes traded during the third month after the issue. We include in the sample only institutions that received a positive allocation. Consistent with the laddering motive for hiding, γ_1 and γ_2 are not significantly positive after the first month of trading; both coefficients enter the regression with a negative sign. In addition, γ_1 is statistically significant, consistent with hiding incentives not being at place after the first month.

The remaining volume component, that is the relative amount of shares bought through brokers other than the lead underwriters ($BuyNonLead$ or $B_{i,j}^{NL}/N_i$), is in general positively correlated with the proportion of sell trades executed through the lead underwriters, especially in placebo samples. Intuitively, it makes sense: institutions that buy more through non-lead brokers also tend to sell more through non-lead brokers. Noticeably, this positive correlation disappears in specifications (3) and (4), where the laddering hiding motive becomes an important driver of institutions' behavior. Unsurprisingly, the coefficient of $NormalTradeLead$ is negative and significant in all specifications, including the placebo analyses: the higher the proportion of trades that the institution usually executes through the lead under-

[20] In an unreported analysis, we aggregate data at the lead underwriter level and, indeed, we observe a negative correlation between the proportion of sell trades through non-lead brokers and the volume components of interest, confirming the importance of controlling for IPO fixed effects in our regressions.

writers, the lower the proportion of sell trades executed through non-lead brokers in IPOs. *AllocPerc* enters the regression with a positive sign, but only during the first month of trading.

The laddering motive for hiding produces two other testable predictions. First, if institutions that enter in a laddering agreement break it, it has to be the case that they sell the shares that they bought through the lead underwriters. Hence, there should be a positive correlation between the volume bought through the lead underwriters and the volume of "secondary" shares sold. Second, since laddering involves an aggreement between the lead underwriter and the institution, an institution that engages in laddering is most likely a lead underwriter client. Hence, the lead underwriter is going to detect any of its flipping activities thanks to detailed flipping reports. Flipping trades signal to the lead underwriter that the institution sold more shares than it bought in the aftermarket. Hence, flipping trades reveal to the lead underwriter that the institution sold also "secondary" shares through other brokers, thus breaking the laddering agreement, if any. Therefore, institutions that buys through the lead underwriters and want to hide their "secondary" sales should avoid allocation sales, thus generating a negative correlation between flipping and buying through the lead underwriters. If these correlations are driven by hiding incentives related to laddering, then they should hold only for institutions that received IPO allocations and trade during the first month after the issue.

In order to test these predictions, we regress the amount of net buy through the lead underwriters on the other trading volume components:

$$\frac{B_{i,j}^L - S_{i,j}^L}{N_i} = \theta_0 + \theta_1 \frac{S_{i,j}^T - F_{i,j}^T}{N_i} + \theta_2 \frac{F_{i,j}^T}{N_i} + \theta_3 \frac{B_{i,j}^{NL}}{N_i} + X_{i,j}\Gamma + \kappa_j + \eta_i + \varepsilon_{i,j}$$

where $X_{i,j}$ is a vector of control variables (which includes $NormalTradeLead_{i,j}$ and $AllocPerc_{i,j}$), κ_j and η_i are institution and firm fixed effects, and $\varepsilon_{i,j}$ is the error term, which we allow to be correlated within institution. The laddering motive for hiding predicts $\theta_1 > 0$ and $\theta_2 < 0$ for institutions that received allocations and trade during the first month after the issue. We use net buy, $(B_{i,j}^L - S_{i,j}^L)/N_i$, instead of total buy, $B_{i,j}^L/N_i$, as dependent variable in order to prevent a mechanical correlation between $B_{i,j}^L/N_i$ and $(S_{i,j}^T - F_{i,j}^T)/N_i$ to arise when we control for $B_{i,j}^{NL}/N_i$ in the regression. Table 15 reports the OLS results. All ratios are multiplied by 100, thus being expressed as percentages. We use institution-clustered standard errors for inference.[21]

[Table 15 about here.]

In columns (1)–(4), we perform the regression on first-month trading data, including in the sample institutions that received some allocations (i.e., institutions with $AllocPerc_{i,j} > 0$). Overall, results are consistent with the laddering motive for hiding. The coefficient θ_1 (θ_2) is positive (negative) and statistically significant at the 1% level in all specifications. Column (4) reports that selling an additional "secondary" share-per-shares-issued is associated with an increase in the net buying

[21]In unreported analyses, we allow the error term to be correlated within IPO, clustering standard errors at the firm level. Results are consistent.

through the lead underwriters of 0.33 shares-per-share-issued; flipping an additional share-per-shares-issued is associated with a decrease in the net buying through the lead underwriters of 0.4 shares-per-share-issued.

We perform placebo analyses running the regression on institutions with no allocations (column (5)) and institutions that trade during the third month after the issue (column (6)). Consistent with the correlations in columns (1)-(4) being driven by laddering hiding motives, the coefficients θ_1 and θ_2 are either insignificant or with the opposite sign in the placebo regressions.

The remaining volume component, that is the relative amount of shares bought through brokers other than the lead underwriters ($BuyNonLead$ or $B_{i,j}^{NL}/N_i$), is positively correlated with the net buy through the lead underwriters in month 3. Intuitively, it makes sense: institutions that buy a stock, buy it through any broker they trade with. Noticeably, this positive correlation disappears in specifications (1)-(4): when laddering and hiding incentives are at place, buying through lead underwriters is not anymore correlated with buying through non-lead brokers. Intriguingly, lead underwriters' usual clients have a significantly lower net buy through the lead underwriters: $NormalTradeLead$ enters the regression with a negative sign. Instead, the coefficient of $AllocPerc$ is positive and statistically significant in month 1, while being insignificant in month 3. This is broadly consistent with laddering practices (Griffin et al. (2007)): institutions with higher allocations tend to buy more shares through the lead underwriters in the IPO aftermarket. However, an alternative interpretation could be that institutions receive rationed IPO allocations and buy more shares in the aftermarket to reach their optimal holdings. Since we do not observe institutions' bidding behavior, we cannot draw conclusions about the interpretation of the positive correlation between net buy and IPO allocations.

Overall, our evidence suggests that, contrary to the conventional view, flipping does not seem to be an important motive for hiding sell trades from the lead underwriters. Instead, we find evidence consistent with the laddering motive being a relevant driver of institutions' hiding behavior.

6 Conclusion

We document that institutional investors are less likely to sell than buy through the lead underwriters in the aftermarket of IPOs issued between 1999 and 2010 in the United States. The probability of trading through a lead underwriter during the first month after the issue is about 6 percentage points less for sell trades than for buy trades. This result holds when controlling for important determinants of the choice to trade with a lead underwriter, such as the relationship between the institution and the lead underwriters, and is robust to institution, IPO, and institution-IPO fixed effects. We find that the documented buy/sell asymmetry varies consistently with hiding incentives: it is stronger when the aftermarket demand for IPO stocks is weaker (i.e., in cold IPOs), it does not hold after the first month of trading, and it does not hold for a matched sample of non-IPO stocks.

We rule out potential alternative explanations for the buy/sell asymmetry. Our findings are not driven by underwriters' strategically setting differential brokerage commissions to disincetivize sell trades. Moreover, our evidence suggests that the buy/sell asymmetry is not only driven by investors buying more through the lead underwriters, but also by investors selling less through the lead underwriters. Finally, potential endogeneity concerns are unlikely to make the buy/sell asymmetry vanish and we find evidence consistent with this view in an IV setting, using a proxy for institutional fire-sales as exogenous shock for the decision to sell an IPO.

We investigate the motives behind institutional investors' hiding behavior. Contrary to the conventional view, we find that flipping IPO allocations is not an important motive for hiding sell trades from the lead underwriters. This is reasonable, as underwriters have access to reports that document investors' flipping activity. We propose a novel hiding motive and find evidence in favor of it. Institutional investors that agree with the underwriters to buy additional shares in the IPO aftermarket in exchange of receiving allocations (a practice known as "laddering"), might break this agreement by hiding-and-selling the shares bought in the aftermarket through other brokers. Consistent with the laddering motive for hiding, we find that: *i)* the percentage of sell volume executed through non-lead brokers is higher when institutional investors buy more shares through the lead underwriters in the IPO aftermarket and when institutional investors execute more "secondary" sales (i.e., sales other than allocation sales); and *ii)* the volume of "secondary" shares sold in the aftermarket by an institution is positively correlated with its net buy volume through the lead underwriters. Moreover, an investor that hide his/her sell trades because of laddering motives should avoid flipping trades. These trades signal to the lead underwriters that the investor sold more shares than it bought in the aftermarket, thus revealing that it has broken the laddering agreement. Consistently, we find a negative correlation between the flipping volume of an institution and its net buy volume through the lead underwriters.

Our evidence sheds light on how hiding incentives affect institutions' choice of their broker in the IPO aftermarket and stimulates further research to investigate how the incentives of IPO investors may influence the IPO allocation process.

A Appendix

This data appendix provides a detailed description of ANcerno data inspired by years of exchanges with the data provider, as well as the explanation of the mapping procedure we use to produce the dataset. Our sample consists of institutional transaction-level trading data from ANcerno/Abel Noser Solutions. ANcerno clients (money managers, pension plan sponsors, and brokers) provide their trading data to ANcerno to monitor their transaction costs. Each client has a unique numerical identifier in the dataset (*clientcode*) that allows distinguishing among the three types of clients. Nevertheless, the identity of the client is anonymized. We use *clientcode* mainly as a technical variable in several matching exercises we perform. One of the main variables of interest to us is *managercode* by ANcerno attributed to the trading institutions. After receiving data from their clients, ANcerno assigns a code to each manager within the clients portfolio. Because several clients may use the same manager, in order to associate a manager with a particular client, ANcerno codes the manager in relation to a client. Another reason they do this is because different clients may report the same managers differently (e.g., different spelling). By coding the manager in relation to a customer, ANcerno can trace back the manager to a particular client. Managers can be grouped across clients by using the *managercode*. ANcerno uses the same logic in mapping executing brokers in the data. The main ANcerno trading dataset includes *clientcode*, *clientmgrcode* and *clientbkrcode* we use in our matching process.

ANcerno data is subscription specific. For a limited period of time in 2010, ANcerno provided its academic subscribers with the identification table "MasterManagerXref" that includes *managercodes* with the associated names of trading institutions. The file we got includes 1088 unique institutions. Additional identification files "ManagerXref" and "BrokerXref" include *clientcode*, *clientmgrcode*, and *clientbkrcode* variables allowing to link fund families and brokers to the trading data in the main ANcerno dataset. The mapping procedure we use is shown in detail in Figure 5. Figure 5 shows the two-step matching we use to get the managing company name on the main ANcerno trading dataset. In the first step, we merge "ManagerXref" file on the main ANcerno table using *clientcode-clientmgrcode* as a key identifier. We further link the resulting table with the managing company name (variable *manager*) from the "MasterManagerXref" file on provided (*managercodes*).

We use the S12type5 Table provided by Wharton Research Data Services (WRDS) to map management companies from SEC 13F filings to mutual funds reporting their holdings in the Thomson Reuters S12 Mutual fund holdings database. S12 data contains funds associated to fund families in 13F. Finally, we match ANcerno institutions with the institutions from S12/13F Thomson Reuters database. We manually match managing company names from both datasets: variable *manager* in ANcerno and *mgrco* in S12 database.

[Figure 5 about here.]

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Table 1. This table presents summary statistics for institutional trades in IPOs during the first year from the issue. Trades refer to executions of orders placed by institutions in the database. Columns 2-13 disaggregate summary statistics by month from the issue date; column 14 reports summary statistics for the first year after the IPO.

	month1	month2	month3	month4	month5	month6	month7	month8	month9	month10	month11	month12	All
No. of institutions	167	154	155	168	164	181	185	179	182	190	184	188	227
No. of brokers	448	362	355	370	369	376	381	387	373	371	392	380	700
No. of trades (thousands)	231.2	76.6	75.3	77.3	81.0	94.4	91.6	94.4	116.3	114.8	96.3	105.3	1255
Tot. Share volume (millions)	1581	479	477	440	493	623	696	663	677	671	715	719	8236
Tot. Dollar volume ($billion)	39.1	13	13.3	13.8	16.6	19.2	23.1	21.7	25.3	22.5	22.1	22.1	251.9
Tot. Commissions ($million)	45.7	12.4	13.4	11.2	13.3	15.4	15.8	14.4	14.1	15.1	17.4	16.7	204.9
Mean share volume per trade	6840	6255	6336	5697	6089	6600	7600	7021	5821	5843	7431	6829	6565
% volume lead underwriters	40.4%	23.1%	23.3%	20.3%	20.4%	19.1%	19.3%	15.3%	16.3%	15.2%	16.5%	14.7%	22.3%
% volume syndicate members	7.3%	14.6%	13.9%	13.8%	16.4%	15.5%	15.8%	15.5%	14.3%	14.0%	16.1%	16.6%	14.0%
% volume other brokers	52.4%	62.3%	62.8%	65.9%	63.2%	65.3%	65.0%	69.2%	69.5%	70.9%	67.5%	68.7%	63.4%

Table 2. This table provides IPO allocations summary statistics at the institution level (*AllocPerc*) and issuer level (Number of Allocations; Total % Institutional Allocation). *AllocPerc* is the percentage of IPO shares allocated to an institution (winsorized at the 95% level). The table reports summary statistics for all IPOs and for subsamples of IPOs based on *Underpricing* terciles: hot IPOs (highest tercile), weak IPOs (middle tercile), and cold IPOs (lowest tercile). For each variable, the table reports its average (mean), its median (p50), and its standard deviation (sd).

	mean	p50	sd
AllocPerc (all IPOs)	1.89	0.54	3.05
AllocPerc (hot IPOs)	1.53	0.40	2.71
AllocPerc (weak IPOs)	1.98	0.67	3.02
AllocPerc (cold IPOs)	2.53	0.78	3.67
Number of Allocations (all IPOs)	22.7	21	14.4
Number of Allocations (hot IPOs)	30.6	30	13.9
Number of Allocations (weak IPOs)	23.7	22	13.6
Number of Allocations (cold IPOs)	13.6	12	10.1
Total % Institutional Allocation (all IPOs)	42.7	42.5	21.7
Total % Institutional Allocation (hot IPOs)	47.0	45.6	23.1
Total % Institutional Allocation (weak IPOs)	46.9	47.3	20.6
Total % Institutional Allocation (cold IPOs)	34.3	33.9	18.7

Table 3. This table reports the estimation results of several specification of a linear probability model in a sample of institutional trades in 1,361 IPO stocks issued between 1999 and 2010. The dependent variable is a dummy equal to one if the broker executing the trade is any of the lead underwriters of the IPO (*LeadDummy*). Panel (A) includes 44,576 trades executed in the first 21 trading days after the issue date; Panel (B) includes 24,891 trades executed in the first 7 trading days after the issue date. Column (1) reports the results of an OLS regression of *LeadDummy* on a dummy variable equal to one if the institution is selling and zero otherwise (*Sell*). Column (2) introduces several control variables: $RelVol$ is the number of shares traded by the institution scaled by the number of shares issued; $NormalTradeLead$ is the percentage volume of sell or buy trades in non-IPO stocks made by the institution through the lead underwriters in a 6-month period prior to the issue; *Day* is the day in which the trade is executed, relative to the issue date; *ExcLeadComm* is the average percentage commission to the lead underwriters minus the average percentage commission to any other broker paid by sample institutions for their buy or sell trades in the first 21 trading days after the issue date; *AllocPerc* is the percentage IPO allocation received by the institution. Columns (3), (4), and (5) introduce institution, firm, and institution-firm fixed effects. All non-dummy variables are winsorized at the 95% level. Standard errors are clustered at the institution level (t-statistics are in parentheses). Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

(A) First 21 trading days after the issue date

	(1)	(2)	(3)	(4)	(5)
Sell	-0.060**	-0.078***	-0.053***	-0.057***	-0.052**
	(-2.08)	(-3.62)	(-3.15)	(-2.97)	(-2.44)
RelVol		0.070**	0.14***	0.13***	0.14***
		(2.44)	(6.72)	(8.06)	(7.12)
NormalTradeLead		0.0086***	0.0092***	0.0094***	0.0025
		(5.20)	(9.55)	(7.40)	(0.64)
Day		-0.011***	-0.0092***	-0.0089***	-0.0081***
		(-5.78)	(-6.02)	(-5.76)	(-4.77)
ExcLeadComm		-0.17	-0.16	-0.057	-0.25**
		(-1.13)	(-1.53)	(-0.49)	(-2.29)
AllocPerc		-0.00028	0.0021	0.0024*	
		(-0.06)	(1.41)	(1.82)	
Constant	0.32***	0.35***	0.30***	0.18***	0.33***
	(9.91)	(10.12)	(21.54)	(9.49)	(17.34)
Institution fixed effects	No	No	Yes	Yes	No
Firm fixed effects	No	No	No	Yes	No
Institution-Firm fixed effects	No	No	No	No	Yes
Adjusted R2	0.0043	0.049	0.15	0.26	0.45
Observations	44576	44576	44576	44576	44576
% Outside [0,1]	0	0	0	0.080	0

(B) First 7 trading days after the issue date

	(1)	(2)	(3)	(4)	(5)
Sell	-0.11***	-0.13***	-0.080***	-0.095***	-0.089***
	(-5.09)	(-5.59)	(-3.03)	(-3.46)	(-2.63)
Controls	No	Yes	Yes	Yes	Yes
Institution fixed effects	No	No	Yes	Yes	No
Firm fixed effects	No	No	No	Yes	No
Institution-Firm fixed effects	No	No	No	No	Yes
Adjusted R2	0.014	0.044	0.16	0.28	0.44
Observations	24891	24891	24891	24891	24891
% Outside [0,1]	0	0	0	0.077	0

Table 4. This table reports the estimation results of several specification of a linear probability model in a sample of institutional trades in 1,361 IPO stocks issued between 1999 and 2010. The dependent variable is a dummy equal to one if the broker executing the trade is any of the lead underwriters of the IPO (*LeadDummy*). The table includes 44,576 trades executed in the first 21 trading days after the issue date. Column (1) reports the results of an OLS regression of *LeadDummy* on a dummy variable equal to one if the institution is selling and zero otherwise (*Sell*), a dummy variable equal to one if the IPO is in the lowest tercile of *Underpricing* (*ColdIPO*), and an interaction variable between the two (*Sell*ColdIPO*). Column (2) introduces several control variables: *RelVol* is the number of shares traded by the institution scaled by the number of shares issued; *NormalTradeLead* is the percentage volume of sell or buy trades in non-IPO stocks made by the institution through the lead underwriters in a 6-month period prior to the issue; *Day* is the day in which the trade is executed, relative to the issue date; *ExcLeadComm* is the average percentage commission to the lead underwriters minus the average percentage commission to any other broker paid by sample institutions for their buy or sell trades during the third month after the issue date; *AllocPerc* is the percentage IPO allocation received by the institution. Columns (3), (4), and (5) introduce institution, firm, and institution-firm fixed effects. All non-dummy variables are winsorized at the 95% level. Standard errors are clustered at the institution level (t-statistics are in parentheses). Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5)
Sell	-0.051*	-0.072***	-0.044**	-0.048**	-0.041*
	(-1.66)	(-3.06)	(-2.42)	(-2.43)	(-1.88)
Sell*ColdIPO	-0.066***	-0.053**	-0.065***	-0.053***	-0.064***
	(-2.71)	(-2.46)	(-3.47)	(-3.10)	(-3.23)
ColdIPO	0.0018	-0.011	-0.00063		
	(0.09)	(-0.56)	(-0.05)		
Controls	No	Yes	Yes	Yes	Yes
Institution fixed effects	No	No	Yes	Yes	No
Firm fixed effects	No	No	No	Yes	No
Institution-Firm fixed effects	No	No	No	No	Yes
Adjusted R2	0.0053	0.050	0.15	0.26	0.45
Observations	44576	44576	44576	44576	44576
% Outside [0,1]	0	0.00060	0	0.081	0

Table 5. This table reports the estimation results of several specification of a linear probability model in a sample of institutional trades in 1,361 IPO stocks issued between 1999 and 2010. The dependent variable is a dummy equal to one if the broker executing the trade is any of the lead underwriters of the IPO (*LeadDummy*). The table includes 24,643 trades executed during the third trading mont after the issue date. Column (1) reports the results of an OLS regression of *LeadDummy* on a dummy variable equal to one if the institution is selling and zero otherwise (*Sell*). Column (2) introduces several control variables: $RelVol$ is the number of shares traded by the institution scaled by the number of shares issued; $NormalTradeLead$ is the percentage volume of sell or buy trades in non-IPO stocks made by the institution through the lead underwriters in a 6-month period prior to the issue; *Day* is the day in which the trade is executed, relative to the issue date; *ExcLeadComm* is the average percentage commission to the lead underwriters minus the average percentage commission to any other broker paid by sample institutions for their buy or sell trades during the third month after the issue date; *AllocPerc* is the percentage IPO allocation received by the institution. Columns (3), (4), and (5) introduce institution, firm, and institution-firm fixed effects. All non-dummy variables are winsorized at the 95% level. Standard errors are clustered at the institution level (t-statistics are in parentheses). Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5)
Sell	0.029	0.025	0.0061	0.0048	-0.0013
	(1.35)	(1.51)	(0.63)	(0.62)	(-0.13)
Controls	No	Yes	Yes	Yes	Yes
Institution fixed effects	No	No	Yes	Yes	No
Firm fixed effects	No	No	No	Yes	No
Institution-Firm fixed effects	No	No	No	No	Yes
Adjusted R2	0.0015	0.030	0.091	0.26	0.48
Observations	24643	24643	24643	24643	24643
% Outside [0,1]	0	0	0	0.15	0

Table 6. This table reports the estimation results of several specification of a linear probability model in a sample of institutional trades in non-IPO stocks matched to 1,109 IPOs issued between 1999 and 2010. The dependent variable is a dummy equal to one if the broker executing the trade is any of the lead underwriters of the matched IPO (*LeadDummy*). The table includes 28,990 trades executed during the first 21 trading days after the issue date of the matched IPO. Column (1) reports the results of an OLS regression of *LeadDummy* on a dummy variable equal to one if the institution is selling and zero otherwise (*Sell*). Column (2) introduces several control variables: $RelVol$ is the number of shares traded by the institution scaled by the number of shares outstanding; $NormalTradeLead$ is the percentage volume of sell or buy trades in non-IPO stocks made by the institution through the lead underwriters in a 6-month period prior to the issue; *Day* is the day in which the trade is executed, relative to the issue date of the matched IPO; *ExcLeadComm* is the average percentage commission to the lead underwriters minus the average percentage commission to other brokers paid by sample institutions for their buy or sell trades during the first 21 trading days after the issue date in the matched non-IPO stock; *Holdings* is the number of shares held by the institution in the non-IPO stock at the first filing date prior to the issue date scaled by the number of shares outstanding. Columns (3), (4), and (5) introduce institution, firm, and institution-firm fixed effects. All non-dummy variables are winsorized at the 95% level. Standard errors are clustered at the institution level (t-statistics are in parentheses). Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5)
Sell	0.0030	0.0066	0.0076*	0.0061*	-0.0010
	(0.33)	(1.28)	(1.70)	(1.85)	(-0.27)
Controls	No	Yes	Yes	Yes	Yes
Institution fixed effects	No	No	Yes	Yes	No
Firm fixed effects	No	No	No	Yes	No
Institution-Firm fixed effects	No	No	No	No	Yes
Adjusted R2	0	0.10	0.11	0.21	0.44
Observations	28990	28990	28990	28990	28990
% Outside [0,1]	0	0.16	0.16	0.32	0.0013

Table 7. This table reports difference of means tests for the percentage trading commission paid to lead underwriters and to any other broker by financial institutions in IPOs issued between 1999 and 2010. The sample includes 20,107 sell trades and 24,469 buy trades executed during the first month after the issue date. The percentage trading commission paid by an institution to the broker is winsorized at the 95% level. Standard errors are corrected for unequal variances (t-statistics are in parentheses). Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	All others	Lead UWs	Diff. of means
% sell commissions	0.0886	0.0895	-0.000869
			(-0.472)
% buy commissions	0.109	0.122	-0.0124***
			(-6.814)

Table 8. This table reports Difference-in-Differences estimation results. The dependent variable is $SV_{i,m,l}$, which is the total number of shares of IPO i sold by financial institutions in month m through a broker of type l. The volume is divided by the number of shares issued and multiplied by 100. We consider trades executed in month 1 and month 3 after the IPO; $Month1_{i,m}$ is a dummy variable equal to one if the shares are sold in month 1 and zero otherwise. We consider trades executed through two types of brokers: lead underwriters and brokers that do not belong to the underwriting syndicate; $LeadDummy_{i,l}$ is a dummy variable equal to one 1 if the shares are sold through the lead underwriters and zero otherwise. $LeadDummy^{*}Month1_{i,m,l}$ is the interaction between the two variables. The sample includes 1,227 IPOs issued between 1999 and 2010 that have a non-zero selling activity during the first month or the third month of trading after the offer. Standard errors are clustered at the IPO level (t-statistics are in parentheses). Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	(1)
LeadDummy	-0.56***
	(-8.53)
Month1	1.67***
	(16.85)
LeadDummy*Month1	-0.64***
	(-5.46)
Constant	0.87***
	(15.09)
Adjusted R2	0.12
Observations	4908

Table 9. This table reports the estimation results of several specification of a 2SLS regression in a sample of institutional trades in 1,361 IPO stocks issued between 1999 and 2010. The dependent variable is a dummy equal to one if the broker executing the trade is any of the lead underwriters of the IPO (*LeadDummy*). The sample includes 44,576 trades executed in the first 21 trading days after the issue date. Panel (A) reports the first stage results; Panel (B) reports the second stage results. Column (1) reports the results of a 2SLS regression of *LeadDummy* on a dummy variable equal to one if the institution is selling and zero otherwise (*Sell*), instrumented by $LnDistressFunds$. $LnDistressFunds$ is the natural logarithm of the number of funds managed by the institution that are in distress. A fund is defined to be in distress if: 1) its total volume traded in all stocks in the IPO month is more than 25 million dollars and 2) its total dollar netbuy in all stocks divided by the total volume traded is less than -0.99. Column (2) introduces several control variables: $RelVol$ is the number of shares traded by the institution scaled by the number of shares issued; $NormalTradeLead$ is the percentage volume of sell or buy trades in non-IPO stocks made by the institution through the lead underwriters in a 6-month period prior to the issue; *Day* is the day in which the trade is executed, relative to the issue date; *ExcLeadComm* is the average percentage commission to the lead underwriters minus the average percentage commission to any other broker paid by sample institutions for their buy or sell trades in the first 21 trading days after the issue date; *AllocPerc* is the percentage IPO allocation received by the institution. Columns (3) and (4) introduce institution and firm fixed effects. All non-dummy variables are winsorized at the 95% level. Standard errors are clustered at the institution level (t-statistics are in parentheses). Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

(A) First stage

	(1)	(2)	(3)	(4)
LnDistressFunds	0.11***	0.13***	0.054***	0.030***
	(3.17)	(3.98)	(8.02)	(4.22)
Controls	No	Yes	Yes	Yes
Institution fixed effects	No	No	Yes	Yes
Firm fixed effects	No	No	No	Yes
F-stat	10.0	70.3	96.4	.
Adjusted R2	0.0058	0.067	0.18	0.31
Observations	44576	44576	44576	44576

(B) Second stage

	(1)	(2)	(3)	(4)
Sell	-1.32***	-1.12***	-0.56***	-1.35*
	(-3.92)	(-5.26)	(-3.02)	(-1.65)
Controls	No	Yes	Yes	Yes
Institution fixed effects	No	No	Yes	Yes
Firm fixed effects	No	No	No	Yes
Observations	44576	44576	44576	44576

Table 10. This table reports the coefficient estimates of logit and conditional logit models in a sample of institutional trades in 1,361 IPO stocks issued between 1999 and 2010. The dependent variable is a dummy equal to one if the broker executing the trade is any of the lead underwriters of the IPO (*LeadDummy*). The original sample includes 44,576 trades executed in the first 21 trading days after the issue date. Column (1) reports the results of a logit regression of *LeadDummy* on a dummy variable equal to one if the institution is selling and zero otherwise (*Sell*). Column (2) introduces several control variables: $RelVol$ is the number of shares traded by the institution scaled by the number of shares issued; $NormalTradeLead$ is the percentage volume of sell or buy trades in non-IPO stocks made by the institution through the lead underwriters in a 6-month period prior to the issue; *Day* is the day in which the trade is executed, relative to the issue date; $ExcLeadComm$ is the average percentage commission to the lead underwriters minus the average percentage commission to other brokers paid by sample institutions for their buy or sell trades in the first 21 trading days after the issue date; *AllocPerc* is the percentage IPO allocation received by the institution. Column (3) controls for institution-firm fixed effects by means of a conditional logit model. All non-dummy variables are winsorized at the 95% level. Standard errors are clustered at the institution level (t-statistics are in parentheses). Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	(1)	(2)	(3)
Sell	-0.29**	-0.40***	-0.37**
	(-2.07)	(-3.64)	(-2.44)
RelVol		0.31**	1.04***
		(2.57)	(7.60)
NormalTradeLead		0.041***	0.019
		(4.92)	(0.78)
Day		-0.059***	-0.069***
		(-6.09)	(-5.07)
ExcLeadComm		-0.87	-1.56*
		(-1.15)	(-1.79)
AllocPerc		-0.0011	
		(-0.05)	
Constant	-0.76***	-0.62***	
	(-5.12)	(-4.08)	
Institution-Firm fixed effects	No	No	Yes
Pseudo R2	0.0036	0.041	0.078
Observations	44576	44576	21693

Table 11. This table reports the estimation results of several specification of a linear probability model in a sample of institutional trades in 698 IPO stocks issued between 2001 and 2010. The dependent variable is a dummy equal to one if the broker executing the trade is any of the lead underwriters of the IPO (*LeadDummy*). The sample includes 24,109 trades executed in the first 21 trading days after the issue date. Column (1) reports the results of an OLS regression of *LeadDummy* on a dummy variable equal to one if the institution is selling and zero otherwise (*Sell*). Column (2) introduces several control variables: $RelVol$ is the number of shares traded by the institution scaled by the number of shares issued; $NormalTradeLead$ is the percentage volume of sell or buy trades in non-IPO stocks made by the institution through the lead underwriters in a 6-month period prior to the issue; *Day* is the day in which the trade is executed, relative to the issue date; *ExcLeadComm* is the average percentage commission to the lead underwriters minus the average percentage commission to other brokers paid by sample institutions for their buy or sell trades in the first 21 trading days after the issue date; *AllocPerc* is the percentage IPO allocation received by the institution. Columns (3), (4), and (5) introduce institution, firm, and institution-firm fixed effects. All non-dummy variables are winsorized at the 95% level. Standard errors are clustered at the institution level (t-statistics are in parentheses). Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5)
Sell	-0.052**	-0.067***	-0.060***	-0.055***	-0.050**
	(-2.27)	(-3.48)	(-3.78)	(-3.43)	(-2.34)
Controls	No	Yes	Yes	Yes	Yes
Institution fixed effects	No	No	Yes	Yes	No
Firm fixed effects	No	No	No	Yes	No
Institution-Firm fixed effects	No	No	No	No	Yes
Adjusted R2	0.0032	0.063	0.15	0.24	0.40
Observations	24109	24109	24109	24109	24109
% Outside [0,1]	0	0.0016	0.00040	0.072	0

Table 12. This table reports the estimation results of several specification of a linear probability model in a sample of institutional trades in 1361 IPO stocks issued between 1999 and 2010. The dependent variable is a dummy equal to one if the broker executing the trade is any of the underwriters of the IPO ($UWDummy$). The sample includes 44,576 trades executed in the first 21 trading days after the issue date. Column (1) reports the results of an OLS regression of $UWDummy$ on a dummy variable equal to one if the institution is selling and zero otherwise (*Sell*). Column (2) introduces several control variables: $RelVol$ is the number of shares traded by the institution scaled by the number of shares issued; $NormalTradeUW$ is the percentage volume of sell or buy trades in non-IPO stocks made by the institution through the underwriters in a 6-month period prior to the issue; *Day* is the day in which the trade is executed, relative to the issue date; $ExcUWComm$ is the average percentage commission to the underwriters minus the average percentage commission to other brokers paid by sample institutions for their buy or sell trades in the first 21 trading days after the issue date; *AllocPerc* is the percentage IPO allocation received by the institution. Columns (3), (4), and (5) introduce institution, firm, and institution-firm fixed effects. All non-dummy variables are winsorized at the 95% level. Standard errors are clustered at the institution level (t-statistics are in parentheses). Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5)
Sell	-0.066**	-0.080***	-0.051***	-0.049**	-0.044*
	(-2.07)	(-3.33)	(-2.90)	(-2.59)	(-1.85)
Controls	No	Yes	Yes	Yes	Yes
Institution fixed effects	No	No	Yes	Yes	No
Firm fixed effects	No	No	No	Yes	No
Institution-Firm fixed effects	No	No	No	No	Yes
Adjusted R2	0.0047	0.053	0.14	0.25	0.42
Observations	44576	44576	44576	44576	44576
% Outside [0,1]	0	0	0	0.042	0

Table 13. This table reports the estimation results of several specification of a linear probability model in a sample of institutional trades in 1,361 IPO stocks issued between 1999 and 2010. The sample includes 44,576 trades executed in the first 21 trading days after the issue date. The dependent variable is a dummy equal to one if the broker executing the trade is any of the lead underwriters of the IPO (*LeadDummy*). Column (1) reports the results of an OLS regression of *LeadDummy* on a dummy variable equal to one if the institution is selling and zero otherwise (*Sell*) and a dummy variable equal to one if the sell trade contains an allocation sale and zero otherwise (*Flip*). Column (2) introduces several control variables: $RelVol$ is the number of shares traded by the institution scaled by the number of shares issued; $NormalTradeLead$ is the percentage volume of sell or buy trades in non-IPO stocks made by the institution through the lead underwriters in a 6-month period prior to the issue; *Day* is the day in which the trade is executed, relative to the issue date; *ExcLeadComm* is the average percentage commission to the lead underwriters minus the average percentage commission to any other broker paid by sample institutions for their buy or sell trades in the first 21 trading days after the issue date; *AllocPerc* is the percentage IPO allocation received by the institution. Columns (3), (4), and (5) introduce institution, firm, and institution-firm fixed effects. All non-dummy variables are winsorized at the 95% level. Standard errors are clustered at the institution level (t-statistics are in parentheses). Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5)
Sell	-0.15***	-0.14***	-0.087***	-0.081***	-0.076***
	(-5.40)	(-5.65)	(-4.47)	(-3.84)	(-3.62)
Flip	0.12***	0.074***	0.045***	0.034***	0.059***
	(5.71)	(3.98)	(3.94)	(2.88)	(2.76)
Controls	No	Yes	Yes	Yes	Yes
Institution fixed effects	No	No	Yes	Yes	No
Firm fixed effects	No	No	No	Yes	No
Institution-Firm fixed effects	No	No	No	No	Yes
Adjusted R2	0.0093	0.051	0.15	0.26	0.45
Observations	44576	44576	44576	44576	44576
% Outside [0,1]	0	0.00070	0	0.080	0

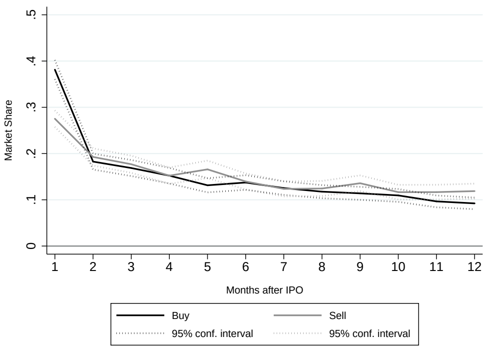
Table 14. This table reports the estimates of an OLS regression of the volume of sales executed through the lead underwriter as a percentage of total sales $[S^{NL}_{i,j}/S^{T}_{i,j}]$ on the trading volume components scaled by the number of shares issued: *BuyLead* is the relative number of shares bought through the lead underwriters $[B^{L}_{i,j}/N_i]$; *OtherSales* is the relative volume of sales other than allocation sales $[(S^{T}_{i,j} - F^{T}_{i,j})/N_i]$; *SharesFlipped* is the relative number of shares flipped $[F^{T}_{i,j}/N_i]$; and *BuyNonLead* is the relative number of shares bought through non-lead brokers $[B^{NL}_{i,j}/N_i]$. Control variables are described in Table 3. All ratios are multiplied by 100. Columns (1)-(4) include trades executed during the first month after the issue by financial institutions that received an IPO allocation. Column (5) includes trades executed during the first month after the issue by financial institutions with no IPO allocations. Column (6) includes trades executed during the third month after the issue by financial institutions that received an IPO allocation. Standard errors are clustered at the institution level (t-statistics are in parentheses). Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5) Placebo	(6) Placebo
BuyLead	1.12*	0.41	1.64***	1.24***	-0.37	-2.24**
	(1.75)	(0.58)	(4.60)	(3.50)	(-0.23)	(-2.08)
OtherSales	1.74***	1.22*	3.11***	2.02***	0.92	-0.96
	(2.77)	(1.78)	(3.29)	(2.83)	(0.42)	(-1.01)
SharesFlipped	0.23	-1.48*	-0.14	-1.93***		0.41
	(0.28)	(-1.87)	(-0.14)	(-3.15)		(0.45)
BuyNonLead	1.55***	1.18**	0.40	-0.045	1.56*	3.30***
	(2.71)	(2.52)	(0.80)	(-0.11)	(1.93)	(4.96)
NormalTradeLead	-3.81***	-3.86***	-4.43***	-3.96***	-2.17*	-3.12***
	(-14.61)	(-15.06)	(-19.52)	(-18.52)	(-1.95)	(-9.04)
AllocPerc		1.99***	1.41***	0.82***		0.24
		(6.90)	(4.37)	(3.21)		(1.15)
Constant	75.2***	73.9***	75.4***	62.2***	84.0**	78.7***
	(34.32)	(33.25)	(33.91)	(16.55)	(2.60)	(11.63)
Institution fixed effects	No	No	Yes	Yes	Yes	Yes
Firm fixed effects	No	No	No	Yes	Yes	Yes
Adjusted R2	0.20	0.20	0.32	0.41	0.33	0.34
Observations	8539	8539	8539	8539	479	2421

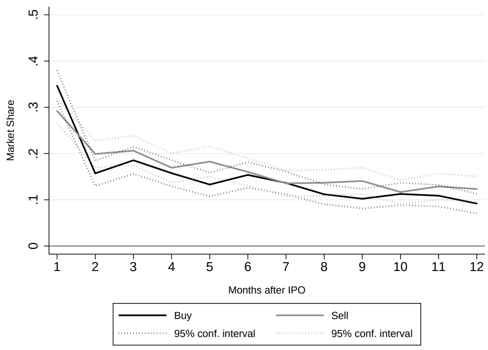
Table 15. This table reports the estimates of an OLS regression of the net buy through the lead underwriters scaled by the number of shares issued $[(B^L_{i,j} - S^L_{i,j})/N_i]$ on other trading volume components scaled by the number of shares issued: *OtherSales* is the relative volume of sales other than allocation sales $[(S^T_{i,j} - F^T_{i,j})/N_i]$; *SharesFlipped* is the relative number of shares flipped $[F^T_{i,j}/N_i]$; and *BuyNonLead* is the relative number of shares bought through non-lead brokers $[B^{NL}_{i,j}/N_i]$. Control variables are described in Table 3. All ratios are multiplied by 100. Columns (1)-(4) include trades executed during the first month after the issue by financial institutions that received an IPO allocation. Column (5) includes trades executed during the first month after the issue by financial institutions with no IPO allocations. Column (6) includes trades executed during the third month after the issue by financial institutions that received an IPO allocation. Standard errors are clustered at the institution level (t-statistics are in parentheses). Significance levels are denoted as: * 0.1, ** 0.05, *** 0.01.

	(1)	(2)	(3)	(4)	(5) Placebo	(6) Placebo
OtherSales	0.31***	0.28***	0.35***	0.33***	0.20	-0.20***
	(8.43)	(7.31)	(5.97)	(5.49)	(0.67)	(-3.04)
SharesFlipped	-0.30***	-0.36***	-0.36***	-0.40***		-0.018
	(-6.81)	(-6.80)	(-9.45)	(-8.25)		(-0.30)
BuyNonLead	0.00013	-0.011	-0.023	-0.028	-0.028	0.44***
	(0.00)	(-0.34)	(-0.59)	(-0.68)	(-0.21)	(9.48)
NormalTradeLead	-0.0051*	-0.0071***	-0.0082***	-0.0068***	0.059	-0.0075***
	(-1.73)	(-2.77)	(-3.26)	(-2.75)	(0.41)	(-4.34)
AllocPerc		0.066***	0.064***	0.059***		0.012
		(4.86)	(5.67)	(5.23)		(0.89)
Constant	0.073***	0.029*	0.029**	0.072	1.16	-0.088
	(2.80)	(1.74)	(2.27)	(1.39)	(0.88)	(-1.07)
Institution fixed effects	No	No	Yes	Yes	Yes	Yes
Firm fixed effects	No	No	No	Yes	Yes	Yes
Adjusted R2	0.14	0.15	0.24	0.23	0.31	0.17
Observations	8539	8539	8539	8539	479	2421

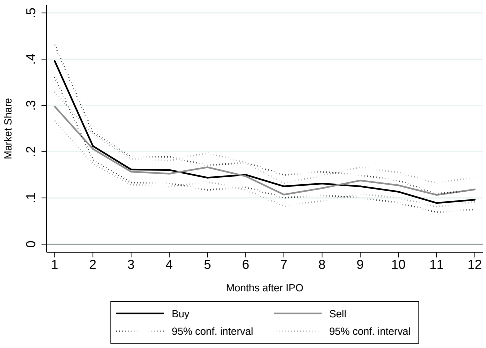
Figure 1. This figure shows the average brokerage market share for buying trades (dark grey lines) and selling trades (light grey lines) of the lead underwriters by month from the IPO date. For each IPO, we compute the percentage of institutional buying and selling trades executed by the lead underwriters in each month from the IPO date; then we average these percentages across IPO and we compute 95% confidence intervals of the means (dashed lines). Panels (A) reports the brokerage market share for all IPOs. Panel (B) reports the brokerage market share for hot IPOs (highest tercile of *Underpricing*); Panels (C) reports the brokerage market shares for weak IPOs (middle tercile of *Underpricing*); and Panels (D) reports the brokerage market share for cold IPOs (lowest tercile of *Underpricing*).



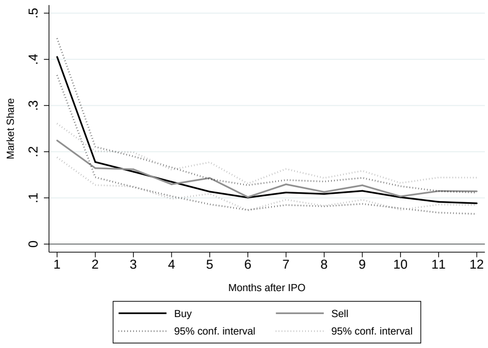
(A) All IPOs



(B) Hot IPOs

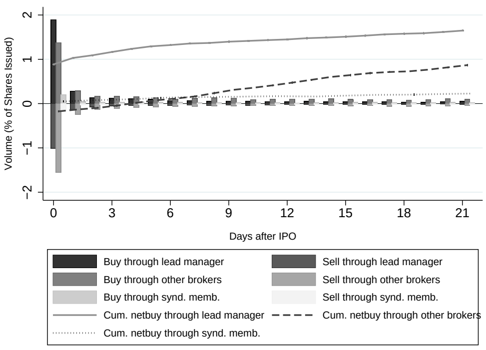


(C) Weak IPOs

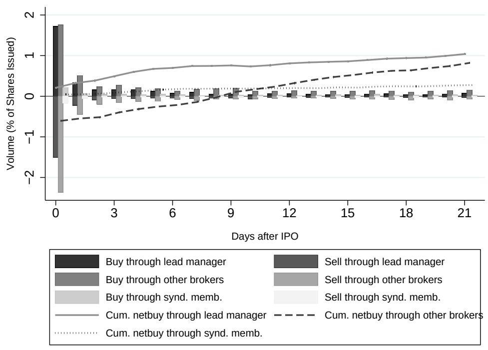


(D) Cold IPOs

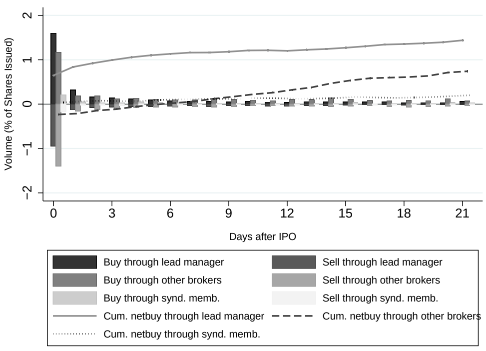
Figure 2. This figure shows the average cumulative netbuy and buy/sell volume in the first 21 trading days after the issue date. For each IPO, we compute the total amount bought and sold in each day by institutions that trade through the lead underwriters, through other syndicate members, and through brokers that did not participate in the IPO syndicate. We also compute the cumulative netbuy of lead managers' clients, syndicate members' clients, and other brokers' clients in the first 21 trading days after the IPO. We scale the volume traded by the number of shares issued and we average it across IPOs. Bars show institutions' daily volume bought and sold; lines plot institutions' cumulative netbuy. Panel (A) averages buy and sell volumes and cumulative netbuy for all IPOs. Panels (B)-(D) break the averages down for hot IPOs (highest tercile of *Underpricing*), weak IPOs (middle tercile of *Underpricing*), and cold IPOs (lowest tercile of *Underpricing*).



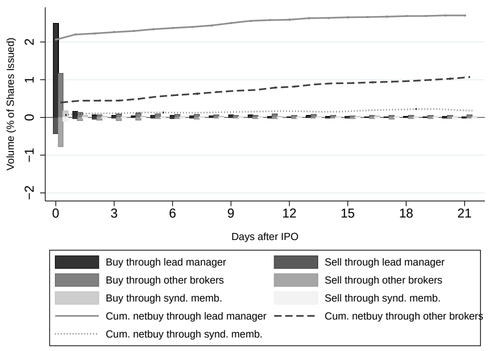
(A) All IPOs



(B) Hot IPOs



(C) Weak IPOs



(D) Cold IPOs

Figure 3. This figure plots the average cumulative percentage of IPO shares flipped, scaled by the number of shares offered, by month from the issue date. 95% confidence intervals are reported with dotted lines. The black line report the average for the whole sample of IPOs. The grey lines break the averages down for hot IPOs (highest tercile of *Underpricing*), weak IPOs (middle tercile of *Underpricing*), and cold IPOs (lowest tercile of *Underpricing*).

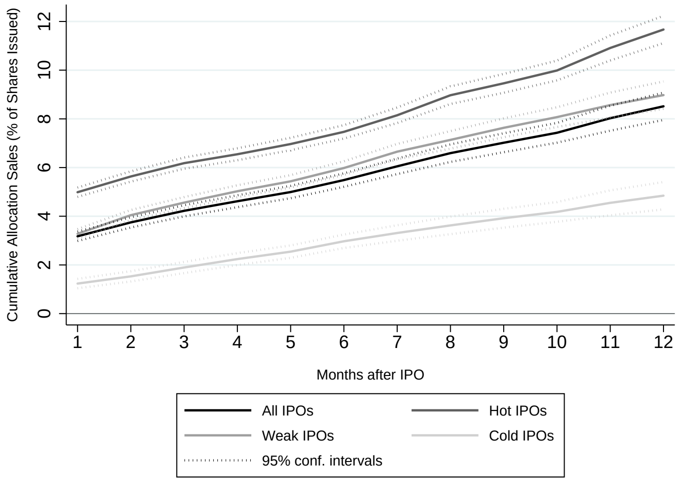


Figure 4. This figure plots the average trading commission paid to the lead underwriters for buying trades (dark grey line) and sell trades (light grey line) by month from the issue date. Commissions are scaled by the dollar volume traded. 95% confidence intervals are reported with dotted lines.

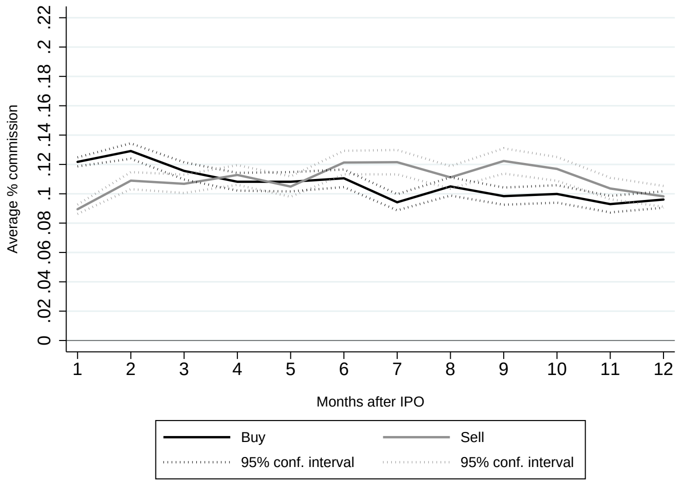


Figure 5. Mapping money managers and brokers across databases (key identifier(s) for the match are provided in bold).