Underwriter Manipulation in Initial Public Offerings*

Rajesh K. Aggarwal†
University of Minnesota

Amiyatosh K. Purnanandam‡
University of Michigan

Guojun Wu§
University of Houston

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Abstract

We provide a new explanation for the extremely high level of initial public offering (IPO) underpricing during the Internet bubble years of the late 1990s based on the allocation practices of underwriters. By requiring their customers to buy the stock in the aftermarket in return for IPO allocations (a tie-in agreement), the underwriters created artificial excess demand for the IPOs, leading to distorted (manipulated) price levels in the immediate aftermarket. Empirically, we show that IPOs with tie-in agreements exhibit a distinctly different pattern for the first-day and long-term returns than do IPOs without tie-in agreements during the 1998 to 2000 period, consistent with the model. IPOs with tie-in agreements have seven times higher first-day returns and considerably higher first-day trading volume. However, these stocks begin to underperform significantly starting from the sixth month after the IPO, and the lower returns persist for up to three years after the IPO. We also find that the tie-in IPOs experience significantly lower returns around the lock-up expiration period. Even after controlling for hot market conditions and issuer characteristics such as issue size, underwriter quality, and whether the IPO is a technology stock, we find that manipulation explains much of the unusually high level of underpricing during the late 1990s.

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† Carlson School of Management, University of Minnesota, 321 19th Avenue S., Minneapolis, MN 55455, E-mail: raggarwal@csom.umn.edu
‡ Ross School of Business, University of Michigan, 701 Tappan Street, Ann Arbor, MI 48109, Phone: (734) 764-6886, E-mail: amiyatos@umich.edu
§ Bauer College of Business, University of Houston, 334 Melcher Hall, Houston, TX 77204, Phone: (713) 743-4813, E-mail: gwu2@uh.edu

1. Introduction

The average first-day return on initial public offerings (IPOs) increased from 7% in the 1980s to 15% during 1990-1998, before jumping to 65% during the Internet bubble years of 1999-2000 (see Loughran and Ritter (2002, 2004)). What explains such a high level of IPO underpricing during the late 1990s? Ljungqvist and Wilhelm (2003) argue that changes in IPO ownership structure such as decreased CEO ownership and increased ownership fragmentation led to decreased incentives for insiders to maximize IPO proceeds in this period. Loughran and Ritter (2004), on the other hand, argue that the objective function of the issuers changed during the Internet bubble period. Issuers were more willing to "leave money on the table" in return for greater analyst coverage or side payments in this period.[1]

In this paper, we provide an alternative explanation for the extremely high level of IPO underpricing during this period. We argue that underwriters used manipulative practices in the form of tie-in agreements during the IPO process. By requiring their IPO investors to buy the stock in the aftermarket in return for IPO allocations (tie-in agreements or laddering), the underwriters created artificial excess demand for the IPOs, leading to distorted price levels in the immediate after-market. Since these manipulated prices cannot be sustained in the long run, the manipulated IPOs underperform significantly when fundamental values are finally reflected in prices. We show that these practices can explain a significant portion of the extremely high level of IPO underpricing during the Internet bubble period.

The manipulative practices, though prohibited by Securities and Exchange Commission (SEC) regulations, were prevalent during the 1998 to 2000 period as is evident from numerous lawsuits that followed in subsequent years. In our sample of 908 IPOs during 1998-2000, a total of 173 IPOs were the subjects of SEC or class action lawsuits for tie-in agreements. While there has been a considerable debate about such practices in the popular press, there has been little attempt in the academic literature to understand the effect of such manipulative practices on the performance and functioning of the IPO market.[2]

To address these issues, we test predictions from a model of underwriter manipulation of IPOs through tie-in agreements. In the model, there are three types of investors in addition

[1] Aggarwal, Krigman, and Womack (2002) argue that insiders might be willing to underprice IPOs if increased underpricing generates more attention from analysts and the media, resulting in higher prices when insiders sell shares at the expiration of the lockup period.
[2] An exception is a recent paper by Hao (2004), which we discuss in more detail later.

to the underwriter. First, there is an investor affiliated with the underwriter (the ladderer). This investor gets information from the underwriter about the future value of the stock, which can be either high or low. If the affiliated investor learns that the stock value in the future will be high, then the affiliated investor chooses to trade on this information by buying shares. If the affiliated investor learns that the stock value in the future will be low, then he may still choose to buy shares in order to manipulate the stock via a laddering arrangement with the underwriter. The second group of investors is momentum investors. One can also think of them as being arbitrageurs, day-traders, sentiment investors, or information seekers (as in Aggarwal and Wu (2005)). The momentum investors can observe past prices and volume, but they have no access to fundamental information themselves. Instead, they try to infer from prices and volumes whether an affiliated (informed) investor is buying the stock, and whether they should be buying the stock as well, i.e., trading on momentum. Aside from having limited information, they are in all other respects completely rational. The third group of investors is a continuum of noise or uninformed traders. These traders do not update or condition on any information, and they provide liquidity to the market.

In this model, there exists a pooling equilibrium where price manipulation is successful and profitable for the ladderer. The key to this argument is information asymmetry. The momentum traders are uncertain whether an affiliated investor who buys the stock in the aftermarket does so because he knows it is undervalued or because he intends to manipulate the price through laddering. It is this pooling that allows manipulation to be profitable (see also Allen and Gale (1992)). An important comparative static that emerges is that the possibility of pooling is increasing in the number of momentum investors. To the extent that we think of momentum investors as sentiment investors, this suggests that in periods of high investor sentiment, manipulation through laddering will be more likely. There are several important predictions from the model. For example, manipulated stocks have higher prices (returns) during aftermarket trade and this eventually reverses itself in the long-run. In addition, the price differentials between manipulated and non-manipulated stocks in early aftermarket trading are increasing in the number of momentum investors in the market.

Empirically, to ascertain whether an IPO was subject to tie-in agreements by the underwriters or not, one needs to have access to the underwriters' books and their exact allocation procedure. In the absence of such information, we consider lawsuits alleging the presence of such practices as the best available proxy for tie-in agreements. There are two kinds of lawsuits that we consider in this paper – (a) civil actions brought by the SEC, and (b) lawsuits brought by the investing public (class-action lawsuits). An obvious concern about class-action lawsuits is that investors or attorneys might target IPOs with high first-day returns and subsequent poor performance for class-action lawsuits. Therefore, this proxy for tie-in agreements may overstate the extent to which there actually are such agreements in place.

Civil actions brought by the SEC against specific underwriters are based on direct evidence of tie-in agreements.[3] However, it is also likely that the SEC civil actions miss instances of tie-in or laddering arrangements due to limited SEC enforcement (see Aggarwal and Wu (2005) for a general discussion of SEC enforcement). As a result, we consider the SEC civil actions to be a lower bound on the prevalence of tie-in agreements and the class-action lawsuits to be a likely upper bound on the prevalence of tie-in agreements. There are 33 IPOs that were named in SEC civil actions and 140 that are the subjects of class-action lawsuits (but have not been named in SEC civil actions thus far). We perform all our analyses based on both definitions of tie-in agreements (SEC lawsuits and class-action lawsuits) and the results are qualitatively similar.

We find that the IPOs with tie-in agreements (manipulated IPOs) exhibit a significantly different pattern for the first-day and long-term returns than do non-manipulated IPOs during the same period. The median manipulated IPO earned seven times higher first-day returns (115.48% for manipulated IPOs based on SEC civil actions versus 16.25% for non-manipulated IPOs) and experienced considerably higher first-day trading volume than did the non-manipulated IPOs. The extremely high level of underpricing during these years can largely be attributed to the group of manipulated IPOs, since the extent of underpricing for the non-manipulated IPOs is comparable to earlier periods. Excluding the first-day return, the manipulated IPOs continue to earn higher returns than the non-manipulated firms in the immediate aftermarket. However, there is a significant reversal of the pattern starting from the sixth month after the IPO. In subsequent periods (months six through twelve and years one

[3] See Securities and Exchange Commission (2003, 2005a, and 2005b).

through two post-IPO), the manipulated IPOs continue to underperform the non-manipulated IPOs by economically large margins.[4]

We argue that manipulative tie-in agreements explain these patterns. The difference in underpricing across the manipulated and non-manipulated samples is not explained by known determinants of IPO underpricing such as the size of the firm, venture capital backing, underwriter ranking and whether the firm is an Internet/technology firm. We also control for the effect of perceived hotness on IPO underpricing using the filing-to-offer return as well as a relative valuation proxy based on Purnanandam and Swaminathan (2004). The manipulated IPOs have significantly higher filing-to-offer returns and are considerably overvalued at the offer price as compared to their industry peers. Thus, these IPOs are perceived to be "hot" or "glamour" IPOs. But even after controlling for such characteristics, the tie-in agreements explain the largest portion of cross-sectional variation in IPO underpricing. In summary, our results establish a strong link between high first-day returns and the existence of tie-in agreements. Further, the abnormal volume on the first day of trading lends additional support to the manipulation hypothesis.

Our use of SEC and class-action lawsuits to identify instances of manipulation naturally raises endogeneity concerns. Specifically, we are concerned that investors who bought IPOs with high first day returns but then subsequently lost money holding the stock have chosen to litigate and this explains our findings, as opposed to the possibility that manipulation (identified by litigation) explains price run-ups post-IPO followed by negative returns. We address this concern in three ways. First, our results hold for the SEC based definition of manipulation, which we argue contains no ambiguity about whether manipulation occurred. Second, we instrument for the likelihood of class action lawsuits using turnover on unrelated IPOs to address potential endogeneity concerns. While the magnitude of the effect of manipulation on IPO underpricing is reduced, it is still large and significant. Third, we examine subsets of our data based on whether the offerings had large price run-ups or experienced subsequent negative returns. In these subsets, our results continue to hold in the sense that manipulated stocks have much greater underpricing than do the non-manipulated

[4] There are slight differences in these patterns depending upon the definition (SEC civil action versus class action lawsuit) of manipulated IPOs used.

stocks. Thus, it is not just the fact that there were large price runups or subsequent price drops that resulted in litigation that drives the result.

If underwriters engage in manipulation, does it benefit the insiders of the firms and other pre-IPO investors? To address this question, we analyze returns around the lock-up expiration. We find significantly larger price drops around the lock-up expiration for the manipulated firms relative to the rest of the IPOs. In the seven-day window surrounding the lock-up expiration (typically 180 days after the IPO date), manipulated IPOs earn –8.56% as compared to –1.57% for the non-manipulated IPOs. This finding is consistent with the hypothesis that in IPOs with tie-in agreements, insiders and other pre-IPO investors subject to lock-up arrangements sold at least a portion of their holdings at the first possible opportunity. The initial run-up in prices benefits these investors. Thus, our results are consistent with the corruption hypothesis of Loughran and Ritter (2004), where the objective function of the issuing firm is to maximize not only the IPO proceeds but also the proceeds from subsequent sales by the insiders.[5]

The manipulation explanation is unlikely to be valid if underwriters are not able to benefit from price manipulation. Do underwriters gain from their manipulative activities? The answer appears to be yes. Underwriters have an incentive to artificially influence aftermarket activity because they have underwritten the risk of the offering, and poor aftermarket performance could result in reputational and subsequent financial losses (of course, underwriters sued for manipulation also risk reputational and financial losses).

More directly, if manipulation is likely to lead to higher aftermarket prices and increase the perceived hotness of an IPO, then underwriters are capable of extracting benefits due to increased demand for allocations. In order to receive hot IPO allocations, investors often are willing to pay excess commissions and other forms of kickbacks. Indeed, underwriters sued for tie-in agreements are also frequently involved in excess commission lawsuits. Nimalendran, Ritter and Zhang (2004) find that commission revenue paid to underwriters by investors is one of the determinants of IPO allocations. Further, Reuter (2004) argues that there exists a quid pro quo between underwriters and investors in IPO allocations. He finds a positive relation between the commissions paid by the mutual fund families to the lead underwriter and their reported holdings of the offerings brought public by the same underwriter. He further interprets

[5] See Aggarwal, Krigman, and Womack (2002) for a theory along these lines.

this relation as evidence that lead underwriters use IPO allocations to compete for brokerage business from mutual fund families.

Our findings have interesting implications for research in this area. They complement existing research on short-term underpricing and long-term underperformance issues. Perhaps most surprisingly, our results suggest that the high levels of IPO underpricing in the late 1990s can be attributed primarily to price manipulation by underwriters and ladderers. The rest of the paper is organized as follows. In Section 2, we discuss tie-in agreements and the regulatory environment governing securities issues (Regulation M) in further detail. In Section 3, we present the empirical implications and predictions of our model of manipulation by underwriters. The model itself, where underwriters use tie-in agreements with affiliated investors, is presented in the Appendix. In Section 4, we describe the data and the characteristics of the sample IPOs. In Section 5, we conduct empirical tests of the relations between underpricing, turnover, returns, and tie-in agreements. Section 6 contains concluding remarks.

2. SEC Regulations, Lawsuits and Description of Data

In this section we first review SEC regulations against tie-in agreements. We then provide some examples of tie-in agreements based on court documents filed by the SEC.

2.1. SEC Regulations against Tie-in Agreements

Underwriters, broker-dealers, and anyone else participating in the distribution of securities are prohibited from soliciting or requiring their customers to make aftermarket purchases until the distribution is completed. These practices are prohibited by Rules 101 and 102 of Regulation M and may violate other anti-fraud and anti-manipulation provisions of the federal securities laws as well.[6] As an anti-manipulation regulation, Regulation M is intended to protect the integrity of the offering process by precluding activities that could artificially influence the market for the offered security. In particular, Regulation M prohibits participants in the distribution from directly or indirectly attempting to induce any person to bid for or

[6] Regulation M applies to a "distribution" of securities, which is defined to mean any offering of securities that is distinguished from ordinary trading transactions by the magnitude of the offering and the presence of special selling efforts and selling methods. The SEC has held that tie-ins are fraudulent devices that violate Section 17(a) of the Securities Act of 1933 and Section 10(b) of the Securities Exchange Act of 1934, and Rule 10b-5 under the Securities Exchange Act.

purchase any security that is the subject of a distribution other than through the distribution itself. The SEC prohibits solicitations for aftermarket purchases since they can give other purchasers in the offering the impression that there is a scarcity of the offered securities. This can stimulate demand and support the pricing of the offering. Moreover, traders in the aftermarket will not know that the aftermarket demand, which may appear to validate the offering price, has been stimulated by the distribution participants.[7]

One interesting question that arises is how tie-in agreements differ from other, legal forms of price stabilization by underwriters. Conceptually, price stabilization, where the underwriter agrees to purchase shares if the price falls, is a bonding mechanism underwriters use to commit to IPO investors that they will not overprice shares in the IPO. The underwriters risk losses through price stabilization in order to offset their informational advantage relative to the IPO investors. Tie-in agreements represent no risk of loss to the underwriter. Instead, we argue that tie-in agreements allow underwriters to profit from their informational advantage by distorting (manipulating) aftermarket prices. Thus, in our view, there are two key distinctions between price stabilization and tie-in based manipulation. Price stabilization prevents price decreases and creates risk (and cost) to the underwriter to offset an informational advantage. Tie-in agreements lead to (artificial) price increases and create an opportunity to profit from the underwriter's informational advantage relative to other aftermarket participants. We believe that it is the opportunity to profit that makes tie-in agreements illegal while the risk of loss makes price stabilization legal.

2.2. Examples of Tie-in Agreements[8]

J.P. Morgan Chase agreed to settle charges of unlawful IPO allocation practices and paid a penalty of $25 million to the SEC on October 1, 2003. In its complaint, the SEC alleged that J.P. Morgan violated Rule 101 of Regulation M under the Securities Exchange Act of 1934 by attempting (successfully) to induce customers who received allocations of IPOs to place purchase orders for additional shares in the aftermarket. The following instances of manipulation were noted in the SEC complaint.

[7] Tie-in agreements are not a completely recent phenomenon. As far back as 1961, the SEC addressed reports that certain dealers participating in distributions of new issues had been making allotments to their customers only if such customers agreed to make some comparable purchase in the open market after the issue was initially sold.
[8] This section is based on Securities and Exchange Commission (2003).

1. J.P. Morgan solicited customers to provide information about whether, at what price, and in what quantity they intended to place aftermarket orders for IPO stock. J.P. Morgan communicated to customers that expressing an interest in buying shares in the aftermarket would help them obtain allocations of hot and oversubscribed IPOs. For example, in the Large Scale Biology IPO, a sales representative reported in an e-mail that she "was very aggressive in pushing the customer for aftermarket action - stressing how important it was going to be for the process."
2. J.P. Morgan encouraged customers providing aftermarket interest to increase the prices they were willing to pay typically because other customers seeking allocations had provided aftermarket interest at higher prices. For example, a sales representative told a customer that their aftermarket price limit was "sort of out of the game" and there was "interest at much higher levels." In the Dyax IPO, a sales representative told the syndicate desk in an e-mail, "If the customer gets 50,000 IPO shares, he will buy 50,000 more (up to $16). If need be, I will tell him to increase his aftermarket price sensitivity to a higher number."
3. J.P. Morgan solicited aftermarket interest from customers J.P. Morgan knew had no interest in owning the stock for the long term. J.P. Morgan knew that these customers usually or always immediately sold their IPO allocations. Nevertheless, J.P. Morgan expected these customers to follow through and buy in the aftermarket when they provided aftermarket interest. A number of these customers bought in the aftermarket and then sold their allocation or closed out their entire position within days of the IPOs.
4. After the restricted period, J.P. Morgan solicited aftermarket orders by making follow-up calls to customers who had previously indicated aftermarket interest. Further, J.P. Morgan often tracked whether customers followed through and actually purchased in the aftermarket. When customers did not follow through, J.P. Morgan encouraged its sales force to place follow-up calls to these customers to solicit orders to purchase stock. For instance, on August 6, 1999, the day after the Interactive Pictures IPO started trading, the head of Global Equity Capital Markets sent an e-mail to regional sales managers and the head of syndicate which included the following comments: (1) one customer "owes it to us to be in buying the stock today"; (2) "we should push another customer today and if they

don't show up, keep them out of these tiers going forward"; and (3) "let's make another customer show up today." In addition, J.P. Morgan described customers' aftermarket interest as promises, obligations, and commitments. For example, in an e-mail about the Genentech IPO, a sales representative said that an institutional customer "followed up in the aftermarket exactly as promised."

The media reported instances of tie-in agreements even before the SEC took action. The *Wall Street Journal* reported on April 14, 2003 that the SEC notified Morgan Stanley formally that it may face civil charges of awarding hot IPO shares to investing clients who signaled plans to buy additional shares at higher prices in aftermarket trading. Prior to this, Morgan Stanley's use of tie-in agreements was reported in the online technology newsletter *RedHerring* on May 2, 2001 in an article titled "The Art of the Tie-in." On January 25, 2005, the SEC announced that Morgan Stanley settled the tie-in agreement charges and paid a fine of $40 million (see Securities and Exchange Commission (2005a)).

3. Theoretical Predictions

In the appendix, we present a model of underwriter manipulation of IPOs based on the model of Aggarwal and Wu (2005). In the model, there are three types of investors in addition to the underwriter. First, there is an investor affiliated with the underwriter (the ladderer). This investor gets information from the underwriter about the future value of the stock, which can be either high or low. If the affiliated investor learns that the stock value in the future will be high, then the affiliated investor chooses to trade on this information by buying shares. If the affiliated investor learns that the stock value in the future will be low, then he may still choose to buy shares in order to manipulate the stock via a laddering arrangement with the underwriter. The second group of investors is momentum investors. One can also think of them as being arbitrageurs, day-traders, sentiment investors, or information seekers (as in Aggarwal and Wu (2005)). The momentum investors can observe past prices and volume, but they have no access to fundamental information themselves. Instead, they try to infer from prices and volumes whether an affiliated (informed) investor is buying the stock, and whether they should be buying the stock as well, i.e., trading on momentum. Aside from having limited information, they are in all other respects completely rational. The third group of investors is a continuum of noise or uninformed traders. These traders do not update or condition on any information, and they provide liquidity to the market.

We show that there exists a pooling equilibrium where price manipulation is successful and profitable for the ladderer. The key to this argument is information asymmetry. The momentum traders are uncertain whether an affiliated investor who buys the stock in the aftermarket does so because he knows it is undervalued or because he intends to manipulate the price through laddering. It is this pooling that allows manipulation to be profitable (see also Allen and Gale (1992)). An important comparative static that emerges is that the possibility of pooling is increasing in the number of momentum investors. To the extent that we think of momentum investors as sentiment investors, this suggests that in periods of high investor sentiment, manipulation through laddering will be more likely. There are several important predictions from the model.

Prediction 1: The time path of prices for manipulated stocks is that prices rise on the first day of trade from the IPO price, increase over the subsequent time period, and then in the long run fall to their fundamental value. We identify three specific time periods for the purposes of returns. Returns on the first day of trade relative to the IPO offer price represent what is usually referred to as IPO underpricing. Returns between the end of the first day of trade and beginning of sixth months after the IPO are what we refer to as the subsequent time period or time 2. These returns are predicted to be positive. The choice of six months coincides with the end of the lockup period. Long-run returns are measured from the beginning of the sixth month to three years post-IPO and these returns are predicted to be negative.

Prediction 2: The time path of prices for non-manipulated stocks is that prices initially fall from the IPO offer price, before increasing over the subsequent period, and increasing in the long-run. Because there is a “normal” amount of underpricing for all IPOs, our theory is meant to capture abnormal underpricing for manipulated stocks. Thus, we are more interested in the comparison between returns for the manipulated and non-manipulated stocks, which is our next prediction.

Prediction 3: Returns for manipulated stocks are higher than for non-manipulated stocks on the first day of trade (there is greater IPO underpricing for manipulated stocks). Returns for manipulated stocks are higher over the subsequent time period (the end of the first day of trade to the beginning of the sixth month post-IPO) than for non-manipulated stocks. Over the long-run (the sixth month to the end of three years), returns for non-manipulated stocks are higher than for manipulated stocks, i.e., there is a long-run price reversal.

Prediction 4: The difference in IPO underpricing (first day returns) between manipulated stocks and non-manipulated stocks is increasing in the number of momentum investors (investor sentiment). The difference in returns over the subsequent time period (the end of the first day to six months post-IPO) is also increasing in the number of momentum investors.

Prediction 5: As mentioned before, more momentum investors imply a greater likelihood of manipulation through laddering.

Prediction 6: Turnover (shares traded) is greater for manipulated stocks than for non-manipulated stocks.

These are the empirical implications of our model that we test below. Our results can be compared with those in Hao's (2004) model of IPO manipulation through laddering arrangements. Similar to the predictions in our model, Hao finds that laddering is more likely in hot markets and that laddering is associated with the possibility of kickbacks from the ladderer to the underwriter. She also finds that laddering leads to greater IPO underpricing, especially in the presence of kickbacks. Perhaps the largest distinction between Hao's model and ours is that her model generates intentional IPO underpricing, while our model generates price run-ups for manipulated IPOs that are eventually corrected as the true value of the firm is revealed. Thus, our model can explain the long-run performance of manipulated IPOs as well. Another important difference between the two models is that we assume there is information asymmetry between the ladderer and the "rational" momentum traders, and price momentum is derived in equilibrium. In contrast, there is no information asymmetry in Hao's model and price momentum is assumed to be present for laddering to be profitable.

4. Data Description

We next empirically examine the importance of tie-in agreements. We obtain data on common stock IPOs from the Thompson Financial Securities (SDC) database for the 1998-2000 period. Consistent with the prior literature, we remove unit offerings, REITs, ADRs, closed-end funds, and financial firms from the sample. To be covered in our sample, a firm must be covered by the CRSP and COMPUSTAT databases. We obtain accounting data from COMPUSTAT and returns data from CRSP. The resulting sample consists of 908 IPOs.

4.1. SEC and Class Action Lawsuits

Of the 908 IPOs in our sample, a number of them are likely to have been manipulated through the use of a tie-in agreement. To see if a particular IPO has been manipulated, we look for civil actions by the SEC and class action lawsuits against underwriters alleging such activities. We describe each in turn.

(1) SEC lawsuits: The SEC filed civil suits against J.P. Morgan, Morgan Stanley, and Goldman Sachs for tie-in agreement practices during our sample period. These firms settled with the SEC and paid penalties of $25 million, $40 million, and $40 million, respectively. In the civil complaints (Securities and Exchange Commission (2003, 2005a, and 2005b)) filed with federal courts, the SEC detailed instances of tie-in practices in 35 IPOs by these three underwriters. Of these IPOs, 33 are in our sample of 908 IPOs. For these IPOs, there is direct evidence of tie-in agreements along the lines of what we describe in Section 2.2.

(2) Class-action lawsuits: Multiple class action lawsuits were filed at the U.S. District Court (Southern District of New York) against underwriters and issuers for tie-in agreement practices. These cases were consolidated into one master allegation and the court issued a preliminary opinion and order on February 19, 2003 that for 185 IPOs, there was sufficient evidence to proceed.[9] For these IPOs, there is an allegation of a tie-in agreement supported by a preliminary court finding, but no direct evidence. Of these 185 IPOs, we have 140 in our sample once we eliminate the overlap with the SEC lawsuit group.

Based on these two forms of lawsuits, we classify the sample IPOs into manipulated and non-manipulated groups. First, we classify the 33 stocks named by the SEC in the settled civil suits against J.P. Morgan, Morgan Stanley, and Goldman Sachs as “SEC-manipulated” IPOs. Out of the remaining IPOs, if an IPO is subject to the class-action lawsuit, we classify it as a “class-action-manipulated” IPO. There are 140 firms in our sample that fall into this category. The remaining 735 firms that have not been named in either of the above lawsuits are called

[9] For a list of these IPOs and the court’s ruling, see U.S. District Court, Southern District of New York (2003).

"non-manipulated" IPOs. We conduct all our analyses based on both of these definitions of manipulation. We believe the SEC civil actions represent a lower bound on the prevalence of tie-in agreements given limited SEC enforcement. We treat the class-action lawsuits as a likely upper bound on the prevalence of tie-in agreements since some of these lawsuits may simply represent opportunistic filing based on subsequent poor performance. However, we cannot rule out the possibility that there are even more IPOs that were manipulated that we have classified as non-manipulated due to lack of information. In other words, there may be instances in which the underwriters successfully manipulated the stock and successfully remained undetected.

We compare the behavior of SEC-manipulated and class-action-manipulated IPOs to the reference portfolio of non-manipulated stocks in the subsequent analysis. Table 1 provides the yearly distribution of sample IPOs and the number of SEC-manipulated and class-action-manipulated firms in each year. The majority of lawsuits pertain to IPOs conducted during the years 1999 and 2000. The manipulated firms (based on either definition) account for about 22% of the total proceeds raised by IPOs during the sample period and therefore represent an economically significant portion of the total IPO market. As shown in Table 1, there are 619 Internet and technology stocks in our sample.[10] Relative to their proportion of our overall sample, Internet/technology firms are over-represented in the manipulated IPOs (25 out of 33 SEC-manipulated and 133 out of 140 class-action manipulated IPOs are Internet/technology).

4.2. Descriptive Statistics at the Time of the IPO

Table 2 provides descriptive statistics for the sample firms. In Panel A, we provide the mean and median characteristics of SEC-manipulated and non-manipulated firms. Panel B provides the corresponding statistics for class-action-manipulated IPOs. Several interesting patterns emerge from Table 2. The SEC-manipulated firms have significantly higher offer prices (median of $18) and raise significantly more proceeds from the market (median of $76.75 million) relative to the non-sued firms (medians of $13 and $52.35 million). However, when we look at accounting numbers, we find that the median manipulated firm is about half the size of the median non-manipulated firm in terms of sales. Manipulated firms are less

[10] We combine Internet firms (firms such as uBID and Priceline.Com) with technology stocks in our later analysis. Thus, in defining technology stocks as Internet plus other technology stocks, we are following the SIC code based classification of Loughran and Ritter (2004).

profitable and a higher proportion of these firms are backed by a venture capitalist. These findings show that the firms involved in litigation have relatively inferior operating performance at the time of IPO, but raise significantly more proceeds in the IPO.

The median first-day return (calculated as the return from the offer price to the first-day closing price net of the market return) for the SEC-manipulated IPOs is 115.48% (mean of 146.65%) versus 16.25% (mean of 36.73%) for the non-manipulated IPOs.[11] These return differences across the two groups are economically large and statistically significant. We find similar patterns for the trading volume on the first day of trading. We compute an IPO's trading volume by dividing the shares traded by the number of shares offered in the IPO. To account for different reporting standards by NASDAQ as compared to NYSE and AMEX, we divide NASDAQ IPO's turnover by a factor of two. While the manipulated IPOs exhibit a median turnover of 74.41% (mean of 81.42%), the non-manipulated IPOs have a significantly lower median turnover of 54.17% (mean of 57.45%). There is an interesting difference in the price revisions from the filing date to the offer date for these two groups of IPOs. While the median manipulated IPO revises its price upwards 41.67% from the suggested range (mid-point of filing to offer price), the median non-manipulated IPO sets it offer price at the middle of the filing range itself (i.e., filing-to-offer revision of 0%).[12]

The median underwriter ranking based on Carter, Dark and Singh (1998) and as modified by Loughran and Ritter (2004) is 9.10 for manipulated firms and 8.10 for the non-manipulated firms. Larger and more prestigious underwriters underwrite the manipulated IPOs. This finding is consistent with the deep pockets hypothesis of lawsuits, which argues that investors (and possibly government regulators) are more likely to sue firms and underwriters with more capital in order to profit from the lawsuit. Of course, these are also the underwriters who have the ability to enforce tie-in agreements with their investors given that they are likely to underwrite more IPOs and more attractive IPOs in the future.

In summary, our univariate results suggest that the SEC-manipulated IPOs are underwritten by more prestigious and bigger underwriters, have significantly higher filing-to-

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[11] We subtract the market return from the first day raw return of IPO firms to remove the effect of overall market conditions from IPO underpricing. Our results remain similar, qualitatively and quantitatively, if we just use the first day raw return as a proxy for IPO underpricing. Both of these measures have been used by earlier studies in the literature.

[12] To the extent that the filing-to-offer revision is a good measure of investor sentiment, this suggests that investor sentiment is high in the manipulated IPOs but not in the non-manipulated IPOs.

offer price revisions and first-day returns, and are traded very actively on the first day of trading relative to non-manipulated IPOs. Further, the manipulated IPOs are smaller (by sales), less profitable, and are actively backed by venture capitalists. In Panel B of Table 2, we repeat the analysis presented in Panel A for the class-action-manipulated IPOs. The univariate results across Panels A and B are qualitatively similar. This finding suggests that both definitions of manipulation – SEC-based and class-action-based – represent relatively similar sets of IPOs.

4.3. Post-IPO Returns and Turnover

We next examine the mean buy-and-hold returns for various holding periods after the IPO starting from the close of the first day of trading (i.e., excluding the first-day return). Panel A presents results for SEC-manipulated and non-manipulated groups and Panel B replicates the analysis for class-action-manipulated IPOs. To analyze the price and return dynamics more precisely after the IPO date, in Table 3 we provide returns for the following holding periods: (a) the first-week after the IPO excluding the first-day return; (b) the first five months after the IPO excluding the first-week return; (c) sixth month after the IPO; (d) the end of the sixth month after the IPO until the first anniversary of the IPO; (e) the end of the first year after the IPO to the end of the second year after the IPO and (f) the end of the second year to the end of the third year after the IPO.

A word is in order about these various holding periods. Relative to the model, the IPO offer price is P_0. We treat the price at the end of the first day of trade as P_1, although anecdotal evidence suggests that affiliated investors could be buying shares for as much as a week after the IPO. Because we do not precisely observe when affiliated investors buy or sell, we necessarily exercise judgment in choosing the time periods for our empirical results. Thus, one could think of time 1 as encompassing the first week post-IPO (and all of our results would also be consistent with this choice), but for the rest of the paper we choose the first day of trade as time 1 to be conservative with respect to when affiliated investors establish their additional positions.

Similarly, the choice for time 2 is somewhat arbitrary. We do know that affiliated investors trade out of their positions fairly quickly, and we believe that they will certainly trade out prior to the expiration of the lockup period for insiders, usually six months post-IPO. In principle, time 2 could occur at any time up to six months post-IPO. As an empirical matter, prices increase through month 5 and then start to fall, presumably in anticipation of lockup expiration selling. For our empirical results, we treat time 2 as running through five months post-IPO, although we could also have used the exogenous event of the lockup expiration as defining the end of time 2. We chose not to use the lockup expiration because Brav and Gompers (2003) show that underwriters frequently release insiders from their lockup agreements early, and this is especially true for IPOs with large price runups and that are backed by venture capitalists and more prestigious underwriters. These characteristics describe our manipulated IPOs. We view five months post-IPO as the conservative choice, while also being consistent with the possibility that insiders may benefit from the manipulation. Our empirical results would continue to hold if we defined time 2 as running through the end of month 6. The holding periods starting after five months post-IPO then define time 3 (the long-run).

For each holding period, we provide raw compounded returns as well as buy-and-hold abnormal returns (BHAR) with respect to two benchmark portfolios: (a) returns on the value-weighted market index and, (b) returns on a size-controlled matching firm. This second benchmark follows Ritter (1991) who uses a size-adjusted approach in computing the long-term performance of IPOs. To find a size-controlled firm, we select a seasoned firm that is closest to the market value of the IPO firm (based on the offer price) on the IPO date. This seasoned firm is the benchmark. We make sure that the benchmark firm is at least 5 years old (i.e., it did not go public in the last 5 years). If the benchmark firm delists, we use the firm next closest in market capitalization as of the offer date and so on. To compute the BHAR, we first compute the buy-and-hold return for the control firm and then subtract it from the IPO firm's return for the same holding period. If an IPO delists during the holding period, we compute returns until the delisting date.

There is an interesting pattern that emerges from the analysis of post-IPO returns across manipulated and non-manipulated IPOs. As seen in Panel A of Table 3, in the first week after the IPO the SEC-manipulated IPOs earn average raw returns (excluding the first-day return) of 7.18% versus -1.17% for the non-manipulated firms. Based on market-adjusted and size-adjusted returns, the manipulated stocks earn about 8.24% to 8.39% more than the non-manipulated stocks in the first week. The difference between the manipulated and non-manipulated groups is statistically indistinguishable beyond the first week and up to the first five-months. However, beginning with the sixth month from the offer date, manipulated firms begin to underperform the non-manipulated IPOs. In the sixth month after an IPO, manipulated-IPOs underperform non-manipulated IPOs in the range of 5% to 12% depending on the benchmark. This underperformance is statistically significant for the size-adjusted return, but is insignificant for the other two models. In the following six months (i.e., in the second half of the first year after an IPO) the magnitude of underperformance ranges from 15% to 31% depending on the benchmark and is statistically significant for all models. This underperformance continues in the second year after the IPO. In the third year, these two groups earn statistically equal returns.

The returns across various holding periods provide remarkably different pictures of the price patterns of the manipulated and non-manipulated IPOs. IPOs with tie-in agreements experience about 110% higher first-day returns than the non-manipulated IPOs and continue to earn higher returns in the first week after the offer. Starting with the sixth month (typically 30 days before the lock-up expiration date) after an IPO, there is significant underperformance by manipulated firms relative to the non-manipulated sample. When we analyze the return patterns of class-action-manipulated IPOs in Panel B of Table 3, we find similar results. Due to the larger sample size of manipulated IPOs based on this definition, we now obtain stronger statistical significance for return differences across the two sets of firms. The class-action-manipulated IPOs continue to earn significantly higher returns for the first five months. In the sixth month they significantly underperform the non-manipulated group in the range of -10% to -11% depending on the benchmark. The underperformance continues for three years post-IPO. Overall, the evidence from post-IPO returns is consistent with the first three predictions from Section 3. Manipulated stocks exhibit high first-day returns, continue to run-up for a period up to five months and then subsequently underperform.

Our model of underwriter manipulation predicts that manipulated stocks trade more often than non-manipulated stocks. For manipulation to be successful, the momentum investors must be present and buying in the aftermarket. The ladderers must also buy and sell shares. Table 4 provides the average turnover across manipulated and non-manipulated firms for the first six months after the IPO. As with the first day turnover, we define turnover as shares traded divided by shares offered in the IPO and divide NASDAQ firm's turnover by a factor of two to account for the double counting of NASDAQ trades. We noted earlier that on the first day of trading, manipulated IPOs trade considerably more often than the non-manipulated IPOs. In the first week after an IPO, the pattern continues. Based on the SEC-manipulated definition, we find that manipulated stocks have 8.92% higher turnover (t-statistic of 4.06) than the non-manipulated stocks. Based on the class-action-manipulated definition, the difference increases to 10.07% (t-statistic of 9.17). Since turnover data exhibits skewness, we also analyze the differences in log turnover across the two groups. Based on log turnover (unreported), the difference between the SEC-manipulated and the non-manipulated IPOs remains positive and highly significant. In the following months, manipulated IPOs (based on both classifications) continue to trade significantly more often than the rest of the firms as shown in Table 4. This finding is consistent with the manipulation hypothesis, the sixth prediction from Section 3, and previous studies of stock market manipulation. Aggarwal and Wu (2005) find that manipulated stocks exhibit higher turnover than do similar stocks matched on size and measures of risk. Mei, Wu and Zhou (2005) also find that the turnover of stocks subject to pump-and-dump manipulative schemes is much higher than that of non-manipulated stocks.

4.4. Returns around Lock-up Expiration

To further enhance our understanding of who benefits and who loses from tie-in agreements, we next investigate the returns around the lock-up expiration, typically six months after the IPO. For this analysis, we only have data for a sub-sample of 470 firms for which the lock-up expiration date is available on SDC. Since our sample size drops significantly due to non-availability of lock-up expiration dates, we focus our attention on class-action-manipulated IPOs only for this analysis. Out of 470 firms for which we have data available, 54 fall into the class-action-manipulated category.[13]

We report the results in Table 5. For the full sample of 470 firms, we find a negative market adjusted return (CAR) of –2.47% (raw return of –2.37%) in the event window starting three days prior to the lock-up expiration date and ending three days after. For the same event window, Brav and Gompers (2003) find market-adjusted returns of –1.08% in their sample of over 2,700 IPOs for the 1988-1996 period. The price-drop around the lock-up expiration date has increased for our sample period relative to earlier periods. We find that the manipulated

[13] We only have lock-up expiration data for 5 sued firms if we classify them based on the SEC lawsuits.

IPOs experience market-adjusted cumulative returns of -8.56% in the t-3 to t+3 event window versus -1.68% for the non-manipulated IPOs. Thus, while the price-drop for the non-manipulated IPOs is comparable to the earlier Brav and Gompers sample period, the manipulated IPOs exhibit a significantly larger price-drop around the lock-up expiration.

One possible explanation for the large difference between manipulated and non-manipulated IPOs is that insiders and other investors subject to lock-up restrictions sell a large quantity of shares at the first possible opportunity in those IPOs with artificial manipulation of prices. We showed earlier that the manipulated IPOs exhibit higher returns for the first few months after the offer date. Subsequently, starting from the sixth month, these firms exhibit significant negative returns for the following two years (with some variation depending upon the definition of manipulation used). Using our full sample of 908 IPOs and using returns in the sixth month as a crude proxy for the lock-up event window return, we find that cumulative returns for manipulated IPOs (based on SEC-manipulated IPOs and using size-adjusted returns) from the offer date to six months post-IPO are 131.15%. If insiders are released from the lockup period early as suggested by Brav and Gompers (2003), the returns for the insiders in the manipulated IPOs would be even higher. The comparable figure for non-sued firms is 31.84%. These results suggest that insiders are able to profit from the manipulation. One interpretation of the return dynamics around the lock-up expiration is that insiders know the stock is being manipulated (or are participating in the manipulation) and the stock is overvalued. They are selling the stock in expectation of lower prices when the true value of the stock is revealed in the long-run.[14]

4.5. Fundamental Valuations

Given that our theory suggests that manipulated IPOs will be overvalued at the time of the IPO relative to their long-run value, we would like to know if the manipulated IPOs exhibit overvaluation based on fundamentals. Specifically, are IPO offer prices high relative to

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[14] While insider trading is not a feature of our model, such a result would be consistent with our model. If insiders are unable to fully trade out of their positions at the lock-up expiration, then they will clearly receive lower returns over the subsequent eighteen to thirty months. Since the true value was low, any shares insiders are able to sell prior to the revelation of the true value allow the insiders to profit. While Table 3 shows that in the long-run non-manipulated IPOs perform better than manipulated-IPOs, it is worth noting that the non-manipulated IPOs are composed of both high-value stocks and non-manipulated low-value stocks. The manipulated IPOs are composed of manipulated low-value stocks, so insiders of manipulated IPOs profit whenever they can sell for more than the low value.

fundamental value for the manipulated stocks? Purnanandam and Swaminathan (2004) provide a methodology for estimating the degree of overvaluation of IPOs based on comparable companies. Fundamental value is based on either sales or profit margin (earnings before interest, taxes, depreciation, and amortization or EBITDA). For each IPO firm with available accounting data in the fiscal year prior to the IPO date, we find a comparable seasoned firm in the same industry (based on the Fama-French industry classification) with similar sales and EBITDA. The offer price to value (P/V) ratio is calculated as: 1) the ratio of the IPO firm's Price/Sales to the comparable firm's Price/Sales, and 2) the ratio of the IPO firm's Price/EBITDA to the comparable firm's Price/EBITDA. The P/V ratio is an objective measure of firm's overvaluation. Purnanandam and Swaminathan (2004) use the P/V ratio to explain cross-sectional variation in short-term and long-term returns for IPOs. They show that IPOs with higher P/V ratios consistently outperform low P/V firms on the first day of trading. Further, they show that the high P/V IPOs significantly underperform the low P/V IPOs in the long run. We use their methodology to compute the P/V ratio for our sample firms and take it as a proxy for perceived hotness and investor interest in these IPOs.

In our sample, there are 764 IPOs for which we have data available to compute the P/V ratio based on Price/Sales. Given that during our sample period a large number of firms going public had negative profits, there are only 257 IPOs for which we can compute the P/V ratio based on Price/EBITDA. In our sample, IPOs in general have very high P/V ratios based on their seasoned industry peers. The median P/V ratio is 4.59 using Price/Sales and 2.69 using Price/EBITDA. These ratios are much higher than the P/V ratio of 1.50 reported by Purnanandam and Swaminathan (2004) for the earlier period of 1980-1997.[15]

We next separately analyze the P/V ratios of manipulated and non-manipulated IPOs and find that the manipulated IPOs have considerably higher P/V ratios relative to the non-

[15] Aggarwal, Bhagat, and Rangan (2005) provide an explanation for the increase in P/V ratios based on endogenous changes in the way fundamentals are valued over time. Roughly, when expected discount rates decrease or growth rates increase, then the valuation of a given level of fundamentals (e.g., earnings) will increase for two reasons. The first is that any change in discount rates and growth rates will have a straightforward impact on value. If these changes affect both the IPO firm and the comparable firm equivalently, there will be no impact on the P/V ratio. On the other hand, if the impact of a change in, for example, growth rates is greater for an IPO firm than a comparable seasoned firm, then we should expect the P/V ratio to increase. Second, changes in growth rates and discount rates can increase the value of various forms of signaling (such as insider retention), which in turn will lead to more signaling by IPO firms and higher valuations resulting in higher P/V ratios. Thus, higher P/V ratios themselves need not imply overvaluation. What matters is the differential between the manipulated and non-manipulated IPOs' P/V ratios.

manipulated IPOs. While the non-manipulated IPOs have a median P/V ratio of 4.02 based on Price/Sales, the corresponding ratios for SEC-manipulated and class-action-manipulated IPOs are 9.21 and 8.13 respectively. Both of the manipulated groups' ratios are significantly higher than the non-manipulated group's P/V ratio at the 1% level of significance. Similarly, based on Price/EBITDA, the median P/V ratio for non-manipulated IPOs is 2.68, whereas the corresponding number for the class-action-manipulated IPOs is significantly higher at 23.10.[16]

The manipulated IPOs clearly command a much higher premium relative to their industry peers. Thus, manipulated IPOs appear much more likely to be overvalued than do non-manipulated IPOs during our sample period. While this finding is suggestive, it is not sufficient to conclude that manipulation is what causes the overvaluation. For example, that manipulated IPOs have higher P/V ratios along with the fact that the offer price is set much higher than the mid-point of the filing range could simply indicate that sued IPOs are perceived to be more glamorous than the rest of the IPOs. This would not be surprising since the vast majority of our sued IPOs are Internet firms. In addition, the first-day return and trading volume could be attributed to these characteristics of the IPOs. Thus, higher valuation (or overvaluation) of sued IPOs alone is not sufficient to show that manipulation matters. Next we show that individual IPO characteristics alone cannot fully explain the differences between the sued and non-sued IPOs.

5. Tie-in Agreements and IPO Underpricing

In the previous section, we showed that the price patterns for manipulated IPOs are quite different from the price patterns for non-manipulated IPOs. In this section, we provide formal tests of the relations between tie-in agreements, underpricing, turnover, and long-run returns. In particular, we want to control for general market conditions and issuer characteristics to better understand the patterns we observe. For example, an obvious concern is that these results may simply be a function of the fact that the majority of the firms named in the lawsuits are Internet/technology firms. Further, the majority of the named firms went public in 1999 and 2000. We want to control for these effects.

[16] For SEC-manipulated IPOs, there are only three IPOs with positive EBITDA in the year prior to the IPO. Therefore, we do not estimate the P/V ratio based on Price/EBITDA for this sample.

5.1. Multivariate Regressions of First-Day Returns

To assess the impact of manipulative practices of underwriters on IPO underpricing, we start by estimating a cross-sectional OLS regression with the first-day return as the dependent variable. Our primary variable of interest is a dummy variable "tie-in" that equals one if the firm was named in a lawsuit for tie-in agreements and zero otherwise. We provide results for both SEC-manipulated and class-action-manipulated definitions of tie-in agreements.

We include several control variables that have been documented in the literature as explanatory variables for IPO underpricing. The dummy variable "Tech" equals one if the firm is an Internet/technology firm, and zero otherwise. We want to separate the effect of tie-in agreements from the effect of simply being an Internet/technology firm. In addition, we include the log of proceeds raised by the firm as a measure of issue-size.[17] Following Brav and Gompers (1997), we include the dummy variable "VC," which equals one if the firm is backed by a venture capitalist and zero otherwise.

Barry (1989) and Habib and Ljungqvist (2001) argue that IPO underpricing is higher if firms sell purely primary shares in the IPO. When existing shareholders offer their own shares in the IPO, they have an incentive to minimize the extent of underpricing. We include the fraction of shares sold by the pre-IPO shareholders to control for this effect. This variable, called "insider sale," is defined as the ratio of (total shares offered minus primary shares offered) to (total shares offered). We also include the ranking of the underwriter as an explanatory variable to capture the effect of underwriter prestige on IPO underpricing. Beatty and Welch (1996) and Loughran and Ritter (2004) show that there was a negative relation between underwriter prestige and underpricing in the 1980s, which then changed in the 1990s.

To capture the effect of market conditions at the time of an IPO, we include the return on NASDAQ stocks over a holding period of 15 days prior to the IPO date. We also include a dummy variable that equals one if there was a positive price revision (from filing to offer) and zero otherwise. Revision from the filing price to offer price proxies for investors' interest in an IPO and provides a control for the perceived hotness of an IPO in our sample.[18]

Panel A of Table 6 provides results based on SEC-manipulated IPOs whereas Panel B repeats the analysis for class-action-manipulated IPOs. In each panel, we report the results of

[17] We have also repeated our analysis with prior year sales as a measure of size and obtained similar results.
[18] As a robustness check, in unreported results, we also include the Offer-Price-to-Value (P/V) ratio as described in section 4.5 as a measure of an IPO's perceived hotness. The results are similar.

two models – with and without year dummies. We do this because virtually all of the sued IPOs are from 1999 and 2000, which coincides with the Internet bubble period. Thus, we want to separately assess the impact of different years. Our results indicate that the tie-in dummy has positive and significant coefficients in all regression models. Tie-in agreements explain underpricing of 78-80% relative to non-manipulated IPOs. This is true even after controlling for various known determinants of IPO underpricing, such as whether the firm is an Internet company. This result is both economically and statistically significant for all four models. In terms of magnitude, the effect of tie-in agreements is larger than any of the other factors. From Table 2, the mean difference in first-day return (underpricing) between SEC-manipulated and non-manipulated IPOs is 109.9%. Of this, 78-80% is attributable to the manipulation dummy after controlling for other characteristics thought to influence underpricing.

We find in our analysis that IPO underpricing is positively affected by the state of the market (NASDAQ return variable) and hotness of the issue (as proxied by the filing revision dummy). To the extent that the hotness of the issue is capturing investor sentiment in the market, then greater investor sentiment is correlated with more underpricing, as predicted by the theory. We do not find a significant relation between insider sales and IPO underpricing. Similarly the relation between underwriter ranking and underpricing is insignificant. Depending on the model, the technology and Internet stocks earned 13-18% higher returns on the first day as compared to other stocks. In our estimation, the coefficients on the year 1999 and 2000 dummies are marginally significant and range in magnitude between 8.5 and 11%. This result is important—it suggests that once we control for the existence of manipulative practices, the effect attributed to the bubble period of 1999-2000 is relatively small in magnitude.

We have established that tie-in agreements positively affect the first-day returns of IPOs. For the SEC-manipulated IPOs, we have direct evidence of tie-in agreements as outlined in the SEC civil actions. Thus, there is no ambiguity about whether manipulation actually happened in these cases. However, there is a possibility of endogeneity between the class action lawsuits and IPO underpricing. It may be argued that investors sued these IPOs for tie-in arrangements only when the first-day returns were very high, followed by subsequent poor performance. For this reason, we emphasize results for IPOs named in the SEC civil actions, where we have direct evidence on the presence of tie-in agreements. Since we are also interested in the much larger number of class action lawsuits, we address the issue of endogeneity by using instrumental variables as discussed below.

5.2. Two-Stage Regression of Tie-in Lawsuits and First-Day Returns

Lowry and Shu (2001) use a two-stage approach to address the issue of endogeneity between IPO underpricing and IPO lawsuits. They argue that the trading volume on a matching firm (a firm similar to the IPO firm) can be used as an instrument for the probability of lawsuits. Our theoretical model provides an economic motivation for such an instrument. In our context, trading volume from recent IPOs tells us about the number of momentum investors in the market (or investor sentiment). We argue that recent IPO trading volume is a good proxy for when there are more momentum traders in the market, and thus is a good instrument for when manipulation is likely to happen. For every IPO, we consider all IPOs in the prior one month and take the average first-week turnover (shares traded divided by shares offered) of these IPOs as an instrument for the probability of lawsuits faced by the IPO. We take a log transform of this variable to remove the skewness in turnover data.

Our theoretical motivation is that if underwriters engage in manipulative practices requiring IPO investors to buy additional shares on the first day of trading, the probability of tie-in could be correlated with the turnover in the IPO market, since turnover on these IPOs (investor sentiment) will indicate when it is worthwhile to have tie-in agreements. Aside from our theoretical motivation, there is no other economic reason to believe that there would be any correlation between the first-day return on an IPO and the trading volume of recent IPOs after controlling for general market conditions (which we do by including the return on NASDAQ stocks over a holding period of 15 days prior to the IPO date). Importantly, we separate the effect of our instrument from (and control for) the general hotness of the IPO itself by including the filing-to-offer revision in the first-stage regression. Thus, what we are capturing is the effect of investor sentiment in the market for IPOs rather than investor sentiment for the particular IPO. While this should weaken our instrument, it should also provide cleaner inference. To be concrete, if there are more momentum traders in the market as captured by high turnover on other recent IPOs, then underwriters should observe this and be more likely to engage in manipulation. This is the content of Prediction 5 from Section 3.

With this instrument, we estimate the following model in a two-stage instrumental variable regression framework:

$$R_0 = \sum \alpha_k X_k + \beta_{tie-in} Tie-In + \varepsilon_r ,$$

$$Tie-In = \sum \gamma_k X_k + \delta Turnover_{recent} + \varepsilon_t .$$

In the above structural equations R_0 refers to the first-day return on IPOs; X_k*'s* are all the control variables discussed earlier; *Tie-In* is a dummy variable that equals 1 for manipulated stocks and zero otherwise. $Turnover_{recent}$ refers to the turnover on recent IPOs. The return regression is estimated using an OLS model, whereas the tie-in equation is estimated as a Probit model. In the first stage we fit a Probit model with the occurrence of a lawsuit as the dependent variable and all the other explanatory variables including the instrument (turnover on the recent IPOs) as independent variables. The predicted value of the probability of "tie-in" lawsuits from the first stage regression is used as an independent variable in the second stage OLS regression.

5.2.a. First-Stage Regression

Table 7 provides the results of both first-stage probit estimation and second stage OLS estimation for our instrumental variable regression. In Model 1 of Table 7, we do not control for year dummies, whereas in Model 2, we do. The first stage estimations in the first two columns show that class-action lawsuits are positively associated with both overall market hotness and issue-specific investor interest as evident from a positive and significant coefficient on NASDAQ returns in the prior 15 days and filing-to-offer returns. Venture backed stocks, technology stocks, and stocks underwritten by prestigious underwriters are more likely to be manipulated. These findings are consistent with our theoretical motivation behind manipulation. First, we show in our model that manipulation is profitable only under hot market conditions. Second, in order for manipulation to be successful we require the underwriter to have enough market-power to capture the rents in subsequent periods from engaging in such activities. Larger and more prestigious underwriters are more likely to be able to do so.

It is important to obtain statistically valid instruments (i.e., a properly identified system with satisfactory order and rank conditions) for making meaningful inferences in a two-stage regression model. We perform various econometric tests to ensure this. Since we only have one endogenous explanatory variable (tie-in) and one instrument (turnover on recent IPOs), our model is exactly identified and the order condition is easily satisfied (see Wooldridge (2001), page 93). Further, in the first stage Probit regression, the coefficient on the instrument (i.e., turnover on recent IPOs) is positive and significant with a p-value of 0.01 (t-statistic of 5.15 in Model 1 and 2.78 in Model 2), which ensures that the rank condition is also satisfied. The pseudo R-squared of the first stage Probit regression is reasonably high at 26.06% for Model 1 and 27.90% in Model 2. These tests provide evidence in support of the econometric validity of our instrument. Even with a valid instrument, a weak instrument can aggravate the effect of simultaneity bias, rather than solving it. In particular, the IV estimates may be inconsistent and even biased for finite sample sizes (see Bound, Jaeger and Baker (1995), Staiger and Stock (1997) and Ljungqvist (2005)).

To estimate the strength of the instrument, Bound et al. suggest reporting the partial R^2 and the F-statistics from the first stage regression. High levels of partial correlation (bounded away from zero) and joint significance of the instruments in the first stage regression ensures that the instrument is strong. The partial R^2 is calculated as the difference between the pseudo R^2 of the first stage regression with and without the instrument. For Model 1, we find that the partial pseudo R^2 is high at 3.87% (26.06% with instrument and 22.19% without it). Further, the joint significance of all variables is high at the 0.00001 level in the first stage probit estimation. Similarly, for Model 2, which also includes year controls, the partial pseudo R^2 is bounded away from zero at 0.98% (27.90% with instrument and 26.92% without it) . The joint significance remains high at 0.00001 for this model as well.[19]

[19] We also experimented with an additional instrument for class-action manipulated IPOs, suggested by our WFA discussant Grace Hao. We take average daily turnover during the past four months (about 90 trading days) for all seasoned firms in the same 4-digit SIC codes as the IPO firm. If we do not find firms in same 4-digit SIC codes in the past four months, we drop those firms (46 observations). If we substitute turnover on 3-digit SIC codes for these 46 firms, the results are similar. Turnover is computed in a similar fashion to our base instrument. We take a log transform of the average turnover variable and use it as an instrument instead of turnover on recent IPOs. When we repeat our analysis with this instrument, we find that its statistical properties are not desirable for a two-stage estimation. It has an insignificant coefficient in the first stage regression and it produces a partial R^2 of 0.02% (21.18% with instrument and 21.16% without it) indicating that this instrument suffers from the weak instrument problem discussed by Staiger and Stock (1997) and others. Thus we discard this instrument in favor of turnover on recent IPOs as discussed above.

5.2.b. Second-Stage Estimation

The estimation results are also provided in Table 7. In the third and fourth columns, we replace the tie-in dummy with the estimated tie-in probability from the first stage regression. All standard errors in the second stage estimation are corrected for simultaneous equation bias (see Maddala (1983)). Our main results remain similar. Consistent with the manipulation hypothesis and Prediction 4 from Section 3, we find a positive and significant coefficient on the tie-in probability variable. Firms with higher likelihood of tie-in manipulation earn significantly higher first-day returns. The economic magnitude of this effect decreases to about 25% to 34% from 78% in the model without endogeneity correction (Table 6). However, the tie-in probability remains one of the most important determinants of the cross-sectional returns on the first day of IPO trading. In the instrumental variable regression, the effects of NASDAQ returns and filing-to-offer revision remain the same as in the OLS regression. Now we find that the year dummies have become insignificant and even negative. This suggests that the extremely high level of underpricing in the years 1999 and 2000 is successfully explained by the subset of manipulated IPOs and other known determinants of IPO underpricing.[20]

5.3 First-day return for initial winners and long-term loser IPOs

One of the concerns with our analysis is that investors sued those firms in which they lost money in the long-term leading to the endogeneity problem discussed earlier. We addressed this issue with the IV regression model. As a further robustness check, we focus our attention on subsets of those IPOs in which initially stock prices went up considerably only to revert back to very low levels in the long-run. This subset of high initial gains and high subsequent losses limit our sample considerably, but allows us to investigate the first-day returns across manipulated and non-manipulated (i.e., sued vs. non-sued) firms independent of the observed price pattern post-IPO. If lawsuits are highly correlated with such patterns, i.e., if investors are suing firms with high initial gains (presumably because they bought it at high price) and then had long-term poor performance, our sub-sample should be able to separate out

[20] The t-statistic of the “tie-in probability” variable decreases to 2.18 in Model 2 of Table 7. This is not surprising since we use year dummies (for 1999 and 2000) as additional controls in this model. Almost all of our manipulated IPOs occurred during these two years. Our instrument for tie-in agreements is a calendar-time based measure (namely the turnover on all IPOs in the one month prior to the IPO). Therefore, when we use year dummies, they capture a part of the correlation between our instrument and tie-in agreements.

such effects. A difference in first-day returns across manipulated and non-manipulated IPOs on this sub-sample will be evidence that our results are not mechanically driven by the subset of hot IPOs with poor long run performance.

In Table 8, we provide results based on five models—each model uses a different algorithm for finding initial winners and subsequent losers. In all models, to classify firms, we remove the first day return from our classification criteria. We do this because our dependent variable is underpricing (i.e., first day returns) and we do not want to sort on our dependent variable. In Panel A, we provide the difference in first-day return across manipulated and non-manipulated IPOs for each of the five models. In Model 1, we focus on the 280 firms that earned positive returns in the first five months following the IPO (excluding the first day return) and earned negative returns from month 6 through year 3. There are 202 non-manipulated and 78 manipulated IPOs in this sub-sample. We find a difference of 88.77% in the first day return of manipulated and non-manipulated IPOs with statistical significance at the 1% level. In Model 2, we limit our attention to only those IPOs that earned above median returns (based on the overall sample) in the first five months and then below median returns in month 6 through year 3. With 222 firms in this sub-sample, we find that manipulated IPOs earned statistically significant higher returns of 90.10% than the non-manipulated IPOs on day 1. In Model 3, we investigate firms with poor long-term returns—those with below median returns from month 6 through year 3. Model 4 uses extremely good performers in the first five months by focusing on firms with returns in top quartile of the sample distribution. Finally, Model 5 uses IPOs in the bottom quartile of returns over the three year post-IPO life including the first five months of returns. All five models show a consistent pattern of very high difference in initial returns across manipulated and non-manipulated IPOs.

Next, we run our instrumental variables regression on these five sub-samples and provide the results in Panel B of Table 8. Due to the smaller sample sizes, the t-statistics on ‘tie-in probability’ are lower as compared to the results in Table 7. But for every model, we find a positive and significant (8% or better) coefficient on the tie-in probability variable. The economic magnitude of the estimate (25% to 45%) is comparable to the IV estimation results with all IPOs in the sample. Thus we conclude that our results are not driven by IPOs with initially big gains and large losses later.

5.4. First-day Trading Volume

In addition to predicting higher first-day returns (or underpricing) for manipulated IPOs, our theory also has predictions for trading volume. The determinants of the trading volume of an IPO in the immediate aftermarket are not well understood since the current theoretical models and empirical studies have mostly focused on explaining the observed pattern of the first-day return. Our model predicts that in the presence of manipulation, manipulated stocks will exhibit higher trading volume than other IPOs. Consistent with this hypothesis, we showed in our univariate analysis that manipulated IPOs have significantly higher turnover than the non-manipulated group in the aftermarket. In this section, we provide a more formal test of this hypothesis.

First-day turnover is defined as the shares traded on the IPO date scaled by the shares offered in the public issue. We take a log transformation of this variable to remove skewness from the data. As in the first-day return regression, for the class-action-manipulated IPOs, we consider the probability of lawsuits and first-day trading volume as endogenous variables in our model.[21] We obtain the probability of lawsuit in the first stage regression by fitting a Probit model on all exogenous variables that enter our first-day return regression, so the probability of tie-in variable that we use for the turnover model is the same as in the return model. Subsequently, in the second stage regression, we regress the first-day turnover on the predicted probability of tie-in and control variables. Unlike the return model, however, we do not have well developed theoretical guidance on the cross-sectional determinants of trading volume. Therefore, we use proxies for market conditions (NASDAQ return on prior 15 days), the perceived hotness of the IPO (filing to revision offer dummy) and the size of the offer (log proceeds) as our control variables in the regression.

We provide results based on the SEC-manipulated IPOs in Panel A of Table 9. Panel B provides results from the second stage regression with the instrumented tie-in probability for the class-action-manipulated IPOs. Our results support the hypothesis that IPOs with tie-in agreements have significantly higher turnover than the non-manipulated IPOs on the first day of trading. These results are consistent with the idea that greater investor sentiment in general

[21] To save space, for class-action based definitions, we only present results for the instrumental variable regression. Our estimation results, without endogeneity correction, provide qualitatively similar estimates.

in the market for IPOs leads to an increased likelihood of manipulation and hence greater turnover for those manipulated IPOs (Prediction 6 from Section 3).

5.5. Medium to Long-Run Returns

As a final test, we extend our analysis to longer run returns. We explore the relation between the existence of tie-in agreements and post-IPO returns. We want to see if the estimated tie-in probabilities for the class-action lawsuit based definition of manipulation explain returns over several time horizons: 1) between the end of the first day of trading and five months post-IPO; 2) in the sixth month post-IPO; and 3) from the end of six months till three years post-IPO. We break-up the post-IPO holding periods into these three groups to capture the effect of the lock-up expiration more precisely. Specifically, we believe that ladderers will want to exit their positions before insiders are released from their lockup agreement and start selling.

We compute the buy-and-hold return for IPO firms for the three holding periods. As in Ritter (1991) and Loughran and Ritter (1995), if a firm delists during the holding period, we compute returns till the delisting date only. We also compute the buy-and-hold return for the value weighted market index for the corresponding holding period. We define the market-adjusted log buy-and-hold abnormal return (LBHAR) as follows:[22]

LBHAR=log[(1+ IPO Buy-and-Hold Return)/(1+Market Buy-and-Hold return)].

The LBHAR measure should suffer less from the skewness problems typically associated with the traditional buy-and-hold abnormal return and therefore has better distributional properties. To assess the impact of manipulation on post-IPO returns, we regress LBHAR on predicted tie-in probabilities from the class-action-manipulated definition and the control variables. Consistent with existing empirical work, we control for firm size and the book-to-market ratio of the IPO in these cross-sectional regressions.[23] The results are reported in Panel A of Table 10. We estimate the model with 766 observations. We miss a few observations for these

[22] Ritter (1991) investigates the long-term performance of IPOs by constructing a wealth-relative measure of IPO's buy-and-hold returns and benchmark firm's buy-and-hold returns. Our LBHAR measure can be interpreted as the log wealth relative with respect to the market return (see Purnanandam and Swaminathan (2004)).

[23] To compute the book-to-market ratios, we take the book values from the most recent fiscal year after the IPO. This ensures that we have accounted for the proceeds from IPO in the computation of book-to-market ratios.

regressions due to the unavailability of data on book values. In Panel B, we restrict our sample to IPOs with positive book-to-market ratios and replicate our analysis with a subset of 663 firms. Our results are similar for these two samples.[24]

The tie-in agreement probability is significantly related to returns in the first five months excluding the first day of trading. Interestingly, the tie-in agreement probability is negatively and significantly related to returns in the sixth month post-IPO as well as returns in the long term. These results are consistent with our theoretical predictions. The tie-in agreement leads to price increases both when the ladderer establishes the position (on the first day of trading) and then when the ladderer exits the position by selling to the momentum investors over the next few months. Subsequently, from the sixth month after the IPO, the stock price is reverting to its true value as insiders begin to trade out of their holdings. The tie-in agreement probability is negatively and significantly related to returns between six months and three years post-IPO. This result is consistent with the idea that reversion to the true value occurs over a fairly long time horizon. Overall, these results are consistent with Predictions 3 and 4 from our model.

5.6. Fama-French Regression Results

It is well known that the long-run return studies suffer from statistical biases such as clustering of data in event time and skewness in stock returns. This has led to disagreements over the correct methodological approach (see Barber and Lyon (1997), Kothari and Warner (1997)) to detect long-term performance after any corporate event. Earlier, we presented event-time, long-term, buy-and-hold returns. As an alternative, we investigate the calendar time based Fama-French regression across manipulated (based on class-action lawsuit) and non-manipulated stocks in this section. Calendar time returns are less likely to suffer from biases due to overlapping observations in buy-and-hold returns and therefore produce reliable t-statistics. Further, we do not need information on the book-to-market ratios of all IPOs to estimate the Fama-French regression model. To investigate the short-to-medium term returns post-IPO, we estimate the three-factor model with returns for the first five months after an IPO. Next, for long term returns we estimate the three-factor model with returns starting from month

[24] We have also examined the LBHAR for the SEC-manipulated IPOs (unreported). The results are of the predicted sign but are insignificant, presumably due to the relatively small sample.

six and ending with the third year anniversary of the IPO. For the short-to-medium term analysis, each IPO is allocated to either the manipulated or non-manipulated portfolio starting from the day after the IPO (i.e., excluding the first-day returns) and it remains in the portfolio for the first 150 days. For long-term returns, the IPOs enter the portfolio from day 151 and remain there until their third year anniversary or delisting, whichever is earlier. While a regression involving monthly observations is preferable, due to fewer observations in our sample we estimate the following daily Fama-French regression model for short-to-medium term and long-term:

$$r_{pt}-r_{ft}=\alpha_p + b_p(R_{mt}-r_{ft}) + s_pSMB_t + h_pHML_t + u_t$$

In this equation r_{pt} is the daily return on the manipulated, non-manipulated, or hedged (manipulated minus non-manipulated) IPO portfolio for day t; r_{ft} is the risk-free rate; R_{mt} is the value weighted market return for day t; SMB_t is the return on small capitalization stocks minus large capitalization stocks for day t and HML_t is the return on high book-to-market firms minus low book-to-market firms. All factor returns are obtained from Ken French's website. Since Fama-French regressions put equal weight on average portfolio returns in a given period (days in our estimation), we need to be careful with days with very few observations. To prevent our results from being contaminated by days with very few IPOs, we require that a given trading day have at least 10 stocks in both manipulated and non-manipulated portfolio to enter our estimation. We have 406 trading days for the first-five months returns and 1258 trading days for long-term returns that meet our filter with which to estimate our regression models.

Panel B of Table 10 presents the results. The results from the first five months regression show that the manipulated IPOs earned about 35 basis points higher daily returns than the non-manipulated group after controlling for the effects of market return, SMB and HML. This is economically large (about 127% on annualized basis) and statistically significant at the 1% level. The long-term regression (month six through year 3) shows that the manipulated IPOs underperform non-manipulated IPOs by about 5.8 basis points on a daily basis – translating into about 21% per annum underperformance after controlling for the three risk factors. This difference is statistically significant with a 0.088 p-value. Thus Fama-French regressions confirm our earlier findings that manipulated IPOs continue to run-up for the first five months and, starting with the lock-up expiration month, they underperform.

The Fama-French regressions also provide insights into the characteristics of manipulated and non-manipulated stocks. As seen in Table 10 (both short-to-medium term and long term), both groups have positive and significant coefficients on SMB, indicating that returns on our sample of IPOs move together with returns on small firms – this pattern is consistent with the fact that IPOs are smaller firms. While the coefficient on the HML factor is statistically zero for non-manipulated IPOs, manipulated IPOs behave more like glamour stocks as evident by the negative and significant coefficient on this factor. This is consistent with the fact that manipulated IPOs are more likely to be technology firms and technology firms behaved like glamour stocks during our sample period. It is also consistent with investor sentiment being high in these stocks.

Overall, our results establish an interesting relation between tie-in agreements and first-day IPO returns and turnover and suggest that manipulation explains a large portion of the abnormal IPO underpricing during the late 1990s. Further, the price patterns during the first few months after the IPO, in the lock-up expiration period, as well as in the long-term provide additional evidence that manipulation explains the subsequent poor performance of those IPOs with tie-in agreements.

6. Conclusion

In this paper we provide an alternative explanation for the extremely high level of IPO underpricing during the Internet bubble years of 1999-2000. Our explanation is based on manipulative practices in the form of tie-in agreements adopted by the underwriters during the IPO process. By requiring their IPO customers to buy the stock in the aftermarket in return for IPO allocations, the underwriters created artificial excess demand for the IPOs, leading to distorted price levels in the immediate aftermarket. We show that the IPOs with tie-in agreements exhibit a distinctly different pattern for the first-day and long-term returns from the rest of the IPOs during the same period. IPOs with tie-in agreements have considerably higher filing-to-offer revisions, seven times higher first-day returns, and considerably higher first-day trading volume than do IPOs without tie-in agreements. However, IPOs with tie-in agreements underperform significantly in subsequent periods relative to IPOs without tie-in agreements.

We find that the tie-in IPOs experience significantly lower returns around the lock-up expiration event window. Our results are robust to controlling for hot market conditions and issuer characteristics such as insider sales, underwriter quality, venture capitalist backing, and whether the firm is an Internet/technology company. In addition, our results are robust to controlling for potential endogeneity in the selection of manipulated IPOs. Our main result is that manipulation explains much of the unusual IPO underpricing in the late 1990s.

More generally, to the extent that there was misallocation of capital during the late 1990s, our results suggest that focusing only on investor sentiment is insufficient to understanding how price signals were distorted. While investor sentiment is clearly an important factor (and in our model, is necessary), it alone need not lead to price distortions. The willingness of informed market participants—in this case, underwriters operating through affiliated investors—to distort demand through manipulation for private gain is essential to understanding why prices deviated so dramatically from fundamentals.

Appendix

To provide some theoretical guidance in understanding tie-in agreements, we consider a simple model of IPO price manipulation based on Aggarwal and Wu (2005). There are three types of investors in our model. First, there is an investor affiliated with the underwriting investment banker. This investor gets information from the investment banker about whether the stock value in the future will be high (V_H) or low (V_L). If the affiliated investor learns that the stock value in the future will be high, then the affiliated investor (superscripted I in this case) will choose to trade on this information by buying shares. If the affiliated investor learns that the stock value in the future will be low, then the affiliated investor (superscripted M in this case) may still choose to buy shares in order to manipulate the stock via a laddering arrangement with the underwriter.[25]

The second group of investors is N symmetric momentum investors (superscripted A^i, $i \in N$). One can also think of them as being arbitrageurs, day-traders, sentiment investors, or information seekers. The momentum investors are limited to several types of information. They can observe past prices and volume. They have no access to fundamental information themselves. Instead, they try to infer from prices and volumes whether an affiliated (informed) investor is buying the stock, and whether they should be buying the stock as well, i.e., trading on momentum. Aside from having limited information, they are in all other respects completely rational.

The third group of investors is a continuum of noise or uninformed traders (superscripted U). These traders do not update or condition on any information. They simply stand ready to sell shares, so their role is to provide liquidity to the market. We model the uninformed traders as providing a supply curve to the market that determines the market price:

$$P(Q) = a + bQ \,, \qquad (1)$$

where P is the market price of the stock, Q is the quantity demanded, and b is the slope of the supply curve. We assume that initially all IPO shares are allocated to the uninformed traders

[25] One may wonder why the affiliated investor, knowing that the stock value will be low in the future, does not short sell to take advantage of this information. Here the affiliated investor is affiliated with the investment bank that has taken the company public. Short-selling would undercut the underwriter's price-stabilization efforts and so it will not be observed. Our point here is to examine what other mechanisms (specifically, manipulation by laddering) exist for underwriters and affiliated investors to profit from their information.

and the affiliated investor, who comprises part of the supply curve. If no one wishes to purchase the stock, then the price of the stock is simply *a*. For completeness, we assume that the total number of shares outstanding after the IPO is:

$$\frac{V_H - a}{b}.$$

This implies that if someone wished to buy all of the shares outstanding after the IPO, the price would be V_H. It is important to note that this is not because the uninformed update about the stock's value. Instead, it is simply governed by the uninformed's willingness to sell more if offered a higher price.

The timing of the model is as follows. At time 0, all shares are held by the uninformed investors and the affiliated investor. We can think of *a* as being the time 0 price (IPO offer price). We will say more shortly about what this IPO offer price is.

At time 1, the affiliated investor can enter the market. The affiliated investor is the ladderer (and has information that the value is low) with probability γ, and has information the value is high with probability δ. Since telling the affiliated investor the future stock value is high (V_H) is clearly a profitable strategy for the underwriter when the future stock value is in fact high, this is equivalent to saying the probability the future stock value is high is δ. With probability $1-\gamma-\delta$ the affiliated investor does not enter the market and the future stock value is V_L. It is worth noting that the underwriter through the affiliated investor will not always try to manipulate the IPO by laddering when the future stock value is low. If the probability of laddering is too high, then the market will break down in the sense that momentum investors will not be willing to purchase shares. The momentum investors observe the stock price and the quantity demanded at time 1, which we can think of as the first day of trading.

At time 2, the momentum investors can buy shares. They will condition the number of shares they purchase on what they observed at time 1. If there is no purchase of shares at time 1, it is natural to assume that the stock will be short sold until its value is driven to V_L. At time 2, the affiliated investor can buy or sell shares. Here we focus on the case in which the affiliated investor sells shares. At times 1 and 2, the uninformed investors stand ready to sell shares. We can think of time 2 as the period after the first day of trading, which may extend as far as the lockup expiration (six months after the IPO).

At time 3, the fundamental stock price is revealed to be either V_H or V_L. We make an additional assumption about the affiliated investor. We assume that the affiliated investor dislikes holding shares until time 3. Time 3 represents the long-run, when stock prices have adjusted to fundamental values. This could be from after the lockup expiration to as much as three years post-IPO. The long-run may be very long, and thus it may be costly to hold shares for the affiliated investor. In particular, the affiliated investor is typically not a buy-and-hold investor (nor is the investment bank that provides the information). We model the cost of holding shares until time 3 as a scalar k. If the stock price at time 3 is V_H, the value to the affiliated investor of a share is V_H-k. In order for our problem to be meaningful, it must be the case that V_H-k-a>0, otherwise no affiliated investor would ever buy shares at the IPO (time 0) price and hold them until time 3, and this would then be fully anticipated. There is no cost for the affiliated investor to holding a share until time 2. Note that, if the affiliated investor learns that the stock value is low, he would not want to hold shares until time 3 because the value of the share will be revealed to be V_L.

Now we consider what happens when the affiliated investor and underwriter can potentially manipulate the stock price through a tie-in agreement. The affiliated investor is a ladderer with probability γ. In equilibrium, it will be the case that $\gamma<1-\delta$. We solve the model backwards.

The momentum investors condition their demand at time 2 on what they observe at time 1. They explicitly account for the possibility that the purchaser of the shares is a ladderer. In this case there is a multiplicity of equilibria. We focus on the pooling equilibrium here.[26] It is convenient to talk about the affiliated investor who knows the value is high (the informed) and the affiliated investor who knows the value is low (the ladderer) as separate entities, rather than just as the affiliated investor with different information (high or low future stock value).

The ladderer and the informed pool in their strategies by buying the same quantity of shares at time 1 and selling these shares at time 2. Since the ladderer and the informed choose to purchase the same number of shares at time 1, the momentum investors' posterior beliefs that the purchaser of the shares is the ladderer are:

[26] A discussion of other equilibria and a complete derivation of the results for the pooling equilibrium we discuss here can be found in Aggarwal and Wu (2005).

$$\beta = \frac{\gamma}{\gamma + \delta} \ .$$

In this setup, Aggarwal and Wu (2005) show that the aggregate demand from the N momentum investors at time 2 is:

$$q_2^A = \frac{N}{N+1} \frac{(1-\beta)V_H + \beta V_L - a}{b} . \tag{2}$$

The time 2 price is:

$$p_2^M = p_2^I = a + \frac{N}{N+1}\left((1-\beta)V_H + \beta V_L - a\right). \tag{3}$$

Each momentum investor makes expected profits of:

$$\pi^{A_i} = \frac{\left((1-\beta)V_H + \beta V_L - a\right)^2}{(N+1)^2 b} . \tag{4}$$

Several points emerge from this analysis. First, the aggregate demand from the momentum investors is increasing in the number of momentum investors. Second, as a result, the price paid by the momentum investors to buy shares in the aftermarket (at time 2) will be increasing in the number of momentum investors. Third, as a consequence of the preceding two points, the individual and aggregate profits made by the momentum investors decreases in the number of momentum investors. As the momentum investors compete with each other, they erode their potential profits. Thus, in periods when momentum or investor sentiment is high, returns will be eroded.

Working backwards, Aggarwal and Wu (2005) show that the time 1 quantity demanded by the informed and the ladderer is:

$$q_1^M = q_1^I = \frac{N}{N+1} \frac{(1-\beta)V_H + \beta V_L - a}{2b} , \tag{5}$$

and the price is:

$$p_1^M = p_1^I = a + \frac{N}{N+1} \frac{(1-\beta)V_H + \beta V_L - a}{2} . \tag{6}$$

It is straightforward to see that as long as there is at least one momentum investor, the quantity of shares bought by the momentum investors at time 2 is greater than the quantity of shares bought at time 1 and sold at time 2 by the informed or the ladderer. Both the informed and the ladderer's expected profits from shares bought *at time 1* are:

$$\pi_1^M = \pi_1^I = \frac{N^2}{(N+1)^2}\frac{((1-\beta)V_H + \beta V_L - a)^2}{4b}. \quad (7)$$

Several points emerge from this analysis as well. First, the informed and the ladderer will purchase more shares at time 1 when the number of momentum investors is greater. Thus, volume at time 1 depends on the number of momentum investors even though the momentum investors are not active in the market at time 1. Second, as a consequence, the price at time 1 will be higher when the number of momentum investors is greater. Since in this model, time 1 corresponds to the first day of trading, higher prices at time 1 suggest higher first-day returns when the number of momentum investors is greater. However, momentum investors alone are not sufficient to generate the full set of price dynamics—the possibility of manipulation due to informational asymmetry is also critical, as we discuss below. Third, both the informed and the ladderer make more profits when the number of momentum investors is greater.

Next we consider the underwriter's decisions at time 0. The underwriter makes two choices at the time of the IPO. First, the underwriter chooses the IPO offer price. Second, the underwriter chooses an allocation to go to the affiliated investor (ladderer or informed). We assume that the underwriter can extract some of the profits from the affiliated investor in the form of higher than normal commissions, agreements for future business, or simple kickbacks. The underwriter extracts a fraction κ of the affiliated investor's profits from the affiliated investor's time 0 allocation. The other choice for the underwriter is the IPO offer price. We assume that the underwriter gets a standard fraction λ of the IPO proceeds (typically 7%, see Chen and Ritter (2000)). The IPO proceeds are the IPO offer price P_0 times the number of shares issued in the IPO Q_0, where we take the number of shares issued in the IPO as given.[27] The underwriter's objective function is:

$$\max_{q_0^M, P_0} \quad \kappa\pi_0^M + \lambda P_0 Q_0 \ .$$

This objective function is subject to several constraints. We begin by noting that allocating some shares in the IPO to the affiliated investor maximizes the underwriter's profits. The momentum investors demand more shares at time 2 than the affiliated investor bought at time 1. The difference is the maximum number of shares that can be allocated to the affiliated

[27] For simplicity, we take this number of shares to be $\frac{V_H - a}{b}$, but this is immaterial.

investor in the IPO so that the affiliated investor is able to sell all of his shares at time 2 to the momentum investors. Therefore, the number of shares allocated to the affiliated investor in the IPO is:[28]

$$q_0^M = q_0^I = \frac{N}{N+1}\frac{(1-\beta)V_H + \beta V_L - a}{2b}. \tag{8}$$

This is the number of shares allocated to the affiliated investor if the future stock value is high or if the underwriter wants the affiliated investor to manipulate the stock by buying additional shares at time 1 (laddering).[29] The underwriter does not allocate shares to the affiliated investor otherwise.[30] Given this number of shares allocated to the affiliated investor, the affiliated investor's profit from these shares, assuming they are sold at time 2 is:

$$\pi_0^M = \pi_0^I = \frac{N^2}{(N+1)^2}\frac{\left((1-\beta)V_H + \beta V_L - a\right)^2}{2b}. \tag{9}$$

Again, the number of shares allocated to the affiliated investor and the affiliated investor's profits are increasing in the number of momentum investors.

The other choice for the underwriter is the IPO offer price. The underwriter knows the distribution of stock values as well as the actual values for the firms going public. That is, the investment bank (and the market as a whole) knows that a fraction δ of the firm's going public will have high future stock values V_H (good firms) and a fraction 1- δ will have low future stock values V_L (bad firms). In addition, the investment bank knows which specific companies are good and bad. The investment bank can choose two pricing policies—either price at the expected value or try to price credibly according to the actual future firm value. Pricing credibly requires a commitment mechanism or costly signaling as the underwriter has the incentive to try to price a low value firm as if it were high value. However, even if the underwriter could price credibly (and costlessly), the underwriter's profits from the IPO proceeds are, in expectation, identical for pricing at the expected value or the actual value:

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[28] Note that we can also think of shares held by insiders at the time of the IPO (time 0) and then sold at time 2 (say, the lockup expiration) in this light—they are shares allocated by the underwriter to the insiders in the IPO who then sell at the manipulated time 2 price.

[29] One interesting feature of the number of shares allocated to the ladderer is that it equals the number of shares that the ladderer purchases at time 1. This is consistent with anecdotal evidence about laddering arrangements in which underwriters allocated shares to investors on the condition that they purchase an equivalent number of shares in the aftermarket.

[30] This is immaterial. We could just as easily assume that the underwriter always allocates the same number of shares to the affiliated investor, whether the future stock value is high or low. This will simply reduce the affiliated investor's net profits, as he now loses on some low value IPOs.

$$\lambda\left(\delta V_H + (1-\delta)V_L\right) = \delta\lambda V_H + (1-\delta)\lambda V_L. \quad (10)$$

By pricing at the actual value, the underwriter foregoes the possibility of there being future trade because all price relevant information has been revealed. By contrast, pricing at the expected value allows for price relevant information to be revealed over time, and allows the underwriter to profit from this information revelation and manipulation. Thus, the underwriter prices the IPO at its long-run expected value in order to profit from the underwriter's informational advantage. As a result, in expectation, there is no IPO underpricing for any IPO relative to their expected, fundamental, long-run value (this includes low value IPOs that are not manipulated, superscripted by L):[31]

$$p_0^M = p_0^I = p_0^L = \delta V_H + (1-\delta)V_L = a. \quad (11)$$

The preceding analysis is predicated on the existence of a pooling equilibrium. In order for the pooling equilibrium to be sustainable, it must be incentive compatible for the informed not to deviate and thus separate from the ladderer. The incentive compatibility condition requires that the informed will want to sell shares at time 2 rather than hold them until time 3. The value to holding shares until time 3 for the informed is V_H-k, so the incentive compatibility condition is:

$$p_2^I = a + \frac{N}{N+1}\left((1-\beta)V_H + \beta V_L - a\right) \geq V_H - k. \quad (12)$$

The key comparative static that emerges from this condition is that the possibility of pooling is increasing in the number of momentum investors. To the extent that we think of momentum investors as sentiment investors, this suggests that in periods of high investor sentiment, manipulation through laddering will be more likely. Further, there are two effects in our model of having a greater number of momentum investors. First, as just discussed and as shown in the incentive compatibility condition, more momentum investors make manipulation more likely. Second, more momentum investors increase the aftermarket prices and quantities when either the ladderer or the informed are in the market. In order for this second effect to occur, it must be the case that manipulation happens (at least probabilistically). If not, the model would not be able to generate price reversals, as we now discuss.

[31] In practice, even in the absence of manipulation, there is some amount of normal IPO underpricing. We take this normal IPO underpricing as given—our theory is designed to explain abnormal IPO underpricing.

Our model generates some other empirical predictions as well. The first of these is the time path of prices for manipulated versus non-manipulated stocks. For manipulated stocks, we should see:

$$p_3^M < p_0^M < p_1^M < p_2^M. \tag{13}$$

Prices first rise, then fall when the true value of the stock is revealed over time, because the manipulated stocks were, by definition, bad firms. This price pattern is also predicted by the model of Ljungqvist, Nanda, and Singh (2005). In that paper, the price run-up between the IPO offer price and aftermarket trade (between time 0 and time 1) is attributed to investor sentiment. Our model has both investor sentiment (momentum investors) and laddering. As a result, we are able to generate different price dynamics for manipulated and non-manipulated stocks. We next show what those dynamics are for non-manipulated stocks. In the text, we demonstrate that the price dynamics generated by our model can match the data for both manipulated and non-manipulated IPOs.

For non-manipulated stocks, expected prices will differ. As an empirical matter, for non-manipulated stocks, we do not observe high-value firms and low-value firms separately. Therefore, we must compute expected prices for the non-manipulated stocks. The time 0 (IPO offer) price will be the same for both manipulated and non-manipulated stocks, $p_0^M = p_0^{NM}$. The time 1 expected price for the non-manipulated stocks is composed of the prices from the high-value firms (that pool with the manipulated stocks), p_1^I, and the prices from the non-manipulated low-value firms, $p_1^L = V^L$:

$$p_1^{NM} = \frac{\delta}{1-\gamma}\left(a + \frac{N}{N+1}\frac{(1-\beta)V_H + \beta V_L - a}{2}\right) + \frac{1-\gamma-\delta}{1-\gamma}V_L. \tag{14}$$

The time 2 expected price for non-manipulated stocks is similarly composed of the prices from the high-value firms and the non-manipulated low-value firms:

$$p_2^{NM} = \frac{\delta}{1-\gamma}\left(a + \frac{N}{N+1}\left((1-\beta)V_H + \beta V_L - a\right)\right) + \frac{1-\gamma-\delta}{1-\gamma}V_L. \tag{15}$$

The time 3 (when the true values are realized) expected price for non-manipulated stocks is:

$$p_3^{NM} = \frac{\delta}{1-\gamma}V_H + \frac{1-\gamma-\delta}{1-\gamma}V_L. \tag{16}$$

The time path of prices we should see for non-manipulated stocks is:

$$p_1^{NM} < p_2^{NM} < p_0^{NM} < p_3^{NM}. \tag{17}$$

This last result may seem counterintuitive at first. The non-manipulated stocks consist of both the high-value firms in which the affiliated investor was able to invest as well as the low-value firms that the affiliated investor did not manipulate. Because some of the low-value firms were manipulated, the group of non-manipulated stocks has fewer low-value firms as a fraction of the group than does the population as a whole. However, prices for the high-value firms do not converge to their true values until the long-run (time 3) because of the possibility of manipulation through laddering. Thus, there are two offsetting effects. As a result, prices first fall from the IPO offer price and then rise with trade for the non-manipulated stocks before ultimately the true values are revealed.

As a consequence, the model has the following additional predictions:

$$p_0^{NM} = p_0^{M} \tag{18a}$$

$$p_1^{NM} < p_1^{M} \tag{18b}$$

$$p_2^{NM} < p_2^{M} \tag{18c}$$

$$p_3^{NM} > p_3^{M} \tag{18d}$$

$$\frac{\partial(p_1^{M} - p_1^{NM})}{\partial N} > 0 \tag{18e}$$

$$\frac{\partial(p_2^{M} - p_2^{NM})}{\partial N} > 0. \tag{18f}$$

The first statement is simply that the IPO offer price is the same for manipulated and non-manipulated stocks. The second and third statements are that manipulated stocks have higher prices (returns) during aftermarket trade in the short-run. The fourth statement is that this eventually reverses itself in the long-run, when non-manipulated stocks have higher prices than manipulated stocks. The fifth and sixth statements are important comparative statics. They say that the price differentials between manipulated and non-manipulated stocks at times 1 and 2 are increasing in the number of momentum investors in the market.

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Table 1
Yearly Distribution of IPOs

In this table we report the yearly distribution of IPOs in the sample. We follow Loughran and Ritter (2004) to classify firms into Internet/Technology stocks. Column IV provides the number of IPOs that were sued for tie-in agreements by the SEC. In column V we report the number of firms sued under class-action lawsuit for the tie-in agreement. The set of IPOs under column V represent IPOs sued by the class-action lawsuit but not by the SEC. The last column provides the proceeds raised by manipulated-IPOs (either under column IV or V) as a percentage of total proceeds raised by sample IPO firms in a given year.

I Year	II # of IPOs in Sample	III Internet/ Technology Stocks	IV Firms Sued by SEC	V Firms with Class-Action Lawsuits	VI % Proceeds raised by manipulated IPOs
1998	217	101	0	5	1.41%
1999	385	295	13	102	31.60%
2000	306	223	20	33	22.06%
Total	908	619	33	140	22.43%

Table 2
Characteristics of Sample IPOs

We provide the mean and median characteristics of sample firms in this table. Panel A is based on SEC lawsuits whereas Panel B is based on class-action lawsuits. The non-manipulated group under both panels represents the set of IPOs that were neither sued by the SEC nor by the class-action lawsuits. For both panels, we provide the mean and median characteristics of manipulated and non-manipulated firms. The last two columns report the difference and its t-statistics (Wilcoxson z-test for the median). Net proceeds are the amount of funds raised in the IPO net of underwriters' commissions. First-day return is computed from the offer price to the first-day closing price net of market return on the offer day. Filing to offer revision provides the percentage revision from the mid-point of filing range to the offer price. Underwriter ranking comes from Carter et al. (1998) as modified by Loughran and Ritter (2004). IPO Turnover represents the number of shares traded on the first day of trading as a percentage of shares offered in the IPO. Sales and EBITDA margins are from the fiscal year prior to the IPO.

	Mean				**Median**			
	Manip. Firms (I)	**Non-Manip. Firms (II)**	**I-II**	**t-stat**	**Manip. Firms (I)**	**Non-Manip. Firms (II)**	**I-II**	**z-stat**
Panel A: SEC-Manipulated IPOs								
Offer Price ($)	22.61	13.39	9.21	9.01	18.00	13.00	5.00	6.15
Net Proceeds (mn $)	105.68	74.66	31.02	1.96	76.75	52.35	24.40	3.45
First-day Return (%)	146.65	36.73	109.92	9.51	115.48	16.25	99.23	5.94
Filing to Offer Revision (%)	52.18	5.52	46.66	8.16	41.67	0.00	41.67	6.26
Underwriter Ranking	9.10	7.83	1.27	3.59	9.10	8.10	1.00	4.92
IPO Turnover (%)	81.42	57.45	23.97	3.70	74.41	54.17	20.24	4.41
Sales(mn $)	144.93	161.76	-16.84	-0.07	6.73	15.89	-9.16	-1.45
EBITDA Margin	-2.27	-7.42	5.16	0.47	-0.72	-0.15	-0.57	-2.38
% of firms with negative margin	86.36%	56.59%	29.78%	2.79				
% of firms with VC backing	82.76%	51.23%	31.53%	3.35				
Number of obs.	33	735						
Panel B: Class-Action-Manipulated IPOs								
	Mean				**Median**			
Offer Price ($)	16.58	13.39	3.19	6.77	16.00	13.00	3.00	6.64
Net Proceeds (mn $)	88.42	74.66	13.76	1.71	67.80	52.35	15.45	4.54
First-day Return (%)	146.66	36.73	109.93	16.75	120.08	16.25	103.83	13.97
Filing to Offer Revision (%)	36.52	5.52	31.00	10.30	31.01	0.00	31.01	10.19
Underwriter Ranking	8.72	7.83	0.89	5.54	9.10	8.10	1.00	5.33
Turnover (%)	83.00	57.45	25.55	8.14	78.44	54.17	24.27	8.95
Sales(mn $)	28.04	161.76	-133.72	-1.30	7.68	15.89	-8.21	-4.18
EBITDA Margin	-19.61	-7.42	-12.19	-1.64	-1.01	-0.15	-0.86	-5.37
% of firms with negative margin	84.75%	56.59%	28.16%	5.87				
% of firms with VC backing	81.43%	51.23%	30.20%	6.76				
Number of obs.	140	735						

Table 3
Holding Period Mean Returns

This table presents the buy-and-hold returns for manipulated and non-manipulated IPOs for various holding periods after the offer date. Panel A is based on SEC-manipulated stocks, whereas Panel B produces numbers for class-action manipulated stocks. The first sub-panel (Sub-Panel 1 in both panels) provides raw buy-and-hold returns. Sub-Panels 2 and 3 provide returns adjusted for (a) value-weighted market index and (b) returns on a similar size firm. For size adjustment, first we consider all seasoned firms (firms without an IPO in the last five years) on the CRSP tape as of the offer date and take the firm with closest market cap to the IPO firm (based on IPO firm's offer price and number of shares outstanding on the first day of trading) as the control firm. If this firm delists within the holding period, we take the next closest firm as the benchmark and so on. The size adjusted return is simply the difference between IPO buy-and-hold return and control firm's buy-and-hold return for the same period. In the first column of Sub-Panel 2, we provide the first-day return defined as (first-day closing price – offer price)*100 divided by offer price adjusted for the return on valued weighted market index of CRSP for the offer day. Since raw and size adjusted first-day returns are similar to market adjusted numbers, we only provide market adjusted returns for the first day. In the second column, we provide returns on the first week (first five days) of trading excluding the first-day return. In the column labeled 'First week-5 month', we provide returns for the first five month holding period excluding the first week's return. Subsequent columns provide returns for holding periods corresponding to (a) the sixth month post-IPO (b) 6-12 months after the IPO, (c) second year after the IPO ('yr2') and (d) third year after the IPO ('yr3'). We provide the difference between the two groups (manipulated minus non-manipulated) and the corresponding t-stats in the table. All t-stats are corrected for heteroskedasticity.

PANEL A: Based on SEC-Manipulated Stocks

	First Day Return	First Week Return	First week – 5 month	Sixth month	6-12m	yr2	yr3
Panel 1: Raw Returns							
All Firms		-0.81	6.61	-4.00	-10.50	-5.22	-1.64
Non-Manipulated		-1.17	6.56	-3.74	-9.57	-3.93	-2.19
Manipulated		7.18	7.53	-9.83	-31.80	-35.50	8.92
Difference		8.35	0.97	-6.09	-22.23	-31.57	11.11
t-stat		1.70	0.04	-1.16	-2.89	-2.07	0.71
Panel 2: Market Adjusted Returns							
All Firms	57.67	-0.84	5.11	-4.50	-11.40	1.15	6.68
Non-Manipulated	36.73	-1.19	5.02	-4.26	-10.70	1.99	6.25
Manipulated	146.65	7.05	7.20	-9.88	-26.00	-18.50	14.96
Difference	109.92	8.24	2.18	-5.62	-15.30	-20.49	8.71
t-stat	9.51	1.71	0.10	-1.14	-2.11	-1.40	0.62
Panel 3: Size Adjusted Returns							
All Firms		-0.88	0.98	-5.30	-16.00	-21.90	-10.50
Non-Manipulated		-1.24	0.81	-4.75	-14.60	-20.90	-10.80
Manipulated		7.15	4.65	-17.50	-46.10	-42.60	-5.71
Difference		8.39	3.83	-12.75	-31.50	-21.70	5.09
t-stat		1.57	0.17	-2.31	-3.18	-1.41	0.27

Table 3
PANEL B: Based on Class-Action Manipulated Stocks

	First Day Return	First Week Return	First week– 5 month	Sixth month	6-12m	yr2	yr3
Panel 1: Raw Returns							
All Firms		-0.08	18.86	-5.53	-9.79	-13.8	-6.32
Non-Manipulated		-1.17	6.56	-3.74	-9.57	-3.93	-2.19
Manipulated		5.61	83.44	-14.90	-11.00	-63.10	-24.90
Difference		6.78	76.88	-11.16	-1.43	-59.17	-22.71
t-stat		2.45	4.63	-3.82	-0.15	-7.57	-2.77
Panel 2: Market Adjusted Returns							
All Firms	57.67	-0.20	16.35	-5.97	-10.60	-6.11	2.99
Non-Manipulated	36.73	-1.19	5.02	-4.26	-10.70	1.99	6.25
Manipulated	146.66	5.01	75.83	-15.00	-10.20	-46.80	-11.70
Difference	109.93	6.20	70.81	-10.74	0.50	-48.79	-17.95
t-stat	16.75	2.31	4.35	-3.89	0.06	-6.43	-2.27
Panel 3: Size Adjusted Returns							
All Firms		0.01	11.52	-6.55	-15.30	-29.30	-13.40
Non-Manipulated		-1.24	0.81	-4.75	-14.60	-20.90	-10.80
Manipulated		6.53	67.65	-16.00	-18.70	-73.40	-27.30
Difference		7.77	66.84	-11.25	-4.10	-52.50	-16.50
t-stat		2.81	3.91	-3.52	-0.38	-5.62	-2.04

Table 4
Average Turnover

This table provides the average daily turnover for the first six months after an IPO for the sample of manipulated and non-manipulated firms. We define daily turnover by shares traded as a percentage of shares offered in the IPO and report the average of these daily turnovers over the given month in the table below. Panel A provides the turnover for manipulated and non-manipulated firms based on SEC lawsuits for the tie-in agreements. Panel B provides corresponding numbers for the firms sued under class action lawsuits. Column 1 provides the turnover for the first week after an IPO, subsequent columns provide the turnover for monthly intervals. The difference between the two samples and corresponding t-stats (corrected for heteroskedasticity) are provided in the table.

	Week1	*Month 1*	*Month 2*	*Month 3*	*Month 4*	*Month 5*	*Month 6*
Panel A: Based on SEC Manipulated IPOs							
All	17.08	5.68	2.07	1.72	1.63	1.74	1.89
Non-Manipulated	16.74	5.56	2.04	1.67	1.58	1.68	1.82
Manipulated	25.66	8.61	2.85	3.05	2.93	3.32	3.78
Difference	8.92	3.05	0.81	1.38	1.35	1.64	1.96
t-stat	4.06	3.08	1.72	3.77	3.54	3.67	3.56
Panel B: Based on Class Action Manipulated IPOs							
All	18.36	6.23	2.32	1.95	1.92	2.07	2.23
Non-Manipulated	16.74	5.56	2.04	1.67	1.58	1.68	1.82
Manipulated	26.81	9.72	3.77	3.38	3.7	4.09	4.4
Difference	10.07	4.16	1.73	1.71	2.12	2.41	2.58
t-stat	9.17	8.41	6.9	8.67	9.13	10.07	9.13

Table 5
Lock-Up Expiration Returns

We report the average returns for the sample of manipulated and non-manipulated IPOs around the lock-up expiration date. We also report the statistics for four different event windows around the lock-up expiration date (the event date). These windows correspond to: (a) three days prior to the event date till three days after the event date (-3 to +3), (b) two days prior to two days after (-2 to +2), (c) one day prior to one day after (-1 to +1) and (d) the event day (0) itself. This table is based on a sample of 470 IPOs (54 sued under class action lawsuits, 416 non-sued) for which the data on lock up expiration was available in SDC. Since there are only a few SEC-manipulated stocks with lock-up expiration date available, this table is based on class-action lawsuits only. Panel A produces the average raw return over event windows, whereas Panel B provides market adjusted return (i.e., return on IPO stock minus return on value weighted CRSP index).

	Cumulative Returns							
	Panel A: Mean Raw Returns				**Panel B: Mean Market-Adjusted Returns**			
Event Window	-3 to +3	-2 to +2	-1 to +1	0	-3 to +3	-2 to +2	-1 to +1	0
All Firms	-2.37%	-2.13%	-1.71%	-1.37%	-2.47%	-2.17%	-1.73%	-1.31%
Non-Manipulated	-1.57%	-1.21%	-1.04%	-1.03%	-1.68%	-1.30%	-1.10%	-1.02%
Manipulated	-8.56%	-9.24%	-6.87%	-4.01%	-8.56%	-8.84%	-6.56%	-3.56%
Difference	-6.99%	-8.04%	-5.83%	-2.98%	-6.88%	-7.53%	-5.46%	-2.53%
t-stat	-2.38	-2.95	-2.59	-2.20	-2.28	-2.86	-2.49	-1.90

Table 6
Cross-Sectional Regression of the First-Day Return

In this table we report the results of OLS regressions with percentage first-day return as the dependent variable. The table provides the parameter estimates and t-statistics for two different models. The t-stats are corrected for heteroskadisticiy in the sample. The R-squared and the number of observations in each model are provided at the bottom of the table. In Panel A, tie-in dummy equals 1 if there is a regulator (SEC) lawsuit, zero otherwise. In Panel B, tie-in dummy refers to class action lawsuits. NASDAQ return measures the return earned on NASDAQ value weighted index in prior 15 day period from the issue date. Log Proceeds measures the log of proceeds raised in the IPO in millions of dollars. VC dummy equals 1 if the IPO is backed by a venture capitalist, zero otherwise. Tech dummy equals 1 if the firm belongs to high tech industry as defined by Loughran and Ritter (2004), zero otherwise. Insider sale measures the percentage of IPO shares that were sold by the pre-IPO shareholders of the firm. When offer price is set above the filing price, we set positive revision dummy to 1, zero otherwise. Underwriter ranking measures the reputation of underwriter as described in Carter et al. (1998) and modified by Loughran and Ritter (2004). R-squared and the number of observations used to estimate the model are provided at the bottom of the Panels.

Panel A: Based on SEC-Manipulated Stock

	Model 1		**Model 2**	
	Estimate	**t-stat**	**Estimate**	**t-stat**
Intercept	-29.32	-3.42	-29.38	-3.40
Tie-In Dummy	**79.71**	**3.19**	**78.49**	**3.12**
NASDAQ Return (last 15 days)	1.33	4.70	1.39	4.42
Log Proceeds	4.44	1.40	3.17	0.97
VC Dummy	14.91	3.32	13.02	2.82
Tech Dummy	18.42	4.68	17.15	4.27
Insider Sales	0.23	1.66	0.24	1.79
Positive Revision Dummy	42.32	10.90	42.27	10.96
Underwriter Ranking	0.45	0.31	0.45	0.31
Year 99 Dummy			8.54	1.83
Year 00 Dummy			9.64	1.87
R-squared	28.93%		29.02%	
No of observations	737		737	

Panel B: Based on Class-Action-Manipulated Stocks

	Model 1		**Model 2**	
	Estimate	**t-stat**	**Estimate**	**t-stat**
Intercept	-26.74	-3.16	-26.39	-3.08
Tie-In Dummy	**78.77**	**7.86**	**78.07**	**7.53**
NASDAQ Return (last 15 days)	1.44	4.63	1.52	4.57
Log Proceeds	1.49	0.48	-0.03	-0.01
VC Dummy	13.13	2.61	11.13	2.25
Tech Dummy	14.46	3.26	13.07	2.84
Insider Sales	0.22	1.50	0.24	1.63
Positive Revision Dummy	43.45	9.91	43.42	10.03
Underwriter Ranking	1.98	1.29	2.00	1.29
Year 99 Dummy			8.64	1.53
Year 00 Dummy			10.98	2.07
R-squared	36.72%		36.78%	
No of observations	848		848	

Table 7
First-Day Return: Instrumental Variable Regression

This table provides the results of simultaneous equation estimation of the percentage first-day return and litigation probability under class action lawsuit. We provide the results of both first stage and second stage estimation in the table below. The litigation probability is instrumented with the average first week turnover (first week shares traded as a percentage of shares offered) on all IPOs in the past one month of the given IPO. We call this variable 'Past IPO Turnover'. In the first stage we estimate a probit model and obtain the predicted tie-in probability, which is used in the second stage that regresses first-day returns on explanatory variables in an OLS framework. All standard errors in the second stage are corrected for two-stage simultaneity bias as in Maddala (1983).

Model 1

	First-Stage		**Second-Stage**	
	Estimate	**t-stat**	**Estimate**	**t-stat**
Intercept	-8.29	-8.46	93.17	2.22
Tie-In Probability			**24.87**	**3.53**
NASDAQ Return (last 15 days)	0.03	3.71	1.17	2.72
Log Proceeds	0.11	1.11	-1.89	-0.45
VC Dummy	0.45	3.17	6.22	0.93
Tech Dummy	0.98	4.95	-2.70	-0.27
Insider Sale	0.01	1.49	0.16	0.78
Positive Revision Dummy	0.37	2.57	38.00	5.72
Underwriter Ranking	0.24	3.23	-2.55	-0.87
Past IPO Turnover	1.07	5.15		
R-squared		26.06%		27.43%
No of observations		847		847

Model 2

	First Stage		**Second Stage**	
	Estimate	**t-stat**	**Estimate**	**t-stat**
Intercept	-7.47	-7.28	149.12	1.64
Tie-In Dummy			**34.02**	**2.18**
NASDAQ Return (last 15 days)	0.03	3.26	0.97	1.57
Log Proceeds	0.13	1.16	-3.48	-0.69
VC Dummy	0.42	2.84	2.90	0.32
Tech Dummy	0.98	4.85	-12.01	-0.69
Insider Sale	0.01	1.67	0.05	0.21
Positive Revision Dummy	0.39	2.68	34.10	3.59
Underwriter Ranking	0.22	2.86	-3.96	-0.9
Year 99 Dummy	0.72	2.72	-19.31	-1.01
Year 00 Dummy	0.27	0.99	-6.78	-0.53
Past IPO Turnover	0.69	2.78		
R-square		27.90%		27.29%
No of observations		847		847

Table 8
Sub-Sample of Initial Winners and Subsequent Losers

This Table analyzes the difference in first-day return of manipulated and non-manipulated stocks on sub-samples of IPOs that are classified as initial winners and subsequent losers based on their stock-return in first five months after IPO (excluding the first-day return) and thereafter. We analyze five models (i.e., five sub-samples) based on the following sample selection criteria:

Model 1: Includes IPOs with positive returns in first five months, negative during month 6 to year 3.
Model 2: Above the median IPO return in first five months and below median during month 6 to year 3.
Model 3: Below median return during month 6 to year 3.
Model 4: Top quartile return in first five months.
Model 5:Bottom quartile return in first three years of the IPO.

For each sub-sample, in Panel A we provide the number of firms that are classified as manipulated (based on class-action lawsuits) and non-manipulated. Panel A also provides the mean first-day return across the two groups and their difference. All differences are significant at 1% level. In Panel B, we run two stage instrumental variable regression with recent IPO's turnover as an instrument for class-action lawsuits. These regressions are estimated with only those IPOs that enter the a given sub-sample as per the criteria listed above. All standard errors are corrected for tow-stage estimation bias as in Maddala (1983).

Panel A: Mean First-Day Return for the Two Groups for different sub-samples

	Model 1		**Model 2**		**Model 3**		**Model 4**		**Model 5**	
	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean
Non-Manipulated	202	38.91	145	41.66	302	47.33	153	42.80	151	54.64
Manipulated	78	127.68	77	131.76	120	150.30	62	113.30	61	179.89
Difference		**88.77**		**90.10**		**102.97**		**70.50**		**125.25**

Panel B: Instrumental Variable Regression for different sub-samples

	Model 1		**Model 2**		**Model 3**		**Model 4**		**Model 5**	
	Estimate	**t-stat**	**Estimate**	**t-stat**	**Estimate**	**t-stat**	**Estimate**	**t-stat**	**Estimate**	**t-stat**
Intercept	128.92	1.48	105.43	1.03	76.05	1.11	74.73	0.98	132.63	1.38
Tie-In Probability	**45.06**	**2.35**	**39.84**	**1.81**	**25.29**	**1.87**	**31.38**	**1.93**	**24.46**	**1.87**
NASDAQ Return (last 15 days)	0.04	0.04	0.12	0.10	1.19	1.54	-0.16	-0.17	2.39	2.09
Log Proceeds	-0.27	-0.03	3.32	0.29	-0.32	-0.05	2.83	0.29	-8.45	-0.78
VC Dummy	-1.17	-0.09	8.66	0.58	14.77	1.38	12.68	0.95	15.33	0.95
Tech Dummy	-19.91	-0.74	-13.02	-0.44	-3.66	-0.20	-14.57	-0.52	-17.82	-0.67
Insider Sale	0.06	0.16	0.29	0.56	0.73	1.95	0.32	0.77	1.07	1.80
Positive Revision Dummy	24.49	1.57	29.12	1.73	43.08	3.82	40.75	3.08	40.78	2.09
Underwriter Ranking	-3.04	-0.40	-4.97	-0.50	-3.40	-0.61	-2.96	-0.43	-4.12	-0.55
R-squared		22.84%		18.56%		22.28%		23.29%		20.30%
No of observations		280		222		422		215		212

Table 9
First-Day Trading Volume Regression

This table provides the regression results for the first-day trading volume. In Panel A, we regress the first-day turnover of an IPO (defined as the shares traded as a percentage of shares offered) on tie-in dummy (based on SEC lawsuit), return on NASDAQ in the past 15 days from the IPO date, log proceeds and the positive revision dummy. For IPOs involved in the class-action lawsuits, we model the trading volume on the first-day and the probability of class action lawsuits (tie-in) as endogenous variables in a two-stage regression framework. We provide the results of second stage estimation in Panel B of the table. The tie-in probability is instrumented with the average first week turnover (first week shares traded as a percentage of shares offered) on all IPOs in the past one month of the given IPO. In the first stage we estimate a probit model and obtain the predicted tie-in probability, which is used in the second stage regression. All standard errors in the second stage are corrected for two-stage simultaneity bias as in Maddala (1983).

Panel A: Based on SEC-Manipulated Stocks

	Estimate	t-stat
Intercept	22.30	4.06
Tie-In	**11.42**	**1.91**
NASDAQ Return (last 15 days)	0.47	2.91
Log Proceeds	5.85	4.13
Positive Revision Dummy	23.66	4.06
R-squared		19.24%
No of observations		755

Panel B: Based on Class-Action-Manipulated Stocks (Second Stage Results)

	Estimate	t-stat
Intercept	90.47	9.45
Tie-In Probability	**15.31**	**7.40**
NASDAQ Return (last 15 days)	-0.02	-0.12
Log Proceeds	-2.87	-1.37
Positive Revision Dummy	10.39	3.13
R-squared		31.40%
No of observations		847

Table 10
Post-IPO Buy-and-Hold Returns & Fama-French Regressions

Panel A provides regression results for buy-and-hold returns on IPOs over various holding periods. In Model 1, the dependent variable is the log(1+ IPO's buy-and-hold return for the first five months excluding the first-day return) minus log (1+market return during the same holding period). In Model 2 the dependent variable is log (1+Return during the sixth month after an IPO) minus log (1+market return during the same month). Finally in Model 3 the dependent variable is log(1+ IPO's buy-and-hold return starting from the end of the sixth month after the IPO till its 3 year anniversary) minus log(1+market return for the same holding period). We regress these market adjusted buy-and-hold returns on the log of the size of IPO firm (shares outstanding x offer price of the IPO), its book to market ratio, and the predicted tie-in probability that comes from the first stage regression of tie-in dummy (based on the class action lawsuit). Market return is computed by compounding the returns on value weighted NYSE/AMEX/NASDAQ return. B/M ratio is based on the book value as of the most recent fiscal year-end after the offer date. Market value for the computation of B/M ratio is same as the size variable described above. Due to a few missing observations on book-values, Panel A regressions are estimated with 760 observations. In Panel B, we estimate Fama-French three factor regression models with daily return on the IPO firm. We estimate these models separately for the manipulated (based on class-action lawsuit) and non-manipulated IPOs as well as a hedge portfolio that buys manipulated stocks and sells short non-manipulated ones. We divide our holding periods into 'first five months' and 'months six through year 3' after an IPO to estimate the short-term and long-term risk-adjusted returns. Daily IPO returns are regressed on the corresponding day's return on the three Fama-French factors. 'α' captures the intercept from the regression; *rm* stands for the coefficient on excess market return factor; *smb* for small minus big stock return factor and *hml* for value minus glamour stock return factor. The corresponding t-statistics are provided in the bracket.

Panel A: Buy-and-Hold Return Regressions

	Model 1 First Five Months		**Model 2 Sixth Month**		**Model 3 Month 6- Year 3**	
	Estimate	**t-stat**	**Estimate**	**t-stat**	**Estimate**	**t-stat**
Intercept	0.0961	0.45	-0.4039	-4.37	-2.2696	-4.53
Log Size	-0.0453	-1.39	0.0328	2.31	0.0211	0.27
B/M Ratio	0.2214	3.47	0.0422	1.48	0.0781	0.49
Tie-in Probability	**0.0659**	**2.26**	**-0.0634**	**-4.76**	**-0.4557**	**-6.05**
R-squared		1.18%		4.31%		9.12%

Panel B: Fama-French Regressions

	α	*rm*	*smb*	*hml*
First Five Months				
Non-manipulated (I)	0.0493	1.3012	1.4784	-0.0638
	(0.63)	(11.31)	(12.72)	(-0.38)
Manipulated (II)	0.4015	1.3815	1.3212	-1.1426
	(2.80)	(6.55)	(6.20)	(-3.68)
Difference (II-I)	0.3523	0.0803	-0.1572	-1.0788
	(3.13)	(0.49)	(-0.94)	(-4.42)
Month six through year 3				
Non-manipulated (I)	0.0257	1.0999	1.0868	0.0520
	(1.12)	(45.74)	(30.49)	(1.24)
Manipulated (II)	-0.0326	1.3802	1.4670	-0.8155
	(-0.74)	(29.94)	(21.47)	(-10.13)
Difference (II-I)	-0.0582	0.2803	0.3803	-0.8675
	(-1.71)	(7.84)	(7.17)	(-13.88)