How Does Going Public Affect Employee Behavior? Evidence from Brokerage IPOs

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Abstract: We provide new evidence on how initial public offerings (IPOs) influence employee behavior using the brokerage industry as an empirical setting. We examine whether IPOs influence the objectivity of equity research analysts. Contrary to conventional wisdom suggesting that IPOs lead to myopic employee behavior due to increased demands from new stakeholders, we provide robust evidence that analysts actually produce *less* optimistically biased earnings forecasts relative to their peers around the time of their employer's IPO. Our results are consistent with analysts' responding to increased scrutiny around the IPO, which in turn incentivizes more objective behavior. These findings are consistent with beneficial impacts of monitoring of publicly listed firms and enhances our understanding of how IPOs are associated with changes in behavior within a firm.

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1. Introduction

The decision to go public is one of the most significant decisions a firm can make and is an important part of a firm's life cycle. Initial public offerings (IPOs) provide firms with a substantial injection of equity capital, facilitate the expansion of operations, and create a public market for owners to liquidate their interests (Ritter and Welch, 2002). However, the process of becoming a public company is a costly endeavor. IPOs can attract intense scrutiny from various parties including investors, information intermediaries such as financial analysts and the media, underwriters, and lawyers. While prior research provides some evidence on how IPOs are associated with changes in *firm*-level behavior (Teoh et al., 1998; Heaton, 2002; Kedia et al., 2014; Ertimur et al., 2018), there is limited empirical evidence on how going public impacts the behavior of individual employees, and in particular non-executive employees.[1] In this study, we contribute to this literature by examining how *employee*-level behavior changes around IPOs in the financial sector (henceforth, broker IPOs). Our specific focus is on how sell-side research analysts' bias changes around their brokerage's IPO.

We use the financial sector as a laboratory for examining employee behavior surrounding IPO events because it offers employee-specific data that is typically unobservable to researchers, offering several important advantages. First, because specific analysts are identifiable from their research reports, we are able to observe detailed individual employee outputs that we can track across time and employers. Second, the employee outputs are observable regardless of whether a brokerage is private or public, which allows us to track the behavior of employees both before and after the IPO. Third, the analyst setting provides us with an ideal control sample because we can

[1] We follow Core and Guay (2001) and define non-executive employees as all employees other than the five most highly compensated executives.

observe a large set of employees working at other brokers, performing identical tasks, in both the pre-IPO and post-IPO periods.

Our primary conjecture is that analyst forecasts dynamically change around the time of the IPO to align with the objective of their brokerage. However, the direction of this change is unclear ex ante. On one hand, going public significantly increases the level of monitoring and scrutiny from key stakeholders of the brokerage. IPO firms face enhanced scrutiny from external monitors such as analysts, lawyers, underwriters, auditors, boards, and the business press during the process. Ball and Shivakumar (2008) provide evidence that this increased monitoring is associated with improved corporate behavior (i.e., improved financial reporting quality). Outside of the IPO literature, a large body of research across various disciplines finds that individuals are more likely to behave appropriately when they are more closely monitored (see, e.g., Mayo, 1949; Bouchet et al., 1996; Kedia and Rajgopal, 2011; Hope et al., 2013). In the financial sector, equity analysts face well-known incentives that can lead them to compromise their objectivity by biasing forecasts (e.g., Lin and McNichols, 1999; Cowen et al., 2006). The increased level of scrutiny around an IPO may potentially discipline such behavior of that brokerage's own analysts. Thus, this 'scrutiny hypothesis' suggests that analysts will reduce optimistic bias and become more objective in their reports (i.e., less optimistically biased) during the IPO process.

On the other hand, IPOs create significant economic incentives for firms to report profitability and growth to shareholders. Consistent with the existence of these incentives, prior research finds that IPO firms are more likely to take actions to boost short-term earnings after going public (Teoh et al., 1998). These pressures can naturally extend to non-executive employees through a variety of mechanisms, including bonuses tied to firm performance or equity-based compensation. IPOs may also be associated with changes in corporate culture that influence short-

term gains at the expense of long-term value and potentially encourage unethical behavior (Guiso et al., 2015). This argument is consistent with Dimmock et al. (2018), who find that an individual's propensity to commit fraud increases after a merger exposes co-workers to such tendencies. Indeed, Falato and Scharfstein (2016) provide evidence that post-IPO pressure to maximize short-term stock price performance increases banks' risk-taking. Kedia et al. (2014) report that, following its IPO, employees at Moody's were allegedly asked to focus solely on revenues and market share in an effort to boost short-term profits. In contrast to the scrutiny hypothesis, this 'economic incentives hypothesis' suggests analysts will increase optimistic bias and become less objective in their reports (i.e., more biased) around the IPO of their brokerage.

It is also possible that analysts' optimistic bias may be unaffected by IPO-related pressures. Analysts largely manage their own "franchise," and it may be the case that individual reputation concerns provide a disciplining mechanism that encourages objectivity regardless of external pressures (Fang and Yasuda, 2009). The fact that analysts sign their reports is a particularly salient feature of the equity research setting, as this enables investors to hold analysts accountable when they produce biased outputs. Thus, the credible null is that analysts' forecasts do not change around their brokerage's IPO because reputational concerns encourage objective research.

We use data from ThomsonOne to identify IPOs in the financial services industry to examine the impact of broker IPOs on analyst objectivity. The intersection of the ThomsonOne IPO data with I/B/E/S analyst data provides a sample of 23 broker IPOs that meet our sample criteria spanning across three decades.

We test our prediction by examining changes in individual analysts' forecast bias around the IPO event. We use a generalized difference-in-difference (hereafter DiD) methodology, where we examine changes in one-year ahead earnings forecast bias in the two-year period centered on

the IPO event (i.e., years *t-1* and *t+1*).[2] We focus on analysts employed at IPO brokers during this time (hereafter "IPO analysts") and compare the change in the bias of their forecasts to the bias of all other analysts ("control analysts"). We use analyst and covered firm-year fixed effects to reduce the possibility that our tests are confounded by alternative explanations related to the covered firms (e.g., need for financing), analyst-specific characteristics (e.g., talent or inherent optimism), or time-varying economic conditions. We focus primarily on earnings forecasts because they are revised frequently, have a clear benchmark for evaluating performance (reported earnings), and provide a continuous measure of objectivity (i.e., forecast bias) with substantial variation. In supplemental analyses, we also examine stock recommendation revisions. We include the full panel of forecasts archived by I/B/E/S from 1982 (the first year when analyst data is available from I/B/E/S) to 2012, which is five years after the last broker IPO found in our search. Our primary sample includes more than 795,000 firm-year observations with analyst coverage.

The results are consistent with the scrutiny hypothesis and indicate that IPO analysts reduce optimism bias in the two-year period centered on the IPO event. In terms of economic magnitude, we estimate a decrease in forecast bias for IPO analysts that is roughly 6.8 basis points of price compared to that of control analysts. We also examine the timing of this IPO effect, by separately examining indicators for additional pre and post event years. Across each specification, we document robust evidence that the documented decrease in forecast bias is observed only in the two years centered on the IPO event, and there are no significant changes in bias in the years before or after this period. Our results thus indicate a marked decrease in analyst optimistic bias that appears isolated to the IPO event window.

[2] We label this two-year window as t-1 through t+1, but we exclude data from the month immediately before through the month immediately after the IPO, which is effectively time 0. See discussion in section 3.

We then conduct four sets of additional analyses to further our understanding of the IPO scrutiny effect. First, we re-examine our results after incorporating alternative fixed effect structures that account for different types of unobserved heterogeneity. In these tests, we examine models with broker fixed effects to account for time-invariant brokerage policies (e.g., culture), analyst-broker fixed effects to account for individual analysts' employment spells (e.g., an analyst's behavior at one firm of interest), and analyst-covered firm fixed effects to control for the analyst's relationship with the covered firm. Across all specifications, the results consistently indicate that IPOs are associated with reductions in bias. In terms of economic magnitude, we continue to find that IPO events reduce optimistic bias by roughly 5 to 7 percentage points, depending on the specification. We also find that results are robust to the use of a within-firm-year ranked measure of analyst forecast bias, as well as various methods of standard error clustering. These results provide further support for the scrutiny hypothesis, suggesting that our results are not driven by different forms of unobserved heterogeneity.

Second, we partition our sample to further test the scrutiny hypothesis by assessing whether our findings hold for analysts forecasting in different regulatory regimes when their brokerage goes public. Specifically, we consider the role of industry regulation in reducing optimistic bias. The Global Settlement was enacted with the objective of reducing the optimistic bias perceived to be prevalent in analyst research. It is thus not clear whether analyst objectivity in the pre-Global Settlement period will exhibit the same effect as our main results that are consistent with a disciplining effect of IPO-related scrutiny. We therefore partition our sample into two periods, pre-Global Settlement (1982-2002) and post-Global Settlement (2003-2012). We find consistent results of reductions in forecast bias around IPOs in both periods, suggesting that a scrutiny effect present even prior to the enactment of analyst regulations. Specifically, the economic magnitudes

suggest that IPOs reduce optimism by 6.5 percentage points prior to Global Settlement and 9.6 percentage points after the Settlement.

Third, we examine the extent to which changes in employee composition at the brokerage could be affecting our evidence for the scrutiny hypothesis. Our objective is to examine how IPO incentives influence the objectivity of employees working at the brokerage at the *employee*-level. For example, in response to increased scrutiny around the IPO, it could be the case that IPO brokerages change the composition of their equity research departments – by hiring less optimistic analysts, firing more optimistic analysts, or both. Such hiring policies would be consistent with the effects we document thus far. To investigate this possibility, we re-examine our primary analysis by partitioning our IPO event variable based on whether the forecast is made by an analyst that was employed by the IPO brokerage during the entire two-year IPO window versus an analyst that either joined or left the brokerage at some point during this window. We find that our primary results are concentrated among legacy analysts (who neither joined nor left the brokerage) during the IPO.

Finally, we examine the robustness of our result to another important analyst output, stock recommendation revisions. We examine regressions of the degree to which an analyst revises his or her recommendation on the IPO event dummies. Consistent with the primary results based on earnings forecasts, we find that IPO analyst recommendation revisions also become significantly less optimistic during the IPO period. Overall, the evidence from each of our supplemental analyses provides strong support for the scrutiny hypothesis.

Our results offer several contributions to the literature. First, we provide the first granular analysis of non-executive employee behavior that contributes to the literature examining the relation between IPOs and earnings management (e.g., Teoh et al., 1998; Ball and Shivakumar,

2008; Cecchini et al., 2012). Although Teoh et al. (1998) provide evidence suggesting market pressures lead *firms* (or *executives*) to manage earnings following an IPO, more recent evidence in Ball and Shivakumar (2008) and Cecchini et al. (2012) suggests the increased scrutiny of going public leads to reduced earnings management. Consistent with these more recent studies on firm-level behavior, we find that going public leads to reduced optimistic bias in equity analyst forecasts, which is consistent with the existence of scrutiny effects throughout the firm.

Second, our study also relates to a growing line of research examining changes in organizational structure of financial institutions. For example, recent studies show that broker mergers and closures influence analyst behavior (e.g., Hong and Kacperzyk, 2010; Balakrishnan et al., 2014; Merkley et al., 2017). Our findings shed light on how a change in a financial institution's public status can also influence individual employee behavior.

Finally, our finding that equity analysts become *less* optimistic around IPOs contrasts with Kedia et al.'s (2014) finding that credit ratings issued by Moody's became *more* favorable after Moody's IPO. Our analysis and results differ from Kedia et al. (2014) in that we focus specifically on individual employee behavior surrounding the IPO event as opposed to the firm-level behavior captured by Moody's ratings. Further, our unique ability to identify activities of distinct employees reinforces the credibility of our inferences relative to those based on the IPO of a single firm.[3] Our evidence on employee-level behavior enhances understanding of the impacts of IPOs on employees, which complements prior mixed evidence on firm and executive behavior around IPOs.

[3] Similarly, our sample of 23 brokerage IPOs also allows us to employ a broader sample with a more robust generalized difference-in-difference methodology and extensive fixed effects structure.

2. Prior Research and Hypothesis Development

2.1 Prior research

Public equity offerings represent a significant strategic event in the life cycle of a firm. The process of going public is arduous, as it can take a year or more of preparation and result in immense changes within the organization that can impact employees at all levels. Evidence from the strategic human resource management literature suggests companies that ignore the effects of an IPO on its employees are more likely to fail (Welbourne and Andrews, 1996). IPOs can also lead to significant cultural changes as the organization adjusts to new layers of monitoring from regulators and capital markets, and new pressures to report profits and growth to outside shareholders. For example, after the IPO of Facebook, Mark Zuckerberg often encouraged employees to ignore the stock price and to "stay focused and keep shipping."[4] In addition, employees at Moody's (a credit rating agency) testified before the Financial Crisis Inquiry Commission that the corporate culture changed after the firm went public such that employees were asked to "look the other way," essentially "trading the firm's reputation for short term profits" (Kedia et al., 2014, The Financial Crisis Inquiry Commission (2011) p. 207). Google reportedly appointed a chief culture officer to help manage such frictions and address employee morale problems following its IPO.[5]

Public offerings also result in new layers of oversight and scrutiny from regulators. All U.S. companies with publicly traded securities are regulated by the Securities Exchange Act of 1934, which requires public companies to comply with an extensive set of rules and regulations that have become costlier over time (e.g., Sarbanes Oxley, Regulation Fair Disclosure, 8-K reporting requirements, etc.). Public companies also face significant pressure and monitoring from

[4] https://www.thestreet.com/story/12094909/1/what-to-do-when-your-companys-going-ipo.html
[5] http://www.workforce.com/2011/12/07/wealth-after-an-ipo-can-cause-employees-to-go/.

other external parties including investors, equity analysts, rating agencies, business press reporters, auditors, and lawyers. These parties reinforce regulatory initiatives for increased transparency (e.g., Becker et al., 1998; Sweeney, 1994; Chung et al., 2002; Dyck et al., 2008; Yu 2008) and expose public companies to greater litigation risk (Lowry and Shu, 2002; Badertscher et al., 2014).

Prior research documents mixed effects of IPOs on firm behavior. Some studies suggest that increased scrutiny around IPOs motivates more conservative behavior. For example, in the context of financial reporting, Ball and Shivakumar (2008) provide evidence that IPO firms report more conservatively because they are held to a higher reporting standard. The authors attribute this finding to enhanced scrutiny. Cecchini et al. (2012) provide similar evidence that IPO firms are more conservative with estimates reflected in the allowance for uncollectible accounts.

However, other studies suggest public companies face new pressures from external parties, and investors in particular, to meet short-term performance targets, and this motivates opportunistic behavior. These new pressures could have detrimental effects by encouraging firms to engage in negative, short-sighted behavior. For example, Stout (2012) makes the following sardonic argument: "Shareholder value thinking causes corporate managers to focus myopically on short-term earnings reports at the expense of long-term performance; discourages investment and innovation; harms employees, customers, and communities; and causes companies to indulge in reckless, sociopathic, and socially irresponsible behaviors" (p. vi). In the context of financial reporting, Friedlan (1994) and Teoh et al. (1998) provide evidence that IPO firms adopt discretionary accounting choices that inflate earnings around the offering. More recently, Kedia et al. (2014) document that Moody's credit ratings become more favorable after the agency went public. Kedia et al. (2014) further report that employees at Moody's were allegedly asked to focus solely on revenues and market share in an effort to boost short-term profits.

2.2 Hypothesis Development

We are specifically interested in whether the decision to go public influences the actions of non-executive employees. Our study extends the IPO literature by examining how going public impacts the behavior of individual, non-executive employees working in the financial industry. A relatively small body of research indirectly examines employee behavior, but inevitably focuses on the decisions of the senior management team. For example, Heaton (2002) argues that managers of IPO firms are overly optimistic about the firm's prospects and, as a result, overinvest in projects. This behavior could, in part, explain why IPO firms tend to experience a subsequent decline in operating performance and negative returns (Jain and Kini, 1994).

The focus on senior management in prior research on IPOs is natural given that they are ultimately responsible for the performance of the firm. However, it is surprising that no research focuses on the effects of going public on the behavior of non-executive employees. Welbourne and Andrews (1996) motivate a seminal examination of IPO firms in the organizational behavior literature by noting that research on human resource management had previously focused only on senior management teams of large, established firms. They argue that "actions taken by corporate offices … might not translate into the behaviors of managers at the division, business unit, or plant level" [p. 893], suggesting that IPO-related forces can create rich variation in *employee*-level outcomes. Kedia et al. (2014) suggest that non-executive employees may change their behavior around IPOs, but are unable to empirically test this assertion with aggregated data for Moody's.

Given the mixed results in prior literature and the dearth of research on non-executive employees, it is unclear ex ante how broker IPOs will influence the objectivity of equity research analysts. On one hand, as discussed previously, becoming a publicly traded entity introduces new layers of monitoring and scrutiny from a host of external parties. A long line of research, beginning

with the early Hawthorne Works field experiments, finds that individuals are more likely to behave appropriately when they are being more closely monitored (e.g., Cook, 1962; Bouchet et al., 1996; Mangione-Smith et al., 2002; Hyde, 2007; Gerber et al., 2008; Schwartz et al., 2013). In the context of financial reports, prior studies find that managers exhibit less bias when they are more closely monitored by regulators (Kedia and Rajgopal, 2011), the financial press (Miller, 2006; Dyck et al., 2008), institutional investors (Chung et al., 2002), debt holders (Sweeney, 1994), government agencies (Buckwalter et al., 2014), and equity analysts (Yu, 2008). In the IPO setting, monitoring by these parties is greatly increased (Ball and Shivakumar, 2008). It is possible that this "all eyes on the firm" environment mollifies any legacy incentives analysts at IPO brokerages have to distort their outputs, thus reducing optimistic bias. We label this the 'scrutiny hypothesis.'

On the other hand, IPOs introduce significant pressures for firms to deliver profitability and growth commensurate with the hype surrounding the IPO. Such pressures may naturally extend down to non-executive employees within the organization. Because research departments have traditionally been funded indirectly through institutional business (e.g., investment banking and brokerage fees), analysts may optimistically bias their forecasts to support these business lines. For example, prior studies show that more optimistic research helps support investment banking and trading business (Lin and McNichols, 1998; Michaely and Womack, 1999; Jackson, 2005; Cowen et al., 2006). Thus, the incentive to issue biased research may be even more salient to analysts just around the IPO of their brokerage when pressures to achieve short-term performance targets become even more important. We label this the 'economic incentives hypothesis.' The incentives and pressures to focus on building these business lines could either be explicitly communicated through compensation structures, or implicitly through a corporate culture that focuses on short-run results (e.g., Guiso et al., 2015; Pacelli, 2019).

Given these competing hypotheses, the net effect of broker IPOs on analysts' bias is ultimately an empirical question. In addition, we note that it is possible that IPOs may have no influence on analyst objectivity. Prior studies find that personal reputation concerns are highly effective in disciplining sell-side analysts (Jackson, 2005; Fang and Yasuda, 2009). Because analysts effectively sign their reports, changes in objectivity around IPOs could potentially threaten the analyst's credibility with clients and harm future career outcomes. Thus, analysts' desire to maintain their own reputations may constrain the extent to which their brokerage's IPO can influence their actions. Overall, the credible null is that analysts' objectivity remains unchanged around their brokerage's IPO.

3. Data and Sample

We use several datasets to construct our sample. We begin by identifying IPOs in the financial services industry by searching the ThomsonOne database for all U.S. IPOs in SIC industry groups 6211 (Security Brokers, Dealers, and Flotation) and 6282 (Investment Advice) during the 1985 to 2014 period.[6] The focus on these particular industry groups is motivated by prior studies that use industry group 6211 when examining broker mergers and closures (Hong and Kacperzyk [2010]), whereas industry group 6282 captures dedicated equity research firms. We manually match the issuer name in ThomsonOne to I/B/E/S broker names using the I/B/E/S broker translation file.

Table 1 provides a list of our final sample of 23 broker IPOs along with the IPO date. For each IPO, we verify the dates and details of each IPO by manually collecting S-1 filings from the

[6] We begin our sample of IPOs in 1985 because the I/B/E/S dataset begins in 1982, and we require pre-IPO forecast data for our tests. The I/B/E/S earnings forecast database is less populated in the 1980s. Our results are robust to partitioning on later years (i.e., years after 1990). Additionally, although we searched for IPOs through 2014, the last IPO meeting all of our sample inclusion criteria occurred in 2007 (see Table 1).

SEC EDGAR website and relevant news articles from Factiva. The IPOs in our sample appear well distributed over time, with eight occurring in the 1980s, nine in the 1990s, and six in the 2000s.

We follow prior literature and obtain from the I/B/E/S detail file the last one-year ahead annual earnings forecast issued by each analyst during the 11 month period ending 30 days before each covered firm's fiscal year end date (Clement, 1999).[7] We include forecasts from 1982 (the start of the I/B/E/S detail file) through 2012 (five years after our last IPO event). We retain all forecasts with necessary data to calculate forecast bias and control variables from I/B/E/S as described below. We exclude forecasts from IPO brokers issued from the month before through the month after the IPO.[8] We also remove observations where the covered firm has a missing market value of equity, a negative book-to-market ratio, or the stock price of the covered firm is less than $3 (Gu and Wu, 2003). Finally, we require forecasts from at least two analysts for a given firm-year. Our final sample contains 797,125 analyst-firm-year observations. Within this sample, the treatment sample of forecasts issued during the year prior to and following the IPO contains 11,362 forecasts issued by 650 unique analysts.

4. Does Going Public Influence Existing Analysts' Objectivity?

4.1 Empirical Models

We examine whether analysts' forecast bias changes in the period immediately surrounding their broker's IPO using a generalized DiD framework (see, e.g., Christensen et al., 2016;

[7] Prior research documents that the last forecast released during a fiscal year is the most accurate, on average (Richardson et al., 2004).

[8] We remove these observations to increase the power of our analyses examining pre- versus post-IPO effects. We note that there are 1,362 forecasts during this blackout period, representing only 0.17 percent of our sample, and in untabulated analyses, we find consistent results if we instead retain them.

Bourveau et al., 2018). This approach uses the full panel of analyst forecasts available from I/B/E/S during the 1982 to 2012 period. Our model of bias is as follows:

$$Bias_{ibjt} = \alpha_1 IPO[-3,-2]_{bt} + \alpha_2 IPO[-1,+1]_{bt} + \alpha_3 BrokerSize_{bt} + \alpha_4 Horizon_{ijt} + \alpha_5 Coverage_{it} + \alpha_6 Experience_{ijt} + \beta Fixed\ Effects_{ij\times t} + \varepsilon_{ibjt} . \quad (1)$$

In equation (1), *i* indexes analysts, *b* indexes broker (employing analyst *i*), *j* indexes the covered firm, and *t* indexes year. The dependent variable, *Bias*, is an analyst's last forecast for the year minus the covered firm's actual earnings, scaled by stock price measured two days prior to the forecast issuance date, and multiplied by 100. Higher values of *Bias* indicate more optimistic bias. The variable of interest in equation (1) is *IPO[-1,+1]*, which is an indicator variable that takes the value of one for forecasts issued by analysts at IPO brokerages during the two-year period centered on the IPO date, and zero otherwise.[9] A negative (positive) and significant coefficient on *IPO[-1,+1]* provides evidence consistent with the scrutiny (economic incentives) hypothesis. To assess pre-trends and control for any potential changes in analyst behavior prior to this period, we also include an indicator variable *IPO[-3,-2]* that takes the value of one for forecasts issued by analysts at IPO brokerages during the two-year period ending one year before the IPO date, and zero otherwise.

Equation (1) includes an extensive fixed effect structure to help isolate the effect of going public and reduce the plausibility of alternative explanations associated with characteristics of the analyst, covered firm, or time. Our primary specification controls for time-invariant characteristics of the analyst through analyst fixed effects, as well as time-varying characteristics of the covered firm through covered firm-year fixed effects. The covered firm-year fixed effects control for

[9] The *IPO* variables can be viewed as interaction terms for IPO analysts and time period indicators. The main effects for these variables are subsumed by our fixed effect structure, as discussed in the following paragraph.

unobservable characteristics of the firm that vary over time that might influence bias, such as the need for financing. We consider alternative fixed effect structures in Section 5.1 below.

Equation (1) also includes a set of control variables that relate to various characteristics of the analyst and the broker to capture any remaining residual variance related to temporal changes in such factors. In all of our models, we control for broker size (*BrokerSize*), defined as the natural log of the total number of analysts employed by the broker of interest. We also control for analyst forecast horizon (*Horizon*), defined as the natural log of the number of days between the forecast issuance date and the fiscal period end date. We also control for two important analyst characteristics. *Coverage* is the natural log of the total number of firms covered by the analyst. *Experience* is the natural log of the number of years of experience the analyst has been covering the firm. We winsorize all continuous variables at the top and bottom 1 percent to reduce the influence of outliers, and all variables are formally defined in the appendix. Finally, we cluster standard errors by covered firm-year, although we discuss results using alternative clustering methods in Section 5.1.

We also expand equation (1) to isolate and examine the year-by-year IPO effects in both the pre- and post-IPO periods. To do this, we create separate indicator variables for each year from *t-3* to *t+2* (*IPO[-3]*, *IPO[-2]*, *IPO[-1]*, *IPO[+1]*, *IPO[+2]*). In addition, we include another indicator variable, *IPO[3+]*, which takes the value of one for all forecasts issued by analysts at IPO brokerages three or more years after the IPO, and zero otherwise. We specify the model as follows:

$$\begin{aligned} Bias_{ibjt} = {} & \alpha_1 IPO[-3]_{bt} + \alpha_2 IPO[-2]_{bt} + \alpha_3 IPO[-1]_{bt} + \alpha_4 IPO[+1]_{bt} \\ & + \alpha_5 IPO[+2]_{bt} + \alpha_6 IPO[3+]_{bt} + \alpha_7 BrokerSize_{bt} + \alpha_8 Horizon_{ijt} \\ & + \alpha_9 Coverage_{it} + \alpha_{10} Experience_{ijt} + \beta Fixed\ Effects_{ij\times t} + \varepsilon_{ibjt} . \end{aligned} \quad (2)$$

4.2 Descriptive Statistics

We provide descriptive statistics for the variables used in our primary analysis (i.e., equation (1)) in Table 2. We report unlogged values of the variables for ease of interpretation. On average, we find that annual earnings forecasts are optimistically biased at approximately 0.854 percent of stock price. Analysts in our sample issue forecasts approximately 114 days before the fiscal year end. Further, analysts work for brokers that employ, on average, 61 analysts, cover approximately 18 firms, and have about 2 years of experience covering the firm of interest.

4.3 Effects of Going Public on Analysts Forecast Optimism

In Table 3, we provide the estimation results from equation (1), which is the model we use to test our primary hypothesis. In Column (1), we only include the IPO event indictor variable *IPO[-1,+1]* along with analyst and firm-year fixed effects (i.e., no control variables or pre-trend). In column (2), we add the control variables. Finally, in column (3) we present the full equation (1) that includes the pre-event indicator variable *IPO[-3,-2]*, which is the specification we use in subsequent analyses unless otherwise noted.

In all three columns, we find that the coefficient on *IPO[-1,1]* is negative and significant at the 5 percent or 1 percent level. These results suggest that analyst forecasts become less optimistically biased during the IPO event window and are thus consistent with the scrutiny hypothesis. The magnitudes on the coefficients appear economically meaningful. The coefficient on *IPO[-1,1]* is -0.068 in column (3), which indicates that brokerage IPOs are associated with a reduction in analyst optimism of 6.8 basis points of price. We also note that the coefficient on the pre-tend indicator variable, *IPO[-3,-2]*, is not significantly different from zero in all cases. This

provides evidence that forecasts from analysts at IPO and non-IPO brokers are not statistically different in the years immediately prior to the IPO event.

In Table 4, we provide the estimation results for equation (2) which examines both pre- and post-IPO trends on a year-by-year basis. We expand the number of yearly indicator variables included in the model incrementally across the three columns until we present the full model in column (3). Consistent with the main results in Table 3, we find negative and significant (at the 5 percent level) coefficients on the IPO event indicator variables *IPO[-1]* and *IPO[+1]*. None of the coefficients on the pre- or post-event indicator variables for the years before or after the IPO event window are statistically different from zero. In Figure 1, we plot the coefficients on each of the indicator variables along with their 90 percent confidence intervals. We observe a marked changed in analyst forecast bias in the two years around the IPO event, but not before or after. This provides additional support for the scrutiny hypothesis and further supports the inference that the observed changes in analyst bias are concentrated during the IPO event window only and not at other time periods. The evidence in Table 4 also indicates that the reduction in forecast bias documented in Table 3 occurs both immediately prior and immediately following the IPO event.

5. Additional Analysis

The results presented thus far provide strong support for the scrutiny hypothesis. We next conduct a series of additional analyses to further assess the validity of our inferences. These tests include alternative fixed effects and methods of clustering standard errors, a sample partition test based on time periods, tests based on changes in employee composition, and tests using stock recommendation revisions as an alternative analyst research output.

5.1 Alternative fixed effects and clustering

We first examine the robustness of our result to various alternative fixed effects structures. We consider three additional models. In column (1) of Table 5, Panel A, we add broker fixed effects to equation (1). This controls for unobserved time-invariant heterogeneity at the broker level. In column (2), we add analyst-broker interactive fixed effects to equation (1), which holds any particular analyst-brokerage pairing constant. For example, this specification separately exploits variation in bias from analyst A's forecasts while employed at Goldman Sachs from those while analyst A is employed at Morgan Stanley. In column (3), we add broker fixed effects as well as analysts-covered firm interactive fixed effects to equation (1). This model holds both broker effects and the effects within a particular analyst-covered firm relationship (e.g., analysts A's forecasts of Microsoft) constant. The analyst-covered firm fixed effect thus controls for the analyst's relationship with the covered firm over time.

We present the results of these alternative models in Table 5, Panel A. We continue to find negative and significant coefficients on the IPO event indicator variable *IPO[-1,+1]* with each of these alternative fixed effects structures. The magnitudes of the coefficient on *IPO[-1,+1]* in columns (1) and (2) are similar to each other and to the magnitude presented in our primary results in Table 3 (approximately 6.8 basis points of price; see Table 3, column (3)). In column (3) of Table 5, when we include both brokerage and analyst-covered firm fixed effects (in addition to firm-year), we still find a negative and significant (at the 10 percent level) coefficient on *IPO[-1,+1]*, but the magnitude decreases to 4.6 basis points of price.

An alternative method of controlling for unobserved heterogeneity within a covered-firm year is to examine the relative bias in analyst forecasts *within* the set of analysts covering a particular firm in a given year. This type of relative bias measure has been used in prior literature

(see, e.g., Cowen et al., 2006). As shown in Panel B of Table 6, we change the dependent variable in equation (1) to $RelBias_{ibjt}$ measured as the rank of analyst *i*'s forecast of covered firm *j* among all analyst forecasts of firm *j*'s in year *t*. We present results after removing the covered firm-year fixed effects from this model (as the dependent variable is ranked within firm-year), but retain the analyst fixed effects. Because we remove the firm-year fixed effects from the model we add several firm-level control variables – the natural log of the market value of equity, book to market ratio, the natural log of analyst following, and return on assets (coefficients on these variables are again suppressed for simplicity). We find the coefficient on *IPO[-1,+1]* remains negative and statistically significant at the 5% level.

In Panel C of Table 5, we provide results for three alternative methods of estimating equation (1), where the fixed effects are presented as in our primary analyses in Table 3, but we now vary the method by which we cluster our standard errors. In the first column, we cluster two-dimensionally by firm and year, in the second column by broker and year, and in the final column, by analyst and year. As shown, these changes to how we cluster standard errors has very little impact on the results.

5.2 Regulatory Regimes

Next, we examine whether our results differ across samples partitions related to differing regulatory regimes. Specifically, the equity analyst landscape changed dramatically in the early 2000s with the passage of the Global Settlement (2002-2003). One of the goals of the settlement was to reduce conflicts of interest that arose between the investment bank and equity research divisions of the broker. Prior to the Global Settlement, equity research was primarily funded through the investment banking business, which created incentives for the equity research analysts

to produce optimistically biased research. The Global Settlement prohibited such within brokerage cross-subsidization to increase the objectivity of equity analysts. This raises the question of whether we will observe the effects of the scrutiny hypothesis in the pre-Global Settlement period when analysts faced greater incentives to issue biased research.

We test this conjecture by partitioning the sample into two periods, 1982-2002 and 2003-2012, using the Global Settlement date as the breakpoint. We then estimate equation (1) separately for these two periods and present the results in Table 6. We find that our results hold in both periods – the coefficient on *IPO[-1,+1]* is negative and significant in both periods. This evidence provides additional support for the scrutiny hypothesis.

5.3 Employee Composition

Our primary analyses are focused on the effects of IPOs on bias at the individual analyst-level. It is also possible that the brokerage takes firm-level actions to influence of bias reflected in the forecasts of their analysts. More specifically, we examine whether our results are observed two different groups of analysts employed at the brokerage during the IPO – those who are employed by the brokerage for the entire period and those that were not. We do this by partitioning our variable of interest, *IPO[-1,+1]* in equation (1) into two variables based on whether the forecast is made by an analyst that was employed by the IPO brokerage during the entire two-year IPO window *t-1* to *t+1* (*IPO[-1,+1] Full Period*) versus an analyst that either joined or left the brokerage at some point during this window (*IPO[-1,+1] Partial Period*). We present the results from this analysis in column (1) of Table 7. We find a significantly negative coefficient on *IPO[-1,+1] Full Period* only, which indicates the effect of going public on analyst bias is concentrated among the subsample of analysts that remain at the IPO firm through the IPO event window. In

addition, the results of a within regression F-test confirm that the negative coefficient on *IPO[-1,+1] Full Period* is statistically more negative than the coefficient on *IPO[-1,+1] Partial Period*. We then partition the pre-effect indicator variable *IPO[-3,-2]* in a similar fashion and create *IPO[-3,-2] Full Period* and *IPO[-3,-2] Partial Period*. As presented in column (2) of Table 7, we find similar results when these pre-IPO effects are included in the model. These results suggest that the effect of going public on analyst bias is driven by employee-level decisions and not by firm-level changes in the hiring or firing practices of the brokerage.

5.4 Stock recommendation revisions

For our final analysis, we examine the robustness of our results to an important alternative analyst output, stock recommendation revisions.[10] Optimistic bias in analyst stock recommendations has been documented in numerous prior studies (e.g., Lin and McNichols, 1998; Michaely and Womack, 1999; Mehran and Stulz, 2007), and indeed was one of the primary drivers for the Global Settlement.

We obtain analyst stock recommendations from I/B/E/S and define the variable *RecRevision* as the difference between an analyst's current recommendation and prior recommendation for the covered firm, where recommendations are coded on a 5-point scale (1 = strong sell, 2 = sell, 3 = hold, 4 = buy , 5 = strong buy). Thus, positive values indicate upward revisions and negative values indicate downward revisions. We use a 5-point rating scale since this was the rating scheme used by analysts through most of our IPO events, but note that our results are robust to using a 3-point rating scale (Kadan et al., 2009). We then replace our

[10] In untabulated analysis, we also examine quarterly earnings forecasts. To do this, we estimate variations of equation (1), where firm-year fixed effects are replaced with firm-year-quarter fixed effects, using either one-, two-, three- or four-quarter-ahead forecasts. The results provide no evidence of significant changes in bias in quarterly forecasts around IPOs for any of the quarterly forecast horizons.

dependent variable *Bias* with *RecRevision* in equation (1). The model is again estimated with analyst and covered firm-year fixed effects. We note that our sample drops to approximately 271,000 observations for this test due to the fact that recommendations are far less frequently revised than are earnings forecasts.

We present the results from our recommendation revision tests in Table 8. Similar to our primary forecast bias results in Table 3, in column (1), we present the results without the pre-trend or control variables. In column (2) we include the control variables from equation (1), with the exception of forecast horizon, and in column (3) we add the pre-trend control. Consistent with our primary results, we find a negative and significant (at the 1 percent level) coefficient on the IPO event indicator variable *IPO[-1,+1]* in all three columns, suggesting analysts are more likely to revise recommendations downward in the IPO window. In column (3), the coefficient on the pre-tend indicator variable *IPO[-3,-2]* is insignificant. Collectively, the results of these robustness tests provide additional support for the scrutiny hypothesis.

6. Conclusion

We examine the effects of a broker going public on the objectivity of the brokerage's sell-side equity analysts. Using a sample of 23 IPOs spanning three decades, we find that analysts at IPO brokers decrease forecast bias relative to other analysts during the IPO period. This result is consistent across various robustness tests and suggests analysts respond to increased scrutiny around IPOs by increasing their objectivity.

Our study provides an important contribution to the literature, which has been unable to examine changes in individual employees' behavior around IPOs. Prior studies are primarily focused on firm-level outcomes and evidence is somewhat mixed as to whether IPOs are associated

with positive or negative firm outcomes. We extend this literature by focusing on individual employees working at the IPO firm and our evidence suggests that IPO effects trickle down to and are experienced by non-executives at the firm. These IPO effects are associated with an increase in objectivity, which is inconsistent with earlier research but consistent with more recent findings that IPOs are associated with improved financial reporting quality at the firm level (Ball and Shivakumar, 2008).

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Appendix
Variable Definitions

Variable	
AllStar	an indicator variable equal to one if the analyst is ranked as an AllStar analyst by Institutional Investor magazine, and zero otherwise
Bias	the value of the forecast minus reported earnings, scaled by price as of two days prior to the forecast announcement date, and multiplied by 100
BrokerSize	the natural log of the number of analysts in the analyst's brokerage house who issued a forecast during the prior year
Coverage	the natural log of the number of firms the analyst forecasted for during the prior year
Experience	the natural log of the number of years the analyst has issued a forecast for the firm of interest as of the prior year
Horizon	the natural log of the number of days between the forecast and the fiscal period end date
IPO[-1,+1]	an indicator variable equal to one for forecasts issued by IPO analysts during the two-year period surrounding their brokerage's IPO, and zero otherwise
IPO[-1,+1] Full Period	an indicator variable equal to one for forecasts issued during the two-year period surrounding their brokerage's IPO by analysts who remain at the IPO brokerage throughout that two-year period, and zero otherwise
IPO[-1,+1] Partial Period	an indicator variable equal to one for forecasts issued during the two-year period surrounding their brokerage's IPO by analysts who join or leave the IPO brokerage throughout that two-year period, and zero otherwise
IPO[-3,-2]	an indicator variable equal to one for forecasts issued by IPO analysts during the two-year period ending one year before the IPO date, and zero otherwise
IPO[-3,-2] Full Period	an indicator variable equal to one for forecasts issued during the two-year period ending one year before the IPO date, for those IPO analysts who remain at the IPO brokerage throughout the two-year period surrounding their brokerage's IPO, and zero otherwise
IPO[-3,-2] Partial Period	an indicator variable equal to one for forecasts issued during the two-year period ending one year before the IPO date, for those IPO analysts who join or leave the IPO brokerage during the two-year period surrounding their brokerage's IPO, and zero otherwise
RecRevision	the difference between an analyst’s current recommendation and prior recommendation for the covered firm, on a 5-point scale

Figure 1
Trends in analyst forecast bias around IPO events

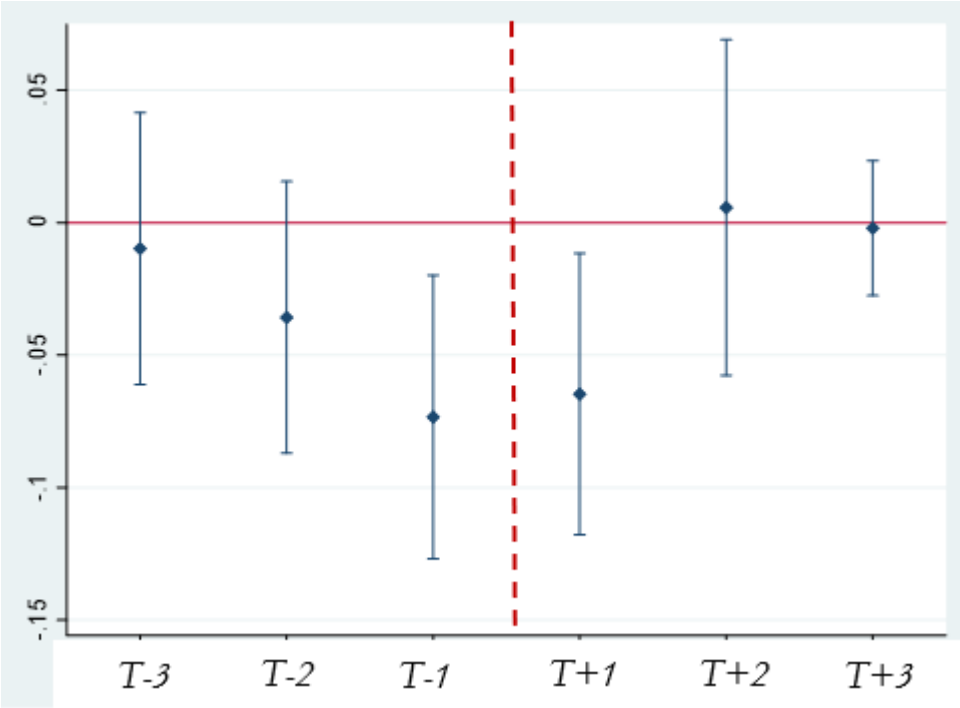

Table 1
IPO Events

Broker Name	IPO Date
Bear Stearns Cos Inc	10/29/1985
First Albany Cos Inc	12/11/1985
Alex Brown Inc	02/28/1986
Morgan Stanley Group Inc	03/21/1986
Rodman & Renshaw Capital Group	05/29/1986
Ryan Beck & Co	06/27/1986
Oppenheimer Capital LP	07/01/1987
The Charles Schwab Corp	09/23/1987
Southwest Securities Group Inc	10/11/1991
Dean Witter Discover & Co	02/22/1993
Donaldson Lufkin & Jenrette	10/25/1995
Hambrecht & Quist Group Inc	08/09/1996
Conning Corp	12/15/1997
Ragen MacKenzie Group Inc	06/22/1998
Goldman Sachs Group Inc	05/03/1999
Wit Capital Group Inc	06/04/1999
TD Waterhouse Group Inc	06/23/1999
Instinet Group Inc	05/17/2001
Lazard Ltd	05/04/2005
Thomas Weisel Partners Group	02/01/2006
KBW Inc	11/08/2006
JMP Group Inc	05/10/2007
FBR Capital Markets Corp	06/07/2007

This table details our final sample of broker IPOs.

Table 2
Descriptive Statistics

	Mean	Std. Dev	25th	Median	75th
IPO[-3,-2]	0.013	0.115	0.000	0.000	0.000
IPO[-1,+1]	0.014	0.119	0.000	0.000	0.000
Bias	0.854	4.910	-0.173	0.000	0.312
BrokerSize	60.75	58.43	19.00	43.00	81.00
Horizon	114.06	81.33	58.00	73.00	155.00
Coverage	18.32	13.73	10.00	16.00	22.00
Experience	2.10	3.29	0.00	1.00	3.00

This table provides descriptive statistics for our primary sample of 797,125 one-year ahead EPS forecasts. Unlogged values of the variables are reported for ease of interpretation. Variable definitions are provided in the appendix.

Table 3
Broker IPOs and Analyst Forecast Bias

Dependent Variable: *Bias*	(1)	(2)	(3)
IPO[-3,-2]			-0.023
			(-0.98)
IPO[-1,+1]	**-0.056****	**-0.064*****	**-0.068*****
	(-2.40)	**(-2.71)**	**(-2.80)**
BrokerSize		-0.009	-0.009*
		(-1.64)	(-1.65)
Horizon		0.378***	0.378***
		(37.58)	(37.58)
Coverage		0.015***	0.015***
		(2.91)	(2.93)
Experience		-0.016***	-0.016***
		(-4.18)	(-4.18)
Analyst FE	**Yes**	**Yes**	**Yes**
Firm-Year FE	**Yes**	**Yes**	**Yes**
N	795,472	795,472	795,472
Adj. R^2	0.796	0.797	0.797

This table presents results from estimating equation (1). T-statistics are reported in parentheses, and standard errors are clustered by firm-year. All p-values are two-tailed. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4
Broker IPOs and Analyst Forecast Bias
(Yearly Effects)

Dependent Variable: *Bias*	(1)	(2)	(3)
IPO[-3]			-0.010
			(-0.31)
IPO[-2]		-0.034	-0.036
		(-1.12)	(-1.15)
IPO[-1]	**-0.067****	**-0.072****	**-0.073****
	(-2.18)	**(-2.27)**	**(-2.25)**
IPO[+1]	**-0.061****	**-0.063****	**-0.065****
	(-1.97)	**(-2.02)**	**(-2.00)**
IPO[+2]		0.007	0.006
		(0.20)	(0.15)
IPO[3+]			-0.002
			(-0.13)
BrokerSize	-0.009	-0.009*	-0.009*
	(-1.64)	(-1.65)	(-1.65)
Horizon	0.378***	0.378***	0.378***
	(37.58)	(37.58)	(37.58)
Coverage	0.015***	0.015***	0.016***
	(2.91)	(2.92)	(2.93)
Experience	-0.016***	-0.016***	-0.016***
	(-4.18)	(-4.18)	(-4.17)
Analyst FE	**Yes**	**Yes**	**Yes**
Firm-Year FE	**Yes**	**Yes**	**Yes**
N	795,472	795,472	795,472
Adj. R^2	0.797	0.797	0.797

This table presents results from estimating equation (2). T-statistics are reported in parentheses, and standard errors are clustered by firm-year. All p-values are two-tailed. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5
Broker IPOs and Analyst Forecast Bias
(Alternative Fixed Effects, Ranked Bias, and Clustering)

Panel A: Fixed Effects

Dependent Variable: *Bias*	(1)	(2)	(3)
IPO[-3,-2]	-0.019	-0.022	-0.029
	(-0.77)	(-0.84)	(-1.11)
IPO[-1,+1]	**-0.067*** **	**-0.068** **	**-0.046***
	(-2.69)	**(-2.46)**	**(-1.65)**
Controls	**Yes**	**Yes**	**Yes**
Analyst FE	**Yes**	**No**	**No**
Broker FE	**Yes**	**No**	**Yes**
Analyst-Broker FE	**No**	**Yes**	**No**
Analyst-Firm FE	**No**	**No**	**Yes**
Firm-Year FE	**Yes**	**Yes**	**Yes**
N	795,421	792,794	692,094
Adj. R^2	0.798	0.799	0.812

Panel B: Relative Bias

Dependent Variable: *RankedBias*	(1)
IPO[-3,-2]	0.343
	(0.94)
IPO[-1,+1]	**-0.762** **
	(-2.16)
Controls	**Yes**
Firm Controls	**Yes**
Analyst FE	**Yes**
Firm-Year FE	**No**
N	794,585
Adj. R^2	0.031

Table 5 (continued)

Panel C: Clustering

Dependent Variable: *Bias*	Clustering: Firm and Year	Clustering: Broker and Year	Clustering: Analyst and Year
IPO[-3,-2]	-0.023	-0.023	-0.023
	(-0.91)	(-1.30)	(-0.96)
IPO[-1,+1]	**-0.068****	**-0.068*****	**-0.068*****
	(-2.74)	**(-3.15)**	**(-2.80)**
Controls	**Yes**	**Yes**	**Yes**
Analyst FE	**Yes**	**Yes**	**Yes**
Firm-Year FE	**Yes**	**Yes**	**Yes**
N	795,421	792,794	692,094
Adj. R^2	0.798	0.799	0.812

This table presents results from estimating equation (1) using alternative fixed effects, a within-firm-year ranked measure of bias, and alternative standard error clustering. T-statistics are reported in parentheses. All p-values are two-tailed. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6
Broker IPOs and Analyst Forecast Bias
(Global Settlement Sample Partition)

Dependent Variable: *Bias*	Pre-GS	Post-GS
IPO[-3,-2]	-0.027	-0.052
	(-0.97)	(-0.95)
IPO[-1,+1]	**-0.065****	**-0.096***
	(-2.31)	**(-1.68)**
BrokerSize	-0.010	0.004
	(-1.42)	(0.40)
Horizon	0.419***	0.284***
	(34.21)	(16.29)
Coverage	0.014**	0.018
	(2.33)	(1.37)
Experience	-0.014***	-0.019***
	(-2.71)	(-3.19)
Analyst FE	**Yes**	**Yes**
Firm-Year FE	**Yes**	**Yes**
N	485,553	309,330
Adj. R^2	0.817	0.763

This table presents results from estimating equation (1), partitioning the sample into pre- and post-Global Settlement periods (1982-2002 vs 2003-2012). T-statistics are reported in parentheses, and standard errors are clustered by firm-year. All p-values are two-tailed. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7
Broker IPOs and Analyst Forecast Bias
(Analysts who Remain at the Brokerage Throughout the IPO Period)

Dependent Variable: *Bias*		(1)	(2)
IPO[-3,-2] Full Period			-0.044
			(-1.63)
IPO[-3,-2] Partial Period			0.141
			(1.41)
IPO[-1,+1] Full Period		**-0.072*** **	**-0.081*** **
		(-3.02)	**(-3.25)**
IPO[-1,+1] Partial Period		**0.132**	**0.152**
		(1.32)	**(1.47)**
BrokerSize		-0.009	-0.009*
		(-1.64)	(-1.67)
Horizon		0.378***	0.378***
		(37.58)	(37.58)
Coverage		0.015***	0.015***
		(2.92)	(2.92)
Experience		-0.016***	-0.016***
		(-4.18)	(-4.19)
Analyst FE		**Yes**	**Yes**
Firm-Year FE		**Yes**	**Yes**
N		795,472	795,472
Adj. R^2		0.797	0.797
Within Reg F-Tests			
IPO[-1,+1] Full =	Diff	-0.204**	-0.233**
IPO[-1,+1] Partial	F-Stat	(3.98)	(4.86)

This table presents results from estimating a variation of equation (1), where the IPO effect is estimated separately for analysts who remain at the IPO brokerage throughout years t-1 to t+1 from those analysts who join or leave the brokerage during that period. T-statistics are reported in parentheses, and standard errors are clustered by firm-year. All p-values are two-tailed. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8
Broker IPOs and Stock Recommendations

Dependent Variable: *RecRevision*	(1)	(2)	(3)
IPO[-3,-2]			-0.008
			(-0.26)
IPO[-1,+1]	**-0.120*** **	**-0.118*** **	**-0.121*** **
	(-4.79)	**(-4.74)**	**(-4.65)**
BrokerSize		0.004	0.004
		(0.64)	(0.63)
Coverage		-0.017**	-0.017**
		(-2.39)	(-2.38)
Experience		-0.004	-0.004
		(-1.13)	(-1.13)
Analyst FE	**Yes**	**Yes**	**Yes**
Firm-Year FE	**Yes**	**Yes**	**Yes**
N	270,932	270,932	270,932
Adj. R^2	-0.033	-0.033	-0.033

This table presents results from estimating a modified version of equation (1) where the dependent variable is stock recommendation revisions (*RecRevision*). T-statistics are reported in parentheses, and standard errors are clustered by firm-year. All p-values are two-tailed. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.