

**Pre-IPO Communications and Analyst Research:
Evidence Surrounding the JOBS Act**

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Abstract

The JOBS Act allows certain analysts to engage in pre-IPO communications with investors, bankers, and company management, but does not affect other analysts nor does it relax existing restrictions on analyst compensation structure. Analysts privy to these pre-IPO communications issue reports that are more optimistically biased, less accurate, and generate smaller market reactions. This increased optimism seems to benefit analysts and investment banks, as pre-IPO communications increase the positive relation between analyst optimism and both post-IPO trading volume and IPO pricing. Our evidence suggests that such communications are an important determinant of how analysts manage their conflicts of interest.

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1. Introduction

Analysts face a conflict of interest because they benefit from both accurate and optimistically biased reports. Accurate earnings forecasts can improve analysts' reputations and facilitate more favorable labor market outcomes.¹ Alternatively, optimistic research can increase future investment banking business and generate higher trading volume.² Given that analysts' compensation and career success are influenced by investment banking and brokerage revenues (Groysberg et al., 2011), analysts may be prone to bias their research output optimistically. For example, using samples of initial public offerings (IPOs) and seasoned equity offerings (SEOs) during the relatively unregulated environment prior to the turn of the century, Lin and McNichols (1998), Michaely and Womack (1999), and Dechow, et al. (2000) all find that analysts affiliated with a firm's underwriter tend to be more optimistically biased.

In part because of these conflicts of interest, in the early 2000s regulators limited analysts' role in the securities issuance process. These regulations, among other things, simultaneously limited permissible pre-deal communications, prohibited analyst compensation from being tied to investment banking revenue, and mandated changes in analyst report content (Kadan et al., 2009). After these regulatory changes, analyst optimism generally declined (Kadan et al., 2009; Guan et al., 2012; Clarke et al., 2011), and recommendations became more aligned with earnings and growth forecasts (Barniv et al., 2009; Chen and Chen, 2009). However, as noted by Bradshaw (2009), these changes occurred simultaneously, making it difficult to identify the consequences of any one particular regulatory change.

We contribute to this literature by providing direct evidence on how pre-deal communications prior to the IPO (i.e., pre-IPO communications) affect analyst research. On the one hand, the private information and industry knowledge analysts acquire through pre-IPO interactions make it easier for analysts to produce accurate reports.³ On the other hand, pre-IPO interactions with bankers and investors increase the benefits to issuing optimistic forecasts because doing so enhances investor enthusiasm for the IPO, which can influence trading revenues and IPO pricing. Despite these channels through which pre-IPO communications may affect analyst research, there is little direct evidence on the topic. Existing research has been

¹ See Jackson, 2005; Hong and Kubik, 2003; and Mikhail et al., 1999.

² See Degeorge, et al., 2007; Ljungqvist, et al., 2009; Jackson, 2005; and Niehaus and Zhang, 2010.

³ See Boni and Womack, 2003; Bradley et al., 2016a; Brown et al., 2015; Chen and Marquez, 2009; Green et al., 2014a; Jacob et al., 2008; Soltes, 2014.

limited by the lack of available data, as the extent of pre-IPO interactions between analysts and managers, investors, and investment bankers is not observable.

We utilize the April 5, 2012 passage of the Jumpstart Our Business Startups Act (JOBS, or the “Act”) to identify the effect of pre-IPO communications on analyst research. The JOBS Act was designed to reduce the risks and burdens of going public for IPO issuers with less than \$1 billion in pre-IPO annual revenue, referred to as emerging growth companies (EGCs).⁴ An important component of the Act is a set of provisions designed to involve research analysts affiliated with members of the EGC issuer’s IPO underwriting syndicate (“EGC affiliated analysts”) more extensively in the IPO process. To this end, the JOBS Act allows EGC affiliated analysts to attend pitch meetings and due diligence sessions with investment bankers and to interact with potential investors at the request of investment bankers, even before the IPO.

Two important features of the JOBS Act allow us to plausibly identify the effect of pre-IPO communications on analyst behavior. First, the JOBS Act applies only to EGC affiliated analysts. Thus, analysts covering EGCs that are not affiliated with any of the issuer’s underwriters (“EGC unaffiliated analysts”) and all analysts covering non-EGCs represent natural control groups whose permissible activities are unaffected by JOBS. Second, unlike previous legislative changes affecting permissible pre-IPO communications, the JOBS Act does not relax restrictions on analyst compensation or report content because these restrictions were viewed as necessary for investor protection (IPO Task Force, 2011). Thus, we argue that the differential change in the behavior of EGC affiliated analysts (i.e., treated analysts) relative to untreated analysts following JOBS can reasonably be interpreted as the effect of pre-IPO communications on analyst behavior.

We begin by investigating how pre-IPO communications affect analysts’ initial earnings forecasts after the IPO. Increased pre-IPO communications may result in more accurate reports if they increase the information available to analysts. Alternatively, increased pre-IPO communications may result in more optimistically biased, and less accurate, analyst reports if analysts use pre-IPO communications to facilitate banking or trading revenues. We find that following JOBS, EGC affiliated analysts have become significantly less accurate and more optimistic. After JOBS, the relative accuracy of affiliated analysts (compared to their unaffiliated

⁴ Throughout the paper we refer to issuers with less (greater) than \$1 billion in pre-IPO annual revenue as EGCs (non-EGCs) whether their IPO occurs before or after the JOBS Act.

counterparts) declines by 0.42% of price or 0.5 standard deviations more for EGCs than it does for non-EGCs. The post-JOBS increase in the relative bias of EGC affiliated analysts is of similar magnitude. To the extent that we have identified the JOBS Act treatment effect of greater communication, our results suggest that pre-IPO analyst communications increase analysts' optimistic bias.

Given the increased optimistic bias we document amongst EGC affiliated analysts, a natural question is how the market views the informativeness of their reports. For example, if the market recognizes the bias we document, then the response to affiliated analyst reports may be muted following JOBS. Indeed, we find that the cumulative abnormal returns surrounding analyst coverage initiations are significantly muted for post-JOBS affiliated analysts relative to unaffiliated analysts covering EGCs. This finding is robust to removing confounding events (Altinkılıç, et al., 2013), restricting the sample to optimistic analysts, and using two-hour intraday CARs. These results corroborate prior literature documenting that affiliated analyst reports were discounted by the market before regulations in the early 2000s limited analysts' access to the IPO process.⁵ Unlike the regulations in the early 2000s, however, the JOBS Act affects only pre-IPO communications. Thus, our setting is better able to identify whether pre-IPO communications contribute to this muted market reaction.

Our results suggest that post-JOBS, affiliated EGC analysts issue less accurate reports and reports that are less informative to investors at the time of report announcement. Although this increased optimism may harm an analyst's reputation (Jackson, 2005), it may be rational if pre-IPO communications increase the benefits to issuing optimistic reports. We find empirical evidence consistent with this as pre-IPO communications appear to make pre-IPO optimism a more important driver of post-IPO trading volume and IPO pricing, which are important inputs into analyst compensation and investment banking revenues. Specifically, affiliated analyst optimism becomes more positively related to both post-IPO share turnover (our proxy for investment banking trading revenues) and IPO price (i.e., price revisions and IPO underpricing) for EGCs relative to non-EGCs following the passage of JOBS. Notably, these tests rely on analysts' initial forecast bias, which is not revealed until the end of the post-IPO quiet period, to proxy for pre-IPO optimism. This proxy is reasonable because analysts' reputational considerations make it costly to change their optimism between their pre-IPO communications

⁵ Refer to Dugar and Nathan, 1995; Kadan et al., 2009; and Michaely and Womack, 1999.

and their official coverage initiation, however our inability to directly observe pre-IPO optimism is an important caveat to our analysis.

Overall, our findings are consistent with pre-IPO communications expanding the benefits to optimistic reports for analysts, investment banks, issuers, and investors.⁶ However, these benefits of analyst optimism do not accrue to all market participants. For example, investors that rely on analyst reports as they are released or invest at higher post-IPO prices are at a disadvantage, especially if they do not recognize the increased incentives for affiliated analysts of EGCs to be optimistic post-JOBS. Since our identification strategy uses unaffiliated analysts of EGCs and all analysts of non-EGCs to control for overall market conditions, as well as a matched sample to control for potential differences in pre- and post-JOBS issuers, it is unlikely that these findings are driven by factors unrelated to the JOBS Act analyst provisions. Robustness tests further demonstrate that our findings are unlikely to be driven by an increased propensity for informed analysts to piggyback off of other news releases (e.g., Altinkılıç and Hansen, 2009), an increased ability of EGC management to select or manipulate analysts, or other JOBS Act consequences.

This study provides new evidence on the link between analyst incentives and optimistic bias. Specifically, we find that pre-IPO analyst communications are an important determinant of analyst optimism, which motivate analysts to serve their employer at the expense of those who consume their research. We also complement the existing literature investigating the impact of various legislations between 2000 and 2003 that simultaneously disallowed selective disclosure to sell-side analysts (Regulation Fair Disclosure), restricted equity analyst permissible communications (Global Settlement), and changed the format of sell-side equity analyst reports and the compensation structure of sell-side analysts (imposed by the self-regulatory organizations, “SRO rules”).⁷ By contrast, the JOBS Act allows us to isolate the effect of pre-IPO communications on analyst behavior.

This study also adds to recent literature on the importance of private communications initiated by sell-side equity analysts (see, e.g., Soltes, 2014). This literature provides empirical evidence of the importance of such communications (Green et al., 2014a, 2014b; Kirk and

⁶ Existing research shows that institutions sell into the liquidity created by analyst coverage initiations (Bradley et al., 2003).

⁷ See Barniv et al. (2009), Boni (2006), Chen and Chen (2009), Clarke et al. (2011), Guan et al. (2012), and Kadan et al. (2009).

Markov, 2016), which appear to improve analysts' forecasting ability and ultimately the information content of their research (Brown et al., 2015; Green et al., 2014a). Unlike the prior literature, which generates identification from analysts or managers choosing to engage in private communications via presentation events or private meetings, we exploit the passage of the JOBS Act to examine how changing the level of permissible private communications influences analyst behavior. Our study shows that these communications affect analysts differently in an IPO setting. In this setting, we find that increasing private communications results in lower forecast accuracy for affected analysts and a decrease in the informativeness of their research.

Finally, this paper extends the emerging literature on the consequences of the JOBS Act. Prior research on the JOBS Act has focused on provisions that reduce the risks and burdens of going public. For example, Dambra et al. (2015) find that the JOBS Act provisions reducing the risks of going public result in an increase in IPO volume, especially for firms with high proprietary disclosure costs, while Barth et al. (2017) and Chaplinski et al. (2017) find that these same provisions also increase IPO underpricing. In this paper, we provide the first evidence on the consequences of the JOBS Act's analyst provisions. Our findings on the effect of changing analyst interactions are particularly important given the ongoing policy discussions in the United States and abroad. For example, FINRA recently passed Rules 2241 and 2242 to expand JOBS Act deregulations to both non-EGCs and debt analysts (Morrison-Foerster, 2014a). In addition, the European Union has begun the process of overhauling its analyst industry by proposing new rules that may encourage analysts to cater more to higher paying customers (Patrick et al., 2015). Our collective evidence offers insights into the benefits and consequences of further deregulating analyst interactions and their asymmetric communications with various customer segments.

2. Literature Review

2.1 Analyst Conflicts of Interest

Analysts encounter conflicts of interest from their interactions with managers, investors, and investment bankers in the production of their research. Mehran and Stulz (2007) provide a comprehensive literature review on the long-standing debate in the literature of whether and how these interactions influence the quality of analysts' research output.

The conflict of interest that analysts face is rooted in the fact that they benefit both from issuing accurate and optimistically biased reports. Accurate earnings forecasts can improve

analysts' reputations and facilitate more favorable labor market outcomes (Mikhail et al., 1999; Hong and Kubik, 2003; Jackson, 2005; Ke and Yu, 2006). Consistent with the value of personal reputation, Fang and Yusada (2009) find that reputation is an effective disciplinary device against analysts' conflicted interests. Alternatively, sell-side equity analysts affiliated with investment banks that underwrite a firm's securities issuances have the incentive to optimistically bias their research output in order to increase future investment banking fees and/or brokerage revenues (e.g., Ljungqvist et al., 2009; Degeorge et al., 2007). Existing literature suggests that these incentives dominate the aforementioned benefits to issuing accurate reports in the absence of regulation. As a result, analysts that are affiliated with an investment bank which recently acted as a firm's underwriter tend to bias their research output optimistically (Lin and McNichols, 1998; Michaely and Womack, 1999; Dechow et al., 2000). Evidence suggests that this optimistic bias serves to increase trading revenues (Jackson, 2005; Neihaus and Zhang, 2010), and possibly also future underwriting business, although the evidence on this latter effect is mixed.⁸ Importantly, Groysberg et al. (2011) link this behavior to analyst compensation, showing that analyst pay is positively related to the effect that an analyst's research has on equity underwriting and brokerage fees.⁹

2.2 Regulating Analyst Behavior

In part to address the concern that analyst involvement in the investment banking process created incentives for analysts to optimistically bias their research, in the early 2000s regulators limited analyst access to managers, investors, and investment bankers through the SRO rules and the Global Settlement. These regulatory changes required separate reporting lines for research analysts and investment bankers; banned analysts from being involved in investment banking pitch meetings; disallowed joint meetings between investment bankers, analysts, and management; prohibited ties between analyst compensation and investment banking revenues; and required additional disclosure in analyst reports. The extant literature generally finds that analyst research was less optimistically biased following these regulations, especially amongst affiliated analysts. However, recommendations ultimately became less informative,¹⁰ and as

⁸ See Ljungqvist, et al., 2006, 2009; Clarke, et al., 2007; and Degeorge et al., 2007.

⁹ Additional explanations posited by the literature for issuing analysts to provide more optimistic research include supporting the price set by the affiliated bank's underwriter (James and Karceski, 2006; Huyghebaert and Xu, 2015), currying favor with managers for access to private information (Dugar and Nathan, 1995), and managers being able to select more optimistic analysts (Lin and McNichols, 1998).

¹⁰ See Barber et al., 2006, 2007; Chen and Chen 2009; and Kadan et al., 2009.

noted by Guan et al. (2012), these regulations also were followed by decreased forecast accuracy.

Around the same time, Reg FD was introduced in 2000 to prevent managers from selectively disclosing information to analysts without simultaneously disclosing such information to the public (Heflin et al., 2003). A robust literature has examined the effect of the rule change on investors, managers, and analysts (e.g., Gintschel and Markov, 2004; Heflin et al., 2016). In terms of analyst informativeness, market reactions to analyst reports are more muted following Reg FD.¹¹ However, the evidence on how Reg FD affected analyst forecast outcomes is mixed. Bailey et al. (2003) and Heflin et al. (2003) find no change in analyst forecast accuracy, whereas Agrawal et al. (2006) show that analyst forecast accuracy decreases after Reg FD. Mohanram and Sunder (2006) find no average decrease in forecast accuracy, but provide cross-sectional evidence that forecast accuracy declines for larger brokerage houses.

Despite the apparent impact of these regulations on analyst behavior, Bradshaw (2009) and Leuz and Wysocki (2016) note that it is difficult to identify the consequences of any single legislation and, thus, any single economic cause for the observed changes in analyst behavior. For example, Bailey et al. (2003) find that some of the market responses attributed to Reg FD were driven by contemporaneous changes to the decimalization of stock markets. Furthermore, all analysts were affected by Reg FD and the SRO rules, leaving no natural control group to help identify the effects of the rules without contamination from concurrent market conditions or other factors. By contrast, we are able to take advantage of a unique empirical setting in an attempt to isolate the effect of pre-IPO communications on analyst behavior.

3. Pre-IPO Communications and Analyst Behavior

In this paper, we investigate the effects of analysts' pre-IPO communications with the issuer's management, investors, and the analyst's investment banking colleagues on analyst research quality. Here, we discuss two mechanisms through which such pre-IPO communications may affect how analysts manage their conflicts of interest.

¹¹ For broader studies of the decline in market inefficiencies and/or the information content of analyst output over this time period, please see Jenkinson, et al. (2016), Altinkılıç, et al. (2016), and Chordia, et al. (2014).

3.1. Pre-IPO Communications and Increased Forecast Accuracy

Pre-IPO communications may result in an increase in the quality of analyst coverage because these communications lower the cost of producing accurate analyst reports. Interactions with other pre-IPO participants can provide analysts with private information that is otherwise costly (or impossible) for investors to obtain. Green et al. (2014a) show that private interactions between analysts and management increase forecast accuracy, while Soltes (2014) provides evidence that private meetings offer more diverse interactions with company personnel and increase the depth of analyst access to management.

Anecdotal evidence suggests that pre-IPO communications can provide underwriting analysts access to management and information that is otherwise unavailable during the IPO process (Jarzemsky and Demos, 2013; Lattman and Craig, 2013; Hirsch and Baker, 2017). For example, prior to Twitter's IPO, affiliated analysts "forecast[ed] 2015 revenue at Twitter to be about 28% below the average of four unaffiliated analysts who have published forecasts." The affiliated analysts, who "have the best view of the company's prospects, thanks to their access to executives, haven't published their views.... Instead, the information has been passed on in discussions with the firm's clients" (Jarzemsky and Demos, 2013).

Though Reg FD explicitly prohibits the selective release of material information, analysts can acquire nonmaterial information from private meetings with managers to complement public information (or industry expertise from their investment bank) in order to develop an informed opinion, an act explicitly allowed by Reg FD.¹² Soltes (2014) notes that managers are allowed to review an analyst's model, and analysts seek private interactions with managers "to 'triangulate their hypotheses', 'bounce ideas', or 'calibrate expectations of future performance'." Prior literature indicates that this behavior is often limited to private conversations with management. Analysts are averse to acquiring information in a public setting such as Q&A in conference calls, where management responses can inform an analyst's competitors (Brown et al., 2015, Soltes 2014). Increased interactions with management can also lead to unintentional information transfers, such as vocal cues or body language (Brown et al., 2015, Mayew and Venkatachalam 2012, Hobson, et al., 2012). Through these pre-IPO interactions, affiliated analysts may be able to better identify firms' future prospects.

¹² See, for example, Green et al. (2014a), Soltes (2014), Brown et al. (2015).

Moreover, pre-IPO communications may also provide analysts with increased access to industry-specific knowledge (Boni and Womack, 2003), enabling them to issue more accurate reports. Bradley et al. (2016, 2017) empirically support this idea, finding that industry-specific information allows analysts to issue more accurate forecasts, while Brown et al. (2015) provide survey evidence confirming these channels through which private communications affect analyst research quality. In the survey, industry knowledge and private communications with management were the two most important inputs to analyst earnings forecasts, ahead of publicly available information such as management earnings forecasts, earnings reports, and recent stock price performance. This leads to our first hypothesis.

Increased Accuracy (H1): Pre-IPO communications will increase forecast accuracy.

3.2. Pre-IPO Communications and Increased Forecast Bias

Alternatively, pre-IPO communications may lead to more optimistically biased and less accurate reports, because such communications increase the benefits of issuing biased reports. Although the current regulatory regime prohibits analyst compensation from being explicitly tied to investment banking revenues, Groysberg et al. (2011) find that brokerage trading revenues continue to be an important input into analyst compensation. Importantly, brokerage trading revenues are positively associated with analyst optimism (Cowen et al., 2006; Jackson, 2005; Niehaus and Zhang, 2010). Enhanced pre-IPO communications provide analysts the opportunity to interact with investors earlier, thus allowing their optimistic outlook to have a greater impact on IPO allocations, IPO pricing, and trading immediately following the IPO. This increases analysts' incentives to sacrifice report accuracy for optimism. Furthermore, the issuance of more optimistic forecasts by affiliated analysts may also provide increased liquidity for their institutional clients, and earlier communications may facilitate information transfers. Prior research shows that analyst coverage initiations following an IPO result in trading volume spikes, which allow institutional investors to sell into new investor demand (Bradley, et al., 2003; Ofek and Richardson, 2003). Such volume spikes may be more valuable to institutional investors when they can be anticipated, which is more likely to be the case with increased pre-IPO communications.

In addition, joint meetings with members of the underwriting syndicate may also result in analysts feeling pressured to become more optimistic in order to increase the future deal-making capabilities of the bank. For instance, analysts employed by the underwriter may have incentives

to produce optimistic research if doing so increases the likelihood that their employer is awarded future banking mandates (see, e.g., Ljungqvist et al., 2006, 2009) or raises underwriter fees through increased IPO pricing. Finally, earlier and more frequent interactions with company management may reinforce career concern motives for analysts to optimistically bias their research because doing so has the added benefit of increasing an analyst's future access to management (Lim, 2002; Ke and Yu, 2006), even in a post Reg FD regime (Mayew, 2008). This leads to our second hypothesis.

Increased Bias (H2): Pre-IPO communications will increase forecast bias.

4. Empirical Design and Data Description

4.1. Empirical Setting: The JOBS Act and Pre-IPO Communications

To identify the effect of pre-IPO communications on analyst behavior, we utilize the unique setting created by the analyst provisions in the JOBS Act, which was signed into law on April 5, 2012. These provisions are largely based on recommendations from the IPO Task Force (2011), which concluded that: (1) analyst regulations served to depress analyst following and information dispersion around IPOs, resulting in small-firm IPOs being less attractive to potential underwriters and investors, and (2) “existing limitations [on research coverage] are unnecessarily restrictive and unfairly favor institutional investors that have greater access to research analysts than retail investors.”

To address these concerns, the Act contains a set of provisions that reintegrate EGC affiliated analysts into the IPO process through increased pre-IPO communications. Section 105(b) of the Act removes restrictions on pre-IPO communications between bankers, managers, prospective investors, and affiliated analysts. Post-JOBS, affiliated analysts of EGC issuers may engage in pre-IPO conversations with investors arranged by investment bankers and participate in presentations by EGC management to educate the issuer's sales force (Morrison-Foerster, 2014b; Sidley Austin LLP, 2012). In contrast, prior to the Act, affiliated analysts could not contact potential investors before an IPO (Morrison-Foerster, 2014b), and New York Stock Exchange (NYSE) Rule 472 prohibited investment bankers from facilitating communication between equity analysts and prospective investors. The Act also increases analysts' pre-IPO communications with managers and bankers by allowing affiliated analysts to attend pitch meetings and due diligence meetings, a practice that was banned following the regulations of the

early 2000s. Figure 1 provides a typical timeline for analyst reports before and after the JOBS Act went into effect.

Even with these changes, some limitations to analyst involvement in the IPO process are still in place post-JOBS. Although analysts of EGCs can introduce themselves, describe factors relevant to their research, and ask follow-up questions at pre-IPO pitch meetings, they cannot adjust their research to obtain investment banking business or commit to optimistic post-IPO coverage (Sidley Austin LLP, 2012). In addition, analysts are still prohibited from attending road show presentations, and analyst compensation cannot be tied to investment banking revenues. Thus, the JOBS Act reintroduces equity analysts into the IPO process but not to the extent they were involved before the passage of regulations at the turn of the century. See Appendix A for more details on the JOBS Act.

4.2. Sample

To empirically investigate how the increased pre-IPO communications afforded by the JOBS Act affected analyst behavior, we use a sample of IPO issuers between January 1, 2004 and June 30, 2014, collected from Thomson One's Securities Data Company (SDC) new equity issues database. We begin with 1,149 issuers over this period, which excludes non-firm-commitment offerings, foreign issues, closed-end trusts, blank-check companies, unit offerings, and real estate investment trusts.¹³ After excluding issuers for which we cannot match stock price data from CRSP, financial statement information from Compustat, identification information from I/B/E/S, or founding dates from Jay Ritter's Founding Dates database, we are left with 1,118 deals. We then exclude IPOs issued between April 5, 2012 and November 11, 2012 because there was uncertainty during this period regarding how the JOBS Act would affect permissible analyst behavior, especially given prior analyst regulations. This ambiguity was clarified in an SEC Q&A released on August 22, 2012 and the subsequent FINRA proposals to amend NYSE Rule 472 and NASD Rule 2711 on October 11, 2012 (Sidley Austin LLP, 2012). This restriction reduces the sample to 1,062 IPO deals.

We obtain recommendations, analyst quarterly earnings per share (EPS) forecasts, and actual EPS values from the I/B/E/S unadjusted detail file. Because the JOBS Act only applies to analyst behavior around the time of the IPO, we restrict our sample to initiations of analyst

¹³ We also drop 13 deals with SIC codes of 6091, 6371, 6722, 6726, 6732, 6733, or 6799 to eliminate any remaining leveraged buyouts, closed and open-end funds, and special purpose vehicles.

coverage within 180 calendar days of the IPO. If an analyst simultaneously initiates earnings forecasts for multiple quarters, we retain only the quarter closest to the IPO date. In total, we have 1,035 issuers with at least one report issued in the first 180 days and without non-missing firm characteristics, of which 791 occur before the enactment of JOBS on April 5, 2012, and 244 occur after November 11, 2012.

Throughout the analysis, we consider all IPO issuers with less than \$1 billion in pre-IPO revenue as “EGC issuers”, although technically the term has been used only since the JOBS Act was enacted on April 5, 2012. Using this terminology, 700 of the 791 IPOs that occur before JOBS are designated as EGC issuers and 91 as non-EGCs. Of the 244 IPOs that occur during our post-JOBS period, 207 are EGC firms and 37 are non-EGC firms. As we explain below, our primary identification strategy compares affiliated and unaffiliated analysts behavior within the same firm. Thus, most of our tests further restrict the sample to issuers that have both affiliated and unaffiliated analyst coverage in the 180 days following the IPO. This restriction reduces our sample to 506 firms, 411 of which are EGCs and 95 of which are non-EGCs. To mitigate the possibility that differences in the timing of coverage initiation affect our results, we also replicate our main tests using a sample of issuers with both affiliated and unaffiliated coverage within the 60-days after the end of the post-IPO quiet period. This sample, which we refer to as our 60-day sample, contains 363 issuers.

4.3. Identification Strategy

To identify the effect of pre-IPO communications on analyst behavior, we exploit the fact that JOBS targets only EGC affiliated analysts. Affiliated analysts are defined as those employed by any brokerage in the issuer’s underwriting syndicate, as listed in the “Underwriting” section of the IPO prospectus (data from SDC). The syndicate includes lead and co-lead managers, as well as non-managing members of the syndicate. We define unaffiliated analysts as those not employed by a brokerage in the underwriting syndicate. Because JOBS targets only affiliated analysts of EGC firms, we have two natural control groups that are not affected by the JOBS Act: (1) unaffiliated analysts covering EGCs and (2) all analysts covering non-EGCs. We use these control groups in two ways.

In our main specification, the dependent variable is the within-firm difference between median affiliated and unaffiliated analyst outcomes. We regress this on an EGC indicator equal to one for issuers with less than \$1 billion in pre-IPO annual revenue (whether their IPO occurs

before or after the JOBS Act), a post-JOBS indicator equal to one if the IPO occurred after April 5, 2012 (zero otherwise), and their interaction. The explanatory variable of interest is the interaction between the EGC and post-JOBS indicators, which captures the differential post-JOBS change in EGCs relative to the control group of non-EGCs. Because this coefficient isolates the changes in EGC behavior, after controlling for changes in other firms, it identifies the post-JOBS change in EGC outcomes after accounting for any broad market changes that do not specifically target EGCs. Specifically, we estimate Equation (1) as:

$$\text{Relative Outcome}_i \text{ (i. e., median outcome affiliated}_i \text{ – median outcome unaffiliated}_i \text{)} = \beta_0 + \beta_1 \text{EGC}_i + \beta_2 \text{Post-JOBS}_i \times \text{EGC}_i + \text{Year FEs} + \text{Industry FEs} + \text{Controls} + \varepsilon_i. \quad (1)$$

We include Fama-French 12 industry and year fixed effects (based on IPO issue date) to capture any time series differences affecting all analysts.¹⁴ Consistent with prior literature, we further control for observable differences in firm, market, analyst, and brokerage characteristics, for which we provide variable definitions in Appendix B. All analyst control variables represent the firm-level median outcome. In the Online Appendix, we provide additional analyses at the forecast level where we use the same control groups (i.e., non-EGC analysts and EGC unaffiliated analysts) in a triple-differencing framework. A benefit of this approach over the within-firm analysis in Equation (1) is that we can directly control for analyst-level explanatory variables.¹⁵

An important benefit to Equation (1) is that it identifies the effect of pre-IPO communications on analyst behavior using within-firm variation. Consequently, only issuers with both affiliated and unaffiliated coverage contribute to the estimate. In addition, this procedure equally weights each issuer as opposed to each analyst report, which ensures that our coefficients are not driven by the relative number of affiliated and unaffiliated reports.

Because our empirical tests compare the behavior of analysts targeted by JOBS to two control groups not affected by the Act, we argue that it is unlikely that factors unrelated to JOBS will materially affect the coefficients of interest. Nonetheless, to further rule out this possibility, we replicate all of our analyses using a propensity score matched (PSM) sample. By making pre-

¹⁴ We also include a post-JOBS indicator, but we do not tabulate the coefficient because year fixed effects make it difficult to interpret.

¹⁵ We also use this setting to demonstrate the robustness of our main results to the inclusion of brokerage fixed effects and more precise industry and time fixed effects, and to partition our analyses on various analyst characteristics.

and post-JOBS issuers similar along observable dimensions, this procedure mitigates the concern that pre- and post-JOBS EGC issuers differ in ways that affect the relative quality of affiliated and unaffiliated analyst research. We apply our matching procedure separately for EGCs and non-EGCs because the two types of firms are mechanically different (by definition, as EGCs have less than \$1 billion in pre-IPO revenues). We match pre- and post-JOBS issuers using a logit propensity score model that predicts the probability of issuing in the post-JOBS period as a function of Ln(Assets), Ln(Revenue), Ln(Tobin's Q), Ln(Age), Leverage, Return on Assets, Operating at Loss, Ln(Proceeds), and indicators for venture capital (VC) backing, private equity (PE) backing and high-tech industries (as defined by Loughran and Ritter, 2004). We use nearest neighbor matching without replacement to match each EGC (or non-EGC) issuer to a single control firm in the same Fama-French 12 industry with the smallest absolute difference in propensity scores (i.e., predicted values from the logit model). This procedure results in pre- and post-JOBS issuers that are similar along observable dimensions, reducing the risk of observing changes in analyst outcomes around the passage of JOBS for reasons unrelated to the Act's provisions.

4.4. Dependent Variable Definitions

We apply the identification techniques outlined above to three analyst report level outcomes: Accuracy, Bias, and Informativeness. We measure analyst accuracy and optimism using standard measures in the literature.¹⁶ Because our sample's earnings forecasts often occur immediately following the IPO, consistent with Huyghebaert and Xu (2015) and Lin and McNichols (1998), we do not compare them to a forecast consensus, as no consensus exists prior to the initiating forecasts. Rather, we benchmark the initiating forecasts of affiliated to those of unaffiliated analysts.

Accuracy is defined as $-1 \times \left| \frac{\text{Forecast}_{i,t} - \text{Actual}_i}{\text{Price}_{i,t-1}} \times 100 \right|$, where $\text{Forecast}_{i,t}$ is the analyst's quarterly EPS forecast i on day t and Actual_i is the I/B/E/S unadjusted actual EPS for the quarter-end. $\text{Price}_{i,t-1}$ is issuer i 's stock price on the last trading day prior to the analysts' coverage initiation date.¹⁷ *Relative Accuracy* is the difference between the median *Accuracy* for affiliated analysts minus the same median for unaffiliated analysts.

¹⁶ See, for example, Agrawal and Chen, 2012; Brown et al., 1987; and Dugar and Nathan, 1995.

¹⁷ Given the negative skewness of forecast outcomes (Abarbanell and Lehavy, 2003), we winsorize all forecast-level variables at the 2.5% and 97.5% level.

We define *Bias* (also referred to as forecast error in the literature) as $\frac{\text{Forecast}_{i,t} - \text{Actual}_i}{\text{Price}_{i,t-1}} \times 100$. Similar to our relative measure of accuracy, *Relative Bias* is the difference between the median *Bias* for affiliated analysts minus the same median for unaffiliated analysts.¹⁸

Finally, we use the three-day market-adjusted cumulative abnormal return (*Three-day CAR*) surrounding the date of an analyst's coverage initiation as a proxy for report informativeness. We use the CRSP value-weighted return as our market return. For this three-day CAR, we measure the abnormal return from one trading day prior to coverage initiation to one trading day after (see Figure 2 for a graphical representation). To measure the informativeness of an analyst's coverage initiation, we require a measure of CAR that provides insight into how much the market moves in the expected direction. To determine the expected direction, we restrict the sample to analyst initiations that contain a recommendation.¹⁹ For a buy recommendation, we expect positive returns, while for a hold or sell recommendation, we expect negative returns. To measure the extent to which the market moves in a direction consistent with the analyst's report, we multiply the sign of the returns on days with sell or hold initiations by negative one. Thus, if the response moves in the direction of the report (positive for a buy recommendation, negative for a hold or sell recommendation), the sign of our CAR measure will be positive (larger means more informative). However, if the response moves in the opposite direction of the report (negative for a buy, positive for a hold or sell), the sign of our CAR measure will be negative (more negative means less informative).

Frequently, more than one analyst will issue a report on the same day. Thus, in addition to the CAR measure described above, which does not account for the number of analyst initiations over the three-day window (and is thus unscaled), we also use a scaled measure of CAR to capture the mean abnormal returns attributable to each analyst report. Our scaled measure equals the unscaled CAR divided by the number of analyst reports released in the three-day window.

¹⁸ We also consider using *Recommendation Optimism* as an alternative measure of analyst bias; however, it is difficult to identify changes in affiliated analyst recommendation optimism surrounding JOBS because even before JOBS, almost all affiliated analysts issued favorable recommendations. For example, in 2010, over 80% of affiliated analysts in our sample issued the most positive recommendations possible on a three-tier rating scale. Therefore, we rely on forecast bias throughout our analyses, which allows us to directly investigate the tradeoff between analyst accuracy and bias.

¹⁹ Because we investigate initiations, we have no benchmark to determine the expected direction of market response to earnings forecast announcements. In our sample, the initiating recommendation is issued simultaneously with the initiating earnings forecast 90% of the time.

We exclude windows with conflicting reports from our CAR tests (i.e., we drop observations that include two or more coverage initiations within the same three-day window that disagree: some reports are buy, and others are hold or sell). We use this sample restriction because when conflicting reports are issued on the same day, it is unclear whether daily stock market returns are an appropriate measure of report informativeness. For example, if we observe a 1% positive return over a window that includes both a buy and a sell, we do not know if the return is comprised of a large 5% response and a -4% response, or if it encompasses a 1% response and 0% response. This limits the value of studying three-day CARs when the objective is to estimate the information content of each report.²⁰ We also exclude three-day CAR windows containing merger, earnings, and management forecast announcements to mitigate concerns that the market response surrounding analyst coverage initiations is driven by analysts who may piggyback on already public news events. To capture differences in report informativeness, *Relative CAR* measures a within-firm difference in *Three-day CARs* between affiliated and unaffiliated analysts.

4.5. Descriptive Statistics

Table 1 presents sample sizes (Panel A) and univariate statistics for the differences in analyst outcome variables across the treatment and control groups, pre- and post-JOBS (Panel B). As shown in Panel A, our sample contains 700 pre-JOBS EGCs and 207 post-JOBS EGCs with analyst coverage, with almost all of these firms having affiliated analyst coverage and approximately 48% having unaffiliated coverage. There are fewer non-EGCs; however, a higher percentage of non-EGCs have unaffiliated analysts. In total, our sample has 5,862 analyst forecasts, approximately 20% of which relate to non-EGCs.

Panel B of Table 1 provides averages of firm-level median analyst outcomes. There is a significant post-JOBS decrease in EGC affiliated forecast accuracy and a corresponding increase in forecast optimism, which is consistent with increased pre-IPO communications strengthening analysts' incentives to bias their forecasts upward. These changes cannot be explained by a time trend since our two control groups (unaffiliated EGCs and affiliated non-EGCs) do not

²⁰ For example, Antero Resources Corp received 10 recommendations in the two-day period of November 4 to November 5, 2013: one was a hold, six were buys, and three were strong buys. Simply attributing the full three-day return to each recommendation could incorporate two types of bias: 1) the nine positive initiations (buy or strong-buy) would each receive the full-amount of the daily return which would over-weight the importance of each recommendation, and 2) the single hold recommendation would be categorized as generating the full amount of the return received over that interval despite it being outweighed by positive recommendations nine to one.

experience a post-JOBS decrease in forecast accuracy. We detect no significant post-JOBS univariate change in three-day CARs or scaled three-day CARs.

Although this descriptive evidence suggests that increased pre-IPO communications permitted by JOBS may have affected analyst behavior, it is important to control for any possible differences in pre- and post-JOBS issuers. For instance, Columns 1 and 2 of Table 2 show that post-JOBS EGC issuers are smaller in terms of pre-IPO revenues, are younger, have higher Tobin's Q ratios, and are less profitable, as measured by return on assets. In contrast, Columns 3 and 4 show few significant differences between the types of non-EGCs going public pre- and post-JOBS.

The evidence in Table 2 is consistent with the existing literature on the JOBS Act (e.g., Barth et al., 2017; Chaplinski et al., 2017; Dambra et al., 2015; Gupta and Israelsen, 2014; Westfall and Omer, 2015) and thus highlights the need for a representative control group to identify the consequences of the JOBS Act's provisions on analyst outcomes. Our matched sample provides assurance that these observable differences between pre- and post-JOBS issuers do not influence our conclusions. Table 3 shows that the averages of all inputs into the matching model are statistically similar before and after the passage of JOBS for both EGCs and non-EGCs. Thus, our matching procedure minimizes the influence of the observable descriptive differences between pre- and post-JOBS issuers displayed in Table 2.

5. Results

5.1. Accuracy

We begin our investigation into the effect of pre-IPO communications on analyst behavior by testing whether EGC affiliated analyst forecast accuracy changed relative to the accuracy of other analysts after JOBS went into effect. Accuracy may improve via our increased accuracy hypothesis (H1) but may decline if pre-IPO communications cause analysts to increase their optimistic forecast bias, as predicted by our increased bias hypothesis (H2).

Figure 3 illustrates how the accuracy of EGC affiliated analysts changes over time relative to non-EGC affiliated and EGC unaffiliated analysts. The annual labels in the figure run from July through June of the labeled year, such that 2012 is entirely in the pre-JOBS period and

2013 is entirely in the post-JOBS period.²¹ The figure reveals no clear trend in analyst behavior in the years leading up to the passage of JOBS. The average accuracy of all three groups (i.e., EGC affiliated analysts, EGC unaffiliated analysts and non-EGC affiliated analysts) is between -0.8 and -0.2 for every year ending between June of 2005 and June of 2012. Following the passage of JOBS, the accuracy of control analysts continues to be within this band (on average it is approximately -0.4 in the post-JOBS period), while the accuracy of treated analysts drops significantly to approximately -1.3 in 2013 and -1.2 in 2014.

The multiple regressions in Table 4 provide further evidence that the relative accuracy of affiliated analysts (compared to unaffiliated analysts) has declined significantly more for EGCs since they have been allowed to conduct pre-IPO communications. In Columns 1, 2, and 3, the dependent variable is the median affiliated accuracy minus the median unaffiliated accuracy within the same firm, as described in Equation (1). Column 1 includes all firms with both affiliated and unaffiliated analysts initiating coverage within the first 180 days following the IPO. Column 2 uses a smaller sample of issuers with affiliated and unaffiliated analysts initiating coverage in the 60 days following the end of the post-IPO quiet period. The coefficients on the Post \times EGC interaction are negative, statistically significant, and indicate an economically large decline in EGC affiliated analyst accuracy. For example, the estimated coefficient of -0.42 in Column 1 is large relative to the pre-JOBS level of inaccuracy for EGC affiliated analysts, representing a 0.55 standard deviation decrease in forecast accuracy. Column 2 shows that the estimated effect is similar limiting our sample to only those analysts who initiated coverage within 60 days following the IPO.

In Column 3 we restrict the sample to firms in our PSM sample. The estimated post-JOBS decrease in EGC affiliated accuracy is larger using the PSM sample than the full sample, indicating that our findings are not an artifact of the type of EGC issuers post-JOBS. We find similar results in Column 4, where the dependent variable is median affiliated analyst accuracy, rather than the difference between median affiliated and median unaffiliated accuracy as shown in Columns 1 through 3. This expands our sample to firms with no unaffiliated coverage and shows that our findings do not rely on the control group of unaffiliated analysts. In the Online Appendix we show that the post-JOBS decline in EGC affiliated analyst accuracy relative to

²¹ Note that we exclude the second and third quarters of 2012 from our sample due to a lack of clarity during that period regarding the analyst provisions of the JOBS Act. Thus, all observations in the 2012 figure entry are conducted between July 1, 2011 and April 4, 2012.

unaffiliated and non-EGC analysts is robust to a forecast-level triple differencing specification, where the explanatory variable of interest is the triple interaction between affiliated analysts, the post-JOBS period, and an EGC issuer. These findings are inconsistent with the increased accuracy hypothesis (H1) and suggest that following the JOBS Act, EGC affiliated analyst research has become less accurate.

5.2. Optimism

Our second hypothesis (H2) predicts that the increased pre-IPO communications afforded by the JOBS Act will result in an increase in the bias of EGC affiliated analyst reports, providing one explanation for why EGC affiliated analysts have become less accurate since the passage of JOBS. We empirically investigate this possibility by comparing EGC affiliated analyst forecast bias with the forecast bias of other analysts surrounding the passage of JOBS.

The results in Table 5 suggest that EGC affiliated forecasts have become more optimistic post-JOBS relative to other analyst forecasts, consistent with the increased bias hypothesis (H2). Columns 1, 2, and 3 present this evidence for our within-firm tests, where the dependent variable is the median affiliated bias minus the median unaffiliated bias within the same firm. In all three columns, the interaction between the post-JOBS period and EGC issuers is positive and significant. Moving from Columns 1 and 2 to Column 3 shows that this finding is robust to our matched sample, and moving from Column 3 to 4 shows that the result is not dependent on the behavior of unaffiliated analysts, as the dependent variable in Column 4 is the firm-level median optimism of affiliated analysts. Notably, the estimated post-JOBS increase in EGC affiliated forecast bias is of similar magnitude as the post-JOBS decrease in accuracy documented in Table 4. In the Online Appendix, we find similar evidence using an analyst forecast-level triple-differencing approach.

Collectively, these results support the increased optimism hypothesis. This analysis provides some of the first evidence that direct interactions between analysts and investors, bankers, and management in the pre-IPO period influence analysts' incentives to optimistically bias their research. This observation speaks to the importance of private communication channels to sell-side equity analysts (Green et al., 2014b; Kirk and Markov, 2016). Prior literature indicates that such communications are important inputs to analysts' research (e.g., Brown et al., 2015) and can improve analyst forecast accuracy (Ke and Yu, 2006; Green et al., 2014a).

However, in the IPO setting, we find evidence that private interactions can exacerbate analysts' conflicts of interests and ultimately lead to increases in analyst forecast optimism.

6. Consequences to Increased Optimism

Given that pre-IPO communications appear to significantly affect analyst behavior, an important question is how the various market participants are affected by the increased analyst optimism brought about by enhanced pre-IPO communications. One challenge with investigating this question is that the IPO setting involves several known market anomalies and capital market inefficiencies (see Ritter, 2011 for a comprehensive survey). Although our empirical design allows us to isolate the market response to analyst reports relative to control groups not directly impacted by JOBS, we cannot completely rule out the possibility that known IPO return anomalies contribute to our findings.

6.1 Announcement Returns

We first examine whether the post-JOBS affiliated analyst coverage initiations, which we find to be more optimistic on average, generate a more muted market reaction. The dependent variable in Columns 1 and 2 of Table 6 is *Relative CAR*, which measures the difference between the median *Three-day CARs* surrounding affiliated coverage initiations and unaffiliated initiations within the same firm. Section 4.4 contains a detailed description of our CAR measure construction; however, it is important to note that we multiply the sign of the returns on days with hold or sell recommendation announcements by negative one to account for the fact that we expect positive returns to accompany buy recommendations and negative returns to accompany hold and sell reports. Thus, *Relative CAR* captures how much more the market moves in the direction of the analyst's recommendation for affiliated initiations, compared with unaffiliated initiations for the same issuer. If EGC affiliated reports have become less informative to market participants upon their release since the passage of JOBS, we expect a negative coefficient on the Post-JOBS \times EGC interaction. Alternatively, we may find the opposite effect if EGC affiliated analyst reports incorporate qualitative information garnered from increased pre-IPO discussions.

The negative interaction coefficients in Table 6 suggests that EGC affiliated analyst reports have become less informative since they have been allowed to engage in more pre-IPO communications. The Online Appendix includes additional forecast-level tests where we replicate our analyses in Table 6 using a triple-differencing approach. For robustness, we run additional forecast level tests where we restrict our sample to buy recommendations and limit our

market responses to two-hour windows using TAQ data. These tests yield similar results to those in Table 6.

Notably, the above analysis assumes that analyst coverage initiations contain some new information. Recent research on sell-side equity analysts and investment consultants casts doubt on this assumption.²² One potential alternative explanation for our announcement return results is that there has been an increase in analysts piggybacking off of news events (e.g., Altinkılıç and Hansen, 2009; Altinkılıç et al., 2013). Importantly, for this to impact our findings, piggybacking would have to occur more for our treated group of EGC affiliated analysts following the JOBS Act. In unreported tests, we follow the Altinkılıç et al. (2013) hand-collection approach and more aggressively exclude potential confounding events. We find no evidence that the likelihood of confounding events differentially changes for EGC affiliated analysts surrounding the passage of JOBS, and our forecast level results remain statistically significant when removing announcements surrounding the main categories of events that Altinkılıç et al. (2013) consider (i.e., earnings, new business, and financing events).

These results extend prior literature documenting a relation between access to management and investment bankers, and the discounting of analyst research. Although Dugar and Nathan (1995), Kadan et al. (2009), and Michaely and Womack (1999) all provide evidence that affiliated analyst reports were discounted by the market before regulations in the early 2000s limited analysts' access to the IPO process, the fact that many regulations were simultaneously passed in the early 2000s makes it hard to identify the economic mechanism driving this result. Using the JOBS Act to identify a change in permissible pre-IPO communications, we find that the market response is muted for EGC affiliated analysts report initiations following JOBS. This could be driven by the market de-biasing analyst reports as argued in the literature cited above or through affiliated analyst initiations representing a liquidity event for institutional investors (Bradley et al., 2003, Ofek and Richardson, 2003). Regardless, to the extent our empirical specification isolates the pre-IPO communications channel, such communications appear to make analyst reports less informative to investors who consume reports at the time of report announcement.

²² See, for example, Barber et al., 2001; Altinkılıç and Hansen, 2009; Li and You, 2015; Hansen, 2015; Altinkılıç et al., 2013, 2016; and Jenkinson et al., 2016.

6.2 Additional Consequences

We now investigate whether there are other meaningful consequences to the increased EGC affiliated analyst optimism brought about by increased pre-IPO communications. The purpose of this analysis is to provide evidence on why pre-IPO communications incentivize analysts to become more optimistic and to get a more complete understanding of the potential costs and benefits of this increased optimism on various market participants.

In Section 3, we introduced several channels capable of explaining the observed effects of pre-IPO communications on analyst behavior that we document. One of those channels is that pre-IPO communications may amplify the effect of optimistic reports on trading revenue, which is an input into analyst compensation. We empirically test this mechanism by using post-IPO share turnover as a proxy for trading revenue. Ellis et al. (2000) and Niehaus and Zhang (2010) find that post-IPO trading is conducted almost exclusively through the IPO underwriter's brokerage house, suggesting that post-IPO share turnover is a reasonable, albeit imperfect, proxy for brokerage trading revenues.

We implement this empirical test in Table 7 by regressing post-IPO share turnover on the triple interaction between affiliated analyst optimism and the post-JOBS \times EGC interaction. Specifically, we estimate the following regression.

$$\begin{aligned} \text{Turnover}_{ij} = & \beta_0 + \beta_1(\text{Affiliated} - \text{Unaffiliated Optimism})_{ij} + \beta_2\text{EGC}_i + \beta_3\text{Post-JOBS}_i \times \\ & (\text{Affiliated} - \text{Unaffiliated Optimism})_{ij} + \beta_4\text{Post-JOBS}_i \times \text{EGC}_i + \beta_5\text{EGC}_i \times (\text{Affiliated} - \\ & \text{Unaffiliated Optimism})_{ij} + \beta_6\text{Post-JOBS}_i \times \text{EGC}_i \times (\text{Affiliated} - \text{Unaffiliated Optimism})_{ij} + \\ & \text{Year-Qtr FEs} + \text{Industry FEs} + \text{Controls} + \varepsilon_{ij}. \end{aligned} \quad (2)$$

The coefficient of interest, β_6 , relates to the triple interaction between the relative optimism of affiliated and unaffiliated analysts, the post-JOBS period, and EGC issuers. To interpret this coefficient, one can think of the above specification as two separate difference-in-differences estimations of how the sensitivity of turnover to affiliated analyst optimism (benchmarked to unaffiliated analyst optimism) changes surrounding the passage of JOBS, where one of the estimations uses a sample EGCs and the other a sample of non-EGCs.²³ The coefficient, β_6 , estimates the difference between these two difference-in-differences estimates. Put differently, β_6 estimates how the sensitivity of post-IPO share turnover to affiliated analyst

²³ This will be a close approximation of Equation (2). However, the estimate in Equation (2) will differ slightly because it restricts the coefficients on control variables to be the same for EGCs and non-EGCs.

optimism differentially changes following JOBS for EGCs relative to non-EGCs. Broader effects, such as a post-JOBS change in all EGC analysts' behavior, are controlled for with other interaction terms. For example, it is reasonable to expect that analyst activity has a smaller effect on share turnover overall since the passage of JOBS because a large portion of volume is now attributable to high frequency traders.

We measure optimism as the difference between the median forecast bias in affiliated and unaffiliated reports initiated during the first 60 days following the IPO. Notably, this measure uses unaffiliated bias as a benchmark, which in our setting of coverage initiations is similar to benchmarking affiliated analysts' optimism to the consensus of other analysts. In an ideal specification, we would measure analyst optimism as of the IPO date. However, since analysts do not issue their earnings forecasts until the quiet period expires, we must proxy for an analyst's pre-IPO optimism with their ex-post forecast optimism, which is typically revealed when their report is released several weeks after the IPO. This proxy is reasonable because reputational considerations (and the frequent requirement that stock recommendations are backed with matching earnings forecasts) make it unlikely that an analyst will meaningfully change their outlook between their pre-IPO communications with investors and their coverage initiation.²⁴

One potential drawback of this proxy is that there may be reverse causality in the cases in which our dependent variable is measured before the explanatory variable of interest. Importantly, our triple difference specification makes it unlikely that such reverse causality will affect our estimate of β_6 in Equation (2), which is our explanatory variable of interest. For example, a simple story whereby post-IPO trading volume affects analyst optimism would not affect this estimate because (1) we benchmark affiliated analyst optimism to the unaffiliated analysts within the same firm, (2) we have a control group of non-EGCs, and (3) we compare the sensitivity of turnover to optimism pre- and post-JOBS. Thus, for reverse causality to affect the estimated coefficient on the Post-JOBS \times EGC \times (Affiliated – Unaffiliated Optimism) triple interaction, post-IPO trading volume would have to be a particularly important input into the relative optimism of affiliated analysts (compared to their unaffiliated counterparts) for post-JOBS EGC analysts compared with other issuers.

²⁴ Prior literature suggests that the improved mapping between analyst recommendations and analysts forecasts was, in part, attributable to the regulatory changes in the early 2000s (Chen and Chen, 2009; Barniv et al., 2009).

In Panel A of Table 7 we present the estimated $\text{Post-JOBS} \times \text{EGC} \times (\text{Affiliated} - \text{Unaffiliated Optimism})$ coefficients from Equation (2), using the natural log of shares traded over various post-IPO horizons as the dependent variable. The significantly positive coefficients on the triple interactions across all three columns of Panel A of Table 7 are consistent with pre-IPO communications increasing the positive relation between analyst optimism and post-IPO trading volume. To interpret the triple interaction coefficients, it is important to note that the standard deviation of optimism (i.e., the relative optimism of affiliated analysts) is 0.86% of price, and the standard deviation of the dependent variables in Columns 1 through 3 are 0.67, 0.75, and 0.88, respectively. Thus, the triple interaction coefficient of 0.579 in Column 2 suggests that, moving from the pre- to post-JOBS periods, a one standard deviation increase in optimism results in a 0.5 unit (or two-thirds standard deviation) larger predicted increase in share turnover for EGCs relative to non-EGCs. The magnitude of this effect is slightly larger in Column 3 and slightly smaller in Column 1. The negative coefficient on the $\text{Post} \times \text{Relative Optimism}$ interaction in all three columns highlights the importance of non-EGCs as a control sample of firms that are unaffected by JOBS but otherwise similarly exposed to any market wide changes that may affect the relation between analyst optimism and trading volume.²⁵

Although data limitations prevent us from directly observing bank-specific trading revenues, these findings provide empirical support for the idea that, as a result of pre-IPO communications through the JOBS Act, analysts rationally increase their optimistic bias because it generates more brokerage trading revenues. This is one important benefit to the post-JOBS increase in EGC affiliated analyst increased analyst optimism, which we argue is due to increased pre-IPO communications. Turnover benefits investment banks and analysts because trading revenues increase their revenues and compensation, respectively. Turnover also benefits investors because it lowers trading costs.²⁶

Pre-IPO analyst communications may also change how analyst optimism affects IPO pricing. Specifically, analyst optimism is likely to be more positively related to the IPO price if analysts are able to converse more with investors prior to the IPO. In Panel B of Table 7, we

²⁵ In unreported tests, we find no evidence that the quantity of analyst coverage becomes a more significant determinant of post-IPO share turnover for EGCs following JOBS. This suggests that the our findings are not simply due to a correlation between analyst optimism and analyst coverage, which may itself result in increases in visibility (e.g., Merton, 1987) rather than optimism (e.g., Jackson, 2005).

²⁶ However, in unreported tests, we find no evidence that trading costs are significantly reduced along other dimensions, such as bid-ask spreads.

investigate this by using two measures of IPO pricing as dependent variables in Equation 2: the price revision, which is the percentage change from the midpoint of the IPO filing range to the offer price, and IPO underpricing, which is the stock return from the offering price to the closing price on the first day of trading. In Column 1, the dependent variable is price revision. The significantly positive $\text{Post-JOBS} \times \text{EGC} \times (\text{Affiliated} - \text{Unaffiliated Optimism})$ triple interaction suggests that the IPO price revision is more sensitive to the optimism of affiliated analysts (relative to unaffiliated analysts) for post-JOBS EGCs, whose affiliated analysts have access to increased pre-IPO communications. Column 2 (3) provide similar evidence using IPO underpricing (the combination of underpricing and price revision) as a proxy for IPO pricing. This is consistent with pre-IPO communications making analyst optimism a more important driver of investor demand.

These findings suggest several additional costs and benefits to the increased optimism of post-JOBS EGC affiliated analysts. A more positive price revision (i.e., higher final offer price) benefits both the investment bank and the issuer. The investment bank receives higher fees from the offering, while the issuer receives a lower cost of equity. Increased underpricing benefits initial investors and is important for investment bank reputation (Beatty and Ritter, 1986). Whether the issuer benefits from increased underpricing is unclear. On the one hand, increased underpricing means that the issuer left more money on the table. On the other, underpricing is related to increased visibility and greater wealth increases for insiders (Loughran and Ritter, 2002). We note, however, that we find no effect of pre-IPO communications on other visibility measures, such as the number of downloads from the SEC's EDGAR database in the days either prior to or following IPO issuance.

7. Additional Analyses

Our main results in Sections 5 and 6 suggest that the JOBS Act has had a significant effect on analyst behavior and market responses to their research. We argue that these changes are a result of the JOBS Act permitting increased pre-IPO communications between analysts and other participants in the IPO process. In this section, we discuss potential alternative explanations and additional empirical specifications.

7.1. Alternative Explanations

7.1.1. Other JOBS Act Consequences

Most of the JOBS Act provisions relating to analysts involve expanding the scope of pre-IPO communications, which is the motivation for our hypotheses. The exception to this is the Act's provision eliminating the 40-day post-IPO quiet period for EGC affiliated analysts (discussed in Appendix A). As illustrated in Figure 4, EGC affiliated analysts have responded to this deregulation by adhering to a 25-day de facto quiet period. We argue that the modest increase in speed with which analysts initiate coverage after the IPO is an unlikely explanation for our primary findings on the quality of analyst research. Although the earlier initiation of EGC affiliated analysts' coverage post-JOBS could, all else equal, partially contribute to their reduced accuracy and less informative reports, it is not obvious how earlier coverage would lead to reports being more optimistic. In fact, an earlier report release may reduce analyst optimism because the JOBS Act does not provide additional legal protection for pre-deal research reports, as was recommended by the IPO Task Force (2011). Moreover, Dubois et al. (2014) find that analysts understand that optimistic reports involve more legal risk.²⁷ Our empirical results show that the increase in forecast optimism is similar in magnitude to the decrease in forecast accuracy, suggesting that our reduced accuracy result is at least in part driven by an increase in optimism.

One timing change that could jointly affect analyst accuracy and optimism would be if the JOBS Act alters analysts' preferences regarding when to initiate coverage relative to future earnings announcement dates. However, in unreported tests we find no significant post-JOBS change in the timing of when EGC affiliated analysts initiate coverage relative to future earnings announcements, compared to either EGC unaffiliated analysts or analysts of non-EGCs. This precludes an explanation whereby EGC affiliated analysts become more optimistic post-JOBS because they initiate coverage farther away from the earnings report and subsequently walk-down their estimates (Richardson et al., 2004; Ke and Yu, 2006).

Finally, Dambra et al. (2015) document a significant increase in the number of firms going public post-JOBS, which is largely concentrated in biotech and pharmaceutical IPOs (see Appendix A for a discussion of non-analyst provisions of the Act). While this increase in IPO

²⁷ This concept plays out similarly at the corporate level, as voluntarily disclosing negative information can reduce a firm's exposure to litigation risk (Skinner, 1994; Field et al., 2005).

volume does not relate directly to analysts, it may create a self-selection problem whereby different types of firms go public post-JOBS. This is the primary motivation for our PSM analysis, which we employ throughout the paper. Nonetheless, in unreported tests we exclude biotech and pharmaceutical IPOs from our main analyses and find qualitatively similar results.

7.1.2. Market Trends Unrelated to the JOBS Act Analyst Provisions

Our empirical design isolates the effect of the JOBS Act on analyst behavior from concurrent market trends. However, one market trend worth specific mention is the weakening of the Global Settlement, which is a 2003 legal settlement against 12 of the largest investment banks with securities analysts. On March 15, 2010, regulators repealed a small set of provisions in the Global Settlement, which overlapped with existing SRO Regulations. Although our empirical framework should control for such an event, we conduct an additional robustness test in which we drop the approximately 30% of our sample associated with Global Settlement banks. Estimates using only non-sanctioned banks produce similar results (untabulated).

7.1.3. The Selection of Optimistic Analysts

Ex ante, it is possible that the JOBS Act facilitates an increase in the optimism of EGC affiliated analysts because EGC issuers may be better able to identify and select optimistic analysts post-JOBS. For this type of analyst selection to become more prevalent for EGCs post-JOBS, it would have to be the case that analysts have attended pitch meetings prior to their investment bank being hired. We view this as an unlikely explanation for our findings for several reasons. First, anecdotal evidence and discussions with industry experts suggest that analysts rarely (if ever) attend pre-hiring pitch meetings. Rather, they get involved later in the process by attending due diligence meetings and communicating with potential investors prior to the IPO. Second, the estimates we observe are similar for Global Settlement sanctioned banks, which are not allowed to attend pitch meetings post-JOBS, and non-sanctioned banks. Third, we find that analysts of non-lead members of the underwriting syndicate exhibit similar increases in post-JOBS optimism as analysts of lead underwriters. Because syndicate formation is mainly the responsibility of the lead underwriter rather than the issuer, it is unlikely that pre-IPO analyst selection drives the change in optimism observed in our empirical findings.

7.1.4. Influence of Optimistic Managers

Finally, it is possible that pre-IPO communications allow optimistic managers of IPO firms to more directly influence analyst behavior, either through intentional manipulation or

merely as an unintentional byproduct of management's optimistic outlook. We believe this is unlikely to be the primary driver of our findings because the discussion and evidence presented thus far suggest that there are several rational reasons for analysts to increase their optimism in response to increased pre-IPO communications. Indeed, it would be fortuitous for analysts to be manipulated into being more optimistic in a situation where this increased optimism appears to benefit the analyst, along with other IPO participants.

To more directly investigate the plausibility of the story that post-JOBS EGC affiliated analysts are optimistic because they are misled by optimistic managers, we partition our triple difference analysis on analyst experience and all-star status in OA3 of the Online Appendix. The logic behind this test is that if increased analyst optimism is not a rational response to increased pre-IPO communications (i.e., if analysts are being misled by management), then we expect the post-JOBS increase in EGC affiliated analyst optimism to be largest for inexperienced and non-all-star analysts. We find that the Post-JOBS \times EGC \times Affiliated triple interaction is more positive and only statistically significant within the sample of experienced, and roughly twice as large for three out of the four columns for all-stars analysts compared to non-all-stars. In unreported quadruple differencing tests we find no significant quadruple interaction between Post-JOBS \times EGC \times Affiliated \times Ln(1+Analyst Experience). The quadruple interaction between Post-JOBS \times EGC \times Affiliated \times All-Star is sometimes statistically positive; however, the statistical significance depends on the empirical specification. These findings are not consistent with a story in which pre-IPO communications allow optimistic managers to mislead naïve analysts.

7.2. Seasoned Firms as an Alternative Control Group

Title I of the JOBS Act applies only to recent IPO issuers. Thus, analysts' coverage of more seasoned firms represents another logical control group for EGC affiliated analyst behavior. Analysis using this additional control group helps alleviate concerns that non-JOBS differences between affiliated EGC analysts and non-EGC or unaffiliated analysts (that coincidentally change surrounding JOBS) contribute to our findings.

In this specification, we benchmark EGC affiliated analyst behavior to the behavior these same analysts exhibit when covering seasoned firms (which have been public for more than two years). Table 8 shows that EGC affiliated analyst research on IPOs becomes less accurate, more optimistic, and more discounted relative to research from the same analysts issued for seasoned

firms, but only in the post-JOBS period. Unreported tests reveal that these findings are also robust to using our PSM sample. These findings provide further evidence that our results are likely attributable to an abnormal post-JOBS shift in EGC affiliated analyst behavior when restrictions on pre-IPO communications were relaxed between analysts and other members of the IPO process.

8. Conclusion

In this paper, we provide new evidence on how analyst behavior changes following the passage of the JOBS Act. The JOBS Act allows certain analysts to conduct pre-IPO communications with investors, bankers, and company management. Relative to the existing literature, the JOBS Act provides a less fettered setting to investigate the effect of pre-IPO communications on analyst behavior. Unlike prior legislative shifts, the JOBS Act changes the extent of analyst pre-IPO communications without a corresponding change in the banned practice of tying compensation to banking revenue, which prior literature identifies as the main determinant of biased reporting from affiliated analysts (Michaely and Womack, 1999; Reingold and Reingold, 2006).

Post-JOBS, initiating reports issued by affected analysts are less accurate, more optimistic, and are accompanied by more muted market reactions compared to other analyst reports. While prior literature demonstrates that private communications improve the quality of analyst research (e.g., Ke and Yu, 2006; Soltes 2014; Brown et al., 2015), our findings suggest that analysts' increased involvement in the IPO process can have the opposite effect. More broadly, our paper extends the recent debate on the value of analyst research for seasoned-firm investors²⁸ to an IPO setting, where analyst coverage plays an important role in mitigating informational asymmetries and improving firm visibility (Mehran and Peristiani, 2010; O'Brien and Tan, 2015). Our findings suggest that pre-IPO communications make it less likely that analysts provide benefits to investors via reduced informational asymmetry.

We conjecture that these unintended consequences of the JOBS Act's provisions that allow for increased pre-IPO communications with analysts are rooted in the effect that deregulation has on analyst incentives. Expanded pre-IPO communications increase the incentives for analysts to issue optimistic reports. Consistent with this, we show that pre-IPO

²⁸ See, for example, Altinkiliç et al., 2013, 2016; Bradley et al., 2014; Li and You, 2015; Hansen, 2015; and Jenkinson et al., 2016.

communications magnify the positive effect of analyst optimism on post-IPO trading volume and IPO pricing. Thus, increased post-IPO trading, which increases brokerage revenues, and higher IPO prices, which increase underwriter fees, represent channels through which analysts rationally become more optimistically biased in response to expanded pre-IPO communications.

Our findings are timely given the recent FINRA proposal to extend several of the JOBS Act provisions regarding securities analysts to debt analysts and non-EGC issuers and ongoing discussions in the European Union about substantial changes to the analyst regulatory environment. Our findings suggest that deregulation designed to further integrate analysts into the IPO process may have adverse unintended consequences, such as overly optimistic and less informative analyst research. One limitation to our analysis is that we exploit a legislative change that simultaneously increases permissible communications with investors, managers, and bankers. An interesting topic for future research would be to separately identify the consequences of different pre-IPO communication partners. It is possible that increasing analysts' exposure to a subset of these IPO participants, but limiting their ability to communicate with others, may lead to higher quality analyst research.

Appendix A: The JOBS Act

In response to concerns that regulatory overreach had caused a decline in the market for initial public offerings (IPOs), Title 1 of the Jumpstart Our Business Startups Act (JOBS Act) was signed into law on April 5, 2012. The cornerstone of the JOBS Act was the creation of an “IPO on-ramp” that was designed to increase IPO activity by streamlining the IPO process for Emerging Growth Companies (EGCs), which are firms with less than \$1 billion in annual revenues. The provisions of Title 1 of the JOBS Act broadly (1) exempt EGCs from certain accounting and disclosure requirements, such as the auditor attestations of internal controls mandated by the Sarbanes-Oxley Act of 2002 (“de-burdening provisions”); (2) allow EGCs to file IPO draft registration statements confidentially and to communicate with qualified institutional investors before publicly filing (“de-risking provisions”); and (3) allow affiliated analysts to issue reports immediately after the IPO (eliminating the prior 40-day quiet period) and allow increased pre-IPO communications with analysts, investors, and investment bankers (“analyst provisions”). The various provisions of Title I of the JOBS Act are summarized below in Tables A1–A3.

While this paper examines the effects of the analyst provisions of the JOBS Act on analyst behavior, several papers have examined the effects of the de-risking and de-burdening provisions of the Act. For example, Dambra et al. (2015) find that, after controlling for market conditions, the JOBS Act resulted in a 25% increase in IPO volume. Dambra et al. find that firms with high proprietary disclosure costs, such as biotechnology and pharmaceutical firms, increase IPO activity most after JOBS. Dambra et al. find that these firms are also more likely to take advantage of the Act’s de-risking provisions, allowing these firms to file their IPOs confidentially while testing the waters.

Barth et al. (2017), Chaplinsky et al. (2017), Gupta and Israelsen (2014), and Westfall and Omer (2015) all examine the costs and benefits of the reduced disclosure of JOBS on both the issuer’s decision to go public and the cost of capital. Chaplinsky et al. find no evidence that the direct costs of issuance (accounting, legal, or underwriting fees) have significantly decreased for firms affected by the Act, while Westfall and Omer (2015) find that the reduced disclosures afforded by JOBS have increased accounting fees. All of the above literature documents increased IPO underpricing for post-JOBS EGCs, especially those taking advantage of the JOBS Act provisions allowing for reduced disclosure. For example, Chaplinsky et al. (2017) find that smaller firms experience less underpricing when they choose to disclose more. Barth et al.

(2017) provide evidence that these changes in underpricing proxy for a more general increase in information uncertainty that extends beyond the IPO date as firms taking advantage of the provisions allowing for reduced disclosure experience increased post-IPO return volatility. And Gupta and Israelsen (2014) find an accompanying decline in post-IPO liquidity and probability of informed trading following the IPO that indicates a rise in asymmetric information costs.

Finally, Agarwal et al. (2016) use the JOBS Act to examine the effects of firms' choices regarding the optimal mix of hard and soft information, and the SEC's comment-letter response to change in equilibrium disclosure. In response to the reduction in mandatory disclosure of hard information afforded by JOBS, they find that firms' disclosure of soft information changes for EGCs relative to a matched sample prior to the Act. They also find that since JOBS went into effect, the SEC has increased the amount of information it discloses in its comment letters for prospectuses by EGCs, which now generate a market reaction when made public. Overall, Agarwal et al. argue that their results document a shift in the optimal mix of hard and soft information for IPO firms.

Table A1: Summary of Title 1 JOBS Act Provisions: De-Burdening and De-Risking Provisions

Before JOBS	Since JOBS
DE-BURDENING PROVISIONS:	
Reduced Financial Statement Disclosure:	
Three years of audited financial statements, five years of selected financial data.	Two years of audited financial statements and selected financial data.
Reduced Compensation Disclosure:	
Issuers must provide a Compensation Discussion and Analysis (CD&A) section and compensation disclosure for five named executive officers.	No CD&A required for EGC issuers. Only Summary Compensation Table for three (not five) executives must be provided.
Auditor Attestation Opt-Out:	
Issuers must provide auditor attestation of internal controls (as required by Section 404(b) of SOX).	EGCs need not provide auditor attestation of internal controls.
Future Accounting Standards Opt-Out:	
Issuers must comply with any new or revised FASB accounting standards.	EGCs not required to comply with any new or revised FASB accounting standards unless these standards also apply to private companies.
PCAOB Rulings and Executive Compensation Opt-Outs:	
Issuers must comply with future rules implemented by the PCAOB.	EGCs may opt-out of future rules implemented by the PCAOB and are not subject to Say-on-Pay shareholder advisory votes required by Dodd-Frank.
DE-RISKING PROVISIONS:	
Confidential Filing:	
Issuers were required to publicly file their IPO registration statement.	EGCs may confidentially submit a draft of the registration statement. If the firm decides to go forward with the IPO, the registration statement must be filed 21 days before the road show.
Testing the Waters:	
Issuers and underwriters were prohibited from communicating with potential investors prior to issuing an IPO registration statement.	EGCs may engage in oral or written communications with qualified investors prior to issuing an IPO registration statement.

Derived from JOBS Act Quick Start, Morrison/Foerster; JOBS Act, Goodwin/Procter; The JOBS Act: 18 months later, EY November 2013

Table A2: Summary of Title 1 JOBS Act Provisions: Analyst Provisions

Before JOBS	Since JOBS
Research Reports and Public Appearances by Research Analysts:	
Research reports by offering participants in connection with the offering may be considered prospectuses and offers for purposes of Section 12 liability and Section 5 “gun jumping” restrictions of the Securities Act of 1933.	Research reports by offering participants in connection with offerings for common equity securities are not considered prospectuses or offers for purposes of Section 12 liability and Section 5 “gun jumping” restrictions of the Securities Act of 1933.
Research reports and public appearances by managers and co-managers are prohibited by FINRA rules for up to 40 days after the date of the offering and within 15 days before or after the expiration of lock-up provisions, subject to certain exceptions.	FINRA rules prohibiting publication of research reports and public appearances do not apply to those by offering participants following the IPO or prior to the expiration of lock-up provisions.
Research Reports and Public Appearances by Research Analysts:	
FINRA rules include extensive restrictions on the ability of research analysts and investment bankers to interact.	SEC and FINRA rules may not restrict investment bankers from arranging for communications between research analysts and potential investors or research analysts from participating in communications with management in the presence of investment bankers; rules are otherwise unaffected.
Global Settlement further restricts the ability of research analysts and investment bankers to interact at firms subject to the settlement.	Global Settlement is unaffected.

Derived from Goodwin Procter LLP publication: “JOBS ACT: A New IPO Playing Field for Emerging Growth Companies”
http://www.goodwinprocter.com/Publications/Newsletters/Client-Alert/2012/~/_/media/E7463DA9940544CF83D8715CC1E67A98.pdf

Table A3: Roles of Sell-Side Research Analysts Pre- and Post-JOBS

<i>May research personnel...</i>	Pre-JOBS Act	Post-JOBS Act	
	All Issuers	EGC	Non-EGC
Publish research reports concerning the securities of an issuer immediately following its IPO or expiration of any lock-up agreement?	Prohibited	<i>Permitted</i>	Prohibited
Publish research reports concerning issuers that are the subject of <i>any</i> public offering of common equity securities (even if the firm is participating in the offering)?	Prohibited	<i>Permitted</i>	Prohibited
Participate in meetings with representatives of an issuer, attended by investment banking personnel?	Prohibited	<i>Permitted</i>	Prohibited
Contact potential investors in an issuer's IPO?	Prohibited	<i>Permitted</i>	Prohibited
Make public appearances concerning the securities of an issuer?	Prohibited	<i>Permitted</i>	Prohibited
Solicit business for investment banking personnel?	Prohibited	Prohibited	Prohibited
Engage in communications with potential investors in the presence of investment banking personnel?	Prohibited	Prohibited	Prohibited
Share price targets and ratings with an issuer prior to the launch of a deal?	Prohibited	Prohibited	Prohibited
Be compensated based on investment banking revenue?	Prohibited	Prohibited	Prohibited

Derived from Morrison & Foerster LLP publication: "Frequently Asked Questions About Separation of Research and Banking"
<http://media.mofo.com/files/Uploads/Images/Frequently-Asked-Questions-about-Separation-of-Research-and-Investment-Banking.pdf>

Appendix B
Data Definitions

Variable Name	Variable Definition (source in parentheses)
Issuer Characteristics	
Assets	Total assets in March 2012 dollars (nominal) from the most recent fiscal year prior to the IPO (Collected from Compustat if available, and the IPO prospectus if not available).
Revenue	Total revenue in March 2012 dollars (nominal) from the most recent fiscal year prior to the IPO (Compustat, prospectus).
Tobin's Q	Measured as assets plus the market value of equity minus book value of equity minus IPO proceeds, all scaled by assets. Market value of equity is measured as total shares outstanding times the offer price. Book value of equity is measured as of the most recent fiscal year prior to IPO (Compustat, SDC).
Age	The number of years between the founding date and the offer date, where founding date is taken from Jay Ritter's webpage.
Leverage	The ratio of total debt (long-term debt plus debt in current liabilities) to total assets, both from the most recent fiscal year prior to the IPO (Compustat, prospectus).
Return on Assets	The ratio of net income (item 172 from Compustat) to total assets, both from the most recent fiscal year prior to the IPO (Compustat, prospectus).
Operating at Loss	An indicator variable equal to one when the firm has net income less than zero, and zero otherwise (Compustat, prospectus).
High-Tech	An indicator variable that equals one if the firm is in a high technology industry based on the Loughran and Ritter (2004) industry classification, and zero otherwise.
PE-Backed	An indicator variable equal to one if the firm is marked by SDC as having private equity backing leading up to its IPO, and zero otherwise.
VC-Backed	An indicator variable equal to one if the firm is marked by SDC as having venture capital backing leading up to its IPO, and zero otherwise.
Proceeds	IPO proceeds in March 2012 (nominal), measured as the total shares offered times the offer price (SDC).
Underpricing	The price at the close of the first day of trading divided by the offer price, minus one (CRSP).
Pre-IPO Market Return	The buy-and-hold compound return on the CRSP value-weighted market index over the 63 trading days (three months) ending five days before the offer date (CRSP).
Number of Days Before Report	The number of calendar days between the offer date and the date of the analyst's initial report announcement, always counting the first trading date as day 1.
Total Number of Analysts	The total number of analysts issuing a recommendation within the first 180 days following the offer date (I/B/E/S).
Number Analysts Affiliated	The number of analysts issuing a recommendation within the first 180 days that are employed by one of the banks in the underwriting syndicate provided by SDC.
Number Analysts Unaffiliated	The number of analysts issuing a recommendation within the first 180 days that are not employed by any of the banks in the underwriting syndicate provided by SDC.
Percent Analysts Affiliated	Number of Analysts Affiliated divided by Total Number of Analysts.
Managers	The number of syndicate members underwriting the IPO provided by SDC.
Broker Characteristics	
Brokerage Size	The number of firms covered (with a recommendation or forecast, respectively) by all analysts employed by the broker in the same year as the given recommendation or forecast (I/B/E/S).

(continued)

Appendix B (continued)

Variable Name	Variable Definition (source in parentheses)
Analyst Characteristics	
Analyst Experience	The number of years the analyst has been issuing recommendations in the I/B/E/S detail recommendation or forecast files, respectively (I/B/E/S).
Analyst Coverage	The number of firms for which the analyst issues a recommendation or forecast, respectively, in the same year as the studied recommendation or forecast (I/B/E/S).
Days from Report to Earnings	The number of days between the forecast period end date and the forecast announcement date plus 45 days (to ensure that the forecast horizon is non-negative) (I/B/E/S).
Historical Rec Informativeness	The average three-day cumulative abnormal announcement return for all previous unconflicted recommendations issued by the analyst for U.S. firms, as of the most recent date with an unconflicted report (I/B/E/S).
Historical Forecast Optimism	The average Forecast Optimism for all EPS quarterly forecasts preceding the given forecast issued by the analyst for U.S. firms. For the historical forecasts, we retain only the first forecast issued within 180 days of the quarter-end date (I/B/E/S).
Historical Forecast Accuracy	The average Forecast Accuracy for all EPS quarterly forecasts preceding the given forecast issued by the analyst for U.S. firms. For the historical forecasts, we retain only the first forecast issued within 180 days of the quarter-end date (I/B/E/S).
Outcome Variables	
Informativeness (Scaled Three-Day CAR)	The daily abnormal return for the firm (i.e., net of the return on the CRSP value-weighted market index) summed over the interval (-1,+1), where day 0 is the recommendation announcement date from I/B/E/S, or the closest preceding trading day if the announcement occurs on a non-trading day. The CAR is then divided by the number of recommendations issued in the three-day window. Announcement windows in which there are conflicting recommendations (at least one positive and one negative), and windows that coincide with earnings, management guidance, or merger announcements from COMPUSTAT, I/B/E/S, or SDC are excluded. Also, the CARs for negative recommendations are flipped in sign to make interpretation consistent with positive recommendations (i.e., positive returns represent market reactions consistent with expectations, and negative returns represent market reactions inconsistent with expectations). Initiations of buy and strong-buy are considered positive, and initiations of hold, underperform, and sell are considered negative.
Informativeness (Unscaled Three-Day CAR)	The definition is identical to Scaled Three-Day CAR, except that the cumulative market-adjusted issuer stock returns surrounding a recommendation announcement are not divided by the number of analysts initiating coverage in the three-day window.
Bias	Calculated as $\frac{\text{Forecast}_{it} - \text{Actual}_i}{\text{Price}_{i,t-1}} \times 100$, where Forecast is the analyst's initiating quarterly EPS forecast i on day t and Actual is the I/B/E/S Actual EPS for the quarter-end. Price is the issuer's stock price on the most recent trading day prior to the analyst forecast date. For quarters with multiple forecasts, we retain only the first forecast issued within 180 days of the period-end date for the initiating quarterly forecast closest to the IPO (I/B/E/S).
Optimistic Bias Indicator	An indicator variable equaling one if Forecast Bias (described above) is positive, and zero otherwise.
Optimistic Component of Bias	The variable takes the value of Forecast Bias (described above), when Forecast bias is greater than zero, and zero otherwise. In other words, this variable is the maximum of zero, and the value of Forecast bias for a given analyst's initiating forecast.
Accuracy	Forecast Accuracy is calculated as $-1 \times \text{Forecast Optimism} $. For quarters with multiple forecasts, we retain only the first forecast issued within 180 days of the period-end date for the initiating quarterly forecast closest to the IPO (I/B/E/S).
Other Variables	
Post-JOBS	An indicator variable equaling one if the offer date occurs after April 5, 2012, and zero otherwise.
EGC	An indicator variable equaling one if the analyst issuing the recommendation or forecast is covering an issuer with less than \$1 billion in pre-IPO annual revenue.

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Figure 1

Typical Timeline for Analyst Reports Pre- and Post-JOBS

This figure presents a typical timeline for EGCs and non-EGCs, pre and post-JOBS, from the initial IPO filing date to 180 days after the issue date (an EGC is defined as an issuer with less than \$1 billion in revenue in the fiscal year preceding the IPO). Although unaffiliated analysts may issue reports at any time after the IPO, affiliated analysts cannot issue reports during the quiet period. Before the JOBS Act, the quiet period ended 40 days after the IPO; since JOBS, for non-EGCs the quiet period continues to be 40 days, while for EGCs, there has been a “de facto” 25-day quiet period during which affiliated analysts do not issue reports. The JOBS Act allows increased pre-IPO communications for EGC affiliated analysts (since April 5, 2012).

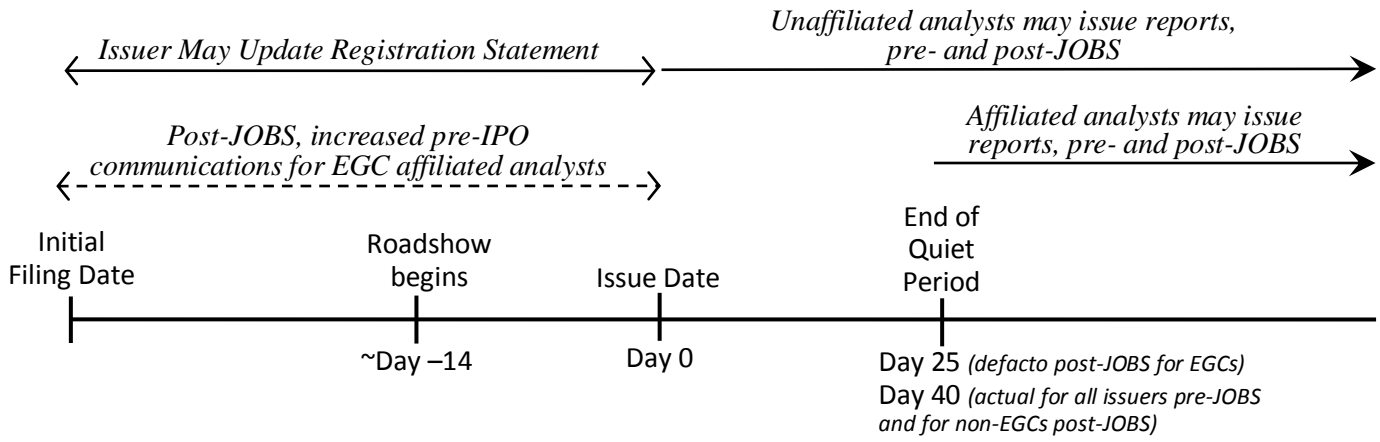


Figure 2

Examples of Measurement of Cumulative Abnormal Returns (CARs) for Hypothetical Analyst Report Releases

This figure presents a graphical depiction of how cumulative abnormal returns (CARs) for analyst reports are measured for three hypothetical analyst report announcements: on days 15, 45, and 54. The days marked in the figure (other than the issue date and the quiet period ending date) represent days over which returns are cumulated. Although unaffiliated analysts may issue reports at any time after the IPO, affiliated analysts issue reports only after the end of the quiet period (which was 40 days after the IPO before JOBS, and has been a de facto quiet period of 25 days after the IPO post-JOBS for EGCs). Three-day CARs for analyst report announcements are measured from the trading day preceding announcement to the trading day following announcement (-1,+1).

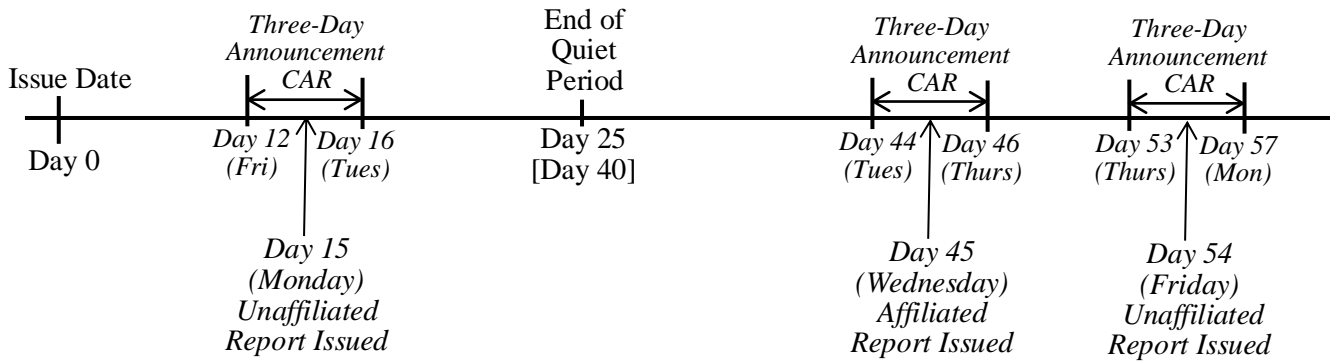


Figure 3

Average Analyst Forecast Accuracy: Difference between EGCs and Non-EGCs

This figure plots the average forecast accuracy in each year. Forecast accuracy is calculated as $-1 \times \left| \frac{\text{Forecast}_{it} - \text{Actual}_i}{\text{Price}_{i,t-1}} \times 100 \right|$. The solid line plots the average accuracy of EGC affiliated analysts, while the short- and long-dashed lines plot the same information for EGC unaffiliated and non-EGC affiliated analysts, respectively. Each year is measured as July 1 of the previous year to June 30 of the year marked on the horizontal axis. An EGC is defined as an issuer with less than \$1 billion in revenue in the fiscal year preceding the IPO. The shaded region, composed of IPOs between July 2012 and June 2014, marks results for firms issuing in the post-JOBS treatment period.

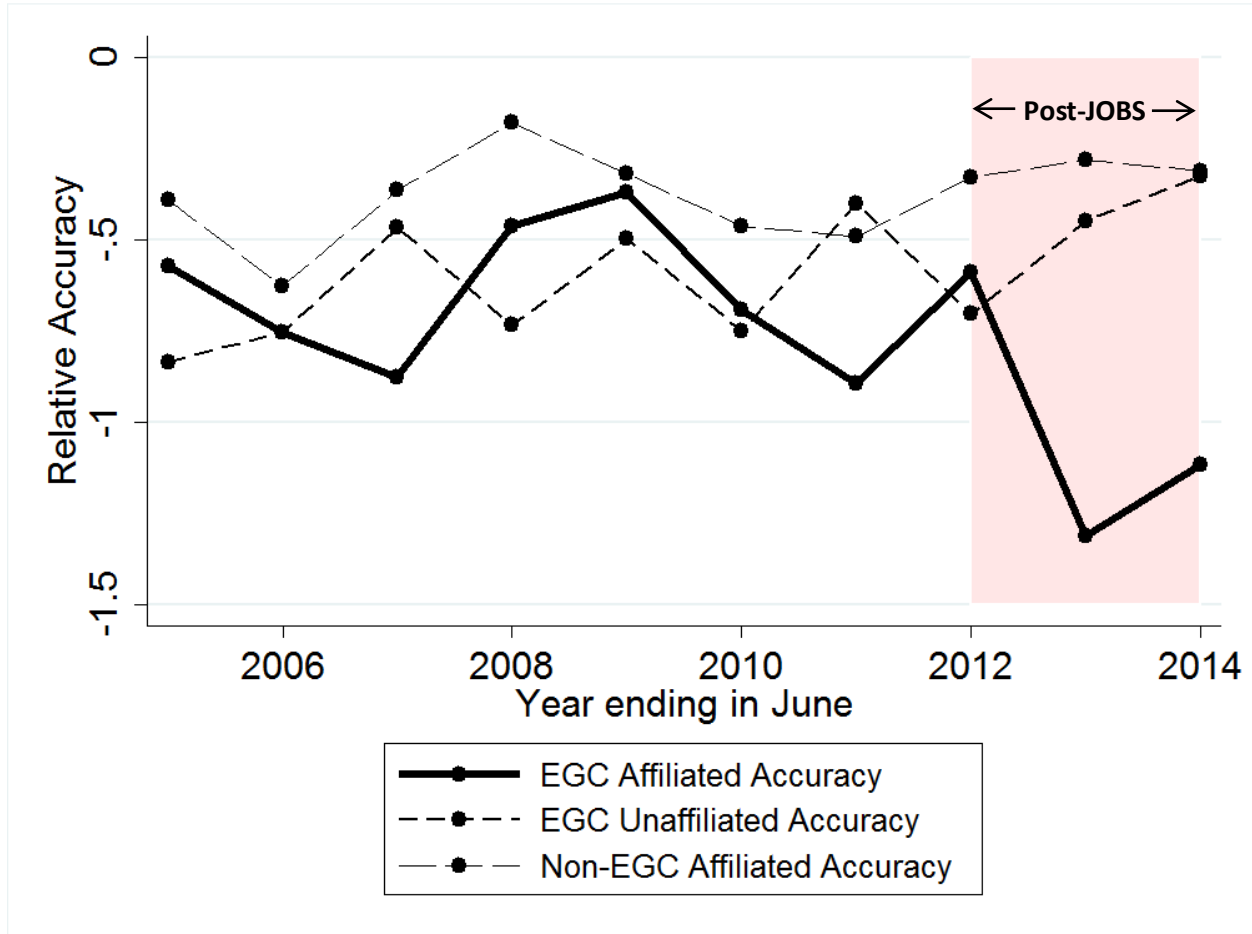


Figure 4

Histogram of Affiliated Analyst Coverage Initiation Post-JOBS

This figure presents the number of days from the IPO offer date to coverage initiation for affiliated analysts covering EGCs during the pre and post-JOBS periods. Pre-JOBS represents all deals occurring between January 1, 2004 and April 5, 2012, while post-JOBS represents all deals occurring between November 11, 2012 and June 30, 2014. The horizontal axis plots the number of days following the offer date, and the vertical axis plots the percentage of affiliated analysts who initiate coverage within the first 180 days. An analyst is classified as affiliated if he or she is employed by a bank in the underwriting syndicate.

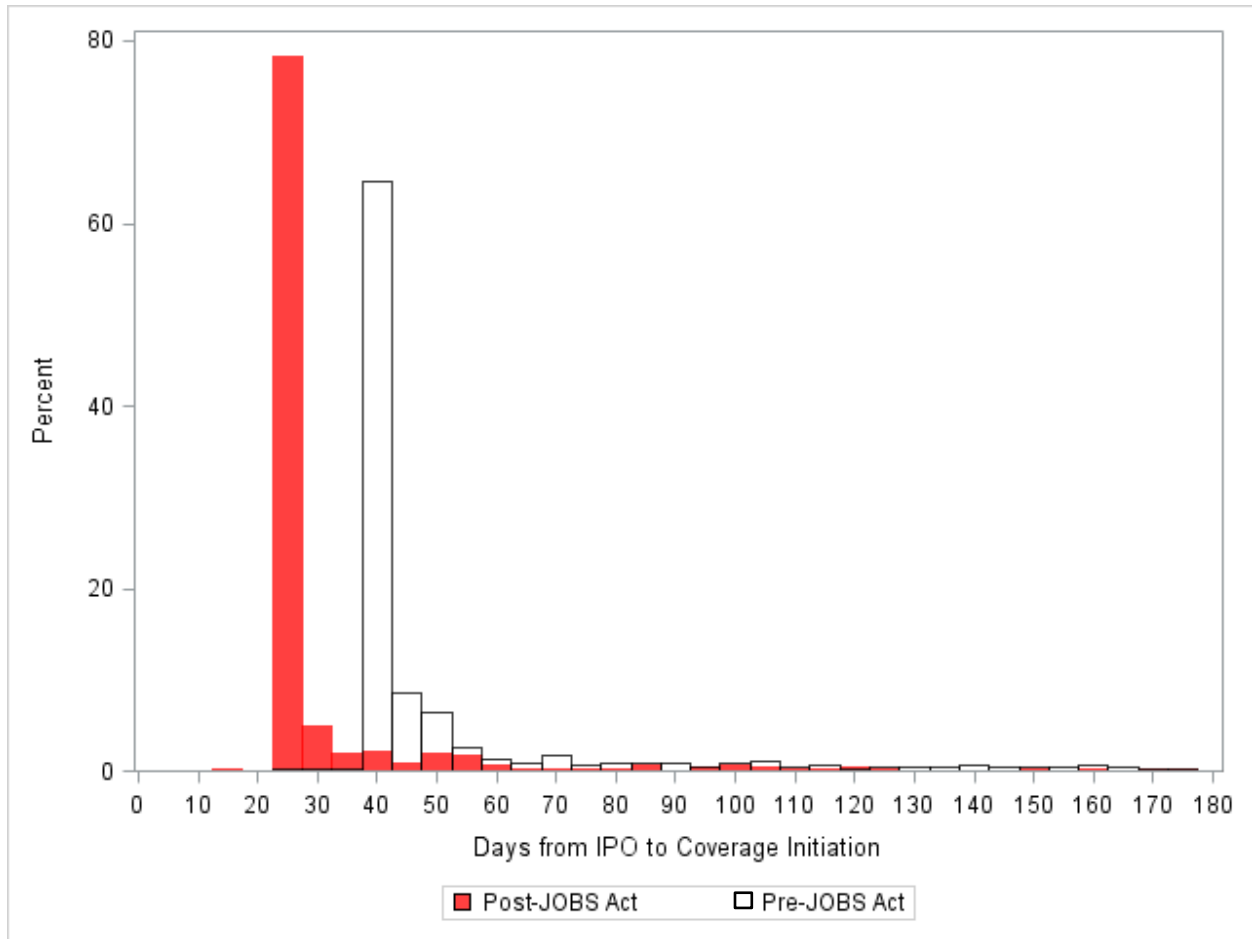


Table 1

Firm-Level Analyst Outcomes

This table presents descriptive statistics partitioned by EGC and post-JOBS status for analysts initiating coverage in the first 180 days following the IPO. EGCs are issuers with less than \$1 billion in revenue in the fiscal year preceding the IPO. Pre-JOBS represents all deals occurring between January 1, 2004 and April 5, 2012, and post-JOBS represents all deals occurring between November 11, 2012 and June 30, 2014. Panel A presents sample sizes, and Panel B presents analyst characteristics. In Panel B, all statistics are computed by first taking the median analyst characteristics within a firm and then averaging across firms. An analyst is classified as affiliated if he or she is employed by a bank in the underwriting syndicate. All remaining accuracy, optimism, informativeness, and timing measures are explained in Appendix B. ***, **, * signify significant difference in means between the pre- and post-JOBS period at the 1%, 5%, and 10% level, respectively.

Panel A: Sample Size Breakdown

	EGCs		Non-EGCs	
	Pre-JOBS	Post-JOBS	Pre-JOBS	Post-JOBS
Number of Firms with Forecasts	700	207	91	37
Number of Firms with Affiliated Forecasts	696	207	91	37
Number of Firms with Unaffiliated Forecasts	338	78	66	29
Number of Firms with Affiliated and Unaffiliated Forecasts	334	78	66	29
Number of Affiliated Forecasts	2,765	931	563	356
Number of Unaffiliated Forecasts	777	208	185	77

Panel B: Averages of Firm-Level and Deal Characteristics

Characteristic	EGCs		Non-EGCs	
	Pre-JOBS	Post-JOBS	Pre-JOBS	Post-JOBS
Accuracy				
Forecast Accuracy (Affiliated)	-0.79	-1.52***	-0.69	-0.42**
Forecast Accuracy (Unaffiliated)	-0.42	-0.44	-0.64	-0.34**
Optimism				
Forecast Bias (Affiliated)	0.24	0.69***	-0.11	-0.23
Forecast Bias (Unaffiliated)	-0.05	-0.01	-0.15	0.07
Informativeness				
Three-day CAR (Affiliated)	2.70	2.50	1.27	1.59
Three-day CAR (Unaffiliated)	1.55	2.56	0.57	1.20
Scaled Three-day CAR (Affiliated)	1.41	0.99	0.97	1.08
Scaled Three-day CAR (Unaffiliated)	1.45	2.47	0.40	1.22
Analyst Characteristics				
Analyst Experience (Years)	4.72	5.41***	6.72	10.27***
Analyst Coverage	5.26	5.67*	8.69	8.86
Brokerage Size	4.08	4.75***	85.30	76.27*
Timing				
Number of Days Before Report (Affiliated)	49.48	32.08***	49.29	44.70**
Number of Days Before Report (Unaffiliated)	97.13	92.24	79.25	80.33

Table 2

EGC Issuer and Deal Characteristics

This table presents sample averages of firm-level issuer and deal characteristics for EGC issuers (i.e., firms with less than \$1 billion in revenue for the fiscal year prior to the IPO) in Columns 1 and 2, and non-EGCs in Columns 3 and 4. Issuer and Offer characteristics are sample averages as of the date of the prospectus (Underpricing includes the first day of trading), while Coverage characteristics include information as of the 180th day following the IPO. Pre-JOBS represents all deals occurring between January 1, 2004 and April 5, 2012. Post-JOBS represents all deals occurring between November 11, 2012 and June 30, 2014. All variables are defined in Appendix B. ***, **, * signify significant differences in means (using a *t*-test) between the pre- and post-JOBS period at the 1%, 5%, and 10% level, respectively.

Characteristic	Means for:			
	EGCs		Non-EGCs	
	Pre-JOBS	Post-JOBS	Pre-JOBS	Post-JOBS
Issuer Characteristics:				
Total Assets (\$ million)	360.53	396.83	8095.38	15526.57
Revenue (\$ million)	188.13	142.20**	4910.94	4110.02
Tobin's Q	7.10	14.59***	1.75	1.62
Firm Age	18.56	14.23**	49.45	56.05
Leverage	0.44	0.55	0.51	0.59
Return on Assets	-27.78	-63.71***	2.81	2.06
Operating at Loss	0.49	0.67***	0.27	0.24
High-Tech	0.35	0.27**	0.08	0.08
Offer Characteristics:				
PE-Backed	0.32	0.21***	0.66	0.89***
VC-Backed	0.49	0.62***	0.04	0.00
Offer Proceeds (\$ million)	163.21	146.37	865.76	711.19
Underpricing (%)	13.30	21.14***	6.21	13.76**
Pre-IPO Market Return (%)	4.27	4.91	5.95	5.80
Coverage				
Total Number Underwriters	4.63	5.33***	9.48	13.38***
Total Number Analysts	5.11	5.55*	8.74	11.86***
Number Affiliated Analysts	3.96	4.66***	6.64	10.14***
Number Unaffiliated Analysts	1.14	0.89	2.10	1.73
% Analysts Affiliated	0.83	0.91***	0.81	0.88**
Number of Observations	700	207	91	37

Table 3

Propensity Score Matched Sample Descriptive Statistics

This table presents descriptive statistics for the propensity score matched sample of EGC issuers (i.e., firms with less than \$1 billion in revenue for the fiscal year prior to the IPO) in Columns 1 and 2, and non-EGCs in Columns 3 and 4. The sample is constructed by estimating a logit propensity score model that predicts the probability of issuing in the post-JOBS period as a function of Ln(assets), Ln(Revenue), Ln(Tobin's Q), Ln(Age), Leverage, ROA, Operating at Loss, High-Tech Indicator, Ln(Proceeds), and indicators for PE and VC-backed. Each post-JOBS EGC (non-EGC) issuer is matched, without replacement, to a single EGC (non-EGC) control firm issuing in the pre-period, in the same industry using the lowest absolute difference in propensity scores. The differences in means are presented for the firm-characteristic variables used in the propensity score model. Pre-JOBS represents all deals occurring between January 1, 2004 and April 5, 2012. Post-JOBS represents all deals occurring between November 11, 2012 and June 30, 2014. See Appendix B for definitions of the remaining variables. Differences in means are based on a *t*-test. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Propensity Score Matching Variables:	Means for:			
	Pre-JOBS	Post-JOBS	Pre-JOBS	Post-JOBS
Ln(Assets)	18.25	18.26	22.08	22.27
Ln(Revenue)	15.19	15.35	21.83	21.83
Ln(Tobin's Q)	1.93	2.00	0.91	0.94
Ln(Age)	2.41	2.47	3.83	3.83
Leverage	0.60	0.55	0.50	0.59
Return on Assets	-54.85	-63.71	2.54	2.06
Operating at Loss	0.69	0.67	0.14	0.24
High-Tech	0.28	0.27	0.08	0.08
Ln(Proceeds)	18.31	18.40	20.04	20.08
PE-Backed	0.20	0.21	0.81	0.89
VC-Backed	0.66	0.62	0.00	0.00
Number of Observations	207	207	37	37

Table 4

Firm-Level Forecast Accuracy

This table presents ordinary least squares (OLS) regressions in which the dependent variable in Columns 1, 2, and 3 is *Relative Accuracy*, defined as the within-firm difference between affiliated and unaffiliated analysts. This difference is the median forecast accuracy for affiliated analysts (calculated as $-1 \times \left| \frac{\text{Forecast}_{it} - \text{Actual}_i}{\text{Price}_{i,t-1}} \times 100 \right|$) minus this same median for unaffiliated analysts. This sample is restricted to all EGC and non-EGC issuers that have at least one affiliated and one unaffiliated forecast. In Columns 1, 3, and 4 we require these reports to be released in the 180 days following the IPO. In Column 2, we require these reports to be released in the 60 days after the end of the post-IPO quiet period. In Column 4, the dependent variable is firm-median forecast accuracy, only including affiliated analysts. Column 2 and 3 rely on a propensity score matched set of pairs to each of the Post-JOBS EGCs and non-EGCs. Column 2 matches 78 Post-JOBS EGCs with 78 Pre-JOBS issuers that meet the \$1 billion revenue cutoff and matches 29 Post-JOBS non-EGCs to 29 Pre-JOBS non-EGCs. The number of matches for Column 3 are 207 EGCs and 37 Non-EGCs. The variables used in the PSM estimation Column are Ln(assets), Ln(Revenue), Ln(Q), Ln(Age), Leverage, ROA, Operating at Loss, High-Tech Indicator, Ln(Proceeds), and indicators for PE and VC-backed. All remaining variables are defined in Appendix B. PSM refers to the propensity score matched sample. All regressions include Fama-French 12 industry fixed effects and issue-year fixed effects. Note that we include but do not tabulate the post-JOBS indicator coefficient, as the year fixed effects make it difficult to interpret. *t*-statistics using White (1980) robust standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	<i>Relative Accuracy:</i> Affiliated – Unaffiliated			<i>Accuracy:</i> Affiliated Only
	Full Sample (1)	60-day Sample (2)	PSM (3)	PSM (4)
Post × EGC	-0.422** (-2.25)	-0.351** (-2.31)	-0.541*** (-3.02)	-0.719*** (-2.95)
EGC	0.175 (1.50)	0.049 (0.43)	0.215 (1.24)	0.764*** (3.31)
Firm Characteristics:				
High-Tech	-0.129 (-1.10)	-0.192** (-2.52)	0.066 (0.40)	0.120 (0.67)
PE-Backed	0.067 (1.05)	0.102* (1.74)	0.072 (0.66)	-0.142 (-0.79)
VC-Backed	-0.043 (-0.48)	0.031 (0.46)	-0.342* (-1.81)	-0.320 (-1.49)
Ln(Total Assets)	-0.082 (-1.47)	-0.013 (-0.37)	-0.092 (-1.38)	-0.109 (-1.12)
Ln(Revenue)	0.129** (2.39)	0.035 (0.88)	0.069 (1.10)	0.214*** (3.60)
Leverage	-0.117 (-1.20)	-0.062* (-1.78)	-0.090 (-0.79)	-0.057 (-0.45)
Return on Assets	-0.065 (-1.00)	-0.061 (-1.45)	-0.037 (-0.55)	0.012 (0.14)
Operating at Loss	-0.140** (-2.33)	-0.035 (-0.69)	-0.199** (-2.11)	-0.316** (-2.30)
Ln(Tobin's Q)	0.029 (0.33)	0.083 (1.35)	0.242 (1.52)	-0.122 (-0.66)
Ln(Firm Age)	-0.018 (-0.42)	0.046 (1.22)	-0.029 (-0.51)	-0.249*** (-3.00)

(continued)

Table 4 (continued)

	<i>Relative Accuracy:</i> Affiliated – Unaffiliated			<i>Accuracy:</i> Affiliated Only
	Full Sample (1)	60-day Sample (2)	PSM (3)	PSM Affiliated (4)
<i>Analyst/Brokerage Characteristics:</i>				
Number Days Before Report	-0.003 (-1.28)	-0.005* (-1.73)	-0.006** (-2.01)	0.005 (1.57)
Number Affiliated Analysts	-0.005 (-0.38)	-0.020* (-1.87)	-0.014 (-0.78)	0.034 (1.21)
Number Unaffiliated Analysts	-0.006 (-0.35)	0.005 (0.38)	0.037 (1.29)	-0.035 (-1.16)
Percent Affiliated Analysts	-0.332 (-0.83)	0.166 (0.55)	0.787 (1.21)	-0.776 (-1.55)
Ln[Analyst Experience (Years)]	-0.171 (-1.47)	-0.083 (-0.81)	-0.179 (-1.10)	-0.167 (-1.14)
Ln(Analyst Coverage)	0.148 (0.92)	0.140 (0.78)	-0.069 (-0.26)	-0.065 (-0.25)
Days from Report to Earnings	-0.004*** (-3.21)	-0.002* (-1.77)	-0.006** (-2.42)	-0.012*** (-5.12)
Historical Average Optimism	0.141 (0.52)	-0.188 (-1.18)	0.310 (0.85)	0.684** (2.42)
Historical Average Accuracy	0.277* (1.66)	-0.007 (-0.08)	0.049 (0.20)	0.724*** (3.59)
Ln(Brokerage Size (Analysts))	0.006 (0.08)	-0.090 (-1.53)	0.220* (1.69)	0.086 (0.70)
<i>IPO Characteristics:</i>				
Ln(Proceeds)	0.066 (1.12)	0.032 (0.68)	0.109 (1.38)	0.203 (1.47)
Underpricing	0.002 (0.01)	0.096 (0.70)	-0.012 (-0.04)	0.478* (1.82)
<i>Market Conditions:</i>				
Pre-IPO Market Return	0.008 (1.06)	0.002 (0.57)	0.002 (0.27)	0.011 (0.80)
Adjusted R-squared	0.078	0.024	0.092	0.344
Number of Observations	506	363	214	488

Table 5

Firm-Level Forecast Bias

This table presents OLS regressions in which the dependent variable in Columns 1, 2 and 3 is *Relative Bias*, defined as the within-firm difference between affiliated and unaffiliated analysts. This difference is the median forecast bias for affiliated analysts (calculated as $\frac{\text{Forecast}_{it} - \text{Actual}_i}{\text{Price}_{i,t-1}} \times 100$) minus this same median for unaffiliated analysts. In Columns 1, 3, and 4 we require these reports to be released in the 180 days following the IPO. In Column 2, we require these reports to be released in the 60 days after the end of the post-IPO quiet period. In Column 4, the dependent variable is firm-median forecast bias, only including affiliated analysts. The sample is restricted to all EGC and non-EGC issuers that have at least one affiliated and one unaffiliated forecast (or just at least one affiliated forecast in Column 3). The PSM model in Column 2 matches 78 EGCs and 29 non-EGCs; the PSM model in Column 3 matches 207 EGCs and 37 Non-EGCs. Firm, IPO, and Market Controls refer to the respective control variables under these headings in Table 4, and all variables are defined in Appendix B. PSM refers to the propensity score matched sample. All regressions include Fama-French 12 industry fixed effects and issue-year fixed effects. Note that we include but do not tabulate the post-JOBS indicator coefficient, as the year fixed effects make it difficult to interpret. *t*-statistics using White (1980) robust standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	<i>Relative Bias:</i> Affiliated – Unaffiliated			<i>Bias:</i> Affiliated Only
	Full Sample (1)	60-day Sample (2)	PSM (3)	PSM (4)
Post × EGC	0.463** (2.22)	0.602*** (3.68)	0.765*** (3.57)	0.570** (2.17)
EGC	-0.299** (-2.08)	-0.248** (-2.39)	-0.710*** (-3.63)	-1.137*** (-4.73)
Number Days Before Report	0.003 (1.11)	0.010*** (2.81)	0.003 (0.78)	-0.002 (-0.58)
Number Affiliated Analysts	0.009 (0.49)	0.020 (1.46)	0.025 (1.02)	-0.027 (-0.85)
Number Unaffiliated Analysts	-0.001 (-0.06)	0.000 (0.03)	0.005 (0.18)	0.033 (0.98)
Percent Affiliated Analysts	0.089 (0.18)	0.127 (0.38)	0.087 (0.13)	0.383 (0.70)
Ln[Analyst Experience (Years)]	0.062 (0.50)	-0.099 (-1.01)	-0.248 (-1.20)	0.224 (1.32)
Ln(Analyst Coverage)	0.157 (0.87)	0.118 (0.70)	0.111 (0.37)	-0.090 (-0.32)
Days from Report to Earnings	0.002 (1.11)	0.001 (0.58)	0.004 (1.63)	0.006** (2.52)
Historical Average Optimism	-0.396 (-1.48)	0.245 (1.46)	-0.359 (-0.96)	-0.192 (-0.64)
Historical Average Accuracy	-0.202 (-1.44)	0.131 (1.58)	-0.084 (-0.39)	-0.271 (-1.37)
Ln[Brokerage Size (Analysts)]	0.034 (0.38)	0.035 (0.49)	-0.102 (-0.70)	-0.070 (-0.52)
Firm, IPO, and Market Controls	Yes	Yes	Yes	Yes
Adjusted R-squared	0.187	0.091	0.231	0.264
Number of Observations	506	363	214	488

Table 6

Firm-Level Informativeness: Average Analyst Recommendation Announcement Returns

This table presents OLS regressions in which the dependent variable in Columns 1–4 is *Relative CARs*, defined as the within-firm difference between affiliated and unaffiliated analysts. This difference is the firm median three-day recommendation announcement CAR for affiliated analysts, minus the firm median for unaffiliated analysts. CARs for negative recommendations (i.e., hold, underperform, and sell) are flipped in sign to make interpretation consistent with positive recommendations, e.g., a positive response to a negative recommendation becomes a negative return. Announcement windows in which there are conflicting recommendations are excluded, in addition to any windows that coincide with an earnings, manager guidance, or merger announcement identified from COMPUSTAT, I/B/E/S, or SDC, respectively. In Columns 3 and 4, the CARs are then divided by the number of recommendations issued in the three-day window, while in Columns 1 and 2, the CARs are unscaled. The sample is restricted to all EGC and non-EGC issuers that have at least one unconflicted affiliated and one unconflicted unaffiliated recommendation. The PSM models in Columns 2 and 4 match 44 EGCs and 16 non-EGCs. All remaining variables (Firm, IPO, and Market controls are identical to those shown in Table 4) are defined in Appendix B. All regressions include Fama-French 12 industry fixed effects and issue-year fixed effects. Note that we include but do not tabulate the post-JOBS indicator coefficient, as the year fixed effects make it difficult to interpret. *t*-statistics using White (1980) robust standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	<i>Relative CARs:</i> Unscaled		<i>Relative CARs:</i> Scaled	
	Full Sample (1)	PSM (2)	Full Sample (3)	PSM (4)
Post × EGC	-6.289^{***} (-2.85)	-6.153^{**} (-2.03)	-4.231^{**} (-2.45)	-4.574^{**} (-2.18)
EGC	3.399 ^{**} (2.46)	1.615 (0.57)	1.435 (1.27)	0.683 (0.35)
Number of Days Before Report	0.002 (0.07)	-0.019 (-0.65)	-0.005 (-0.23)	-0.017 (-0.84)
Number of Affiliated Analysts	-0.107 (-0.34)	-0.444 (-0.69)	-0.117 (-0.58)	-0.521 (-1.19)
Number of Unaffiliated Analysts	0.796 (1.32)	-0.412 (-0.32)	0.439 (1.12)	-0.020 (-0.03)
Percent Affiliated Analysts	5.585 (0.64)	-15.764 (-0.85)	3.279 (0.54)	-9.280 (-0.80)
Ln(Analyst Experience)	0.450 (0.35)	3.283 (0.91)	-0.178 (-0.18)	-0.112 (-0.05)
Ln(Analyst Coverage)	-2.365 (-1.55)	-3.787 (-1.37)	-2.839 ^{**} (-2.18)	-2.732 (-1.40)
Historical Average Analyst CAR	0.260 (0.26)	-6.345 ^{***} (-2.82)	0.782 (1.03)	-3.232 ^{**} (-2.20)
Ln(Brokerage Size)	1.044 (1.18)	-1.588 (-1.15)	0.512 (0.76)	-1.568 (-1.45)
Firm, IPO, and Market Controls	Yes	Yes	Yes	Yes
Adjusted R-squared	-0.011	0.119	-0.006	0.075
Number of Observations	351	113	351	113

Table 7

Consequences to Increased Optimism

This table presents OLS regressions. In Panel A the dependent variable represents the natural log of share turnover (i.e., volume/shares outstanding) in the period following a firm's IPO. Column 1 measures turnover from the second through 10th trading day post-IPO, Column 2 from the sixth through the tenth trading day following the IPO, while Column 3 measures turnover in the tens days following the end of the post-IPO quiet period. In Panel B the dependent variable is price revision (i.e., the percentage change from IPO filing to offer price) in Column 1, IPO underpricing (i.e., first day stock returns) in Column 2, and the sum of the price revision and underpricing in Column 3. *Relative Optimism* is defined as the median forecast bias of affiliated analysts initiating coverage in the 60 days following the end of the quiet period minus the median forecast bias of unaffiliated analysts initiating coverage over the same period. Each column includes Firm, IPO, and Market controls that are identical to those reported in Table 4 of the paper. Analyst controls include the number of affiliated analysts, number of unaffiliated analysts, and percentage of affiliated analysts. See Appendix B of the paper for variable definitions. All regressions include Fama-French 12 industry fixed effects and issue-year fixed effects. Note that we include but do not tabulate the post-JOBS indicator coefficient, as the year fixed effects make it difficult to interpret. *t*-statistics using White (1980) robust standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Post-IPO Share Turnover

	Ln(Turnover, Days 2-10) (1)	Ln(Turnover, Days 6-10) (2)	Ln(Turnover, Post Quiet Period) (3)
Post × EGC × Relative Optimism	0.327* (1.68)	0.579** (2.56)	1.110*** (3.55)
EGC	0.227* (1.80)	0.149 (1.00)	-0.017 (-0.10)
Relative Optimism	-0.007 (-0.07)	0.040 (0.31)	0.218 (1.54)
Post × EGC	-0.004 (-0.02)	0.060 (0.30)	0.517** (2.20)
EGC × Relative Optimism	-0.020 (-0.19)	-0.070 (-0.50)	-0.278* (-1.81)
Post × Relative Optimism	-0.257 (-1.42)	-0.448** (-2.19)	-1.002*** (-3.76)
Firm, IPO, and Market Controls	Yes	Yes	Yes
Adjusted R-squared	0.335	0.249	0.076
Observations	363	363	363

Panel B: IPO Pricing

	Price Revision (1)	Underpricing (2)	Price Revision + Underpricing (3)
Post × EGC × Relative Optimism	0.112** (2.01)	0.157* (1.75)	0.269** (2.22)
EGC	0.112*** (3.19)	0.136*** (3.07)	0.248*** (3.64)
Relative Optimism	-0.007 (-0.36)	0.034 (1.51)	0.026 (0.72)
Post × EGC	-0.044 (-0.94)	0.001 (0.01)	-0.043 (-0.39)
EGC × Relative Optimism	0.004 (0.18)	-0.052** (-2.02)	-0.047 (-1.17)
Post × Relative Optimism	-0.109** (-2.23)	-0.040 (-0.53)	-0.149 (-1.46)
Firm, IPO, and Market Controls	Yes	Yes	Yes
Adjusted R-squared	0.373	0.247	0.345
Observations	363	363	363

Table 8

Affiliated Analyst Research Performance, IPO versus Seasoned Firms

This table presents OLS regressions measuring the relative change in affiliated analyst forecast optimism, accuracy, and report informativeness following JOBS for IPO firms, compared to the change in respective performance for seasoned firms (defined as firms that have been public for at least two years) covered by the analyst in the 90 days preceding the IPO offer date. Column 1 measures the relative change in forecast accuracy (see Table 5 for definition); Column 2 measures the relative change in forecast bias (see Columns 1–4 of Table 7 for definition); and Column 3 measures the relative change in three-day recommendation announcement CARs using the unscaled cumulative returns (see Columns 1–2 of Table 9 for definition). All remaining variables are defined in Appendix B. All regressions include Fama-French 49 industry fixed effects and year-quarter fixed effects. Note that we include but do not tabulate the post-JOBS indicator coefficient, as the year-quarter fixed effects make it difficult to interpret. *t*-statistics using standard errors clustered at the year-quarter and industry levels are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	<i>Accuracy</i> (1)	<i>Bias</i> (2)	<i>Informativeness</i> (<i>Three-Day CAR</i>) (3)
Post × IPO	-0.299^{***} (-3.75)	0.163^{**} (2.20)	-1.171^{***} (-2.91)
IPO	0.081 [*] (1.82)	0.033 (0.70)	-1.554 [*] (-1.83)
Ln(Total Assets)	0.081 ^{***} (4.32)	-0.030 (-1.63)	-0.469 ^{***} (-3.01)
Ln(Revenue)	-0.004 (-0.31)	0.018 (1.16)	-0.045 (-1.18)
Ln(Tobin's Q)	0.456 ^{***} (13.92)	-0.201 ^{***} (-6.07)	-0.506 (-0.89)
Leverage	-0.179 ^{***} (-3.65)	0.060 (0.83)	-0.128 ^{**} (-2.16)
Return on Assets	0.906 ^{***} (9.20)	-0.729 ^{***} (-6.12)	-0.020 (-0.18)
Operating at Loss	-0.286 ^{***} (-11.43)	-0.068 ^{**} (-2.46)	1.381 ^{***} (2.81)
High-Tech	0.051 [*] (1.93)	0.024 (1.14)	-0.668 (-1.57)
Market Return	0.043 (0.12)	0.146 (0.47)	1.911 (0.59)
Ln(Analyst Experience)	0.071 ^{***} (3.36)	-0.040 (-1.45)	0.210 ^{***} (2.67)
Ln(Analyst Coverage)	0.183 ^{***} (7.73)	-0.004 (-0.14)	-0.743 ^{***} (-3.05)
Ln(Brokerage Size)	-0.048 (-1.14)	0.020 (0.32)	0.247 (1.58)
Horizon	-0.001 ^{***} (-6.24)	0.001 ^{***} (2.79)	
Historical Accuracy	0.065 ^{***} (4.33)		
Historical Optimism		0.026 ^{**} (2.16)	
Historical Informativeness			0.269 ^{**} (2.57)
Adjusted R-squared	0.217	0.047	0.030
Number of Observations	24,552	24,552	9,066