The Consequences to Analyst Involvement in the IPO Process: Evidence Surrounding the JOBS Act

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Abstract

The JOBS Act allows certain analysts to be more involved in the IPO process, but does not relax restrictions on analyst compensation structure. We find that these analysts initiate coverage that is more optimistically biased, less accurate, and generates smaller stock market reactions. Investors purchasing shares following these initiations lose over 3% of their investment by the firm's subsequent earnings release. By contrast, issuers, analysts, and investment banks appear to benefit from this increased bias, as optimism is more positively associated with proxies for firm visibility and investment banking revenues when analysts are involved in the IPO process.

Keywords: Equity Analyst Research; Conflicts of Interest; JOBS Act; IPOs

JEL Classifications: G2, G29

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Abstract

The JOBS Act allows certain analysts to be more involved in the IPO process, but does not relax restrictions on analyst compensation structure. We find that these analysts initiate coverage that is more optimistically biased, less accurate, and generates smaller stock market reactions. Investors purchasing shares following these initiations lose over 3% of their investment by the firm's subsequent earnings release. By contrast, issuers, analysts, and investment banks appear to benefit from this increased bias, as optimism is more positively associated with proxies for firm visibility and investment banking revenues when analysts are involved in the IPO process.

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1. Introduction

Analysts face conflicting incentives regarding the precision of their research 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] but optimistic research can increase future investment banking business and generate higher trading volume.[2] To the extent that analyst reports affect stock prices, investors' understanding of how analysts manage this tradeoff will determine whether they benefit from or are harmed by the content of analyst reports. One way that regulators have sought to influence how analysts manage this tradeoff is by controlling the extent to which analysts are permitted to participate in the securities issuance process. However, there is no direct academic evidence on how such participation actually affects analyst behavior. On the one hand, such involvement may endow analysts with private information and industry knowledge to produce more accurate reports.[3] On the other hand, increased interactions with bankers and investors may increase analysts' incentives or ability to influence trading and issue prices through their optimism.

The purpose of this paper is twofold. We first exploit a policy experiment to identify how analysts' participation in the securities issuance process affects their research output. Next, we use this policy experiment as a legislative shock to analyst behavior to provide new evidence on the value and implications of analyst research.

Empirical evidence on the effect of analysts' participation in the securities issuance process on their research is limited, in large part because such participation is unobservable. Evidence that does exist is indirect, inferred from a series of regulations passed in the early 2000s. A limitation to this identification strategy is that these regulations simultaneously introduced several other changes beyond constraining analysts' participation in securities offerings, such as restrictions on analyst report content and compensation structure (Kadan et al., 2009). Bradshaw (2009), Koch et al. (2013), and Leuz and Wysocki (2016) discuss how the large number of simultaneous regulatory changes in this period, such as the Global Settlement, Regulation FD, and stock market decimalization, make it difficult to isolate the consequences of any one particular regulatory shock.[4] Some literature overcomes this challenge by using the fact

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[1] See Mikhail et al. (1999), Hong and Kubik (2003), Jackson (2005), and Ke and Yu (2006).

[2] See Jackson (2005), Degeorge et al. (2007), Ljungqvist et al. (2009), and Niehaus and Zhang (2010).

[3] See Boni and Womack (2003), Jacob et al. (2008), Chen and Marquez (2009), Green et al. (2014a), Soltes (2014), Brown et al. (2015), and Bradley et al. (2017).

[4] Consequently, existing research provides contradictory evidence on the source of the change in analyst behavior in the early 2000s (e.g., Francis et al., 2006; Arping and Sautner, 2013; Chen et al., 2017).

that the Global Settlement targets only a subset of banks to identify the consequences of the punitive and reputational costs associated with the Global Settlement.[5] Corwin et al. (2017) find that these costs resulted in analysts employed by sanctioned banks becoming less optimistic, but they provide little evidence that concurrent analyst rule changes, including limiting analysts' involvement in the IPO process, significantly affected analyst research. Indeed, the large number of simultaneous changes in the regulatory and economic environments at that time prevent any direct takeaways regarding the empirical question of whether or how analyst involvement in the underwriting process affects the quality of their research.

We use the April 5, 2012 passage of the Jumpstart Our Business Startups Act (JOBS, or the "Act") as a less fettered setting to identify the effect of analysts' participation in the securities issuance process on their research. The JOBS Act was designed to reduce the regulatory burdens of going public for issuers of initial public offerings (IPOs) with less than $1 billion in pre-IPO annual revenue, referred to as emerging growth companies (EGCs).[6] An important component of the Act is a set of provisions that allow analysts employed by members of the EGC issuer's IPO underwriting syndicate ("EGC affiliated analysts") to be more extensively involved 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 IPO participation 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 IPO participation, 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 interpret the differential change in the behavior of EGC affiliated analysts (i.e., treated analysts) relative to untreated analysts following JOBS as the effect of IPO participation on analyst behavior.

To evaluate the consequences of analyst participation in the IPO process, we begin by investigating how this involvement affects analysts' initial earnings forecasts after the IPO. We

[5] See, for example, Kadan et al. (2009) and Guan et al. (2012).

[6] 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.

find that following JOBS, EGC affiliated analysts become significantly less accurate and more optimistic: after JOBS, the relative accuracy of affiliated analysts (compared to their unaffiliated 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 optimistic bias of EGC affiliated analysts is of similar magnitude. To the extent that we have identified the JOBS Act treatment effect of greater analyst participation in the offering process, our results suggest that participation results in more optimistic and less accurate analyst research.

We next examine the economic consequences of this post-JOBS increase in EGC affiliated optimism. We begin by investigating the three-day cumulative abnormal returns surrounding analyst coverage initiations to determine whether the reports initiated by post-JOBS EGC affiliated analysts produce less value-relevant information for investors consuming the reports at the time of their release. We find that the reports initiated by analysts affiliated with the underwriters of post-JOBS EGCs garner a more muted stock market reaction. This finding is robust to removing confounding events (Altınkılıç et al., 2013), restricting the sample to optimistic analysts, and using two-hour intraday CARs. This muted market reaction suggests that allowing analysts to participate in the IPO process results in less value-relevant information at the time their reports are released. However, the extent to which investors are harmed by this change in information production depends on whether the excess optimism in the reports is sufficiently de-biased.

Although investors appear to rationally de-bias perceived optimism from analyst forecasts (e.g., Dugar and Nathan, 1995; Michaely and Womack, 1999), evidence in Malmendier and Shanthikumar (2007), So (2013), and Veenman and Verwijmeren (2017) suggests that de-biasing is incomplete, especially among smaller, individual investors. To determine whether complete de-biasing occurs in our setting, we examine stock returns between the time that affiliated analysts initiate coverage and the first subsequent earnings announcement by the firm. If investors do not sufficiently de-bias optimistic reports, we expect post-JOBS EGCs to exhibit more negative cumulative abnormal returns between the time when affiliated analysts initiate coverage (typically at expiration of the quiet period) and the earnings announcement date (i.e., the latest point at which the market may learn of the optimism). We find evidence consistent with incomplete de-biasing: investors entering into a position in post-JOBS EGCs following affiliated analyst coverage initiation lose between 3 and 4 percent of their investment by the first earnings announcement. Our results are similar using comparison of means or medians, using raw, market-adjusted, or style-adjusted returns, and remain statistically significant relative to the inclusion of non-EGC firms as a control group in a difference-in-differences specification.

Existing literature provides some insight regarding the types of investors who are most likely to be harmed from these returns patterns and why such an equilibrium may persist. Bradley et al. (2003) and Ofek and Richardson (2003) argue that institutional investors sell into the liquidity generated by affiliated analyst coverage at the end of the post-IPO quiet period. Furthermore, Malmendier and Shanthikumar (2007) find that small investors are less likely to fully de-bias analyst reports. Combined with evidence that retail investors consistently overpay for IPO shares (Dorn, 2009), the negative returns we observe are consistent with a wealth transfer from smaller retail investors purchasing stock at the time of affiliated analyst coverage initiation to the institutional investors selling the stock. Analysts and/or investment banks may benefit from this wealth transfer (perhaps via improved relations with their preferred institutional clients), which may explain why analysts are willing to harm both their reputation for accurate research (Jackson, 2005) and the welfare of a subset of retail investors.

Following increased IPO participation, analysts and their investment banks will also benefit from more optimistic research if there is an associated increase in the effect of optimism on: (1) post-IPO share turnover, which is positively related to analyst compensation and investment banking revenues via trading commissions, (2) the pre-IPO price revision, which is positively related to the IPO offer price and the dollar value of underwriting fees,[7] or (3) IPO underpricing, which builds an investment bank's reputation with its institutional clients (Beatty and Ritter, 1986; Reuter, 2006). These effects are likely since pre-IPO analyst participation allows institutions to learn of analyst optimism earlier. Evidence in Jackson (2005) suggests that this knowledge of analyst optimism may increase trading volume in the days following the IPO. Moreover, this optimism allows institutions to anticipate that they will be able to offload shares to retail investors at the end of the quiet period, which will increase institutions' willingness to buy IPO shares. This increased willingness to purchase IPO shares will likely manifest itself in both a larger pre-IPO price revision and increased IPO underpricing, since Hanley (1993) provides evidence that positive information during the bookbuilding period is not fully captured in the IPO offer price.

We find evidence consistent with all three benefits to analyst optimism, as affiliated analyst optimism becomes more positively related to post-IPO share turnover, the pre-IPO price

[7] We find no evidence that post-JOBS EGCs pay differential fees on a percentage basis.

revision (i.e., leading to a higher offer price), and IPO underpricing for EGCs following the passage of JOBS. Notably, these tests use bias in analysts' initial reports to proxy for pre-IPO optimism. This proxy is reasonable because analysts' reputational considerations make it costly for them to substantially alter their level of optimism between their pre-IPO interactions and their official coverage initiation. However, this proxy does introduce the potential for reverse causality, which precludes a causal interpretation.

Nevertheless, our findings are consistent with an equilibrium in which IPO involvement increases the benefits that analysts, investment banks, and institutional investors accrue from optimistic reports. Issuers may also benefit more from optimistic research when analysts participate in the IPO process because IPO participation increases the association between optimistic research and the pre-IPO price revision, thereby increasing the IPO offer price and thus reducing the issuer's cost of capital. In addition, our evidence suggests that analyst involvement also enhances the relation between optimism and firm visibility, in the form of increased trading volume (Mehran and Perestiani, 2009). However, the increased IPO underpricing also represents a cost to the issuer, as they are leaving more money on the table.

Since our identification strategy uses unaffiliated analysts of EGCs and all analysts of non-EGCs to control for overall market conditions, and also uses a matched sample to control for potential differences in pre- and post-JOBS issuers, it is unlikely that our findings are driven by factors unrelated to the JOBS Act analyst provisions. Robustness tests 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., Altınkılıç and Hansen, 2009), an increased ability of EGC management to select or manipulate analysts, or other JOBS Act consequences.

Our study provides new evidence on the link between analyst incentives and optimistic bias. The JOBS Act allows us to isolate the effect of analysts' involvement in the IPO process on the content and value of analyst research. Specifically, we find that pre-IPO participation increases analyst optimism and results in a more muted market reaction. This evidence contributes 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 private communications via presentation events and investor meetings (Green et al., 2014a, 2014b; Kirk and Markov, 2016), which appear to improve analysts' forecasting ability and the information content of their research (Green et al., 2014a; and Brown et al., 2015). Our study shows that these forms of interactions affect analysts differently in an IPO setting.

These findings also contribute to the ongoing debate on whether and how analysts create value for firms and investors. Whether analysts add value by increasing investor recognition (Li and You, 2015), monitoring (Chen et al., 2015), or providing new information to the market (Bradley et al., 2014) remains a debated issue in the literature. In fact, recent research casts doubt on whether security analysts provide any new information (e.g., Altınkılıç et al., 2013; Jenkinson et al., 2016; and Loh and Stulz, 2011), especially following the advent of high frequency trading (e.g., Chordia et al., 2014). Our evidence suggests that when analysts become more involved in the IPO process, they may be better able to influence firm visibility (via post-IPO trading), which has been argued to be positively related to firm value (Merton, 1987). Although investors appear to partially de-bias optimistic earnings forecasts at the time of their release, we find that investors who purchase shares of post-JOBS EGCs following coverage initiation suffer negative abnormal returns when earnings are subsequently announced. Thus, investors who cannot perfectly anticipate analysts' forecast errors are subsequently harmed. Whether the importance of this role of analysts extends to other settings is uncertain, especially given existing evidence that analyst recommendation changes are most likely to matter for small, growth firms (Loh and Stulz, 2011). Therefore, we note that our findings apply to analysts' forecasts for small, young, growth companies at the time of their IPOs and may not generalize to other firms.

Finally, this paper extends the emerging literature on the consequences of the JOBS Act. Dambra et al. (2015) find that the JOBS Act provisions that reduce the risks of going public result in an increase in the number of firms going public, especially those with high proprietary disclosure costs, while Barth et al. (2017) and Chaplinski et al. (2017) find that these same provisions also increase informational asymmetry between insiders and post-IPO investors. 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 IPO participation 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 among various IPO participants.

2. Literature Review

2.1 Analyst Conflicts of Interest

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

The conflicts of interest that analysts face is rooted in the fact that they benefit from issuing both 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 Yasuda (2009) find that reputation is an effective disciplinary device against analysts' conflicted interests. Alternatively, sell-side equity analysts affiliated with investment banks that underwrite firms' securities issuances have an incentive to optimistically bias their research output in order to increase future investment banking fees and/or brokerage revenues (e.g., Jackson, 2005; Degeorge et al., 2007; Ljungqvist et al., 2009; Neihaus and Zhang, 2010).[8] Additionally, Groysberg et al. (2011) link this behavior to analyst compensation, showing that analyst pay is increasing in the effect that an analyst's research has on equity underwriting and brokerage fees.[9] Existing literature suggests that, in the absence of regulation, these incentives dominate the aforementioned benefits to issuing accurate reports. As a result, when left unregulated, analysts affiliated with an investment bank that recently acted as a firm's underwriter bias their research output optimistically (Lin and McNichols, 1998; Michaely and Womack, 1999; Dechow et al., 2000).

2.2 Regulating Analyst Behavior

In part to address the concern that analyst involvement in the investment banking process creates incentives for analysts to optimistically bias their research, regulators in the early 2000s limited analyst access to managers, investors, and investment bankers through self-regulatory organization rule changes (the "SRO rules") and the Global Settlement. These regulatory

[8] The evidence on the effect of optimistic bias on investment banking fees is more mixed than the effect of optimistic bias on brokerage fees (Ljungqvist et al., 2006, Clarke et al., 2007, Degeorge et al., 2007, and Ljungqvist et al., 2009).

[9] Additional explanations posited by the literature for affiliated 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).

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, following these regulations, analyst research was less optimistically biased, especially among affiliated analysts. However, recommendations became less informative (see e.g., Barber et al., 2006; Barber et al., 2007; Chen and Chen, 2009; and Kadan et al., 2009) and forecasts became less accurate (Guan et al., 2012).

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 this rule change on investors, managers, and analysts (e.g., Gintschel and Markov, 2004; Francis et al., 2006; and Heflin et al., 2016). In terms of analyst informativeness, market reactions to analyst reports are more muted following Reg FD. 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 Agarwal 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 during this time period 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 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.

To isolate the effect of the provisions unique to the Global Settlement on analyst behavior, a handful of studies have taken advantage of the fact that the Global Settlement targets a subset of banks.[10] As emphasized by Corwin et al. (2017), this empirical setting allows researchers to identify the consequences of the settlement's punitive and reputational costs on

[10] See, for example, Kadan, Madureira, Wang, and Zach (2009), Guan, Lu, and Wong (2012), and Corwin, Larocque, and Stegemoller (2017).

analyst behavior. While Corwin et al. (2017) find that the aforementioned costs resulted in less optimistic analyst research, they find little net effect of concurrent rule changes on analyst research. A limitation to the Global Settlement setting is that the large number of concurrent changes affecting the entire industry prevents any direct takeaways regarding the empirical relevance of other regulatory changes around this time. In particular, the existing literature has not identified the extent to which analyst participation in the underwriting process affects analyst behavior.

3. IPO Participation and Analyst Behavior

In this section, we discuss two mechanisms through which analysts' participation in the IPO underwriting process may affect the quality of their research.

3.1. IPO Participation and Increased Forecast Accuracy

IPO participation may result in an increase in the quality of analyst coverage because it lowers the cost of producing accurate analyst reports. For instance, increased interactions with investment banking colleagues and the issuing firm's management can provide analysts with private information that is otherwise costly (or impossible) to obtain. Green et al. (2014a) show that private interactions between analysts and management increase forecast accuracy, while evidence in Soltes (2014) suggests that private meetings offer analysts the opportunity to better understand a firm's operations. Anecdotal evidence corroborates this idea in an IPO setting (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.[11] Soltes (2014) notes that managers are allowed to review an analyst's model and that analysts seek private interactions with managers "to 'triangulate their hypotheses', 'bounce ideas', or 'calibrate expectations of future

[11] See, for example, Green et al. (2014a), Soltes (2014), and Brown et al. (2015).

performance'."[12] Increased interactions with management can also lead to unintentional information transfers, such as vocal cues or body language (Mayew and Venkatachalam, 2012; Hobson et al., 2012; and Brown et al., 2015). Through these interactions during the IPO process, affiliated analysts may be better able to identify firms' future prospects.

Moreover, IPO participation 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 this information channel 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): IPO participation increases forecast accuracy.

3.2. IPO Participation and Increased Forecast Bias

Alternatively, IPO participation may lead to more optimistically biased and less accurate reports because this participation increases the benefits to issuing biased reports. Although the current regulatory regime prohibits analyst compensation from being explicitly tied to investment banking revenue, Groysberg et al. (2011) find that brokerage trading revenue is an important input into analyst compensation. Importantly, brokerage trading revenue is positively associated with analyst optimism (Jackson, 2005; Cowen et al., 2006; and Niehaus and Zhang, 2010). Involvement in the IPO process provides an opportunity for analysts 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.

The issuance of more optimistic forecasts by affiliated analysts may also provide increased liquidity for their institutional clients, and earlier communications may more easily allow analysts to convey their opinions. Prior research shows that analyst coverage initiations following the IPO result in trading volume spikes, which allow institutional investors to sell into

[12] Prior literature indicates that this behavior is often limited to private conversations with management, as opposed to those in the public domain. 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 (Soltes 2014; Brown et al., 2015).

new investor demand (Bradley et al., 2003; Ofek and Richardson, 2003). Such liquidity may be more valuable to institutional investors when it can be anticipated, as is likely the case with increased IPO involvement.

In addition, joint meetings with members of the underwriting syndicate may also result in analysts feeling pressured to be 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 revenues through a higher IPO offer price. Finally, earlier and more frequent interactions with company management may reinforce career concern motives for analysts to optimistically bias their research to increase an analysts' 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): IPO participation increases forecast bias.

4. Empirical Design and Data Description

4.1. Empirical Setting

To identify the effect of analysts' IPO participation on their behavior, we utilize the analyst provisions in the JOBS Act, which were 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. 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 they may participate in presentations by EGC management to educate the issuer's sales force (Sidley Austin LLP, 2012; Morrison-Foerster, 2014b). In contrast, prior to the Act affiliated analysts could not contact potential investors before an IPO, and New York Stock Exchange (NYSE) Rule 472 prohibited investment bankers from facilitating communication between equity analysts and prospective investors (Morrison-

Foerster, 2014b). The Act also increases analysts' pre-IPO interactions 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 in the early 2000s. See Appendix A for more details on the JOBS Act.

4.2. Sample

To empirically investigate how the IPO involvement 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

[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.

analyst behavior around the time of the IPO, we restrict our sample to initiations of analyst 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.[14] 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 IPO involvement on analyst behavior, we exploit the fact that the JOBS Act targets only EGC affiliated analysts. In accordance with the JOBS Act definition, we define affiliated analysts 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 of analysts that are not affected by the JOBS Act: (1) unaffiliated

[14] Prior studies utilize a post-IPO sample period of one year following the IPO (e.g., Michaely and Womack, 1999; Huyghebaert and Xu, 2015). To increase comparability between the timing of our affiliated and unaffiliated analyst initiations, we conservatively restrict our sample to 180 days following the IPO. Results are qualitatively similar using the full year following the IPO. Furthermore, we only retain one forecast for each analyst to avoid serial correlation between simultaneous forecast issuances for different time periods.

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, while 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:

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

We include Fama-French 12 industry and year fixed effects (based on IPO issue date) to capture any industry time series differences affecting all analysts.[15] 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.[16]

An important benefit to Equation (1) is that it identifies the effect of IPO involvement 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.

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

[16] 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. It also allows us to partition our analysis on various analyst characteristics.

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- 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, 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).[17] 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. Analyst Outcomes

We apply the identification techniques outlined above to three analyst report level outcomes: Accuracy, Bias, and Three-Day CAR. We measure analyst accuracy and optimism using standard measures in the literature.[18] Because our sample's earnings forecasts often occur immediately following the IPO, consistent with Lin and McNichols (1998) and Huyghebaert and Xu (2015), 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 analysts to those of unaffiliated analysts.

Accuracy is defined as $-1 \times \left|\frac{\mathrm{Forecast}_{i,t} - \mathrm{Actual}_i}{\mathrm{Price}_{i,t-1}} \times 100\right|$, where $\mathrm{Forecast}_{i,t}$ 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. $\mathrm{Price}_{i,t-1}$ is issuer i's stock price on the last trading day prior to the analysts'

[17] Detailed definitions of these variables are provided in Appendix B.

[18] See, for example, Brown et al. (1987), Dugar and Nathan (1995), and Agrawal and Chen (2012).

coverage initiation date.[19] *Relative Accuracy* is the difference between the median *Accuracy* for affiliated analysts of a given issuer minus the same median for unaffiliated analysts.

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 of a given issuer minus the same median for unaffiliated analysts.[20]

Finally, we use the three-day market-adjusted cumulative abnormal return (*Three-day CAR*) for the issuer's stock 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. To compute this three-day CAR, we compound the daily abnormal return from one trading day prior to coverage initiation to one trading day after (see Panel A of 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.[21] We expect positive returns for buy recommendations, while we expect negative returns for hold or sell recommendations. 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

[19] 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.

[20] 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 of buy-hold-sell. Therefore, we rely on forecast bias throughout our analyses, which allows us to directly investigate the tradeoff between analyst accuracy and bias.

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

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.[22]

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, in which 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.[23] 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 the within-firm difference in median *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. Although there are fewer non-EGCs, 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.

[22] This alternative measure prevents attributing the entire three-day market response to each individual report released within the window.

[23] 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.

Panel B of Table 1 provides averages of the firm-level median analyst outcomes that we use as dependent variables throughout our analysis. There is a significant post-JOBS decrease in EGC affiliated forecast accuracy and a corresponding increase in forecast optimism, which is consistent with increased IPO involvement 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 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, on average, CARs decline for EGC affiliated analysts and increase for all three control groups.

Panel C of Table 1 provides averages of our firm level outcomes. We find no statistically significant differences in IPO price revisions for EGCs and non-EGCs before and after JOBS and similar increases in IPO underpricing for both groups following JOBS. This suggests that the JOBS Act does not significantly affect how underwriters set IPO prices. Trading volume is larger for EGC firms after JOBS, although the statistical significance of the difference depends on the functional form.

Table 2 provides means for issuer characteristics, offer characteristics, and analyst coverage for EGCs and non-EGCs before and after JOBS. We observe some statistically significant differences in EGCs pre- and post-JOBS. For instance, Columns 1 and 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 pre-IPO 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.

This evidence is consistent with the existing literature on the JOBS Act (e.g., Gupta and Israelsen, 2014; Dambra et al., 2015; Westfall and Omer, 2015; Barth et al., 2017; and Chaplinski et al., 2017). To ensure that we have a representative control group to identify the consequences of the JOBS Act's analyst provisions on analyst behavior, we replicate our analyses using a propensity score matched sample. Table 3 shows that within this matched sample, 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 IPO involvement on analyst behavior by testing whether EGC affiliated analyst forecast accuracy changes relative to the accuracy of other analysts after the passage of JOBS. Accuracy may improve via our increased accuracy hypothesis (H1), but may decline if IPO involvement causes 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 year 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.[24] 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 (it is approximately -0.4 in the post-JOBS period on average), 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 following the JOBS Act. In Columns 1–3, 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 × 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.

[24] 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 occur between July 1, 2011 and April 4, 2012.

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 EGC unaffiliated and all 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 analysts' increased IPO participation permitted by the JOBS Act will increase the optimistic bias of EGC affiliated analyst reports. We 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 1–3 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, which more precisely controls for analyst characteristics, such as brokerage fixed effects. This evidence is robust to replacing the continuous measure of optimistic bias with an indicator for an optimistically biased forecast as the dependent variable. Following

JOBS, EGC affiliated analysts become approximately 25% more likely to initiate coverage with an optimistically biased earnings forecast (see Table OA-2). Collectively, our results support the increased optimism hypothesis. This analysis provides some of the first direct evidence that analysts' involvement in the IPO process motivates analysts to optimistically bias their research.

6. The Costs and Benefits of Analyst Optimism

In this section, we examine the costs and benefits to the heightened affiliated analyst optimism we observe for EGCs following the passage of JOBS. For optimistic bias to have any effect on market participants, it is necessary that analyst research informs investor decisions. By investigating the economic consequences of changes in affiliated analyst optimistic bias, we contribute to the ongoing debate concerning the extent to which analysts add value. A large body of literature provides evidence that analyst reports contain value-relevant information.[25] However, some recent research casts doubt on this conclusion.[26] For instance, Altınkılıç and Hansen (2009) find that revisions are "information-free" for investors, and Li and You (2015) conclude that analysts primarily add value by increasing visibility and investor demand, rather than by producing information. Even if analyst reports do not produce value-relevant information *on average*, whether the lack of information content extends to the newly public firms we study is an empirical question. Loh and Stulz (2011) find that analyst recommendation changes are most likely to matter for small, high growth, and high institutional ownership firms, and firms with high analyst forecast dispersion. Given that the majority of newly public firms fall into some or all of these categories, the analyst coverage initiations that we study may be uniquely valuable to investors. To examine this issue, we conduct a variety of empirical tests to determine the extent to which market participants respond to, or are affected by, the optimistically biased coverage initiations generated by affiliated analysts covering post-JOBS EGCs.

6.1. Are Investors Harmed?

We begin by examining whether investors who purchase shares based upon the optimistically biased reports of post-JOBS EGC affiliated analysts are harmed by analyst optimism. For these investors to be harmed, they must rely on the analyst's report and not completely de-bias the optimistic forecast. Mehran and Stulz (2007) discuss the fact that while

[25] See Womack (1996), Ivkovic and Jegadeesh (2004), Jegadeesh et al. (2004), Loh and Stulz (2011), Bradley et al. (2014), and Huang, Zang, and Zheng (2014).

[26] See Altınkılıç and Hansen (2009), Altınkılıç, Balashov, and Hansen (2013), Kim and Song (2014), Li and You (2015), and Altınkılıç, Hansen, and Ye (2016).

conflicts of interest at financial institutions could foster biased research, bias will not harm customers as long as they understand these conflicts. However, as Fischer and Verrecchia (2000) point out, it may be difficult for investors to back out bias from forecasts if they do not fully understand analysts' incentives or how these incentives feed into earnings forecasts. Several papers provide empirical evidence suggesting that investors do not always perfectly account for bias. Malmendier and Shanthikumar (2007) demonstrate that small investors do not fully de-bias analyst recommendations, Veenman and Verwijmeren (2017) find that investors are unable to fully unravel predictable pessimistic bias, and So (2013) identifies a profitable trading strategy that exploits investors' overweighting of analyst forecasts. Taken together, this research suggests that investors may not be able to perfectly adjust for bias in the information they receive from analysts.

As a first step toward understanding whether the increased optimism we observe harms investors who buy at the time of coverage initiation, we examine the market response to analyst coverage initiations. Table 6 uses the same difference-in-differences strategy as in Tables 4 and 5 to investigate whether the more optimistic post-JOBS EGC affiliated coverage initiations are accompanied by a more muted market reaction.[27] The dependent variable in Columns 1 and 2 is *Relative CAR*, which measures the difference between the median *Three-day CARs* surrounding affiliated and unaffiliated coverage initiations within the same firm. As discussed in Section 4.4, 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 to initiations by unaffiliated analysts for the same issuer. If market participants respond less to EGC affiliated reports following JOBS, we expect a negative coefficient on the Post-JOBS × EGC interaction. Alternatively, if EGC affiliated analyst reports incorporate qualitative, value-relevant information garnered from increased pre-IPO participation, we may find the opposite effect.[28]

The negative interaction coefficients in Table 6 suggest that EGC affiliated analyst reports have become less informative (relative to unaffiliated reports) since these analysts have been allowed to participate more in the IPO process. In addition to the firm-level results

[27] We examine how EGC affiliated analyst optimism affects trading volume in Section 6.2.

[28] Another possible explanation for a muted response would be that EGC affiliated analyst reports come out after unaffiliated analyst reports post-JOBS. We find no evidence of this. See Section 7.1.1 for more details.

presented in Table 6, we also conduct forecast-level tests (in the Online Appendix), where we replicate the Table 6 analyses using a triple-differencing approach. We find similar results at the forecast level. These results are robust to restricting the sample to buy recommendations and using TAQ data to limit our market responses to the two hours after initiation.

The robustness of our results to intra-day trading data combined with our identification strategy makes it unlikely that our findings relate to the recent literature suggesting that the market reactions surrounding analyst report announcements are primarily driven by analysts piggybacking off of news events (e.g., Altınkılıç and Hansen, 2009; Altınkılıç et al., 2013; Hansen, 2015). Additionally, for piggybacking to explain our results, there would have to be more piggybacking for our treated group of EGC affiliated analysts following JOBS. In unreported tests, we follow the Altınkılıç 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 Altınkılıç et al. (2013) consider (i.e., earnings, new business, and financing events).

These findings suggest that one consequence to the post-JOBS increase in EGC optimism is that investors consuming analysts' reports at the time of their release receive less useful information from these reports. However, a test focused solely on announcement reactions cannot determine whether investors fully de-bias optimistic reports, as it is possible that an analyst's optimism was impounded into prices before the report is released. To better assess the extent to which investors de-bias analyst reports, we next conduct an event study examining stock returns from coverage initiation through the subsequent earnings announcement by the firm. If at the time of report initiation the market fully de-biases the affiliated analyst's optimism, we would not expect to find systematic differences in stock returns for EGCs after JOBS. Alternatively, if the market does not adequately recognize and adjust for affiliated analyst bias, then post-JOBS EGCs should exhibit negative returns in the period between coverage initiation and the earnings announcement.

Panel A of Table 7 presents the cumulative raw and abnormal returns from analyst coverage initiation through the subsequent earnings announcement. We examine returns for each issuer in the period beginning after the first affiliated initiation and ending one day after the firm's subsequent earnings announcement. Given the evidence in Figure 4 that affiliated analyst initiations cluster following the post-IPO quiet period, our primary analysis computes compounded abnormal returns beginning five days after the first affiliated analyst initiates coverage (see Panel B of Figure 2 for a graphical representation). We allow for five trading days after the first affiliated report issuance to ensure that our return window cleanly captures issuer performance subsequent to the issuance of the majority of analysts' report initiations. In unreported tests, we find that our results are similar beginning the returns window two, three, or four days after affiliated coverage initiation.

As shown in the univariate comparisons in Panel A, the average EGC underperformed the market by a statistically insignificant 0.32% prior to JOBS, but following JOBS, the underperformance jumps to a significant 3.68%. The difference between the underperformance of pre- and post-JOBS EGCs is a statistically significant 3.37%. The third and fourth rows provide similar evidence for style-adjusted and median returns,[29] each of which indicate that investors lose between 2% and 4% of their investment by the first earnings announcement if they purchase shares following the initiation of affiliated analyst coverage. The fifth row of Panel A further demonstrates that this result is not due to outliers: investing in a post-JOBS EGC after coverage initiation by the first affiliated analyst and holding the stock through the first earnings announcement results in a loss roughly 60% of the time.

In Panel B of Table 7, we introduce a difference-in-differences specification with non-EGCs as the control group. The significant Post-JOBS × EGC interactions using raw, market-adjusted, and style-adjusted returns suggest that the underperformance of post-JOBS issuers between analyst coverage initiation and the subsequent earnings announcement is unique to EGCs. These findings demonstrate that investors who purchase shares of post-JOBS EGCs following the release of optimistic affiliated analyst reports are harmed by the time the optimism is revealed. Panel C of Table 1 provides evidence that such purchase behavior does exist as there is no less trading volume at the end of post-IPO quiet period, when most affiliated analysts initiate coverage, for post-JOBS EGCs compared to other IPO issuers.

Existing literature provides some insight regarding the types of investors that are most likely to be harmed from these returns patterns and why such an equilibrium may persist. Bradley et al. (2003) and Ofek and Richardson (2003) provide evidence that institutional investors typically sell into the liquidity generated by optimistic analyst reports at the end of the post-IPO quiet period. Furthermore, Malmendier and Shanthikumar (2007) find that small investors are less likely to fully de-bias analyst reports. Thus, it is likely that the negative returns

[29] Style-adjusted returns are computed by matching each issuer to a seasoned firm with the closest size and book-to-market as of the issuer's first post-IPO filing. See Appendix B for more detail on how we select matches.

we observe result in a wealth transfer from (likely small) investors purchasing at the end of the quiet period to the institutional investors holding the stock at the time of affiliated analyst report release. Dorn (2009) provides additional evidence supportive of this wealth transfer dynamic between small and large investors by showing that retail investors consistently overpay for IPO shares. Finally, Patatoukas et al. (2016) note that short-selling constraints mitigate traders' abilities to arbitrage away post-IPO underperformance, contributing to predictable negative returns patterns.[30]

These findings on market reactions and subsequent buy-and-hold returns contribute to the literature on whether analyst forecasts and recommendations influence investor decisions. Recent literature suggests a variety of ways in which analysts add value, including monitoring (Altınkılıç et al., 2013; Bradley et al., 2017), marketing (Li and You, 2015), and information production (Bradley et al., 2014; Huang et al., 2014). However, whether the information in analyst recommendations and forecasts adds value on average is not clear, especially in today's high frequency trading environment (Altınkılıç and Hansen, 2009; Loh and Stulz, 2011; Altinkilic et al., 2013; Kim and Song, 2014; and Altınkılıç et al., 2016). Our findings suggest that the overly optimistic coverage initiations of affiliated analysts for post-JOBS EGCs influence the purchase decisions of some investors who become exposed to a predictable stock price decline. Thus, analyst optimism appears to influence investors' trading and valuation of newly public firms.

6.2. Why does Pre-IPO Involvement Increase Analyst Optimism?

For pre-IPO participation to incentivize analysts to become more optimistic, it must increase the net benefits to optimism. As discussed in Section 3.2, there are several reasons why pre-IPO participation may increase an analyst's incentive to be optimistic. One possibility is that increased pre-IPO participation enhances an analyst's ability to facilitate a transfer of wealth from retail investors to their preferred institutional clients. Conceptually, if pre-IPO participation allows initial investors in the IPO to better predict analyst optimism, these investors can wait to optimally exit their positions after analysts' initiate coverage. The evidence in Section 6.1 is consistent with this line of reasoning.

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[30] Derrien (2005) and Ljungqvist et al. (2006) formalize a theoretical basis for such an equilibrium where small investors continually overpay, showing that the combination of sentiment investors and selling constraints can result in IPOs having high initial returns (i.e., underpricing), which subsequently reverse. However, the IPO setting involves several known market anomalies and capital market inefficiencies (see Ritter, 2011 for a comprehensive survey). Therefore, there are other possible drivers of the post-IPO return patterns that we observe.

In this section, we empirically investigate two additional reasons that increased pre-IPO participation may heighten incentives for analysts to sacrifice their reputation for accuracy to facilitate optimism to the benefit of their investment banks. The first such reason is based on the potential impact that optimism has on post-IPO trading activity, which is important to underwriters and analysts for several reasons. First, Ellis et al. (2000) and Niehaus and Zhang (2010) find that post-IPO trading is conducted primarily through the IPO underwriter's brokerage house, suggesting that post-IPO share turnover is a reasonable, albeit imperfect, proxy for brokerage trading revenues. Second, post-IPO turnover is important from a stabilization and turnover perspective. For these reasons, underwriters engage in quid-pro-quo arrangements in which they grant allocations in underpriced IPOs in exchange for a commitment of strong aftermarket purchase activity (see e.g., Griffin et al., 2007).

We empirically examine whether pre-IPO participation amplifies the association between optimistic reports and post-IPO trading revenue, using post-IPO share turnover in the initial trading days after the IPO (i.e., the first ten trading days) as a proxy for trading revenue. We implement this empirical test in Table 8, where we regress post-IPO share turnover on the triple interaction between affiliated analyst optimism and the post-JOBS × EGC interaction. Specifically, we estimate the following regression.

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

The coefficient of interest, β_6, is on 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 relative sensitivity of share turnover to affiliated analyst optimism (compared to unaffiliated optimism) changes surrounding the passage of JOBS, where one of the estimations uses a sample EGCs and the other a sample of non-EGCs.[31] 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 optimism differentially changes following JOBS for EGCs relative to non-EGCs. Broader effects, such as a

[31] 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.

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

We measure optimism, i.e., *Relative Optimism*, as the difference between the median forecast bias in affiliated and unaffiliated reports initiated during the first 60 days following the IPO. In an ideal specification, we would measure analyst optimism at the IPO date. However, since analysts do not issue their earnings forecasts before the quiet period expires, we must proxy for pre-IPO optimism with ex-post forecast optimism, which is revealed when the analyst report is released several weeks after the IPO. This proxy is reasonable because reputational considerations (and the fact that stock recommendations are typically aligned with earnings forecasts) make it unlikely that analysts will meaningfully change their outlook between pre-IPO discussions with investors and their coverage initiation.[32]

One potential drawback of this proxy is that it is subject to reverse causality concerns because the dependent variable is determined before the explanatory variable of interest. This precludes us from making causal statements. However, our triple difference specification helps mitigate such concerns with respect to our estimate of interest, which is β_6 in Equation (2). For example, a simple reverse causality explanation whereby post-IPO trading volume affects analyst optimism is unlikely to 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 × EGC × (Affiliated – Unaffiliated Optimism) triple interaction, post-IPO trading volume would have to be a particularly important input into the relative optimism of affiliated analysts for post-JOBS EGC analysts compared with analysts of other issuers.

In Columns 1 and 2 of Panel A of Table 8, we present the estimated Post-JOBS × EGC × Relative Optimism coefficients from Equation (2) using the natural log of post-IPO share turnover as the dependent variable (measured as the average share turnover between either days 2-10 or days 6-10 of trading). The significantly positive coefficients on the triple interactions in Columns 1 and 2 suggest that there is a more positive association between affiliated analyst optimism and post-IPO trading volume for post-JOBS EGCs, the issuers for which affiliated

[32] 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).

analysts are more involved in the IPO process. 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 turnover dependent variables in Columns 1 and 2 are 0.67 and 0.75, 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 is associated with a 0.5 unit (or two-thirds standard deviation) larger predicted increase in share turnover for EGCs relative to non-EGCs. The negative coefficient on the Post × Relative Optimism interaction highlights the importance of non-EGCs as a control sample of firms that are unaffected by JOBS but are otherwise similarly exposed to any market-wide changes that may affect this relation.[33]

In Column 3, we conduct a similar analysis using the natural log of share turnover during the ten days after the end of the quiet period as the dependent variable. Consistent with pre-IPO participation increasing institutions' ability to wait and sell into liquidity created by optimistic analyst reports, we find that affiliated analyst optimism is significantly more positively associated with post-quiet period share turnover for post-JOBS EGCs. The magnitude of the association is almost twice as large as when measuring share turnover in the ten days following the IPO. Although the possibility of reverse causality precludes us from identifying a causal relation, these findings are consistent with the idea that analysts rationally increase their optimistic bias because doing so generates more brokerage trading revenues as a result of the increased pre-IPO participation afforded by JOBS.

IPO participation may also increase the incentives for analysts to sacrifice their reputation for accuracy if this participation changes how analyst optimism affects IPO pricing. We posit that analyst optimism will be more positively related to the IPO offer price when institutions can more easily interact with analysts prior to the IPO. This is because institutions conceptualize analyst's optimism into their determination of an IPO share price bid.[34] Thus, we predict that the pre-IPO filing price revision (the percentage change from the midpoint of the IPO filing range to the offer price) will be positively associated with EGC affiliated analyst optimism.

[33] 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 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).

[34] Notably, even if we assume that sophisticated investors can see through the affiliated analysts' optimism (e.g., Michaely and Womack 1999), based on such pre-IPO interactions with analysts, they may be able to anticipate future demand from retail investors following the IPO.

Given the evidence in Hanley (1993) that only a portion of the positive information obtained during the book-building period enters into the offer price, we predict a positive relation between IPO underpricing and EGC affiliated analyst optimism. As with our tests of post-IPO trading volume, we cannot claim a causal effect of pre-IPO participation on the relation between analyst optimism and IPO pricing. Although we cannot show causation, this descriptive evidence suggests that analysts' increased influence over IPO pricing may be a motivating factor as to why they issue more optimistic reports following enhanced pre-IPO participation.

In Panel B of Table 8, we investigate how pre-IPO participation affects the association between affiliated analyst optimism and IPO pricing. We use our two measures of IPO pricing as dependent variables in Equation 2: the price revision, which measures the percentage change from the midpoint of the initial filing range to the IPO offer price and IPO underpricing, which measures the percentage change from the offer price to the first-day market closing price. In Column 1, the dependent variable is the price revision. The significantly positive Post-JOBS × EGC × Relative 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.

Because not all positive information is incorporated in the IPO offer price, we similarly observe a positive association between the optimism of EGC affiliated analysts and post-IPO underpricing (the sum of the pre-IPO offer price revision and post-IPO underpricing) in Column 2 (3) of Table 8, Panel B. Although we cannot show causation, we interpret these results as descriptive evidence consistent with pre-IPO participation of affiliated analysts increasing the influence of affiliated analyst optimism on investor demand.

6.3. The Costs and Benefits to Increased Optimism

Section 6.1 provides evidence that the more optimistic reports issued by EGC affiliated analysts since JOBS are less informative to investors when they are released, and investors who buy shares following the release of these reports lose between 3 and 4 percent of their investment by the firm's next earnings announcement. Thus, this increased optimism is costly to those who rely on analyst reports, most likely retail investors. However, investors who sell after the release of overly optimistic analyst reports, most likely institutional investors, benefit from affiliated analyst optimism because they are able to sell at a higher price.

In Section 6.2, we explored why increased pre-IPO involvement may lead analysts to become more willing to sacrifice their reputation for accuracy. One possible explanation is that analysts and banks benefit from the aforementioned wealth transfer from retail investors toward institutional investors, perhaps via increased reputation with their preferred institutional clients. Additionally, our evidence shows that increased pre-IPO analyst participation leads to a more positive relationship between optimistic reports and post-IPO trading volume, potentially benefitting both analysts and investment banks, as post-IPO trading volume is an input into both analyst compensation and investment banking revenues. Moreover, we find that increased pre-IPO analyst participation is more positively related to IPO pricing (i.e., via both a higher offer price and more IPO underpricing). Dambra, et al. (2015) provide evidence that underwriters charge post-JOBS EGCs the same 7% of IPO proceeds that has been paid by IPO issuers for decades (see e.g., Chen and Ritter, 2000; Hansen, 2001), thus making optimism more positively related to IPO underwriting revenues. Finally, any increase in IPO underpricing generated from optimistic analyst research is likely to increase the investment bank's reputation among its preferred clients (Beatty and Ritter, 1986; Reuter, 2006).

Our findings also suggest that the more positive relation between analyst optimism and IPO pricing brought on by increased pre-IPO analyst participation benefits IPO issuers in two important ways. First, optimism becomes more positively related to the IPO offer price, suggesting that optimistic reports are more likely to lower the issuer's cost of capital when analysts are involved in the IPO process. Second, analyst involvement makes optimism more positively related to issuer visibility in terms of post-IPO trading volume (Mehran and Perestiani, 2009).

7. Additional Analyses

Our main results above suggest that the JOBS Act has significantly affected analyst behavior and market responses to their research. We argue that these changes are a result of the JOBS Act permitting increased analyst participation in the IPO process. In the next 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 analyst participation in the IPO process, which is the motivation for our hypotheses. The exception to this is the Act's provision that eliminates 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.[35] 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, relative 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 volume does not directly relate 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 twelve of the largest investment banks with securities analysts. On March 15, 2010, regulators repealed a small set of provisions

[35] 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; and Field et al., 2005).

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 the 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 before their investment bank was hired. This is unlikely 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 the 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 analyst involvement in the IPO process allows optimistic managers of IPO firms to more directly influence analyst behavior, either through intentional manipulation or as an unintentional byproduct of management's optimistic outlook. We believe this is unlikely to be the primary driver of our findings because our evidence thus far suggests that there are several rational reasons for analysts to increase their optimism in response to increased IPO participation. Indeed, it would be fortuitous for analysts to be manipulated into being more optimistic in a situation where this increased optimism benefits the analyst, along with other IPO participants.

To 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 based on analyst experience and all-star status in OA-3 of the Online Appendix. The logic behind this test is that if increased analyst optimism is not a rational response to increased

IPO participation (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 × EGC × Affiliated triple interaction is more positive and only statistically significant within the sample of experienced analysts and roughly twice as large for three out of the four columns for all-stars analysts compared to non-all-star analysts. In unreported quadruple differencing tests, we find no significant quadruple interaction between Post-JOBS × EGC × Affiliated × Ln(1+Analyst Experience). The quadruple interaction between Post-JOBS × EGC × Affiliated × All-Star is sometimes significantly positive; however, the statistical significance depends on the empirical specification. These findings are not consistent with a story in which IPO participation allows optimistic managers to mislead 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. Exploiting 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 9 shows that EGC affiliated analyst research on IPOs becomes less accurate and more optimistic, with a more muted market response 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 analyst participation in the IPO process were relaxed.

8. Conclusion

In this paper, we use the JOBS Act, which increases analysts' participation in the IPO process, as a policy experiment to provide new evidence on how analyst involvement in the securities issuance process affects analyst research. Two features of the JOBS Act make it a less fettered setting to examine this question, relative to the existing literature. First, unlike prior legislative shifts, the JOBS Act changes the extent of analyst IPO participation 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). Second, the JOBS Act only targets certain analysts, which allows us to control for simultaneous changes in economic conditions.

Analysts treated with increased IPO participation initiate coverage that is less accurate, more optimistic, and accompanied by more muted market reactions at its release. However, investors purchasing shares following these initiations are harmed, losing over 3% of their investment by the firm's subsequent earnings release. While prior literature demonstrates that private communications improve the quality of analyst research (e.g., Soltes 2014; Brown et al., 2015), our findings suggest that analysts' increased involvement in the IPO process has the opposite effect.

We discuss several ways in which increasing analyst involvement in the IPO process may enhance analysts' and investment banks' incentives to produce optimistic research. We find empirical evidence consistent with two such channels, as pre-IPO involvement of affiliated analysts is associated with a more positive relation between analyst optimism and both post-IPO trading volume (which increases brokerage revenues) and IPO pricing (which, via a higher IPO offer price, results in higher underwriter fees). Issuers also benefit more from optimism when analysts are more involved in the IPO process. Not only does the higher IPO offer price reduce their cost of capital, but we also find that optimism becomes more positively related to IPO underpricing, which can increase firm visibility. Thus, we extend the literature on how analysts add value through investor recognition (e.g., Hansen, 2015; Li and You, 2015).

Although certain investors benefit from analysts being re-integrated into the IPO process, a subset of investors that rely on the information content of analyst reports appear worse off. Our findings are especially 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 regarding substantial changes to the analyst regulatory environment. These findings raise the possibility that deregulation designed to further integrate analysts into the IPO process may have adverse unintended consequences, such as overly optimistic research or further tilting the playing field in favor of large institutional investors.

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 interactions 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. Chaplinksy 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. (2017) 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 company 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

	Pre-JOBS Act	Post-JOBS Act	
May research personnel…	**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	*Permitted*	Prohibited
Publish research reports concerning issuers that are the subject of *any* public offering of common equity securities (even if the firm is participating in the offering)?	Prohibited	*Permitted*	Prohibited
Participate in meetings with representatives of an issuer, attended by investment banking personnel?	Prohibited	*Permitted*	Prohibited
Contact potential investors in an issuer's IPO?	Prohibited	*Permitted*	Prohibited
Make public appearances concerning the securities of an issuer?	Prohibited	*Permitted*	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)
Outcome Variables	
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.
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 $\frac{\text{Forecast}_{\text{it}} - \text{Actual}_{\text{i}}}{\text{Price}_{\text{i,t}-1}} \times 100$, 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 $-1\times$ |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).
Style-Adjusted Returns	The firm's return minus the return of a matched seasoned firm based on size and book-to-market, over the period beginning five days following the first affiliated report through the day following the first post-report earnings announcement. The matched firm is matched to an IPO issuer by selecting a seasoned firm (trading for at least five years) that lies within the same market capitalization decile as the issuer and has the closest book-to-market ratio. If this matched firm stops trading before the end of the holding period, we use the second best match to complete the period. The issuer's market capitalization is computed using the issuer's price and shares outstanding as of the first post-IPO filing and the seasoned firm's market capitalization is as of the closest month-end prior to this date. Book-to-market is computed as the book value of equity from COMPUSTAT scaled by the market capitalization figure used for each firm. The issuer's book value of equity is from the first post-IPO filing, and the seasoned firm's book value of equity is from the most recent quarterly filing on or before this date.
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.

(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).

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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 involvement for EGC affiliated analysts (since April 5, 2012).

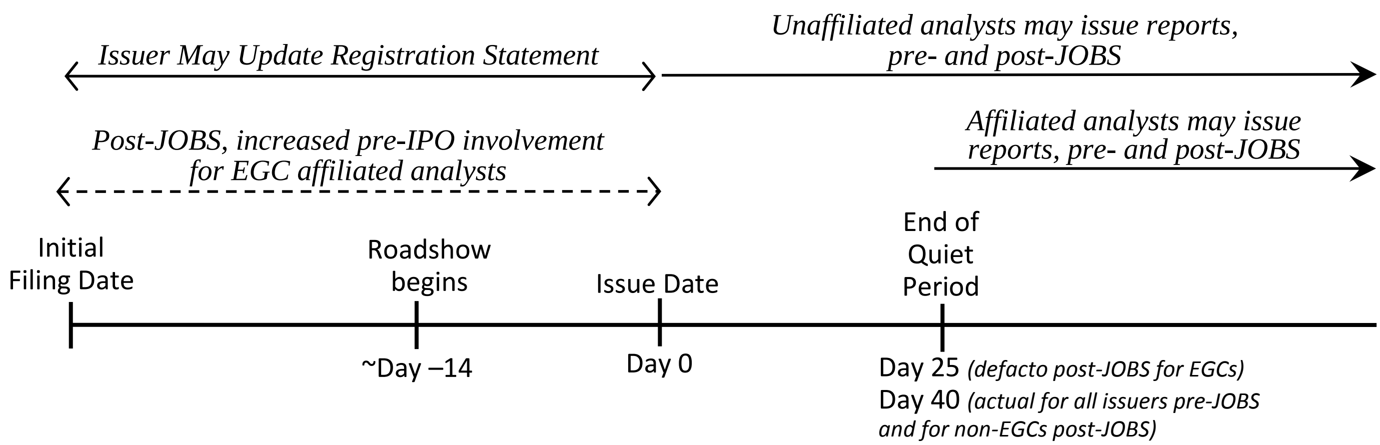
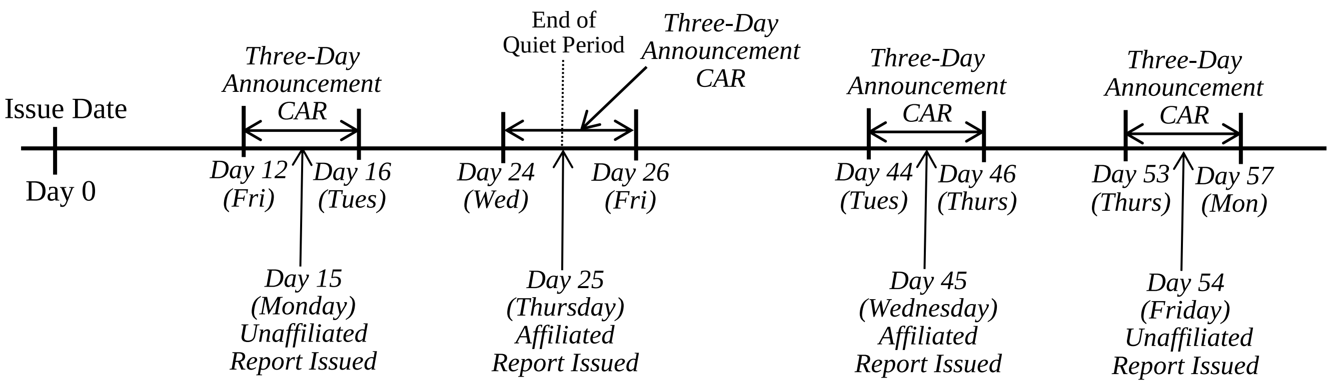


Figure 2

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

Panel A presents a graphical depiction of how three-day cumulative abnormal returns (CARs) for analyst report releases are measured for four hypothetical analyst report announcements: on days 15, 25, 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). Panel B demonstrates how abnormal returns following release of the first affiliated analyst's report through the earnings announcement by the firm are measured.

Panel A: Three Day Returns Surrounding Release of Analyst Reports



Panel B: CARs After Release of First Affiliated Analyst Report through Firm's Earnings Announcement

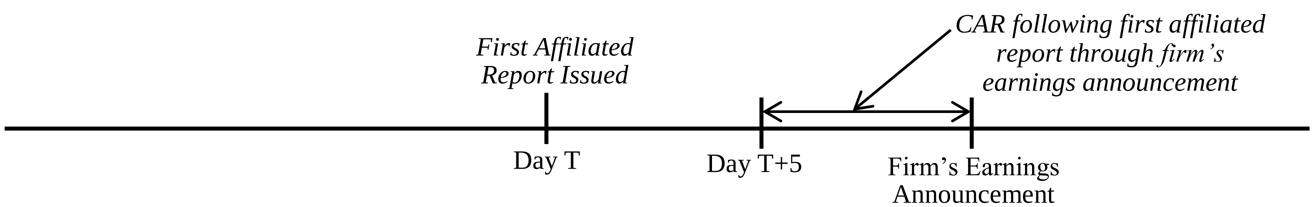


Figure 3

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 indicates the post-JOBS period, which in our sample is comprised of IPOs between November 11, 2012 and June 30, 2014.

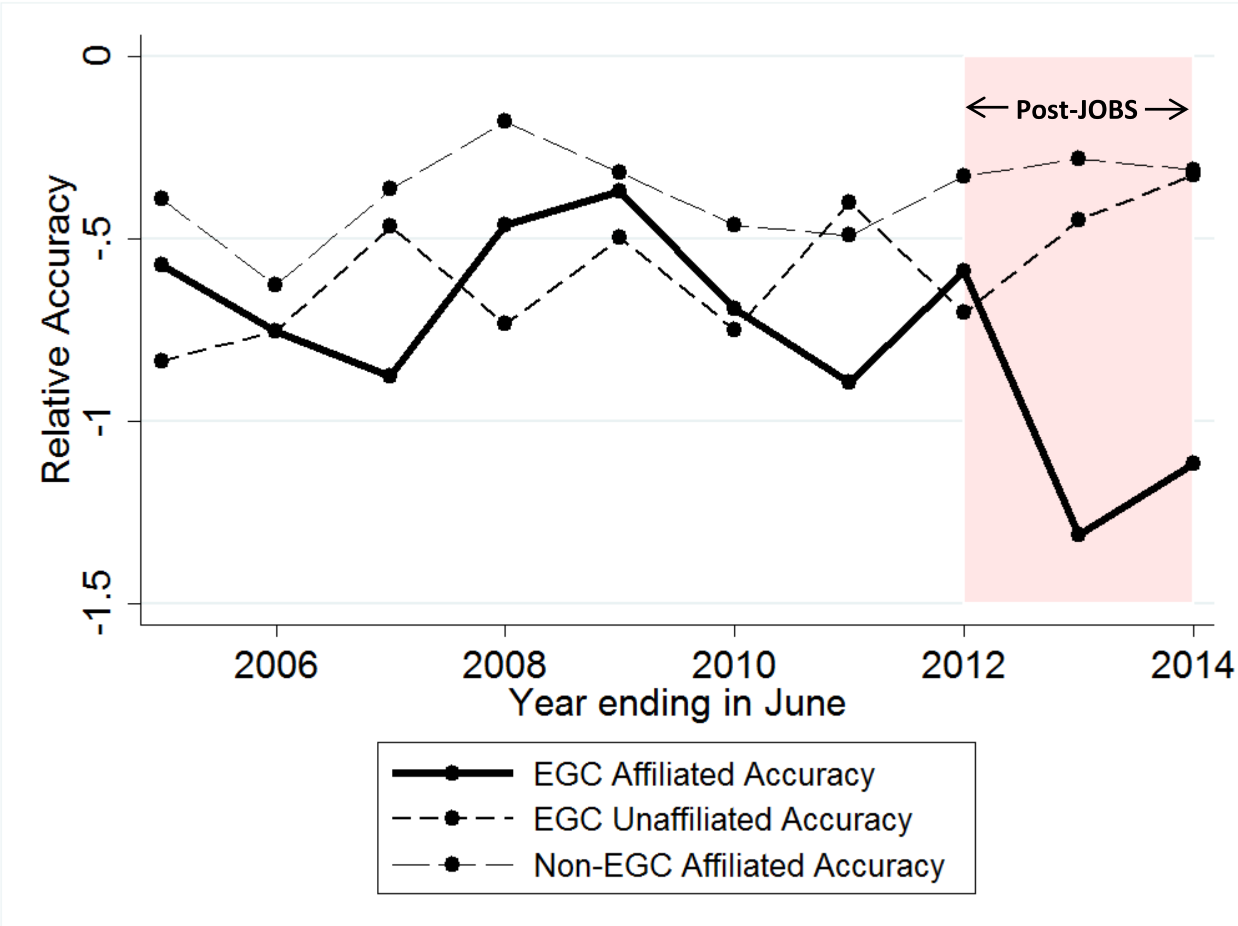


Figure 4

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.

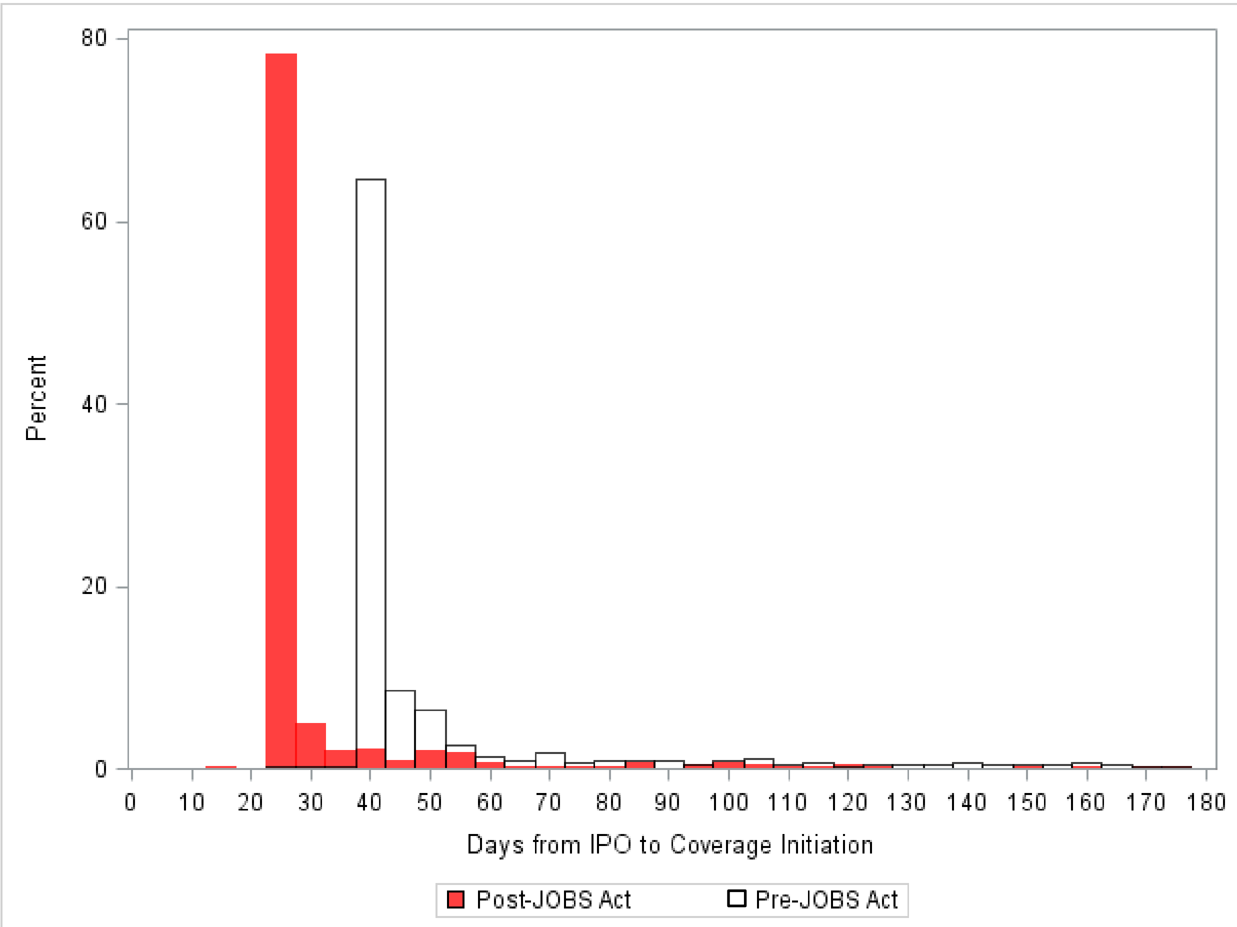


Table 1
Sample Descriptive and Dependent Variable Statistics

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, Panel B presents analyst report-related outcomes, and Panel C presents firm-related outcomes. 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 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 Analyst-Related Dependent Variables

	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
Market Response:				
Three-day CAR (Affiliated)	2.70	2.50	1.27	1.59
Three-day CAR (Unaffiliated)	1.55	2.56	0.57	1.20

Panel C: Averages of Firm-Related Dependent Variables

	EGCs		Non-EGCs	
	Pre-JOBS	Post-JOBS	Pre-JOBS	Post-JOBS
Market Response:				
Post-Quiet Period CARs	-0.32	-3.68**	-0.74	2.50
Firm Pricing and Trading Volume:				
Initial Post-IPO Turnover	11.98	12.58	12.72	10.13
Ln(Initial Post-IPO Turnover)	2.21	2.24	2.33	2.19
Post-Quiet Period Turnover	6.80	8.26	7.43	5.15
Ln(Post-Quiet Period Turnover)	1.34	1.53***	1.52	1.31
Price Revisions	-5.53	-4.42	-6.00	-1.89
Underpricing (%)	13.30	21.14***	6.21	13.76**

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, 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.

	Means for:			
	EGCs		Non-EGCs	
Characteristic	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
Pre-IPO Market Return (%)	4.27	4.91	5.95	5.80
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
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	Means for:			
Matching Variables:	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 3 and 4 use propensity score matched samples. Column 3 matches 78 Post-JOBS EGCs with 78 Pre-JOBS issuers that are below the $1 billion revenue cutoff and matches 29 Post-JOBS non-EGCs to 29 Pre-JOBS non-EGCs. The number of matches for Column 4 are 207 EGCs and 37 Non-EGCs. The variables used in the PSM estimation 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.

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

(continued)

Table 4 (continued)

	Relative Accuracy: Affiliated – Unaffiliated			*Accuracy*: Affiliated Only
	Full Sample (1)	60-day Sample (2)	PSM (3)	PSM Affiliated (4)
Analyst/Brokerage Characteristics:				
Number Days Before Report	-0.003	-0.005*	-0.006**	0.005
	(-1.28)	(-1.73)	(-2.01)	(1.57)
Number Affiliated Analysts	-0.005	-0.020*	-0.014	0.034
	(-0.38)	(-1.87)	(-0.78)	(1.21)
Number Unaffiliated Analysts	-0.006	0.005	0.037	-0.035
	(-0.35)	(0.38)	(1.29)	(-1.16)
Percent Affiliated Analysts	-0.332	0.166	0.787	-0.776
	(-0.83)	(0.55)	(1.21)	(-1.55)
Ln[Analyst Experience (Years)]	-0.171	-0.083	-0.179	-0.167
	(-1.47)	(-0.81)	(-1.10)	(-1.14)
Ln(Analyst Coverage)	0.148	0.140	-0.069	-0.065
	(0.92)	(0.78)	(-0.26)	(-0.25)
Days from Report to Earnings	-0.004***	-0.002*	-0.006**	-0.012***
	(-3.21)	(-1.77)	(-2.42)	(-5.12)
Historical Average Optimism	0.141	-0.188	0.310	0.684**
	(0.52)	(-1.18)	(0.85)	(2.42)
Historical Average Accuracy	0.277*	-0.007	0.049	0.724***
	(1.66)	(-0.08)	(0.20)	(3.59)
Ln(Brokerage Size (Analysts))	0.006	-0.090	0.220*	0.086
	(0.08)	(-1.53)	(1.69)	(0.70)
IPO Characteristics:				
Ln(Proceeds)	0.066	0.032	0.109	0.203
	(1.12)	(0.68)	(1.38)	(1.47)
Underpricing	0.002	0.096	-0.012	0.478*
	(0.01)	(0.70)	(-0.04)	(1.82)
Market Conditions:				
Pre-IPO Market Return	0.008	0.002	0.002	0.011
	(1.06)	(0.57)	(0.27)	(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 3 matches 78 EGCs and 29 non-EGCs; the PSM model in Column 4 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.

	Relative Bias: Affiliated – Unaffiliated			*Bias*: Affiliated Only
	Full Sample	60-day Sample	PSM	PSM
	(1)	(2)	(3)	(4)
Post × EGC	**0.463****	**0.602*****	**0.765*****	**0.570****
	(2.22)	**(3.68)**	**(3.57)**	**(2.17)**
EGC	-0.299**	-0.248**	-0.710***	-1.137***
	(-2.08)	(-2.39)	(-3.63)	(-4.73)
Number Days Before Report	0.003	0.010***	0.003	-0.002
	(1.11)	(2.81)	(0.78)	(-0.58)
Number Affiliated Analysts	0.009	0.020	0.025	-0.027
	(0.49)	(1.46)	(1.02)	(-0.85)
Number Unaffiliated Analysts	-0.001	0.000	0.005	0.033
	(-0.06)	(0.03)	(0.18)	(0.98)
Percent Affiliated Analysts	0.089	0.127	0.087	0.383
	(0.18)	(0.38)	(0.13)	(0.70)
Ln[Analyst Experience (Years)]	0.062	-0.099	-0.248	0.224
	(0.50)	(-1.01)	(-1.20)	(1.32)
Ln(Analyst Coverage)	0.157	0.118	0.111	-0.090
	(0.87)	(0.70)	(0.37)	(-0.32)
Days from Report to Earnings	0.002	0.001	0.004	0.006**
	(1.11)	(0.58)	(1.63)	(2.52)
Historical Average Optimism	-0.396	0.245	-0.359	-0.192
	(-1.48)	(1.46)	(-0.96)	(-0.64)
Historical Average Accuracy	-0.202	0.131	-0.084	-0.271
	(-1.44)	(1.58)	(-0.39)	(-1.37)
Ln[Brokerage Size (Analysts)]	0.034	0.035	-0.102	-0.070
	(0.38)	(0.49)	(-0.70)	(-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 the median three-day CAR for affiliated and unaffiliated analysts within the same firm. 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 43 EGCs and 14 non-EGCs. All remaining variables (Firm, IPO, and Market controls) are identical to those shown in Table 4 and 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.

	Relative CARs: Unscaled		*Relative CARs*: Scaled	
	Full Sample (1)	PSM (2)	Full Sample (3)	PSM (4)
Post × EGC	**-6.289***** **(-2.85)**	**-7.275**** **(-2.19)**	**-4.231**** **(-2.45)**	**-4.057*** **(-1.79)**
EGC	3.399** (2.46)	-0.280 (-0.08)	1.435 (1.27)	-0.904 (-0.36)
Number of Days Before Report	0.002 (0.07)	-0.045 (-1.08)	-0.005 (-0.23)	-0.008 (-0.29)
Number of Affiliated Analysts	-0.107 (-0.34)	-0.829 (-1.48)	-0.117 (-0.58)	-0.311 (-0.76)
Number of Unaffiliated Analysts	0.796 (1.32)	1.058 (1.10)	0.439 (1.12)	0.240 (0.38)
Percent Affiliated Analysts	5.585 (0.64)	6.256 (0.45)	3.279 (0.54)	0.970 (0.10)
Ln(Analyst Experience)	0.450 (0.35)	1.396 (0.61)	-0.178 (-0.18)	-0.185 (-0.11)
Ln(Analyst Coverage)	-2.365 (-1.55)	-0.143 (-0.05)	-2.839** (-2.18)	-1.435 (-0.60)
Historical Average Analyst CAR	0.260 (0.26)	-2.452 (-1.40)	0.782 (1.03)	-0.491 (-0.39)
Ln(Brokerage Size)	1.044 (1.18)	-1.868 (-1.02)	0.512 (0.76)	-1.469 (-1.11)
Firm, IPO, and Market Controls	Yes	Yes	Yes	Yes
Adjusted R-squared	-0.011	0.160	-0.006	0.040
Number of Observations	351	114	351	114

Table 7
CARs between Affiliated Initiations and Subsequent Earnings Announcement

Panel A reports average and median firm cumulative returns (both raw and adjusted), and the percentage of firms with negative cumulative returns between the release of the first affiliated analyst report and the first post-report earnings announcement for EGC issuers, partitioned by whether the IPO occurs in the pre- or post-JOBS periods. To account for the fact that multiple affiliated analysts tend to initiate coverage in a condensed window following the end of the post-IPO quiet period (see Figure 4), we begin the return window five trading days following the first affiliated report. To allow for a full market response to the earnings announcement we end the window one day after the firm's subsequent earnings announcement. All issuers with EPS announcements between the release of the first report and the fifth trading day following the report are excluded from the analysis, leaving 815 pre and post-JOBS EGC issuers. Market-adjusted returns are computed as the firm's return minus the return of the CRSP value-weighted market index. Style-adjusted returns are computed as the firm's return minus the return of a matched seasoned firm based on size and book-to-market. The matched firm is matched to an IPO issuer by selecting a seasoned firm (trading for at least five years) that lies within the same market capitalization decile as the issuer using the issuer's price and shares outstanding as of the first post-IPO filing and the seasoned firm's price and shares as of the closest month end, and has the closest book-to-market ratio, computed using the book value of equity from the first post-IPO filing and the most recent quarterly filing for the seasoned firm. Panel B reports OLS regressions estimating the post-report returns for post-JOBS EGCs relative to both non-EGCs and pre-JOBS EGCs. The dependent variables in Columns 1-3 are cumulative raw returns, cumulative market-adjusted returns, and cumulative style-adjusted returns over this window for each issuer. The first two columns of Panel A compute t-tests for differences in means from 0, and Column 3 of Panel A computes a difference in means t-test between Columns 1 and 2 using White (1980) robust standard errors. *t*-statistics using White (1980) robust standard errors are reported in parentheses in Panel B. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: EGC CARs between Report Release and EPS Announcement

	Pre-JOBS	Post-JOBS	Difference
Raw Returns	0.77%	-2.35%	-3.12%*
Market-Adjusted Returns	-0.32%	-3.68%**	-3.37%**
Style-Adjusted Returns	0.06%	-3.71%**	-3.77%**
Median Raw Returns	0.29%	-2.09%***	-2.37%***
Percentage of Negative Raw Returns	49.00%	59.60%***	10.60%***

Panel B: Regression Analysis of CARs between Report Release and EPS Announcement

	Cumulative Raw Return	Market-Adjusted Returns	Style-Adjusted Returns
	(1)	(2)	(3)
Post × EGC	**-8.549****	**-6.599****	**-8.689****
	(-2.56)	**(-2.05)**	**(-2.02)**
Post	5.429*	3.232	4.917
	(1.90)	(1.17)	(1.27)
EGC	0.794	0.420	1.303
	(0.39)	(0.22)	(0.57)
Adjusted R-squared	0.003	0.003	0.002
Number of Observations	930	930	928

Table 8
Turnover, Pricing, and Optimism

This table presents OLS regressions estimating relative changes in share turnover and pricing following JOBS for EGC IPO firms, as a function of affiliated analyst optimism. 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 10^{th} trading day post-IPO, Column 2 from the sixth through the tenth trading day following the IPO, and Column 3 over the ten 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***	**0.579****	**1.110*****
	(1.68)	**(2.56)**	**(3.55)**
EGC	0.227*	0.149	-0.017
	(1.80)	(1.00)	(-0.10)
Relative Optimism	-0.007	0.040	0.218
	(-0.07)	(0.31)	(1.54)
Post × EGC	-0.004	0.060	0.517**
	(-0.02)	(0.30)	(2.20)
EGC × Relative Optimism	-0.020	-0.070	-0.278*
	(-0.19)	(-0.50)	(-1.81)
Post × Relative Optimism	-0.257	-0.448**	-1.002***
	(-1.42)	(-2.19)	(-3.76)
Firm, IPO, and Market Controls	Yes	Yes	Yes
Adjusted R-squared	0.335	0.249	0.076
Number of Observations	363	363	363

Table 8 (continued)

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
Number of Observations	363	363	363

Table 9

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 analyst-level forecast accuracy (see Table 4 for definition); Column 2 measures the relative change in analyst-level forecast bias (see Table 5 for definition); and Column 3 measures the relative change in analyst-level three-day recommendation announcement CARs using the unscaled cumulative returns (see Columns 1–2 of Table 6 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.

	Accuracy (1)	*Bias* (2)	*Three-Day CAR* (3)
Post × IPO	**-0.299*****	**0.163****	**-1.171*****
	(-3.75)	**(2.20)**	**(-2.91)**
IPO	0.081*	0.033	-1.554*
	(1.82)	(0.70)	(-1.83)
Ln(Total Assets)	0.081***	-0.030	-0.469***
	(4.32)	(-1.63)	(-3.01)
Ln(Revenue)	-0.004	0.018	-0.045
	(-0.31)	(1.16)	(-1.18)
Ln(Tobin's Q)	0.456***	-0.201***	-0.506
	(13.92)	(-6.07)	(-0.89)
Leverage	-0.179***	0.060	-0.128**
	(-3.65)	(0.83)	(-2.16)
Return on Assets	0.906***	-0.729***	-0.020
	(9.20)	(-6.12)	(-0.18)
Operating at Loss	-0.286***	-0.068**	1.381***
	(-11.43)	(-2.46)	(2.81)
High-Tech	0.051*	0.024	-0.668
	(1.93)	(1.14)	(-1.57)
Market Return	0.043	0.146	1.911
	(0.12)	(0.47)	(0.59)
Ln(Analyst Experience)	0.071***	-0.040	0.210***
	(3.36)	(-1.45)	(2.67)
Ln(Analyst Coverage)	0.183***	-0.004	-0.743***
	(7.73)	(-0.14)	(-3.05)
Ln(Brokerage Size)	-0.048	0.020	0.247
	(-1.14)	(0.32)	(1.58)
Horizon	-0.001***	0.001***	
	(-6.24)	(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

Online Appendix for
The Consequences to Involving Analysts in the IPO Process:
Evidence Surrounding the JOBS Act

December, 2017

1. Introduction

This online appendix provides supplementary analyses to Dambra, et al. (2017). Specifically, this appendix replicates the within-firm analyses presented in the paper for *Relative Accuracy*, *Relative Bias*, and *Relative CAR* at the analyst level. The online appendix proceeds as follows. In Section 2, we provide our empirical design of our analyst level tests. In Section 3, we report our results from these analyses.

2. Forecast Level Identification Strategy

The JOBS Act targets only affiliated analysts of EGC firms. Therefore, 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. To complement our within-firm analysis in the paper, we also consider the effect of JOBS using a triple-differencing approach, as presented in Equation (OA-1) below. We regress analyst j's outcome of interest for firm i on an indicator for affiliated analysts, an indicator for EGC firms, a full complement of interactions between these indicators and the post-JOBS period, and control variables including Fama-French 48 industry and year-quarter fixed effects:

$$\begin{aligned}\text{Outcome}_{ij} = {} & \beta_0 + \beta_1\text{Affiliated}_{ij} + \beta_2\text{EGC}_i + \beta_3\text{Post-JOBS}_i \times \text{Affiliated}_{ij} + \beta_4\text{Post-JOBS}_i \times \\ & \text{EGC}_i + \beta_5\text{EGC}_i \times \text{Affiliated}_{ij} + \boldsymbol{\beta_6\textbf{Post-JOBS}_i \times \textbf{EGC}_i \times \textbf{Affiliated}_{ij}} + \text{Year-Qtr FEs} + \\ & \text{Industry FEs} + \text{Controls} + \varepsilon_{ij}. \qquad \text{(OA-1)}\end{aligned}$$

The coefficient of interest, $\beta_{6,}$ relates to the triple-interaction between the affiliated, EGC, and post-JOBS indicators. To interpret this coefficient, one can think of the above specification as two separate difference-in-differences estimations, each estimating the differential change in behavior between affiliated and unaffiliated analysts surrounding the passage of JOBS, but one of the estimations occurring only in a sample EGCs and the other only in a sample of non-EGCs. This will be a close approximation of Equation (1) in the main analysis; however, the estimate in Equation (OA-1) will differ slightly because it restricts the coefficients on control variables to be the same for EGCs and non-EGCs. The coefficient in OA-1 above, β_6, estimates the difference between these two difference-in-differences estimates. Put differently, β_6 estimates the difference between how affiliated analyst output changes relative to unaffiliated analyst output for EGCs relative to non-EGCs following the passage of JOBS. Broader effects, such as a post-JOBS change in all EGC analysts' behavior, are controlled for with the other interaction terms.

2.1 Analyst Level Variable Descriptions and Sample

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. We define *Bias* as $\frac{\text{Forecast}_{i,t} - \text{Actual}_i}{\text{Price}_{i,t-1}} \times 100$. Clearly, *Accuracy* and *Bias* for our forecast level tests are mechanically related. However, they are not highly correlated, as they have a Spearman correlation of 0.15. Although the value of *Accuracy* does not significantly predict *Bias* (for any accuracy value, expected bias is close to zero), the value of *Bias* perfectly maps to *Accuracy*. To break this mechanical link, we also use an *Optimistic Bias Indicator*, which equals one for an analyst forecast with positive bias and zero for a forecast with zero or negative bias. There is no mechanical link between this measure and accuracy. Finally, we use a third optimism measure, *Optimistic Component of Bias*, which equals the maximum of zero or *Bias*. Our *Accuracy* and *Bias* tests provide a sample of 5,862 reports for 1,035 unique issuers.

To proxy for report informativeness, we use the analyst level three-day market-adjusted cumulative abnormal return (*Three-day CAR*) surrounding the date of an analyst's coverage initiation. As in the firm-level tests, we remove observations announced on days with conflicting reports (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). Refer to section 4.4 of the paper for additional details. Our *Three-day CAR* tests provide a sample of 3,257 reports. In addition to the three-day CAR analyses, we run additional forecast level tests on a two-hour window using intraday Trade and Quote (TAQ) data. Not only are these two-hour returns more likely to capture the information generated by analyst reports, as opposed to confounding market events, but it is also less likely that we will observe conflicting reports within such a narrow window. Thus, we are able to retain more observations using the intraday TAQ data because we only drop conflicting recommendations that occur within a two-hour interval. A limitation of the intraday analysis is that we have TAQ data only through the end of 2013, restricting our sample size.

3. Analyst Level Results

3.1. Accuracy

In Table OA-1, we investigate the effect of reintegrating analysts into the IPO process on analyst accuracy at the forecast level, rather than at the firm level (as in Table 4 of the paper), by utilizing the triple-differencing framework shown in Equation (OA-1). Consistent with the within-firm results, we find a significant decline in EGC affiliated analyst accuracy following JOBS in the analyst-level tests. As a first step toward interpreting the magnitude of this effect, we compare how the relative accuracy of affiliated and unaffiliated analysts changes surrounding JOBS for EGCs. Summing the Affiliated coefficient of 0.104 and the EGC × Affiliated coefficient of -0.051 indicates that EGC affiliated forecasts were a statistically insignificant 0.05% of price more accurate than EGC unaffiliated analysts prior to JOBS. Adding the Post × Affiliated coefficient of -0.084 and the Post × EGC × Affiliated coefficient of -0.322 suggests that EGC affiliated analysts become approximately 0.41% of price less accurate compared to EGC unaffiliated analysts following JOBS. This change is significantly larger than that observed among non-EGCs. Prior to JOBS, the affiliated coefficient indicates that non-EGC affiliated analysts were 0.1% of price more accurate than non-EGC unaffiliated analysts, while after JOBS this gap decreases by 0.08% of price. The statistically significant triple interaction coefficient of -0.322 demonstrates that the post-JOBS decline in relative accuracy between affiliated and unaffiliated analysts was larger for EGCs than for non-EGCs. The 0.32% of price decline in accuracy is economically large, representing an almost 50% reduction in accuracy as compared to pre-JOBS EGC affiliated average accuracy. Columns 2 through 4 show that these results are robust to the inclusion of brokerage fixed effects and using the matched sample.

3.2. Optimism

The triple differencing results in Table OA-2 at the forecast level corroborate our firm-level evidence in Table 5 of the paper. The coefficients on the Post-JOBS × EGC × Affiliated triple interaction term in Columns 1 through 4 suggest that EGC affiliated forecast bias increases relative to other analysts by about the same amount as EGC affiliated analysts' accuracy deteriorates (similar to the firm-level results). As above, this result is virtually unchanged by matching (Columns 2 and 4) or the inclusion of brokerage fixed effects (Columns 3 and 4). In Columns 5 and 6, we replicate our optimism analyses using an indicator for an optimistically biased forecast as the dependent variable. Using this alternative measure of analyst optimism, we continue to find evidence that EGC affiliated analysts provide more optimistic earnings forecasts following JOBS. After JOBS, EGC affiliated analysts become approximately 25% more likely to initiate coverage with an optimistically biased earnings forecast. Finally, Columns 7 and 8 confirm this result using a bias measure that equals zero for negatively biased reports and equals the actual forecast bias for positively biased reports.

3.2.1. Additional Sample Partitions for Analyst Optimism

We attribute the observed increase in analyst forecast bias for post-JOBS EGC affiliated analysts to an increase in the incentives for such analysts to produce more optimistic research following increases in permissible IPO involvement. However, one possible alternative explanation is that IPO involvement increases the ability of optimistic managers of IPO firms are better able to influence analyst behavior, either through intentional manipulation or as an unintentional byproduct of management's optimistic outlook. We provide further discussion and analysis of this alternative explanation in Section 7.4.1 of the paper. If the increase to analyst forecast bias is not a rational response to increased pre-IPO involvement (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 either inexperienced analysts or non-all-star analysts. To examine this possibility we partition our triple difference analysis of forecast bias on the experience and all-star status of analysts.

We define experience as the number of years that the analyst has been issuing forecasts in the I/B/E/S detail forecast file. In Panel A (B) of Table OA-3, we restrict our sample to only those analysts with below or equal to (above) median analyst experience. We find that the Post-JOBS × EGC × Affiliated triple interaction is more positive and only statistically significant within the sample of experienced analysts. In Panel C of Table OA-3, we restrict our sample to analysts who are not recognized by *Institutional Investor (II)* as all-stars in the year of the forecast; Panel D is restricted to analysts recognized as all-stars in the year of the forecast. In Panels C and D of Table OA-3, we find that the Post-JOBS × EGC × Affiliated triple interaction is more positive within the sample of all-star analysts. We find it unlikely that the subset of analysts being deceived by management would be concentrated in either more experienced or higher quality analysts. Therefore, our collective results from these sample partitions are not consistent with optimistic managers being better able to mislead naïve analysts involved in the IPO.

3.3. Announcement Returns

Table OA-4 reports our analyst-level tests using either the unscaled or scaled *Three-day CAR* as the dependent variable. 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 × EGC × Affiliated interaction. Alternatively, we may find the opposite effect if EGC affiliated analyst reports incorporate qualitative information garnered from increased IPO involvement.

The negative interaction coefficient in Table OA-4 corroborates our finding in Table 6 of the paper that EGC affiliated analyst reports have become less informative since EGC affiliated analysts have been allowed more involvement in the IPO. After scaling by the number of reports, the full and matched sample triple interaction coefficients in Columns 3 and 4 of Table OA-4 are -2.698 and -3.436, respectively. The coefficient in Column 3 can be decomposed into several effects. Summing the Affiliated and EGC × Affiliated coefficients (of -0.156 and 0.619, respectively) suggests that EGC affiliated analyst reports garnered a 0.463% larger market reaction prior to JOBS than did unaffiliated EGC analysts. This difference is statistically insignificant. Further, adding the Post × Affiliated and Post × EGC × Affiliated coefficients indicates that following JOBS, this difference declines by 1.989%, indicating that EGC affiliated analyst reports garner a 1.526% more muted market reaction compared to their unaffiliated counterparts (i.e., 0.463% – 1.989%). The post-JOBS reduction in the relative accuracy of affiliated analysts is unique to EGCs. The insignificant Post × Affiliated coefficient of 0.709 suggests that the relative CAR surrounding affiliated and unaffiliated reports for non-EGCs did not significantly change following the passage of JOBS.

Columns 5 and 6 of Table OA-4 restrict the sample to buy recommendations, reducing the sample by 905 reports (i.e., from 3,257 to 2,352 reports) for the non-matched sample and 355 (i.e., from 1,479 to 1,124 reports) reports for the PSM-matched sample. The fact that our results hold within this sample suggests that the market anticipates the increased optimistic bias of post-JOBS EGC affiliated analysts and reacts less favorably. Columns 7 and 8 investigate CARs in the two hours following an analyst's coverage initiation. These intraday findings corroborate the interday results.

Table OA-1
Analyst-Level Forecast Accuracy

This table presents OLS regressions measuring a triple difference in analyst forecast accuracy, where forecast accuracy is calculated as $-1 \times \left|\frac{\text{Forecast}_{it}-\text{Actual}_{i}}{\text{Price}_{i,t-1}} \times 100\right|$. The triple interaction measures the differential change in forecast accuracy for affiliated versus unaffiliated analysts, incremental to the relative change in accuracy for EGCs versus non-EGCs following JOBS. All remaining variables are defined in Appendix B. Firm, IPO, and Market Controls refer to the respective control variables shown in Table 4 under these headings. PSM refers to the propensity score matched sample. 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 analyst and firm levels are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Analyst Accuracy			
	Full Sample (1)	PSM (2)	Full Sample (3)	PSM (4)
Post × EGC × Affiliated	**-0.322****	**-0.312****	**-0.340****	**-0.386****
	(-2.34)	**(-2.12)**	**(-1.98)**	**(-2.10)**
Post × EGC	-0.251*	-0.239	-0.253	-0.213
	(-1.70)	(-1.51)	(-1.07)	(-0.88)
EGC	0.273**	0.232*	0.334	0.302
	(2.28)	(1.80)	(1.40)	(1.24)
Post × Affiliated	-0.084	-0.050	-0.021	0.075
	(-0.90)	(-0.46)	(-0.18)	(0.58)
Affiliated	0.104	0.072	0.043	-0.067
	(1.50)	(0.83)	(0.45)	(-0.58)
EGC × Affiliated	-0.051	-0.041	0.046	0.105
	(-0.59)	(-0.43)	(0.36)	(0.71)
Number Days Before Report	0.002***	0.002***	0.003***	0.002***
	(6.13)	(5.80)	(4.37)	(3.93)
Number Affiliated Analysts	0.038***	0.039***	0.036*	0.038**
	(3.51)	(3.67)	(1.90)	(2.04)
Number Unaffiliated Analysts	-0.027**	-0.028**	-0.024	-0.023
	(-2.31)	(-2.43)	(-1.43)	(-1.36)
Percent Affiliated Analysts	-0.613***	-0.644***	-0.491	-0.515
	(-3.13)	(-3.30)	(-1.50)	(-1.54)
n[Analyst Experience (Years)]	0.004	-0.003	0.031	0.021
	(0.20)	(-0.13)	(1.09)	(0.69)
Ln(Analyst Coverage)	0.014	0.026	-0.041	0.007
	(0.65)	(1.13)	(-1.13)	(0.17)
Days from Report to Earnings	-0.005***	-0.005***	-0.006***	-0.006***
	(-6.25)	(-6.43)	(-4.45)	(-4.51)
Historical Average Optimism	0.117**	0.147***	0.122	0.212**
	(2.19)	(2.75)	(1.31)	(2.37)
Historical Average Accuracy	0.144***	0.163***	0.177***	0.244***
	(3.81)	(4.13)	(2.82)	(3.83)
Ln[Brokerage Size (Analysts)]	0.004	-0.018	0.005	0.103
	(0.26)	(-0.29)	(0.25)	(0.83)
Firm, IPO, and Market Controls	Yes	Yes	Yes	Yes
Brokerage Fixed Effects	No	No	Yes	Yes
Adjusted R-squared	0.335	0.383	0.334	0.381
Number of Observations	5,862	2,825	5,862	2,825

Table OA-2
Analyst-Level Forecast Bias

This table presents OLS regressions measuring a triple difference in analyst forecast bias. The dependent variable in Columns 1–4 is forecast bias, defined as $\frac{\text{Forecast}_{it}-\text{Actual}_{i}}{\text{Price}_{i,t-1}} \times 100$. In Columns 5 and 6, the dependent variable is an indicator for a positive (or optimistic) forecast bias (i.e., forecast greater than reported earnings). In Columns 7 and 8, the dependent variable is a continuous measure of the optimistic component to forecast bias, which is constructed by setting negative forecast bias values to zero. Odd numbered columns use our full sample, while even numbered columns use our propensity score matched (PSM) sample. The triple interaction measures the differential change in forecast bias for affiliated versus unaffiliated analysts, incremental to the relative change in forecast bias for EGCs versus non-EGCs following JOBS. All control variables used in Table 5 are included, and Appendix B contains a complete list of explanatory variable definitions. PSM refers to the propensity score matched sample. 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 analyst and firm levels are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Forecast Bias				Optimistic Bias Indicator		Optimistic Component of Bias	
	Full Sample (1)	PSM (2)	Full Sample (3)	PSM (4)	Full Sample (5)	PSM (6)	Full Sample (7)	PSM (8)
Post × EGC × Affiliated	**0.401****	**0.453****	**0.405****	**0.485****	**0.228*****	**0.258****	**0.322****	**0.382****
	(2.51)	**(2.13)**	**(2.35)**	**(2.03)**	**(2.60)**	**(2.53)**	**(2.48)**	**(2.10)**
Post × EGC	-0.237	-0.021	-0.256	-0.026	-0.090	-0.091	-0.040	0.086
	(-1.37)	(-0.07)	(-1.41)	(-0.08)	(-0.93)	(-0.85)	(-0.27)	(0.35)
EGC	-0.137	-0.462*	-0.081	-0.434	-0.022	-0.045	-0.152	-0.401*
	(-1.04)	(-1.73)	(-0.56)	(-1.51)	(-0.44)	(-0.54)	(-1.27)	(-1.69)
Post × Affiliated	-0.277**	-0.295**	-0.267*	-0.244	-0.203***	-0.196**	-0.114	-0.136
	(-2.25)	(-2.02)	(-1.91)	(-1.44)	(-2.71)	(-2.42)	(-1.11)	(-1.10)
Affiliated	-0.017	0.008	0.021	0.110	0.035	0.042	-0.008	0.083
	(-0.21)	(0.08)	(0.23)	(0.79)	(1.02)	(0.87)	(-0.11)	(0.78)
EGC × Affiliated	-0.024	-0.120	-0.068	-0.192	-0.046	-0.118*	-0.020	-0.128
	(-0.24)	(-0.74)	(-0.65)	(-0.98)	(-1.19)	(-1.79)	(-0.23)	(-0.84)
Analyst & Brokerage Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm, IPO, & Market Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Brokerage Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.210	0.253	0.204	0.241	0.204	0.240	0.287	0.333
Number of Observations	5,862	2,825	5,862	2,825	5,862	2,825	5,862	2,825

Table OA-3

Optimism triple differencing analyses partitioned by analyst experience and analyst all-star status

This table presents OLS regressions measuring a triple difference in analyst forecast bias. Panel A is restricted to analysts with experience equal to or below the median and Panel B is restricted to analysts with above median experience. Panel C is restricted to analysts without all-star status (measured as analysts not ranked in the top four by *Institutional Investor (II)* in the year of the forecast) and Panel D is restricted to analysts with all-star status. The dependent variable is forecast bias, defined as $\frac{\text{Forecast}_{it} - \text{Actual}_{i}}{\text{Price}_{i,t-1}} \times 100$. Odd numbered columns use our full sample, while even numbered columns use our PSM matched sample. The triple interaction measures the differential change in forecast bias for affiliated versus unaffiliated analysts, incremental to the relative change in forecast bias for EGCs versus non-EGCs following JOBS. All control variables shown in Table 5 of the paper are included, and Appendix B contains a complete list of explanatory variable definitions. PSM refers to the propensity score matched sample. All regressions include Fama-French 49 industry fixed effects and year-quarter fixed effects. *t*-statistics using standard errors clustered at the analyst and firm levels are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Inexperienced Analysts

	Analyst-Level Forecast Bias (Inexperienced Analysts)			
	Full Sample	PSM	Full Sample	PSM
	(1)	(2)	(3)	(4)
Post × EGC × Affiliated	0.295	0.207	0.339	-0.083
	(1.39)	(0.73)	(1.47)	(-0.27)
Firm, IPO, and Market Controls	Yes	Yes	Yes	Yes
Brokerage Fixed Effects	No	No	Yes	Yes
Adjusted R-squared	0.190	0.235	0.194	0.223
Number of Observations	3,276	1,422	3,276	1,422

Panel B: Experienced Analysts

	Analyst-Level Forecast Bias (Experienced Analysts)			
	Full Sample	PSM	Full Sample	PSM
	(1)	(2)	(3)	(4)
Post × EGC × Affiliated	0.443**	0.688***	0.446**	0.819***
	(2.42)	(2.66)	(2.27)	(2.80)
Firm, IPO, and Market Controls	Yes	Yes	Yes	Yes
Brokerage Fixed Effects	No	No	Yes	Yes
Adjusted R-squared	0.246	0.288	0.249	0.300
Number of Observations	2,586	1,403	2,586	1,403

Table OA-3 (continued)

Panel C: Non All-Star Analysts

	Analyst-Level Forecast Bias (Non-All Star Analysts)			
	Full Sample	PSM	Full Sample	PSM
	(1)	(2)	(3)	(4)
Post × EGC × Affiliated	0.368**	0.400*	0.372**	0.451*
	(2.28)	(1.75)	(2.13)	(1.70)
Firm, IPO, and Market Controls	Yes	Yes	Yes	Yes
Brokerage Fixed Effects	No	No	Yes	Yes
Adjusted R-squared	0.209	0.252	0.201	0.237
Number of Observations	4,939	2,333	4,939	2,333

Panel D: All-Star Analysts

	Analyst-Level Forecast Bias (All Star Analysts)			
	Full Sample	PSM	Full Sample	PSM
	(1)	(2)	(3)	(4)
Post × EGC × Affiliated	0.712**	0.608	1.150***	1.224**
	(2.42)	(1.54)	(3.03)	(2.27)
Firm, IPO, and Market Controls	Yes	Yes	Yes	Yes
Brokerage Fixed Effects	No	No	Yes	Yes
Adjusted R-squared	0.226	0.239	0.231	0.257
Number of Observations	923	492	923	492

Table OA-4

Analyst-Level Informativeness: Announcement Returns for Recommendations

This table presents OLS regressions measuring a triple difference in recommendation announcement CARs. The triple interaction measures the differential change in announcement CARs for affiliated versus unaffiliated analysts, incremental to the relative change in announcement CARs for EGCs versus non-EGCs following JOBS. In Columns 1−6, the dependent variable is three-day cumulative market-adjusted abnormal returns, while in Columns 7–8 the dependent variable is two-hour raw firm returns beginning at the time of the I/B/E/S timestamp, using TAQ trade-level data. In each column, announcement windows in which there are conflicting recommendations are excluded, and windows that coincide with an earnings, manager guidance, or merger announcement are also excluded. In Columns 1–4 and 7–8, CARs surrounding 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. Columns 3 and 4 are identical to Columns 1 and 2, except the three-day CARs are divided by the number of recommendations issued in the three-day window. Columns 5 and 6 are identical to Columns 1 and 2, except only positive recommendations (i.e., buy and strong-buy) are included (i.e., no sign flipping). All remaining variables (Analyst and Brokerage controls are identical to those reported in Table 6, but computed on the analyst level as opposed to firm medians; and Firm, IPO, and Market controls are identical to those reported in Table 4, except these are also computed on the analyst level as opposed to firm medians) 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 analyst and firm levels are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	CAR Unscaled		CAR Scaled		Buy-Only CAR Unscaled		TAQ 2hr Return	
	Full Sample	PSM	Full Sample	PSM	Full Sample	PSM	Full Sample	PSM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × EGC × Affiliated	**-4.466*****	**-6.021*****	**-2.698*****	**-3.436*****	**-4.932****	**-6.188***	**-1.746****	**-3.000*****
	(-3.06)	**(-3.31)**	**(-2.81)**	**(-2.61)**	**(-2.13)**	**(-1.91)**	**(-2.03)**	**(-2.92)**
Post × EGC	1.667	3.631**	1.309	2.324**	1.079	3.743	1.144*	2.678***
	(1.58)	(2.46)	(1.55)	(1.96)	(0.58)	(1.44)	(1.88)	(3.30)
EGC	-0.865	-2.440*	-0.424	-1.785*	-1.838**	-3.503*	-0.117	-0.950
	(-1.23)	(-1.67)	(-0.79)	(-1.75)	(-2.07)	(-1.78)	(-0.35)	(-1.29)
Post × Affiliated	2.856**	4.945***	0.709	2.212*	3.469*	4.919*	1.370**	2.728***
	(2.45)	(3.01)	(0.89)	(1.90)	(1.92)	(1.88)	(2.13)	(3.20)
Affiliated	-0.224	-1.390	-0.156	-0.883	-1.999**	-2.192	0.093	-0.494
	(-0.32)	(-1.03)	(-0.31)	(-0.92)	(-2.00)	(-1.30)	(0.27)	(-0.69)
EGC×Affiliated	2.176***	3.308**	0.619	1.394	3.137***	3.672*	0.267	1.095
	(2.89)	(2.37)	(1.19)	(1.29)	(2.86)	(1.70)	(0.71)	(1.42)
Analyst & Brokerage Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm, IPO, and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Brokerage Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.034	0.014	0.054	0.100	0.036	0.028	0.047	0.047
Number of Observations	3257	1479	3257	1479	2352	1124	3502	1260