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The Global Settlement, All-Star Analyst Departures, and Their Impact on the Capital Markets

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Abstract

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THE GLOBAL SETTLEMENT, ALL-STAR ANALYST DEPARTURES, AND THEIR IMPACT ON THE CAPITAL MARKETS

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ABSTRACT

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The Global Research Analyst Settlement prohibited twelve large investment banks from tying equity analysts' compensation to investment banking revenues, causing a large number of Institutional Investor "all-star" analysts to exit the sell-side industry. Using a difference-in-differences specification, I find that the departure of all-stars caused their bank-industry underwriting groups to lose equity issuance market share. Market share losses were more severe for IPOs than for IPOs and follow-on underwritings combined. The higher the average quality of all-stars in a bank-industry, the more severe were the bank-industry's losses. Additionally, the departure of all-stars raised the cost of equity capital for IPOs underwritten by their bank-industry groups, particularly for IPOs that were more difficult to value. Ultimately, the loss of sell-side research talent, an unintended consequence of regulation, forced issuers to accept research coverage of inferior quality, raising the cost of obtaining public capital.

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CHAPTER 1: INTRODUCTION

The Global Research Analyst Settlement (the "Global Settlement") is an enforcement agreement proposed in December 2002 and finalized on April 28, 2003, among the SEC, the NASD, the NYSE, the New York State Attorney General and ten (later twelve) of the then-largest investment banks operating in the United States.[1] Allegedly, from approximately mid-1999 through mid-2001, under the pressure to attract investment banking business, research analysts at these banks had been generating research that was tainted by conflicts of interest. The goal of the Global Settlement was to restore the integrity of research by severing the ties between the banks' research and investment banking divisions. Under the terms of the agreement, the banks were required to make total payments approximating $1.5 billion, including $942 million in disgorgement and penalties, $85 million for investor education, and $460 million to fund independent research. In addition, the banks were required to separate their research and investment banking divisions, adhere to new disclosure rules, and make independent research available to their investing customers by contracting with independent research firms.

This paper studies the impact of the Global Settlement on the capital markets. How did the Global Settlement affect the investment banking deal flow of the sanctioned banks or equity issuers' underwriter choices? How did the Global Settlement affect the cost of equity capital for issuances underwritten by the sanctioned banks?

These questions are motivated by the evidence documented in Guan, Lu, and Wong (2013) that an exceptionally large number of "all-star" analysts – as named by the *Institutional*

[1]The original ten investment banks included in the Global Settlement are Bear, Stearns & Co. Inc.; Citigroup Global Markets Inc. (f/k/a Salomon Smith Barney, Inc.); Credit Suisse First Boston LLC; Goldman, Sachs & Co.; J.P. Morgan Securities Inc.; Lehman Brothers Inc.; Merrill Lynch, Pierce, Fenner & Smith Incorporated; Morgan Stanley & Co. Incorporated; UBS Warburg LLC; and U.S. Bancorp Piper Jaffray Inc. Deutsche Bank Securities Inc. and Thomas Weisel Partners LLC joined the settlement in August 2004.

Investor magazine annually for every classified industry – exited the sell-side research profession in the post-Settlement period, due to changes in their banks' analyst compensation structures as required by the settlement agreement. Specifically, the agreement prohibits research analysts from participating in the solicitation of investment banking business, including any investment-banking-sponsored pitches and roadshows. Furthermore, the agreement requires that an analyst's compensation not be based directly or indirectly upon investment banking revenues or input from investment banking personnel, but that it should be based in significant part on the quality and accuracy of the analyst's research.

Since investment banking contributions was one of the major factors in determining an analyst's compensation prior to the Global Settlement (Groysberg, Healy, and Maber (2011)), the settlement brought significant reforms to the ways in which analysts were compensated. Using proprietary compensation data from a major investment bank, Groysberg et al. document that median total analyst compensation decreased from a peak of $1,148,435 in 2001 to $647,500 in 2005, driven almost exclusively by variation in bonuses, the median of which decreased from a peak of $940,007 in 2001 to $450,000 in 2005. Guan et al. find that investment bank research analysts were on average more likely to leave the sell-side research profession in the post-Settlement period, and that the increase in the propensity of all-star analysts to exit the sell side was significantly higher than the corresponding increase for non-all-star analysts, which is consistent with all-star analysts earning significantly more than their unranked peers (Groysberg, Healy, and Maber (2011)). Additionally, banks appeared to be unable to retain their all-star analysts by promoting or transferring them to positions with higher earnings potential or better career prospects; moving to the buy side – presumably for more lucrative opportunities – became a much more attractive career option in the post-Settlement period for all-star analysts. What were the economic consequences of the loss of top sell-side research talent, caused by a decline in compensation and perhaps by dimming career prospects?

I use a difference-in-differences specification with a sample period from 1998 to 2007. I define the "event years" to be 2002 and 2003 for two reasons. First, the Global Settlement was proposed in 2002, following extensive investigations into the business practices of the sanctioned banks, making its effectiveness in 2003 a highly-anticipated event in 2002. Second, new rules became effective in 2002 through the self-regulatory organizations (SROs) - the NASD and the NYSE - to address conflicts of interest between research and investment banking, which closely mirrored the terms under the Global Settlement and applied to virtually all brokerages. My study focuses on the twelve investment banks sanctioned in the Global Settlement since these banks were likely to be under the most regulatory pressure to reform. My "treatment" group contains bank-industries that had at least one research analyst named an all-star by *Institutional Investor* in the pre-Settlement period. My unit of observation is at the bank-industry-year level. I include bank-year and industry-year fixed effects in all regression specifications to control for time-varying differences across banks and industries.

I find that a bank-industry that had at least one all-star analyst in the pre-Settlement period on average suffered losses in its equity issuance market share in the post-Settlement period, relative to a bank-industry that did not have an all-star analyst in the pre-Settlement period. Market share losses were more severe for IPO underwritings than for IPO and follow-on underwritings combined, likely due to the stickiness of underwriting relationships. My difference-in-differences coefficients indicate that the relative losses average 2.96% in market share for IPO and follow-on underwritings combined, and 3.79% in market share for IPO underwritings alone, equivalent to relative losses of about $15 million and $9 million per "treatment" bank-industry, respectively, in total underwriting fees between 2004 and 2007. Whereas the average market share is 0.93% to 2.06% higher for the "treatment" bank-industries than for the "control" bank-industries in the pre-Settlement period, the "treatment" bank-industries' relative losses bring these percentages down to negative levels in the post-Settlement period, making the relative losses economically significant.

Results remain qualitatively similar when I limit my sample to include only market share observations that are computed from industry-years with at least three equity issuances in total. A placebo test using an identical methodology and a sample period from 1990 to 1999 yields no statistically significant results, indicating that the aforementioned patterns observed in the 1998-2007 period are likely due to the Global Settlement.

One explanation for these results is my "Departure Hypothesis," which states that the post-Settlement departure of all-star analysts from the sell-side industry caused the analysts' bank-industry underwriting groups to suffer losses in equity issuance market share relative to bank-industry groups that did not have all-star analysts to begin with. This hypothesis is motivated by two strands of literature. The first strand of literature documents that all-star analysts exhibit superior performance relative to non-all-star analysts (e.g., Bonner, Hugon, and Walther (2007); Fang and Yasuda (2009, 2014); Gleason and Lee (2003); Leone and Wu (2007); Stickel (1992)). The second strand of literature documents that equity issuers place a high value on securing high-quality research coverage (e.g., Clarke, Khorana, Patel, and Rau (2007); Dunbar (2000); Krigman, Shaw, and Womack (2001); Ljungqvist, Marston, and Wilhelm (2006)).

Consistent with the Departure Hypothesis, I first document that 68.33% of my treatment bank-industries had at least one all-star analyst departure from the sell side in the post-Settlement period. Next, my descriptive statistics show that, compared to non-all-star analysts, all-star analysts were on average more accurate and timely in their earnings forecasts, issued more earnings revisions, and were less optimistically biased in both their earnings forecasts and their stock recommendations. Last, I show that the higher the average quality of all-star analysts in a treatment bank-industry in the pre-Settlement period, the more severe the bank-industry's post-Settlement losses. For example, losses were more severe in bank-industries that had a repeat (i.e., multi-year) all-star than in bank-industries that did not have a repeat all-star. Additionally, losses were more severe in bank-industries in which the all-star analysts were more accurate in their earnings forecasts, issued more earnings revisions, and were less optimistically biased in their earnings forecasts and stock recommendations.

These results are inconsistent with my alternative "Optimism Hypothesis" as an explanation for the treatment bank-industries' post-Settlement market share losses relative to the control bank-industries. This hypothesis posits that all-star analysts became much less likely to publish overly optimistic research in the post-Settlement period than they had been in the pre-Settlement period, thus driving away potential investment banking clients. One assumption behind this hypothesis is that all-star analysts were more likely to be optimistically biased than non-all-star analysts in the pre-Settlement period. An empirical prediction is that the more optimistically biased were a bank-industry's all-star analysts in the pre-Settlement period, the more severe were the bank-industry's post-Settlement market share losses. Both the assumption and the prediction are rejected by my aforementioned results.

Turning to the impact of the Global Settlement on the cost of equity capital – which I measure using equity issuance underpricing – I consider two hypotheses that generate opposite empirical predictions. My "Changing Risk Hypothesis" states that the departure of all-star analysts introduces valuation risk into equity issuances underwritten by the bank-industries from which they depart, raising the level of underpricing demanded by investors. Furthermore, the rise in underpricing would be more pronounced for issuances that are intrinsically more difficult to value. My "Analyst Lust Hypothesis," on the other hand, assumes that underwriters always want to underprice equity issuances. This hypothesis states that as all-star analysts depart from their bank-industries, equity issuers become less willing to tolerate underpricing for the benefit of high-quality analyst coverage, thus lowering the level of underpricing. The decrease in underpricing could be more pronounced for issuances that are intrinsically more difficult to value.[2]

I use a triple-difference specification to compare the effect of the Global Settlement on the cost of equity capital between issuances with high valuation risk and those with low valuation risk. I use two measures of valuation risk. The first measure is whether an issuer belongs in a high-technology industry, assuming that tech issuances are more difficult to value than non-tech issuances. The second measure is issuer age, assuming that issuances from younger issuers are more difficult to value than those from older issuers. I find that, whereas the effect of the Analyst Lust Hypothesis dominates in the subgroup of low-risk IPOs, a positive triple-difference coefficient indicates that the Changing Risk Hypothesis plays a more important role in the subgroup of high-risk IPOs than in the subgroup of low-risk IPOs. Specifically, the differential effect of the Global Settlement on the cost of equity capital between these two groups of issuances amounts to an economically significant 17.8% in underpricing, equivalent to an average amount of money left on the table of about $34 million.

The existing literature provides little empirical evidence regarding the impact of the Global Settlement on the capital markets. The contribution of my paper is to fill this void in the literature. My results suggest that the quality of analyst coverage remains an important determinant of equity underwriter choice in spite of the Global Settlement's restrictions on analyst participation in equity issuances. I show that the departure of some of the most highly-regarded research analysts from the sell-side industry, as an unintended consequence

[2]Both the "Changing Risk Hypothesis" and the "Analyst Lust Hypothesis" are adapted from Loughran and Ritter (2004), who examine the reasons for changes in IPO underpricing levels between 1980 and 2003.

of well-intentioned regulation, has not only caused issuing companies to reconsider their underwriter choices, but more importantly has forced issuing companies to accept research coverage of inferior quality, even though companies place a high value on securing high-quality research coverage from sell-side analysts. I also find suggestive evidence that the departure of all-star analysts has introduced valuation risk into IPOs underwritten by the bank-industries from which they have departed, raising the cost of equity capital, particularly for IPOs that are more difficult to value. For these reasons, the disappearance of high-quality research coverage could be deterring companies from seeking public capital, limiting their options in obtaining resources necessary for growth, impairing the efficiency of the equity capital markets and of the real economy.

A few papers have found empirical evidence supporting the concerns that motivated the Global Settlement. For example, in the pre-Settlement period, affiliated analysts – analysts whose banks were also a company's underwriters – were on average faster to upgrade and slower to downgrade a company's stock recommendations than were non-affiliated analysts; their buy recommendations underperformed – and their hold and sell recommendations outperformed – those of non-affiliated analysts (e.g., Barber, Lehavy, and Trueman (2007); Cliff (2004); Lin, McNichols, and O'Brien (2005)).

On the impact of the Global Settlement and related conflict of interest regulations on the quality of analyst stock recommendations, a few papers find that, post-Settlement, stock recommendations became less affected by conflicts of interest and more consistent with intrinsic value estimates based on analysts' earnings forecasts, suggesting that regulation enhanced analysts' independence (e.g., Barniv, Hope, Myring, and Thomas (2009); Bradshaw (2009); Chen and Chen (2009)). Furthermore, optimistic recommendations became less frequent and more informative, neutral and pessimistic recommendations became more frequent and less informative, and overall informativeness of stock recommendations declined (e.g., Barber, Lehavy, McNichols, and Trueman (2006); Boni (2005); Clarke, Khorana, Patel, and Rau (2011); Kadan, Madureira, Wang, and Zach (2009)). The decline in the overall informativeness of stock recommendations likely resulted from the departure of some of the most talented research analysts from the sell-side industry, as documented in Guan, Lu and Wong (2013) and in this paper.

Lastly, a number of papers compare the quality of stock recommendations among different types of research providers. For example, although the Global Settlement only sanctioned twelve investment banks, the sanctioned banks' recommendation practices did not differ from those of other investment banks in the pre-Settlement period; sanctioned banks' stock recommendations, however, became much more conservative and much less informative in the post-Settlement period than those of other investment banks (e.g., Barber, Lehavy, McNichols, and Trueman (2006); Kadan, Madureira, Wang, and Zach (2009)). Settlement-funded independent research firms provided lower-quality and less informative stock recommendations than investment banks and independent research firms that did not participate in the Global Settlement or receive Settlement funds, likely due to the lack of experience of the research analysts employed by these relatively new independent research firms (e.g., Buslepp, Casey, and Huston (2014); Clarke, Khorana, Patel, and Rau (2011)).

The rest of this paper is organized as follows. Section 2 describes my research design and hypotheses. Section 3 describes my data and presents summary statistics. Section 4 presents my results. Section 4.1 presents results on the impact of the Global Settlement on the investment banking deal flow of the sanctioned banks, and Section 4.2 presents results on the impact of the Global Settlement on the cost of equity capital for issuances underwritten by the sanctioned banks. Section 5 concludes.

CHAPTER 2: RESEARCH DESIGN AND HYPOTHESES

2.1. Research Design

I estimate the effect of the Global Settlement using a difference-in-differences framework. My sample period is from 1998 to 2007. I define 2002 and 2003 to be the "event years," 1998-2001 to be the "pre-Settlement period," and 2004-2007 to be the "post-Settlement period." My sample contains the twelve sanctioned banks. My "treatment" group contains bank-industries that had at least one all-star analyst during 1998-2001, provided that the analyst stayed with the bank-industry until the end of 2001. I exclude a bank-industry from the treatment group if its all-star analyst(s) exited the sell-side industry, or moved to another sell-side firm, prior to the end of 2001. If the analyst continued to be named an all-star after arriving at the new sell-side firm, I assign the new firm's bank-industry to the treatment group. If the analyst was no longer named an all-star after arriving at the new sell-side firm, I assign the new firm's bank-industry to the "control" group. Table 1 presents the treatment group, which contains a total of 341 bank-industries across the twelve sanctioned banks. Piper Jaffray has zero observations in the treatment group since this bank did not have an all-star analyst during 1998-2001.

My regression specification is as follows:

$$y_{ijt} = \alpha * Treatment_{ij} + \beta * Treatment_{ij} * Post_t + \theta_{it} + \delta_{jt} + \mu_{ijt} \tag{1}$$

y_{ijt} is the dependent variable for bank i, industry j, and year t. $Treatment_{ij}$ is an indicator that equals 1 for observations that belong in the treatment group, and 0 otherwise. $Post_t$ is an indicator that equals 1 for observations from the post-Settlement period, and 0 for observations from the pre-Settlement period.[3] The coefficient (β) on the interaction term $Treatment_{ij} * Post_t$ is the difference-in-differences coefficient of interest.

In order to investigate whether the Global Settlement had an "effect" prior to its announcement, I estimate the following regression to examine the dynamics of the difference-in-differences coefficient:

$$\begin{aligned} y_{ijt} = \alpha * Treatment_{ij} + \beta_1 * Treatment_{ij} * GS(-1)_t + \beta_2 * Treatment_{ij} * GS(0)_t \\ + \beta_3 * Treatment_{ij} * GS(+1)_t + \beta_4 * Treatment_{ij} * GS(2+)_t + \theta_{it} + \delta_{jt} + \mu_{ijt} \end{aligned} \tag{2}$$

$GS(-1)_t$ is an indicator for one year before the event years (2001), $GS(0)_t$ is an indicator for the event years (2002 and 2003), $GS(+1)_t$ is an indicator for one year after the event years (2004), and $GS(2+)_t$ is an indicator for two or more years after the event years (2005 to 2007). If the Global Settlement had a causal effect on the capital markets, the effect should show up only after the announcement of the regulation and not before. In other words, the coefficient on $Treatment_{ij} * GS(-1)_t$ should be statistically insignificant, and the effect of the regulation should be reflected in later years.

In both specification (1) and specification (2), I include bank-year (θ_{it}) and industry-year (δ_{jt}) fixed effects to control for time-varying differences across banks and industries. Coefficients are estimated by ordinary least squares, and robust standard errors are clustered at the bank level.

[3] I exclude observations from the event years, 2002 and 2003, from my analysis under specification (1).

2.2. Hypotheses and Predictions

This section presents four hypotheses and their empirical predictions. To study the impact of the Global Settlement on the investment banking deal flow of the sanctioned banks, I examine the Departure Hypothesis and the Optimism Hypothesis. To study the impact of the Global Settlement on the cost of equity capital – measured by equity issuance underpricing – for issuances underwritten by the sanctioned banks, I examine the Changing Risk Hypothesis and the Analyst Lust Hypothesis.

2.2.1. Departure Hypothesis

Both the Global Settlement and the SRO regulations prohibited investment banks from tying research analysts' compensation to their investment banking contributions. As documented in Guan, Lu, and Wong (2013), both all-star analysts and non-all-star analysts from investment banks exited the sell-side research industry at increased rates in the post-Settlement period, but the increase in the rate of departure was significantly higher for all-star analysts. This result is unsurprising given that all-star analysts have been found to receive 61% higher total compensation than their unranked peers, at a major, unnamed investment bank (Groysberg, Healy, and Maber (2011)). Guan et al. find that, in the post-Settlement period, the higher the level of investment banking activities in an all-star analyst's core industry, the higher the propensity of the all-star analyst to exit the sell side, inconsistent with analyst departures being driven by a decline in the demand for their research services. Among the all-star analysts who exited the sell side, 32.32% moved to buy-side firms, up from 24.71% in the pre-Settlement period. This 7.61% increase is the largest percentage point increase among all all-star career choice categories, suggesting that the buy side became a much more attractive career choice in the post-Settlement period. There was no significant change in the percentage of all-star analysts being promoted to managerial positions or transferred to different departments within the same bank (e.g., investment banking), suggesting that banks were unable to retain their all-star analysts by relocating them to positions with higher pay or improved career prospects.

The Departure Hypothesis posits that the loss of an all-star analyst impairs her bank's ability to obtain investment banking mandates in the all-star analyst's industry, more than the loss of a non-all-star analyst would. This hypothesis is motivated by two strands of literature.

In the first strand of literature, all-star analysts are found to demonstrate superior performance compared to their unranked peers (e.g., Bonner, Hugon, and Walther (2007); Fang and Yasuda (2009, 2014); Gleason and Lee (2003); Leone and Wu (2007); Stickel (1992)). For example, all-star analysts provide more accurate earnings forecasts, more frequent coverage, and more profitable stock recommendations. They are less likely to herd with other analysts, and their superior performance is persisting. Their stock recommendations are followed by stronger market reactions, a sign that the market perceives all-star analysts to be more credible.

In the second strand of literature, equity issuers are found to highly value high-quality research coverage (e.g., Clarke, Khorana, Patel, and Rau (2007); Dunbar (2000); Krigman, Shaw, and Womack (2001); Ljungqvist, Marston, and Wilhelm (2006)). For example, using data on individual analyst job changes from 1988 to 1999, Clarke et al. document that when an all-star analyst moves from one investment bank to another, the old bank loses equity issuance market share while the new bank gains market share. Dunbar finds that analyst reputation, as measured by *Institutional Investor* rankings, is an important determinant of the IPO market share of investment banks that act as book managers. Krigman et al. study the underwriter choice of issuers completing secondary equity offerings for the first time. Their empirical evidence and survey results suggest that both dissatisfaction with the quality of analyst coverage provided by the IPO underwriter and strong analyst reputation at the secondary offering underwriter are driving factors in an issuer's decision to switch to a new underwriter. Also, in studying underwriter choice for follow-on offerings, Ljungqvist et al. find that having an all-star analyst increases the likelihood of a bank being chosen as the lead underwriter in the all-star analyst's industry.

The Departure Hypothesis predicts a negative difference-in-differences coefficient (β) on the interaction term in equation (1). That is, the bank-industries that had at least one all-star analyst in the pre-Settlement period on average suffered losses in their equity issuance market shares in the post-Settlement period, relative to bank-industries that had zero all-star analysts in the pre-Settlement period.

Furthermore, the Departure Hypothesis predicts a stronger treatment effect (i.e., a more negative difference-in-differences coefficient) for the bank-industries whose all-star analysts were on average of higher quality than for the bank-industries whose all-star analysts were of lower quality. That is, the more talented an all-star analyst, the higher were her bank's losses in its equity issuance market share in the analyst's industry after the analyst's departure.

2.2.2. Optimism Hypothesis

Since all-star analysts were on average more involved in their banks' investment banking activities than their unranked peers prior to the Global Settlement, one might expect all-star analysts to have been under more intense pressure to attract investment banking business and thus subject to more severe conflicts of interest. This argument is backed by at least two prominent examples: Multi-year all-star analysts Jack Grubman from Citigroup and Henry Blodget from Merrill Lynch, both commanding annual salaries of over $10 million prior to the Global Settlement, were charged with producing fraudulent research reports and barred from the securities industry for life.

The Optimism Hypothesis assumes that all-star analysts were more likely to be optimistically biased than non-all-star analysts in the pre-Settlement period. This hypothesis states that, as research analysts became less likely to publish overly optimistic research coverage in the post-Settlement period, the bank-industries that had all-star analysts in the pre-Settlement period on average suffered losses in their equity issuance market shares in the post-Settlement period, relative to bank-industries that had no all-star analysts in the pre-Settlement period.

Like the Departure Hypothesis, the Optimism Hypothesis predicts a negative difference-in-differences coefficient in equation (1). Unlike the Departure Hypothesis, however, the Optimism Hypothesis is not contingent upon the post-Settlement departure of all-star analysts from the sell side.

The Optimism Hypothesis has a second prediction: The treatment effect should be stronger (i.e., the difference-in-differences coefficient more negative) for the bank-industries whose all-star analysts were on average more optimistically biased than for the bank-industries whose all-star analysts were less optimistically biased. That is, the more optimistically biased an all-star analyst, the higher were her bank's losses in its equity issuance market share in the analyst's industry in the post-Settlement period.

The existing literature provides little evidence that all-star analysts are more optimistically biased than non-all-star analysts. On the contrary, the opposite appears to be true. For example, compared to non-all-star analysts, all-star analysts have been found to be on average less positively biased and less aggressive in issuing stock upgrade recommendations; during market peaks, all-star analysts working at top-tier banks have become more accurate relative to their non-all-star colleagues (e.g., Fang and Yasuda (2009); Ljungqvist, Marston, and Wilhelm (2006)). These results are consistent with personal reputation acting as an effective discipline mechanism against conflicts of interest. Lastly, the literature provides little evidence that issuing optimistic stock recommendations helps an investment bank win underwriting mandates (e.g., Clarke, Khorana, Patel, and Rau (2007); Ljungqvist, Marston, and Wilhelm (2006)).

2.2.3. Changing Risk Hypothesis

Rock (1986) develops a model in which IPO shares are underpriced to compensate uninformed investors for their information disadvantage. When issues are underpriced, both informed and uninformed investors submit orders. However, when issues are overpriced, only uninformed investors submit orders. This leads to the outcome that when uninformed investors are allocated shares in an IPO, too often the shares are overpriced. In equilibrium, underpricing arises to induce uninformed investors to participate in the market. Ritter (1984) interprets equilibrium underpricing as compensating uninformed investors for the cost of becoming informed – performing securities analysis and so on. Ritter argues that the greater the fundamental uncertainty about an issue, the greater the required compensation. In particular, high-risk offerings should be underpriced more than low-risk offerings. Here, "risk" is not beta-type risk, but reflects technological or valuation uncertainty. Empirical evidence from the same paper supports this argument.

During equity offerings, research analysts could play an important role of certifying to potential investors that the shares being issued are appropriately priced (e.g., Dunbar (2000)). The departure of an all-star analyst, however, would deprive a bank's potential investors of the analyst's securities analysis. Without access to high-quality research from the bank, investors would be forced to devote resources to either performing the analysis themselves or obtaining research from a third party. As a form of compensation, investors could demand underpricing from the bank in the form of a lower offer price. More underpricing would be demanded for issuances that were intrinsically more difficult to value, since more resources would be required to perform high-quality analysis on such issuances.

Therefore, the Changing Risk Hypothesis predicts a positive difference-in-differences coefficient on the interaction term in equation (1). More importantly, the treatment effect should be stronger (i.e., the difference-in-differences coefficient more positive) for high-valuation-risk issuances than for low-valuation-risk issuances. That is, the difference in average underpricing between the treatment group and the control group should widen in the post-Settlement period, and it should widen more for issuances with higher valuation risk.

2.2.4. Analyst Lust Hypothesis

The Analyst Lust Hypothesis assumes that underwriters always want to underprice equity issuances. Since equity issuers value high-quality analyst coverage, they are willing to tolerate underpricing as a form of compensation to the underwriter for providing high-quality analyst coverage (e.g., Cliff and Denis (2004); Loughran and Ritter (2004)). Previous studies have shown that equity issuers put high value on securing high-quality research coverage (e.g., Clarke, Khorana, Patel, and Rau (2007); Dunbar (2000); Krigman, Shaw, and Womack (2001); Ljungqvist, Marston, and Wilhelm (2006)). Furthermore, Cliff and Denis (2004) argue that equity issuers purchase analyst coverage with underpricing, by showing that first-day returns are positively related to the presence of an all-star analyst on the research staff of the lead underwriter, and to the quality of analyst coverage provided by the lead underwriter, controlling for endogeneity between underpricing and analyst coverage.

When an all-star analyst departs from a bank, the bank's equity-issuing clients should become less willing to tolerate underpricing, bringing underpricing down from its pre-Settlement level. To the extent that the average quality of research analysts in the control group bank-industries improved relative to that of research analysts in the treatment group bank-industries, average underpricing for the control group should rise from its pre-Settlement level.

Therefore, the Analyst Lust Hypothesis predicts a negative difference-in-differences coefficient on the interaction term in equation (1). That is, the difference in average underpricing between the treatment group and the control group should narrow in the post-Settlement period.

Additionally, the treatment effect could be stronger (i.e., the difference-in-differences coefficient more negative) for issuances that are intrinsically more difficult to value, to the extent that high-valuation-risk issuers put more value on research coverage by reputable analysts, due to the analysts' certification role to uninformed investors prior to an issuance or their continuing supply of securities analysis to such investors in the aftermarket. That is, the difference in average underpricing between the treatment group and the control group could narrow more for issuances with higher valuation risk.

Table 1: Global Research Analyst Settlement, and Treatment Group

Column A lists the twelve investment banks involved in the Global Settlement. The settlement agreement reached in April 2003 originally included ten investment banks operating in the US. The list expanded to twelve when Deutsche Bank and Thomas Weisel joined the settlement in August 2004. A bank-industry belongs in the treatment group if at least one of its research analysts was named an all-star by *Institutional Investor* in any year during 1998-2001 *and* stayed with the bank until the end of 2001. Industries are classified using GICS codes. Each all-star analyst is first assigned to the GICS industry in which she issued the largest fraction of earnings forecasts during 1998-2001. GICS classifications are then checked against industry classifications assigned by *Institutional Investor*, and changes are made whenever inconsistencies occur. For each investment bank, column B lists the number of industries in the treatment group, and column C lists the number of industry-year observations in my difference-in-differences specification. Sample period is 1998-2007, with event years 2002 and 2003 excluded from this sample.

Investment Bank	No. of Industries with All-Stars	Industry-Year Observations
Bear Stearns	30	199
Citigroup	47	298
Credit Suisse	48	317
Deutsche Bank	18	100
Goldman Sachs	44	299
J.P. Morgan	19	133
Lehman Brothers	30	183
Merrill Lynch	44	298
Morgan Stanley	44	277
Piper Jaffray	0	0
Thomas Weisel	1	7
UBS	16	99
Total	341	2210 (44.50%)

CHAPTER 3: DATA AND SUMMARY STATISTICS

3.1. Data

3.1.1. All-Star Analysts

An analyst is an "all-star" in a given year if the analyst is named to the All-America Research Team by the *Institutional Investor* magazine in that year. Every year the magazine surveys a large number of consumers of sell-side research, including portfolio managers, investment officers, and buy-side analysts, who are asked to evaluate the quality of sell-side research analysts. Important performance metrics include accuracy of earnings estimates, quality of stock picking, quality of written reports, timeliness of communications with investors, and responsiveness to investor requests. Analyst rankings – including first, second, and third position, and runners-up – are compiled based on survey results and published in the October issue.

I use the GICS industry classification, following previous studies, since it has been shown that partitions on the basis of GICS codes provide a good proxy for how analysts specialize by industry (Boni and Womack (2006); Clarke, Khorana, Patel, and Rau (2007)). I obtain GICS industry codes from Compustat. To assign an all-star analyst to a GICS industry, I first assign the analyst to her "primary" GICS industry, in which the analyst issued the largest fraction of her earnings forecasts during 1998-2001. I then check this GICS industry assignment against the *Institutional Investor* magazine's industry assignment, and make changes whenever inconsistencies occur. For example, I update the analyst's primary GICS industry code with a different GICS industry code if the analyst is named an all-star in an industry other than her primary industry. If the analyst is named an all-star in more than one industry, I assign additional GICS industry codes to the analyst.

There were a total of 462 all-star analysts during the period of 1998-2001. The final treatment group retains 382 all-star analysts after dropping: (1) all-star analysts who did not work for a sanctioned bank during 1998-2001; (2) all-star analysts who moved to different sell-side firms during 1998-2001 and were never named all-stars at their new firms during 1998-2001; and (3) all-star analysts who left the sell side during 1998-2001.[4] I identify all-star analyst departures from the sell-side industry in the post-Settlement period using I/B/E/S and Nelson's Directory of Investment Research. For every year up to 2008, Nelson's published comprehensive information on the sell-side investment research industry, including information on research firms, their executives, and all research analysts active in that year. I first identify the last year in which an analyst's earnings estimates appear in I/B/E/S. I then check this information against the Analyst Registry in Nelson's – which lists, for every year, the names and employers of all active sell-side analysts – to ensure that the analyst did indeed leave the sell-side industry. If an analyst disappears from the Nelson's registry but appears as an executive at the same firm under Executives by Job Function, I do not consider that analyst as having departed from the sell-side industry.

3.1.2. Market Share

I obtain data on equity issuances, including gross proceeds, offer price, the identities of the lead or co-lead managers, and the number of lead or co-lead managers, from Thomson Reuters's SDC database. I drop ADRs, units, partnerships, and stocks not listed on the NYSE, NASDAQ, or AMEX. I also drop categories with missing GICS industry codes, including funds, investment trusts, and non-operating establishments. I calculate an investment bank's industry market share in a given year as the gross proceeds raised by the investment bank in that industry through deals in which the investment bank acted as a

[4] All-star analysts who were fired are excluded from my analysis. Prominent examples include Jack Grubman from Citigroup and Henry Blodget from Merrill Lynch, who, under the terms of the Global Settlement, were barred from the securities industry for life.

lead or co-lead manager, divided by the total gross proceeds of all deals completed in that industry in that year. Market share is computed both for IPO underwritings and for IPO and follow-on underwritings combined.

If an investment bank acquired or merged with another investment bank at any time during my sample period, I add the market share of the acquired bank to that of the acquirer for the period prior to the acquisition or merger, in order to account for the effect of consolidation. For example, PaineWebber was acquired by UBS in 2000. I therefore compute the market share of UBS in 1998, 1999, and 2000 as the sum of the market share of UBS and that of PaineWebber. Additionally, if a treatment bank-industry had an all-star starting from, but not before, for example, 2000, I include only market share observations starting from, but not before, 2000 in my regression analysis.

3.1.3. Underpricing and Valuation Risk Measures

I obtain stock prices from CRSP. I calculate the underpricing of an equity issuance as its first-day return, or the percentage change from the offer price to the first-day closing price. To obtain underpricing at the bank-industry-year level, I compute the weighted average underpricing of equity issuances underwritten by a bank in an industry in a year. The higher the number of lead or co-lead managers in an issuance, the lower is the weight given to the underpricing of that issuance, for the following reason. Both the Changing Risk Hypothesis and the Analyst Lust Hypothesis posit that the degree of underpricing of an issuance is affected by the quality of the research analysts at the underwriter. Since an issuance sometimes involves multiple lead or co-lead managers, I assume that the higher the number of lead or co-lead managers there are in a deal, the less important is the analyst quality at each underwriter in determining the level of underpricing of the deal.

I use two measures of valuation risk of an equity issuance. The first measure is whether an issuer belongs in a high-technology industry, assuming that tech issuances are more difficult to value. In my triple-difference specification, this measure is represented by an indicator that equals 1 for observations that belong in a tech industry, and 0 otherwise.[5] The second measure is issuer age, assuming that issuances from younger issuers are more difficult to value due to the lack of an operating history. I measure the age of an issuer at its public offering as the difference between the issuer's founding year, obtained from Jay Ritter's website, and the year of the public offering.[6] In my triple-difference specification, this measure is represented by a continuous variable that equals the average issuer age of issuances underwritten by a bank in an industry in a year.

3.1.4. Analyst Quality Proxies

I obtain earnings forecasts and stock recommendations from I/B/E/S. I measure the quality of an analyst along five dimensions: earnings forecast accuracy, frequency of forecast revisions, forecast timeliness, earnings forecast optimism, and abnormal recommendation levels. Since earnings estimates, timeliness, and responsiveness are among the factors that determine an analyst's ranking by *Institutional Investor*, I use the first three proxies – earnings forecast accuracy, frequency of forecast revisions, and forecast timeliness – to measure the reputation of an analyst. I use earnings forecast optimism and abnormal recommendation levels to proxy for the bias of an analyst.

Since measures of forecast accuracy and optimism are issuer- and industry-dependent, comparing the forecast error of one analyst to that of another, for example, could be prob-

[5] Tech industries are defined as those in GICS codes 254010 (media), 255020 (internet and catalog retail), 351010 (health care equipment and supplies), 351030 (health care technology), 352030 (life science tools and services), 451010 (internet software and services), 451020 (IT services), 451030 (software), 452010 (communications equipment), 452020 (computers and peripherals), 452030 (electronic equipment, instruments and components industry), 453010 (semiconductors and semiconductor equipment), 501010 (diversified telecommunication services), and 501020 (wireless telecommunication services).

[6] I obtain data on founding years from this website: `http://bear.warrington.ufl.edu/ritter/FoundingDates.htm`.

lematic if some issuing firms are more difficult to value than others. For this reason, I follow previous papers and use a scoring methodology to compute performance scores based analysts' relative performance (e.g., Clarke, Khorana, Patel, and Rau (2007); Hong and Kubik (2003); Hong, Kubik, and Solomon (2000); Ljungqvist, Marston, Starks, Wei, and Yan (2007)).

Forecast Accuracy: I compute analyst k's forecast error for firm s and year t as the absolute difference between the analyst's then-most recent forecast of annual earnings per share issued before the fiscal year-end of firm s in year t, and the realized earnings per share of firm s in year t. I then rank all analysts who covered firm s in year t based on their forecast errors, assigning the most accurate analyst a rank of one. Next, to obtain the forecast accuracy score of analyst k for firm s in year t, I scale each rank by the number of analysts who covered firm s in year t, as follows:

$$\textit{Forecast Accuracy Score}_{k,s,t} = 100 - \frac{Rank_{k,s,t} - 1}{\textit{Number of Analysts}_{s,t} - 1} \times 100 \tag{3}$$

Last, I compute an analyst's pre-Settlement forecast accuracy score by averaging the analyst's forecast accuracy scores for 1998-2001. I label this measure *Proxy I*, which is similar to *Relative Forecast Accuracy* constructed in Hong and Kubik (2003), among others. Following Clarke, Khorana, Patel, and Rau (2007), I also compute an alternative measure, *Proxy II*, by counting the proportion of the analyst's scores for 1998-2001 that are 50 or higher. *Proxy I* ranges from 0 for the least accurate analyst to 100 for the most accurate analyst. *Proxy II* ranges from 0 for the least accurate analyst to 1 for the most accurate analyst.

Coverage Frequency: I compute analyst k's coverage frequency score for firm s in year t by ranking all analysts who covered firm s in year t, based on the number of times they revised their annual earnings estimates for firm s in year t, and scaling the ranks using equation (3). I then compute *Proxy I* and *Proxy II* for each analyst in a similar way to how I compute *Forecast Accuracy*. *Proxy I* ranges from 0 for the least frequent forecaster to 100 for the most frequent forecaster. *Proxy II* ranges from 0 for the least frequent forecaster to 1 for the most frequent forecaster. The use of this measure is motivated by the finding in Krigman, Shaw, and Womack (2001) that dissatisfaction with the frequency of coverage is a major reason for which follow-on offering issuers switch underwriters.

Coverage Timeliness: I compute analyst k's coverage timeliness score for firm s in year t by ranking all analysts who covered firm s in year t, based on how quickly they issued their first annual earnings estimates for firm s in year t after the release of the previous year's earnings. As for previous measures, I compute *Proxy I* and *Proxy II* such that *Proxy I* ranges from 0 for the least timely analyst to 100 for the most timely analyst, and *Proxy II* ranges from 0 for the least timely analyst to 1 for the most timely analyst. I include this measure since it has been argued that an analyst who issues the first annual forecast is less likely to be herding with other analysts, and that analysts who exhibit herd behavior have lower ability (e.g., Clement and Tse (2005); Hong, Kubik, and Solomon (2000)).

Forecast Optimism: I compute analyst k's forecast optimism bias for firm s in year t by subtracting the consensus (average) forecast made by all other analysts covering firm s in year t from analyst k's forecast. I then compute analyst k's forecast optimism score for firm s in year t by ranking all analysts who covered firm s in year t, based on their forecast optimism biases for firm s in year t. Following the methodology described above, I compute *Proxy I* and *Proxy II* such that *Proxy I* ranges from 0 for the least optimistically biased analyst to 100 for the most optimistically biased analyst, and *Proxy II* ranges from 0 for the least optimistically biased analyst to 1 for the most optimistically biased analyst.

Abnormal Recommendation Level: I compute analyst k's abnormal recommendation level for firm s in year t by subtracting the consensus (average) recommendation level made by all other analysts covering firm s in year t from analyst k's recommendation level. I then compute analyst k's abnormal recommendation score for firm s in year t by ranking all analysts who covered firm s in year t, based on their abnormal recommendation levels for firm s in year t. As for previous measures, I compute *Proxy I* and *Proxy II* such that *Proxy I* ranges from 0 for the least optimistically biased analyst to 100 for the most optimistically biased analyst, and *Proxy II* ranges from 0 for the least optimistically biased analyst to 1 for the most optimistically biased analyst.

To assess the overall quality of an analyst, I also construct three composite measures using the five measures described above. The above procedures result in two scores, *Proxy I* and *Proxy II*, for each of the five measures, for each analyst who exists in I/B/E/S for 1998-2001. For each analyst k, I compute the composite measures as follows, separately for *Proxy I* and for *Proxy II*:

$$\textit{Reputation}_k = \frac{\textit{Forecast Accuracy}_k + \textit{Coverage Frequency}_k + \textit{Coverage Timeliness}_k}{3} \tag{4}$$

$$\textit{Bias}_k = \frac{\textit{Forecast Optimism}_k + \textit{Abnormal Recommendation Level}_k}{2} \tag{5}$$

$$[\textit{Reputation-Bias}]_k = \textit{Reputation}_k - \textit{Bias}_k \tag{6}$$

3.2. Summary Statistics

In Table 2 Panel A, I present summary statistics for two regression samples. In the first sample, labeled *IPOs & SEOs*, I compute *Market Share* using IPO and follow-on underwritings combined. In the second sample, labeled *IPOs Only*, I compute *Market Share* using IPO underwritings alone. For each, I show the number of observations, mean, standard deviation, minimum value, maximum value, and 25th/50th/75th/90th percentile values.

My regressions are at the bank-industry-year level. Whenever an industry has at least one equity issuance in a given year, I assign each bank a *Market Share* value for that industry-year, including zeros for banks that have no underwritings in that industry-year. If an industry has no equity issuances in a given year, I assign missing *Market Share* values to all banks in that industry-year. There are fewer observations in the *IPOs Only* sample than in the *IPOs & SEOs* sample because some industries have only follow-on offerings, but not IPOs, in certain years.

There are fewer *Underpricing* observations than there are *Market Share* observations for the following reason. For a bank-industry-year to have a non-missing *Underpricing* value, it is necessary that the bank underwrites at least one equity issuance in that industry in that year. On the other hand, a non-missing *Market Share* value for a bank-industry-year only requires that the given industry, not necessarily the given bank, has at least one equity issuance in that year. Additionally, values of *Avg. Issuer Age* are missing for some bank-industry-years due to the lack of information on the founding years of some issuers.

Lastly, *No. of Deals in Industry-Year* has a minimum value of 1 because I assign missing values to *Market Share* for industry-years that have zero equity issuances. This number increases to 3 at the 25th percentile for the *IPOs & SEOs* sample, and at the 50th percentile for the *IPOs Only* sample. The low number of issuances in certain industry-years gives rise to undesirable discontinuity in market shares across banks, with a few banks claiming 100% and the rest 0%. As an attempt to address this problem, I perform additional analysis by limiting my sample to include only *Market Share* observations from industry-years that have at least three equity issuances.

In Table 2 Panel B, I compare the treatment group bank-industries to the control group bank-industries along a few observable dimensions, using data from the pre-Settlement period of 1998-2001. I organize the observables into two categories, namely, those associated with the bank-industries' equity research coverage, and those associated with their equity underwriting. I show the mean and standard deviation (in parentheses) of each observable, separately for the treatment group and for the control group. The last column reports the *p*-value from a *t*-test comparing the mean of each observable between the two groups. I obtain accounting variables from Compustat and market value of equity (i.e., stock price times number of shares) from CRSP.

The two groups do not differ in the characteristics of the companies that they covered or underwrote, based on the *p*-values associated with Tobin's Q and the accounting variables, namely, total assets, sales, leverage ratio, payout ratio, capital expenditures as a fraction of total assets, and R&D expenses as a fraction of total assets. The treatment group bank-industries, however, tended to cover more companies and employ more analysts. That is, all-star analysts tended to be from larger research departments. Each analyst from the treatment group on average covered more companies, which is perhaps a reflection of the all-star analysts' capability and experience. Analysts from the treatment group (including non-all-stars) were on average more senior than those from the control group, which is perhaps unsurprising given that all-star analysts tend to be more senior than their unranked peers. The treatment group bank-industries also underwrote more and larger equity issuances, consistent with my finding, discussed in the next section, that the treatment group had significantly higher average market share than the control group in the pre-Settlement period. Even though these summary statistics suggest that my treatment group and control group are different along a few observable dimensions – likely driven by the way I define these two groups – I demonstrate in the next section that the “parallel trends” assumption is satisfied. That is, the average market share of the treatment group and that of the control group behaved similarly in the period leading up to the Global Settlement.

Table 2: Summary Statistics

Panel A presents regression sample summary statistics. Sample period is 1998-2007, with event years 2002 and 2003 excluded. The sample of equity issuances, including IPOs and follow-on offerings (SEOs), excludes ADRs, units, partnerships, and stocks not listed on NYSE, NASDAQ, or AMEX. Also excluded are categories with missing GICS codes, including funds, investment trusts, and non-operating establishments. *Market Share* is the industry market share in a given year, calculated as the gross proceeds raised by an investment bank in a particular industry through deals in which the investment bank acts as a lead or co-lead manager, divided by the total gross proceeds of all deals completed in that industry in that year. *Post* is an indicator that equals 1 for observations from 2004-2007, and 0 for observations from 1998-2001. *Treatment* is an indicator that equals 1 for observations in the treatment group, and 0 otherwise. *No. of Deals in Industry-Year* is the total number of equity issuances in a given industry in a given year. *Underpricing* is the weighted average underpricing of equity issuances underwritten by a bank in an industry in a year; the higher the number of lead or co-lead managers in an issuance, the lower is the weight given to the underpricing of that issuance. Underpricing of an issuance, or first-day return, is defined as the percentage change from the offer price to the first-day closing price. *Tech* is an indicator that equals 1 for observations that belong in a tech industry, and 0 otherwise. *Avg. Issuer Age* is a continuous variable equal to the average issuer age of equity issuances underwritten by a bank in an industry in a year, where the age of an issuer is defined as the difference between the issuer's founding year and the year of the public offering. Panel B reports ex ante characteristics calculated using data from 1998-2001, separately for the treatment group and for the control group. Accounting variables under *Equity Coverage* are of companies that are covered by the treatment and control bank-industries. Accounting variables under *Equity Underwriting* are of companies whose equity issuances are underwritten by the treatment and control bank-industries. The last column reports *p*-values from *t*-tests comparing the mean values between the treatment group and the control group. Standard deviations are shown in parentheses.

Panel A. Regression Sample Summary Statistics

	N	Mean	Std. Dev.	Min	p25	p50	p75	p90	Max
IPOs & SEOs									
Market Share	4966	0.03956	0.08003	0	0	0	0.04508	0.1298	0.45
Post	4966	0.5322	0.499	0	0	1	1	1	1
Treatment	4966	0.445	0.497	0	0	0	1	1	1
No. of Deals in Industry-Year	4966	12.54	18.51	1	3	7	14	27	207
Underpricing	2161	0.05066	0.1161	-0.1	0.002336	0.0133	0.045	0.1378	0.7083
Tech	2161	0.3281	0.4696	0	0	0	1	1	1
Avg. Issuer Age	1772	24.88	25.29	1	8	15	31	62	109
IPOs Only									
Market Share	3792	0.0309	0.07237	0	0	0	0.01045	0.1249	0.375
Post	3792	0.5477	0.4978	0	0	1	1	1	1
Treatment	3792	0.4483	0.4974	0	0	0	1	1	1
No. of Deals in Industry-Year	3792	5.878	9.556	1	2	3	7	12	159
Underpricing	1105	0.1446	0.2674	-0.1458	0.003955	0.05556	0.18	0.3953	1.643
Tech	1105	0.4208	0.4939	0	0	0	1	1	1
Avg. Issuer Age	1050	22.41	26.98	1	6	11	26	59.75	125

Panel B. Ex Ante Characteristics

	(1) Treatment Group	(2) Control Group	(3) p-value
Equity Coverage			
Tobin's Q	1.961 (1.092)	1.914 (1.311)	0.5944
Assets (Millions)	12800 (21400)	12300 (25400)	0.7623
Sales (Millions)	6080 (6300)	5570 (8030)	0.3341
Leverage Ratio	0.3895 (0.1581)	0.4007 (0.1849)	0.3748
Payout Ratio	0.3084 (0.2576)	0.3355 (0.3828)	0.2617
Capex/Assets	0.07488 (0.04266)	0.07463 (0.04224)	0.9366
R&D/Assets	0.05261 (0.06284)	0.05167 (0.06804)	0.8603
No. of Companies Covered (per Year)	22.65 (15.66)	10.11 (9.666)	0.000
No. of Analysts Employed (per Year)	5.696 (3.821)	3.333 (2.413)	0.000
No. of Companies Covered per Analyst (per Year)	3.801 (1.856)	2.895 (1.997)	0.000
Seniority of Analysts	5.898 (2.486)	5.49 (3.188)	0.052
Equity Underwriting			
Tobin's Q	2.776 (2.32)	2.617 (2.769)	0.5448
Assets (Millions)	4750 (9530)	4550 (9170)	0.834
Sales (Millions)	2320 (4180)	2080 (4120)	0.5769
Leverage Ratio	0.3234 (0.2316)	0.3495 (0.2944)	0.3423
Payout Ratio	0.296 (0.5096)	0.3703 (0.7463)	0.3258
Capex/Assets	0.1152 (0.09579)	0.1141 (0.2117)	0.9587
R&D/Assets	0.1215 (0.1693)	0.1621 (0.2221)	0.2032
No. of Equity Issuances Underwritten	5.684 (7.277)	2.731 (3.637)	0.000
Mean Proceeds (Millions)	254.8 (293.9)	202.1 (264.9)	0.0572

CHAPTER 4: RESULTS

4.1. Impact of Global Settlement on Investment Banking Deal Flow

4.1.1. Main Results

Both the Departure Hypothesis and the Optimism Hypothesis predict that the bank-industries that had at least one all-star analyst in the pre-Settlement period would suffer losses in their equity issuance market shares in the post-Settlement period, relative to the bank-industries that had no all-star analysts in the pre-Settlement period. Table 3 Panel A presents difference-in-differences results that are consistent with these predictions. The dependent variable is the industry market share of a bank in a year. Columns (1) and (2) present full-sample results using market share of IPO and follow-on underwritings combined, and market share of IPO underwritings alone, respectively. Similarly, columns (3) and (4) present subsample results using only market share observations that are computed from industry-years with at least three equity issuances in total. The regression samples in this panel exclude the event years 2002 and 2003.

All difference-in-differences coefficients are negative and statistically significant. Full-sample results show that the relative losses suffered by the treatment bank-industries average 2.96% in market share for IPO and follow-on underwritings combined, and 3.79% in market share for IPO underwritings alone, equivalent to relative losses of about $15 million and $9 million *per treatment bank-industry*, respectively, in total underwriting fees between 2004 and 2007. *For all treatment bank-industries*, relative losses in total underwriting fees between 2004 and 2007 amount to about $5.14 billion and $2.72 billion, respectively, for IPO and follow-on underwritings combined and for IPO underwritings alone. Subsample results are qualitatively similar with difference-in-differences coefficients of slightly smaller magnitudes: The relative losses suffered by the treatment bank-industries average 2.48% in market share for IPO and follow-on underwritings combined, and 2.94% in market share for IPO underwritings alone, equivalent to relative losses of about $13 million and $7 million *per treatment bank-industry*, respectively, in total underwriting fees between 2004 and 2007. *For all treatment bank-industries*, subsample results show that relative losses in total underwriting fees between 2004 and 2007 amount to about $4.31 billion and $2.11 billion, respectively, for IPO and follow-on underwritings combined and for IPO underwritings alone.[7]

Both full-sample and subsample results indicate that the treatment bank-industries' relative market share losses were more severe for IPO underwritings than for IPO and follow-on underwritings combined. One explanation for this result is that secondary offering issuers, having established business relationships with underwriters in previous issuances, have the tendency to rehire those underwriters. The stickiness of underwriting relationships could cause the quality of analyst coverage to become a weaker (albeit still important – see, e.g., Krigman, Shaw, and Womack (2001)) determinant in the underwriter choice for secondary offering issuers. Unreported results show a statistically significant, but weaker, treatment effect for follow-on underwritings alone than for both IPO underwritings alone and IPO and follow-on underwritings combined.

The pre-Settlement market share of the treatment bank-industries is on average higher than that of the control bank-industries, as indicated by the positive and statistically significant coefficient on *Treatment*. The difference ranges from 1.68% to 2.06% for IPO and follow-on underwritings combined, which is economically significant given the average pre-Settlement market share of 4.54% for the treatment bank-industries (not reported). The

[7] I compute underwriting fees using an average gross spread of 5.01% of total proceeds for IPO and follow-on underwritings combined, and an average gross spread of 6.66% of total proceeds for IPO underwritings alone. I compute these percentages using equity issuance data from between 2004 and 2007.

difference ranges from 0.75% to 0.93% for IPO underwritings alone, which is economically significant given the average pre-Settlement market share of 2.22% for the treatment bank-industries (not reported).

The sum ("*Sum*") of the coefficient of *Treatment* and the difference-in-differences coefficient represents the difference in average market share between the treatment group and the control group in the post-Settlement period. Wald tests ("*F Statistic, Wald Test*" and "*Two-Sided p-value*") indicate that this sum is negative and statistically significant at the 5% level or lower in all four regressions. In other words, unlike in the pre-Settlement period, in the post-Settlement period, the average market share of the treatment bank-industries became statistically significantly *lower* than that of the control bank-industries, making the relative market share losses of the treatment bank-industries economically significant.

Table 3 Panel B presents the coefficient trend. The regression samples in this panel include the event years 2002 and 2003. Consistent with a causal interpretation of the above results, the coefficient on *Treatment*GS(-1)* is statistically insignificant, but the coefficients on *Treatment*GS(+1)* and *Treatment*GS(2+)* are statistically negative. In other words, the effect of the Global Settlement appears only after the regulation is announced and not before.

Figure 1 Panel A plots the average market share of IPO and follow-on underwritings combined, separately for the treatment bank-industries and for the control bank-industries, for each year from 1998 to 2007. Figure 1 Panel B plots the coefficient trend, or the difference in average market share between the treatment group and the control group, for four years before and four years after the Global Settlement, *relative to the event years*. Figure 2 Panels A and B plot the same graphs but for the market share of IPO underwritings alone. These figures demonstrate that the average market shares of the treatment bank-industries and of the control bank-industries behaved similarly, or exhibit "parallel trends," prior to the Global Settlement. The difference in average market share between these two groups does not exhibit a downward trend prior to the Global Settlement; it declines only after the regulation is announced.

4.1.2. Placebo Results

To further examine whether the patterns observed above are a result of the Global Settlement, I perform an identical analysis using a different sample period, from 1990 to 1999. I define 1994 and 1995 to be the "event years," 1990-1993 to be the "pre-event years," and 1996-1999 to be the "post-event years." The "treatment group" contains sixteen investment banks that I identify to be the then-largest investment banks in the United States, based on both the number of equity issuances underwritten and the equity issuance market share of each bank during 1990-1993. Table 4 Panel A presents the treatment group used in my placebo test. Table 4 Panel B presents the results. All difference-in-differences coefficients are statistically insignificant, consistent with the patterns described in the previous section being specific to the period of the Global Settlement and not a recurring phenomenon.

4.1.3. All-Star Analyst Departures from Sell Side

This section documents post-Settlement departures of all-star analysts from the sell side. There are a total of 382 all-star analysts in my analysis, whom I divide into three categories, as shown in Table 5 Panel A: (1) Full Departure, or all-star analysts who left the sell side from their 2001-employer during 2002-2007; (2) Partial Departure, or all-star analysts who left their 2001-employer to join another sell-side securities firm during 2002-2007, regardless of whether they eventually left the sell side; and (3) No Departure, or all-star analysts who stayed at their 2001-employer throughout the period of 2002-2007. Panel A shows that a total of 217 all-star analysts, or 56.81%, departed from the sell-side research industry from their 2001-employer during 2002-2007, as indicated under *Full Departure*.

Next, to provide a description of my sample of treatment bank-industries in terms of their all-star analyst departure statuses, I translate the three all-star analyst categories from Panel A into three bank-industry categories, as shown in Table 5 Panel B: (1) Full Departure, or bank-industries that had at least one all-star analyst who left the sell side from their 2001-employer during 2002-2007; (2) Partial Departure, or bank-industries that had at least one all-star analyst who left their 2001-employer to join another sell-side securities firm during 2002-2007, but no all-star analysts who left the sell side altogether; and (3) No Departure, or bank-industries whose all-star analysts stayed at their 2001-employer throughout the period of 2002-2007. Panel B shows that a total of 233 bank-industries, or 68.33% of the treatment bank-industries, contained at least one all-star analyst who departed from the sell-side research industry from their 2001-employer during 2002-2007, as indicated under *Full Departure*. The number of bank-industries under *Full Departure* in Panel B is higher than the number of all-star analysts under *Full Departure* in Panel A since one analyst could be named an all-star in multiple industries.

Lastly, Table 5 Panel C reports the number of post-Settlement departures of all-star analysts from the sell side (see Panel A, *Full Departure*) by year from 2002 to 2007. Panel C shows that among the all-star analyst departures from the sell side during 2002-2007, 67.74% occurred during 2002-2004, which are the years during which the Global Settlement and the SRO regulations were announced, plus the year immediately after. Over 20% of departures occurred in each of 2002, 2003, and 2004, and about 11% occurred in each of 2005, 2006, and 2007. These results are consistent with all-star analysts departing from the sell side in response to the Global Settlement and the SRO regulations.

4.1.4. Repeat All-Stars

The Departure Hypothesis predicts a stronger treatment effect for the bank-industries whose all-star analysts were on average of higher quality than for the bank-industries whose all-star analysts were of lower quality. This section tests this hypothesis by using the number of times for which an analyst was named an all-star as a proxy for the analyst's quality. For each treatment bank-industry, I compute the average number of times for which its all-star analysts were named all-stars during 1998-2001. Since analysts are ranked by *Institutional Investor* annually, an analyst could be named an all-star for a maximum of four times during this period. I then divide the treatment bank-industries into two groups: Treatment group 1 contains bank-industries whose all-star analysts were named all-stars for an average of one to two times during 1998-2001, and treatment group 2 contains bank-industries whose all-star analysts were named all-stars for an average of three to four times during 1998-2001.

Table 6 Panel A shows that the difference-in-differences coefficient is more negative for treatment group 2 than for treatment group 1, consistent with bank-industries with higher-quality all-star analysts suffering more post-Settlement losses in their equity issuance market shares than bank-industries with lower-quality all-star analysts. The difference between the two difference-in-differences coefficients ("*Difference (1-2)*"), however, is not statistically significant based on Wald tests ("*F Statistic, Wald Test*" and "*Two-Sided p-value*").

In order to better distinguish multi-year all-stars, or "superstars," from the rest of the all-stars, I extend my analyst rankings data back to 1994 in order to compute the number of times for which an analyst was named an all-star during the eight-year period from 1994 to 2001. I then divide the treatment bank-industries into four groups: Treatment group 1 contains bank-industries whose all-star analysts were named all-stars for an average of one to two times during 1994-2001, treatment group 2 contains bank-industries whose all-star analysts were named all-stars for an average of three to four times during 1994-2001, treatment group 3 contains bank-industries whose all-star analysts were named all-stars for an average of five to six times during 1994-2001, and treatment group 4 contains bank-industries whose all-star analysts were named all-stars for an average of seven to eight times during 1994-2001.

Table 6 Panel B shows that the difference-in-differences coefficients increase in magnitude (i.e., become increasingly negative) from treatment group 1 to treatment group 4 in all four regressions, and the increase is monotonic for IPO underwritings. The difference between the difference-in-differences coefficient of treatment group 1 and that of treatment group 4 ("*Difference (1-4)*"), however, is not statistically significant based on Wald tests ("*F Statistic, Wald Test*" and "*Two-Sided p-value*"). Nevertheless, as in Panel A, the relative strength of treatment effects across the treatment groups in Panel B shows support for the Departure Hypothesis, which predicts that the more talented an all-star analyst, the higher is her bank's losses in its equity issuance market share in the analyst's industry after the analyst's departure.

4.1.5. Analyst Quality

The Departure Hypothesis predicts a stronger treatment effect for the bank-industries whose all-star analysts were on average of higher quality than for the bank-industries whose all-star analysts were of lower quality. The Optimism Hypothesis predicts a stronger treatment effect for the bank-industries whose all-star analysts were on average more optimistically biased than for the bank-industries whose all-star analysts were less optimistically biased. This section tests these predictions using five measures of analyst quality constructed based on analyst behavior – namely, forecast accuracy, coverage frequency, coverage timeliness, forecast optimism, and abnormal recommendation levels – and three composite measures – namely, *Reputation*, *Bias*, and *Reputation-Bias* – constructed using these five measures. I first present descriptive statistics on analyst quality in Table 7. I then report regression results in Table 8 and Table 9.

For each sell-side analyst that exists in I/B/E/S for 1998-2001, I compute a forecast accuracy score for each firm in each year using the scoring methodology described in Section 3.1.4. I then compute the analyst's pre-Settlement *Forecast Accuracy* score by averaging the analyst's scores for 1998-2001 (*Proxy I*), or by counting the proportion of the analyst's scores for 1998-2001 that are 50 or higher (*Proxy II*). *Proxy I* ranges from 0 to 100, and *Proxy II* ranges from 0 to 1. I compute *Coverage Frequency*, *Coverage Timeliness*, *Forecast Optimism*, and *Abnormal Recommendation Levels* in a similar way. I compute *Reputation* by averaging *Forecast Accuracy*, *Coverage Frequency*, and *Coverage Timeliness*, *Bias* by averaging *Forecast Optimism* and *Abnormal Recommendation Levels*, and *Reputation-Bias* by subtracting *Bias* from *Reputation*. Last, for each measure I rank the scores of all analysts to obtain a percentile for each analyst.

Table 7 Panel A presents descriptive statistics on the percentiles of all-star analysts, separately for *Proxy I* and for *Proxy II*. For *Forecast Accuracy*, *Coverage Frequency*, *Coverage Timeliness*, and *Reputation*, both the average percentile of all-stars and the percentage of all-stars with a percentile equal to or above 50 are consistently higher, and in most cases much higher, than 50. For *Forecast Optimism*, *Abnormal Recommendation Levels*, and *Bias*, these statistics are consistently below 50. Lastly, for *Reputation-Bias*, which measures the overall quality of an analyst, the statistics are again higher than 50. Consistent with existing studies in the literature, these statistics suggest that, compared to non-all-star analysts, all-star analysts are on average more accurate in their earnings forecasts, provide more frequent and timely coverage, and are less optimistically biased in their earnings forecasts and stock recommendations.

Table 7 Panel B compares average analyst quality between the treatment bank-industries and the control bank-industries. For each composite measure, I report the mean and standard deviation (in parentheses), separately for the treatment group and for the control group.

The third column reports the difference in the mean value between the two groups, and the last column reports the *p*-value from a *t*-test comparing mean values between the two groups. The results show that the average *Reputation* score is statistically significantly more positive, the average *Bias* score statistically significantly more negative, and the average *Reputation-Bias* score again statistically significantly more positive, for the treatment group than for the control group. These results suggest that, compared to analysts at the control bank-industries, analysts at the treatment bank-industries are on average less optimistically biased and are of higher overall quality. This finding is unsurprising given that all-star analysts are present only in the treatment group and not in the control group, and that all-star analysts exhibit superior performance compared to their unranked peers, as shown in Panel A.

Table 8 presents regression results for each of the five analyst quality measures, using *Proxy I*.[8] *Low Accuracy* is an indicator that equals 1 if the average *Forecast Accuracy* percentile of all-star analysts in a given bank-industry is below 50, and 0 otherwise. *High Accuracy* is an indicator that equals 1 if the average *Forecast Accuracy* percentile of all-star analysts in a given bank-industry is 50 or above, and 0 otherwise. *Low Frequency* and *High Frequency*, *Low Timeliness* and *High Timeliness*, *Low Optimism* and *High Optimism*, and *Low Recommendation* and *High Recommendation* are constructed in similar ways. Panel A presents regression results for IPO and follow-on underwritings combined, and Panel B presents regression results for IPO underwritings alone.

The relative magnitudes of the difference-in-differences coefficients show that the treatment effect is stronger for bank-industries whose all-star analysts were on average more accurate in their earnings forecasts, provided more frequent coverage, and were less opti-

[8]Unreported results using *Proxy II* are qualitatively similar. Unreported results using subsamples that include only market share observations from industry-years with at least three equity issuances are also qualitatively similar.

mistically biased in their earnings forecasts and stock recommendations. Although the difference in treatment effect between the “low quality” group and the “high quality” group is statistically insignificant for IPO and follow-on underwritings combined, it is statistically significant for IPO underwritings alone, based on Wald tests (“*F Statistic*, *Wald Test*” and “*Two-Sided p-value*”).

There is, however, no evidence that the treatment effect is stronger for bank-industries whose all-star analysts were on average more timely in their coverage. It is likely that coverage timeliness is considered a valuable quality by issuers only to the extent that it is correlated with superior forecast ability and frequent coverage. To simultaneously account for different aspects of an analyst’s quality, I examine composite quality measures in Table 9.

Table 9 presents regression results for each of the three composite measures using *Proxy I*, separately for IPO and follow-on underwritings combined, and for IPO underwritings alone.[9] The “low quality” and “high quality” indicators are constructed in a similar way as in Table 8. The results show that the treatment effect is stronger for bank-industries whose all-star analysts on average have higher *Reputation* scores, lower *Bias* scores, and higher *Reputation-Bias* scores. As in Table 8, although the difference in treatment effect between the “low quality” group and the “high quality” group is statistically insignificant for IPO and follow-on underwritings combined, it is statistically significant for IPO underwritings alone based on Wald tests (“*F Statistic*, *Wald Test*” and “*Two-Sided p-value*”).

Overall, the results in Table 8 and Table 9 are consistent with the Departure Hypothesis: The higher the average quality of the all-star analysts in a treatment bank-industry in the pre-Settlement period, the more severe were the bank-industry’s relative losses in equity

[9]Unreported results using *Proxy II* are qualitatively similar. Unreported results using subsamples that include only market share observations from industry-years with at least three equity issuances are also qualitatively similar.

issuance market share in the post-Settlement period. The results are inconsistent with the Optimism Hypothesis. This hypothesis predicts that the more optimistically biased the treatment bank-industry's all-star analysts were in the pre-Settlement period, the more severe would be the bank-industry's post-Settlement losses in equity issuance market share. I find results that are the opposite of this prediction.

4.2. Impact of Global Settlement on Cost of Equity Capital

In this section, I test the Changing Risk Hypothesis and the Analyst Lust Hypothesis using the same difference-in-differences framework with a different dependent variable – the weighted average underpricing of equity issuances underwritten by a bank in an industry in a year. Both hypotheses predict a differential effect of the Global Settlement on the cost of equity capital between issuances with higher valuation risk and issuances with lower valuation risk, which I evaluate using a triple-difference specification with two measures of valuation risk. I provide suggestive evidence that, while both hypotheses seem to be at work, the Changing Risk Hypothesis plays a more important role in the subgroup of high-valuation-risk IPOs than in the subgroup of low-valuation-risk IPOs, consistent with the departure of research talent causing the cost of equity capital to rise, especially for issuances that are intrinsically more difficult to value.

To recall, the Changing Risk Hypothesis considers underpricing from the perspective of investors, whereas the Analyst Lust Hypothesis considers underpricing from the perspective of issuers. Specifically, the Changing Risk Hypothesis states that, as all-star analysts depart from the treatment bank-industries in the post-Settlement period, investor clients at these bank-industries demand more underpricing as compensation for increased valuation risk, causing the difference in average underpricing between the treatment bank-industries and the control bank-industries to widen, *and to widen more for issuances that are more* *difficult to value*. On the other hand, the Analyst Lust Hypothesis states that, as all-star analysts depart from the treatment bank-industries in the post-Settlement period, issuers become less willing to tolerate underpricing as compensation to the underwriter for providing high-quality research coverage, causing the difference in average underpricing between the treatment bank-industries and the control bank-industries to narrow, *and to narrow more for issuances that are more difficult to value*.

4.2.1. Tech vs. Non-Tech

The first measure of valuation risk is whether an issuer belongs in a tech industry, assuming that tech issuances are intrinsically more difficult to value than non-tech issuances. Tech industries are defined in Section 3.1.3. Table 10 Panel A presents regression results using this risk measure, represented by an indicator that equals 1 if the corresponding industry is a tech industry, and 0 otherwise. The coefficient on *Treatment*Tech*Post* is the triple-difference coefficient of interest.

Column (1) shows that results are statistically insignificant for IPO and follow-on underwritings combined. Since secondary offering issuers have been publically traded and have established a track record, their issuances likely have a low valuation risk profile in general regardless of the issuer's industry categorization. I therefore focus on IPO underwritings alone.

Column (2) shows a positive and statistically significant triple-difference coefficient, which suggests a positive differential effect of the Global Settlement on the cost of equity capital between high-valuation-risk IPOs and low-valuation-risk IPOs. This preliminary observation suggests consistency with the Changing Risk Hypothesis. The negative and statistically significant coefficient on *Treatment*Post*, however, suggests that the effect of the Analyst Lust Hypothesis dominates in the subgroup of low-valuation-risk IPOs. This result is confirmed in column (3), which reports the regular difference-in-differences results for the subgroup of low-risk IPOs. Column (4) reports the regular difference-in-differences results for the subgroup of high-risk IPOs. The difference-in-differences coefficient is statistically insignificant, suggesting that the effects of the two hypotheses offset each other. I infer from this result and from the positive triple-difference coefficient in column (2) that the effect of the Changing Risk Hypothesis is stronger in the subgroup of high-risk IPOs than in the subgroup of low-risk IPOs. That is, the departure of research talent has a larger impact on the decisions of investors in IPOs that are more difficult to value, raising the underpricing of these IPOs more substantially. Importantly, the triple-difference coefficient in column (2) suggests that the differential effect of the Global Settlement on the cost of equity capital between high-risk IPOs and low-risk IPOs amounts to an economically significant 17.8% in underpricing, or an average amount of money left on the table of about $34 million.[10]

Figure 3 plots average underpricing for the treatment bank-industries and for the control bank-industries, separately for the low-risk subgroup (i.e., *Non-Tech Industries*) and for the high-risk subgroup (i.e., *Tech Industries*), for each year from 1998 to 2007. First, the graphs show that the average underpricings of the treatment group and of the control group behaved similarly prior to the Global Settlement, satisfying the “parallel trends” assumption. Second, the graphs show some evidence that the difference in average underpricing between the treatment group and the control group widens more for high-risk IPOs than for low-risk IPOs, consistent with the positive triple-difference coefficient discussed above. It is, however, not clear from the “Non-Tech” graph that the difference between average underpricing narrows for the subgroup of low-risk IPOs, as suggested by the regression results discussed above.

[10] A placebo test using the sample period of 1990-1999, "event years" of 1994 and 1995, and the "treatment group" described in Section 4.1.2 yields statistically insignificant results (not reported).

Lastly, column (5) in Panel A presents the triple-difference coefficient trend. Statistically significant coefficients on *Treatment*GS(0)* and *Treatment*Tech*GS(0)* suggest that the effect of the Global Settlement appeared not only after but also during the event years. An explanation for this result is that investors and issuers likely anticipated the regulation and its potential impact of causing some of the most highly-regarded and highly-paid analysts to exit the sell-side research profession. Since a large portion of a research analyst's service is provided in the aftermarket, the anticipation of an analyst's future departure from the underwriter of an equity offering could affect the decisions of the investors and issuers prior to, or at the time of, the equity offering.

4.2.2. Issuer Age

The second measure of valuation risk is issuer age, assuming that issuances from younger issuers are intrinsically more difficult to value than those from older issuers. Table 10 Panel B presents regression results using this risk measure. The coefficient on *Treatment*Avg Issuer Age*Post* is the triple-difference coefficient of interest, where *Avg Issuer Age* is a continuous variable equal to the average issuer age for equity issuances underwritten by a bank in an industry in a year. Column (1) shows that results are statistically insignificant for IPO and follow-on underwritings combined. As discussed above, follow-on issuers have traded securities and are known to the marketplace, mitigating their youth. Column (2) shows a negative and statistically significant triple-difference coefficient for IPO underwritings, suggesting a positive differential effect of the Global Settlement on the cost of equity capital between high-valuation-risk IPOs (i.e., from younger issuers) and low-valuation-risk IPOs (i.e., from older issuers). Specifically, the coefficient of -0.00336 means that a one-year *decrease* in average issuer age (i.e., an *increase* in valuation risk) is associated with a positive differential effect of 0.336% in underpricing. This result is consistent with the Changing Risk Hypothesis. Column (3) presents the triple-difference coefficient trend.

The statistically significant coefficient on *Treatment*Avg Issuer Age*GS(0)* suggests that the effect of the Global Settlement appeared not only after but also during the event years.

To compare the triple-difference result (i.e., the differential effect of the Global Settlement between high-risk IPOs and low-risk IPOs) between Panel A and Panel B, I note that the standard deviation of *Tech* is 0.4939, and the standard deviation of *Avg Issuer Age* is 26.98 (Table 2 Panel A). The triple-difference coefficient of 0.178 in Panel A suggests that a two-standard-deviation increase in *Tech* from 0 to 1 (i.e., an *increase* in valuation risk) leads to a positive differential effect of 17.8% in underpricing. In comparison, the triple-difference coefficient of -0.00336 in Panel B suggests that a two-standard-deviation *decrease* in Avg Issuer Age (i.e., an *increase* in valuation risk) leads to a positive differential effect of about 18.13% in underpricing. Thus, the two sets of triple-difference results are of comparable magnitudes.

Table 3: Effect of Global Settlement on Investment Banking Deal Flow

Panel A reports difference-in-differences results. Panel B reports coefficient dynamics. Sample period is 1998-2007, with event years 2002 and 2003 excluded from the regression samples in Panel A and included in the regression samples in Panel B. Results are reported separately for IPOs and follow-on offerings (SEOs) combined, and for IPOs alone. Regressions (1) and (2) use full samples. Regressions (3) and (4) use subsamples that include only market share observations computed from industry-years with a total of at least three equity issuances. The dependent variable is industry market share in a given year, calculated as the gross proceeds raised by an investment bank in a particular industry through deals in which the investment bank acts as a lead or co-lead manager, divided by the total gross proceeds of all deals completed in that industry in that year. *Treatment* is an indicator that equals 1 for observations in the treatment group, and 0 otherwise. *Post* is an indicator that equals 1 for observations from 2004-2007, and 0 for observations from 1998-2001. *GS(-1)* is an indicator for one year prior to the event years, *GS(0)* is an indicator for the event years, *GS(+1)* is an indicator for one year after the event years, and *GS(2+)* is an indicator for two or more years after the event years. Coefficients are estimated by OLS. All regression specifications include both bank-year and industry-year fixed effects. Robust standard errors are clustered at the bank level. *p*-values are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Difference-in-Differences Results

	(1) IPOs & SEOs Full Sample	(2) IPOs Only Full Sample	(3) IPOs & SEOs Subsample	(4) IPOs Only Subsample
Treatment	0.0206***	0.00930***	0.0168***	0.00745***
	(0.000)	(0.001)	(0.000)	(0.009)
Post*Treatment	-0.0296***	-0.0379***	-0.0248***	-0.0294***
	(0.000)	(0.000)	(0.000)	(0.000)
Bank-Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Sum	-0.009	-0.0286	-0.008	-0.02195
F Statistic, Wald Test	4.69	21.69	3.75	14.99
Two-Sided p-value	0.0308	0.000	0.0534	0.0001
Observations	4966	3792	3924	2272
R-squared Within	0.188	0.195	0.208	0.265

Panel B. Coefficient Dynamics

	(1) IPOs & SEOs Full Sample	(2) IPOs Only Full Sample	(3) IPOs & SEOs Subsample	(4) IPOs Only Subsample
Treatment	0.0207***	0.00790**	0.0174***	0.00850***
	(0.000)	(0.011)	(0.000)	(0.007)
Treatment*GS(-1)	-0.000157	0.00719	-0.00211	-0.00794
	(0.985)	(0.395)	(0.848)	(0.425)
Treatment*GS(0)	0.00285	0.00933	0.00452	0.00412
	(0.692)	(0.268)	(0.517)	(0.619)
Treatment*GS(+1)	-0.0232***	-0.0399***	-0.0124	-0.0262***
	(0.005)	(0.000)	(0.111)	(0.001)
Treatment*GS(2+)	-0.0318***	-0.0354***	-0.0299***	-0.0323***
	(0.000)	(0.000)	(0.000)	(0.001)
Bank-Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	6316	4589	4890	2553
R-squared Within	0.176	0.182	0.199	0.260

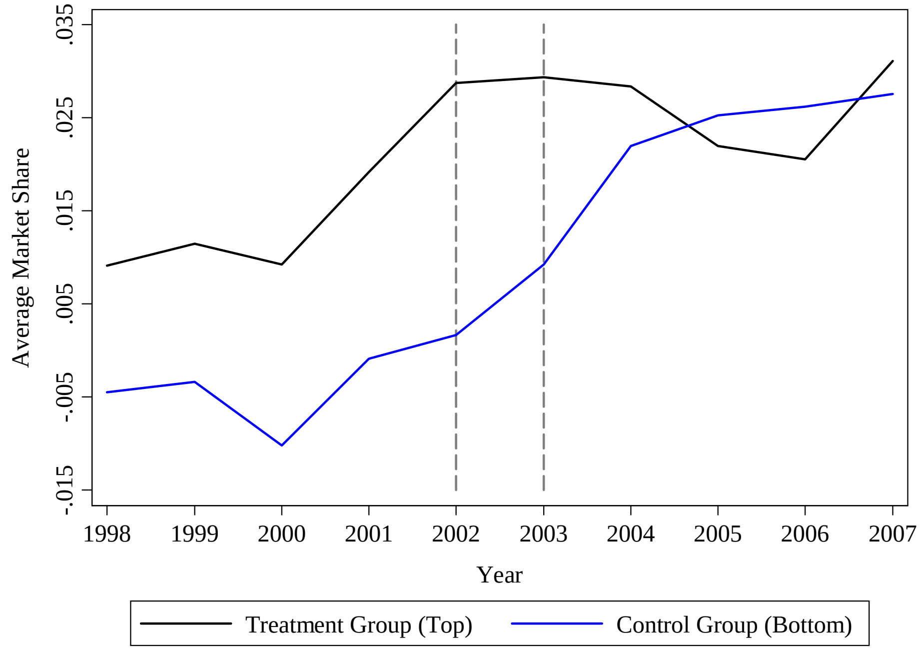

Figure 1: Market Share of IPOs and Follow-On Offerings, Treatment Group vs. Control Group

Panel A. Levels

This panel plots the estimated coefficients of the year dummies (plus the intercept) in the following regression, separately for the treatment group and for the control group:

$$MarketShare_{ijt} = Intercept + \sum_{t=1999-2007} \beta * Year\_Dummy_t + \zeta_i + \lambda_j + \mu_{ijt}$$

The dependent variable is market share of IPOs and follow-on offerings of bank i in year t in industry j. The regression specification includes both bank fixed effects (ζ_i) and industry fixed effects (λ_j), where industry is defined using 6-digit GICS codes. Sample period is 1998-2007. Coefficients are estimated by OLS. Robust standard errors are clustered at the bank level.

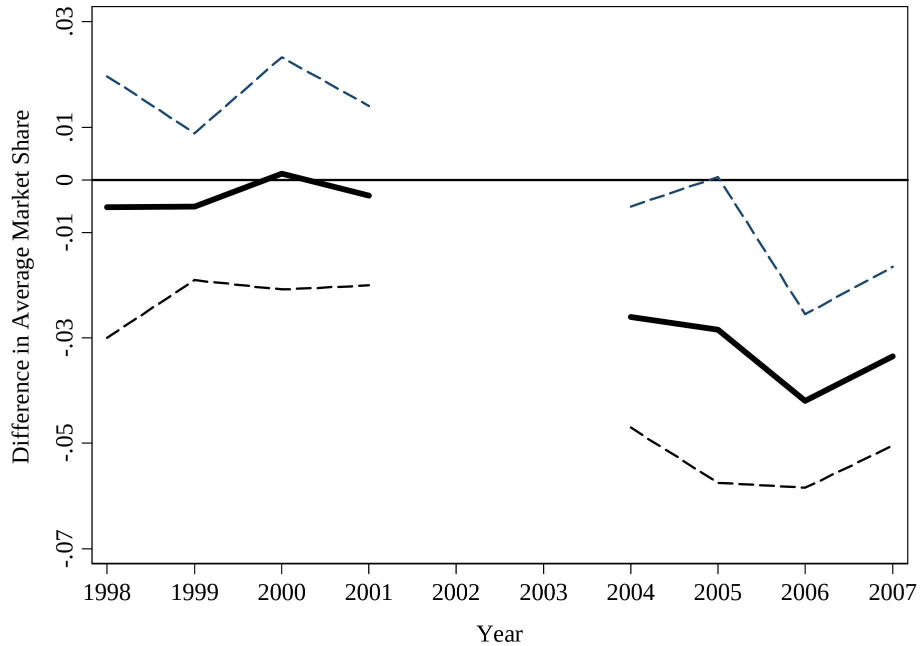

Figure 1 (continued)

Panel B. Difference

This panel plots the estimated coefficients of the interaction terms in the following regression:

$$MarketShare_{ijt} =$$

$$Treatment_{ij} + \sum_{t=1998-2001,2004-2007} \beta * Treatment_{ij} * Year\_Dummy_t$$

$$+\theta_{it} + \delta_{jt} + \mu_{ijt}$$

The dependent variable is market share of IPOs and follow-on offerings of bank i in year t in industry j. The regression specification includes both bank-year fixed effects (θ_{it}) and industry-year fixed effects (δ_{jt}), where industry is defined using 6-digit GICS codes. Sample period is 1998-2007. Each point estimate represents the difference in average market share relative to the event years 2002 and 2003. The dashed lines show the 95% confidence interval. Coefficients are estimated by OLS. Robust standard errors are clustered at the bank level.

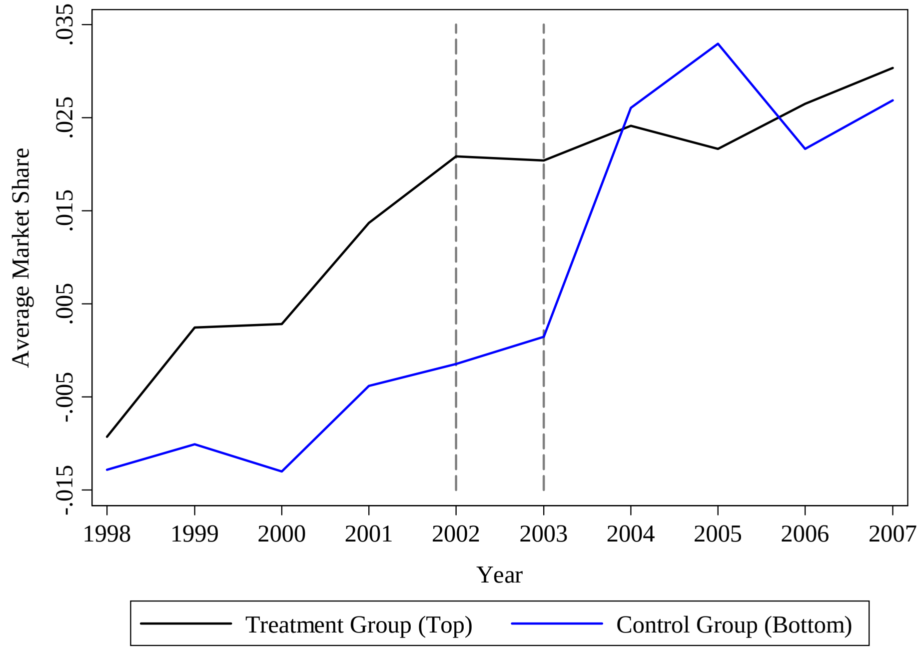

Figure 2: Market Share of IPOs, Treatment Group vs. Control Group

Panel A. Levels

This panel plots the estimated coefficients of the year dummies (plus the intercept) in the following regression, separately for the treatment group and for the control group:

$$MarketShare_{ijt} = Intercept + \sum_{t=1999-2007} \beta * Year\_Dummy_t + \zeta_i + \lambda_j + \mu_{ijt}$$

The dependent variable is market share of IPOs of bank i in year t in industry j. The regression specification includes both bank fixed effects (ζ_i) and industry fixed effects (λ_j), where industry is defined using 6-digit GICS codes. Sample period is 1998-2007. Coefficients are estimated by OLS. Robust standard errors are clustered at the bank level.

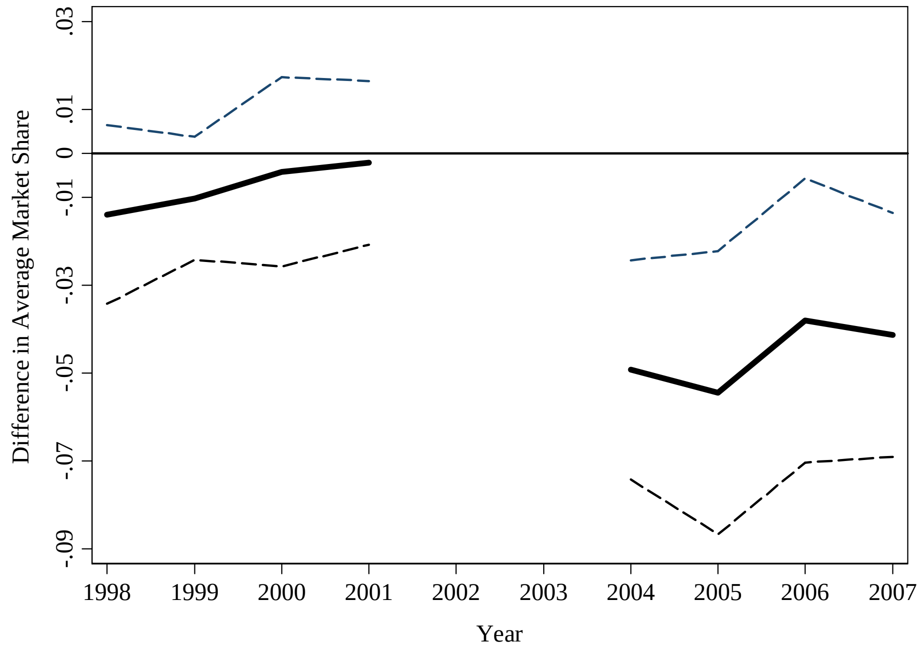

Figure 2 (continued)

Panel B. Difference

This panel plots the estimated coefficients of the interaction terms in the following regression:

$$MarketShare_{ijt} =$$

$$Treatment_{ij} + \sum_{t=1998-2001,2004-2007} \beta * Treatment_{ij} * Year\_Dummy_t$$

$$+\theta_{it} + \delta_{jt} + \mu_{ijt}$$

The dependent variable is market share of IPOs of bank i in year t in industry j. The regression specification includes both bank-year fixed effects (θ_{it}) and industry-year fixed effects (δ_{jt}), where industry is defined using 6-digit GICS codes. Sample period is 1998-2007. Each point estimate represents the difference in average market share relative to the event years 2002 and 2003. The dashed lines show the 95% confidence interval. Coefficients are estimated by OLS. Robust standard errors are clustered at the bank level.

Table 4: Placebo Test

The placebo sample period is 1990-1999, with "event years" 1994 and 1995 excluded. Analysis is performed on sixteen investment banks identified to be the then-largest investment banks in the US, based on both the number of equity issuances underwritten and the equity issuance market share of each bank during 1990-1993. Treatment and control groups are defined in exactly the same way as in the Global Settlement analysis. Panel A describes the treatment group for the placebo test. Panel B reports difference-in-differences results, separately for IPOs and follow-on offerings (SEOs) combined, and for IPOs alone. Regressions (1) and (2) use full samples. Regressions (3) and (4) use subsamples that include only market share observations computed from industry-years with a total of at least three equity issuances. The dependent variable is industry market share in a given year, calculated as the gross proceeds raised by an investment bank in a particular industry through deals in which the investment bank acts as a lead or co-lead manager, divided by the total gross proceeds of all deals completed in that industry in that year. *Treatment* is an indicator that equals 1 for observations in the treatment group, and 0 otherwise. *Post* is an indicator that equals 1 for observations from 1996-1999, and 0 for observations from 1990-1993. Coefficients are estimated by OLS. All regression specifications include both bank-year and industry-year fixed effects. Robust standard errors are clustered at the bank level. *p*-values are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Treatment Group

Investment Bank	No. of Industries with All-Stars	Industry-Year Observations
Alex. Brown & Sons	9	69
Bear Stearns	16	120
CS First Boston	38	281
Donaldson, Lufkin and Jenrette	37	275
Goldman Sachs	33	245
Kidder Peabody	31	230
Lehman Brothers	46	330
Merrill Lynch	46	332
Montgomery Securities	10	77
Morgan Stanley	37	268
Oppenheimer & Co.	24	176
PaineWebber	28	198
Prudential Securities	34	252
Robertson, Stephens & Co.	1	8
Salomon Brothers	28	205
Smith Barney Shearson	31	231
Total	449	3297 (45.38%)

Panel B. Difference-in-Differences Results

	(1) IPOs & SEOs Full Sample	(2) IPOs Only Full Sample	(3) IPOs & SEOs Subsample	(4) IPOs Only Subsample
Treatment	0.00824	0.0136*	0.00935	0.0157**
	(0.181)	(0.054)	(0.124)	(0.048)
Post*Treatment	0.00211	-0.00500	0.00249	-0.00204
	(0.769)	(0.434)	(0.742)	(0.775)
Bank-Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	7265	6385	6292	4754
R-squared Within	0.148	0.109	0.179	0.145

Table 5: All-Star Analyst Departures

An all-star analyst's year of departure from the sell side is first identified as the last year in which the analyst's earnings estimates appear in I/B/E/S. This departure year is then checked against the Analyst Registry from Nelson's Directory of Investment Research, which reports, for every year up to 2008, the names and employers of all active sell-side research analysts. There are a total of 382 all-star analysts in my analysis, who are divided into three categories as shown in Panel A: 1) Full Departure, or all-star analysts who left the sell side from their 2001-employer during 2002-2007; 2) Partial Departure, or all-star analysts who left their 2001-employer to join another sell-side security firm during 2002-2007, regardless of whether they eventually left the sell side; and 3) No Departure, or all-star analysts who stayed at their 2001-employer throughout the period of 2002-2007. Panel B translates the three all-star analyst categories from Panel A into three bank-industry categories: 1) Full Departure, or bank-industries that had at least one all-star analyst who left the sell side from their 2001-employer during 2002-2007; 2) Partial Departure, or bank-industries that had at least one all-star analyst who left their 2001-employer to join another sell-side security firm during 2002-2007, but not all-star analysts who left the sell side altogether; and 3) No Departure, or bank-industries whose all-star analysts stayed at their 2001-employer throughout the period of 2002-2007. Panel C reports the number of all-star analyst departures from the sell side (see Panel A, Full Departure) by year from 2002 to 2007.

Panel A. Categories of All-Star Analysts

	No. of All-Stars	Percentage of All-Stars (%)
Full Departure	217	56.81
Partial Departure	64	16.75
No Departure	101	26.44
Total	382	100.00

Panel B. Categories of Bank-Industries

	No. of Bank-Industries	Percentage of Bank-Industries (%)
Full Departure	233	68.33
Partial Departure	44	12.9
No Departure	64	18.77
Total	341	100.00

Panel C. Full Departures by Year

	No. of Departures	Percentage of Departures (%)
2002	52	23.96
2003	50	23.04
2004	45	20.74
2005	22	10.14
2006	24	11.06
2007	24	11.06
Total	217	100.00

Table 6: Repeat All-Stars

This table reports difference-in-differences results obtained by distinguishing between repeat all-stars and non-repeat all-stars. Sample period is 1998-2007, with event years 2002 and 2003 excluded from the regression samples. Results are reported separately for IPOs and follow-on offerings (SEOs) combined, and for IPOs alone. Regressions (1) and (2) use full samples. Regressions (3) and (4) use subsamples that include only market share observations computed from industry-years with a total of at least three equity issuances. The dependent variable is industry market share in a given year. *Post* is an indicator that equals 1 for observations from 2004-2007, and 0 for observations from 1998-2001. Panel A reports difference-in-differences results using two treatment subgroups. *Treatment Grp 1* is an indicator for bank-industries whose all-star analysts were named all-stars for an average of one to two times during 1998-2001, and *Treatment Grp 2* is an indicator for bank-industries whose all-star analysts were named all-stars for an average of three to four times during 1998-2001. *Difference (1-2)* is computed by subtracting the coefficient on *Post*Treatment Grp 2* from the coefficient on *Post*Treatment Grp 1*. Wald test examines whether *Difference (1-2)* is statistically significantly different from 0. Panel B reports difference-in-differences results using four treatment subgroups. *Treatment Grp 1* is an indicator for bank-industries whose all-star analysts were named all-stars for an average of one to two times during 1994-2001, *Treatment Grp 2* is an indicator for bank-industries whose all-star analysts were named all-stars for an average of three to four times during 1994-2001, *Treatment Grp 3* is an indicator for bank-industries whose all-star analysts were named all-stars for an average of five to six times during 1994-2001, and *Treatment Grp 4* is an indicator for bank-industries whose all-star analysts were named all-stars for an average of seven to eight times during 1994-2001. *Difference (1-4)* is computed by subtracting the coefficient on *Post*Treatment Grp 4* from the coefficient on *Post*Treatment Grp 1*. Wald test examines whether *Difference (1-4)* is statistically significantly different from 0. Coefficients are estimated by OLS. All regression specifications include both bank-year and industry-year fixed effects. Robust standard errors are clustered at the bank level. *p*-values are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Two Treatment Groups

	(1) IPOs & SEOs Full Sample	(2) IPOs Only Full Sample	(3) IPOs & SEOs Subsample	(4) IPOs Only Subsample
Treatment Grp 1	0.0129**	0.00930***	0.0101*	0.00715*
	(0.021)	(0.002)	(0.093)	(0.057)
Treatment Grp 2	0.0255***	0.00930***	0.0213***	0.00764***
	(0.000)	(0.004)	(0.000)	(0.004)
Post*Treatment Grp 1	-0.0235***	-0.0365***	-0.0193***	-0.0276***
	(0.002)	(0.000)	(0.002)	(0.009)
Post*Treatment Grp 2	-0.0324***	-0.0399***	-0.0274***	-0.0316***
	(0.000)	(0.000)	(0.000)	(0.000)
Bank-Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Difference (1-2)	0.0089	0.0034	0.0081	0.004
F Statistic, Wald Test	2.85	0.34	1.8	0.17
Two-Sided p-value	0.0919	0.559	0.1808	0.6796
Observations	4966	3792	3924	2272
R-squared Within	0.189	0.195	0.209	0.265

Panel B. Four Treatment Groups

	(1) IPOs & SEOs Full Sample	(2) IPOs Only Full Sample	(3) IPOs & SEOs Subsample	(4) IPOs Only Subsample
Treatment Grp 1	0.00952***	0.00340	0.00844**	-0.00147
	(0.010)	(0.504)	(0.043)	(0.803)
Treatment Grp 2	0.0139	0.0114**	0.0111	0.00794**
	(0.132)	(0.018)	(0.112)	(0.039)
Treatment Grp 3	0.0349***	0.0137***	0.0305***	0.0133**
	(0.000)	(0.005)	(0.000)	(0.019)
Treatment Grp 4	0.0230***	0.00692**	0.0189***	0.00921***
	(0.000)	(0.014)	(0.003)	(0.003)
Post*Treatment Grp 1	-0.0235***	-0.0344***	-0.0201***	-0.0206*
	(0.002)	(0.000)	(0.006)	(0.058)
Post*Treatment Grp 2	-0.0245**	-0.0370***	-0.0205***	-0.0297***
	(0.018)	(0.000)	(0.003)	(0.000)
Post*Treatment Grp 3	-0.0360***	-0.0362***	-0.0323***	-0.0314**
	(0.000)	(0.009)	(0.000)	(0.019)
Post*Treatment Grp 4	-0.0292**	-0.0432***	-0.0245**	-0.0364***
	(0.034)	(0.001)	(0.023)	(0.001)
Bank-Year FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Difference (1-4)	0.0057	0.0088	0.0044	0.0158
F Statistic, Wald Test	0.29	0.42	0.22	1.4
Two-Sided p-value	0.5879	0.5191	0.6387	0.2384
Observations	4966	3792	3924	2272
R-squared Within	0.191	0.197	0.211	0.267

Table 7: Analyst Quality – Descriptive Statistics

Panel A reports descriptive statistics on all-star analysts' quality calculated using data from 1998-2001. To compute an analyst's *Forecast Accuracy*, a scoring methodology is used to compute the analyst's forecast accuracy score for each firm in each year. The analyst's *Forecast Accuracy* score is then computed by averaging the analyst's scores during 1998-2001 (*Proxy I*), or by counting the proportion of the analyst's scores during 1998-2001 that are 50 or higher (*Proxy II*). *Coverage Frequency, Coverage Timeliness, Forecast Optimism,* and *Abnormal Recommendation Levels* are computed in a similar way. *Reputation* is the average of *Forecast Accuracy, Coverage Frequency,* and *Coverage Timeliness*. *Bias* is the average of *Forecast Optimism* and *Abnormal Recommendation Levels*. *Reputation-Bias* is computed by subtracting *Bias* from *Reputation*. For each quality measure, percentiles of all-star analysts are obtained by ranking all analysts that exist in I/B/E/S during 1998-2001 based on their scores. Column A reports the average percentile of all-star analysts. Column B reports the percentage of all-star analysts with a percentile of 50 or higher. Panel B compares average analyst quality between the treatment group and the control group using the composite quality measures calculated using data from 1998-2001. The last column reports *p*-values from *t*-tests comparing the mean values between the treatment group and the control group. Standard deviations are shown in parentheses.

Panel A. All-Star Analyst Quality

	(1) Average Percentile of All-Stars (%)	(2) Percentage of All-Stars with Percentile >=50 (%)
Proxy I		
Forecast Accuracy	56.20	66.57
Coverage Frequency	74.47	90.91
Coverage Timeliness	66.23	83.58
Reputation	72.06	88.56
Forecast Optimism	44.85	37.83
Abnormal Recommendation Levels	48.97	49.85
Bias	46.51	42.52
Reputation-Bias	66.85	79.77
Proxy II		
Forecast Accuracy	54.73	63.93
Coverage Frequency	71.92	88.56
Coverage Timeliness	63.24	78.59
Reputation	69.94	87.10
Forecast Optimism	43.48	38.12
Abnormal Recommendation Levels	46.28	44.87
Bias	44.59	41.64
Reputation-Bias	66.19	78.89

Panel B. Composite Proxies, Treatment vs. Control

	(1) Treatment Group	(2) Control Group	(3) Difference	(4) p-value
Proxy I				
Reputation	51.19	50.43	0.7625	0.003
	(2.995)	(3.932)		
Bias	48.84	51.65	-2.807	0.000
	(4.489)	(7.253)		
Reputation-Bias	2.389	-0.4536	2.842	0.000
	(5.586)	(10.85)		
Proxy II				
Reputation	0.5417	0.5341	0.007553	0.043
	(0.04443)	(0.05641)		
Bias	0.5229	0.5606	-0.03766	0.000
	(0.06957)	(0.0965)		
Reputation-Bias	0.01904	-0.01854	0.03757	0.000
	(0.08266)	(0.1344)		

Table 8: Analyst Quality – Regression Results

This table reports difference-in-differences results obtained by distinguishing between all-star analysts of "high" and "low" quality levels, where analyst quality is measured using *Proxy I* (Table 7). Sample period is 1998-2007, with event years 2002 and 2003 excluded from the regression samples. Panel A reports results on IPOs and follow-on offerings (SEOs) combined. Panel B reports results on IPOs alone. The dependent variable is industry market share in a given year. *Post* is an indicator that equals 1 for observations from 2004-2007, and 0 for observations from 1998-2001. *Low Accuracy* is an indicator that equals 1 if the average forecast accuracy percentile of all-star analysts in the given bank-industry is below 50, and 0 otherwise. *High Accuracy* is an indicator that equals 1 if the average forecast accuracy percentile of all-star analysts in the given bank-industry is 50 or above, and 0 otherwise. Similarly for *Low Frequency* and *High Frequency*, *Low Timeliness* and *High Timeliness*, *Low Optimism* and *High Optimism*, and *Low Recommendation* and *High Recommendation*. Wald test compares the two interaction term coefficients. Coefficients are estimated by OLS. All regression specifications include both bank-year and industry-year fixed effects. Robust standard errors are clustered at the bank level. *p*-values are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. IPOs & SEOs

	(1)	(2)	(3)	(4)	(5)
Low Accuracy	0.0204***				
	(0.002)				
High Accuracy	0.0207***				
	(0.000)				
Post*Low Accuracy	-0.0252***				
	(0.005)				
Post*High Accuracy	-0.0320***				
	(0.000)				
Low Frequency		0.0290			
		(0.153)			
High Frequency		0.0198***			
		(0.000)			
Post*Low Frequency		-0.0312			
		(0.263)			
Post*High Frequency		-0.0297***			
		(0.000)			
Low Timeliness			0.0305***		
			(0.003)		
High Timeliness			0.0188***		
			(0.000)		
Post*Low Timeliness			-0.0379*		
			(0.100)		
Post*High Timeliness			-0.0281***		
			(0.000)		
Low Optimism				0.0251***	
				(0.000)	
High Optimism				0.0143***	
				(0.005)	
Post*Low Optimism				-0.0340***	
				(0.000)	
Post*High Optimism				-0.0233***	
				(0.001)	
Low Recommendation					0.0156**
					(0.010)
High Recommendation					0.0254***
					(0.002)
Post*Low Recommendation					-0.0344***
					(0.000)
Post*High Recommendation					-0.0247***
					(0.002)
Bank-Year FE	YES	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES	YES
F Statistic, Wald Test	0.77	0	0.22	2.26	1.21
Two-Sided p-value	0.3802	0.9549	0.636	0.1333	0.2724
Observations	4966	4966	4966	4966	4966
R-squared Within	0.188	0.188	0.188	0.189	0.191

Panel B. IPOs Only

	(1)	(2)	(3)	(4)	(5)
Low Accuracy	0.00712**				
	(0.022)				
High Accuracy	0.0106***				
	(0.001)				
Post*Low Accuracy	-0.0245***				
	(0.004)				
Post*High Accuracy	-0.0463***				
	(0.000)				
Low Frequency		0.00554**			
		(0.034)			
High Frequency		0.0110***			
		(0.001)			
Post*Low Frequency		-0.0293***			
		(0.000)			
Post*High Frequency		-0.0421***			
		(0.000)			
Low Timeliness			0.0124		
			(0.157)		
High Timeliness			0.00876***		
			(0.001)		
Post*Low Timeliness			-0.0446***		
			(0.007)		
Post*High Timeliness			-0.0367***		
			(0.000)		
Low Optimism				0.0142***	
				(0.001)	
High Optimism				0.00309	
				(0.229)	
Post*Low Optimism				-0.0423***	
				(0.000)	
Post*High Optimism				-0.0325***	
				(0.000)	
Low Recommendation					0.0103**
					(0.013)
High Recommendation					0.00840***
					(0.002)
Post*Low Recommendation					-0.0431***
					(0.000)
Post*High Recommendation					-0.0331***
					(0.000)
Bank-Year FE	YES	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES	YES
F Statistic, Wald Test	5.81	3.22	0.3	2.84	2.33
Two-Sided p-value	0.0164	0.0735	0.5861	0.0928	0.1273
Observations	3792	3792	3792	3792	3792
R-squared Within	0.199	0.196	0.195	0.196	0.196

Table 9: Analyst Quality, Composite Measures – Regression Results

This table reports difference-in-differences results obtained by distinguishing between all-star analysts of "high" and "low" quality levels, where analyst quality is measured using the composite *Proxy I* (Table 7). Sample period is 1998-2007, with event years 2002 and 2003 excluded from the regression samples. Regressions (1)-(3) report results on IPOs and follow-on offerings (SEOs) combined. Regressions (4)-(6) report results on IPOs alone. The dependent variable is industry market share in a given year. *Post* is an indicator that equals 1 for observations from 2004-2007, and 0 for observations from 1998-2001. *Low Reputation* is an indicator that equals 1 if the average *Reputation* percentile of all-star analysts in the given broker-industry is below 50, and 0 otherwise. *High Reputation* is an indicator that equals 1 if the average *Reputation* percentile of all-star analysts in the given broker-industry is 50 or above, and 0 otherwise. Similarly for *Low Bias* and *High Bias*, and *Low 'Reputation-Bias'* and *High 'Reputation-Bias'*. Wald test compares the two interaction term coefficients. Coefficients are estimated by OLS. All regression specifications include both bank-year and industry-year fixed effects. Robust standard errors are clustered at the bank level. *p*-values are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	IPOs & SEOs			IPOs Only		
	(1)	(2)	(3)	(4)	(5)	(6)
Low Reputation	0.0200*			0.00482		
	(0.062)			(0.123)		
High Reputation	0.0207***			0.0105***		
	(0.000)			(0.002)		
Post*Low Reputation	-0.0204			-0.0294***		
	(0.199)			(0.000)		
Post*High Reputation	-0.0311***			-0.0404***		
	(0.000)			(0.000)		
Low Bias		0.0214***			0.0118***	
		(0.000)			(0.000)	
High Bias		0.0196***			0.00686**	
		(0.002)			(0.011)	
Post*Low Bias		-0.0364***			-0.0445***	
		(0.001)			(0.000)	
Post*High Bias		-0.0209***			-0.0303***	
		(0.004)			(0.000)	
Low 'Reputation-Bias'			0.0223**			0.00883*
			(0.049)			(0.062)
High 'Reputation-Bias'			0.0201***			0.00949***
			(0.000)			(0.001)
Post*Low 'Reputation-Bias'			-0.0226			-0.0272***
			(0.136)			(0.002)
Post*High 'Reputation-Bias'			-0.0318***			-0.0425***
			(0.000)			(0.000)
Bank-Year FE	YES	YES	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES	YES	YES
F Statistic, Wald Test	0.7	2.03	0.32	2.9	3.83	4.67
Two-Sided p-value	0.4029	0.1545	0.5698	0.0894	0.0509	0.0313
Observations	4966	4966	4966	3792	3792	3792
R-squared Within	0.188	0.189	0.189	0.195	0.196	0.197

Table 10: Effect of Global Settlement on Cost of Equity Capital

Sample period is 1998-2007. The dependent variable is the weighted average underpricing of equity issuances underwritten by a bank in an industry in a year; the higher the number of lead or co-lead managers in an issuance, the lower is the weight given to the underpricing of that issuance. Underpricing of an issuance, or first-day return, is defined as the percentage change from the offer price to the first-day closing price. *Treatment* is an indicator that equals 1 for observations in the treatment group, and 0 otherwise. *Post* is an indicator that equals 1 for observations from 2004-2007, and 0 for observations from 1998-2001. *GS(-1)* is an indicator for one year prior to the event years, *GS(0)* is an indicator for the event years, *GS(+1)* is an indicator for one year after the event years, and *GS(2+)* is an indicator for two or more years after the event years. Panel A reports regression results using the first valuation risk measure, namely, whether an issuance belongs in a tech industry. *Tech* is an indicator that equals 1 if the corresponding industry is a tech industry, and 0 otherwise. Column (1) reports triple-difference regression results for IPOs and follow-on offerings (SEOs) combined. Column (2)-(5) report regression results for IPOs alone. Column (2) reports triple-difference regression results. Column (3) and (4) report difference-in-differences results for non-tech and tech issuances, respectively. Column (5) reports triple-difference coefficient dynamics. Event years 2002 and 2003 are excluded in the regression samples in column (1)-(4), and included in the regression sample in column (5). Panel B reports regression results using the second valuation risk measure, namely, issuer age. *Avg Issuer Age* is a continuous variable equal to the average issuer age of equity issuances underwritten by a bank in an industry in a year, where the age of an issuer is defined as the difference between the issuer's founding year and the year of the public offering. Column (1) reports triple-difference regression results for IPOs and follow-on offerings (SEOs) combined. Column (2) reports triple-difference regression results for IPOs alone. Column (3) reports triple-difference coefficient dynamics for IPOs alone. Event years 2002 and 2003 are excluded in the regression samples in column (1)-(2), and included in the regression sample in column (3). Coefficients are estimated by OLS. All regression specifications include both bank-year and industry-year fixed effects. Robust standard errors are clustered at the bank level. *p*-values are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Tech vs. Non-Tech

	IPOs & SEOs	IPOs Only			
	(1)	(2)	(3)	(4)	(5)
	Full Sample	Full Sample	Non-Tech	Tech	Coefficient Dynamics
Treatment	0.000672	0.0730	0.135*	-0.0681	0.0625
	(0.964)	(0.124)	(0.061)	(0.621)	(0.288)
Treatment*Post	-0.0112	-0.117***	-0.171**	0.0532	
	(0.567)	(0.004)	(0.016)	(0.715)	
Treatment*Tech	0.0105	-0.131***			-0.134**
	(0.739)	(0.008)			(0.014)
Treatment*Tech*Post	0.00168	0.178***			
	(0.961)	(0.003)			
Treatment*GS(-1)					0.0257
					(0.819)
Treatment*GS(0)					-0.121*
					(0.090)
Treatment*GS(+1)					-0.0488
					(0.533)
Treatment*GS(2+)					-0.125**
					(0.038)
Treatment*Tech*GS(-1)					0.0255
					(0.769)
Treatment*Tech*GS(0)					0.236***
					(0.000)
Treatment*Tech*GS(+1)					0.148*
					(0.062)
Treatment*Tech*GS(2+)					0.193***
					(0.006)
Bank-Year FE	YES	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES	YES
Observations	2161	1105	640	465	1250
R-squared Within	0.373	0.546	0.604	0.589	0.551

Panel B. Issuer Age

	IPOs & SEOs	IPOs Only	
	(1)	(2)	(3)
Treatment	0.00490	-0.0209	-0.0273
	(0.815)	(0.808)	(0.769)
Avg Issuer Age	-0.000289	-0.00142	-0.00193
	(0.588)	(0.515)	(0.452)
Treatment*Post	-0.00594	0.0328	
	(0.816)	(0.718)	
Avg Issuer Age*Post	0.0000290	0.00246	
	(0.941)	(0.241)	
Treatment*Avg Issuer Age	-0.000179	0.00160	0.00218
	(0.800)	(0.358)	(0.278)
Treatment*Avg Issuer Age*Post	-0.00000385	-0.00336*	
	(0.995)	(0.077)	
Treatment*GS(-1)			-0.0528
			(0.700)
Treatment*GS(0)			0.150*
			(0.064)
Treatment*GS(+1)			0.0759
			(0.544)
Treatment*GS(2+)			0.0259
			(0.787)
Avg Issuer Age*GS(-1)			0.00153
			(0.682)
Avg Issuer Age*GS(0)			0.00240
			(0.308)
Avg Issuer Age*GS(+1)			0.00149
			(0.704)
Avg Issuer Age*GS(2+)			0.00317
			(0.203)
Treatment*Avg Issuer Age*GS(-1)			-0.00208
			(0.516)
Treatment*Avg Issuer Age*GS(0)			-0.00396**
			(0.016)
Treatment*Avg Issuer Age*GS(+1)			-0.00446
			(0.202)
Treatment*Avg Issuer Age*GS(2+)			-0.00371*
			(0.085)
Bank-Year FE	YES	YES	YES
Industry-Year FE	YES	YES	YES
Observations	1772	1050	1179
R-squared Within	0.410	0.550	0.555

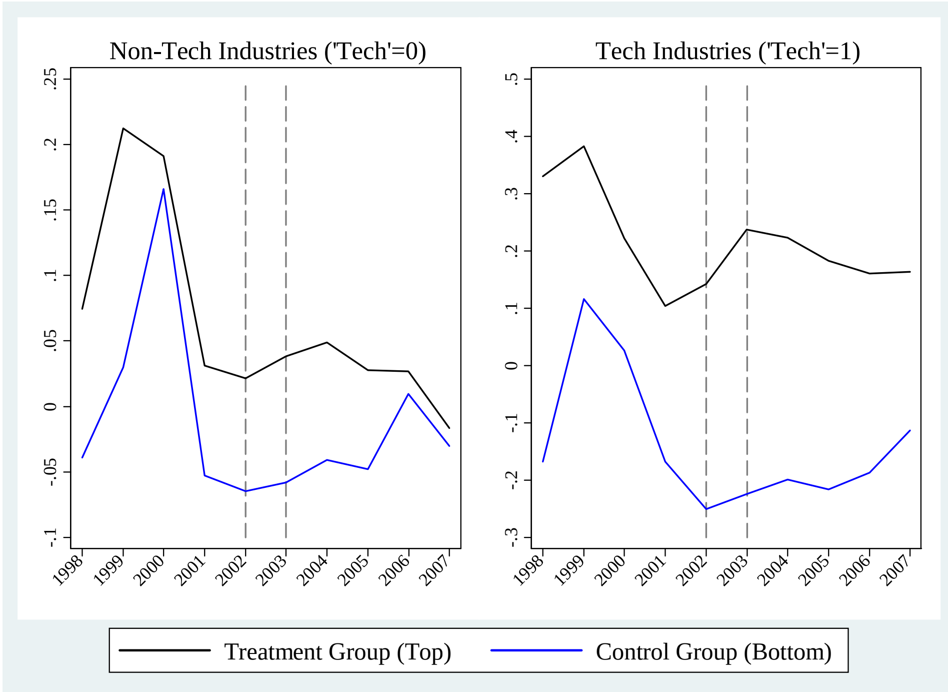

Figure 3: Underpricing of IPOs, Non-Tech vs. Tech, Treatment Group vs. Control Group

This figure plots, separately for non-tech industries and for tech industries, the estimated coefficients of the year dummies (plus the intercept) in the following regression, separately for the treatment group and for the control group:

$$Underpricing_{ijt} = Intercept + \sum_{t=1999-2007} \beta * Year\_Dummy_t + \zeta_i + \lambda_j + \mu_{ijt}$$

The dependent variable is weighted average underpricing of IPOs of bank i in year t in industry j. The regression specification includes both bank fixed effects (ζ_i) and industry fixed effects (λ_j), where industry is defined using 6-digit GICS codes. Sample period is 1998-2007. Coefficients are estimated by OLS. Robust standard errors are clustered at the bank level.

CHAPTER 5: CONCLUSIONS

The Global Settlement banned research analyst participation in investment banking business and prohibited investment banks from linking research analysts' compensation to analysts' investment banking contributions. Perhaps due to a steep decline in compensation and career prospects, an exceptionally large number of all-star analysts – as named by *Institutional Investor* magazine in the pre-Settlement period – departed from the sell-side research industry in the post-Settlement period. Since equity issuers place a high value on research coverage from all-star analysts – who on average exhibit superior performance compared to non-all-star analysts – the departure of all-star analysts caused their bank-industry underwriting groups to lose equity issuance market share, relative to bank-industry groups that did not have all-star analysts to begin with. Market share losses were more severe for IPO underwritings than for IPO and follow-on underwritings combined, likely due to the stickiness of underwriting relationships. The higher the average quality of all-star analysts in a bank-industry in the pre-Settlement period, the more severe were the bank-industry's post-Settlement losses. The departure of all-star analysts also raised the cost of equity capital for IPOs underwritten by their bank-industry underwriting groups, particularly for IPOs that were intrinsically difficult to value, such as those of technology companies, due to IPO investors seeking compensation for increased valuation risk arising from the loss of high-quality research.

High-quality research coverage enhances the efficient operation of the capital markets and of the real economy. The disappearance of high-quality sell-side equity research coverage, as an unintended consequence of well-intentioned regulation, however, has forced issuing companies to accept research coverage of inferior quality. Do the Global Settlement and related SRO regulations deter companies from going public or from obtaining public capital?

Has the deterrence effect been stronger on companies with higher valuation risk, including the technology companies that represent the vanguard of the American economy? How have these regulations affected other aspects of investment banking business, including debt issuances and mergers and acquisitions? More broadly, how have these regulations, and particularly their resultant decline in research analyst quality, altered companies' capital markets behavior? How do these changes in capital markets decision patterns affect the real economy? Future research answering these follow-up questions may provide further insights into the effects of these regulations and may have important policy implications.

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