Conflict-of-Interest Reforms and Analysts' Research Biases[*]

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February 3, 2009

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Abstract

This paper examines the consequences of a series of reforms that aim at resolving analyst conflicts of interest driven by investment banking businesses. In the pre-reform period, earnings forecasts and stock recommendations issued by analysts at research firms are more optimistic than those issued by investment bank analysts. However, investment-bank analysts' forecasts exhibit a higher frequency of optimism-to-pessimism intertemporal biases. After the reforms, forecast optimism decreases for all firm types. The reduction is unexpectedly greater for research firms than investment banks. There is also a significant reduction in the frequency of optimism-to-pessimism forecasts and in the optimism of stock recommendations made by investment bank analysts. These findings are consistent with the reforms achieving the goal of making investment bank analysts' research less optimistic. However, the accuracy of investment bank forecasts drops and the profitability of stock recommendations remains unchanged, suggesting that the reforms adversely affect the quality of earnings forecasts.

1. Introduction

This paper examines the consequences of a series of reforms targeting investment-banking-related conflicts of interest. Biased analyst research is believed to have contributed to large investor losses during the stock market downturn in 2000 and 2001. Investors, business press, and regulators have suspected investment bank analysts of biasing their research in return for investment banking business from the companies they followed because their bonuses were tied to investment banking revenue (Boni and Womack 2002). In response to these allegations, the financial industry endorsed a set of "best practices" in 2000 to restore public confidence in the credibility of equity research. Several other regulations followed and the reforms culminated in April 2003 when the Securities and Exchange Commission imposed enforcement actions against ten of the largest U.S. investment banks (the so-called "Global Research Analyst Settlement"). These reforms resulted in sweeping changes in the investment research industry, especially regarding the way investment banks compensate their research analysts and structure the operations of their research and investment banking departments.

There are concerns regarding the efficacy of these reforms. First, some argue that if the investment bank in question can provide better earnings forecasts and stock recommendations as a consequence of the bias, its investment research consumers will benefit (e.g., see Mehran and Stulz 2007; Agrawal and Chen 2008).[1] In equilibrium, either reputation concerns will lead an investment firm to manage the adverse effects of the conflicts or its customers will discount its forecasts and recommendations accordingly. Second, the incentive related to investment-banking businesses is only one

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[1] However, De Franco, Lu, and Vasvari (2007) document evidence suggests that analysts' research biases adversely affecting small investors.

type among several incentives that could potentially cause investment research biases. Some argue that the incentive to generate trading commissions, which is not addressed by the reforms, will continue to drive analyst research biases (Irvine 2004; Jackson 2005; Cowen et al. 2006). Third, these reforms reduce the amount of research funding, which could lead to an unintended "brain drain," whereby veteran analysts leave sell-side research to pursue other lucrative opportunities (Institutional Investor 2007). Finally, the new rules have added costs, e.g., in the form of increased administrative burdens. In summary, it remains an empirical question whether these reforms have achieved the goal of improving the objectivity of investment research.

If the reforms resolve analyst conflicts of interest, we expect to find a reduction in research biases in the post-reform period and to observe greater reductions when the investment-banking incentives faced by analysts are higher in the pre-reform period. We examine three types of research biases: optimistic earnings forecast bias, optimistic-to-pessimistic intertemporal forecast bias (i.e., initial optimistic and final pessimistic earnings forecasts), and optimistic stock recommendation bias. Following Cowen et al. (2006), we capture analyst incentives using the type of firms employing the analyst: those with no investment-banking activities (research firms) and those with various levels of investment-banking activities (brokerage firms, syndicate banks, and investment banks). We further separate investment banks into non-sanctioned and sanctioned banks to examine the incremental effect of the Global Research Analyst Settlement.

Regardless of whether the reforms achieve their intended goal or not, there are reasons to believe that the reforms adversely affect research quality. First, prior research suggests that analysts use biases to curry favor with management not only to attract

investment banking businesses, but also to get access to managerial private information. Ke and Yu (2006) find that analysts who use optimism-to-pessimism intertemporal forecast biases are more accurate. Hence, if the reforms are effective, analysts lose their ability to maintain access to management and forecast accuracy will be adversely affected. Second, participation of equity analysts in investment banking deals helps analysts become more familiar with the companies and their industries (Institutional Investor 2007). Hence, separating research from investment banking activities will reduce the quality of investment research. Third, funding provided by investment banking businesses is important for supporting high quality research. Without this funding source, research departments might have to reduce their coverage or quality of their research (e.g., Boni 2006 and O'Leary 2007). Moreover, analysts might be pressured to hype stocks, as research funding is shifted from underwriting to trading. In sum, the conflict-of-interest reforms may have an unintended adverse consequence on the quality of equity research. We examine this implication in this study.

We use pre-post and difference-in-differences research designs to investigate the impact of the reforms on analysts' research biases. We document the following empirical results using I/B/E/S analysts' forecasts and stock recommendations for the period from 1998 through 2007. First, we find that earnings forecasts made by research firm analysts are more optimistic than those made by analysts from investment banks in the pre-reform period. After the reforms, forecast optimism reduces for all analyst types, but the reduction is bigger for research firm analysts than investment bank analysts. However, research firm analysts are still significantly more optimistic than their investment bank counterparts in the post-reform period.

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Second, the forecasts of investment bank analysts exhibit a higher frequency of optimism-to-pessimism (OP) biases than those of research firm analysts in the pre-reform period. After the reforms, the difference in the frequency of OP biases across firm types disappears. This reduction is attributed to investment bank analysts making less OP forecasts in the post-reform period.

Third, stock recommendations from investment bank analysts are less optimistic than those from other analysts in the pre-reform period. The reforms significantly reduce the recommendation optimism of the sanctioned investment banks, but have no impact on the recommendation optimism of other firm types. Hence, the reforms achieve the goal of making investment bank analysts' recommendations less optimistic, but have an insignificant effect on other firms' analysts who are actually more optimistic.

Finally, the forecast accuracy of research firm analysts is significantly worse than that of analysts from other firm types before the reforms and the profitability of stock recommendations issued by research firm analysts is not statistically different from that of analysts from other firm types. After the reforms, the accuracy of research firm analysts significantly improves, while that of investment bank analysts drops slightly. Furthermore, the profitability of all analysts' stock recommendations remains unchanged and insignificantly different from each other, despite the fact that investment bank recommendations become more pessimistic after the reforms.

We contribute to the literature in several ways. First, we address whether investment banking incentives are the most important factor affecting analyst research biases. Our evidence supports the contention that investment banking-related conflicts of interest are not the only reason behind analyst research biases (Mehran and Stultz 2007;

Cowen et al. 2006). We find that analysts in research firms actually make more optimistic forecasts than investment bank analysts in the pre-reform period. We believe that regulators target investment banks not because they are the most biased, but because they are influential in the capital markets (Watts and Zimmerman 1986; De Franco, Lu, and Vasvari 2007). Nonetheless, the reforms do achieve the goal of making investment bank analysts' research less optimistic. This eases the concern of Cowen et al. (2006) that the reforms might unexpectedly induce additional biases to investment bank research as their incentive has shifted from gaining investment banking businesses to generating trading commissions.

Second, our findings call into question whether a requirement of the Global Settlement that sanctioned banks contract with research firms to furnish research to the banks' retail clients benefit investors. This is because research firm analysts are more optimistic in their earnings forecasts and recommendations, their forecasts are less accurate, and the profitability of their recommendations is not different from that of investment bank analysts.

Third, Jacob et al. (2003), Clarke et al. (2004), Cowen et al. (2006), and Barber et al. (2007) document differences in analysts' forecast biases and stock recommendation returns between investment banks and non-investment banks in the pre-reform period.[2] On the other hand, Barber et al. (2006) and Kadan et al. (2008) investigate the effect of a

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[2] Another line of research examines the research biases of affiliated and unaffiliated analysts for IPO or SEO firms. Dugar and Nathan (1995), Lin and McNichols (1998), Michaely and Womack (1999), and Dechow, Hutton, and Sloan (2000) find that affiliated analysts make more optimistic earnings growth forecasts and more favourable recommendations than unaffiliated analysts. Their results on the optimism of one-year-ahead forecasts are mixed. Moreover, Michaely and Womack (1999) find that the stock recommendations of affiliated analysts underperform those of unaffiliated analysts for a sample of 1990–1991 IPO firms. However, Dugar and Nathan (1995), Lin and McNichols (1998), and McNichols, O'Brien, and Pamukcu (2007) find no statistical difference in the profitability of buy recommendations issued by affiliated and unaffiliated analysts.

specific analyst-related regulation on the distribution and profitability of stock recommendations in a short post-regulation period. Mehran and Stulz (2007) point out that it needs a long post-regulation period to better assess the permanent effect of the reforms. In this study, we test whether the biases in forecasts and recommendation have changed using a post-reform period covering the four years from 2004 through 2007. Moreover, we exclude 2002 and 2003 from our analysis because the changing nature of the regulatory environment around these two years likely adds noise in detecting the long-term effects of these reforms.

Last, both Barber et al. (2006) and Kadan et al. (2008) find that the distribution of stock recommendations becomes more balanced after the implementation of NASD Rule 2711. We extend this result by showing that the distribution of stock recommendations has become less optimistic and more pessimistic using two alternative measures that take into account the discrete nature of stock recommendations. Moreover, Kadan et al. (2008) examine the relative change in the 3-day market reaction around stock recommendations. We complement their study by examining the relative change in the profitability of recommendations over a one-year period to allow for a slow market response to the recommendation changes (e.g., see Stickel 1995, Barber et al. 2001).

The rest of the paper is organized as follows. Section 2 describes the series of reforms on analyst conflicts of interest. Section 3 explains our research design and reports summary statistics. Sections 4 and 5 present the empirical results on earnings forecasts and stock recommendations, respectively. Section 6 concludes.

2. Reforms on analyst conflicts of interest

Equity research analysts play an important role as information intermediaries. They help investors make investment decisions and improve the informational efficiency of the stock markets. However, concerns exist about the objectivity of analyst research. In particular, analysts are accused of hyping stocks to secure management access, to generate brokerage commissions, or to attract investment-banking business. Starting from 2000, the financial industry, self-regulatory organizations (SROs), and regulators introduced proposals and rules to restore public confidence in the independence of research analysts and objectivity of analyst research.

Recognizing the conflicts of interests in equity research, the Securities Industry Association endorsed a compilation of "best practices" in June 2000. These practices recommend the following guidelines: Research departments should not report to investment banking units; analysts' compensation should not be tied to investment banking business; firms should disclose analysts' financial interests; and analysts should not trade contrary to their recommendations.

Also in 2000, the Association for Investment Management and Research (now CFA Institute) formed a task force on analyst independence and released a white paper entitled "Preserving the Integrity of Research." It addresses the potential conflicts of interest for sell-side analysts that "may bias their research reports and recommendations." Subsequently, CFA Institute established its "Research Objective Standards (ROS)," which provide ethical standards and specific recommended practices to guide investment firms worldwide and their respective employees in achieving objectivity of research reports. These ROS standards are broad, covering issues on public appearances,

investment banking, analyst compensation, relationships with subject companies, personal investments and trading, disclosure, and rating systems.

In February 2002, the New York Stock Exchange (NYSE) and National Association of Securities Dealers (NASD)[3] filed the first round of proposed SRO rules – amendments to NYSE Rule 351 (reporting requirements) and Rule 472 (communications with the public) and New NASD Rule 2711 (Research Analysts and Research Reports). The U.S. Securities and Exchange Commission (SEC) approved these new rules on May 20, 2002. These rules require comprehensive disclosure of conflicts of interest in research reports and public appearances by research analysts. The rules prohibit the involvement of investment banking personnel in exterminating research report content and analyst compensation. These rules also establish stringent disclosure requirements for research reports and prescribe that research reports must explain the meaning of its rating system in stock recommendations and disclose data that help investors track the correlation between the rating and stock price movements.

The U.S. Congress passed the Sarbanes-Oxley Act in July 2002. Section 501 of the Act addresses conflicts of interest that can arise when security analysts recommend equity securities in research reports and public appearances. In December 2002, the SEC proposed enforcement actions against ten of the top U.S. investment banks. The so-called "Global Research Analyst Settlement," aims to resolve "undue influence of investment banking interests on securities research at brokerage firms. The settlement, which was finalized on April 28, 2003, is expected to bring about balanced reform in the

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[3] The two bodies have now consolidated most of these operations into the Financial Industrial Regulatory Authority (Davis, 2007).

industry and bolster confidence in the integrity of equity research (SEC December 20, 2002 press release)."

Both Section 501 of the Sarbanes-Oxley Act and the Global Settlement require structural reforms that fundamentally changed practices in the investment industry. First, firms must physically separate the investment banking and research departments and restrict interaction between them. Senior management of the firm will set the budget of the research department without input from investment bankers and without tying the budget to revenues from investment banking. Research analyst involvement in investment banking activities or receiving compensation derived from investment banking revenues will be prohibited. Investment bankers will not take part in evaluating analysts' job performance or determining their compensation. Research management will make all decisions to initiate or terminate the coverage of companies. Second, sanctioned banks must contract with at least three independent research firms that will furnish independent research to the banks' research clients for a five-year period. Last but not the least, these banks must publicly disclose their research analysts' historical ratings and price-target forecasts to assist investors in evaluating the performance of analysts.

3. Research design

3.1. Sample and data

Our sample comes from I/B/E/S and covers the period from January 1998 to December 2007. We divide the sample into three subperiods: The pre-reform period (January 1998 – December 2001), transition period (January 2002 – December 2003), and post-reform period (January 2004 – December 2007). We examine the change in

research biases between the pre- and post-reform periods. We exclude the transition period from the analysis because it is the period when the various reforms were proposed, deliberated, and implemented. Since the regulatory environment is changing during the transition period, including this period in the analysis may induce noise in estimating the long-term effect of the reforms on analysts' research biases.

Following Cowen et al. (2006), we classify investment firms into four types based on information from Nelson's Directory of Investment Research (2000–2007) and Thomson Financial's SDC database. Investment banks are those listed as investment banks by Nelsons and identified as lead or co-lead underwriters by SDC. Syndicate banks are those firms listed by Nelsons as either investment banks or brokers and identified by SDC as managers or co-managers, but not lead or co-lead underwriters. Research firms are those listed as such by Nelsons and not found in the SDC database. The rest of the firms are classified as brokerage firms if they are not identified as lead/co-lead underwriter or manager/co-manager by SDC. We further divide the investment banks into sanctioned and non-sanctioned banks. The sanctioned banks are Bear Stearns, Credit Suisse First Boston, Goldman Sachs, Lehman Brothers, J.P. Morgan Securities, Merrill Lynch, Morgan Stanley, Citigroup Global Markets (formerly known as Salomon Smith Barney), UBS Warburg, and U.S. Bancorp Piper Jaffray.

We retrieve all quarterly and annual analyst earnings forecasts and stock recommendations from Thomson Financial's I/B/E/S database. We use the I/B/E/S translation file to identify the affiliation of each equity analyst, which allows us to have a

sample spanning the period from January 1998 through December 2007.[4] If an analyst

changes job from one firm type to another, we assign her to the firm type of her original

employer in the switching year. The results are robust if we exclude these analysts from

our sample. Stock price and return data are from *CRSP* and financial statement data are

from *Compustat*.

3.2. Empirical methods

We use the pre-post and difference-in-differences methods to investigate the impact

of the reforms on the biases of analysts' earnings forecasts and stock recommendations.

In the difference-in-differences analysis, we compare the change in the research bias of

investment bank analysts to that of research firm analysts before and after the reforms.

By examining the relative change in the research bias between two groups of analysts, we

explicitly control for time-specific variations that are common across the two groups (i.e.,

confounding effects) but not attributed to the reforms per sec.[5] Furthermore, regulators

generally view research firm analysts as being more objective than investment bank

analysts, because the former are not subject to investment-banking relative incentives.

Hence, by comparing the properties of their research biases, we also shed light on

whether research biases are due to investment banking-related conflicts of interest.

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[4] I/B/E/S stops providing the translation file for academic research after 2006. We use the 2006 translation file to identify analyst affiliation in 2007. Hence, we will lose new investment research firms (and their analysts) that are added to I/B/E/S in 2007.
[5] The difference-in-differences method is typically used to compare the change in the variable of interest of a subject group with that of a benchmark group before and after an event. The benchmark group is chosen based on the criteria that they share similar economic characteristic as the subject group, but they are not affected by the event. In our case, we cannot assert that research firms are not affected by the reforms, because the Global Settlement also provides funding for independent research. Hence, we use this method mainly to control for confounding events.

Besides reporting simple pre-post and difference-in-differences estimates, we also run difference-in-differences (DD) regressions. The DD regressions allow us to control for other sources of variations in research biases across firm type and sample period. The DD regression model is specified as follows:

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\begin{aligned}
DEP_{it} = {} & \alpha_0 + \alpha_1 D + \alpha_2 BROKERAGE + \alpha_3 SYNDICATE + \alpha_4 NONSANC + \alpha_5 SANCTIONED \\
& + \alpha_6 D \times BROKERAGE + \alpha_7 D \times SYNDICATE + \alpha_8 D \times NONSANC + \alpha_9 D \times SANCTIONED \\
& + CONTROLS + e_t,
\end{aligned} \tag{1}
$$

where *DEP* is a measure of research biases (to be defined below). *D* is an indicator variable that equals one in the post-reform period, and zero otherwise. *BROKERAGE*, *SYNDICATE*, *NONSANC*, and *SANCTIONED* are indicator variables that equal one, respectively, for analysts from brokerage firms, syndicate firms, non-sanctioned investment banks, and sanctioned investment banks, and zero otherwise. Following Hong and Kubik (2003) and Cowen et al. (2006), we explicitly control for relative forecast accuracy and forecast horizon in the regressions. We also control for analyst characteristics, brokerage firm characteristics, and characteristics of the portfolio of companies followed by the analysts. The construction of these variables are described in the Appendix.

We estimate the DD regression model using a panel of analysts. The regression includes year dummy variables to control for unobserved time effect. To obtain proper standard errors, we cluster by analyst to absorb unobserved anlayst effects. Petersen (2009) shows that if the time effect is fixed, standard errors clusteded by analyst are unbiased. Given the short time series, we are not able to cluster on both year and analyst or to model the time dependence.

The estimated coefficients α_2 to α_5 represent the pre-reform research biases of brokerage, syndicate, non-sanctioned investment, and sanctioned investment firm analysts, respectively, relative to that of research firm analysts. The estimated coefficients α_6 to α_9 are the difference-in-differences estimates, indicating the changes in the biases of analysts from brokerage firms, syndicate firms, non-sanctioned and sanctioned investment banks, respectively, relative to the change in the bias of research firm analysts.

3.3. Descriptive statistics

Table 1 reports the numbers of firms and analysts by broker type. One-, two-, and three-quarter-ahead quarterly earnings stand for quarterly earnings forecasts issued 1 to 90, 91 to 180, and 181 to 270 days before the announcements of actual earnings in a specific quarter, respectively. The availability of firms and analysts is different across research outputs. In particular, the number of firms and analysts available is largest for annual earnings forecasts. Consistent with prior studies, the analysts included in our sample are mainly from investment banks, which represent about 80% of all the analysts in our sample.

Table 2 reports statistics on analyst, firm, and company characteristics by firm type and sample period (these variables are defined in the Appendix). Prior analyst studies have shown that these characteristics are associated with analyst forecast performance (e.g., Jacob, Lys, and Neale 1999; Mikhail, Walther, and Willis 1999; Hong and Kubik 2003). Analyst characteristics are captured by analyst experience, number of companies followed, analyst industry specialization, analyst turnover, and percent of new

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followings. Firm characteristics are captured by brokerage firm size rank and brokerage

firm specialization. Finally, company characteristics are captured by the average

company size, leverage, gross margin, sales growth, and book-to-market ratio of the

portfolio of companies being followed by a particular analyst. The statistics reported in

Table 2 show large variations in these characteristics not only across firm type, but also

across the pre- and post-reform periods. Hence, it is important to control for these

characteristics in the difference-in-differences regressions.

4. Empirical findings on earnings forecasts

4.1. Forecast optimism

We first examine earnings forecast optimism. We calculate forecast optimism as

follows:

$$FOPT_{ijt}^{t-k} = \frac{FORECAST_{ijt}^{t-k} - ACTUAL_{jt}}{STDDEV(FORECAST_{jt}^{t-k})}, \tag{2}$$

where $FORECAST_{ijt}^{t-k}$ is analysts i's forecast of company j's performance for period t, as

of t-k, where k is forecast horizon. $ACTUAL_{jt}$ is firm j's actual earnings in period t.

$STDDEV(FORECAST_{jt}^{t-k})$ is the standard deviation of all forecasts for company j and

period t, as of t-k.[6] We compute $FOPT_{ijt}^{t-k}$ for companies that are followed by at least

three analysts and we use the first forecast made by each analyst for the same company

and forecast period. $FOPT_{ijt}^{t-k}$ is then averaged across all companies followed by analyst i

in period t to compute analyst i's average forecast optimism, $FOPT_{it}^{t-k}$.

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[6] We winsorize forecast optimism at the 1st and 99th percentiles, because some standard deviations are
extremely small. Results are qualitatively similar if we scale this measure by stock price.

Table 3 presents summary statistics on the optimism of quarterly and annual earnings forecasts by firm type and sample period. Panel A indicates that analysts from all firm types are, on average, pessimistic (i.e., forecasts are lower than realized earnings) in both the pre- and post-reform periods. In contrast, panels B to D show that forecasts of two- and three-quarter ahead quarterly earnings and annual earnings are optimistic for all five firm types and in both periods. These findings are consistent with those documented in prior studies that forecasts made near earnings announcements are pessimistic or less optimistic than those made early in the fiscal year (e.g., O'Brien 1988; Richardson, Teoh and Wysocki 2004).

The results suggest that research firm forecasts are more optimistic than investment bank forecasts before the reforms. In particular, research firm analysts are either less pessimistic (panel A) or more optimistic (panels B, C, and D) than investment bank analysts. For example, the average forecast optimism of annual earnings is 2.639 for research firm analysts and 0.944 for sanctioned bank analysts. In fact, column (9) shows that sanctioned bank analysts are significantly more pessimistic (panel A) or less optimistic (panels B and C) than research firm analysts. After the reforms, analysts from research firms become significantly more pessimistic (panel A) or less optimistic (panels B and C). However, research firm forecasts are still more optimistic than sanctioned bank forecasts. Finally, the reforms also significantly reduce the optimism of analysts from other firm types (except for one-quarter-ahead forecasts under columns 2 to 4).

Table 4 summarizes the difference-in-differences regression results of forecast optimism. First, the significantly negative coefficients on *SYNDICATE*, *NONSANC*, and *SANCTIONED* in columns (1) and (2) suggest that investment bank forecasts of one- and

two-quarter-ahead quarterly earnings are less optimistic than the corresponding research firm forecasts in the pre-reform period. There are no significant differences between research firm analysts and other analysts in the optimism of three-quarter-ahead quarterly earnings forecasts and annual earnings forecasts, as shown in columns (3) and (4). Second, the significant coefficients on $D*NONSANC$ and $D*SANCTIONED$ in columns (1) to (3) suggest that after the reforms, the drop in forecast optimism is statistically bigger for research firm analysts than for investment bank analysts. In other words, the reforms have a relatively larger effect on research analysts than investment bank analysts in reducing their forecast optimism. This finding is unexpected, given that the reforms are aimed at resolving investment banking-related conflicts of interest.

In summary, sanctioned bank analysts not only are less optimistic than research firms analysts, but they are also the least optimistic among all analyst types both before and after the reform. On the contrary, research firm analysts are more optimistic in their earnings forecasts than the analysts from other types of firms in the pre-reform period (1998-2001). The findings are consistent with those in Cowen et al. (2006) that the analysts of syndicate and investment banks are less optimistic than those of brokerage and research firms in their sample period 1996-2002. Second, the conflicts-of-interest reforms affect all firm types, but research firm analysts are affected more than other analysts. In the post-reform period, the relative differences between investment bank and research firm forecasts become smaller. Taken together, these findings are consistent with the argument that incentives other than investment banking business also contribute

to analyst forecast biases and the reforms unexpectedly reduce the forecast optimism of research firms, which are not the main target of the reforms.[7]

4.2. Intertemporal pattern of forecast biases

Optimistic forecasts do not necessarily please company management because they are more difficult for managers to meet or beat. To curry favor with company management, analysts might make optimistic forecasts early in the period and then revise them downward later in the period such that they become meetable or beatable by managers. Ke and Yu (2006) examine four intertemporal forecast patterns and show that managers prefer optimism-to-pessimism forecasts (i.e., initial optimistic forecasts and final pessimistic forecasts). They show that analysts who make optimism-to-pessimism (hereafter, "OP") forecasts are more accurate and are less likely to be fired by their firms.

We examine whether the reforms aimed at resolving investment banking conflicts of interest affect the instance of analysts using OP forecasts to win favor from company management. Following Ke and Yu (2006), we create an indicator variable OP_{it}, which is the average of OP_{ijt} for all companies covered by analyst i in year t. OP_{ijt} is equal to 1 if analyst i's 9-month-ahead forecast of company j's annual earnings in period t is greater than actual earnings and the 1-month-ahead forecast is less than actual earnings; otherwise, OP_{ijt} is set to zero. To be included in the computation of OP_{ijt}, we require companies to be followed by at least three analysts. The quarterly-based OP forecast bias variable is constructed following a similar procedure.

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[7] As another way to control for the differences in the portfolios of companies followed by analysts from different firm types and time-specific effects, we repeat the analysis using the relative optimism measure (Clement 1999; Cowen et al 2006). It is calculated by placing actual earnings with the average of all forecasts for company *j* and period *t* in equation (2). The results (available from the authors) are qualitatively similar to those reported in tables 3 and 4.

Table 5, panels A and B report the results from the univariate analysis of the frequency of quarterly and annual OP forecasts. In the pre-reform period (1998 – 2001), the percentage of quarterly OP forecasts ranges from 26.6% to 32.3% and that of annual OP forecasts ranges from 21.6% to 27.3%. These averages are similar to the 29.1% and 26.0% for quarterly and annual OP forecast biases, respectively, documented in Ke and Yu (2006) over the same sample period. Furthermore, analysts from research firms have the smallest percentage of relative OP forecasts and columns (6) to (9) indicate that research firm analysts use significantly less OP forecasts than analysts from other firm types. However, the difference disappears in the post-reform period because analysts from all firm types, other than research firms, reduce the use of quarterly and annual OP forecasts by about 10% and 5%, respectively. The last rows in the panels under columns (2) to (5) show that the drops are statistically significant for brokerage, syndicate, non-sanctioned, and sanctioned firms.

The simple difference-in-differences estimates are given in columns (6) to (9), the last row of panels C and D. The relative changes are statistically negative for brokerage, syndicate, non-sanctioned, and sanctioned firms. Sanctioned bank analysts have the largest relative drop (-10.8% and -7.1% for quarterly and annual forecast biases, respectively). These results are consistent with the reforms mitigating the incentives of investment bank analysts to use OP forecasts to please company management as a way to get investment banking business.

Table 6, columns (1) and (2) present the difference-in-differences regression estimates of quarterly and annual OP biases, respectively. The estimated coefficients on *SANCTIONED* are statistically positive. This means that in the pre-reform period,

sanctioned bank analysts use quarterly and annual OP forecasts more frequently than research firm analysts. In the post-reform period, there is a significant drop in the use of quarterly and annual OP forecast biases by sanctioned bank analysts, relative to the reduction experienced by research firm analysts. Hence, the evidence further supports that the reforms mitigate the incentives of sanctioned bank analysts to use OP forecasts to curry favor with the management of the companies they followed. The other difference-in-differences estimates are also negative, but indistinguishable from zero.[8]

4.3. Accuracy of earnings forecasts

Next, we examine the consequence of the reforms on the quality of analysts' forecasts. Following Hong and Kubik (2003) and Cowen et al. (2006), we calculate forecast accuracy for each analyst i's average forecast accuracy, $Accuracy_{it}$, which is computed by averaging $Accuracy_{ijt}$ across all companies followed by analyst i in period t. In particular,

$$Accuracy_{ijt} = 100 - 100 \times \left\{ \frac{Rank_{ijt} - 1}{NumberFollowing_{jt} - 1} \right\}, \qquad (3)$$

where $Rank_{ijt}$ is analyst i's forecast accuracy rank for company j in period t, and $NumberFollowing_{jt}$ is the number of analysts following company j in period t. We use the last forecast made by each analyst for the same company and forecast period.

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[8] We note that Regulation FD, which became effective as of October 23, 2000, is applied to all analysts, but the investment banking conflicts-of-interest reforms mainly target the analysts from syndicate banks and investment banks. The difference-in-differences method allows us to filter out the effect of Reg FD to isolate the impact of the reforms. Hence, the findings reported in Tables 5 and 6 on the frequency of intertemporal OP biases are more consistent with the conflicts-of-interest reforms, rather than Reg FD, reducing analysts' use of OP forecasts to win favor with company management.

Table 7 shows striking results. First, the first row of each panel indicates that in the pre-reform period, the forecasts of brokerage, syndicate, non-sanctioned, and sanctioned firm are significantly more accurate than those of research firms (except for three-quarter-ahead quarterly earnings forecasts in panel C). Second, the accuracy of research firm analysts significantly improved after the reforms, when compared with other analysts who followed the same companies in the same forecasting period. In contrast, investment bank analysts become significantly less accurate after the reforms. The simple difference-in-differences estimates reported in columns (6) to (9) indicate that the relative improvement in the accuracy of research firm analysts are statistically different from the relative drop in the accuracy of analysts from other firm types.

To control for other sources of variations in forecast accuracy, we run difference-in-differences regressions. Table 8 reports that the estimated coefficients on *BROKERAGE*, *SYNDICATE*, *NONSANC*, and *SANCTIONED* are all statistically positive, indicating that the forecasts from brokerage, syndicate, non-sanctioned, and sanctioned investment banking firms are more accurate that those from research firms in the pre-reform period. The difference-in-differences estimates (the estimated coefficients on the interaction terms) are all significantly negative, lending support to the conclusion that the reforms have differential impact on research firm and investment bank analysts.

In summary, the reforms targeting investment bank analysts have negatively affected the forecast accuracy of investment bank analysts and unexpectedly improved the accuracy of research firm analysts. The former might be due to the fact that investment bank analysts use OP forecasts less frequently in the post-reform period, which negatively affects their forecast accuracy (Ke and Yu 2006). The latter might be

attributed to the fact that the Global Settlement provides $432.5 million to support

independent research. Indeed, the statistics in table 2 show that research firm analysts

cover less companies and experienced less turnover after the reforms.[9]

5. Empirical findings on stock recommendations

5.1. Recommendation optimism/pessimism

We create two measures to capture relative optimism/pessimism of analysts' stock

recommendations, taking into account the discrete nature of stock recommendations. For

each company j followed by analyst i in period t, we calculate the percentages of other

analysts' recommendations of company j in the same period that are more favorable than

analyst i's recommendation ($LessOPT_{ijt}$) and that are less favorable ($LessPESS_{ijt}$). A high

(low) $LessOPT_{ijt}$ indicates that analyst i's recommendation of company j is relatively less

(more) optimistic. Similarly, a high (low) $LessPESS_{ijt}$ means that analyst i's

recommendation is relatively less (more) pessimistic.

These two measures are motivated by the fact that a stock recommendation could

be favourable, unfavourable, or identical when compared with another recommendation,

so less optimism is not equivalent to more pessimism. Hence, we need both *LessOPT*

and *LessPESS* to capture the relative ranking of stock recommendations. These two

measures are computed for all companies that are followed by at least three analysts. We

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[9] Our results could also be affected by Reg FD, as its effective date of October 2000 falls within our pre-reform period (1998 – 2001). Prior literature examining the impact of Reg FD on analyst forecast accuracy finds mixed results. While Bailey et al. (2003) show that Reg FD has no impact on accuracy, Agrawal et al. (2006) document that forecasts become less accurate post-Reg FD. The sample period used by Bailey et al. (2006) ends in the second quarter of 2001, so it has a short post-Reg FD period. The sample period in Agrawal et al. (2006) ends in June 2004, so it includes the effects of the conflicts-of-interest reforms. Our results remain qualitatively unchanged if we repeat our analysis including only year 2001 in our pre-reform period, suggesting that the conflicts-of-interest reforms (not Reg FD) are associated with the drop in forecast accuracy.

average *LessOPT$_{ijt}$* and *LessPess$_{ijt}$* across all companies followed by analyst *i* in period *t* to obtain the average relative recommendation optimism and pessimism, *LessOPT$_{it}$* and *LessPESS$_{it}$*, of analyst *i* in period *t*.

Table 9 presents the levels and changes in relative stock recommendation optimism/pessimism. Columns (6) to (9) in panel A indicate that there is no significant difference in stock recommendation optimism between research firm analysts and other analysts in the pre-reform period. However, the last row of panel A suggests, sanctioned investment bank analysts become less optimistic while non-sanctioned bank analysts become more optimistic after the reforms. In the post-reform period, sanctioned bank analysts are less optimistic than research firm analysts, as shown in column (9). Specifically, 27.6% of the recommendations issued by sanctioned bank analysts are less optimistic than those of other analysts following the same companies, compared with 21.9% for research firm analysts. The simple difference-in-differences estimate is also significantly positive for sanctioned bank analysts (column 9, last row of panel A), suggesting that the effect of the reforms on them is statistically bigger than that on their research firm counterparts.

Panel B in table 9 reports that after the reforms, research, brokerage, syndicate, and non-sanctioned analysts experience a significant increase in *LessPESS*, while sanctioned-bank analysts experience a significant drop. This evidence suggests that sanctioned bank analysts become more pessimistic (i.e., there are fewer analysts with a less favorable recommendation on the same companies followed by the sanctioned bank analysts) in their stock recommendations and the analysts from other firms become less pessimistic. Further, in the post-reform period, both non-sanctioned and sanctioned banks are

significantly more pessimistic than research firms, as reported in columns (8) and (9), respectively. The simple difference-in-differences estimate reported under column (9), last row of the panel, is significantly negative, which is consistent with the sanctioned-bank analyst recommendations becoming more pessimistic after the reforms.

Table 10 reports the difference-in-differences regressions of stock recommendation optimism/pessimism. The estimated coefficients on *BROKERAGE*, *SYNDICATE*, *NONSANC*, and *SANCTIONED* are not distinguishable from zero, indicating that in the pre-reform period there is no difference in the level of optimism and pessimism between research firms and other firm types. Column (1), where the dependent variable is *LessOPT*, reports that the estimated coefficient on *D×SANCTIONED* is significantly positive, indicating that the reforms have a significant impact on the optimism of sanctioned bank analysts – they become relatively less optimistic. Furthermore, column (2), where the dependent variable is *LessPESS*, shows that the estimated coefficient on *D×SANCTIONED* is negative, providing support that sanctioned bank recommendations become more pessimistic when compared with research firm recommendations after the reforms

Barber et al. (2006) show that the distribution of stock recommendations shifts away from buys and towards sells after securities firms are required to report ratings distributions in 2002 per *NASD* Rule 2711. The disclosure requirement applies to all types of firms, but our results suggest that the impact of this disclosure is bigger for sanctioned bank analysts than for the analysts from other firms. Furthermore, Kadan et al. (2008) find that many securities firms moved from a five-tier stock rating system to a three-tier one in 2002. Since we use a difference-in-differences research design, our

results are not likely driven by such a change, as long as the research firms and investment banks all make the rating system change.

In summary, we document evidence consistent with the conflicts-of-interest reforms affecting the distribution of stock recommendations across different firm types. However, the impact is the greatest for sanctioned bank analysts' recommendations. Since Table 1 shows that the analysts from sanctioned and non-sanctioned banks issued about 80% of all stock recommendations, the reforms reduce the optimism of the majority of the analysts issuing stock recommendations. The change in the optimism and pessimism of stock recommendations, however, does not necessarily benefit investors. One way to address this issue is to examine the impact of the reforms on the profitability of analyst recommendations, which we turn to next.

5.2. Profitability of stock recommendation

To address whether the reforms have any economic consequence on stock recommendations, we compare and contrast the profitability of recommendations in the pre- and post-reform periods. We compute the profitability of stock recommendations using the methodology of Barber et al. (2007), except that we form trading portfolios at analyst level instead of securities firm level. Specifically, we classify the upgrades to buy or strong buy, initiations, resumptions, and reiterations of coverage with a buy or strong buy rating into a buy portfolio. A stock enters the buy portfolio on the date the recommendation is issued. The stock leaves the portfolio either on the day before the next downgraded recommendation or after 255 trading days following the initial recommendation, whichever comes first. The hold/sell portfolio is constructed similarly.

Each portfolio consists of all the companies an analyst follows and is updated daily. Daily abnormal return (alpha) is the intercept from the estimation of the Fama-French three factors plus the Carhart momentum factor regression model, estimated by analyst and over the pre- and post-reform periods.

Table 11 summarizes the findings. Panel A shows that the magnitudes of the average daily abnormal returns for the buy portfolios of research firm analysts are 5.9 and 2.7 basis points, respectively, in the pre- and post-reform periods, but the change is indistinguishable from zero. However, the profitability of non-sanctioned bank recommendations drops significantly by 2.9 basis points. Furthermore, the changes are positive, but insignificant, for the other analyst types. Finally, the profitability of stock recommendations made by brokerage, syndicate, and non-sanctioned bank, and sanctioned firm analysts, relative to that made by research firm analysts are not significantly different in either the pre- or post-period. The insignificant differences are inconsistent with the prediction of Barber et al. (2006) that the buy recommendations of investment banks should underperform those of research firms, because investment banks issue more buy ratings in the pre-reform period.

The results for the hold/sell portfolio are reported in panel B. The average abnormal returns decrease after the reforms for all firm types, but only the decrease for non-sanctioned banks is statistically different from zero. Since investment banks issued less sell/hold recommendations than research firms did in the pre-reform period, their abnormal returns are lower than those of research firms as expected (Barber et al. 2006), but the difference is not significant. In additional, the simple difference-in-differences estimates are also indistinguishable from zero.

Our pre-reform period (1998 – 2001) results are consistent with the findings in Barber et al. (2007) that for the period from 1996 through March 2000, there is no significant difference in the stock recommendation performance between research firms (including brokerage firms) and investment banks. They only find a significant difference in the period from March 2000 through June 2003. However, more than half of this sample period is not included in our analysis, because we exclude the transition years 2002 and 2003 to prevent noise from the changing regulatory environment affecting our tests. Although it should be interpreted cautiously, our evidence is consistent with the reforms not having a permanent impact on the profitability of analysts' stock recommendations.

6. Concluding remarks

In this study, we investigate whether the conflicts-of-interest reforms targeting investment banks have achieved the aimed goal of providing more objective research for investors. We compare and contrast various measures of analysts' research biases across different types of investment firms before and after the reforms.

In the pre-reform period, we document that earnings forecasts and stock recommendations made by independent research firm analysts are more optimistic than those made by investment bank analysts. However, investment bank analysts' forecasts exhibit a higher frequency of optimism-to-pessimism intertemporal biases than analysts from research firms. Research firm analysts' forecast accuracy is significantly worse than that of investment bank analysts, but the profitability of their stock recommendations are indistinguishable from each other. Hence, it appears that regulators

target investment banks because they are influential in the capital markets (Watts and Zimmerman, 1986).

After the reforms, forecast optimism reduces for all firm types, but the reduction is unexpectedly bigger for research firms than investment banks. There is a significant reduction in the frequency of optimism-to-pessimism intertemporal forecast biases and in the optimism of stock recommendations made by investment bank analysts. These findings are consistent with the reforms achieving their intended goal of making investment bank analysts' research more objective, or less upwardly biased. However, the profitability of stock recommendations from different types of firms remains unchanged and insignificantly different from each other. In addition, while the accuracy of research firm analysts significantly improves, that of investment bank analysts drops.

Appendix: Variable definition

Forecast optimism:

$$FOPT_{ijt}^{t-k} = \frac{FORECAST_{ijt}^{t-k} - ACTUAL_{jt}}{STDDEV(FORECAST_{jt}^{t-k})}, \text{ where}$$

$FORECAST_{ijt}^{t-k}$ is analysts i's forecast of company j's performance for period t, as of t-k, where k is forecast horizon. $ACTUAL_{jt}$ is firm j's actual earnings in period t. $STDDEV(FORECAST_{jt}^{t-k})$ is the standard deviation of all forecasts for company j and period t, as of t-k. We compute $FOPT_{ijt}^{t-k}$ for companies that are followed by at least three analysts and we use the first forecast made by each analyst for the same company and forecast period. $FOPT_{ijt}^{t-k}$ is then averaged across all companies followed by analyst i in period t to compute analyst i's average forecast optimism, $FOPT_{it}^{t-k}$.

Optimism-to-pessimism intertemporal forecast bias

The average of OP_{ijt} for all company j's covered by analyst i in year t. OP_{ijt} is equal to 1 if analyst i's 9-month-ahead forecast of company j's annual earnings in period t is greater than the actual earnings (i.e., initial optimism) and the 1-month-ahead forecast is less than the actual earnings (i.e., final pessimism); otherwise, OP_{ijt} is set to zero. To be included in the computation of OP_{ijt}, we require companies to be followed by at least three analysts. The quarterly-based OP forecast bias variable is constructed following a similar procedure.

Stock recommendation optimism/pessimism

For each company j followed by analyst i in period t, we calculate the percentages of other analysts' recommendations of company j in the same period that are more favorable than analyst i's recommendation ("$LessOPT_{ijt}$") and that are less favorable ("$LessPESS_{ijt}$"). A high (low) $LessOPT_{ijt}$ indicates that analyst i's recommendation of company j is relatively less (more) optimistic. Similarly, a high (low) $LessPESS_{ijt}$ means that analyst i's recommendation is relatively less (more) pessimistic. These two measures are computed for all companies that are followed by at least three analysts. We average $LessOPT_{ijt}$ and $LessPess_{ijt}$ across all companies followed by analyst i in period t to obtain the average relative recommendation optimism/pessimism ($LessOPT_{it}$ and $LessPESS_{it}$) of analyst i in period t.

D	An indicator variable that equals one in the post-reform period, and zero otherwise.
BROKERAGE	An indicator variable that equals one for analysts from brokerage firms, and zero otherwise.
SYNDICATE	An indicator variable that equals one for analysts from syndicate firms, and zero otherwise.
NONSANC	An indicator variable that equals one for analysts from non-sanctioned full-service investment banks, and zero otherwise.
SANCTIONED	An indicator variable that equals to one for analysts from sanctioned full-service investment banks, and zero otherwise.
FORECAST_HORIZON	The average number of days between the forecast date and the forecast period end date for the portfolio of companies followed by an analyst.

Forecast accuracy

$$Accuracy_{ijt} = 100 - 100 \times \left\{ \frac{Rank_{ijt} - 1}{NumberFollowing_{jt} - 1} \right\},$$

where $Rank_{ijt}$ is analyst *i*'s forecast accuracy rank for company *j* in period *t*, and $NumberFollowing_{jt}$ is the number of analysts following company *j* in period *t*. We use the last forecast made by each analyst for the same company and forecast period.

ANALYST_CONTROLS:

Analyst experience	The average number of years an analyst has issued earnings forecasts or recommendations for the companies they follow.
No. of companies followed	The number of companies for which analysts provides earnings forecasts in a corresponding calendar year.
Analyst specialization	The average percentage of companies followed by an analyst with the same two digit SIC code as each company being followed. The denominator is the total firms followed by the analyst in the sample period 1998-2007 (without considering firms added/dropped in the middle).
Analyst turnover	An indicator variable that equals to one at the year when an analyst moving from one brokerage house to another on I/B/E/S, leaving the profession, or moving to a brokerage house not included in I/B/E/S; otherwise, it equals to zero.

Percent of new following	The percentage of companies that an analyst covers in the following year that are not companies they are covering this year.

FIRM_CONTROLS:

Brokerage firm size rank	Percentile ranking of the total number of analysts employed by the brokerage house to which analyst k belongs in the calendar year in which the forecast is issued, relative to other brokerage houses. If the number of analysts are tied, we use the smallest of the corresponding ranks.
Brokerage specialization	Percentage of analyst k's brokerage house analysts which follows company j's industry in the calendar year in which the forecast was issued.

COMPANY_CONTROLS:

Average company size (log)	The average logarithm of the market value of equity of companies followed by an analyst.
Average leverage	The average debt to equity ratio of the companies followed by an analyst.
Average gross margin	The average gross margin (= 1 – Cost of goods sold / Sales) of the companies followed by an analyst.
Average sales growth	The average growth in sales of the companies followed by an analyst.
Average book-to-market	The average book-to-market ratio of the companies followed by an analyst.

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Table 1

Sample Characteristics of securities firms and analysts, by firm type and research output

The sample covers the period from January 1998 to December 2007. Analyst earnings forecasts and stock recommendations are from Thomson Financial's I/B/E/S database. Investment banks are those listed as investment banks by Nelson's Directory of Investment Research and identified as lead or co-lead underwriters by Thomson Financial's SDC database. Investment banks are divided into sanctioned and non-sanctioned banks. The sanctioned banks are Bear Stearns, Credit Suisse First Boston, Goldman Sachs, Lehman Brothers, J.P. Morgan Securities, Merrill Lynch, Morgan Stanley, Citigroup Global Markets (formerly known as Salomon Smith Barney), UBS Warburg, and U.S. Bancorp Piper Jaffray. Syndicate banks are those firms listed by Nelsons as either investment banks or brokers and identified by SDC as managers or co-managers, but not lead or co-lead underwriters. Independent research firms are those listed as such by Nelsons and not found in the SDC database. The rest of the firms are classified as brokerage firms if they are not identified as lead/co-lead underwriter or manager/co-manager by SDC.

	(1) Research firms	(2) Brokerage firms	(3) Syndicate firms	(4) Non-sanctioned banks	(5) Sanctioned banks
Panel A: Number of Securities Firms					
1-quarter-ahead forecast	88	91	96	147	10
2-quarter-ahead forecast	86	88	94	147	10
3-quarter-ahead forecast	87	87	94	147	10
1-year-ahead annual forecast	102	137	111	168	10
Stock recommendation	84	112	100	156	10
Panel B: Number of Analysts					
1-quarter-ahead forecast	362	500	969	4,856	2,514
2-quarter-ahead forecast	355	447	896	4,672	2,432
3-quarter-ahead forecast	356	441	866	4,424	2,387
Annual forecast	476	767	1,234	5,741	3,162
Stock recommendation	291	477	863	4,211	2,434

Table 2
Descriptive statistics on analyst, firm, and company characteristics
The pre-reform period covers January 1998 through December 2001 and the post-reform period covers January 2004 through December 2007. See Table 1 for firm classification. All variables are defined in the appendix.

	(1) Research firms	(2) Brokerage firms	(3) Syndicate firms	(4) Non-sanctioned banks	(5) Sanctioned banks
Panel A: Pre-reform period (1998 – 2001)					
Forecast horizon (days)	259	256	254	262	268
Analyst experience (years)	5.35	5.84	5.52	5.96	6.16
Number of companies following	17.36	10.59	11.12	12.00	12.41
Analyst industry specialization	0.43	0.53	0.52	0.56	0.59
Analyst turnover	69.63	53.19	57.19	82.73	95.81
Percent of new following	0.36	0.40	0.42	0.29	0.20
Brokerage firm size rank	0.38	0.31	0.30	0.25	0.20
Brokerage firm specialization	0.60	0.48	0.49	0.46	0.41
Average company size (log)	7.07	7.14	7.02	7.01	7.69
Average leverage	0.53	0.53	0.52	0.52	0.56
Average gross margin	0.29	0.25	0.32	0.28	0.30
Average sales growth	1.26	1.25	1.40	1.40	1.35
Average book-to-market	0.52	0.52	0.51	0.54	0.53
Panel B: Post-reform period (2004-2007)					
Forecast horizon (days)	267	267	273	280	281
Analyst experience (years)	5.25	5.85	5.86	6.42	5.91
Number of companies following	10.37	9.18	10.47	12.90	11.72
Analyst industry specialization	0.50	0.58	0.60	0.60	0.64
Analyst turnover	58.98	55.06	68.04	85.46	97.13
Percent of new following	0.39	0.51	0.44	0.26	0.11
Brokerage firm size rank	0.27	0.34	0.30	0.24	0.22
Brokerage firm specialization	0.42	0.45	0.43	0.38	0.37
Average company size (log)	6.94	7.27	7.40	7.39	8.17
Average leverage	0.49	0.50	0.51	0.52	0.55
Average gross margin	0.25	0.23	0.19	0.23	0.31
Average sales growth	1.23	1.20	1.26	1.25	1.21
Average book-to-market	0.42	0.45	0.44	0.44	0.43

Table 3
Forecast optimism by firm type and sample period
The pre- and post-reform periods cover January 1998 – December 2001 and January 2004 – December 2007, respectively. Forecast optimism, *FOPT*, is forecasted earnings minus actual earnings, scaled by the standard deviation of all forecasts for the same company. Quarterly and annual analyst earnings forecasts are from Thomson Financial's I/B/E/S database. See Table 1 for firm classification and the Appendix for variable definitions. *, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided t-test.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Non-					
Sample	Research	Brokerage	Syndicate	Sanctioned	Sanctioned		Significance of differences		
Period	firms	firms	firms	banks	banks	(2)–(1)	(3)–(1)	(4)–(1)	(5)–(1)
Panel A: One-quarter-ahead quarterly earnings									
Pre-period	-0.354	-0.399	-0.538	-0.511	-0.601				**
Post-period	-0.626	-0.369	-0.618	-0.540	-0.830				**
Change	-0.272 *	0.030	-0.080	-0.029	-0.229 ***				
Panel B: Two-quarter-ahead quarterly earnings									
Pre-period	0.802	0.774	0.764	0.612	0.525				*
Post-period	0.122	0.188	0.129	0.138	0.037				
Change	-0.680 ***	-0.586 ***	-0.635 ***	-0.475 ***	-0.488 ***				
Panel C: Three-quarter-ahead quarterly earnings									
Pre-period	2.032	2.208	1.801	1.765	1.610				*
Post-period	0.232	0.431	0.209	0.237	0.096		**		**
Change	-1.800 ***	-1.778 ***	-1.592 ***	-1.527 ***	-1.514 ***				
Panel D: Annual earnings									
Pre-period	2.639	1.452	0.919	1.030	0.944				
Post-period	0.280	0.324	0.139	0.210	0.004				*
Change	-2.359	-1.128 ***	-0.780 ***	-0.820 ***	-0.941 ***				

Table 4
Difference-in-differences regressions of forecast optimism
The pre- and post-reform periods cover January 1998 – December 2001 and January 2004 – December 2007, respectively. Forecast optimism, *FOPT*, is forecasted earnings minus actual earnings, scaled by the standard deviation of all forecasts for the same company. *D* is an indicator variable that equals to one in the post-reform period, and zero in the pre-reform period. *BROKERAGE* is an indicator variable that equals to one for analysts from brokerage firms, and zero otherwise. *SYNDICATE* is an indicator variable that equals to one for analysts from syndicate firms, and zero otherwise. *NONSANC* is an indicator variable that equals to one for analysts from non-sanctioned full-service investment banks, and zero otherwise. *SANCTIONED* is an indicator variable that equals to one for analysts from sanctioned full-service investment banks, and zero otherwise. See Table 1 for firm classification and the Appendix for variable definitions. *, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided t-test.

	(1) 1-quarter-ahead quarterly earnings	(2) 2-quarter-ahead quarterly earnings	(3) 3-quarter-ahead quarterly earnings	(4) Annual earnings
Intercept	0.934	1.328	2.655	3.004
	(4.55) ***	(4.67) ***	(7.69) ***	(1.89) *
D	-0.798	-1.325	-2.256	-2.263
	(-5.28) ***	(-7.55) *	(-9.83) ***	(-1.41)
BROKERAGE	-0.219	-0.194	0.201	-1.025
	(-1.39)	(-0.92)	(0.67)	(-0.63)
SYNDICATE	-0.307	-0.213	-0.226	-1.643
	(-2.23) **	(-1.17)	(-0.94)	(-1.01)
NONSANC	-0.241	-0.292	-0.280	-1.573
	(-1.93) *	(-1.78) *	(-1.29)	(-0.95)
SANCTIONED	-0.267	-0.302	-0.324	-1.612
	(-2.09) **	(-1.81) *	(-1.46)	(-0.96)
D×BROKERAGE	0.319	0.199	-0.039	0.983
	(1.63)	(0.88)	(-0.12)	(0.60)
D×SYNDICATE	0.248	0.255	0.205	1.501
	(1.49)	(1.34)	(0.82)	(0.93)
D×NONSANC	0.310	0.366	0.346	1.428
	(2.05) *	(2.15) **	(1.54)	(0.88)
D×SANCTIONED	0.130	0.358	0.395	1.415
	(0.84)	(2.06) **	(1.73) *	(0.86)
Relative Accuracy	-0.002	-0.000	0.001	-0.003
	(-1.76) *	(-0.09)	(0.64)	(-0.80)
Analyst Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Company Controls	Yes	Yes	Yes	Yes
Year Dummy	Yes	Yes	Yes	Yes
R-Squared	0.049	0.051	0.127	0.019
N	23,773	22,604	21,532	23,857

Table 5
Percentage of optimism-to-pessimism intertemporal forecast biases by firm type and sample period
The pre- and post-reform periods cover January 1998 – December 2001 and January 2004 – December 2007, respectively. Optimism-to-pessimism, *OP*, is an indicator variable that equals to 1 if an analyst's 9-month-ahead forecast is greater than actual earnings and the 1-month-ahead forecast is less the actual earnings. Quarterly and annual analyst earnings forecasts are from Thomson Financial's I/B/E/S database. See Table 1 for firm classification and the Appendix for variable definitions. *, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided t-test.

Sample Period	(1) Research firms	(2) Brokerage firms	(3) Syndicate firms	(4) Non-Sanctioned banks	(5) Sanctioned banks	(6)	(7)	(8)	(9)
						Significance of differences			
						(2)–(1)	(3)–(1)	(4)–(1)	(5)–(1)
Panel A: Quarterly OP forecast biases									
Pre-period	0.266	0.314	0.314	0.314	0.323	**	**	***	***
Post-period	0.227	0.209	0.220	0.218	0.214				
Change	-0.038 *	-0.105 ***	-0.094 ***	-0.096 ***	-0.108 ***	**	**	***	***
Panel B: Annual OP forecast biases									
Pre-period	0.216	0.260	0.258	0.255	0.273	**	***	***	***
Post-period	0.207	0.211	0.201	0.209	0.202				
Change	-0.009	-0.048 ***	-0.057 ***	-0.046 ***	-0.071 ***	*	**	**	***

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Table 6
Difference-in-differences regression of the percentage in optimism-to-pessimism forecast biases.
The pre- and post-reform periods cover January 1998 – December 2001 and January 2004 – December 2007, respectively. Optimism-to-pessimism, *OP*, is an indicator variable that equals to 1 if an analyst's 9-month-ahead forecast is greater than actual earnings and the 1-month-ahead forecast is less the actual earnings. *D* is an indicator variable that equals to one in the post-reform period, and zero in the pre-reform period. *BROKERAGE* is an indicator variable that equals to one for analysts from brokerage firms, and zero otherwise. *SYNDICATE* is an indicator variable that equals to one for analysts from syndicate firms, and zero otherwise. *NONSANC* is an indicator variable that equals to one for analysts from non-sanctioned full-service investment banks, and zero otherwise. *SANCTIONED* is an indicator variable that equals to one for analysts from sanctioned full-service investment banks, and zero otherwise. See Table 1 for firm classification and the Appendix for variable definitions. *, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided t-test.

	(1) Quarterly OP		(2) Annual OP	
Intercept	0.353		0.289	
	(12.13)	***	(11.44)	***
D	-0.072		-0.049	
	(-3.45)	***	(-2.92)	***
BROKERAGE	0.016		0.029	
	(0.64)		(1.59)	
SYNDICATE	0.027		0.026	
	(1.32)		(1.71)	*
NONSANC	0.030		0.019	
	(1.57)		(1.42)	
SANCTIONED	0.034		0.027	
	(1.76)	*	(1.95)	*
D×BROKERAGE	-0.016		-0.011	
	(-0.60)		(-0.48)	
D×SYNDICATE	-0.025		-0.022	
	(-1.08)		(-1.18)	
D×NONSANC	-0.033		-0.018	
	(-1.62)		(-1.08)	
D×SANCTIONED	-0.042		-0.038	
	(-2.09)	**	(-2.25)	**
Analyst Controls	Yes		Yes	
Firm Controls	Yes		Yes	
Company Controls	Yes		Yes	
Year Dummy	Yes		Yes	
R-Squared	0.085		0.084	
N	21,695		22,682	

Table 7
Relative forecast accuracy by firm type and sample period
The pre- and post-reform periods cover January 1998 – December 2001 and January 2004 – December 2007, respectively. Relative forecast accuracy is forecast accuracy rank for all companies followed by an analyst. Quarterly and annual analyst earnings forecasts are from Thomson Financial's I/B/E/S database. See Table 1 for firm classification and the Appendix for variable definitions. *, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided t-test.

Sample Period	(1) Research firms	(2) Brokerage firms	(3) Syndicate firms	(4) Non-Sanctioned banks	(5) Sanctioned banks	(6) Significance of differences (2)–(1)	(7) (3)–(1)	(8) (4)–(1)	(9) (5)–(1)
Panel A: One-quarter-ahead quarterly earnings									
Pre-period	55.01	60.01	60.26	62.79	64.07	***	***	***	***
Post-period	58.90	56.31	59.86	60.37	58.93	**		**	
Change	3.89 **	-3.70 **	-0.40	-2.41 ***	-5.14 ***	***	**	***	***
Panel B: Two-quarter-ahead quarterly earnings									
Pre-period	50.96	56.01	55.85	59.14	60.61	***	***	***	***
Post-period	57.55	55.77	54.69	55.89	54.81		***	**	***
Change	6.60 ***	-0.23	-1.16	-3.24 ***	-5.80 ***		***	***	***
Panel C: Three-quarter-ahead quarterly earnings									
Pre-period	52.74	54.71	54.80	56.31	58.22		*	***	***
Post-period	58.14	55.19	52.04	52.24	53.50	**	***	***	***
Change	5.40 ***	0.48	-2.76 ***	-4.07 ***	-4.72 ***	**	***	***	***
Panel D: Annual earnings									
Pre-period	48.10	55.17	54.42	57.43	58.80	***	***	***	***
Post-period	55.26	52.70	54.31	56.85	56.33	*		*	
Change	7.16 ***	-2.47 **	-0.11	-0.58 *	-2.47 ***	***	***	***	***

Table 8
Difference-in-differences regressions of relative forecast accuracy.
The pre- and post-reform periods cover January 1998 – December 2001 and January 2004 – December 2007, respectively. Relative forecast accuracy is forecast accuracy rank for all companies followed by an analyst. *D* is an indicator variable that equals to one in the post-reform period, and zero in the pre-reform period. *BROKERAGE* is an indicator variable that equals to one for analysts from brokerage firms, and zero otherwise. *SYNDICATE* is an indicator variable that equals to one for analysts from syndicate firms, and zero otherwise. *NONSANC* is an indicator variable that equals to one for analysts from non-sanctioned full-service investment banks, and zero otherwise. *SANCTIONED* is an indicator variable that equals to one for analysts from sanctioned full-service investment banks, and zero otherwise. See Table 1 for firm classification and the Appendix for variable definitions. *, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided t-test.

	(1) 1-quarter-ahead quarterly earnings	(2) 2-quarter-ahead quarterly earnings	(3) 3-quarter-ahead quarterly earnings	(4) Annual earnings
Intercept	54.167	67.291	61.23	62.85
	(24.45) ***	(25.19) ***	(23.61) ***	(31.24) ***
D	6.901	9.063	6.823	6.304
	(4.23) ***	(5.67) ***	(4.50) **	(4.33) ***
BROKERAGE	5.649	5.480	2.575	6.682
	(3.11) ***	(3.18) ***	(1.67) *	(4.70) ***
SYNDICATE	6.649	5.858	2.931	6.041
	(4.36) ***	(3.96) ***	(2.36) **	(4.90) ***
NONSANC	7.678	7.073	3.616	7.006
	(5.43) ***	(5.28) ***	(3.22) ***	(6.20) ***
SANCTIONED	8.122	7.992	4.767	7.003
	(5.63) ***	(5.88) ***	(4.12) ***	(6.06) ***
D×BROKERAGE	-6.899	-6.536	-4.400	-9.139
	(-3.13) ***	(-3.04) ***	(-2.16) **	(-4.95) ***
D×SYNDICATE	-5.535	-8.719	-9.498	-7.001
	(-3.14) ***	(-4.95) ***	(-5.63) ***	(-4.32) ***
D×NONSANC	-6.637	-10.222	-10.568	-7.297
	(-4.16) ***	(-6.52) ***	(-7.08) ***	(-5.07) ***
D×SANCTIONED	-9.035	-12.148	-10.595	-9.519
	(-5.58) ***	(-7.57) ***	(-6.88) ***	(-6.45) ***
Analyst Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Company Controls	Yes	Yes	Yes	Yes
Year Dummy	Yes	Yes	Yes	Yes
R-Squared	0.047	0.042	0.033	0.195
N	23,797	22,960	21,870	24,222

Table 9

Stock recommendation optimism/pessimism by firm type and sample period

The pre- and post-reform periods cover January 1998 – December 2001 and January 2004 – December 2007, respectively. Relative recommendation optimism (pessimism), *LessOPT* (*LessPESS*), is the percentages of other analysts' recommendations in the same period that are more (less) favorable than an analyst's recommendation. Quarterly and annual analyst earnings forecasts are from Thomson Financial's I/B/E/S database. See Table 1 for firm classification and the Appendix for variable definitions. *, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided t-test.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Non-			Significance of differences		
Sample	Research	Brokerage	Syndicate	Sanctioned	Sanctioned				
Period	firms	firms	firms	banks	banks	(2)–(1)	(3)–(1)	(4)–(1)	(5)–(1)
Panel A: LessOPT									
Pre-period	0.219	0.217	0.237	0.229	0.226				
Post-period	0.219	0.213	0.227	0.216	0.276				***
Change	0.000	-0.003	-0.009	-0.013 ***	0.050 ***				***
Panel B: LessPESS									
Pre-period	0.248	0.265	0.248	0.238	0.233				
Post-period	0.279	0.294	0.268	0.258	0.202			***	***
Change	0.031 *	0.029 **	0.020 **	0.019 ***	-0.031 ***				***

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Table 10

Difference-in-differences regression of stock recommendation optimism and pessimism

The pre- and post-reform periods cover January 1998 – December 2001 and January 2004 – December 2007, respectively. Relative recommendation optimism (pessimism), *LessOPT* (*LessPESS*), is the percentages of other analysts' recommendations in the same period that are more (less) favorable than an analyst's recommendation. *D* is an indicator variable that equals to one in the post-reform period, and zero in the pre-reform period. *BROKERAGE* is an indicator variable that equals to one for analysts from brokerage firms, and zero otherwise. *SYNDICATE* is an indicator variable that equals to one for analysts from syndicate firms, and zero otherwise. *NONSANC* is an indicator variable that equals to one for analysts from non-sanctioned full-service investment banks, and zero otherwise. *SANCTIONED* is an indicator variable that equals to one for analysts from sanctioned full-service investment banks, and zero otherwise. See Table 1 for firm classification and the Appendix for variable definitions. *, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided t-test.

	(1) Less OPT	(2) Less PESS
Intercept	0.162	0.334
	(6.69) ***	(13.12) ***
D	-0.032	0.046
	(-1.65) *	(2.25) **
BROKERAGE	-0.024	0.016
	(-1.14)	(0.74)
SYNDICATE	-0.008	0.014
	(-0.43)	(0.70)
NONSANC	-0.017	0.020
	(-0.92)	(1.02)
SANCTIONED	-0.024	0.020
	(-1.28)	(0.97)
D×BROKERAGE	0.015	0.005
	(0.64)	(0.18)
D×SYNDICATE	0.009	-0.011
	(0.44)	(-0.51)
D×NONSANC	0.013	-0.022
	(0.66)	(-1.08)
D×SANCTIONED	0.071	-0.076
	(3.61) ***	(-3.63) ***
Relative Accuracy	-0.003	0.000
	(-3.13) ***	(0.79)
Analyst Controls	Yes	Yes
Firm Controls	Yes	Yes
Company Controls	Yes	Yes
Year Dummy	Yes	Yes
R-Squared	0.025	0.026
N	19,669	19,669

Table 11

Daily abnormal returns by firm type and sample period

The pre- and post-reform periods cover January 1998 – December 2001 and January 2004 – December 2007, respectively. The buy portfolio consists of stocks that are upgraded to buy or strong buy, initiations, resumptions, and reiterations of coverage with a buy or strong buy rating. A stock enters the buy portfolio on the date the recommendation is issued. The stock leaves the portfolio either on the day before the next downgraded recommendation or after 255 trading days following the initial recommendation, whichever comes first. The hold/sell portfolio is constructed similarly. Each portfolio consists of all the companies an analyst followed and is updated daily. Daily abnormal return (alpha) is the intercept from the estimation of the Fama-French three factors plus the Carhart momentum factor regression model, estimated by analyst and over the pre- and post-reform periods. Stock recommendation data are from Thomson Financial. See Table 1 for firm classification and the Appendix for variable definitions. *, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided t-test.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Non-			Significance of differences		
Sample	Research	Brokerage	Syndicate	Sanctioned	Sanctioned				
Period	firms	firms	firms	banks	banks	(2)–(1)	(3)–(1)	(4)–(1)	(5)–(1)
Panel A: Buy Portfolio (%)									
Pre-period	0.059	-0.014	0.009	0.050	0.018				
Post-period	0.027	0.010	0.027	0.020	0.038				
Change	-0.031	0.024	0.018	-0.029 **	0.020				
Panel B: Hold/Sell Portfolio(%)									
Pre-period	0.127	-0.056	0.030	0.036	0.072	*			
Post-period	0.004	-0.057	-0.002	-0.014	0.010				
Change	-0.123	-0.001	-0.032	-0.050 ***	-0.062	*			